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Structure and object

Dieterle, Jill Marie, Ph.D.
The Ohio State University, 1994

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STRUCTURE AND OBJECT

DISSERTATION

Presented in Partial Fulfillment of the Requirements for
the Degree Doctor of Philosophy in the Graduate
School of the Ohio State University

By

Jill Marie Dieterle, B.S., M.A.

****

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1994

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# TABLE OF CONTENTS

ACKNOWLEDGEMENTS ........................................................................................................ ii
VITA ........................................................................................................................................ iv
INTRODUCTION .................................................................................................................. 1

CHAPTER

I. STRUCTURES AND MATHEMATICAL OBJECTS ........ 4
   § 1. Structuralism .......................................................... 7
   § 2. The Doctrine of the Nonsensicality of Trans-Structural Identity ........................................ 12
   § 3. Dummett, Dedekind, and Abstraction ................. 19
   § 4. Abstract Objects and the Third Man Argument ................................................................ 30
   § 5. Conclusion ............................................................. 35

II. KNOWLEDGE, REFERENCE, AND ACQUISITION ...... 36
   § 1. Realism and Anti-Realism ................................. 36
   § 2. Anti-Realism: Why all the Fuss? ..................... 39
   § 2.1. Knowledge ........................................................ 40
   § 2.2. Reference ........................................................ 57
   § 2.3. Acquisition ..................................................... 62
   § 3. Conclusion ......................................................... 65

III. IS JULIUS CAESAR IDENTICAL TO TWO? ............ 66
   § 1. Objecthood ...................................................... 67
   § 2. Mathematical Objects ....................................... 71
   § 3. Frege’s Numbers ............................................. 75
   § 4. Frege and Structuralism ..................................... 80
   § 5. Conclusion ...................................................... 93

IV. OBJECTHOOD .................................................................. 96
   § 1. ‘Theory’ and Theories ....................................... 102
   § 2. Singular Terms ................................................ 111
   § 3. Truth ............................................................... 123
<table>
<thead>
<tr>
<th>Section</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>§ 4. Advantages</td>
<td>128</td>
</tr>
<tr>
<td>V. OBJECTIONS</td>
<td>132</td>
</tr>
<tr>
<td>§ 1. The Reductionist Objection</td>
<td>133</td>
</tr>
<tr>
<td>§ 2. The “More to Objecthood...” Objection</td>
<td>141</td>
</tr>
<tr>
<td>§ 2.1. Epistemological Considerations</td>
<td>144</td>
</tr>
<tr>
<td>§ 2.2. Metaphysical Considerations</td>
<td>147</td>
</tr>
<tr>
<td>§ 2.3. Pragmatic Considerations</td>
<td>151</td>
</tr>
<tr>
<td>§ 2.4. Summary</td>
<td>153</td>
</tr>
<tr>
<td>§ 3. Relativity</td>
<td>153</td>
</tr>
<tr>
<td>§ 4. Conclusion</td>
<td>156</td>
</tr>
</tbody>
</table>

BIBLIOGRAPHY ............................................................. 157
INTRODUCTION

The notion of objecthood plays a central role in many classic philosophical disputes. Arguments about universals, possible worlds, propositions, sense impressions, and the ontology of mathematics all depend -- in one way or another -- upon the concept of an object. But often these disputes are unclear, because the concept of an object is left unexplicated. My goal in this dissertation is clarify the notion of objecthood; against that backdrop, I try to illuminate several disputes in the philosophy of mathematics.

I begin in Chapter I with the structuralist conception of mathematical objecthood. Structuralists claim that numbers are objects in the sense that they are places or positions in structures; and structures themselves are construed as types. What it is for something to be the number two, for example, is for it to be the appropriate place in the natural number structure. On this view, what qualifies as an object relative to one theory might qualify as a Fregean function relative to another.

However the realist project is described, structuralism is a version of mathematical realism. In Chapter II, I try to show that common, anti-realist objections to mathematical realism can be met by a structuralist. I (1) offer an account of mathematical knowledge; (2) explain how mathematical constants acquire reference; and (3) show how structuralists can answer the nominalist's acquisition challenge.
Chapter III deals with Frege and his conception of mathematical objecthood. I explore Frege's project in *Die Grundlagen der Arithmetik*, showing how his conception of mathematical objecthood eventually led to Russell's paradox. I then propose a slight alteration of Frege's program: take the notion of saturation as theory relative (I call this the "Neo-Fregean position"). On the Neo-Fregean view, objecthood and objectual reference are theory relative. This alteration offers a solution to the problem that led Frege to his final (and fatal) definition of numbers as extensions of concepts.

In Chapter IV, I extend the structuralist's conception of mathematical objecthood to objecthood generally. The claim is that the reason we regard certain fragments of language as referring to objects is that those fragments of language refer to places in a structure, where a structure is characterized by a coherent theory. The thesis can be summarized as follows:

\[(S) \text{ A coherent theory } T \text{ characterizes a structure, and if } T \text{ has explanatory power, then any expression that functions as a singular term in true sentences relative to } T \text{ refers to an object.}\]

Briefly, a theory (as I use the term 'theory') is a fragment of discourse with a high degree of conceptual homogeneity. I attempt to give a quasi-syntactic characterization of singular termhood, and show how this can be relativized in such a way that we can say that an expression functions as a singular term in certain contexts and not in others. Finally, Chapter V deals with objections to \((S)\).

One of the main advantages of the program I embrace is that it offers a straightforward way to interpret questions of the form "Are αs objects?" The ontology of a coherent, explanatory theory T contains αs if there is a structure characterized by T, and expressions referring to αs function as singular terms relative to T. But if the theory is able, through its expressive apparatus, to
discriminate in a more fine-grained fashion, then $\alpha$-terms may function as complex predications.

Philosophers are often suspicious of seemingly occult mathematical objects. In addition to spelling out the criterion of objecthood, a further goal of the dissertation is to ease this suspicion. The idea is that there is no significant difference in kind between mathematical objects and ordinary objects: the specification of objecthood in mathematics is not importantly different from the specification of objecthood generally. Furthermore, as noted above, the structuralist is able to answer many of the common objections to mathematical realism.
CHAPTER I

STRUCTURES AND MATHEMATICAL OBJECTS

What are numbers? What is, say, the number 2? Is it a set? Is it some other kind of object? Perhaps the symbol '2' stands for no thing at all; perhaps, for example, it stands for a property of some sort. Or perhaps numbers are merely fictional, and have no existence outside of human inquiry. Philosophers have argued about the ontological status of numbers (and mathematical entities generally) for centuries. But, as yet, there has been no consensus on the correct answer to the question "What are numbers?"

The development of set theory stifled the dispute for a period of time. It was shown that arithmetic can be reduced to set theory, and for a while it was in vogue to hold that numbers are just sets. But there is more than one set theoretic reduction of arithmetic -- Zermelo and von Neumann offered non-equivalent reductions of arithmetic to set theory, both of which capture arithmetical relations and functions adequately.\(^1\) However, there is an important difference: the reference of

\(^1\)In fact, there are infinitely many non-equivalent reductions of arithmetic to set theory. But the two examples will illustrate the point.
numerical constants differs in the two reductions. According to the Zermelo reduction, the reference of 'two' is \{\phi\}, whereas according to the von Neumann reduction, the reference of 'two' is \{\phi, \{\phi\}\}.

In [1965], Paul Benacerraf argued that if numbers are sets, it cannot be the case that both set theoretic reductions are right in their account of the reference of '2'. To see why, suppose for a moment that they both were right. Then 2 = \{\phi, \{\phi\}\} and 2 = \{\{\phi\}\}. Since identity is transitive, \{\phi, \{\phi\}\} = \{\{\phi\}\}. But this is clearly false. Therefore, it must be that at least one of the accounts is wrong in its specification of the reference of '2'. But now the question arises as to which one is right. If one is right and the other is wrong (or if both of them are wrong and yet another account is right), then there must be some feature of the correct one which makes it so, and it must be possible to give some reason for thinking that it is the correct account. But, again, we face a problem. What would such a feature be, and what could possibly convince us that a particular account was the right one? For arithmetical purposes, each serves as well as the other.

Benacerraf goes on to point out that the reference of '2' can, in fact, be the appropriate object in any progression of objects whatsoever (i.e., we need not confine the discussion to set theory), as long as the system of objects in question is a denumerably infinite progression (i.e., an \omega-sequence). Any \omega-sequence can adequately serve as the natural numbers, for the only properties of numbers which

---

2 There are actually a number of differences, but only one that is important from the standpoint of answering the question 'What are numbers?'

3 One might argue that one account is the right one, and that this is so is just an unknowable truth. This view, however, seems precarious. We are owed at least (1) some reason for thinking that natural numbers are sets; and (2) what sorts of features would make one account the right one. In the absence of replies to (1) and (2), we have no reason to believe that some particular system of sets is the natural numbers.
are important for arithmetic are those that have to do with their relationships to other numbers, and these properties are instantiated by all w-sequences. The structure of the progression of objects is what matters, not the objects themselves.

Benacerraf concludes from this discussion that numbers cannot be objects ...

because in giving the properties (that is, necessary and sufficient) of numbers you merely characterize an abstract structure -- and the distinction lies in the fact that the "elements" of the structure have no properties other than those relating them to other "elements" of the same structure.... That a system of objects exhibits the structure of the integers implies that the elements of that system have some properties not dependent on structure. It must be possible to individuate those objects independently of the role they play in that structure. But this is precisely what cannot be done with the numbers.... Any object can play the role of 3; that is, any object can be the third element in some progression. What is peculiar to 3 is that it defines that role -- not be being a paradigm of any object which plays it, but by representing the relation that any third member of a progression bears to the rest of the progression. ([1965] p. 291.)

A structuralist agrees with Benacerraf up to a point:

she agrees that any denumerably infinite progression has the structure of the natural numbers and any object can play the role of a particular number. But she denies Benacerraf's conclusion; i.e., she denies the claim that numbers are not objects. Numbers are objects, according to a structuralist, but we have something to say about what it is to be an object, or at least a mathematical object.

---

4 There are various mathematical views that go by the name of "structuralism". Benacerraf is sometimes characterized as a structuralist, Charles Parsons is a structuralist about all mathematical theories save set theory (see [1990]), and Penelope Maddy's view is similar to Parsons' (see [1990]). In [1989] and [1990], Geoffrey Hellman defends a modal version of structuralism. The view I am discussing here most resembles Stewart Shapiro's (see [1983], [1989a] and [1989b]) and Michael Resnik's (see especially [1981] and [1988]). Structuralist tendencies are also apparent in Bigelow [1988] and White [1974]. Quine can also be read as a Benacerraf-style structuralist; see Quine [1986] (especially p. 401). For more of Quine's discussion of mathematics, see [1964], [1969], pp. 45-6 and [1981] pp. 15-16.

5 In a sense to be explained later. It is worth noting here, though, that a structuralist does not claim that the reference of '2' can be the second object in any progression. Different objects can play the role of the natural numbers, but they are not identical to the numbers.
The following five theses are central to the structuralist philosophy of mathematics:

1. Mathematics is the study of structures and the places therein.
2. Mathematical objects are places in structures.
3. A coherent mathematical theory or area of discourse characterizes a structure or class of structures.
4. The ontology of mathematics is theory-relative; an expression may refer to a mathematical object from one perspective, but a Fregean function relative to another.
5. Identity statements are legitimate only if the constants that flank the identity sign refer to places in the same structure.

(1) - (5) are central to the structuralist philosophy of mathematics. Thesis (1) identifies the subject matter of mathematics: structures and the places therein. (2) sums up the structuralist’s conception of mathematical objects. Numbers (for example) are objects, but what it is to be a mathematical object is structurally defined: to be a mathematical object is to be a place in a structure. It makes no sense to speak of mathematical objects outside of the structure of which they are a part. ‘2’ has no reference in isolation -- it is only within the context of particular mathematical structures that we can speak of the reference of ‘2’.

Thesis (3) asserts that a coherent theory characterizes a structure. The axioms of a mathematical theory specify the structure that is its subject matter, and the expressive resources of the theory -- its constants, predicates, concepts, and general individuative principles -- determine what we are able to refer to and what

6There is a problem with this use of the notion of "coherence" which will be addressed in § 3 below. Furthermore, "the language of arithmetic" may be an artificial construct. See Chapter IV, § 1 for discussion.
we are able to say about those things we refer to.\textsuperscript{7} The theory of second-order arithmetic, for example, characterizes the natural number structure. Peano's postulates (stated in the metalanguage) explicitly define the structure of the natural numbers by specifying a distinguished element (0) and a successor function for generating the rest of the structure.\textsuperscript{8} The predicates of arithmetic give us the capacity to speak of the arithmetical relations between the numbers (e.g., the less than relation, the addition function, etc.). The individuative principles allow us to distinguish one number from another. But the theory of arithmetic lacks the expressive resources to refer to any non-arithmetical properties or objects because it lacks predicates that would allow such reference. All that is important from the arithmetical point of view is the structural relationships that hold between the objects.

Perhaps a non-mathematical example will be helpful. The Constitution of the United States characterizes the political structure of the country. It has a formula for determining the places of the political structure and the relationships between those places. The political structure contains the following positions: President, Vice President, Senators, Representatives, and Supreme Court Justices. There are two Senators for every state, and one Representative for every voting district. Currently, the political structure of the United States has 100 Senators, 435 Representatives, and 9 Supreme Court Justices. The language pertaining to the political structure of the United States (which is often employed in political science

\textsuperscript{7}The thesis that the expressive resources of a language can, in some sense, determine its objects is discussed in Quine, especially [1950] and [1981] and Kraut [1980] and [1986]. Similar views are held by Dummett (see especially [1986]) and Charles Chihara (see [1990], p. 71). This dissertation is a further articulation of the idea.

\textsuperscript{8}This will be explained in more detail in § 3 of this chapter.
textbooks) has predicates that give us the capacity to speak of the various political offices and the relations between them.

When we teach students how the government works, we use sentences in which the variables range over the positions in the political structure as such, without regard to any particular token of that structure type. The instructor will say things like “The President has the power to veto a bill.” Were a student to ask, “Do you mean George Bush or Bill Clinton?” the instructor would take this as evidence that the student did not understand. The President as such has veto power; it is a property of the position in the political structure, and not merely of an individual who occupies that position.

Imagine that the language used in political science textbooks was precise (analogous to the language of arithmetic) and had the capacity to speak only of the political offices of this country and the relations between them. The expressive resources of this language would then limit what we could refer to and what we could say about those things we refer to. We could not, for example, speak of the President’s height, for height is not a property of the position itself and is not important from the point of view of political theory.

Thesis (4) captures the idea that whether something is countenanced as an object, or at least a mathematical object, depends on the point of view from which we are speaking. The language of second-order arithmetic has constants that refer to and variables that range over natural numbers, and natural numbers are the only objects it has the capacity to refer to and quantify over. 2 is an object relative to the natural number structure, but ‘\{\{\phi\}\}’ has no objectual reference in that structure. 2 is an object relative to other structures, too. For example, 2 is an object in the real number structure. But it is a different object in these other structures,
In our imaginarily precise political language where we have the capacity to speak only of the political offices of this country, the offices themselves will be the things to which our constants refer and over which our variables range. President, from this perspective, is an object.

My suggestion is that there is a connection between what we countenance as an object and the conceptual resources of the language we employ. This idea has its roots in Carnap and Quine, and has been developed in some detail by Robert Kraut. In [1986], Kraut suggests that there is an intimate connection between ontology, the subject matter of a discourse, and ideology, the store of predicables available in that discourse. (p. 406)

In [1980], Kraut suggests that the species *tiger* is an object or individual relative to biological theory and *gold* is an object relative to chemical theory. Kraut writes:

> These... claims are ontological claims -- claims that certain theories are about objects of a certain kind. Yet the claims appear to be little more than claims about the expressive resources of the theories in question. (p. 134)

The expressive resources of the respective theories determine what the theory is about by dictating what we can talk about relative to those theories. We ought to treat tiger and gold as objects relative to biological theory and chemical theory because the individuative apparatus of those theories allows us to discriminate finely enough to refer to gold and tiger, but no more finely.

Thesis (5) follows directly from (4): Identity claims are illegitimate unless the constants that flank the identity sign refer to places in the same structure. This is because identity claims are legitimate only if the constants that flank the identity because the overall structure is different. Of course, there is a natural way to embed the natural number structure in the real number structure.

10See Carnap [1956], Quine [1950], and Kraut [1980] and [1986].
sign refer to objects. But given the relativity of ontology, this will not be the case if the constants refer to places in different structures. Since ontology is structure relative, identity is structure relative.

It will be worthwhile to pause here to deflect some objections. I have stated that objecthood is theory relative and that reference to objects depends on the discriminative capacities of a language. It may initially seem that I am advocating “linguistic idealism”; i.e., one might think that I am claiming that what exists depends on language. However, this would be a mis-interpretation of my view. Common sense dictates that there are objects in the world and those objects are independent of our language; I do not wish to deny this common sense dictum. Rather, my claim is that given what there is, how we individuate depends on the conceptual resources of a language or fragment of language. What we countenance as an object depends wholly on how we individuate. Our concepts allow us to sort the world into distinct objects. Without our conceptual resources, we would have no way to refer to individual objects and distinguish them from other objects. On the structuralist view, it is a confusion to say that yes, we individuate in such and such a way relative to a particular theory and we treat certain terms as referring to particular, individuated places of space-time yet those terms do not refer to objects. It is a mistake, on the structuralist view, to say that there are no objects of the relevant sort if the theory in question has expressions that occur in true sentences and the natural way to interpret those expressions is as singular terms. The structuralist claims that this is what it is to refer to objects.

Suppose that you are standing in front of a table upon which there is a deck of cards. How many objects are on the table?\textsuperscript{11} There is 1 deck, 52 cards, x number

\textsuperscript{11}This type of question originated with Frege.
of molecules, etc. So what is the correct answer to the question? It seems that the
correct answer is context dependent; it depends on what concept is being employed
in the context of utterance -- decks, cards, or molecules. There would still be 1 deck,
52 cards, and x number of molecules even if we had no conceptual means to refer to
the deck, cards, and molecules: once we have specified the relevant context, there is
a determinate answer to "how many" questions. Objects are not invented by
language, but we have no way to individuate without the concepts we use. Of
course, the concepts we actually do employ may be only a fraction of possible
concepts. But given any other possible conceptual grouping, there already are (or
are not) objects "falling under" those concepts (to use a Fregean phrase). The
point is that different ways of individuating are dependent on what conceptual
resources are available. The way we sort the universe depends on the conceptual
resources at hand.

There have been various criticisms of the structuralist philosophy of
mathematics, and perhaps the best way to elucidate the view is to begin with the
critics.

§ 2. The Doctrine of the Nonsensicality of Trans-structural Identity

In [1990] Charles Chihara attacks the claim that the only legitimate identity
statements are those in which the constants that flank the identity sign refer to
objects of the same structure. He calls this "the doctrine of the nonsensicality of
trans-structural identity". Chihara has two main criticisms of this doctrine.

---

12Crispin Wright makes a similar point this way in an unpublished
manuscript; I borrow the phrasing from that work.
The first criticism is directed toward Resnik's [1981], in which our thesis (5) is defended. Chihara claims that the formulation of the doctrine is, itself, incoherent. He says:

... [T]he doctrine of the nonsensicality of trans-structural identity asserts that objects from different structures cannot be said to be identical or not identical; but this very statement makes use of the notion of different structures -- structures that are not identical -- which, according to Resnik, is nonsense: structures cannot meaningfully be said to be not identical. 13

There are actually two distinct issues here which Chihara has run together: (i) The identity of positions in structures -- can positions in different structures meaningfully be said to be identical? (ii) The identity of structures themselves -- can structures meaningfully be said to be the same or different? (In all fairness, perhaps Resnik himself runs (i) and (ii) together; see Resnik [1981].) Chihara's objection only directly addresses (ii), so I will focus my discussion on that issue. 14

There is an ambiguity in the way 'structure' is used in the literature. Sometimes the word 'structure' is used in the sense of certain systems having a structure. For example, both the finite von Neumann ordinals and the Zermelo numerals have the structure of the natural numbers. But sometimes 'structure' is used to talk about a structure itself. The ambiguity arises due to the fact that we can describe a system comprised of particular objects which exemplifies a structure (i.e., a particular token of the structure type), and we can describe a structure (the type itself), independent of any system that might have that structure. Again, think of the political case. We can describe particular administrations which have the political


14It is worth noting that in [1988], Resnik takes back his denial of the cogency of identity claims involving structures themselves (see p. 411, footnote 16). However, he does not offer any way to reconcile this with the claim (to which he supposedly still adheres) that the only meaningful identity statements are those that assert an identity between places in one structure.
structure of the United States -- these administrations are tokens of the political structure type. But we can also describe the political structure itself, independent of any particular administration which exemplifies that structure. When we describe the political structure type, we ascend to a level at which all we talk about are the structural relationships between the offices; we describe the way the various political offices are related to one another vis à vis their role in the political process. We need not speak of any particular individuals who have held or currently hold the offices.

When we say that a system has the structure of the natural numbers, we must be speaking from a perspective relative to which we are able to distinguish between tokens of the appropriate type. If we are confined to the theory of arithmetic, our expressive ability is limited to the linguistic resources of that theory. Relative to the theory of arithmetic, we cannot say that a system has the structure of the natural numbers, for there is no way to individuate objects in ways which would allow us to speak of systems having the structure in question. Within the theory of arithmetic, the only objects we can refer to are the natural numbers. If we say that a system has or exemplifies the natural number structure, we must be using a richer language in which we can speak of objects other than natural numbers; i.e., objects that comprise the system which is a token of the natural number structure type. Within, say, set theory, we are able to discriminate between objects on the basis of their extraneous (from the point of view of arithmetic) properties. Relative to this context, it is cogent to say that the finite von Neumann ordinals have the structure of the natural numbers or exemplify this structure. This particular set-theoretic system is

15 Usually 'administration' is used to refer just to the executive branch. The concept thus does not completely capture the structure token in question because all three branches of government should be included. However, I can find no term that adequately describes the token.
comprised of objects which display (or instantiate) the structure of the natural numbers; the finite von Neumann ordinals are a token of the appropriate type.

In an infinite sequence that is a particular token of the natural number structure type, the objects in that sequence *play the role* of the natural numbers. For instance, in the Von Neumann system, \( \{ \phi, \{ \phi \} \} \) plays the role of two. But to say that \( \{ \phi, \{ \phi \} \} \) plays the role of 2 is to make a *predicative* claim of sorts. The set \( \{ \phi, \{ \phi \} \} \) is "playing the role" of two in the sense that in the von Neumann token, \( \{ \phi, \{ \phi \} \} \) occupies the two-office, and "... is two" is predicatable of it. Analogously, in a particular political system, individuals *play the role* of President, Supreme Court Justice, etc. For example, John F. Kennedy once played the role of President and Earl Warren once played the role of Supreme Court Justice. These claims are predicative in nature, just as is the claim that \( \{ \phi, \{ \phi \} \} \) is playing the role of 2. JFK played the role of President in the sense that in a particular administration, JFK occupied the President-office, and "... is President" was predicatable of him.

We are now in a position to return to Chihara's objection. His complaint was that the doctrine of the nonsensicality of trans-structural identity itself appears incoherent, since to even *state* the doctrine, we must use the phrase "different structures". It thus appears that we are violating our own doctrine in the very statement of it. However, the apparent violation is illusory. The phrase "different structures" does make sense, but only relative to a background language that can treat the structures themselves as objects.

---

16 One might ask how we are to distinguish statements of identity from statements of predication. Identity statements must allow for substitutivity and the relation must exhibit all of the logical features of identity: reflexivity, symmetry, and transitivity. For elaboration, see Chapter III.
Relative to the theory of arithmetic, we cannot speak of different natural
number structures, but this is because the language does not have the expressive
resources to refer to systems that have the appropriate structure. There is only
one natural number structure type, and that is the structure type characterized by the
theory of arithmetic. Once we expand our expressive resources and revert to a
background theory (e.g., set theory), phrases such as “same structure” and “different
structure” are applicable to tokens of the natural number structure type. But these
expressions do not assert or deny an identity between structure types. Rather, if we
use these phrases, we are saying that different tokens have the same structure or are
of the same structure type (two structure tokens have the same structure if and only
if they are isomorphic). These are predicative claims, not identity claims (unless, of
course, we allow ourselves to speak of the identity of properties; if we do so allow,
then we are saying that the structural properties of the tokens are identical, not the
structures themselves). Chihara’s objection misses the relativity of ontology: in
certain contexts, it is possible to speak of different natural number structures, even
though in other contexts it is not.

Chihara’s objection is more damaging to Resnik’s account of structuralism
than it is to the formulation defended here. Resnik favors a first-order formulation
of arithmetic in which numerous structures are characterized by the Peano axioms.
On the second-order formulation which I favor, however, all tokens of the natural
number structure are isomorphic. There is one structure common to all, and this
structure type is the natural number structure. On Resnik’s first-order formulation,
non-standard models of arithmetic are included in those systems that have the
structure of the natural numbers, so it is not the case that there is one structure

17 Relative to arithmetic, we can speak of different structures exemplified
within the natural number structure (e.g., the ten-structure).
common to all. Infinitely many structure types are the natural numbers. And Resnik forbids identity claims dealing with these different types. This is why (it seems to me) Chihara is so perplexed by the doctrine of the nonsensicality of trans-structural identity. The language of arithmetic does not pin down its subject matter on the first-order formulation in the same way it does given a second-order formulation.18

Chihara’s second objection has to do with Leibniz’s Law. I will quote from Chihara at some length:

Resnik’s views seem to lead to a rejection of a version of the principle that... if $X = Y$, then any open sentence true of $X$ is true of $Y$. For the number 2 -- an object in the natural number structure -- is supposed to be a genuine object. So also is the set whose only member is the unit set containing the empty set. Now I can say of this set that it has a member; but according to Resnik I cannot say this of the number 2. But if I cannot say this, then presumably it is not true that the number 2 has a member; for if it were true, then surely it would be correct to say it. Yet according to Resnik, we cannot conclude that the number is a different object from the set.

We cannot draw this conclusion because the two objects belong to different structures, and we cannot make coherent identity statements concerning objects from different structures. Chihara continues:

Schematically, we seem to have objects $X$ and $Y$ which are such that the open-sentence 'x has a member' is true of one but not the other; so by contrapositive of the above principle, it would seem to follow that $X$ is different from $Y$. Resnik, however, maintains that it makes no sense to say $X$ is different from $Y$. Surely all of this is not easy to accept. ([1990] p. 143.)

Again, Chihara misses the relativity of ontology. On Chihara’s view, statements like "\{\{\phi\}\} = 2" and "\neg(\{\{\phi\}\} = 2)" must be legitimate if we are to respect the highly intuitive principle of Leibniz’s Law. Although the structuralist
denies the legitimacy of such identity claims, the denial is not a violation of Leibniz’s Law, due to the relativity of ontology. The two purported identity statements above are illegitimate because the constants that flank the identity sign cannot both refer to objects, and we do not countenance identity claims between objects and non-objects. To see more clearly why this is so, suppose for a moment that statements like “\{\phi\} = 2” and “\neg(\{\phi\} = 2)” did make sense. Then it would have to be the case that \{\phi\} and 2 were both objects. If reference to 2 is objectual, the language one is using cannot have the discriminative ability to distinguish between objects based on things other than their arithmetical structural properties. From the perspective in which we refer to 2 as an object, we do not have the expressive resources to speak of sets with members. In fact, the open sentence ‘x has a member’ is not even expressible from this perspective.

From the perspective in which we can speak of the set whose only member is the unit set containing the empty set, reference to 2 would not be objectual reference. It makes sense to say “\{\phi\} is 2” from this perspective, but this is not an identity statement. Instead, it is a claim of office occupancy or predication. Relative to this perspective, ‘2’ would not refer to the object 2, because 2 would not be an object from this perspective but rather an office that other objects can “fill”.

The doctrine of the nonsensicality of trans-structural identity is not a claim meant to stand on its own. It actually follows from the relativity of ontology. Since reference to 2 is objectual reference relative to the theory of arithmetic, but “twohood” is merely a kind of complex predication from the point of view of set theory, it is easy to see why identity statements involving the objects of the two

\[19\] Of course, there may be a theory whose ontology consists of both numbers and sets, and from the perspective of this theory both constants would refer to objects and the identity statement would be legitimate. From the point of arithmetic and set theory, however, the claim is not legitimate for the reasons that follow.
theories are illegitimate. We do not countenance identity statements relating objects and properties. Furthermore, it is clear why the doctrine does not violate Leibniz's Law -- Leibniz's law applies only to objects.

§ 3. Dummett, Dedekind, and Abstraction

Michael Dummett is another critic of structuralism. In [1991a], he characterizes the philosophical doctrine of mathematical structuralism as follows:

... Mathematics in general is solely concerned with structures in the abstract sense, that is, with systems left no further specified than as exemplifying the structure in question.20

According to Dummett, there are two distinct versions of this doctrine, a "mystical" version and a "hardheaded" version. On the former, the structures in question are abstract,

distinguished by the fact that their elements have no non-structural properties. ([1991a], p. 295)

Dummett attributes this version of structuralism to Dedekind, but it is also the view that I am defending.

The hardheaded structuralist, as Dummett describes her, claims that mathematics is concerned with all systems with a given structure. Since the only properties of "mathematical objects" that are mathematically important are those that have to do with their relationships to other "mathematical objects", and these properties are had by all sequences of objects that display the mathematical structure in question, mathematics is "about" all or any of these sequences. The hardheaded structuralist denies the need to posit a single abstract structure type;

20 Dummett [1991a], p. 295. It is important to note that Dummett uses the word 'system' in the same sense in which it is used in this chapter.
mathematics need only talk about the various token systems. Dummett attributes this of version of structuralism to Paul Benacerraf.\textsuperscript{21}

The weakness of hardheaded structuralism, according to Dummett, is that if there actually are no systems of the appropriate type, then the subject of mathematics is "futile", since mathematical statements would then be, at best, vacuously true.\textsuperscript{22} One might think that the mystical structuralist avoids this weakness since she claims that e.g., arithmetic is concerned with a single abstract structure. But Dummett challenges the purported gain:

Dedekind [i.e., the mystical structuralist] shared with the hardheaded structuralist the need to maintain that we can find infinite systems of objects -- systems isomorphic to the natural numbers and others isomorphic to the real numbers -- in nature; and the thesis is questionable. (Dummett [1991a], p. 296.)

Why would the mystical structuralist need this assumption? To answer this question, I turn to the work of Richard Dedekind.

In [1901], Dedekind says:

If in the consideration of a simply infinite system $\mathbf{N}$ set in order by a transformation $\phi$ we entirely neglect the special character of the elements; simply retaining their distinguishability and taking into account only the relations to one another in which they are placed by the order-setting transformation $\phi$, then are these elements called natural numbers or ordinal numbers or simply numbers... With reference to this freeing the elements of every other content (abstraction) we are justified in calling numbers a free creation of the human mind.\textsuperscript{23}

\textsuperscript{21}In [1990], Parsons calls this view "eliminative structuralism". For detailed criticism of the position, see sections 2-6 of his paper.

\textsuperscript{22}See Parsons [1990] for a much stronger version of this criticism.

On Dedekind’s view, then, we start with a simply infinite system (i.e., a token of the natural number structure), ignore certain properties of its objects, and abstract out the natural numbers. The natural numbers are the places in the structure that results from this process of abstraction. The only properties they have are those that are structurally given: the relations to other places in the structure.

Dummett’s criticism (i.e., that the mystical structuralist is also committed to the questionable claim that we can find infinite systems of objects in nature) seems apt. Without actually existing infinite systems, we could never perform the process of abstraction necessary to obtain the natural numbers; if there were only finite systems of objects, then our process of abstraction would result in merely finite structures.

Dummett’s criticism, then, is damaging to Dedekind’s structuralist view. The criticism is aimed at Dedekind’s abstraction -- does the problem infect modern versions? To answer this, we must look at current views on the existence of

\[24\text{Dedekind has often been read (e.g., by Russell) as a hardheaded structuralist; i.e., he has been read as claiming that mathematics is concerned with all systems that have a given structure. In various places he speaks of the numbers, and he has been criticized for this, for on the hardheaded view, there will be no unique system which is the natural number structure. But if we read Dedekind as a mystical Structuralist (as Dummett suggests), then this criticism misses the point. From the mystical structuralist viewpoint, we can speak of the natural numbers, because the natural number structure (i.e., the type) is unique. Kitcher [1986] also reads Dedekind as a “mystical” structuralist. For a different interpretation of Dedekind, see McCarty [Unpublished Manuscript].}\]

\[25\text{Dedekind apparently shared the view that one could not obtain the natural number structure without an infinite number of objects, for he thought it necessary to prove the existence of a simply infinite system. His argument for this occurs in section 66 of [1901]. He claims that “the totality of things, which can be objects of my thought” is infinite: let } s \text{ be an object of my thought. Then } s(s) \text{ is the thought that } s \text{ can be an object of my thought, and } s(s(s)) \text{ is... But even if the proof were sound (which it is not), it seems odd to ground the claims of mathematics in a contingent fact of this sort.}\]

\[26\text{Not the abstraction process itself, but the system from which Dedekind abstracted.}\]
structures. Thus far, I have spoken very loosely about how structures are characterized. I have said two different types of things: (i) A coherent second-order theory characterizes a structure; and (ii) All tokens of the natural number structure (or real number structure, or etc.) have the same structure. We can speak of this abstract structure type itself by ascending to the level at which all we notice about the tokens are the structural relations among the elements of the tokens. We need to see in more detail what is going on in (i) and (ii). As I will show, ultimately, (i) and (ii) may merely amount to different descriptions of the same sort of process.

As (i) above (and thesis (3) earlier) indicates, a structure is characterized by a coherent, categorical second-order language or theory. The axioms of a coherent theory characterize a structure and the expressive resources of the theory -- the predicates and relations internal to each respective language -- tell us how the objects of the theory are arranged and related to one another. Using arithmetic as the prototype, we can see in more detail how the axioms and expressive resources of the language of (second-order) arithmetic characterize the natural number structure by specifying the objects (numbers) and the pertinent relationships between them.

The language of arithmetic contains certain names (‘1’, ‘2’, ‘3’,…), and predicates and relations (‘successor’, ‘less than’, ‘plus’, etc.). The names pick out the objects of arithmetic (numbers). Peano’s postulates (stated in the metalanguage) give an explicit definition of the structure of the natural numbers by giving us a distinguished element (0) and a successor function for generating the rest of the structure:

(1) 0 is a number.
(2) If n is a number, then the successor of n (s(n)) is a number.
(3) If s(n) = s(m), then n = m.
(4) 0 is not the successor of any number.
(5) Any property which belongs to 0, and also to the successor of any number which has the property, belongs to all numbers.

(1), (2), (3), and (4), characterize an infinite progression in which each element is a number. (1) and (4) give us the unique element with which we begin. (5) is the induction axiom. The natural number structure is thus characterized, and we define the relations of addition, multiplication, etc. on this structure. For example, we can define the addition function recursively as follows:

(a) \( x + 0 = x \)
(b) \( s(x) + y = s(x + y) \)

There are a number of important things to notice at this point. First of all, notice that the language described above does not have the expressive resources to refer to, say, the square root of 2. This is because the square root of 2 is not a part of the natural number structure, and the only objects that arithmetic recognizes are those that are places in the structure of the natural numbers. Secondly, notice that (5) has variables ranging over properties, so the language is (at least) second-order. Second-order Peano arithmetic is categorical. Thus, all possible interpretations of the theory will be structurally isomorphic: there will be one structure common to all, and this is the natural number structure.

Earlier, I noted that I am not advocating linguistic idealism. We don't invent objects through our use of language; whether or not an object exists is independent of our conceptual resources. There would still be numbers even if there were no Peano arithmetic; it's just that we would not divide the world in such a way that we recognized numbers as objects. Conversely, if we do employ the conceptual resources of Peano arithmetic, then we have a way to divide the world which gives us the natural number structure. But as I have just described how we characterize
the natural number structure, it looks like we are merely inventing the natural numbers. We used the axioms of arithmetic to characterize the natural number structure, but what guarantees that the relevant objects exist? If there is a physical token of the natural number structure type, then Peano's axioms are satisfied. But what if there isn't? It seems that in such a situation, either (a) we have merely defined an infinite structure into existence; or (b) arithmetic really is "futile". It looks like Dummett's challenge has come back to haunt us, for, at least thus far, it looks like we need an infinite number of objects in the world to satisfy Peano arithmetic.

If we are confined to dividing up the purely physical world with our conceptual machinery, then structuralism may require an infinite number of purely physical objects to satisfy the Peano axioms. But need we limit ourselves to purely physical objects? Of course not. Structuralism is a version of mathematical realism; we have no qualms about abstract objects. Peano arithmetic is satisfied in set theory, if nowhere else. There are numerous tokens of the natural number structure in the set theoretic hierarchy.

So we can find models of Peano arithmetic in the universe of set theory. We know that tokens of the relevant structures exist because they are part of the set theoretic hierarchy. Now, if we ignore the set theoretic properties of the token structures, then we have the natural number structure type. And this leads us to the other description of characterizing a structure.

27 What, exactly, "realism" amounts to is discussed in some detail in the next chapter.

28 On the structuralist view, the set theoretical hierarchy is itself a structure, and the relativity of ontology applies to sets. See below.
All tokens of the natural number structure are isomorphic so they are structurally indiscernible; i.e., if we concentrate only on the structural features of the tokens, then there is no way to tell them apart.\(^{29}\) To obtain the natural number structure, we want to ascend to a level at which all we notice about these systems is their structure. Loosely, we want something like the Dedekindian method of abstraction; we want to abstract away from the "special character of the elements" and concentrate only on the structure of the tokens. But how is this done?

We want to recharacterize or reconceptualize\(^{30}\) the relevant set theoretic objects. Since our concepts play such a dominant role in our division practices, the goal is to do this through language. Although not all possible tokens of the natural number structure are set theoretic (since e.g., there might be a physical token of the natural number structure), it is easiest to see how this is done via set theory. We want to begin with the full background set theoretic language, alter it in such a way that it only pertains to the interrelationships between the sets, and end up with the language of arithmetic and the natural number structure. We want to rid the language of predicates applicable specifically to sets and confine ourselves to the expressive resources of arithmetic. However, this method is not as straightforward as it may appear. In set theory, arithmetical relations (e.g., addition, multiplication) are defined in terms of the membership relation. If we merely dispense with predicates pertaining to the sets themselves, the defined relations may no longer be expressible. But there is a way around this problem.

We want to alter the language of set theory in such a way that we rid the language of predicates pertaining specifically to sets, yet we want to make sure that

\(^{29}\)Recall that we are working with a second-order language.

\(^{30}\)I borrow this phrase from Crispin Wright, who uses it in an unpublished manuscript.
the arithmetical relations are expressible. As an intuitive picture of how we alter the
language in this way, we want to expand the set theoretic language by explicitly
defining arithmetical relations. In essence, we will be translating the language of
arithmetic into the language of set theory. We will be creating a sub-language (of
the language of set theory) which contains all and only those arithmetical formulas
that result from the translation. Call this sub-language \( A \). Then, if we focus only on
\( A \), we will obtain the language of arithmetic applied to the ontology of set theory.

Suppose we begin with the finite von Neumann ordinals, and let the language
contain a symbol for the membership relation ('\( \epsilon \)'), union ('\( \cup \)'), the empty set ('\( \phi \)'),
and set forming operators ('\( \{ \) and '\( \} \)'). Define 0 as \( \phi \). The goal is to define
'successor' and the addition function.\(^{31}\) The successor of \( x \) can be defined as
follows:

\[
(1) \quad s(x) = \forall n \cdot x \cup \{x\}
\]

Recall that earlier I defined addition recursively by the formulas

\[
\begin{align*}
(a) & \quad x + 0 = x \\
(b) & \quad x + s(y) = s(x + y)
\end{align*}
\]

The goal is to turn these into set-theoretic formulas that express the relation of
addition on sets. We are going to characterize the extension of "y is the sum of x and
z". First, define a set \( v \) to be hereditary if:\(^{32}\)

\[
\begin{align*}
(2) & \quad (x)(z) (x \in w \rightarrow <x, \phi, x> \in v) \\
(3) & \quad (x)(y)(<x, y, z> \in v \rightarrow <x, s(y), s(z)> \in v)
\end{align*}
\]

\(^{31}\) All other arithmetical relations and functions can be defined using these
notions.

\(^{32}\) Ordered pairs are defined set-theoretically as follows: \(<x, y> =
\{x, \{x, y\}\} \).
The ordered triples of sets that correspond to sums will be in every hereditary set, and moreover they will be the only such sets. The following formula expresses this fact (where ‘H(v)’ abbreviates “v is hereditary”):

\[ \Sigma(p, q, r) \equiv (\forall v)(H(v) \rightarrow <p, q, r> \in v) \]

\( \Sigma \) expresses a three place relation that holds only among those sets that correspond to sums. (Multiplication can be defined in a similar way.)

Let the sub-language \( A \) contain all and only those set-theoretical formulas that are the translations of arithmetic formulas. We started with a system -- the finite von Neumann ordinals -- that has the structure of the natural numbers. \( A \) will contain, then, formulas expressing the addition relation on sets in this system. If we focus only on \( A \), we have the expressive resources to speak of the successor relation and the arithmetical relation of addition. So we have, in essence, the language of arithmetic.\(^{33}\) But what if we start with the Zermelo numerals? Define 0, as before, as \( \langle t \rangle \). Define the successor of \( x \) as follows:

\[ (1') \quad s(x) = _z \{x\} \]

Then define addition in the same way sketched above. The strategy is exactly the same, so we will get a sub-language “isomorphic” to \( A \) no matter which system we begin with. The only difference is the ontology of the original system, and thus the sets over which the variables range.

The finite von Neumann ordinals and the Zermelo numerals are both tokens of the natural number structure type. Once we have expanded the language and focused on the sub-language \( A \), we no longer have any discriminative apparatus to say things about sets simpliciter; we thus are not able to “see” the Zermelo \( \{\{\phi\}\} \)

\(^{33}\)As noted earlier, the usual arithmetical relations can be defined using these notions.
independently of its place in the system, analogously for the von Neumann \{\phi, \{\phi\}\} relative to its system. Relative to the language of arithmetic, the two sets are the same object, since any properties the sets may have over and above their relationships to other sets are irrelevant and thus not considered when we move from the system to the structure (i.e., from the token to the type) and not expressible when we focus on A. One cannot express the difference between \{\{\phi\}\} in the Zermelo numerals and \{\phi, \{\phi\}\} in the finite von Neumann ordinals because we have no way to talk about the set-theoretic differences between the two. So we have a kind of Dedekindian abstraction from tokens of the natural number structure to the type. The abstraction process focuses on the language involved; it thus preserves the insight that our division practices are dependent on our conceptual resources.

Earlier I noted that the two methods of characterizing a structure may be merely different descriptions of the same sort of process. The first method characterizes a structure from within a particular theory, but to see if the theory has a model we have to revert to a background theory with a more encompassing language. The second method begins outside the theory with the more encompassing language itself and then focuses on the diminished theory and its expressive resources. In both cases, then, we need the background theory, and in both cases the conceptual resources of the diminished theory characterize the structure. The methods differ only in their perspectives: with the first method, the structure is characterized from within the theory itself, whereas with the second method, the structure is characterized from outside the theory.

It will be helpful to pause here to summarize. Dummett's objection centered on Dedekind's need of an infinite system of objects in nature. The modern structuralist can respond by saying that even if we use a kind of Dedekindian abstraction to characterize a structure, set theory provides us with the infinite system
of objects from which we abstract. So it seems that Dummett's challenge has been met -- we have a simply infinite system and we do not require that it occur "in nature". But, one might object, what about the set theoretic hierarchy itself? On the structuralist view, the set theoretic hierarchy is itself a structure and it is characterized in much the same way as the natural number structure. Relative to set theory, sets are objects; relative to a more encompassing theory, expressions referring to sets may function as complex predications. Of course, ultimately we do have to allow an (non-denumerably) infinite system of objects into our ontology. But, again, all of these objects are mathematical; we do not have to assume an infinite number of objects "in nature", as Dummett claimed.

It is unfortunate that Dummett confines his discussion of structuralism to the views of Benacerraf and Dedekind. The intuition behind the structuralist philosophy of mathematics is shared by Benacerraf, but his view is ultimately not structuralist, at least not as I have expounded the view. Dedekind's view is the root of the modern view, but the need to prove the existence of a simply infinite system in nature is no longer acknowledged. It is doubly unfortunate that Dummett concentrates his criticisms on this particular aspect of Dedekind's philosophy of mathematics.

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34That is, on at least some structuralist views. Parsons [1990] is an exception. See Hellman [1989] Chapter 3 for a detailed account of viewing set theory structurally.

35Actually, this is a bit problematic in the case of set theory. With other mathematical theories, we always have a "more encompassing theory" in which we can find a model: set theory. But what is the relevant more encompassing theory for set theory itself? Where do we go to see if set theory is satisfiable?

36But then what is the gain over traditional Platonism? The structuralist has ready replies to critics on the issues of knowledge, reference, and concept acquisition -- this is the topic of the next chapter. Furthermore, we have something interesting to say about what it is to be a mathematical object, as this chapter has hopefully shown.
§ 4. Abstract Objects and The Third Man Argument

The Third Man Argument (hereafter TMA) is a famous problem for Plato's theory of Forms (Plato himself discusses the problem in the *Parmenides*). In [1993], Michael Hand accuses structuralists of being susceptible to the same problem. Briefly, Plato's TMA generates either an infinite regress of Forms or consists of a *reductio ad absurdum* of the theory of Forms. The TMA is based on the following three premises:37

One Over Many (OOM): If a number of things, a, b, c, are all F, there must be a Form, the F-itself, in virtue of which a, b, c, are F.

Self Predication (SP): Any Form can be predicated of itself. The F-itself is F.

Non-Identity (NI): If x is F, x cannot be identical with the F-itself.

Since the F-itself is F (by SP), we can form a new plurality: a, b, c, and the F-itself. Since each member of the new plurality is F, and by NI none of them can be identical with the form in virtue of which they are F, we need a new Form -- the F-itself*. Insert this plurality into the One Over Many premise, and we get:

One Over Many II: If a, b, c, and the F-itself are all F, there must be another Form, the F-itself*, in virtue of which a, b, c, and the F-itself are F.

But now we can form yet another plurality: a, b, c, the F-itself, and the F-itself*. And so on and so on, *ad infinitum*.

There are a few ways to get around the infinite regress. One option is to strengthen OOM. We could insert "one and only one" in the premise:

One Over Many III: If a number of things, a, b, c, are all F, there must be one and only one Form, the F-itself, in virtue of which a, b, c, are F.

37See, e.g., Vlastos [1969].
But OOMIII, SP, and NI are inconsistent. One cannot hold all of them simultaneously; to do so entails a contradiction and thus gives us a reductio ad absurdum of the theory of Forms.

Another option is to give up NI. Colin Strang has suggested this move.\(^8\) The F-itself is "one of the many"; it is F because it participates in itself, and nothing else is needed. But, as Strang points out, this move does not help us in our explanatory enterprise -- it does not explain why the F-itself is an F. So Plato ends up with either (a) an infinite regress of Forms, (b) a reductio ad absurdum of his theory, or (c) an explanatorily idle account of self-predication.

So how does the above discussion apply to structuralism? Hand suggests that the structuralist is committed to OOM, SP, and NI with regard to structures, and thus is threatened by a TMA. Is Hand correct? Is the structuralist philosophy of mathematics actually susceptible to the TMA? Clearly we accept Self Predication -- the natural number structure type is an w-sequence. It is questionable whether we accept Non-Identity, but (as noted earlier) even without NI there is an apparent problem. The important premise, and the one Hand assumes we must accept is One Over Many. Hand says:

All concrete w-sequences [i.e., the concrete tokens, if there be any] have something in common, in virtue of which they are all w-sequences. What they share is their structure, and this common structure is what makes them all w-sequences. Translating this into structuralist lingo, we obtain the following: the concrete w-sequences resemble each other in that they all instantiate the abstract w-sequence, and this is what makes them w-sequences. (Hand [1993], p. 190.)

Hand assumes that the structuralist must say that what "makes" a concrete w-sequence an w-sequence is that it participates in the abstract w-sequence. Given our

\(^8\) The relevant portions of his paper are discussed in Hand [1993].
commitment to structure types, according to Hand any other answer violates our obligation to give a symmetric explanatory answer to the question ‘Why is this collection an w-sequence?’ This is dubious, and I will return to it momentarily. Hand continues:

But to instantiate the abstract w-sequence is to resemble it in the relevant respect, and here the relevant respect is being an w-sequence: the abstract w-sequence is an w-sequence, too. So the concrete w-sequences together with the abstract one constitute another Many, hence by [OOM], there is a One over them. By [SP], it too is an w-sequence. But by [NI], it is not one of this Many. Hence there is a third Many, with a One over them. And so on. (Hand [1993], p. 190.)

So apparently we are involved in a TMA. Either we have an infinite regress of structures, or (with a strengthened OOM) a reductio ad absurdum of structuralism. There is, of course, the third option discussed above. We can explain the fact that concrete w-sequences are w-sequences by the fact that they “participate” in the abstract w-sequence and deny NI (as Strang suggested Plato could do). The abstract w-sequence is an w-sequence because it participates in itself. But then: “[W]e find that this appeal to self-predication is of no value in explaining the abstract w-sequence’s being an w-sequence...” (Hand [1993], p. 191.)

I am skeptical that any account of “participation” can adequately explain why a particular has a property, but that is a topic for another dissertation. The important question is whether a structuralist needs to claim that the natural number structure type (for example) plays the sort of explanatory role Hand seems to think it must. Is a structuralist committed to the claim that “participation in” the type in question explains why tokens of that type are of that type? Certainly not. Tokens are of the appropriate type because they have a distinguished initial element and a successor function; these are the only explanatory notions needed. Hand assumes that we need the abstract type to explain what “makes” tokens of that type be of that type. Of course, we may say things like “This is an w-sequence because it has the
right structure" (and perhaps we thus invite Hand's criticisms), but the 'because' is not doing the same sort of explanatory work as in Platonic explanations. We are not citing the sameness of structure as an explanation of why the w-sequence is what it is. Rather, the w-sequence is an w-sequence because it satisfies the axioms of Peano arithmetic.

Hand does consider the possibility of this kind of response, but he rejects it immediately. He claims that if we can explain why tokens of w-sequences are w-sequences merely by the fact that they satisfy the definition of an w-sequence, then "introduction of the abstract w-sequence is unmotivated," and we ought to embrace a "quasi-nominalistic" structuralism.\footnote{Hand [1993], p. 191. Hand's quasi-nominalistic structuralism is basically what Parsons calls "eliminative structuralism" and Dummett calls "hardheaded structuralism".} If it were the case that we introduced structure types merely for explanatory purposes of the above kind, then their introduction would be unmotivated. But this is not why they are introduced. Instead, structure types are introduced because they are what structure tokens have in common: there is a type because the tokens resemble each other in the relevant respect. On the structuralist view, Platonists have it backwards: it is not that tokens resemble one another because they "participate" in the type, but rather that they are of the same type because they resemble one another.\footnote{I thank Robert Kraut for this clear statement of what is wrong with traditional views of universals.} Structure types are not the traditional universals; that is, they are not intended to do all of the work of universals as traditionally conceived.

What I hope to have shown thus far is that structuralists do not accept OOM; i.e., they reject the claim that structure tokens are of the appropriate type because they participate in some abstract structure type. But then we are not involved in a...
TMA, and Hand's argument against us is benign. Hand, however, anticipates this kind of response and dismisses it as unsatisfactory. He says:

> The motivation behind structuralism has nothing to do with the possible explanatory function of abstract patterns, at least as far as this sort of explanation is concerned. Nonetheless, the structuralist is committed to more than one might think. After all, abstract patterns, structures, are not entities newly posited by the structuralist to serve as the realm of mathematical reference. Instead, the structuralist is making use of things we already know something about, and that we already put to use metaphysically in various ways. Since this is so, she is responsible to what we already know about structures... and, more importantly for my purposes, she is responsible to the metaphysical uses to which we already put them. (Hand [1993], p. 188.)

The gist of this, I take it, is that since we are making use of abstract types, we have to use these types for the purposes for which they have been traditionally used. Surely this is not easy to accept. If we believe that there is a type "red" -- i.e., that all tokens of red have something in common -- must we admit the traditional universal Redness, with all of its traditional uses, into our ontology? Of course not. We can consistently hold that all tokens of red have something in common without thinking that they participate in some universal that explains why they are red.

> Furthermore, structuralists do have an explanatorily adequate answer to the question 'Why is this collection an w-sequence?' -- it is an w-sequence because it has a unique initial element, etc. We do not need to appeal to some mysterious relation of participation to explain why an w-sequence is an w-sequence.

It seems, then, that Hand's criticisms are unfounded. The structuralist is neither susceptible to an infinite regress of structures nor subject to a reductio ad absurdum.
§ 5. Conclusion

In this chapter I have discussed the structuralist philosophy of mathematics and objections to it. But there are several other objections to structuralism, springing from the fact that it is a version of mathematical realism (e.g., objections concerning knowledge, reference, and concept acquisition). In the next chapter, I turn to these further objections.
CHAPTER II

KNOWLEDGE, REFERENCE, AND ACQUISITION

A current controversy in virtually every area of philosophy is that between realists and anti-realists. In the philosophy of mathematics, structuralists fall on the realist side of this debate. But what, exactly, is at issue?

§ 1. Realism and Anti-realism

There are at least three distinct characterizations of realism in the literature.

(1) Realism is often cast as an ontological thesis about the existence of a certain type of entity. In mathematics, the realist claims that numbers, sets, functions, etc. exist. Call this view ontological realism, or OR. (2) Realism can be expressed as a semantic thesis about the denotation of general terms and/or names. The realist claims that these expressions genuinely refer. Call this view referential realism, or RR. (3) Realism is also expressed as a semantic thesis about statements. The claim

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1Irrealists, non-factualists, quasi-realists, instrumentalists, etc. Mathematical realism is often called "platonism". I will avoid this term because I think the term itself carries unwanted baggage. I wish to separate structuralism from any association with a "Platonic realm of Forms" or a Platonist epistemology.

2A realist need not claim that all of these types of things exist. Penelope Maddy, for instance, thinks that the ontology of mathematics is comprised only of sets. See Maddy [1990].
here is that statements in the particular area of discourse in question are non-vacuously and determinately true or false independent of human inquiry. Call this semantic realism, or SR.

Intuitively, there are close connections between ontological, referential, and semantic realism. For example: it seems that if there are mathematical entities -- as OR claims -- then the singular terms in mathematical discourse refer to those entities. Furthermore, it seems that if mathematical terms are genuinely referential, then there must be mathematical objects. However, both of these inferences are suspect. Perhaps mathematical entities exist, but the singular terms of mathematics do not refer to them. A strict causal theory of reference might disallow referential relations between the constants of mathematics and mathematical entities. On the other hand, perhaps there are no mathematical objects yet mathematical terms are genuinely referential. Perhaps, for example, mathematical constants refer to properties, not objects. The point is that there is no entailment relation between OR and RR.

The connections between SR and the other two characterizations are also controversial. It initially seems that if mathematical statements are determinately true or false (and some are non-vacuously true) then there must be objects denoted by the singular terms in mathematics; that's because there are existence statements in mathematics the truth of which seems to require that objects exist. At least two authors, however, have recently attempted to defend a position which holds SR but

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3 This does not seem like a very attractive position, and I know of no one who actually holds it. Nonetheless, this is a way of holding OR while denying RR.

4 This is a version of RR but not OR if one also holds the further claims that predicate reference is genuine reference and properties are not objects. Maddy holds this combination of views for at least some mathematical entities (see [1990]); see also Hodes [1984].
denies RR and OR. That is, they suggest that mathematical statements are
determinately true or false yet deny both that mathematical constants are genuinely
referential and that mathematical entities exist. These authors replace talk of
mathematical objects with talk of the possibility of mathematical objects, structures
or constructions. A modal “primitive” is introduced to reduce ontology. We are not
to take the modal operator as explicable in terms of possible worlds, possibilia, or
model theory; in fact, quantification over variables within the scope of the modal
operator does not imply existence. The authors thus deny both that mathematical
terms are referential -- i.e., they reject RR -- and that mathematical objects exist
(OR), yet they claim that mathematical assertions are literally and non-vacuously
true or false (SR).

It is also possible to hold OR and/or RR and yet deny SR. Neil Tennant
[1987] endorses a version of this position. On Tennant’s view, there are
mathematical objects and mathematical constants refer to those objects, but the
semantics is not bivalent. The truth conditions of mathematical statements are
spelled out in terms of proof conditions; truth cannot transcend proof. Thus there is
no guarantee that every mathematical statement has a truth value.

I will refer to the denial of any or all of the three forms of realism as anti-
realism. This may not be consistent with the literature, but for the purposes at hand
it will suffice. A structuralist adheres to OR, RR, and SR; that is, she believes:

(1) mathematical entities exist

(2) the singular terms of mathematical discourse are genuinely
    referential

(3) mathematical statements are non-vacuously and determinately
    true or false independent of human inquiry.

^See, e.g., Hellman [1989] and Chihara [1990].
That is to say, structuralism is a realist view in all three senses. Realism has initial plausibility and is supported by the Quine/Putnam indispensability argument. But it also has its foes. The focus of this chapter will be to examine the motivations for denying OR, RR, and SR. I contend that the anti-realist objections can be met.

§ 2. Anti-Realism: Why all the fuss?

Those who reject OR and/or RR usually do so because they wish to avoid commitment to seemingly occult abstract objects. Most attacks on mathematical realism attribute to the realist a naive picture of a realm of mathematical objects, similar to Plato’s Forms. The opponents often assume that realists must posit a strange faculty of mathematical intuition to explain how we can have beliefs about and knowledge of this realm of objects. In this section, I seek to dispel some of the worries about mathematical objects by showing that the naive picture is not the only alternative.

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6 Basically, the indispensability argument claims that mathematics is indispensable for science. If one accepts the truth of science, then one must accept the truth of mathematics. See Quine [1960] and [1981], and Putnam [1971]. In [1980] and [1989], Hartry Field disputes the claim that mathematics is indispensable for science on the grounds that science can be formulated in a purely nominalistic way. Field’s program, however, has been widely criticized and it is doubtful that it can be successfully sustained. See especially Shapiro [1983b] and [1984]. Field responds to Shapiro in [1989], but Hallett [1990] shows why the response is insufficient. For other criticisms of Field, see Maddy [1990], Chihara [1990], and most of the papers in Irvine [1990]. For a rare defense of Field, see Papineau [1990].

7 I will only address the motivations for denying SR when they connect with the denials of the other two versions of realism. Intuitionists (such as Dummett; see e.g., [1973]) typically have reasons for denying SR that spring from general considerations in a philosophical theory of meaning, and I will not discuss those issues here.

8 The classic endorsement of a faculty of mathematical intuition can be found in Gödel [1964]. For a modern defense, see Brown [1990]. Maddy [1990] also endorses mathematical intuition, but supplements this with the claim that we have perceptual knowledge of (at least some) sets.
Most opponents of OR consider the problem of knowledge to be the central challenge to realism. How is knowledge of mathematical objects and/or mathematical statements possible? The classic statement of the problem occurs in Benacerraf [1973], wherein Benacerraf argues that a realist cannot explain how we can have justified beliefs about mathematical objects. The argument, as Benacerraf formulates it, relies on a causal theory of knowledge. There are many versions of the causal theory, but the minimal claim is that for a belief to be justified, there must be an appropriate causal connection between the belief that p and the state of affairs that p. But mathematical objects and/or states of affairs are, on the standard view, without causal efficacy. It therefore seems that knowledge of mathematical objects and/or states of affairs is impossible, for none of our beliefs about them could be justified.

There is an enormous literature on the problem of mathematical knowledge. Many have tried to show that we do causally interact with mathematical objects themselves. Some have tried to show that if there is a problem it lies not with mathematical objects, but rather with the epistemology. E.g., Colin McGinn writes:

We give every appearance of having mathematical... knowledge, but a causal theory is inapplicable to such knowledge; so causation cannot be a necessary condition for the applicability of knowledge per se. (McGinn [1984], p. 539.)

And David Lewis writes:

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9See, e.g., Goldman [1965] and Swain [1972].

10See, e.g., Maddy [1990] and Tymoczko [1991].
Our knowledge of mathematics is ever so much more secure than our knowledge of the epistemology that seeks to cast doubt on mathematics.\(\text{1}\)

I will not address the problem as formulated above because the strict causal theory is no longer widely held. In the philosophy of mathematics, this is not widely acknowledged. Anti-realists continue to frame their objection in terms of the causal theory of knowledge and realists continue to mount their defense with the causal theory in the background. But if the causal theory is deficient generally, then it is no problem for the realist if he or she cannot show how we can come to know about mathematical objects in accordance with that theory. As I see it, the goal of the realist (OR) is to take the most plausible account of justification and show that on that account, we can have mathematical knowledge. If we look to current work in epistemology, we see that the prevailing theory of epistemic justification is reliabilism.\(\text{12}\) So if the realist can show that there are no epistemological problems concerning mathematical knowledge on a reliabilist theory, then all anti-realist challenges on this score will be met.

The most developed reliability theory is Alvin Goldman’s reliable process theory, according to which a belief is justified if it results from a reliable (cognitive)

\[\text{11}\text{Lewis } 1986\text{ p. 109. Lewis tries to show that the Benacerraf problem is merely a pseudo-problem. John Burgess also thinks that the problem is merely a pseudo-problem, but for different reasons. In } 1990,\text{ Burgess argues that for a naturalized epistemology, a premise such as: } "\text{Even if true, belief in an assertion or theory implying or presupposing that there are objects of some particular sort cannot be knowledge unless some objects of that sort act directly or indirectly on us" is questionable, due to the descriptive methodology of the project. The premise, Burgess claims, has no weight with the scientific community so naturalized epistemology should give it up.}\]

\[\text{12}\text{See especially Goldman } 1979\text{ and } 1986,\text{ McGinn } 1984,\text{ Armstrong } 1973\text{, and Nozik } 1981.\text{ All hold some form of reliability theory. See Pollock } 1986\text{ for criticisms. For detailed arguments against the strict causal theory, see Brown } 1990.\]
belief-forming process. Now, a belief-forming process, according to Goldman, is a certain cognitive functional procedure or operation; "something that generates a mapping from certain states -- 'inputs' -- into other states -- 'outputs'." (Goldman [1979], p. 11.) The outputs of belief-forming processes are states of believing. A belief is justified if and only if the process that caused it is a reliable process. According to Goldman, a process is reliable if and only if it tends to produce more true beliefs than false beliefs.

A few words of clarification are in order at this point. The justificatory status of a belief is solely a function of the reliability of the process by which it was formed. A reliabilist does not require the believer to know that the belief is justified. Furthermore, a reliabilist does not require that the believer be able to give some justification for the belief. The belief is justified as long as the functional operation in question tends to have true beliefs as outputs. We can assess processes for reliability, but when we are doing so, we are making meta-judgments about the justificational status of certain belief types -- i.e., those belief types that are outputs of the belief-forming process type in question. The cognizer qua believer, however, is not required to do this (nor even be able to do this) for his or her belief to be justified.

Causation still plays an important role for the reliabilist, but it is an explanatory role instead of a justificatory role. On a strict causal theory, the causal connection between the believer and the object of belief is itself a factor in the justification. On a reliability theory, the causal connection does not enter into the

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13 Goldman's full base clause is: If S's belief in p at t results from a belief-independent process that is reliable, then S's belief in p at t is justified. There is also a recursive clause which talks about conditional reliability: When the inputs themselves are beliefs, there are two conditions that must be met: (1) the process must be reliable and (2) the inputs must be justified.
justification per se -- the belief is justified if it was formed by a reliable process. But it is not enough to say merely *that* the belief-forming process is reliable; one must further explain *why* the process is reliable. And this is where causation usually comes into the picture. Causation plays an explanatory role in that it *explains why* the belief-forming process in question is reliable.

An illustration will be helpful. Reliability theorists typically take certain perceptual beliefs as their paradigm case. Let h be a perceptual belief held by some subject S. Then h is justified for S as long as perception is a reliable process. But now we have to explain *why* perception is a reliable process, and this is where causation comes into the picture. Perception is a reliable process because of a certain causal connection between the perceiver and what is perceived. So the causal connection itself doesn't *justify* the belief, but rather *explains why* the belief-forming process that results in h is reliable.

Benacerraf's challenge remains -- but in a different form -- for those who hold a reliability theory. The challenge is no longer to justify mathematical beliefs, but rather to explain why the belief-forming processes that result in mathematical beliefs are reliable. Hartry Field puts the problem as follows:

[The] challenge is to provide an account of the mechanisms that explain how our beliefs about these remote entities can so well reflect the facts about them. The idea is that if it *appears in principle impossible to explain this*, then that tends to *undermine* the belief in mathematical entities, *despite* whatever reasons we might have for believing in them. (Field [1989], p. 26.)

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14 Those perceptual beliefs that are formed by perception of something in close proximity and in the visual field for an appropriate amount of time.

15 There are different ways of spelling out what is meant by 'reliable'. In [1979], Goldman claims that a process is reliable if it produces more true beliefs than false ones. He modifies this slightly in [1986]; see below.
Field is doubtful that this challenge can be met, and takes this as a strong argument for his version of anti-realism.

The burden of proof is thus on the mathematical realist. If she can give a satisfactory explanation of why and how our mathematical beliefs are justified, then she will have met Field's challenge. The goal, given a reliabilist theory, is to identify (at least some of) the processes that give mathematical beliefs as outputs and show that they are reliable.

If one holds a structuralist view of mathematical objects, a satisfactory explanation of why and how our mathematical beliefs are justified is forthcoming. On the structuralist view, the subject matter of a mathematical theory is its structure; e.g., the subject matter of arithmetic is the natural number structure — the pattern common to any infinite system of elements with a distinguished initial element and a successor relation. Mathematical objects are positions or places in structures. What it is for something to be the number two, for example, is for it to be the appropriate place in the natural number structure.

A structure is a kind of pattern in that it consists of elements related to one another in a certain way; and patterns are types. Tokens of (at least finite) mathematical patterns exist in particular places of space-time. For example, each of the following sequences is an instance of the initial segment of the natural number structure:

(a) I II III IIII IIIII
(b) • • • • • • • • • • • •
(c) ► ► ► ► ► ► ► ► ► ► ► ►
(d) / / / / / / / / / / / / / /
(a), (b), (c), and (d) are physical tokens with which we are capable of causally interacting. We can see the pattern common to the four tokens.

What I will argue is that beliefs about mathematical pattern types are formed through causal interaction with tokens of the appropriate type. The highly general process of acquiring beliefs about pattern types from causal interaction with tokens of those types is not peculiar to mathematical beliefs. For example, we have beliefs about letter types and word types which arise from interacting with physical tokens of letters and words. Our belief that the word 'salt' has four letters, for example, is a belief about the word type in question. Once we have acquired beliefs about this particular word pattern type, we are able to recognize it when we come into contact with a new instance: upon opening a new book or magazine and encountering an as yet unfamiliar token of 'salt' we are able to recognize the pattern common to all tokens of the type and to classify the particular token in the appropriate way.

The general type of belief-forming process at work in cases of beliefs concerning simple mathematical patterns and word types is pattern recognition. Of course, this is a highly general specification of the process. There are many different kinds of patterns. For example, there are social-institutional patterns, musical patterns, and behavior patterns (just to name a few). The general process of pattern recognition could be cited as the process by which we form beliefs concerning each of these pattern types, but it is unlikely that a single, psychologically salient process accounts for beliefs about all of them.

Resnik [1975] and [1982] invokes pattern recognition as an account of knowledge of mathematical structures. He offers a speculative account of how this process works. However, Resnik does not cite any psychological evidence whatsoever to back up his account, and thus it is merely speculative psychology. I hope to offer more.

I thank Robert Kraut for pointing this out and offering the above examples.
Tokens of mathematical patterns such as (a) - (d) above are present in our visual field. Thus, for the purpose of discussing how we form beliefs about finite mathematical pattern types and recognize further instances of those types, I will restrict the discussion to those kinds of processes involved in producing beliefs about small finite patterns that we can perceive visually. For the purpose of assessing the reliability of the belief-forming processes that result in mathematical beliefs, this specification is still too broad, and I will turn to this momentarily. But there are three distinct issues that must be addressed with regard to beliefs about mathematical pattern types: (i) How do we acquire beliefs about small visual patterns? (ii) How do we recognize further instances of the same pattern type? and, the most important question, (iii) Are our beliefs about patterns justified?; i.e., is the process that results in beliefs about small mathematical patterns reliable? Each of these issues will be addressed in turn.

(i) How do we acquire beliefs about small visual patterns?

Much work has been done in psychology in the area of visual pattern acquisition and recognition. The process starts with sensory input. In certain circles, it is a truism that sensory input requires interpretation. A pattern appears in our visual field, but it is not recognized as a pattern until it is interpreted. Pattern acquisition occurs when the physical characteristics (i.e., the features of and

18Of course, blind people have mathematical knowledge, so visual patterns cannot be the only avenue to knowledge of mathematical structures. Presumably, however, there are analogs of the processes I will describe involving other sensory modalities (e.g., acquisition and recognition of acoustic patterns - when we hear one clap, then two claps, then three claps... we perceive a sequential auditory pattern that is a token of the initial segment of the natural numbers.) I thank Stewart Shapiro and Diana Raffman for this point.

relations between the elements of the pattern) are identified. So, for example, a
token of the letter 'E' is perceived. The vertical line, the three horizontal lines, and
the relations between those lines are identified, and an "internal representation" of
the E-pattern is formed based on these characteristics.20

Beliefs about (finite) mathematical structures are beliefs about pattern types.
We acquire beliefs about pattern types by identifying the important features of and
relations between the elements of empirical tokens of the type in question.21
Consider again (a) - (d):

(a) I II III IIIIII
(b) • • • •• ••• •••••
(c) ► ► ►►►►►►►►►►►
(d) / // /// //// //////

The patterns (a) - (d) have something in common; namely, the sequence or ordering
of the elements. Now, how do we recognize and encode this higher-level feature of
the pattern rather than, say, merely the shapes of the elements in each individual
pattern? According to current psychological research, the perceiving subject
classifies patterns differently depending on what he or she "selectively attends" to in
the pattern.22 If the subject selectively attends to the relations between and
ordering of the elements in the pattern, ignoring other irrelevant features, then,
ceteris paribus he or she will acquire (here, encode and store) the relevant
mathematical pattern. By selectively attending to the ordering of the elements in

20 Is this internal representation verbal or imagistic? There is much
controversy over this issue (see, e.g., Kosslyn [1978] and many of the papers in Block
[1981]), but for our purposes it does not matter.

21 I ignore here the problem of how we get from the perception to the belief.

mathematically structured stimuli, we identify the important feature of the patterns instantiated therein.

(ii) How do we recognize further instances of the pattern type?

Pattern recognition consists of classifying a stimulus as an instance of a particular pattern type. There are various psychological theories that attempt to explain how this is done. On one theory, pattern classification is a process of template matching. The claim is that we have internal representations of paradigms of patterns and when we encounter further pattern tokens, we attempt to match those tokens with various templates. According to another theory, the features of and relations between the elements of the pattern are stored in either a verbal or imagistic description of the pattern, and we compare subsequent pattern tokens to these descriptions. But what all theories have in common is the claim that when we perceive a token pattern, it is somehow “compared with” various stored patterns. If we selectively attend to the sequence or ordering of the elements of a mathematically structured pattern, then we ought to classify it appropriately.

At this point, it will be instructive to compare my view with that of another current realist. Penelope Maddy (in [1990]) argues that we acquire mathematical knowledge through perceptual processes. Maddy’s account is a response to Benacerraf’s problem couched in terms of the causal theory of knowledge. She makes use of a psychological theory of D. Hebb to claim that we actually perceive sets. It may seem that I am making a parallel claim; i.e., that we perceive structure types. But this is not my claim. On my view, we come to know about abstract

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23 See Reed [1973] and Corcoran [1971] for discussion.

24 Again, see Reed [1973]. This is the “feature analysis theory”. There are other theories as well.
pattern types by perceiving and classifying concrete pattern tokens. What distinguishes my view from Maddy's is that I do not claim that we actually interact with an abstract pattern type. But, as I will explain momentarily, such interaction is not needed for our beliefs about the pattern type to be justified.

(iii) Are our beliefs about mathematical patterns justified?

Are the processes by which we form beliefs about small, finite mathematical structures reliable? I have cited the general processes of pattern acquisition and recognition as the processes by which we form certain kinds of mathematical beliefs. Earlier, I narrowed the specification slightly to the process of forming beliefs about small finite patterns that are present in our visual field. But this is still too general for the purpose of assessing reliability. To see why, consider for a moment a different belief-type: the reliability theorist's paradigm case of perceptual beliefs. Although it may be that the way we form perceptual beliefs is not significantly different from case to case, we have to individuate belief-forming processes more finely to determine whether they are reliable, since proximity, lighting, etc. all play a role in the justificatory status of the belief. We must specify the input/output relation in more detail. So even though the way we form beliefs about pattern types may not differ significantly from case to case (i.e., we notice the features of and relations between the elements, etc.), this highly general specification of the process is not adequate for the discussion in this section.

So how do we individuate belief-forming processes for the purpose of assessing reliability? This is known in the literature as the "generality problem". We cannot "slice up" belief-forming processes too broadly, for (as the above example of perceptual beliefs is meant to show) probably only a sub-class of the beliefs formed by the general belief-forming process are justified. But if we
individuate too finely (so that e.g., the type in question is just the process involved in
the formation of one belief), then we get instant reliability (assuming the belief is
true). Goldman's response to this problem is to suggest that we individuate as finely
as possible:

The critical type [of belief-forming process] is the narrowest type that
is causally operative in producing the belief token in question.
(Goldman [1986], p. 50.)

To solve the "instant reliability" problem, Goldman suggests that we consider the
process type's propensity to produce true beliefs. So we take into account more than
those beliefs actually formed by the process when we evaluate the reliability of the
process in question; i.e., we consider how that process type would work in various
other situations.

How does this apply to the case of mathematical belief-forming processes?
What would be the narrowest type of process operative in the production of beliefs
about small finite mathematical structures? We might try to specify it as follows:
the process by which we pay selective attention to the ordering of elements in our
visual field. But this is probably still too broad, for the pattern of an initial segment
of the natural numbers can be tokened in various ways and formation of the
relevant beliefs thus might involve processes specifiable in distinct ways.
Furthermore, we are better at recognizing certain initial segments of the natural
number pattern than others (e.g., we are better at recognizing the 4-pattern than,
say, the 59-pattern or the 238,979-pattern), so how we specify the process will
significantly influence its degree of reliability. How, then, should we individuate the
relevant process(es)? I suggest that we take any mathematical pattern that we are
good at recognizing and isolate the process involved in forming beliefs about it. If
the process in question has the propensity to produce true beliefs, then the output
beliefs are justified. For most of us, there are undoubtedly such structures and
processes -- most of us are typically right when we make certain numerosity judgments. In fact, current psychological research confirms this. Studies have been done in which subjects were asked to report the numerosity of sequences or arrays of dots. The subjects were presented with stimulus cards for 100 milliseconds, and their judgments averaged 98% correct for arrays of four or less elements. The capacity to recognize small arrays is called "subitizing", and is distinct from estimating or counting. Mandler and Shebo describe subitizing as "the rapid, confident, and accurate report of the numerosity of arrays of elements presented for short durations." ([1982], p. 1) Thus, there is considerable psychological evidence that the process(es) by which we recognize certain small finite mathematical patterns is reliable. This is enough to justify at least some mathematical beliefs and get the project underway. As long as we can form justified beliefs about some initial segment of the natural number structure, the account of knowledge of mathematical objects can begin.

More will have to be said concerning beliefs about larger structures, and I will turn to this momentarily. However, recall that the objection we are considering concerns the causal inertness of mathematical objects. The objection centers on the fact that mathematical objects are outside of the causal nexus, and the conclusion is drawn that we cannot have knowledge of any mathematical objects. But if the process I described above does produce justified beliefs, then that conclusion is unwarranted.

My account of knowledge of certain mathematical structures begins with causal interaction with tokens of the type of structure in question. The process that

25 The original experiments were conducted by Woodworth and Schlosburg, and were recently confirmed by Mandler and Shebo (see [1982]; see also Gallistel and Gelman [1992] and Dehaene [1992]).
results in beliefs about certain small, finite mathematical patterns is reliable because its outputs are confirmed by epistemologically unproblematic empirical tokens with which we have a perceptual, and thus causal, connection. The account respects the causal constraints on explanation; so we have an account of how at least some of our mathematical beliefs are justified.

Let me pause briefly here to deflect an objection. One might think that my account merely by-passes Benacerraf's problem. The objection goes as follows: we cannot perceive the abstract pattern itself; when we perceive concrete tokens of mathematical patterns, we attain knowledge only of the individual tokens. Pattern recognition cannot help us to achieve knowledge of mathematical structures themselves. I have two replies to this objection. First of all, the objection relies on a certain view of the type/token distinction with which I am not entirely comfortable. On my view, the type just is an abstraction from the tokens. But even if one holds a more traditional view of the type/token distinction, the objection can be met. The objection itself implicitly presupposes the necessity of a causal connection between the object the belief is about and the believer. But on a reliabilist account of justification, we are not bound by such requirements. If pattern recognition is a reliable belief-forming process, then we have met the requirements of a reliabilist theory. In other words, we do not have to perceive the abstract pattern itself to have knowledge of it; as long as the process of acquiring knowledge of these types from interaction with their tokens is a reliable belief-forming process, then the output beliefs are justified. Benacerraf argued that the gap between cognizers and abstract mathematical objects prevents cognizers from attaining knowledge of those objects. My response is that the gap is only a problem on certain (implausible) views of
epistemic justification. Given a structuralist philosophy of mathematics and a reliabilist account of knowledge, Benacerraf's problem vanishes.26

Structuralism has an advantage over the type of naive platonism often criticized in the literature with regard to explaining the justification of our beliefs about mathematical objects. (The "naive platonist" is the mathematical realist most often criticized. It may be that no one actually holds this position, though many have attributed it to Gödel.) The naive platonist regards numbers as objects completely independent of the physical realm. It is difficult to explain how our mathematical beliefs are justified on this picture without positing some faculty of mathematical intuition, for how else could we come into cognitive contact with such objects? The structuralist rejects this naive view of mathematical objects. On the structuralist view mathematical objects are positions or places in structures, and many of these structures are tokened in epistemologically unproblematic empirical patterns. We can come into cognitive contact with these tokens, and this is enough to explain the justification of our beliefs concerning the structures themselves.

26 The above explanation of justification relies on the epistemologically unproblematic nature of certain tokens of pattern types. In [1983], Crispin Wright makes the same sort of move in his account of knowledge of abstract objects. Wright does not directly espouse the view defended above (he is not a structuralist and he does not invoke pattern recognition), but his account is very much like mine. Wright attempts to show how we can have knowledge of directions, and he claims that our knowledge of lines is sufficient to explain our knowledge of directions. The belief-forming process that results in beliefs about directions is wholly dependent upon our perception of lines. Due to the epistemologically unproblematic character of lines, and the fact that the truth of statements about lines verify the truth of statements about directions, the justification of our beliefs about directions is confirmed. See Wright ([1983], pp. 86 - 89). Wright's account of mathematical knowledge is somewhat different, for he ultimately defends a version of logicism. See [1983], Chapter 4 and [1990].
(iv) Onward and upward.

What I’ve said so far goes a long way toward explaining the reliability of our belief-forming processes that result in beliefs about mathematical objects. But it doesn’t go far enough. I have explained how we can have justified beliefs about very small mathematical structures. But what about larger mathematical structures like, say, the 59-structure? Infinite structures present an even bigger problem, for even if there are empirical instantiations of infinite structures, it is doubtful that we are capable of perceiving them as infinite. I suggest that the formation of beliefs about larger and even infinite structures is mediated by verbal descriptions.²⁷ We get the general idea of a “number pattern” from the small finite patterns discussed above. These patterns give us an understanding of how the elements of the pattern are related to one another. We then learn how to add an element to the pattern (i.e., how to continue the pattern in the same way) and eventually we understand that it can be continued indefinitely. Given a number pattern of any cardinality, we can always add one more element.²⁸ Such an explanation might give us an epistemic foothold on infinite structures. Furthermore, we can acquire beliefs about large and even infinite structures through an understanding of the axioms that characterize those structures. Of course, the axioms entail that the structure they characterize is infinite, so if we cannot grasp the concept of infinity, then we cannot fully grasp the axioms. But our beliefs in the axioms and our beliefs concerning infinite structures are interdependent. We come to understand infinity as our understanding of the axioms grows, and we understand the axioms more fully as we come to grasp the import of the axioms.

²⁷Stewart Shapiro suggest this move in [1989b]. He does not, however, address the question of the justificational status of the beliefs thus formed. I will turn to this question momentarily.

²⁸This, of course, invites Wittgensteinian rule-following problems.
The above account clearly involves many different belief-forming process types. How do we know that these processes are reliable, and consequently that the beliefs are justified? Applied mathematics gives us confirmation of the reliability of our mathematical belief-forming processes. Scientific exploration of the world involves the application of mathematical structures to physical reality; Stewart Shapiro writes:

- My account of the relationship between mathematics and science begins with the suggestion that the contents of the non-mathematical universe exhibits the underlying mathematical structures in their interrelations and interactions. In general, physical laws expressed in mathematical terms can be construed as proposals that a certain mathematically defined structure is exemplified in a particular area of physical reality. (Shapiro [1983a], p. 538.)

So discovering mathematical structures via scientific investigation can be seen as pattern recognition of a larger sort. Of course, the kind of pattern recognition discussed earlier is not at work here. Our recognition of small finite patterns is perceptual: we actually perceive tokens of the pattern. In the case of these larger patterns, we (probably) are incapable of perceiving the pattern as a mathematical pattern. However, we can see it as a kind of conceptual pattern recognition: the beliefs are still about patterns and relations between the elements of the pattern. Scientific investigation mediates in our recognition of these larger patterns or structures.

I am not suggesting that the way we actually form beliefs about larger mathematical structures is through scientific investigation of the world; rather, the suggestion is that science can be seen as providing confirmation of the reliability of certain mathematical belief-forming processes. We acquire the pattern through verbal descriptions and an understanding of the axioms, and we recognize these structures as they are tokened in the physical world through scientific investigation. The output beliefs of the mathematical belief-forming processes are confirmed via
observation, and this is enough of a causal connection to account for the reliability of the processes by which we form those beliefs.  

It might be thought that I have approached the problem from the wrong direction. I have explained the reliability of belief-forming processes that result in certain “middle-level” mathematical beliefs. I have suggested that the axioms of mathematics can aid us in forming beliefs about infinite structures. But what about our beliefs concerning the axioms themselves? Aren’t these the fundamental beliefs, and thus the ones that require justification? Furthermore, if we can account for beliefs in the axioms, then since the rest of mathematical statements can be deduced from said axioms, wouldn’t this solve the problem in one fell swoop?

Perhaps it would. But, as Bertrand Russell notes in [1907], we believe the axioms of various mathematical theories because of the consequences that follow from those axioms, not vice versa. The axioms are accepted and known inductively, due to their efficacy in leading to middle-level mathematical beliefs which are confirmed empirically. (Although Gödel thought the axioms “force themselves upon us as being true”, it seems clear that this is not the case.) So the account of knowledge of middle-level mathematical beliefs should come first. Once we have explained the reliability of the belief-forming processes that result in middle-level mathematical beliefs, we can go on to explain the justification of our beliefs in the axioms, for these latter beliefs are, in a sense, inferential. And since a significant

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29 But what about unapplied mathematics? In [1983a], Shapiro argues that the line between applied and pure mathematics is not sharp, and perhaps there is no line at all. Browder [1988] expands on this idea.

30 This is, of course, Quinean confirmation holism. Tymoczko [1991] also suggests this picture of our knowledge of the axioms of mathematics.

31 Gödel [1964], p. 484. There have been many criticisms of this claim, but to consider them here would lead us too far afield.
number of the consequences of the axioms are empirically confirmable, we have adequate justification for our beliefs concerning the axioms themselves.

As noted earlier, the structuralist has an advantage over the naive platonist with regard to knowledge of mathematical entities. On the naive view, mathematical objects exist in some abstract realm which is completely independent of the physical world. On the structuralist view, however, mathematical objects are positions or places in structures. We come into cognitive contact with empirical tokens of mathematical structures, through which we gain an understanding of the way mathematical objects are related to one another. This, in turn, suffices to explain the justification of our beliefs about small finite structures. Without this understanding, the explanation of the reliability of mathematical belief-forming processes discussed above could not even get off the ground.

§ 2.2. Reference

It is often thought that reference poses yet another problem for the mathematical realist. The challenge is usually made in conjunction with a causal theory of reference. For example, Penelope Maddy writes:

Running parallel to Benacerraf's epistemological dilemma, we have two premises -- the causal theory of reference and the abstractness, and hence causal inertness, of mathematical objects -- that lead to another unpalatable conclusion for the [realist]: We can't refer to mathematical objects. If mathematical reality is as the [realist] says it is, we are doomed not only to ignorance, but to silence as well. (Maddy [1990], pp. 40 - 1.)

Crispin Wright notes that if causal interaction is a requirement for successful reference, then:

\[\text{\footnotesize 32\textsuperscript{\textregistered} Another much discussed problem concerning the reference of mathematical constants is the so-called multiple reduction problem.}\]
There can be no names of abstract objects, and something other than a straightforward construal of the plethora of such "names" occurring in the language of pure mathematics will be mandatory. (Wright [1983], p. 91.)

A robust theory of reference such as the causal theory attempts to explain why and/or how terms in our language or idiolect refer to the things they actually refer to. It attempts to account for how terms initially come to refer and how we are subsequently able to use terms to refer. On the causal theory, a term acquires its reference during an initial baptism in which the name is spoken while pointing to the object. Those observing the ceremony are perceptually related to the object, and they thus acquire the ability to refer to the object via the name. A reference-preserving causal chain is initiated, and reference is passed on to other speakers through uses of the name. Subsequent uses of the name, if genuinely referential, must be traceable back to the initial baptism.33

We cannot point to the naive platonist's mathematical objects, since they supposedly exist in some abstract realm completely independent of the physical world. However, it is eminently plausible to claim that we can ostensively name types by pointing to tokens of the appropriate type. (How else could we name letter-types and word-types?) Since we can ostend finite tokens of mathematical patterns, perhaps we can establish reference to at least some mathematical objects in accordance with the causal theory of reference. However, the type of ceremonial dubbing described above is not possible for the majority of mathematical objects, for we can ostensively introduce only small finite patterns; we can neither point to e.g., 238,979 nor indicate any causal role which it plays. If the causal theory is right and it requires such interaction, then we could not have established the referential relation

33See, e.g., Kripke [1972], Donnellan [1972], and Devitt and Sterelny, [1987], p. 55ff.
between '238,979' and 238,979. So the causal chain leading to our use of '238,979' could never have begun, and it would not be possible to refer to 238,979. Hence, if the interaction requirement is legitimate, it seems that we have no reason to believe RR, and many of our motivations for believing SR and OR vanish.

The causal theory of reference has a wide following in philosophy. But does it really rule out reference to the majority of mathematical objects? The answer is "yes" only if causal interaction is necessary in every case where there are genuine referential relations. It will be worthwhile to look at positive arguments for the causal theory to see if they show causal interaction to be essential in every case. Is there an argument that demonstrates that we must causally interact with an object to refer to it?

Saul Kripke and Michael Devitt are the two major proponents of the causal theory of reference. Kripke introduced the causal theory in "Naming and Necessity" (1972) to avoid problems posed by the description theory, which was the prevailing theory of reference at the time.34 But Kripke's arguments do not necessitate that

34 Essentially, the proponents of the description theory held that the denotation of α (where α is a name) is some object x if and only if x is uniquely described by descriptions D_1,...,D_n. In order to successfully refer to x, one must associate D_1,...,D_n with x. (See, e.g., Russell [1918], Searle [1958], and Strawson [1959], pp. 183-6.)

Kripke gives three types of arguments against the description theory of reference: modal arguments, epistemological arguments, and semantic arguments.

(1) Modal arguments. If the description theory were right, then sentences such as

(a) x, if it exists uniquely, has P_1,...,P_n (where P_1,...,P_n are the properties associated with D_1,...,D_n)

(b) If anything uniquely has P_1,...,P_n then it is x

would be analytically or necessarily true. (I ignore here the Quinean problems about analyticity.) But intuitively this is not the case. The theory thus gets the modal truth conditions of the sentences wrong.
the original dubbing be done in the presence of the object -- Kripke allows us to fix
reference by description. When we fix reference by description, we describe the
object and name it. It is important to note that the description only comes into play
in the initial baptism. This is not the description theory revisited, for the description
does not become synonymous with the name (as the description theory claimed) but
merely fixes the initial reference. Once we have named the object, the reference
borrowing and causal ancestry of subsequent uses are traced back to the original
baptism.

Can we name 238,979 on this picture? At least so far, it seems that the
answer is “yes”. We can fix the reference of a term as long as we can uniquely
describe the object to which it will refer. Peano’s postulates explicitly define the
natural number structure (given a second-order language), and once we have
defined a structure, its places can be uniquely described. So the causal chain can
begin. But then all deference to the causal theory has been paid, it seems. Why,
then, is reference seen as a problem for the mathematical realist?

(2) Epistemological arguments. If the description theory were right, then
sentences such as (a) and (b) above would be knowable a priori. But, again, this is
intuitively wrong. The theory thus gives such sentences the wrong epistemic status.

(3) Semantic arguments. Kripke gives us several cases in which the
description theory seems to go astray. For example, we associate certain
descriptions with the name ‘Aristotle’. Suppose some Greek slave actually fulfilled
the descriptions. To say that we denote the slave when we say “Aristotle” seems
intuitively wrong; it seems, rather, that we denote Aristotle but we are confused
about what properties Aristotle had. Or suppose that no one actually fulfills the
descriptions. Then the descriptions denote nothing, but it seems that the name still
refers.

In place of the description theory, Kripke suggests the causal theory. The
causal theory of reference avoids the first two problems of the description theory
because it does not require the possession of associated descriptions to refer to the
object. It avoids the third problem because it is the causal ancestry of the use of the
term that determines reference, so whatever object in which the causal chain
culminates is the object to which the term refers. ‘Aristotle’ would denote Aristotle
even if all of the descriptions we associate with ‘Aristotle’ described someone else
(or no one), because Aristotle is the person to whom we trace back the causal chain.

See Donnellan [1972] for similar arguments.
Even though Kripke does not require that the object be present at the initial dubbing, other causal theorists do. Why? Devitt and Sterenly offer one argument to motivate the requirement:

... The causal theory promises an explanation of the ultimate links between language and the world. [Description theories] cannot explain the ultimate links; they leave reference internal to the language. We need an explanation of the external relation that the whole system of words bears to the world. (Devitt and Sterenly [1987], p. 60.)

External relations between words and the world are necessary, according to Devitt and Sterenly, because of Twin Earth cases. Imagine a planet very much like earth, in which each Earth person has a doppelganger who is molecule-for-molecule exactly like the Earthling. Many Twin-Earthlings speak Twin-English, which is syntactically just like English. But it cannot be semantically the same because the denotations of the names differ from Earth to Twin-Earth: when an Earthling uses a name, he or she refers to an object on Earth, but when a Twin-Earthling uses a name, he or she refers to an object on Twin-Earth. Devitt and Sterenly claim that the Twin-Earth case shows that any adequate account of the reference of a term must involve external relations:

... Reference, and hence meaning, does not depend solely on the association of some words with other words, for all those associations are the same in Twin English as in English. (Devitt and Sterenly [1987], p. 52.)

I will grant Devitt and Sterenly the claim that to solve Twin Earth cases, there must be external world-word relations. Does it follow that in every case where there are genuine referential relations, we must causally interact with the object to which we refer? Certainly not. Ex hypothesi, the languages are semantically different. The

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35See Putnam [1975], pp. 139 - 44. Putnam talks about kinds, not individuals. However, the argument has the same force and in the present context we need to speak of individuals.
meanings (or denotations) of words on Twin-Earth are different from the meanings (or denotations) of the (syntactically) identical words on Earth. But then it does not follow that reference can never be specified via description, for it is not merely the syntactic relations between words that allow us to fix reference by description, but also the semantic relations. When we specify denotations via description, we are relying on the meaning of the words involved. Although the Twin-Earth case may show that not all reference fixing can be done by description, it surely does not show that no referential relations can be established via description.

We wanted to know whether there was a compelling argument for the claim that to establish reference to an object, the object must stand in some causal relation to us when we name it. If we had found an argument of this sort, then we would have reason to doubt RR. But we have not found such an argument. It thus seems that reference is not obviously a problem for the mathematical realist.

§ 2.3. Acquisition

The final problem for realism in mathematics is Dummett's acquisition challenge:

No philosophical account of a concept can be considered adequate if it so represents our understanding of that concept as to be incompatible with, or make mysterious in some way, the possibility of our acquiring a grasp of it empirically.36

There are two possible interpretations of the acquisition challenge. On one reading, the challenge requires us to show that the concept in question can be acquired purely by empirical means. On a weaker reading, the challenge can be read as claiming that our acquisition of a concept requires some sort of empirical

36Wright [1983], p. 5. I choose Wright's formulation of Dummett's acquisition challenge instead of Dummett's own because Wright's is a clearer statement of the problem. But see Dummett [1978].
confrontation. (Although both readings are possible, the latter is most likely the version Dummett intends.) I will discuss each of these interpretations in turn.

The first reading of the acquisition challenge calls into question all concepts that cannot be acquired solely by empirical means. This reading assumes an empiricist principle which claims that one can understand a concept only when the concept is "made evident by" ostensive definition. The assumption is that we are able to grasp the sense of the sortal term 'person', say, because we are empirically confronted with individuals who fall under this sortal. The empirical confrontation supposedly "makes evident" what we are talking about. In the case of abstract sortal terms, however, no empirical confrontation with individuals that fall under the sortal in question is possible. So, the argument goes, we are incapable of grasping abstract sortal terms such as 'number'.

This empiricist line was attacked years ago by Wittgenstein. Ostensive definitions of sortal terms allow us to grasp the sortal terms in question only if we are already familiar with and capable of using an elaborate background of linguistic machinery. The sortal term 'person' cannot be explained merely by individuating instances that fall under the sortal and labelling them: pointing alone will not explain that the concept is sortal rather than attributive. We cannot "make evident" the difference between sortal and attributive terms via ostensive definition, nor can we distinguish between proper names and sortals. The recipient of the definition must have some understanding of "how to go on" and use the term if he or she is to be counted as someone who actually understands the definition, and it is unlikely that all of this understanding was acquired ostensively since the very distinctions at issue cannot be "made evident" by ostensive definition.

37Wittgenstein [1953]. See also Wright [1983] for discussion.
The first reading of the acquisition challenge is thus utterly implausible. No sortal term whatsoever can be acquired merely via ostension, for other mechanisms of understanding are presupposed in order to get the ostensive definitions of empirically demonstrable terms off the ground. If the acquisition challenge is genuinely a problem for mathematical realists, it cannot be this form of the challenge.

The second reading of the acquisition challenge claims that some sort of empirical confrontation is necessary to understand a concept. This reading is weaker than the first because it does not require that our acquisition of the concept be solely via empirical means. In other words, the second reading does not require that the concept be “made evident” by ostensive definition, but merely requires that it be possible to acquire the concept empirically. But even in this modified form, the challenge is questionable. Although it is a matter of controversy how we acquire and understand language, the above argument shows that it is doubtful that empirical confrontation is the only avenue. The background machinery necessary to understand the difference between sortal terms, attributive terms, proper names, etc., is presupposed in our understanding of any further concepts and it is questionable whether these concepts themselves could be acquired empirically. So even the weaker claim that some sort of empirical confrontation is necessary for the understanding of a concept is dubious. Nevertheless, I will set this issue aside. If we suppose that the second reading of the acquisition challenge is defensible, does it pose a problem for the structuralist?

On the second reading, the acquisition challenge claims that some sort of empirical confrontation is necessary to understand a concept. The concept need not be “made evident” by ostensive definition; rather, some sort of ostensive definition must be possible, assuming background in the proper use of language. But once we
have the requisite background conceptual machinery, we can acquire the concept ‘number’ empirically. We can grasp that ‘number’ is a sortal term, and we can point to observable, token finite mathematical structures. As illustrated earlier, tokens of the type in question are present in the physical world. So empirical confrontation with instances of the initial segment of the natural number structure are possible. Given that we can come into contact with tokens of part of the structure of the natural numbers, the acquisition challenge in its weaker form has been met.

§ 3. Conclusion

In this chapter I have discussed the major arguments against realism in mathematics. I have shown that knowledge of mathematical objects is tractable on the structuralist view, that we can account for reference to mathematical objects, and that the structuralist faces no threat from the empiricist acquisition challenge. Realism is initially plausible. It is supported by the indispensability argument. Furthermore, as James Robert Brown notes:

There is always one more virtue that any theory might have, a comparative virtue having to do with the relative merits of its rivals. Here is not the place to list the many failings of formalism and other types of conventionalism, nor the inadequacies of intuitionism and other forms of constructivism, nor the poverty of physicalism in any of its versions. Readers who find [realism] distasteful should simply recall that the alternatives are even less palatable. (Brown [1991], p. 99)
CHAPTER III

IS JULIUS CAESAR IDENTICAL TO 2?

In Chapter I, we considered the following questions: What are numbers? What is, say, the number 2? Is it a property? Is it some sort of object? If so, what kind of object is the number 2? In Die Grundlagen der Arithmetik, Gottlob Frege struggled with these questions. He eventually came to the conclusion that numbers are extensions, and for Frege, extensions are a kind of object.

Frege's overall project failed, for Russell's paradox follows directly from the Fregean theory of extensions.¹ But is there something salvageable in Frege's system? In the course of this chapter, I will argue that there is. But first we need to see in more detail why it failed. The chapter, then, will proceed as follows: In § 1, I discuss Frege's notion of objecthood. § 2 is devoted to Frege's arguments for the claim that numbers are objects. In § 3, I examine sections 55 - 86 of the Grundlagen, discussing why Frege concluded that numbers were a certain kind of extension. Finally, in § 4, I discuss how parts of Frege's project can be salvaged.

¹The paradox follows from Axiom V of Frege's formal system, which spells out the identity conditions for extensions.
§ 1. Objecthood

What is it to be an object? Frege thought a direct answer to this question was impossible. He says:

... [T]he question arises what it is that we are here calling an object. I regard a regular definition as impossible, since we have here something too simple to admit of logical analysis. (Frege [1891], p. 32.)

He did, however, give us hints. A related question is ‘When does an expression stand for an object?’ This question is treated more thoroughly in Frege’s work. In this section of the chapter, I will discuss Frege’s views on the issues of objecthood and objectual reference.

Frege’s [1892a] is devoted to distinguishing objects from concepts. Concepts are functions which take objects as arguments. In Fregean terminology, objects “fall under” concepts. An object, on the other hand, is something that can be taken as an argument for a function, but cannot, itself, have arguments. Objects are saturated or complete, whereas concepts (or functions) are unsaturated or incomplete. Concepts are in need of an object to “complete them”; the argument of a function goes together with the function to make up a complete whole; for the function by itself must be called incomplete, in need of supplementation, or 'unsaturated'. (Frege [1891] p. 24.)

The argument (object) itself, however, is

2 In the mathematical sense.

3 An object falls under a concept if the value for that object as argument is Truth. First-level concepts, however, can fall under second-level concepts. Frege thinks of this as a different relation, though, and to mark the distinction, he uses the words “falling within” instead of “falling under” when speaking of the relation between first-level concepts and second-level concepts.
Thus, for Frege, the saturated/unsaturated distinction appears to be ontological; it seems to have something to do with the way objects and concepts are. But Frege also claims that expressions that stand for objects (and concepts) are themselves saturated (unsaturated). In the same work as that from which the above quote was taken, Frege says:

Statements in general... can be imagined to be split up into two parts; one complete in itself, and the other in need of supplementation, or ‘unsaturated’. Thus, e.g., we split up the sentence ‘Caesar conquered Gaul’ into ‘Caesar’ and ‘conquered Gaul’. The second part is ‘unsaturated’--it contains an empty place; only when this place is filled up with a proper name, or with an expression that replaces a proper name, does a complete sense appear. (Frege [1891] p. 31.)

Here, the distinction is linguistic. So there appears to be a “mirroring” relationship between language and the world -- unsaturated expressions correspond to functions, which are themselves unsaturated; and saturated expressions correspond to objects.

Saturated (or complete) expressions include proper names, definite descriptions, personal and demonstrative pronouns, and whole declarative sentences. We may lump all of these together and call them “Fregean proper names”. Unsaturated (or incomplete) expressions include predicates, relations, and function expressions. Unsaturated expressions are usually made complete by inserting a saturated expression.\(^4\) For example, the predicate ‘... is red’ can be completed by the definite description ‘The book on the shelf’. The resulting sentence “The book on the shelf is red” is a complete expression.

\(^4\)Higher level unsaturated expressions are exceptions to this. For example, a second-level unsaturated expression is completed with a first-level unsaturated expression.
For Frege, there are three basic "symptoms" of an expression having objectual reference: (1) The first symptom concerns the notion of saturation discussed above. Expressions that stand for objects are Fregean proper names. I noted earlier that there is a mirroring relationship between language and the world: saturated expressions stand for saturated things. But is it the case that all and only saturated expressions stand for objects? In [1983], Crispin Wright answers yes, with a qualification. He says that if we

omit the classification of whole declarative sentences as a kind of Fregean proper name, then in the characterisation of objects as what proper names stand for, what we have is essentially, or so I propose, Frege's view at the time of writing the Grundlagen.5

The first symptom of an expression having objectual reference, then, is that it be a Fregean proper name.

(2) The second symptom concerns the definite article. When we are willing to apply the definite article to an expression, then, according to Frege, that is a signal that the expression refers to an object. The definite article signals objecthood because when we speak of the x, the use of 'the' indicates that there is something we are referring to.6

(3) The third symptom of an expression having objectual reference has to do with statements of identity in which the expression occurs. It is a necessary condition of an expression referring to an object that identity statements containing

5Wright [1983], p. 7. Later, of course, Frege thought that even whole declarative sentences referred to objects -- the true and the false.

6It is important to note that the use of the definite article is merely a signal. Since some languages may not contain such indicators, it cannot be a general condition. But in languages that do contain a definite article, it must be possible to refer to any object with a phrase using 'the'. (See Wright [1983], p. 54.) Of course, one does not say "The Gottlob Frege" or "The 6"; we may have to insert sortal expressions before proper names. The sentences then become grammatical: "The person Gottlob Frege" and "The number 6."
that expression can be given a sense, for identity is a relation that holds of objects. If we have significant identity statements in which a term flanks the identity sign, then that signals that the term stands for an object. That Frege held this thesis is evident throughout the Grundlagen.7

One of Frege’s concerns in [1892] is to discuss the difference between identity statements and predicative statements. If the statement is genuinely one of identity, then it must allow for substitution salva veritate; furthermore, the relation must have all of the logical features of identity; i.e., it must be reflexive, transitive, and symmetric. Frege says:

An equation is reversible; an object's falling under a concept is an irreversible relation. (Frege [1892] p. 44.)

The sentence “The morning star is Venus” is “reversible” (i.e., the relation referred to in the sentence is symmetric). It is equally true that “Venus is the morning star” because the ‘is’ in the original statement is the ‘is’ of identity. But “The book on the shelf is red”, where the ‘is’ is the ‘is’ of predication, is not reversible. In Fregean terms, “The book on the shelf is red” means that the object referred to by the Fregean proper name ‘the book on the shelf’ falls under the concept ‘... is red.’ This relation is not reversible, for nothing can fall under the book on the shelf -- objects cannot have arguments.

I have spoken of three different symptoms of an expression having objectual reference. But does Frege take these “symptoms” to be jointly sufficient for an expression to refer to an object? Or is it possible that an expression has all three characteristics yet fails to denote an object? In [1983], Crispin Wright argues that, for Frege, the aforementioned symptoms are in fact, together, sufficient for an

7See, e.g., sections 57 and 66.
expression to refer to an object;\(^8\) that it is not the case that an expression can display all of these symptoms and still fail to have objectual reference.\(^9\) Wright calls this the "syntactic priority thesis"; he says:

> The really fundamental aspect of Frege's notion of object and concept is that they are notions whose proper explanation proceeds \textit{through} linguistic notions. (Wright [1983], p. 13.)

If Wright is correct, then any expression that functions in (true) sentences as a singular term does stand for an object. Wright's argument for the syntactic priority thesis stems from his interpretation of Frege's context principle,\(^{10}\) discussion of which is best saved until after we have examined Frege's notion of mathematical objects.\(^{11}\)

\section*{§ 2. Mathematical Objects}

A central thesis of Frege's \textit{Grundlagen} is that numbers are objects. This claim is based on three types of considerations,\(^{12}\) which correspond to the symptoms

\(^8\)See Wright [1983], Chapters 1, 2, and 3.

\(^9\)That is, if it has a reference at all. 'The present King of France' has no denotation, so naturally it does not refer to an object. But if it \textit{did} refer, then it would refer to an object. The key to whether or not a term has a reference lies in the truth value of sentences in which said term occurs. See below for elaboration and explanation.

\(^{10}\)"Never to ask for the \textit{bedeutung} of a word in isolation, but only in the context of a proposition." \textit{Grundlagen}, Introduction p. x. There is some controversy over how 'bedeutung' is to be translated. Austin translates it as "meaning"; Dummett agrees with this translation (see [1991a]). Crispin Wright, however, argues that it should be translated as "reference", and in [1987], Hale supports Wright's translation. For our purposes, little will hang on the translation because Frege did not yet have a fully worked out theory of sense and reference at the time of writing the \textit{Grundlagen}. For discussion, see Dummett (op. cited).

\(^{11}\)It is worth noting that Thomas G. Ricketts concurs with Wright on the syntactic priority thesis, but he comes to this conclusion by different means. (See Ricketts [1986].)

\(^{12}\)These three considerations are summarized in section 57 of the \textit{Grundlagen}. 
of an expression having objectual reference, as discussed above. First of all, numerical expressions function as singular terms. In the statements “2 is an even number” and “2 is less than 4” ‘2’ functions as a singular term. Of course, sometimes numerical terms are used adjectivally; e.g.:

(a) Jupiter has four moons

But Frege claims that sentences such as (a) can always be recast so that the numerical expression is a singular term. Thus (a) becomes:

(b) The number of Jupiter’s moons is four.

Frege’s claim is that numerical expressions are Fregean proper names, as required by symptom (1) above. Although numerical expressions sometimes seem to function as adjectives, when the sentence in which they appear is properly recast it can be seen that they are actually saturated expressions.

Frege’s second consideration has to do with the definite article (symptom (2) above). He points out that we readily apply the definite article to number words and phrases that attribute number: we speak of the number one and the number twenty-seven, as well as the number of cards in a deck and the number of planets.

\[13\] The possibility of rephrasing adjectival uses of number words into substantival form does not guarantee that numbers are objects, and Frege is aware of this. He goes on to give two other considerations that will add substance to the argument. However, the question arises at this point as to whether this first consideration actually undermines Frege’s contention that numbers are objects. Would it not be equally legitimate to claim that all expressions of number are adjectival, and those that appear to be singular terms can actually be reparsed into adjectival form? (This is actually the strategy that both Hodes and Dummett take. (See Hodes [1984] and Dummett [1991a].) If we accept the claim that the syntactic role of an expression is explanatorily prior to the ontological status of its referent, then on this alternate view, numbers would not be objects since the syntactic role of numerical expressions is adjectival. Frege does not answer this objection, and thus, at least as yet, he has not shown that the adjectival route is unsatisfactory. (He may have held that all adjectival uses can be rephrased into substantival uses and that this does not hold conversely.)
The third consideration is the most important. Frege shows that assertions of numerical identity are significant. This, of course, corresponds to symptom (3). Identity is a relation that holds of objects, and we have significant assertions of numerical identity that have all the logical features of identity (i.e., symmetry, reflexivity, transitivity, and substitutivity) that sustain inference patterns. (b) above is an identity statement; it says that the number of Jupiter's moons is identical to four. Numerical equations are also identity statements which express a relation between objects; "4^2 = 16" says that '4^2' and '16' pick out the same object.

After discussing the above features of numerical expressions, Frege thinks he has shown that numbers are objects. But isn't it possible that these objects really don't exist? Isn't it possible that numerical expressions don't refer to anything? For Frege, the answer is 'no'. Why?

The key to the answer lies in Frege's second methodological principle: "never to ask for the bedeutung of a word in isolation, but only in the context of a proposition." This principle (hereafter, "the context principle") is, in effect, stating that it is illegitimate to ask whether there really are such objects once it has been determined that expressions that refer to them function as singular terms in true sentences. To claim otherwise is to suppose that we can ask about a term's reference or meaning in isolation.

The form of argument that Frege is setting up is as follows:  

14That is, as long as some of the statements in which the expressions occur are true. See below for elaboration and explanation. The main point is that if we use numerical expressions in true sentences in such a way that they function as singular terms (in the ways discussed above), then they do refer to objects.

15Grundlagen, Introduction, p. x.

16The following is substantially from Hale [1987].
(i) If a range of expressions function as singular terms in true sentences, then there are objects denoted by expressions belonging to that range.

(ii) Expressions of type x do so function in many true sentences.

Hence,

(iii) There exist objects denoted by those expressions of type x.

Premise (i) seems questionable. But Frege's point is not merely that if an expression \( \alpha \) functions as a singular term then it denotes an object, but rather that if \( \alpha \) functions as a singular term in true sentences then it refers to an object. Let \( p \) be a sentence in which \( \alpha \) functions as a singular term. Then \( \alpha \)'s role in that sentence is to convey reference to an object. The truth conditions of \( p \) cannot be fulfilled unless the singular term is referential, so if \( p \) is true, then \( \alpha \) refers to an object.

Given the above, Wright argues that the correct way to interpret the context principle is as a principle concerning reference: only in the context of a sentence does a word have reference. He says:

The tendency which Frege is opposing would allow that even if, in terms of [the conception of what it would be for a statement containing \( \alpha \) to be true], an appropriate such statement is true, and even if [\( \alpha \)] functions, by all syntactic criteria, just like a singular term, it may yet be that [\( \alpha \)] has no objectual reference. For Frege this is a confusion: we have no grip on any further question about [\( \alpha \)'s] claim to reference. (Wright [1983] p. 172 (n1).)

One might object at this point that there is no way to decide the truth value of a sentence unless we know what its subsentential parts refer to. How, then, could a question of truth value be prior to a question of reference? The answer lies in the context principle. As long as \( \alpha \) is part of the language, the only way to understand what \( \alpha \) refers to is to understand sentences in which it occurs. There may be some singular terms that do not refer (e.g. 'the present King of France'), but the only way to know that is to understand what it would be for a statement containing the term
to be true. Thus, although it may be that the truth value of a sentence does depend on the reference of its subsentential parts, this is not an objection to Frege’s context principle.

Frege’s own examples are the singular terms ‘equator’ and ‘the center of mass of the solar system’. Both function as singular terms in sentences. Both occur in sentences that are true. But their reference can only be given in terms of what we can truly say about the objects they denote.

§ 3. Frege’s Numbers

Thus far, I have established that Frege held that numbers are objects and why Frege held that numbers are objects. I have yet to examine his attempts to specify what kinds of objects numerical expressions denote. To do this, however, I have to back up a bit. This section of the chapter will be devoted to sections 55 - 86 of the Grundlagen.

In section 55, Frege tentatively proposes the following definition:

the number 0 belongs to a concept, if the proposition that a does not fall under that concept is true universally, whatever a may be.

the number 1 belongs to a concept F, if the proposition that a does not fall under F is not true universally, whatever a may be, and if from the proposition “a falls under F” and “b falls under F” it follows universally that a and b are the same.

the number (n + 1) belongs to a concept F, if there is an object a falling under F and such that the number n belongs to the concept “falling under F, but not a.” (Frege 1884 section 55.)

This is an explanation of numbers as cardinality quantifiers. In symbolic notation the definition says:

\[(0) \quad (E_0x)Fx = df (x) \neg Fx\]

\[(1) \quad (E_1x)Fx = df \neg (x) \neg Fx \& (x)(y)((Fx \& Fy) \rightarrow x = y)\]
\[(n + 1) \cdot (E_{n+1})Fx = \text{df} (Ex)[Fx \& (Eny)(Fy \& \neg(x = y))]\]

Frege eventually rejects the above as a characterization of numbers, for we have not established that numbers are objects; this "definition" characterizes numbers as adjectives:

It is only an illusion that we have defined 0 and 1; in reality we have only fixed the senses of the phrases "the number 0 belongs to" and "the number 1 belongs to"; but we have no authority to pick out the 0 and 1 here as self-subsistent objects that can be recognized as the same again. (Frege 1884 section 56.)

The next attempt begins in section 62. After having shown that numbers are objects and that they are not subjective, Frege asks "How, then, are numbers given to us...?" (Frege 1884 section 62.) This is an epistemological question in that it asks how we apprehend numbers, but Frege's strategy in answering it is to turn to the meanings of sentences in which expressions for numbers occur, and so he invokes the context principle. If we can fix the senses of sentences in which numerical expressions occur, then we will have explained what it is for such sentences to be true, and how we are able to understand such sentences. But then, given the context principle, we will be able to grasp the reference of the numerical expressions.

One might object at this point that the context principle couldn't really allow us to grasp the reference of an expression since it does not enable us to establish some kind of extra-linguistic association between the expression and its referent. The context principle may allow us to conclude that these terms have reference, but

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17 After having said this, Frege devotes sections 57 through 61 to showing that numbers are objects, as discussed above.

18 As Dummett notes, this is the first example of the "linguistic turn" in the history of philosophy. See Dummett [1991a] pp. 111 - 112.
it doesn’t help us grasp the reference. But this objection misses the point of the principle. As Dummett notes:

According to [the context principle], it is only as occurring in the context of a sentence that a name can so much as be said to stand for an object. It follows that to determine what ‘the number 1’ stands for, and hence what the number 1 is, involves fixing the senses of sentences in which that proper name occurs, and does not involve anything beyond this. 19

So if we can understand sentences in which ‘the number 1’ occurs and we can determine the truth values of such sentences, then we will have grasped the reference of ‘the number 1’.

Since Frege has already argued that numbers are objects (sections 57 - 61), identity statements in which numerical expressions occur must have a sense. Identity is a relation that holds of objects, and all objects must have identity conditions. Using the concept of identity, then, Frege’s goal is to explain what it is that is identical. Thus he turns his attention to identity statements containing numerical expressions:

In our present case, we have to define the sense of the proposition “the number which belongs to the concept F is the same as that which belongs to the concept G”, that is to say, we must reproduce the content of this proposition in other terms, avoiding the use of the expression “the Number which belongs to the concept F”. (Frege [1884] section 66.)

Stripped of Fregean terminology, the goal is to find a way to explain the meaning of “the number of Fs is the same as the number of Gs” without using the phrase “the number of Fs”. Frege’s idea is to use a principle of David Hume:

19Dummett [1981], p. 366. See also Dummett [1991a], p. 156.

20David Hume, Treatise, Book I, part iii, Section 1. Quoted in the Grundlagen, section 63.
This is the notion of one-to-one correlation, and Frege intended to use it as the criterion of identity for numerical terms. So this gives us:

The number of Fs is the same as the number of Gs

is equivalent to

There are just as many Fs as Gs.

Notice that ‘the number of Fs’ does not occur in the second sentence. It has been contextually defined in terms of its identity conditions. Henceforth, this attempt to define numbers will be referred to as “the contextual definition”.

The contextual definition initially seems to fulfill Frege's goals. It fixes the sense of statements of identity in which numerical expressions occur, and given the context principle, would seem to enable us to grasp the reference of numerical expressions. Frege is not, however, content with the contextual definition, for it does not solve what we may call “the Julius Caesar problem” In short, we are not able to determine the truth value of sentences of the form

the number of Fs = q

where q is not of the form “the number of Gs”. So we cannot determine whether the number of Fs is Julius Caesar.21

This objection may seem odd, but for Frege, all identity statements must have determinate truth values. In fact, all concepts and relations (in short, all functions) must take every object as argument. For every first level concept F and every object

21The Julius Caesar example actually occurs in section 56 of the Grundlagen; the example Frege uses in section 66 has to do with directions. In 66, he is discussing how to contextually define directions using lines. His objection is that the definition will not tell us whether or not England is identical to the direction of the Earth’s axis. Frege says, “Naturally no one is going to confuse England with the direction of the Earth’s axis, but that is no thanks to our definition of direction.”
α, Fα must be either true or false. Frege does not restrict the range of the variables in any way -- the first-level quantifiers in Frege's system range over all objects. Given this, the proposed contextual definition fails since it does not answer questions about the truth value of sentences of the form "the number of Fs = q" where q is an expression not of the form "the number of Gs".

Thus Frege comes to his final (and fatal) definition. He says:

The Number which belongs to the concept F is the extension of the concept "equal to the concept F." (Frege [1884] section 68.)

In other words, the number which belongs to the concept F is the class of all classes that are equinumerous with the concept F.

After defining the notion of equinumerosity, the remaining sections of the *Grundlagen* are devoted to proving things using this definition of number. In 73, Frege proves Hume's principle; in 78 he proves what are in effect Peano's postulates. In sections 82 and 83, Frege outlines a proof that every finite number has a successor. This will show that there is no last number, and thus that the natural numbers are infinite.

Frege thought that extensions (or classes) are objects, so the final definition respects the arguments for the claim that numbers are objects. However, it is the notion of extension that led to Russell's paradox. Axiom V of Frege's formal system (in the *Grundgesetze*) says that for any concept F, the extension of F is the same as the extension of G if and only if for any object x, Fx iff Gx. Part of the problem with this axiom is that certain extensions are in the range of the variable x; the 'for any object x' includes extensions. (Recall that functions range over all objects, and extensions are objects.) Consider the concept R: 'extensions which are not

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22 After defining '0', '1', and 'successor'.

members of themselves'. Let $x$ be the extension of this concept. Is $x$ in $R$? If so (i.e., it is a member of itself), then it is not a member of itself. But if it is not a member of itself, then it is a member of itself.\footnote{This is, of course, Russell's paradox. Russell originally stated the problem as he saw it in [1902]. He writes to Frege:

Let $w$ be the predicate: to be a predicate that cannot be predicated of itself. Can $w$ be predicated of itself? From each answer its opposite follows. Therefore we must conclude that $w$ is not a predicate. Likewise there is no class (as a totality) of those classes which, each taken as a totality, do not belong to themselves. From this I conclude that under certain circumstances a definable collection does not form a totality.}

It is widely held that Russell's paradox was devastating to Frege's entire project. Of course, the paradox arose from the formal system laid out in the *Grundgesetze*, not from the *Grundlagen* itself. Frege's ontology in the earlier work, however, offers him little protection against said paradox. But can Frege be saved from contradiction? Can any of Frege's program be saved? In the next section, I turn to this question.

§ 4. Frege and Structuralism

From the structuralist's standpoint Frege went too far, for the contextual definition of number is enough to characterize the structure of the natural numbers. In Chapter 4 of [1983], Wright shows how to derive Peano's postulates from second-order logic with an added axiom.\footnote{Boolos and Hodes show something similar. See Boolos [1986/7] and [1987] and Hodes [1984].} This extra axiom is a version of Hume's principle -- Frege's contextual definition of number. Peano's postulates explicitly define a structure, and it will be shown that on the structuralist view, at this point Frege had done enough. Suppose that Frege had stopped with his contextual definition of number. What would have happened to the rest of his program?
The important question is whether or not Hume's principle is satisfiable. If it is, then at least some of Frege's project can be saved. George Boolos, John Burgess, and Harold Hodes have each shown that Hume's principle is, in fact, satisfiable. Hodes' project is to show that a version of logicism is true: mathematics really is (third order) logic. He rejects Frege's claim that numbers are objects, but much of the rest of Frege's program survives. Boolos' project is a bit different, and he actually shows more than just that Hume's principle is satisfiable. Boolos proves that all of the principles Frege actually utilizes in the *Grundlagen* are consistent.

The only extensions Frege makes use of in the *Grundlagen* are extensions of higher-level concepts of the form 'equinumerous with the concept F'. Boolos shows that the existence of these extensions can be assumed and the mathematical program can be completed without contradiction. Boolos shows that

Numbers: \((AF)(E!x)(H)(Hnx \iff H \text{ eq } F)\)

is consistent, where \(n\) is a 2 place predicate letter that connects concept variables and object variables. 'Hnx' can be read 'H is in the extension x'. 'H eq F' means that H is equinumerous with F. Boolos then goes on to show that Hume's principle follows and the rest of Frege's program in the *Grundlagen* can be completed; i.e., Peano's postulates can be derived using standard axiomatic second order logic.

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25 This is, of course, not a question Frege would have asked, for he had no vantage point from which to ask it. From our perspective, however, it is the important question. We want to know if Frege's project can be completed without contradiction, and if Hume's principle is satisfiable, then it can be.

Since Numbers is satisfiable, Frege's program can be completed without contradiction. But the only extensions that can be countenanced are those that Numbers gives us license to include, since to accept a general principle involving the existence of extensions leads to Russell's paradox. This seems to be rather arbitrary; it would be preferable to find an alternate characterization of number.  

As noted earlier, prior to the final definition of number, Frege had done enough to characterize the natural number structure. Hume's principle is satisfiable. Peano's postulates (which characterize a unique structure) can be derived in standard second-order logic with Hume's principle as an added axiom. On the structuralist's view, this is all that is needed. But Fregean questions remain: Is Julius Caesar a number? If so, which one?

Recall that on the strict Fregean view, all identity statements must have determinate truth values; if a term has objectual reference, it can legitimately flank the identity sign in any statement of identity. Furthermore, for Frege, "all objects" refers to a domain which includes all objects. Objects and the expressions that stand for them are saturated, whereas functions and the expressions that stand for them are unsaturated. With the goal of salvaging much of Frege's system, I propose that we alter this position somewhat. Frege's Julius Caesar problem can be solved by taking the notion of saturation as relative -- relative to a coherent theory or area of discourse. (I will call this the "Neo-Fregean" position.) Were one to alter the notion of saturation in this way, then other related notions would change as well.

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27 I do not intend for this to be a criticism of Boolos. He does not advocate that we actually take this route; what he is trying to do is show that Frege's principles in the Grundlagen are, by themselves, not inconsistent.

28 The use of 'theory' here is unfortunately yet unavoidably imprecise. I mean something along the lines of an area of discourse with a high degree of conceptual homogeneity. See Chapter IV §1 for elaboration.
The notion of singular termhood would become relative to a theory or area of
discourse, and the corresponding notion of objecthood would also change --
objecthood would become theory relative. On the Neo-Fregean view, what is object
from one perspective is function from another.

The Neo-Fregean suggestion is to take the Fregean principle:

(F) If an expression functions as a singular term in true sentences,
then there is an object denoted by that expression.

and alter it in the following way:

(NF) If an expression functions as a singular term in true sentences
relative to a theory T, then there is an object denoted by that
expression.

The idea is to leave most of Frege's project intact; the only alteration is to relativize
the notion of saturation, and thereby the corresponding notions of singular
termhood and objecthood. Instead of claiming that there is one domain which
includes all objects, we allow various domains. Whether or not an expression refers
to an object is relative -- relative to which domain we are talking about.

In letting the theory and its true sentences determine ontology, we are
keeping with the Fregean syntactic priority thesis -- expressions that function as
singular terms in true sentences of the theory refer to objects. The structure of
language plays the dominant role in settling ontological questions. But the Neo-
Fregean diverges from Frege insofar as he or she denies that there is one universal
domain of objects. The Neo-Fregean claims that ontology differs from theory to
theory; objecthood is theory relative. The program attempts to codify the intuitively
plausible idea that the world can be divided into objects in different ways. For
example, consider the question, "How many objects are on the table?" asked when
"all there is" on the table is a deck of cards. Is the correct answer "One," "Fifty-
two,” or some number x, where x is the number of molecules in the deck? Frege’s response would be that the correct answer depends on the covering concept, and the Neo-Fregean’s answer is that it depends on which way we divide. What counts as an object depends on the expressive resources of the language one employs and how that language individuates.29

Of course, the principle (NF) raises a number of questions. Most prominently, how does an expression function as a singular term relative to a particular theory or area of discourse? Suppose that Frege’s “three symptoms” (i.e., that the expression is a Fregean proper name, that the definite article is readily applicable to the expression, and that the expression can legitimately flank the identity sign) were adequate to non-circularly demarcate the class of expressions that refer to objects.30 Suppose further that we limit ourselves to the expressive resources of a particular theory; i.e., its concepts and corresponding predicates, constants, and general individuative apparatus. These resources will determine which expressions function as singular terms by expressly limiting (or expanding) what we are able to say relative to that theory, what things we are able to talk about, and what kinds of inferences are licensed. Our ability to discriminate between objects is intimately tied to the conceptual resources of the language we use.31

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29 For a discussion of the expressive resources of ordinary language and how we make sense of our everyday discussions on this picture, see Chapter IV, § 2.

30 They actually are not adequate; see Chapter IV. There, I discuss why Frege’s symptoms fail and discuss alternate methods for characterizing singular termhood. The discussion relies heavily on Dummett [1983], Hale [1979] and [1984], and Wright [1983]. However, the fact that Frege’s symptoms are not adequate will not affect the discussion here; as long as we can give conditions that fulfill the goal, the following position is sustainable.

31 Again, see Chapter IV for elaboration. The following relies heavily on Kraut [1980] and [1986] and Shapiro [1989b].
Suppose, for instance, that a theory T lacks the resources to discriminate between what, from a wider perspective, are different tokens of the same type. The vocabulary of T contains no predicate (monadic or relational) truly applicable to one token but not another; T is incapable of discriminating more finely than types. Relative to T, terms referring to the type will function as singular terms to which we apply the definite article and that we allow to flank the identity sign. Consider, for example, a language that is incapable of discriminating between various individuals who have held the office of President of the United States. Call this language \( L_p \). \( L_p \) contains the usual logical vocabulary plus monadic predicates such as ‘... has veto power’ and ‘... is elected every four years’. But \( L_p \) lacks the linguistic resources for distinguishing between what, from a wider perspective, are distinct individuals of which we can truly predicate ‘... is/was a President of the United States’. Distinctions between Jimmy Carter, Ronald Reagan, and Bill Clinton are inexpressible in \( L_p \); relative to \( L_p \), ‘President’ will function as a singular term, and inferences treating it as such will be licensed. From ‘The President is elected every four years’ we can validly infer that there is something such that it is elected every four years. But the referent of ‘President’ is the office of President. Relative to \( L_p \) -- the language in which the inference is embedded -- we cannot even “see” the various individuals who have held the office of President; i.e., \( L_p \) does not have the expressive resources to make distinctions which allow us to individuate between those individuals.

The Neo-Fregean suggestion is that the objects of a theory or area of discourse are those things to which its singular terms refer. Relative to \( L_p \), the office of President is an object. But relative to a theory with finer grained discriminative apparatus, ‘... is/was a President’ functions predicatively, true of a
number of individuals. From this perspective, 'President' does not refer to an object, but rather a function which takes other objects as arguments.

We are moving toward the structuralist conception of (mathematical) objecthood, a central thesis of which is the theory relativity of objecthood. On the structuralist view numbers are objects, but what it is to be an object is structurally defined: to be a mathematical object is to be a place in a structure. Anything at all can have a mathematical structure; any denumerably infinite system of objects whatsoever can display the structure of the natural numbers. But structures themselves are types. Mathematical objects are places in these structures. What it is to be the natural number two is to be the appropriate place in the structure of the natural numbers. Furthermore, it makes no sense to speak of mathematical objects outside of the structure of which they are a part. '2' has no reference in isolation; it is only in the context of particular mathematical structures that we can speak of the reference of '2'. Relative to other structures, however, 2 is not an object and '2' has no objectual reference.

To fully arrive at the structuralist conception of objecthood, we need to add one thing to the Neo-Fregean strategy: a coherent theory characterizes a structure. The structure is given by the relations between the objects of the theory, or, put linguistically, the structure is characterized by the (interpreted) two- or more place predicates applicable to the expressions that function as singular terms relative to that theory.

Before turning to mathematics, it will be useful to look at a non-mathematical example. Consider, again, the office of the President of the United States. This office is part of a structure that is characterized by the Constitution.

32 See Chapter I.
The political structure of the United States has the following positions: President, Vice President, Senators, Representatives, and Supreme Court Justices. The United States Constitution gives us a formula for determining how many places the political structure has. There are two Senators for each state and one Representative for every voting district; currently, the political structure of the United States has 100 Senators, 435 Representatives, and 9 Supreme Court Justices. Every two years when elections are held, the system changes. New individuals are elected and take on the role of their predecessors. But the overall structure -- the way the various individuals are related to one another vis a vis their role in the political process -- remains the same. The President has veto power regardless of who occupies the office of President, the Congress has the power to override the President's veto no matter who sits in the House and the Senate, and the Supreme Court has the power to declare acts of Congress unconstitutional no matter who wears the robes. The structure of the United States government is just what all possible systems that could instantiate this structure have in common.

Recall that in Chapter 1, I pointed out that in political science textbooks, the language pertaining to the political structure of the United States is often employed. When we teach students how the government works, we use sentences whose variables range over the positions as such, with no regard to any system that might instantiate the political structure. The instructor will say things like “The President has the power to veto a bill.” Were a student to ask, “Do you mean George Bush or Bill Clinton?” the instructor would take this as evidence that the student did not

33 That is, the office holders. It also changes whenever someone resigns or dies or a new Supreme Court Justice is appointed, etc. The point is that the system changes whenever a new individual or individuals occupy an office.

34 The ‘possible’ is used to avoid including irrelevant features that all actual systems have had in common. For example, all actual Presidents have been male, but that is not important to the political structure of the United States.
understand. The President, as such, has the power to veto a bill; it is a property of the position in the political structure, and not merely of an individual who occupies that position.

Let us return to mathematics; specifically, to arithmetic and the natural number structure. The language of second-order arithmetic characterizes the structure of the natural numbers and implicitly defines the objects of arithmetic (see Chapter I). The language of arithmetic does not have the expressive resources to refer to, say, the square root of 2. This is because the square root of 2 is not a part of the natural number structure, and the only objects that the language of arithmetic is capable of referring to are those that are places in the structure of the natural numbers. Relative to the natural number structure, natural numbers are objects. Relative to a more encompassing structure with a richer language, however, numerical expressions may function predicatively.

The language of set theory is one language in which numerical expressions function predicatively. Relative to set theoretic discourse, we can say that particular systems (such as the finite von Neumann ordinals) have the structure of the natural numbers; i.e., they are tokens of the natural number structure type. In such systems, particular sets play the role of the numbers and numerical expressions can be predicated of them. They are not, however, identical to the natural numbers, just as individuals who have held the office of President of the United States are not identical to the office itself.

The distinction I am alluding to is that between offices and office holders. The offices can be thought of as equivalence classes of the individuals who have
held/currently hold the offices. If we restrict our discourse in such a way that we cannot discriminate between the members of the equivalence class, then, relative to that discourse, the equivalence classes themselves are objects. But if a discourse can discriminate between the individuals, then the expressions that refer to the offices/equivalence classes function as complex predications, true of a number of individuals. Consider again the President of the United States. Relative to a full background language, we can discriminate between individuals who have played the role of President. In the sentence “Bill Clinton is the President,” ‘...is the President’ is functioning as a complex predication; the Clinton administration is a token of the type of the political structure of the United States and Clinton himself occupies the appropriate place in this administration.

Analogously, the finite von Neumann ordinals are a token of the natural number structure type. \( \{\phi, \{\phi\}\} \) occupies the appropriate place in this token, and ‘... is two’ is predicable of it. Again, \( \{\phi, \{\phi\}\} \) is not identical to 2; \( \{\phi, \{\phi\}\} \) has the property of filling the two-office. But if we limit our discourse to the language of arithmetic, we cannot discriminate between different tokens of the natural number structure type -- we lack the conceptual resources to distinguish between objects based on any properties other than their arithmetical structural properties -- and these properties are held by all tokens of that type.

It was noted earlier that the relativity of ontology is a central thesis of structuralism. Whether or not we classify something as an object depends on the point of view from which we are speaking. It follows directly from this that there is no absolute domain of objects. Relative to the structure of the natural numbers, 2 is an object in its own right; relative to the political structure of the United States, 

\[35\] This doesn't quite work in the case of mathematics; see Chapter I §3 for elaboration.
President is an object. But neither are objects from the point of view of a more encompassing structure, whose corresponding language has the capacity to refer to sets in the former case, people in the latter, who play those roles in particular systems. In the more encompassing structures, they are functions picked out by predicates, which take objects as arguments.

We are now in a position to return to the original problem. The goal was to salvage as much of Frege's project as possible. I suggested that the Neo-Fregean will take Frege's notion of saturation as relative — relative to a theory. Using the examples of the structure of the natural numbers and the political theory of the United States, we can see how this relative notion of saturation will differ from Frege's original notion.

From the Neo-Fregean position, relative to political theory, 'President' is a saturated expression. It displays all of the Fregean symptoms of an expression standing for an object: we readily apply the definite article to it ("the President"), it is a Fregean proper name, and identity conditions can be given for its referent. Furthermore, the identity conditions can be given for President without reference to a particular person who occupies that office on any given day:

The President is identical to the commander in chief.

No mention was made of any particular person; what is important is the position in the structure. The same can be said of the expression '2'. It displays all of the Fregean symptoms of an expression standing for an object, and identity conditions can be given for its referent without reference to any particular system that instantiates the natural number structure.

Relative to another theory or area of discourse, however, both 'President' and '2' are unsaturated expressions. In a more encompassing theory, '... was a
President of the United States' and '... is 2' require arguments to complete them. There are certain systems which display the structure of the natural numbers (e.g., the finite von Neumann ordinals), and there are systems which instantiate the structure of our political system (particular administrations). With regard to the systems themselves, the expressions are functions; ‘... was a President of the United States’ can be completed by the saturated expressions 'Jimmy Carter', 'Bill Clinton', and so on. ‘... is 2’ is a function which takes \{ < j>, \{<£}\} as one of its arguments.

The Neo-Fregean position is consistent with the structuralist thesis that there is no absolute fixed domain of objects. The saturation of an expression is relative to a theory or an area of discourse, and a coherent theory or area of discourse characterizes a structure. Ontologically, the places in the structure are saturated just as Fregean objects are saturated. Linguistically, predicates or relations pertaining to said structure or its places are unsaturated just as Fregean functions are unsaturated. So the "mirroring" relation survives; language mirrors reality.

Frege's Julius Caesar problem stems from the orientation that the structuralist program rejects; namely, that the phrase 'all objects' refers to a fixed domain which includes all objects. The Julius Caesar problem arose for Frege because he thought that all terms with objectual reference must be admissible in argument position in every statement of identity. The contextual definition of number did not determine the truth value of identity statements of the form

\[
\text{the number of } Fs = q
\]

where Julius Caesar, for example, is the value of q. The structuralist rejects the claim that all objects can be taken as arguments for identity statements (if this claim is taken in the sense that Frege intended) because the phrase 'all objects' makes no sense if the 'all' really means all. Whether or not something is an object is theory
relative. Of course, a structuralist can say that identity statements must take all objects as arguments, but in so saying, the 'all' is restricted; it refers to all objects of a particular structure.

We can meaningfully ask questions concerning the identity of places in one structure, but any question of identity between the objects that comprise systems that have that structure and the places in the structure itself make no sense. For example, "2 + 2 = 4" and "4^2 = 16" are legitimate because all of the constants involved refer to objects in the natural number structure. Identity statements like "\{\phi, \{\phi\}\} = 2", however, are illegitimate. Why is this? Well, suppose that "\{\phi, \{\phi\}\} = 2" was a legitimate identity statement. Then, no matter what one's view of ontology, it would have to be the case that \{\phi, \{\phi\}\} and 2 were both objects. But if one's language has the discriminative capacity to recognize properties of the structure of which \{\phi, \{\phi\}\} is a part other than its arithmetical structural properties, then 2 is not an object, but rather a function ('... is two') which takes other objects as arguments. Relative to the natural number structure and its diminished language, one cannot even formulate the question of whether \{\phi, \{\phi\}\} is identical to two. Relative to a more encompassing structure, the question can be formulated, but it is not a question of identity. It is, instead, a question of office occupancy; it is asking whether, in a particular system, \{\phi, \{\phi\}\} plays the role of two. The question must be asked from outside the language of arithmetic. From the Neo-Fregean position, '... is two' is an unsaturated expression in this context, which needs an argument to complete it. '... is two' is a function which can take objects in the more encompassing structure as its arguments. And we do not countenance identity claims between objects and functions.

Returning to Frege's Julius Caesar problem, one could imagine a system that instantiates the natural number structure in which Julius Caesar plays the role of,
say, two. The question “Is Julius Caesar two?” would make sense in this case, but it would not be a question of identity. This is because in this context, ‘... is two’ is an unsaturated expression. Relative to the area of discourse in which one could even formulate the question, ‘two’ is not a saturated expression; it is a function (‘... is two’) that needs completion. In Fregean terminology, if Julius Caesar does play the role of two in a particular system, then Julius Caesar falls under the concept two, for the sentence “Julius Caesar is two” is true in this context.

§ 5. Conclusion

From the structuralist’s standpoint, Frege went too far in his definition of number. The contextual definition of number characterized the natural number structure, and that should have been enough. Frege took the fatal step because of the Julius Caesar problem, which resulted from his view of objects. The Neo-Fregean position avoids this problem without running into contradiction by claiming that saturation is theory relative. On the Neo-Fregean view, the locution “all objects” makes no sense if the ‘all’ really means all, because objecthood is relative. We can say that identity statements must take all objects as arguments, but the ‘all’ here is restricted to a particular structure.

The Julius Caesar problem is avoided by the Neo-Fregean because in the context in which one could even formulate the question “Is Julius Caesar two?”, ‘... is two’ is a function which takes objects as arguments. To say that Julius Caesar is two from this perspective is not to make an identity claim.

It has been shown that Hume’s principle is satisfiable, and that Frege’s project can be completed in a second order system with Hume’s principle added as an extra axiom. Frege’s fatal step was taking extensions as objects that can fall
under concepts, and this is avoided by the Neo-Fregean position. But what has survived? How much of Frege's program has been salvaged?\(^{36}\)

On the Neo-Fregean view that I have advocated, numbers are objects. This is a central claim of Frege's *Grundlagen*, and it has survived. The mirroring relation between language and reality (i.e., that saturated expressions correspond to objects and unsaturated expressions correspond to functions) has also survived. Furthermore, we have kept the syntactic priority thesis intact. The only fundamental change was the relativization of saturation to a particular theory. By relativizing this notion, the corresponding notions of singular termhood and objecthood are also relativized. This is, admittedly, a somewhat radical change. Perhaps relativizing the notion of saturation would undermine Frege's overall philosophical program. It might actually preclude the possibility of a *Begriffschrift* (a universal language); the driving force behind much of Frege's work. But it saves the Fregean mathematical program from contradiction while providing an answer to the Julius Caesar problem.

Finally, it is worth pointing out that Frege himself would not have liked the Neo-Fregean answer. I have suggested that Peano's postulates explicitly characterize the natural number structure, and this implicitly defines the objects of arithmetic. In a letter to Hilbert dated December 27, 1899, Frege writes:

... axioms and theorems can never attempt to fix the *bedeutung* of a sign or word which appears in them; rather, this must already stand fixed.\(^{37}\)

\(^{36}\) Of course, I have said nothing about Frege's logicism, which was a large part of his overall program.

\(^{37}\) This letter is reproduced in Frege [1980].
On Frege's view, we have not adequately characterized mathematical objects as objects unless we are able to specify of what their essence consists. This must be done independently of the axioms that apply to them -- we have to be able to say what numerical constants refer to in some direct, absolute way. But although this was Frege's view, is it essential to his program in the *Grundlagen*? In his discussion of the Frege/Hilbert controversy, Michael Hallett argues that it is not. He writes:

First, it seems clear from Dedekind's work and, say, from Boolos's recent elementary reconstruction of the theory using just second-order logic, that this extra, ontological, component [i.e., specifying the essence of mathematical objects] is not necessary, even if partially 'theory driven'. Second, even though it produces some more theory to answer the further ontological question, the Frege account does not succeed in answering the question in any suasive way. For, even after being told that "numbers are logical objects of such-and-such kind", one might still ask 'How do you know that you have really got hold of "purely logical" objects?'

Furthermore, how do you know that Julius Caesar is not an extension?

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38Hallett [1990], p. 198, emphasis in original. The reference to Boolos is [1987]; the reference to Dedekind is his *Was sind und was sollen die Zahlen?*, Braunschweig: Viewig und Sohn (1888).
CHAPTER IV

OBJECTHOOD

In philosophical contexts, the notion of objecthood is often ignored or left unexplicated. But this notion plays a central role in many classic, longstanding philosophical disputes. Arguments about universals, possible worlds, propositions, and sense impressions (as well as the ontology of mathematics) all depend -- in one way or another -- upon the concept of an object. Often, the disputes are unclear; perhaps this is because the opponents in the various disputes have different conceptions of what it is to be an object. Until some consensus is reached, the disputes will continue and little (if any) headway will be made. In this chapter, I offer a way to clarify the notion of objecthood.

I explored the structuralist conception of mathematical objecthood in Chapter I. What it is to be a mathematical object, on the structuralist view, is to be a place in a structure. Anything at all can have a mathematical structure; any denumerably infinite system of objects whatsoever can display the structure of, say, the natural numbers. But structures themselves are types. Mathematical objects are places in these structures. What it is to be two is to be the appropriate place in the
structure of the natural numbers. Twohood is a role that is defined by its place in a structure.

In the previous chapter, I suggested that Frege could have avoided the Julius Caesar problem if he had held a similar conception of mathematical objecthood. But a further thesis was suggested; namely, that we can extend the structuralist conception of mathematical objecthood to objecthood generally. The reason we regard certain fragments of language as referring to objects is that those fragments of language refer to places in a structure, where a structure is characterized by a coherent theory. In this chapter, I will expand on this further thesis.

The idea is that there is no significant difference in kind between mathematical objects and ordinary objects. The specification of objecthood in mathematics is not importantly different from the specification of objecthood generally. If this is right, then one consequence is that there is no need to be suspicious of mathematical objects.

Before proceeding, however, a cautionary note is warranted. The claim is that there is no significant difference between mathematical objects and ordinary objects, not that there is no difference at all. As noted above, anything at all can play the role of two in the sense that anything at all can occupy the relevant place in a system that has the structure of the natural numbers. This is not the case for all objects. Take, for instance, the example of the political structure discussed in Chapter I. I claimed that the office of President itself can be regarded as an object and the reason it can be so regarded is that the political theory of the United States characterizes a structure in which the role of President is defined by its relations to other positions in the structure. So far, the example seems analogous to mathematical objects. But any system that has the political structure of the United
States must be comprised of persons. It is thus not the case that anything at all can play the role of President; only persons can play the role of President.¹

The thesis I will defend takes its inspiration from the structuralist philosophy of mathematics. It is my contention that what it is to be an object generally can be characterized in much the same way that a structuralist characterizes what it is to be a mathematical object. There will be modifications in the characterization due to the differences noted above, but the modifications will be slight. With this goal in mind, we begin with Frege.

In the previous chapter, we saw that Frege held the following principle:

(F) If an expression functions as a singular term in true statements, then there is an object denoted by that expression.

The principle seems intuitively right, at least at first. If S is a sentence in which α occurs and α functions in S as a singular term, then α's role in that sentence is to convey reference to an object. The truth conditions of S could not be fulfilled unless α is referential, so if S is true, then α refers to an object.²

Frege's Julius Caesar problem arose because he took the locution "all objects" to refer to all objects. As a way to amend Frege's program and save it from contradiction, I suggested a "Neo-Fregean" position that takes objecthood as relative.³ If we go this route, then a first approximation of the amended version of (F) would be:

¹There are further (non-structural) qualifications: Any person who occupies the office of President must be a natural born citizen of the United States and must be at least 35 years of age. See below.

²Unless, of course, the truth of S requires that the object not exist (e.g., if S is a negative existential).

³See Chapter III.
(NF) If an expression functions as a singular term in true sentences relative to a theory T, then there is an object denoted by that expression.

This principle looks better. It relativizes the notion of objecthood to a particular theory and allows that $\alpha$ can denote an object in one theory even though $\alpha$ may denote a function in a more encompassing theory. But we do not want just any theory to count, for we want to make sure that the theory in question is satisfiable. Of course, (NF) stipulates that the relevant sentences must be true, and truth is stronger than satisfiability. But we want to avoid the possibility of including those true sentences that occur in incoherent theories. So amend (NF) in the following way:

(NF') If an expression functions as a singular term relative to coherent theory T in true sentences, then there is an object denoted by that expression.

(NF') is close to the thesis that I will defend. Recall that in the case of mathematics, I claimed that the axioms of a mathematical theory implicitly define its objects by explicitly characterizing the structure of which they are a part. The singular terms of a mathematical theory refer to objects, which are the places in the structure so characterized. The structuralist thesis concerning objects can thus be put as follows:

(S) A coherent theory T characterizes a structure, and if an expression functions as a singular term in true sentences relative to T, then that expression refers to an object.

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4 An alternative way of accomplishing the same goal might be to define 'theory' in such a way that false sentences are excluded. But given my characterization of "theoryhood" (see §1 below), I prefer not to take that route.

5 Further recall that in mathematics, the theory that characterizes these structures must be categorical.
There is no more, and no less, to being an object on the structuralist view. There is an object that $\alpha$ refers to if $\alpha$ functions as a singular term in a coherent theory.\(^6\)

(S) captures the heart of the structuralist conception of objecthood. It relativizes objecthood and it (indirectly) captures the idea that the expressive resources of a theory or area of discourse implicitly define its objects. The expressive resources of a theory -- its concepts and corresponding predicates, constants, and general individuative apparatus -- expressly limit (or expand) what we are able to say relative to that theory, what things we are able to talk about, and what inferences go through. Say, for instance, a theory lacks the resources to discriminate between what, from a wider perspective, are different tokens of the same type. In such a situation, we are talking about the type as such, and terms referring to the type will function as singular terms. The objects of that theory or area of discourse will just be what the terms that function as singular refer to, given what we are able to express. And if the theory characterizes its structure, then what the singular terms refer to will be places in the structure so characterized. On the other hand, relative to a theory with finer grained discriminative apparatus, the same expression may function predicatively, true of a number of individuals. From this perspective, the expression does not refer to an object but rather a function which take other objects as arguments. Robert Kraut succinctly captures this idea in [1986]. He says:

... a theory is not plausibly treated as containing singular terms which refer to $\sigma$s unless that theory offers the descriptive apparatus for discriminating between distinct $\sigma$s. (Kraut [1986], p. 405.)

\(^6\)Of course, there won't be singular terms for all of the objects of real analysis, because the real numbers are uncountable. Perhaps, then, the principle ought to be amended in such a way so that it allows for possible singular terms. But this would make (S) too cumbersome, so I will just ask the reader to keep this addendum in mind.
Pre-analytically, we regard certain fragments of our world as objects. For example, the paper upon which this is printed is an object and the pen with which I jotted notes in the margin of an earlier draft is an object. If one embraces the structuralist conception of objecthood, one is not led to deny these pre-analytic intuitions; a structuralist believes, along with everyone else, that pieces of paper and pens are objects. But what leads us to classify pieces of paper and pens as objects? What mechanisms are at work in this classification, and what factors are at work when we say that certain fragments of our world are objects as opposed to, say, functions? The structuralist’s claim is that such classifications are theory relative insofar as the referent of \( \alpha \) might be classified as an object relative to one theory yet not so classified relative to another theory. The mechanisms of classification are wholly dependent on the discriminative apparatus of a theory, for our ability to discriminate between objects is intimately tied to the conceptual resources of the fragment of language in question.

As it stands, (S) is open to many questions: (1) What is the criterion of singular-termhood? This must be specified in such a way that it does not beg any questions. (2) What is meant by ‘true sentences’ in the principle (S)? (3) What is meant by ‘theory’? This notion needs some specification, since it plays an essential role in the overall program. If objecthood is relative to a theory, then what a theory is needs to be spelled out. (4) How does a theory characterize a structure? This may be clear in the case of mathematical theories, but how does it work outside of mathematics?

(S) is also open to many challenges, such as: How do we know that the theory “really gets at the world”? In other words, how do we know that there really are objects of the kind required by some theory T, and that the purported singular terms are genuinely referential? There are various ways of interpreting this
challenge, and Chapter V will be devoted entirely to that enterprise. For now, however, I will concentrate on spelling out the view in more detail and addressing questions (1) through (4) above. In § 1, I discuss the meaning of 'theory' as it pertains to my thesis and how a particular theory characterizes a structure. (Questions (3) and (4)) In § 2, I examine the criteria for singular termhood. (Question (1)) In § 3, I discuss the notion of truth. (Question (2)) I will show there that the principle (S) needs a final revision. Finally, in § 4, I discuss some of the advantages of applying the structuralist notion of mathematical objecthood to objecthood generally.

§ 1. 'Theory' and Theories

In mathematics, 'theory' is sufficiently precise.\(^7\) There are borders between mathematical theories, and such borders are easily demarcated. Of course, mathematical theories can often be embedded in other mathematical theories (e.g., arithmetic can be done in set theory), but it still makes sense to speak of the theories of mathematics independently of one another. One can, for instance, speak of the theory of arithmetic itself, independent of set theory, and it is generally understood what one is speaking of.

The goal is to expand the structuralist notion of mathematical objecthood and use it as a general criterion of objecthood. We thus need to clarify the notion of "theory" as it will be used outside of mathematics. The claim I will defend is that objecthood is theory relative, and this use of 'theory' is not sufficiently precise.

Before attempting to give a more precise meaning to 'theory', however, it will be worthwhile to pause for a moment and discuss the notion of relativity invoked

\(^7\)Or at least can be made sufficiently precise.
above. In a certain sense, one can claim that morality is theory relative. Sentences of the form “Action x is right” will vary in truth value dependent on what moral theory one uses to evaluate the sentence. A deontological theory may, for example, assign a different truth value to a particular instance of the sentence form than would a consequentialist theory. ‘Right’ is truly applicable to certain actions relative to one theory but perhaps not so applicable relative to another. The relativity of objecthood thesis, however, goes further than this. It is not just that the truth value of certain ontological claims vary from theory to theory. In fact, sentences asserting the existence of certain objects may not even be formulable in certain theories, if the expressive resources of the theory does not contain predicates which allow us to distinguish between different tokens of the same type. The claim is that it is implausible to treat a theory T as being about $\sigma$s if T cannot distinguish between distinct $\sigma$s (that is, if T treats the type itself as an object). Relative to political theory, the office of President itself is an object, while relative to a more encompassing theory (where one can distinguish between various individuals who have held that office or played that role), it is a function which takes other objects as arguments. What is object from one perspective may be function from another. The subject matter of a theory is relative to that theory, whereas in moral discourse, the subject matter remains unchanged even though the truth value of various moral claims may differ from theory to theory.

It would be nice (perhaps) if I were able to give a set of necessary and sufficient conditions for a particular fragment of discourse to qualify as a theory. The goal of such a set of conditions would be to clearly demarcate the divisions and borders between theories and specify exactly what counts as a theory. We could then go on to regiment our discourse and divide it into separate theories with sharp boundaries, analogous to mathematical theories. Continuing the analogy with
mathematics, we could account for the intermixing of theories by pointing out that some theories can be embedded in others, but in principle each theory could be thought of as autonomous.

As I said, it would be nice if I were able to give a set of necessary and sufficient conditions that would accomplish the above goal. Unfortunately, however, such a set of conditions is unattainable. Our discourse is not susceptible to the sort of precise boundaries required for the project, and even if it were, it is unlikely that we could isolate a set of necessary and sufficient conditions that would clearly demarcate those boundaries. We will thus have to settle for something less ambitious. We want a specification of what it is to be a theory, but we have to realize that in many cases the boundaries will be fuzzy. Furthermore, the specification will not be intended to withstand counterexamples, for in giving such a specification I am not attempting to analyze a concept that we already understand; in other words, my goal is not to attempt to give conditions such that all and only theories, qua pre-analytically understood ‘theories’, fulfill them. Rather, my goal is to explain what it is I take a theory to be for the purposes of the relativity of objecthood. In other words, I will attempt to answer the question: “When does a fragment of language qualify as a theory with its own ontology?”

As I am going to use the term, a theory is a body of sentences which display a high degree of conceptual homogeneity. In other words, a theory is a fragment of language in which there is a more or less well-defined and understood group of concepts and corresponding predicates that “go together”. A theory of Euclidean space, for example, would include the concepts “point”, “line”, “angle”, “plane”, etc. A theory of biology would include the concepts “species”, “genus”, etc. And

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8I thank Pierluigi Miraglia for this suggestion.
political theory would include the concepts "veto", "override", etc. Notice that in each given example, the concepts are related to one another. In a theory, as I intend to use the word 'theory', the descriptive vocabulary must be conceptually interconnected. Furthermore, all uses of a term within a particular theory must be of the same grammatical kind. One cannot use the same expression as both a singular term and as a predicative expression within one theory.

A theory characterizes a structure by implicitly defining its objects and the relations that hold between them. In mathematics, one way of doing this is through the axioms of the theory. In Chapter I, for example, I showed how Peano's postulates explicitly define the structure of the natural numbers. The axioms implicitly define the objects of the theory through the explicit definition of the structure of which they are a part. Unfortunately, most theories (in the sense of 'theory' I intend) are not axiomatized (even in mathematics). Nonetheless, a structure can be characterized by these theories, albeit in a somewhat different fashion. When we limit our discourse to a particular fragment of language, we limit ourselves to the expressive resources (i.e., to the concepts and corresponding predicates) available in that fragment. We are only able to discriminate between objects based on those predicates available. The expressive resources of the theory tell us how the objects of the theory are arranged and related to one another. But a few qualifications are in order at this point.

A structure is a collection of elements with certain relations, and in mathematics structure is all that matters. This is not the case for (at least some) other theories, and this marks two significant disanalogies between mathematical objects and objects of other theories: (1) The only properties mathematical objects have are those that are structurally given. This is not always the case outside of mathematics. (2) Any denumerably infinite system of objects whatsoever can have
the structure of, say, the natural numbers. It makes no difference whatsoever what objects play the role of the individual numbers in systems that display the structure of the natural numbers. Again, this is not always the case outside of mathematics.

It may seem at first glance that the political example sketched in Chapter I does not violate (1); i.e., that the only properties of the positions of the political structure of the United States are those that are structurally given. But this is not the case. Some properties that do not result from structure are essential to the positions. For example, the President must be at least 35 years of age. This is specified by the Constitution, but it is difficult to see how this could be construed as a structural property. Furthermore, (as noted earlier) in connection with (2) it does make a difference what objects have the political structure of the United States and thus play the role of the individual places in the structure. Only persons can occupy those offices.

Given these two disanalogies, it becomes difficult to see how to apply the structuralist conception of objecthood outside of mathematics. In particular, it becomes difficult to see how one characterizes a structure with a fragment of language, for it seems that structure is not all that matters. However, there is a way to save the general program of extending the structuralist conception of objecthood to objecthood generally. We can claim that objecthood is relative to a theory and that the considerations that lead us to designate the referent of $\alpha$ as an object are structural. The only modification necessary is the method of characterizing a structure. We need to keep the two disanalogies noted above in mind: we can't ignore what comprises systems that have the structure and we have to allow that some properties are not structurally given. The modifications will make it the case that we have deviated slightly from the structuralist conception of mathematical objecthood, but not significantly. The general insight is intact: the criterion of
objecthood remains unchanged. What it is to be an object is to be a place in a structure. The aforementioned modifications spring from the fact that there are differences between mathematical objects and other objects. In mathematics, the criterion of objecthood tells us everything there is to know about the objects, but there may be more to objects generally than what is specified by the general criterion of objecthood. This marks a difference between mathematical objects and other objects, but it is not a significant difference in kind.

Leibniz's principle of the Identity of Indiscernibles states that if $\alpha$ and $\beta$ are indiscernible, then they are identical. If anything true of $\alpha$ is also true of $\beta$ then $\alpha$ and $\beta$ are identical. In [1980], Robert Kraut points out that discernibility depends on what conceptual resources are available. For example, consider a fragment of language that does not contain the conceptual apparatus needed to distinguish between different persons who have held the office of President of the United States. The only properties and relations that are recognized in this fragment of language are those that hold of the office of the President. Anything true of Bush is also true of Clinton relative to this fragment of language. Using Leibniz's principle, we can identify Clinton and Bush; relative to political theory, they are the same object. My goal is to use this insight to show how a non-mathematical theory characterizes a structure.

We start with the full background language in which we are able to discriminate between Clinton and Bush. The objects of this background language exemplify the political structure of the United States; they comprise a system which

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9 That is, anything that was true of Bush while he was in office. It might be cleaner to speak of time slices of the persons who occupied the office of President. For example, Clinton in 1994, JFK in 1963, Bush in 1989, etc. In the idealized language of political theory, any predicate true of one of these time slices will be true of all of the time slices.
has this structure. We then define an equivalence relation on the objects so that it divides the objects of the full background theory into mutually exclusive equivalence classes. The equivalence relation should respect the structural relations between the objects of the system. In the political example, the equivalence relation will be “same office”. So Clinton and Bush will be in the same equivalence class if and only if they held the same office. If we restrict ourselves to the language of political theory, the equivalence relation will be a congruence; i.e., any predicate that is applicable to Bush will be applicable to Clinton. Furthermore, distinct positions will be distinguishable with a predicate -- there will be a predicate applicable to anything that does not bear the relation “same office” to Bush and Clinton that is not applicable to them.

The equivalence classes will exemplify a structure, which is given by the relations between them. What it is to be an object relative to political theory is to be a place in the structure exemplified by the equivalence classes. Notice that this method of characterizing a structure respects the differences (noted above) between mathematical objects and other objects. It does matter what exemplifies the structure because we begin with systems of objects that have that structure when we characterize said structure. Furthermore, we are able to respect properties that are not given solely by structure (e.g., the requirement that the President be at least 35), for this method of characterizing a structure does not require that all properties be structurally given. But it leaves the general structuralist insight intact. Objecthood

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10 Recall from Chapter I that all systems with a particular structure must be isomorphic.

11 By “predicate”, I mean something like a formula with a free variable.

12 This application of Kraut’s thesis to structuralism is explored in Shapiro [1989b].
is theory relative and the reason we construe talk of $\sigma$s as talk of objects is structural.

The above method of characterizing a structure requires that there be a background language with a more encompassing ontology. We start with the background language and define an equivalence relation on its objects and then restrict our language to the theory itself, relative to which the equivalence classes are objects. But what about the objects of the background theory? The background theory (hereafter "B") is a theory in the same sense of the word as the more limited theory (hereafter "L"). It is more encompassing, so the conceptual resources must be broader. Nonetheless, the concepts of B must display a degree of conceptual homogeneity. L can be seen as a sub-theory of B which displays a higher degree of conceptual homogeneity. The structuralist's claim is that the objects of B are themselves places in larger structures; structures which contain systems that have structure of L.

The objects of B are the system from which we obtain the structure of L, but they themselves are subject to the same process with respect to an even more encompassing background theory B'. And the objects of B' comprise a system from a yet more encompassing background theory B''.\(^\text{13}\) And so on. In principle, there is no ultimate theory which has objects simpliciter. But we often rest with physical object talk and its objects, just as in mathematics we often rest with the language of set theory and its objects. The fact that we often do so, however, does not mean that the theory in question is the ultimate stopping place.

\(^{13}\)Is there always such a B', B'' etc.? The structuralist's claim is that there always could be.
Ordinary discourse itself, however, does seem to present a problem. We don't speak in fully regimented distinct fragments of language which can be labelled "theories", even in the loose sense of the word 'theory' mobilized here. We often intermix the conceptual resources of various theories in our everyday conversation. Can the structuralist accommodate this without suggesting that we artificially break our everyday talk into distinct fragments of language that are subject to (S)? The answer is "yes and no". "No" insofar as the application of (S) and the principle of the individuation of objects it suggests goes -- in order to apply (S), we do need to distinguish between different fragments of language. (S) specifies that terms are singular only relative to a particular theory; thus, without distinguishing between different fragments of language we will be unable to discern when an expression is functioning as a singular term. But that does not mean that I am suggesting a radical revision in language use. Rather, the suggestion is that for any use of a singular term in ordinary discourse, there is a theory in which that term refers to an object. This is where the "yes" part of the answer to the above question comes from. If $\alpha$ is functioning in a use of a sentence as a singular term, and that sentence is true, then there is a theory in which the referent of $\alpha$ is an object. In principle, we could isolate the theory in which that term refers to an object. The important point to note is that the same term could be used in two different sentences, such that in one it is used as a singular term (and thus refers to an object) and the other it is not so used (and thus does not refer to an object). In ordinary discourse we sometimes intermix the different types of usage (e.g., "Moby Dick is a whale" and "The whale is a mammal" or "Bill Clinton is President" and "The President is up for election every four years"), but in principle they could be separated. And this brings us to the next question: when is an expression functioning as a singular term?
§ 2. Singular Terms

In Chapter III, we saw that Frege claimed that objects are what singular terms stand for. An expression $\alpha$ is a singular term if and only if (1) $\alpha$ is a Fregean proper name, (2) we are willing to prefix $\alpha$ with the definite article (perhaps with a sortal, e.g., "The person Gottlob Frege"), and (3) identity statements containing $\alpha$ have a sense. There is an object that $\alpha$ stands for if appropriate sentences in which $\alpha$ occurs are true.

The principle defended herein is:

(S) A coherent theory $T$ characterizes a structure, and if an expression functions as a singular term in true sentences relative to $T$, then that expression refers to an object.

The principle requires that we be able to pick out singular terms in a way that does not presuppose that there are objects that said terms denote. If the only way to specify which terms are singular is to say that they are those fragments of language that function to pick out objects, then (S) would be hopelessly circular. One might think that Frege's three criteria could be of use here. Unfortunately, they are not.

Frege's first requirement is that the term be a Fregean proper name. Fregean proper names include genuine proper names, definite descriptions, and personal and demonstrative pronouns. Unfortunately, all this requirement does is tell us that the term, in order to stand for an object, must be singular. It does not tell us what it is to be a singular term. So the first condition is of no use.

Frege's second requirement is that we must be willing to prefix the expression with the definite article, perhaps with a sortal term. The problem with this condition, however, is that even if the use of the definite article is a necessary condition for a term to refer to an object, it cannot be sufficient. We use the definite
article in many constructions, not all of which we would be willing to label singular terms. For example, we say things like "She came in the nick of time," and "He left me in the lurch." Presumably we don't want to include nicks of time and lurches in our ontology, but both phrases admit application of the definite article. The obvious way out here is to remark that "the nick of time" and "the lurch" are not genuine singular terms, but Frege's second condition does not give us justification for ruling out such constructions.

It might be thought that Frege's third requirement is the key requirement. Identity only holds of objects, and for any object whatsoever, it must be determinate what counts as the same object. If a term can flank the identity sign in true sentences, then that term is singular. In fact, condition (3) might rule out the above examples concerning lurches and nicks of time. So although condition (2) may not be, alone, sufficient, (2) together with (3) might be individually necessary and jointly sufficient. This, at first, looks plausible. But consider for a moment how, exactly, we pick out identity statements in ordinary discourse. How do we distinguish them from other sorts of sentences? For example, consider a sentence containing the word 'is', where 'is' is not used to mean "is the same object" but rather, say, "has the quality" (i.e., 'is' is used as the 'is' of predication instead of the 'is' of identity). How do we distinguish between these sorts of constructions and genuine identity statements? We know these constructions are statements of identity either (a) because we know beforehand that the terms involved are singular or (b) because we know that the terms involved refer to objects. In either case, this specification of singular termhood becomes circular. The use of a term in identity statements was supposed give us a way to determine that the term is singular, but if our

identification of the statement as one of identity depends either on our knowledge of the fact that the terms are singular or our knowledge that the referent of the term is an object, then we have not explained singular termhood independently. Again, we would be involved in hopeless circularity. Frege's third requirement thus does not seem to assist us in our search for a specification of singular termhood, either.

Perhaps the criterion for singular termhood can be found in the inferences that are licensed regarding that term. The inferential role of the fragment of language can tell us what its function is, and thus whether or not it is a singular term. We allow certain inferences to go through if a term refers to an object that we would not allow were it predicative in nature (e.g., existential generalizations). But once again, isn't this circular? If we were to ask why certain inferences are licensed, the reply would be because the term in question has objectual reference.

The goal is to find a criterion of singular termhood that does not involve reference to an object. If (S) is to be successfully defended, then it must be possible to specify this criterion independently of that particular semantic function of singular terms. But even so, perhaps I have been too hasty to cast aside certain considerations and make the kinds of circularity charges I have made. There are two sorts of concerns at issue here, and it is of extreme importance that they be kept separate. In seeking a criterion of singular termhood, we may be seeking (a) what it is to be a singular term; or (b) how we recognize singular terms.15

Perhaps no more can be said about the answer to (a) than that singular terms are those fragments of language that serve to denote objects. Perhaps the concepts of objecthood and singular termhood are so intimately connected that one cannot be explained without the other. Earlier I claimed that if the only criterion for singular

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15Hale [1984] points out these distinct formulations of the question.
termhood is that singular terms refer to objects then it would not be possible to defend (S) without circularity. But it seems that the circularity would be vicious only if we are unable to answer (b) without a prior understanding of what it is to be an object. If our answer to (a) is that singular terms are those terms that refer to objects, yet we are able to give an answer to (b) that does not presuppose that we are able to recognize that the term refers to an object prior to our recognition that the term is singular, then our criterion of singular termhood is not viciously circular. Put another way, our answer to (b) must not presuppose the notion of objectual reference because otherwise we would not be able to say which terms refer to objects independently of knowing what comprises our ontology, and (S) says that what comprises our ontology is a function of the expressive resources of a particular theory and particularly, which expressions function as singular terms in that theory. But the answer to (a) need not aid us in picking out those singular terms.

In [1983], Dummett writes:

Now Frege’s use of the ontological term ‘object’ is strictly correlative to his use of the linguistic term ‘proper name’: whatever a proper name stands for is an object, and to speak of something as an object is to say that there is, or at least could be, a proper name which stands for it. The question naturally arises in which realm, the linguistic or the ontological, the primary principle of classification is to be applied.... If we leave the application of the term ‘proper name’ to be determined by intuition, then it remains uncertain at which level, the linguistic or the ontological, this intuition operates: are we forced to count numerical terms as proper names, in Frege’s sense, because our intuition compels us to recognize numbers as objects; or is it, rather, the other way round, namely that we are forced to count numbers as objects because our intuition compels us to recognize numerical terms as proper names? (pp. 55 - 6)

Frege’s answer (and mine) is that we count numbers as objects because of the role numerical expressions play in true sentences; i.e., that they function as proper names. But, as Dummett notes, we cannot leave the application of ‘proper name’

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16 Again, this point is made by Hale in [1984].
(or 'singular term') at an intuitive level -- we have to be able to answer (b); to specify criteria of recognition for singular terms.

To defend (S), we must find a way to answer (b) that does not presuppose the notion of objecthood. We need not answer (a) in such a fashion. Of course, the answers to (a) and (b) must somehow be related, but they do not have to be the same. And we avoid circularity if our answer to (b) allows us to determine which expressions function as singular terms independently of our recognition that those expressions refer to objects. If we invoke the inferential role of an expression to answer (b), then we can specify the criteria independently of this recognition. We can then say that the reason that certain inferences are licensed is because the expression refers to an object, but this is not circular because we are not trying to answer (a); i.e., we are not trying to say what it is, generally, to be a singular term. What it is to be a singular term is to refer to an object. Given this, we are able to recognize singular terms by the inferential role of those expressions.

In [1983], Dummett attempts to give criteria that will answer (b). He begins with the insight that singular terms function grammatically as subject terms. But the grammatical role of a term, alone, will not suffice, for words such as 'nothing', 'something', and 'everything' often function grammatically as subject terms. Dummett thus proposes the following three criteria:

17 But the relation between them cannot be specified in such a way that it throws us back into the circularity objection. See below.

18 Dummett does not explicitly distinguish between (a) and (b), but given the criteria he proposes, it is most charitable to read him as answering (b).

19 Dummett [1983], pp. 59 - 60. I added 'atomic' to avoid objections by Linda Wetzel in [1990].
a is a singular term iff

(i) For any atomic sentence ‘A(a)’ the inference from ‘A(a)’ to ‘There is something such that A(it)’ is valid.

(ii) For any atomic sentences ‘A(a)’ and ‘B(a)’, the inference from ‘A(a)’ and ‘B(a)’ to ‘There is something such that A(it) and B(it)’ is valid.

(iii) For any atomic sentence ‘It is true of a that A(it) or B(it)’, the inference from that sentence to ‘A(a) or B(a)’ is valid.

(1) excludes ‘nothing’, ‘nobody’, ‘none’, etc., (2) excludes ‘something’, ‘somebody’, ‘some’, etc. and (3) excludes ‘everything’, ‘everybody’, ‘all’, etc. Plurality quantifiers such as ‘several’, ‘many’, ‘most’, etc. are excluded as well. By his own admission, however, Dummett’s criteria fail to exclude indefinite noun phrases when they occur as grammatical complements. For example, from the sentences ‘George is a policeman’ and ‘Henry is not a policeman’ we may infer ‘There is something which George is and Henry is not.’ Dummett locates the problem in the level of generality (i.e., the above generalization is a second-order generalization), and proposes a solution. However, all that is needed is to add the following fourth condition:

(iv) The conclusion of an inference of the sort described in (i) and (ii) is never such that requesting a specification can lead to a point at which the demand for a further specification, even if grammatically well-formulated, would be rejected as evincing a misunderstanding of the conclusion.

20 ‘several’ is excluded by (2), ‘many’ and ‘most’ are excluded by (3).

21 Hale [1979] gives a counterexample to Dummett’s characterization of the distinction between first and second level generality, and proposes a way to amend (i) - (iii) to fix the problem. Hale’s emendation basically respects Dummett’s solution, but rephrases it in such a way to avoid the counterexample. Wright [1983] (p. 62) proposes a simplification of Hale’s proposal, and it is basically Wright’s version which is quoted below. I have slightly modified Wright’s version to accommodate an objection made by Linda Wetzel in [1990]. (Wright’s version said “although grammatically well-formulated” instead of “even if grammatically well-formulated”.)
So even though we can infer ‘There is something which George is and Henry is not’ from ‘George is a policeman’ and ‘Henry is not a policeman’, were we to ask “Which one?” our question would be rejected as “evincing a misunderstanding of the conclusion.” It makes no sense to ask which policeman George is and Henry is not.

One criticism that has been raised against Dummett’s criteria is that they are language-specific. The criteria allow us to characterize singular termhood in English, but what about German or Swahili? Dummett himself does not answer this particular objection, but given our distinction between (a) a general criterion of what it is to be a singular term and (b) a criteria of how to recognize a singular term, the answer is evident. The criteria of recognition must be language specific. But that does not mean the criterion of what it is to be a singular term is language specific. What it is to be a singular term is to refer to an object. And given any language, we should be able to give criteria of the sort Dummett proposes for recognizing singular terms in that language. There is no way to give general, language neutral conditions for picking out singular terms other than the general statement that singular terms can be identified by their inferential role. How exactly we are able to recognize those terms that play the appropriate role will depend on the language in question and how it works.

Earlier, I noted that there must be some relation between the answer to (a) and the answer to (b). The criteria for picking out singular terms must not completely ignore the specification of what it is to be a singular term. Specifically, it seems that the answer to (b) must somehow be justified by the answer to (a), for it seems that we need some reason to back up the claim that the answer to (b) really does pick out singular terms. But if the answer to (a), namely, that to be a singular

\[22\] E.g., by Wright [1983].
The term is to refer to an object, somehow justifies the criteria for picking out those fragments of language that actually are singular terms, then perhaps my answer is circular. For it seems that we have no independent means of verifying that the criteria are correct other than checking to see if the terms that meet the criteria actually refer to objects. But to do this, we must have a prior notion of what it is to be an object, and thus we seem to have come full circle. On the other hand, if the answer to (a) had not relied on the notion of objecthood, this objection would not have arisen.

The above objection seems very compelling. But it misses an important fact. We have paradigmatic cases of singular terms.\textsuperscript{23} Proper names in ordinary language are prime examples of such paradigmatic cases. We can use these expressions and the inferences we license regarding them as the guideline. We can use proper names in ordinary language as our test cases, and see if the criteria measure up. If the test cases provide the sort of verification we are seeking, then we can go on and use the criteria in other areas, where the terms in question are not paradigmatic. And it should be evident to the reader that Dummett’s criteria, suitably amended by condition (iv), are verified by paradigmatic cases of singular terms.

It seems, then, Dummett has given us an adequate specification of how to recognize singular terms. The goal is to use his criteria in connection with (S). However, Dummett’s criteria are not immediately applicable in connection with (S); they are in need of revision if we are to make use of them. Dummett’s conditions specify that for any sentence ‘A(a)’ the inference from ‘A(a)’ to ‘There is something such that A(it)’ is valid.\textsuperscript{24} But the thesis I am defending is that objecthood is relative

\textsuperscript{23}A similar point is made in a similar context in Hale [1984].

\textsuperscript{24}Analogously for the other criteria. The point is that the criteria single out expressions that function as singular terms in all contexts. Dummett does say "...the
to a theory. Frege's Julius Caesar problem was solved by pointing out that 'two' sometimes functions as a singular term and '... is two,' in other contexts, is predicative in nature. Thus, the criteria we need must recognize that an expression can function as a singular term relative to certain contexts, while functioning as a predicative expression (or some other sort of expression) relative to other contexts.

Ideally, what we would want to do is relativize Dummett's criteria to a theory and amend each condition as such. Relative to a diminished theory, certain inferences will go through that would not go through relative to more encompassing theory. If we actually confine ourselves to the expressive resources of a particular theory, certain questions requesting further specification will not even be askable. Recall that the fourth condition was included to rule out the problem of second-level generality. But it can also be used to show how expressions can function as singular terms relative to a theory. For example, suppose that we have confined ourselves to the expressive resources of political theory. Certain inferences regarding 'President' (qua type from the more encompassing theory) would go through, so the term would satisfy conditions (i) - (iii). Given the assumption that we are confined to the expressive resources of political theory, one cannot even ask questions regarding differences between tokens of the type President, for the fragment of language in question cannot express differences between them. The point is that only certain questions are askable within a fragment of discourse, and only those questions would be relevant to deciding which terms are singular. Any

criteria we are seeking should determine whether or not an expression is a singular term in a particular context...'' (Dummett [1983], p. 59; emphasis mine), but his criteria do not fulfill this goal.

I realize that this seems extremely counterintuitive, for it seems that such questions are always askable, even if asking the question shows a lack of understanding. I ask the reader to be patient, for I will eventually reject this method of relativization as the actual way to recognize singular terms.
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The above method of relativization is highly idealized. In order to make use of it, it is necessary to assume that we actually have confined our discourse to the expressive resources of some theory. In ordinary discourse, however, often it is not at all clear which theory we are “speaking in”. Although in principle the above method would accomplish the relativization needed for (S), it is desirable to find a less idealized way to amend Dummett’s criteria. The goal is to relativize the criteria in such a way so that we can make sense of ordinary discourse without artificially breaking it up into distinct fragments. In [1987] Hale offers a suggestion that will accomplish this goal. Hale suggests that we amend Dummett’s criteria so they speak of uses of a term. The suggestion is driven by the two sentences:

(a) The whale is increasingly scarce
(b) The whale is much improved today

where (b) is spoken by the veterinarian of a sick whale. We cannot infer from (a) and (b) that

(c) There is something such that it is both increasingly rare and much improved today.

It is clear that ‘the whale’ used in (b) is a singular term. The structuralist would want to say that there are constructions similar to (a) in which ‘the whale’ is used as a singular term, too (although (a) itself is probably not one of these). Hale’s point is that given Dummett’s criteria, the fact that we could not infer (c) would disqualify

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26 Hale does not share the view that objecthood is theory relative, so his concerns are different. (His motivation for the relativization is discussed briefly below.) Nevertheless, his revision will solve the problem at hand.

27 Hale [1987], p. 18. The example is originally given in Wright [1983], p. 60.
‘the whale’ as a singular term because of the stipulation that we must be able to infer a sentence such as (c) from any sentence involving that term.

Hale thus suggests the following, which is a relativization of Dummett’s criteria to uses of a term:

- **a** functions as a singular term in a use of an atomic sentence \( A(a) \) iff
  - (i') in that use of \( A(a) \), the inference from ‘\( A(a) \)’ to ‘There is something such that \( A(\text{it}) \)’ is valid.
  - (ii') in that use of \( A(a) \) and some use of some sentence \( B(a) \), the inference from ‘\( A(a) \)’ and ‘\( B(a) \)’ to ‘There is something such that \( A(\text{it}) \) and \( B(\text{it}) \)’ is valid.
  - (iii') in that use of \( A(a) \) and some use of some sentence \( B(a) \), the inference from ‘It is true of \( a \) that \( A(\text{it}) \) or \( B(\text{it}) \)’ to ‘\( A(a) \) or \( B(a) \)’ is valid.\(^{28}\)

Hale’s amended version accomplishes the relativization we are seeking, for it is context sensitive. Inferences concerning the objects of a particular theory will be legitimate only in contexts where the speaker is using an expression such that relative to that theory, the expression functions as a singular term. For example, only in a context in which we treat the species whale as an object will the inference to ‘There is something such that...’ concerning “the whale” go through.\(^{29}\) And, in connection with condition (iv), only in contexts where the speaker presupposes the resources of a more encompassing theory will questions regarding which whale make sense. Suppose, for example, we are speaking of an individual whale and the following sentences are uttered:

- (a) The whale swam around the tank.
- (b) The whale is much improved today.

\(^{28}\) For extended discussion and defense of these relativizations, see Hale [1987], pp. 18 - 21. There is no need to alter condition (iv). Again, the ‘atomic’ is added to avoid an objection by Linda Wetzel in [1990].

\(^{29}\) That is, when we are not talking about individual whales.
From (a) and (b) we can infer:

(c) There is something such that it swam around the tank and is much improved today.

The question, "Which whale swam around the tank and is much improved today?" is intelligible. We can answer the question: "Shamu." On the other hand, suppose we were talking about the object whale:

(d) The whale is studied in biology classes.

(e) The whale is a mammal.

From (d) and (e) we can infer:

(f) There is something which is studied in biology classes and is a mammal.

But were someone to ask for further specification as to which whale we mean ("Which whale is studied in biology classes and is a mammal?") , the question would be rejected as a misunderstanding. When referring to the species whale, we cannot discriminate any further. Only certain questions concerning individuation make sense and any other such questions would be rejected.

We thus have an adequate specification of singular termhood which recognizes that particular fragments of language can function as singular terms relative to certain contexts while functioning otherwise in other contexts. Furthermore, we have a specification that allows us to make sense of ordinary discourse and its objects without artificially breaking it into distinct fragments. By relativizing the criteria of singular termhood to the use of an expression, we can consistently say that in certain contexts a term functions as singular, while in other contexts it may function differently. And that is what (S) required.
The principle (S) says that a coherent theory T characterizes a structure, and if an expression functions as a singular term in true sentences relative to T, then that expression refers to an object. But what is it for a sentence to be true?

Suppose that we invoke a naive correspondence theory and say that a sentence is true if and only if the world actually is the way the sentence says it is. Take, for example, the sentence 'The cat is on the mat.' Now, according to (S), there is an object denoted by 'the cat' if 'the cat' functions in true statements, suitably relativized. Now, how will we know that the sentence 'The cat is on the mat' is true, if in fact it is? The sentence is true, according to the naive correspondence theory, if and only if the cat actually is on the mat. So to verify its truth, we go and see if the cat is, in fact, on the mat. But to do that, we must already know that 'the cat' refers to an object; to assign a truth value to a sentence, we check to see if the object referred to has the property the sentence says it has. Thus, to know that the sentence is true we must already know that 'the cat' refers to an object. It seems, then, that using a naive correspondence theory involves us in circularity. According to (S), any singular term that functions in true sentences refers to an object, but to know that the sentence is true we must already know that the term refers to an object. Perhaps the problem could be solved by invoking some other theory of truth. But it seems evident that the same type of problem will arise -- given any specification of what it is for a sentence to be true, we will have to know what the subsentential parts of the sentence refer to in order to determine its truth value.

The above problem seems extremely damaging to (S). It may be thought that the way out is analogous to the answer to the similar objection in §2. When discussing the criteria for singular termhood, I made a distinction between what it is
for an expression to be a singular term and how we recognize singular terms. Here, we could invoke a similar distinction and separate the metaphysical question "What is it for a sentence to be true?" from the epistemological question "How do we know that a sentence is true?" If we can answer the epistemological question without invoking the notion of objecthood then it would seem that the problem could be solved. However, not all of this is needed.

The inclusion of the clause 'in true sentences' in (S) is needed to rule out expressions that function as singular terms only in false sentences\(^{30}\) such as 'Sherlock Holmes'. It may be the case that in order to know that the sentence is true and thus to apply the criteria from whatever theory of truth we choose, we must know to what its subsentential parts refer.\(^{31}\) But this is not damaging to (S).

Suppose \(a\) is a singular term (and we are able to determine this using the criteria discussed above). Then, if \(a\) refers at all, it refers to an object. Returning to the example used above, consider the sentence 'The cat is on the mat.' 'The cat' is a singular term, so its function is to refer to an object. It does refer to an object if and only if the sentence (or some sentence containing it) is true. So we use the criteria from our favorite theory of truth to determine the truth value of the sentence, knowing that if the sentence is true there must be an object denoted by 'the cat'.

The point of the above paragraph is to show that the inclusion of the phrase 'in true sentences' does not involve (S) in circularity even if the notion of truth presupposes that we already know that the subsentential part must refer to an object for the sentence to be true. If a term is functioning as a singular term in the use of a

\(^{30}\)Or true sentences whose truth requires that the object not exist (e.g., negative existentials).

\(^{31}\)I will not take a stand on a theory of truth. It seems that most any theory of truth is compatible with (S), for we need not specify what it is for a sentence to be true in order to defend (S).
sentence then we already know that its function is to refer to an object. It *does* refer to an object if the sentence (or some appropriate sentence containing it) is true, but it is allowable that the notion of truth invoked presuppose that *if* a term has a reference, then it refers to an object.

As things presently stand, however, (S) appears to be open to a telling objection. Suppose we construct a coherent theory in which ‘Sherlock Holmes’ functions as a singular term. Suppose further that we adopt some theory of truth in which sentences containing ‘Sherlock Holmes’ come out true. Given (S), wouldn’t we be forced to admit Sherlock Holmes into our ontology? There seems to be nothing in (S) as it now stands to rule out such fictional objects. If this is right, then it seems that (S) is going to admit far more into our ontology than most would be willing to accept. It is one thing to admit mathematical objects into our ontology, but quite another to admit Sherlock Holmes.

It seems, then, that I either need to take a stand on what it is for a sentence to be true or to amend (S) in some way. Consider the former alternative: what sort of theory of truth would I need to embrace? It seems that any theory of truth that rules out fictional entities would be adequate to overcome the objection. So the goal would be to rule out those theories of truth that “get it wrong”. The question I need to ask, then, is “On what theory of truth *could* appropriate sentences (i.e., sentences other than ‘Sherlock Holmes does not exist’ and ‘Sherlock Holmes is a fictional detective’) come out true?” Perhaps such sentences could come out true on a naive coherence theory. But any sophisticated coherence theory will be designed to accommodate such objections. In fact, *prima facie*, it is difficult to imagine any sophisticated theory of truth upon which appropriate sentences containing ‘Sherlock Holmes’ could come out true.

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32 E.g., the one Bertrand Russell criticizes in [1912].
Holmes' are true. Nonetheless, I will grant that it is possible. Given the possibility, I propose that the second alternative is the better solution. I will amend (S) in such a way so that it puts constraints on what theories are acceptable.

Two paragraphs back, I pointed out that there is a difference between admitting mathematical objects into our ontology and admitting Sherlock Holmes. Why is this? Mathematical theories as a whole have a certain kind of explanatory power. Mathematical statements figure in our best explanation of the world in that they play an essential role in scientific discourse. A theory involving fictional entities like Sherlock Holmes, on the other hand, does not. So I propose that we amend (S) in such a way that it puts the following constraint on theory acceptance: the theory in question must have some sort of explanatory power.

Notice that I phrased the constraint in such a way that it only requires that the theory have some sort of explanatory power. Mathematical theories aid us in our scientific exploration of the world, but we need not confine "explanatory power" to those theories used in science. The notion of explanatory power is deep and controversial. Invoking this notion and not restricting it to scientific explanation immediately brings up a number of difficult and interesting questions: What is it to explain something? When is an explanation adequate? Does something have to be in the causal nexus to have explanatory power? And so on. Unfortunately, it would take an entire dissertation to adequately address these issues. However, some elaboration is called for at this point.

The notion of an explanation is intimately tied to the notion of understanding. Explanations aid us in our understanding of the world, and a theory has explanatory power in the (perhaps weak) sense I intend if it so aids us. Any time we ask a why-question, we are seeking to understand something and thus seeking an
explanation. The world we live in is vast and complex and we ask why-questions about many things. We ask why the President has the power to veto a bill and why the pitcher stands on the mound. If one is able to answer such questions using the resources of a theory, then that theory has explanatory power.

It is often claimed that why-questions can only be adequately answered by a causal-explanatory explanation couched in scientific terms. But it seems clear that we ask many why-questions for which a scientifically specified causal answer would be inadequate. For example, suppose that I asked why the porch light is on. In asking this question, I may be seeking a causal-explanation, but I may not. In fact, the appropriate answer to my question concerning the porch light is context dependent. In 1977, van Fraasen considers the porch light example. He says:

... the question why the porch light is on may be answered “because I flipped the switch” or “because we are expecting company,” and the context determines which is appropriate... Which factors are explanatory is decided... by concerns brought from outside. (van Fraasen [1977], p. 324)

What question actually is asked by the why-question (or what answer is appropriate given my interests in asking the question) is wholly dependent on the context of the asking. Many different answers could be appropriate, depending on the context. Any explanation of the event that adequately addresses the why-question is legitimate.

So any theory that has explanatory power in the above sense meets the constraints on theory acceptance. Political theory aids us in explaining the political process in the United States. The theory of baseball defense aids us in explaining the strategies of the game. Neither may be of any use whatsoever in physics, but they still have a certain kind of explanatory power. Political theory and baseball defense would thus be two of the theories to which (S) is applicable. But any theory
that is completely devoid of explanatory power in the above sense falls outside the constraints.

The final revision of (S), then, is as follows:

(S) A coherent theory T characterizes a structure, and if T has explanatory power, then any expression that functions as a singular term in true sentences relative to T refers to an object.

§ 4. Advantages

In § 1 - § 3 of this chapter, I have clarified and explained the different clauses of (S). I have shown how (S) works and indicated the kinds of things that follow from (S). But why would anyone want to accept (S)? What advantages are there to holding this conception of objecthood? In this section, I will discuss some of these advantages.33

One of the main advantages of the notion of objecthood embraced by a structuralist is that it shows what it is for a particular area of discourse to be committed to a certain kind of object. The ontology of a theory T contains objects of kind \( k \) if and only if there is a structure characterized by T, and objects of kind \( k \) are the places in that structure. The ontology of that theory is \( k \)-type objects if there are singular terms in said theory that refer to \( k \)-type objects. If the theory is able, through its expressive apparatus, to discriminate in a more fine-grained fashion, then \( k \)-terms may function as complex predications. In Fregean terminology, what is an object from one perspective may be a function which takes objects as arguments from another.

33The serious objections to (S) will be considered in Chapter V. Here, I will just spell out some of the positive features of accepting (S).
The insight behind this conception of objecthood is that there are different ways to divide the world, and the way we do the dividing depends on the expressive resources of the language we use to do so divide it. The structuralist's notion of objecthood allows that a theory with finer grained discriminative apparatus may carve things up differently than a theory with coarser grained discriminative apparatus. It may seem that the structuralist is committed to a vast ontology, but to embrace this conception of objecthood is not ontologically excessive. The claim is not that, on top of all the things we see and touch in this world, there are other things such as Presidents and whales and numbers. Rather, the claim is that given what there is in the world, we can divide it in different ways. The structuralist gives every coherent, explanatory theory equal status. Each theory divides the world differently, but each is way of dividing is legitimate.

A second advantage of the structuralist notion of objecthood is that it offers a clear and precise way of clarifying what it is to be an object. As noted earlier, the question "What is the criterion of objecthood?" is often left unanswered in philosophical contexts, but is a question that needs answering. Philosophers often haggle over the existence of certain kinds of objects, and no ground rules are set. Often, the opponents in the dispute are talking past one another because they have different ideas of what it is to be an object. Until some consensus is reached, the disputes will go on and no headway will be made. The program embraced here offers a straightforward answer to the question, and a straightforward way to settle the disputes. The question "Are universals objects?" becomes "Is there an explanatory fragment of our discourse that characterizes a structure in which terms standing for universals function as singular terms?" "Is 'the good' an object?" becomes "Is there an explanatory theory in which 'the good' functions as a singular term in true sentences?"
The final, and perhaps most important, advantage of the structuralist conception of objecthood that I will mention is that it recognizes and accounts for the relativity of objecthood. The advantage of embracing the thesis of the relativity of objecthood was briefly discussed above in connection with the expressive resources of a language. But to show in more detail why this is an asset of the notion of objecthood being defended, consider for a moment what one would be committed to if one denied that objecthood is relative.

To deny the relativity of objecthood is to embrace the claim that there is one absolute fixed domain of objects. There are a number of problems with this claim. First of all, the defender of the one absolute fixed domain of objects must either (a) hold that we have no access to the domain or (b) explain how we do have access to it. (a) is a skeptical thesis that very well could be defended. I have no reply to the defender of (a), but I know of no philosopher who has successfully refuted skepticism. In the current climate of naturalistic philosophy, however, I think I will have allies in rejecting (a). As for (b), its defender must show either that (i) our access to the domain is theory-independent or (ii) that there are certain privileged theories. (i) is implausible, and (ii) requires that we legislate on which theories are privileged.

A further problem with the "one fixed domain of objects" view is that it ignores the lessons of mathematics. There is no set of all sets; to suppose there is such a set leads one to contradiction. Of course, it is possible to hold that the ontology of mathematics is comprised only of sets. One can embed every other

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34 This may seem to many to be a counterintuitive feature, and objections dealing with that issue will be discussed in the next chapter. However, a few words are in order here as to why it is an advantage.

35 Penelope Maddy is one proponent of this view. See [1987] and [1990].
mathematical theory in set theory, and understand the commitments of all of mathematics as just the commitments of set theory. However, embracing this view has significant drawbacks. It seems desirable to understand mathematical theories such as arithmetic, real analysis, and geometry on their own, independently of set theory. Intuitively, at least, it seems that each mathematical theory has its own subject matter and it is desirable to respect that subject matter.

The notion of objecthood at work here takes the intuitive plausibility of understanding mathematical theories on their own and extends it to every coherent explanatory theory. My opponent might claim that the extension is not plausible; i.e., that just because it is desirable to understand mathematical theories independently of set theory does not mean that it is desirable to understand, say, biology independently of physics. But it is my contention that there is no principled reason why the desirability does not extend to other theories. It is a working assumption of mine that it is an advantage of any philosophical position if it incorporates and illuminates the similarities between the various branches of our investigation into the world. It is an asset of a view if it shows that there is no significant difference in kind between mathematical theories and other scientific theories, and mathematical objects and other objects. And that is what the structuralist notion of objecthood does.
CHAPTER V

OBJECTIONS

In the previous chapter, I formulated and discussed the following principle:

(S) A coherent theory T characterizes a structure, and if T has explanatory power, then any expression that functions as a singular term in true sentences relative to T refers to an object.

I noted that (S) is open to many challenges, but I neither articulated nor responded to those challenges. In this chapter, I will attempt to address at least some of the challenges by considering the following question: How do we know that some theory T "really gets at the world"? In other words, how do we know that there really are objects of the kind required by T, and that the purported singular terms are genuinely referential?

The question articulated above asks for a justification of the principle (S) itself, for it challenges the idea that ontological questions can be settled merely by looking at syntactic features of the true sentences of our language. The proponent of the challenge asks us to consider the possibility that there are no entities of the sort that T posits even if T has singular terms of the relevant sort and even if sentences containing those singular terms are true. There are at least two ways that
an objector might dispute entities of kind \( k \) belonging to a theory \( T \): (a) Perhaps the theory \( T \) can be *reduced* to another theory \( T' \) and thus the objects of kind \( k \) are merely the objects of kind \( k' \); there are no objects of the kind \( T \) talks about because the true sentences of \( T \) can be translated into true sentences of \( T' \) and the ontology of \( T' \) does not contain the objects of \( T \). (b) Perhaps only *some* singular terms (that occur in true sentences) are genuinely referential. Even if singular-termhood can be syntactically specified, perhaps there are some singular terms that neither refer to actual objects nor can be shown to refer to other, ontologically respectable objects. In other words, perhaps syntactic singular termhood is not sufficient for semantic singular termhood.

I will consider the challenges associated with each version of the question noted above individually. In § 1, I consider the first version of the question and its reductionist objection. In § 2, I consider the "more to objecthood..." objection. Finally, in § 3, I discuss in more detail the objection to the theory relativity of objecthood mentioned briefly at the end of Chapter IV. In many cases the motivations for the different objections overlap, but each objection deserves its own hearing. On a cautionary note, I admit that there may be versions of the objections that have been overlooked. I have, however, attempted to present the most reasonable and compelling interpretation of each objection.

§ 1. The Reductionist Objection

The reductionist doesn’t want to accept certain kinds of objects into her ontology. Her response to the dubious objects is to attempt to explain away reference to them; to claim that apparent reference to the dubious entities is actually reference to other, ontologically respectable entities. There are endless examples of this type of move in philosophical contexts. In philosophy of
mathematics, we have the reduction of numbers to sets. In the philosophy of mind, we have type-type identity theories, which attempt to reduce types of mental states (i.e., mental objects) to types of physical states. We have analytical phenomenalism, which was an attempt to reduce talk of physical objects to talk of sense data. And so on and so on.

On the reductionist view, then, there are privileged kinds of entities. These privileged entities are the only ones we ought to accept into our ontology. Any singular terms of a theory that purportedly refer to entities not of the privileged kind and occur in true sentences can, in principle, be shown to refer to objects of the privileged kind.

In order to hold this view consistently, one must claim that there is more to be said about what objects really exist than what our theories tell us there are (unless one holds that one theory is (or some theories are) privileged; see below). Otherwise, there would be no grounds for claiming that certain entities are privileged. (I suppose that one could just say “I like them better,” but this would not warrant the philosophical conclusions drawn.) The objects that “really exist” are those privileged sorts of entities to which we attempt to reduce the non-privileged sorts of entities. Furthermore, a cogent defense of this position must include a claim that we know, or at least have strong grounds for thinking that we know just which objects are the ultimate constituents of reality, for those are the ones to which we presumably want to reduce the others.

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1 Although sets themselves are often considered equally as dubious (or even more dubious).
2 This attempted reduction was prompted more by epistemological considerations than metaphysical considerations.
3 Hallett [1990] makes this last point in a similar context.
One who holds the view under discussion must claim that we can look at the world independently of our language and theorizing, and somehow "pick out" the privileged entities. It is difficult to even discuss this thesis, for it supposes that we can look at the world independently of our conceptual scheme. But aren't all observations theory laden? The claim being considered requires that we can step outside of our language and "look at the world". This implies that there is some meta-position from which we can make judgments about the ultimate constituents of reality (or at least we could in principle) without the aid of our language and theories -- without such a meta-position, how could we know which entities are privileged and which are dubious? On what basis would we rule out various entities? Of course, one might claim that there are privileged entities but we are incapable of knowing which ones they are. This, however, is a very precarious position for the reductionist to take, for it would rob the reductionist of justification for his or her privileged entities. Since we could never know which entities are in fact the privileged ones, any choice of reduction base would be seemingly arbitrary.

There is a position which I have thus far ignored. One might claim that one theory is (or some theories are) privileged. They (to use a common metaphor) "carve reality at the joints" whereas other theories do not. This position would skirt the above objection, because it does not require that we look at the world from some meta-position. Instead, it requires that we look at the world from our preferred theory (or theories) and construe all talk of dubious entities into talk of entities of the preferred theory. For those in the throes of reductionism, this is actually the

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4See almost any work by Hanson, Kuhn, or Quine.

5Perhaps it does -- how else would we know which theory (or theories) are the privileged ones? I will, however, ignore this point and assume that this position has no such requirement.
common claim. The standard attitude is that physics tells us about the ultimate constituents of reality, and that it is the theory to which we want to reduce all others.

If we take the preferred theory to be physics, then the view being examined is a thorough-going reductionist physicalism. As J.D. Trout describes the program, reductionist physicalism is the thesis that the

central terms of "higher-level" theories, from biology to sociology, will ultimately be defined in terms of, and thus eliminated in favor of, the more basic vocabulary of physics.... (Trout [1991], p. 387.)

A strong physicalist program of this sort, then, would claim that we can (or will eventually be able to) explicate the content of all meaningful sentences of the dubious theories in physics; every sentence of the dubious theories is translatable into a sentence of physics. In principle, then, the dubious theories would be eliminable. Perhaps we get along better using the theories and their vocabulary, but such vocabulary is inessential to our explanation of the phenomena with which the dubious theories are concerned. Physics and its vocabulary can ultimately tell us everything there is to know about the world and can capture all of the relevant explanations of the non-privileged theories.

The thesis discussed above is highly dubious. Jerry Fodor has convincingly argued against this view; in [1974], Fodor argues that the goal of reducing all theories to physical theory is, in fact, unattainable. But even if we could

6 Fodor concentrates on the reduction of special sciences to physics, but his arguments actually apply to any and all attempts to reduce one theory to another. Thus if an objector has another preferred theory, Fodor's remarks will still hold. Fodor first points out that to preserve lawlikeness in a reduction, the bridge laws themselves must hold by nomological necessity. For this condition to obtain, the predicates of physics (P) and the predicates of the special science being reduced (T) must be coextensional as a matter of law. But there is no reason to suppose, according to Fodor, that for every natural kind predicate in T there will be a corresponding natural kind predicate in P. Fodor goes on to argue that even if we could find the necessary coextensional predicate (and only brute enumeration would convince us that we had found the right one), it is highly unlikely that this
accomplish the reduction, physics is incapable of expressing important features expressed in the other theories. For example, the expression “currency” is a natural kind predicate in economics. What it is to be currency is a matter of the social institutional role played by that type of object -- the physical realization of currency is not important from an economic point of view. Furthermore, currency can be physically realized in thousands of ways. It seems implausible to think that we could find some intrinsic feature common to all and only modes of currency so that we can reduce economics to physics. But even if we could find such a feature, the concept of currency is important for reasons other than its physical realization -- a predicate of physics would never correctly capture the concept. It is thus difficult to see how the central terms of the dubious theories could be defined in terms of and eliminated in favor of physical vocabulary. Thus, if the claim is that the content of every meaningful sentence of every theory can be captured in physical terms and that physics can adequately capture all of the explanations of every theory, then the physicalist owes us an answer to Fodor’s objection.

The foregoing argument addresses a strict reductionist view which requires that the central terms of the dubious theories can be defined in physical vocabulary. But perhaps the physicalist does not need this strong requirement; perhaps a weaker version of physicalism can avoid Fodor’s charges and yet be damaging to (S). For example, perhaps the claim is that even though certain concepts cannot be adequately captured in physical terms and even though we do not want to eliminate coextension would be lawlike. But since we are reducing laws of T to laws of P, the coextension must be lawlike; otherwise the bridge laws would not hold and the lawlikeness would not be preserved. Hence we would not have accomplished what we set out to do: we needed to show that the laws of T can be expressed as laws of P. Thus, according to Fodor, we should give up trying to reduce theories to other theories.
the dubious theories, the only legitimate objects there are are the objects of physics. Trout characterizes this type of physicalism as "the view that all phenomena are physical or entirely composed of physical phenomena." (Trout [1991], p. 387.) So, for example, this type of physicalist will agree that certain concepts of, say, economics cannot be adequately captured in physics, but ultimately every object of economic theory is a physical object. Even though currency can be realized in thousands of ways, each token of currency is a physical token which can be accommodated in physics.

Let's assume for the moment that the physicalist under consideration wants to accommodate of classical mathematics. Even to get the real number structure, he or she will have to postulate uncountably many physical objects (to get set theory into the picture requires far more, and the assumption begins to look ridiculous). While it may be the case that the world does contain this many physical objects, it is a dubious assumption upon which to rest the whole of mathematics. Nonetheless, let's make this assumption along with the physicalist. Given this assumption, the physicalist's claim is that every object of mathematics (and the objects of all other theories with dubious entities) can be reduced to a physical object. There are no mathematical objects on top of the physical objects to which they can, in principle, be reduced.

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7 Hartry Field is a physicalist who wants to be able to continue to make use of all of classical mathematics (see [1980] and [1989]), but his program differs from that discussed below. For discussion and criticism of Field's program, see Shapiro [1983b] and [1984], Maddy [1990], Chihara [1990], Resnik [1992], and most of the papers in Irvine [1990].

8 Suppose that the physicalist admits space-time points into his or her ontology. According to contemporary physics, we will then have continuum-many physical objects. If every collection of space-time points is also allowed into the ontology (which seems dubious on physicalist grounds), we will still only get the powerset of the original collection of space-time points. How, then, do we accommodate the rest of the cumulative hierarchy?
The structuralist's quarrel with the physicalist discussed above arises only over the reductionist strategy. A structuralist does not claim that on top of everything else in this world, there are numbers and whales and shortstops. The claim is rather that there is no one correct way of dividing the world into objects; any way that any coherent explanatory theory does the dividing is legitimate. So, given the assumption that there are enough physical objects to accommodate even higher set theory (which is highly dubious) a structuralist can agree that everything we actually divide is physical. The quarrel arises over the claim that every object can be reduced to an object of physics. The physicalist claims that the only legitimate objects are the objects of physics and that all other instances of purported singular reference can be shown to be reducible to reference to the objects of physics. The structuralist rejects this claim because it legislates on which theory "gets it right". On the structuralist view, there is no legislation on acceptable and unacceptable theories beyond the fact that they must be coherent and explanatory.9

But what about the physicalist who does not make the assumption that there are enough physical objects to complete the program? Even if the physicalist does not wish to accommodate set theory, he or she will have to assume that there are uncountably many physical objects. Without this assumption, one who holds that everything there is is physical cannot even accommodate real analysis, let alone all of classical mathematics, but his or her reply will be "So much the worse for classical mathematics." This type of physicalist is committed to the thesis that much of mathematics as practiced is in error, and this seems like an unfortunate consequence of the view. Nonetheless, the physicalist accepts this consequence and (probably) holds that we ought to revise the current methodology and practice of mathematics.  

9For further discussion of the illegitimacy of such legislation, see the next section.
Normally, the physicalist supports his or her view with the type of anti-realist objections to realism discussed in Chapter II. I showed there that these objections can be met by a structuralist. We can account for knowledge of and reference to mathematical objects. The physicalist who still refuses to countenance mathematical objects owes us some reason for thinking that we ought to give them up.

I hope that I have convinced the reader that the reductionist objection is not compelling. But I have one final thing to say about the reductionist strategy. Putting the above arguments aside for the moment, suppose that we actually could formulate the required bridge laws between some theory (T) and physics (P). Furthermore, suppose that we could capture the relevant features of T in P pace Fodor. Finally, suppose that we could find some legitimate reason to legislate on acceptable theories and we could support the claim that physics is the acceptable theory. If we could accomplish all of this, we would have formulas (actually, equivalences) reducing T to P, where the left-hand side consists of a sentence of P and the right-hand side consists of a sentence of T. But now the question arises as to what entitles the reductionist to view the left-hand side as primary? Instead of claiming that the equivalence shows that there are really no objects of the sort T requires, why not claim that the left-hand side of the equation actually contains veiled reference to the purported dubious entities? The point is that even if we could formulate reduction sentences of the kind necessary, the claim that we ought to banish T-objects from our ontology requires further argument. It is not enough to show that we can form the equivalences; the reductionist has to support the claim that we ought to give up the objects referred to on the right-hand side.

\footnote{Crispin Wright makes this point in [1983], [1988], and [1990]. He attributes the original insight to W.P. Alston.}
The initial reply of the reductionist may be "Well, we ought to give up the objects referred to on the right-hand side because everything is physical." But this just begs the question. The challenge is to show just why we ought to think everything is physical and thus why we ought to reduce everything to physics.

In this section I have discussed the reductionist objection to (S). I do not pretend to have refuted the objection, but I hope to have given the reader sufficient reasons to doubt its force.

§ 2. The "More to Objecthood..." Objection

The thesis defended herein is that particular theories characterize a structure, and the objects of the theories are implicitly defined when the structure is so characterized. Expressions that function as singular terms relative to that theory denote objects, which are just as "real" as tables and chairs. The objection considered in this section is that there is more to being an object than being the referent of a singular term relative to a particular theory. My opponent will claim that there are some (true) sentences in which expressions seem to function as singular terms, but that there are no objects that serve as the referent of those expressions. The opponent will favor a privileged ontology, but the strategy will not be strictly reductionist. Rather, the claim is that syntactic singular termhood is not sufficient for semantic singular termhood.

To see how the objection works, consider the following sentences:

(i) The whale is a mammal.

Suppose that (i) is found in a biology book, where the context makes it clear that there is no particular whale being discussed. Biology is a coherent theory that characterizes a structure. It divides animals into classes and subclasses which are
specified by the relations between them, and these subclasses are its objects.

Relative to biology, whale is an object.11

Let me put two more examples on the table before turning to the objection.

The following sentence was discussed in Chapter I:

\[(ii) \text{ The President has the power to veto a bill.}\]

The political theory of the United States characterizes a structure, and President is one of its objects.

\[(iii) \text{ The shortstop stands between second base and third base.}\]

The theory of baseball defense characterizes a structure with nine positions.12 Each place in the structure is defined by its relations to other places in the structure and the role it plays in the overall defensive strategy of the game. Shortstop is, relative to baseball defense, an object.

On the structuralist view, whale, President, and shortstop are all objects in their own right. Relative to the theory in which singular terms referring to them are embedded, they are objects which we freely allow variables to range over. In each case, relative to a more encompassing theory the expressions are functions which take other objects as arguments: "x is a whale," where x can be replaced by the name of an individual whale; "x is/was a President," where x can currently be replaced by any of 42 different actual individuals and result in a true sentence; and "x is a shortstop," where x can be replaced by the name of any person who plays the role of shortstop in a game of baseball.

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11 The suggestion that species are the objects of biology is suggested in Kraut [1980].

12 Recall that I am using the word 'theory' to mean something along the lines of an area of discourse with conceptual homogeneity.
The opponent will say that the purported singular terms in sentences (i) - (iii) do not refer to objects, but rather the class of individuals that fill the particular roles in question. (i) should be rephrased so as to speak of all whales instead of the object whale, (ii) should be rephrased so as to speak of all Presidents, and (iii) should be rephrased so as to speak of all shortstops. The opponent favors a privileged ontology that does not include the objects whale, President, and shortstop. But the strategy is not strictly reductionist, for he or she is not trying to reduce biology, political theory, and the theory of baseball defense to an ontologically more respectable theory. Rather, the opponent is saying that these dubious “objects” just don’t exist.

The opponent is claiming that some but not all ways of carving up the world get at the real objects. But the question is “Why?” It is incumbent upon the opposition to tell us just what the privileged objects have and the opposed ones lack that lends support to this position. In [1992], Alan Sidelle makes this point in connection with a “dubious object” which he calls “Prez”. Prez is similar to the structuralist’s object President of the United States. Prez is composed of, but not identical to, all of the people who have held the office of President. Sidelle says:

While there are many differences between people and office people, it is very questionable whether any of these has the right sort of metaphysical significance. 13

I will consider three possible ways the opponent might respond. Ultimately, however, as I will show, none of the three responses justify the rejection of the purported dubious objects.

13 Sidelle [1992], p. 418. Sidelle does not argue against the opponent of privileged entities, but rather shows that he or she is committed to a certain principle of rigidity across possible worlds that requires further argument.
§ 2.1. Epistemological Considerations

One response might go as follows: we come into cognitive contact with individual whales, individual shortstops, and individual Presidents. The epistemic explanation of our knowledge of these objects is clear. However, we do not causally interact with the structuralist's "objects" whale, shortstop, and President.

In Chapter II, I argued that even though we cannot directly causally interact with numbers, we can give a satisfactory explanation of why our beliefs about numbers are justified from the perspective of a reliable process epistemology. I contended that beliefs about small finite mathematical structures are acquired through causal interaction with empirical tokens of the type of structure in question. I discussed what kind of psychological processes are involved in the acquisition of these beliefs and in the later recognition of similar tokens. Finally, I argued that the processes in question are reliable. On a reliable process theory, this is adequate for justification.

We can explain the reliability of the belief-forming processes that result in beliefs about shortstop, whale, and President in a relevantly similar way because we are capable of causally interacting with empirical instances of structures of which these objects are a part. Of course, there are almost certainly different processes and mechanisms at work and it would be unreasonable to claim that our beliefs about shortstop, President, and whale are acquired and justified in exactly the same way as our beliefs about mathematical objects. But the root of the epistemological objection to both mathematical objects and the other types of objects under consideration is the same. In each case, the objection centers on the fact that we cannot causally interact with the objects in question. Since causal interaction is taken by many to be a prerequisite of knowledge, it is doubted that an adequate
explanation of the justification of our beliefs concerning those objects could be forthcoming. But in Chapter II, I argued that causal interaction with tokens of the appropriate type is enough to confer justification.

As noted above, it would be unreasonable to claim that the justification of our beliefs concerning all of the types of objects under discussion works the same way as the justification of our beliefs concerning mathematical objects. But what does seem reasonable to claim is that a similar story can be told for each of these types of objects. The root of the objection is the same in each case (i.e., lack of causal interaction with the dubious objects) and that part of the objection has been addressed. As long as we can causally interact with tokens of the appropriate type, it is reasonable to claim that we can acquire justified beliefs about the type itself. And that can be done with the objects shortstop, President, and whale -- we are capable of encountering empirical instances of systems that have the appropriate structure. This, of course, is only where the story begins. In order to explain just how we acquire the relevant beliefs, I would have to go on to explain just how those beliefs are formed and why they are justified. It would, however, be impossible to give an account of the processes by which our beliefs concerning every kind of object are formed.

My claim, then, is that our knowledge of the structuralist’s “dubious” objects begins with causal interaction with tokens of the type of structure in question. From there, the story would proceed with an explanation of the processes by which the beliefs about the structure itself are formed and why said processes are reliable. This explanation, of course, makes the empirical instances epistemologically prior. The opponent might seize on this fact and use it to discredit the controversial objects. Since our knowledge of the dubious objects is, at least in some cases, dependent (in some sense of “dependent”) on our knowledge of the non-dubious
objects, we have an answer to the question regarding why some ways of carving up the world are correct and others are not. The opponent would respond to this question by stating that the *correct* ways of carving are those which respect our epistemic access to the world.

The above response sounds plausible. There is, however, a disputable assumption lurking underneath the response; namely, that epistemic priority *entails* privileged metaphysical status. But now the question arises as to why we should accept the principle: *why* does epistemic priority entail privileged metaphysical status? Why think that the privileged entities are just those with which we have primary epistemic contact? One who holds this view owes us some reason to accept this assumption, for the claim that the ultimate constituents of reality are just those things that we have primary epistemic access to is, at the very least, controversial.

Furthermore, if we accept the controversial assumption, then we would be committed to the claim that medium-sized physical objects are *the* privileged entities. Medium-sized physical objects are the objects with which we come into contact everyday, and are arguably the ones we learn about first. One who adopts this view, then, faces objections from two sides: those, like me, who think that there is more than one correct way to divide the world *and* thorough-going physicalists, who hold that the ultimate constituents of reality are the objects of physics. The objects of physics are not those that we encounter everyday, and although (given certain observational instruments) there is some observational data to justify our beliefs concerning physics, it is not the case that we have *primary* epistemic access to the objects of physics.

In this section, I have considered one possible response to the question, “Why think that only some ways of carving up the world get at the real objects?” One
might think that certain kinds of epistemological considerations lead to the conclusion that not all of the structuralist's "dubious" objects are legitimate. But we do have at least the beginning of an adequate explanation of how our beliefs about these objects are justified. Even though the explanation of the justification is dependent on our knowledge of objects with which we causally interact, I hope to have shown that it does not immediately follow from the fact that certain objects are epistemologically prior that they are also metaphysically prior.

§ 2.2. Metaphysical Considerations

The opponent might claim that the desire for ontological economy justifies rejection of the dubious entities. Even though 'whale', 'President', and 'shortstop' function as singular terms in sentences (i) - (iii) above, these expressions do not refer to objects, but rather classes of individuals. We can (and ought to) rephrase the sentences so that they speak of all Presidents, all whales, and all shortstops when determining our ontological commitments. Not to do so is ontologically excessive.

The parenthetical remark in the last paragraph is crucial to the objector's position. He or she must claim that since we can rephrase the sentences, we ought not to countenance the objects. Put another way, the objector is claiming that something like the following principle ought to be accepted:

We ought to accept only those kinds of entities into our ontology that are essential in making sense of one or other of the many kinds of utterances we make.\footnote{This a slightly altered version of a principle Kirkham argues against in \cite{1992}, pp. 330 - 1.}

Since we can make sense of utterances in which 'President', 'whale', and 'shortstop' seemingly function as singular terms without countenancing objects like President, whale, and shortstop (i.e., by rephrasing them in such a way that they are predicative...}
expressions which speak of all members of the class to which the predicate is applicable), we ought not to countenance those objects.

But does this type of move always legitimize rejection of kinds of entities? Do we want to claim that every time we can rephrase a sentence so as to eliminate reference to a type of object, we ought to reject those objects? Clearly not.

Consider the following: 15

(a) There are two distinct people, each being a parent of John and each being a parent of Mark.

(b) John is a sibling of Mark.

(b) makes no reference whatsoever to parents; we have eliminated reference to parents as types of objects. Suppose that we could eliminate parental reference in the same way in other contexts. It would not then follow that we ought to reject parents as types of objects, even though the foregoing principle seems to endorse said rejection (i.e., since accepting parents as objects is not essential in making sense of (a)). So what the objector needs is some principle that allows us to distinguish the rephrasings of (i) - (iii) above from (a) and (b). Why is it legitimate to claim that we ought to reject President, whale, and shortstop yet not legitimate to claim that we ought to reject parents?

The objector might re-trench. (a) and (b) are not analogous to (i) - (iii), he or she might claim, because ‘parent’ is not functioning in (a) as a singular term. In order to adequately address the objection, I need to show that there are cases when we can rephrase sentences with purported singular terms into sentences where said terms are functioning as predicative expressions yet it is clear that we do not want to reject the objects in question. In other words, my goal is to find a case where the

15 The following example is discussed in the Introduction to Irvine [1990] in a somewhat different context.
same sort of move as the rephrasings of (i) - (iii) can be made, yet even the objector would not want to give up those kinds of entities.

Given this goal, it becomes important to decipher what, exactly, is going on in the rephrasings of (i) - (iii). In each case, the sentence containing the purported singular term is recast in such a way that the expression in question becomes a general term. So the claim might be as follows: whenever we can rephrase sentences containing an apparent singular term so that the expression is a general term, there is no need to accept entities seemingly denoted by that expression. The general term is true of one or more individuals (i.e., it can be truly predicated of them), but there is no object that the expression refers to when it is apparently functioning as a singular term.

The problem with this version of the objection, however, is that we can eliminate all singular terms in the same way as we eliminated the purported singular terms in (i) - (iii). Quine has given us a uniform method for regimenting all singular terms into predicative position. He writes:

What was in words 'x is Socrates' and in symbols 'x = Socrates' is now in words still 'x is Socrates' but the 'is' ceases to be treated as a separate relative term '='. The 'is' is now treated as a copula which, as in 'is mortal' and 'is a man', serves merely to give a general term the form of a verb and so suit it to predicative position. 'Socrates' becomes a general term that is true of just one object, but general in being treated henceforward as grammatically admissible in predicative position and not in positions suitable for variables. It comes to play the role of the 'F' of 'Fa' and ceases to play that of 'a'. 16

Given any singular term whatsoever, we can rephrase the sentences in which it occurs in such a way that the expression becomes predicative in nature. Do we want

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16 Quine [1960] p. 179. See also the papers "On What There Is" and "Meaning and Existential Inference" in Quine [1953].
to reject, say, Socrates as an object merely because we can eliminate singular terms referring to Socrates in our utterances? I think not.

Once again, however, the objector might re-trench. In the case of Socrates, when we rephrase sentences in which 'Socrates' is functioning as a singular term into sentences where 'Socrates' functions as a predicative expression, the resulting predicative clause is true of one individual. But in the cases of sentences (i) - (iii) above, the resulting predicative clauses are true of many individuals. In other words, the 'x' in 'x is Socrates' can only be replaced by an expression denoting one object and result in a true sentence, but the 'x' in 'x is a shortstop' can be replaced by expressions denoting any number of objects and result in a true sentence.

The structuralist's reply to this objection is that in both cases, how many individual objects can serve as the value of the variable is relative. In the case of the shortstop example, from the background perspective, many individuals play the role of shortstop. But relative to the theory of baseball defense, one object plays that role. In the case of Socrates, from one perspective, the predicative clause is truly applicable to only one individual. But from a different perspective, many individuals can play the role of Socrates. For example, consider a theory which treats time-slices as basic. From this perspective, each and every time-slice of Socrates can serve as the value of the variable in the open sentence 'x is Socrates'. Once again, the strategy in question does not serve to distinguish sentences (i) - (iii) from other sentences where we clearly would not want to reject the entities in question.

I have considered a number of different ways of construing the objection that if we can eliminate reference to kinds of objects when determining our ontological commitments, then we ought not to accept those objects. I have attempted to show
that the different strategies for such elimination lead us to reject objects that we clearly would not want to reject. But there is one last point I wish to make in this section. I have thus far ignored the "ontologically excessive" portion of the objection. Isn't the structuralist principle (S) incredibly permissive in terms of ontological acceptance? Isn't Ockham's razor called for here?

As I have stated elsewhere the claim is not that, on top of all the things we see and touch in this world, there are other things such as Presidents and whales and shortstops and numbers. Rather, the claim is that given what there is in the world, we can divide it in different ways. Each coherent, explanatory theory divides the world differently, but each way of dividing is legitimate. Remember, I am not claiming that there is one domain of objects that includes persons, shortstop, and numbers. Rather, the claim that relative to their respective theories, each is an object. A theory with finer grained discriminative apparatus may carve things up differently than a theory with coarser grained discriminative apparatus.

§ 2.3. Pragmatic Considerations

The opponent might claim that our ordinary judgments about existence do not presuppose the dubious entities, and we ought not to endorse a philosophical doctrine that flies in the face of our common judgments about what kinds of entities exist. In our everyday endeavors, we do not countenance the dubious objects; we speak of and accept only a small subclass of the structuralist's objects.

In [1992], Alan Sidelle argues that our common, everyday conversational practices have no implications concerning our actual judgments about the existence of types of entities. We have implicit conversational customs, according to Sidelle,

17See Chapters I and IV.
and these customs give us certain "default settings". When we ask "how many" questions, for example, the conventions implicit in ordinary conversation (usually) have us fall back on the default. But it does not follow from the fact that we fall back on the default that we do not accept other types of things into our ontology.

Sidelle writes:

> Particles of dust and cookie crumbs, for instance, are understood not to be counted when one asks 'How many things are on the table?' My not counting cookie crumbs in no way shows that I doubt their existence or status as "things". When we use these most general terms we have more or less agreed upon "default settings" that specify more particularly the type of thing in which we are interested. We may have different defaults for different contexts and, certainly we can specify directly at any time. The important point is that we need not verbally explicitly do so. And because these pragmatic factors can be tacit, our ordinary intuitions about things hardly show much about our ontology. (Sidelle [1992], pp. 422 - 3.)

Just because we do not normally count cookie crumbs and particles of dust when we report the number of objects on the table does not mean that we do not consider them to be things. And just because we do not normally count the "dubious entities" does not mean that we do not consider them to be things. Sidelle's point is that the default settings of ordinary conversation are not necessarily where we draw the line concerning the existence of kinds of objects. We do countenance other types of objects than those that are normally considered the default (e.g., cookie crumbs). We are accustomed to speaking of a certain sub-class of entities -- namely, medium-sized dry goods -- but it does not follow that we doubt the existence of other types of entities. In fact, we could explicitly specify a different default and count the dubious entities. For example, one could ask "How many defensive positions are there in the game of baseball?" and the answer would be a report of the number of objects in the theory of baseball defense. The default setting here is not the conversational norm, but it does not follow that we doubt the existence of the objects of baseball defense.
If Sidelle is right, then our common practices and ordinary answers to "how many" questions show little about our ontological commitments. We do, of course, accept those objects that we count when we are giving answers to "how many" questions, but we accept far more than just those. But then the pragmatic objection seems to lose much of its force. The objection suggested that we ought only to accept those entities that we speak of and accept in our everyday endeavors, and that the structuralist's dubious objects fall outside those boundaries. Sidelle has pointed out that we do not doubt the thing-status of many of the entities that fall outside of our conversational default settings. So it does not follow from the fact our default settings do not include the structuralist's dubious objects that we ought to reject them. In fact, were we to explicitly specify a different default, the dubious objects would be counted.

§ 2.4. Summary

In this section, I have considered three different responses to the question as to why some but not all ways of carving up the world get at the real objects. I have discussed epistemological considerations, metaphysical considerations, and pragmatic considerations. None of these responses, however, have compelled us to give up the structuralist's objects.

§ 3. Relativity

In Chapter IV, I claimed that the relativity was one of the assets of the structuralist's conception of objecthood. But, as I noted there, many might find this to be a counterintuitive feature of the view. Why think that objecthood is relative?

To deny that objecthood is relative is to embrace the thesis that there is one fixed domain of objects. One who holds this view must either (a) claim that we have
no access to the domain or (b) explain how we do have access to it. I will examine each of these alternatives.

(a) is a skeptical thesis that very well could be defended. The claim would be that there is one and only one domain of objects. We cannot, however, know of what this domain consists. Given our cognitive machinery, we are incapable of saying with any certainty just what the ultimate objects of the world are. This brand of skepticism challenges the mind/brain's access to the world's objects. The assumption behind the skeptical thesis is that the relation between the mind/brain and the objects of the world is tenuous at best. We have no guarantee that the world is the way we think it is. The defender of (a) thus challenges the relativity of objecthood thesis, but offers us no answer to the question as to of what the "real" domain of objects consists. We do not have epistemological access to the entities (at least, not any access that guarantees us certainty), so we cannot say what they are.

I have no reply to the defender of (a), but I know of no philosopher who has successfully refuted skepticism. In the current climate of naturalistic philosophy, however, I think I will have allies in rejecting (a). Skepticism is a radical thesis (at least, the global skeptical thesis considered above) in that it challenges the very possibility of knowledge of the world. Philosophers have grappled with this problem for centuries, and the skeptical challenge has been dealt with in various ways. None have been completely successful, but nevertheless we find few true skeptics today.

As for (b), its defender must show either that (i) our access to the domain is theory-independent or (ii) that there are certain privileged theories. (i) is implausible. We cannot stand outside of our language and theories and "look at the world". (ii) requires that we legislate on which theories are privileged. In order to hold this thesis, one must offer a legitimate reason for the legislation. Earlier in this
chapter I discussed reductionism and physicalism. These programs have a certain intuitive plausibility, but I hope to have shown that they have their own serious problems. Why, for instance, should we believe that everything is physical? Why should we think that all important facts can be expressed in terms of physical facts? Without adequate answers to these questions, we lack a compelling reason for legislating in favor of physical theory. Of course, one might think that ordinary discourse is the privileged theory and that we ought to accept all and only those entities that we recognize in our everyday talk. But there are two problems with this line: First of all, as Sidelle pointed out, our ontological commitments are not always apparent in our ordinary talk. We accept far more into our ontology than those objects that serve as the default settings in our common, everyday endeavors. We accept cookie crumbs and dust particles as objects even though we normally do not count them when asked to report the number of objects in some location.

The second problem with the view that ordinary discourse is the privileged theory is that Frege's Julius Caesar problem will crop up. In ordinary discourse, we often intermix the conceptual resources of various theories. If this particular fragment of language is the privileged theory, then odd questions of identity will arise: Is Julius Caesar identical to two? Is England the direction of the Earth's axis? These questions represent a kind of "category mistake". On the relativity of objecthood thesis, such questions are avoided because (e.g.) England and the direction of the Earth's axis are objects belonging to distinct domains, and questions of identity are disallowed across domains.

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18 See § 2.3 above.

19 See Chapter III.
Perhaps there are grounds for thinking that theories other than physics or ordinary discourse are privileged, but it is not at all clear what those theories would be, or what grounds someone would give for thinking that they are privileged. Without such grounds, we have no reason to accept the thesis that certain theories are the privileged theories. In summary, then, we have reason to reject (a) the skeptical thesis; (b) the theory independent access to the “real” domain of objects thesis; and (bii) the privileged theory thesis.

§ 4. Conclusion

In this chapter, I have discussed various challenges to the structuralist’s principle (S). I don’t pretend to have refuted the objections, but I hope to have shown that the various versions of the challenges have serious problems. There may be other objections to (S) and other versions of the challenges that I have not considered, and if that is the case then more needs to be said. But thus far, (S) looks preferable to the alternatives.
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