Vineberg's point can be illustrated by the following. It is thought by Platonists that the existence of mathematical objects is required to solve certain problems or puzzles and that this fact alone counts strongly in favor of belief in mathematical objects. For example, according to Steiner, Frege completely solved what Steiner calls "the metaphysical problem of applicability" (this problem and the Fregean solution will be given in Chapter 9). Since Frege's solution presupposes the existence of mathematical objects, it may be thought that the very success of Frege's solution amounts to a kind of evidence for the existence of mathematical objects.[21]

The existence of the above nominalistic alternatives, however, allows one to devise alternative solutions to "the metaphysical problem of applicability" (how the Constructibility Theory can be used to resolve this problem will be shown in detail in Chapter 9)—solutions that do not presuppose the existence of mathematical objects—thus undercutting the idea that the postulation of mathematical objects is required to solve the problem. We can thus see how these nominalistic alternatives can serve to undermine the claims of the Platonist that we have compelling scientific grounds for postulating the existence of mathematical objects.

[21] Frege's solution is supposed to consist in showing how mathematical entities relate, not directly to physical objects, but rather through concepts. Commenting on this solution, Steiner writes: "That physical objects may fall under concepts and be members of sets is a problem only for those who do not believe in the existence of sets" (Steiner, 1995: 138). To this, I reply: what about those who do not believe in the existence of concepts?

CHIHARA, Charles
"A STRUCTURAL ACCOUNT OF MATHEMATICS"
OXFORD: CLARENDON PRESS. 2004

7

The Constructibility Theory

In investigating the reasons Shapiro and Resnik have proposed for being realists (with respect to the "parts" or "positions" in structures), I have concentrated thus far on the positive reasons these philosophers have given for believing in mathematical objects. I have not yet investigated Shapiro's principal reason for not being an eliminative structuralist—what he called the "main stumbling block of the eliminative program" (Shapiro, 1997: 86). As a useful preliminary to such an investigation, I shall begin a characterization of the Constructibility Theory I discussed earlier in Chapter 6. Since this theory has been described in detail in my *Constructibility and Mathematical Existence* (Chihara, 1990), I shall give here only a brief description of the theory, setting out its main philosophically salient features, before giving a presentation of how (finite) cardinality theory is developed within it—a cardinality theory to be used in later sections of this work. This short exposition will be given in the early sections of this chapter. In Sections 4, 5, and 6, I shall take up the objections to the Constructibility Theory that Shapiro and Resnik have detailed in their respective books on structuralism. By means of these discussions, I hope to clear up some widespread misconceptions and confusions about the Constructibility Theory and to provide the reader with some substantial insights into its finer points.

However, before getting involved with the details of this theory, it needs to be emphasized that the term 'Constructibility Theory' is used in this work ambiguously: sometimes to denote my general theory of constructibility put forward in my book, and sometimes to denote the formal theory of constructibility of that work.[1] (Both are theories of constructibility.) When I feel

[1] My general theory of constructibility contains many theses, arguments, and positions that are not found in the formal theory. The ambiguity of the term 'constructibility theory' is similar to an ambiguity found in the use of the expression 'Russell's theory of types': sometimes the expression refers to Russell's general theory about the types of propositional functions there supposedly are, and sometimes it refers to the specific formal theory of propositional functions given in *Principia Mathematica*.

that it is important to indicate that the formal theory is being cited, I shall use the expression 'Ct' instead of 'the Constructibility Theory'.

1. A BRIEF EXPOSITION OF THE CONSTRUCTIBILITY THEORY

Set theory is a theory about sets. It tells us what sets exist and how these sets are related to one another by the membership relationship. The Constructibility Theory is a theory about open-sentences: it tells what open-sentences (of a certain sort) are constructible and how these constructible open-sentences would be related to one another by the *satisfaction relation*. (Thus, in this theory, one open-sentence can satisfy another.) The theory is formalized in what is basically a many-sorted first-order logical language[2] that utilizes, in addition to the existential and universal quantifiers of standard first-order logic, *constructibility quantifiers*.

Constructibility quantifiers are sequences of primitive symbols: either (C—) or (A—), where '—' is to be replaced by a variable of the appropriate sort. Using '$\Psi\phi$' to be short for 'ϕ satisfies Ψ', '$(C\phi)\Psi\phi$' can be understood to say:

It is possible to construct an open-sentence ϕ such that ϕ satisfies Ψ,

whereas '$(A\phi)\Psi\phi$' can be understood to say:

Every open-sentence ϕ that it is possible to construct is such that ϕ satisfies Ψ.

And just as '$(\exists x)Fx$' is equivalent to '$-(x)-Fx$', and '$(x)Fx$' is equivalent to '$-(\exists x)-Fx$', we have '$(C\phi)\Psi\phi$' is equivalent to '$-(A\phi)-\Psi\phi$', and '$(A\phi)\Psi\phi$' is equivalent to '$-(C\phi)-\Psi\phi$'.

The Constructibility Theory asserts the constructibility of various sorts of open-sentences. It needs to be emphasized that what are said to be constructible by means of the constructibility quantifiers are open-sentence *tokens* as opposed to open-sentence types. Open-sentence types are classified by Parsons as "quasi-concrete" objects (in contrast to "purely mathematical objects"), because "they are directly 'represented' or 'instantiated' in the concrete" (Parsons, 1996: 273). Despite the "quasi" qualification, it can be argued that these quasi-concrete objects are abstract Platonic objects as epistemologically disreputable as numbers and sets. But open-sentence tokens

[2] For a clear and rigorous discussion of such languages, see Enderton, 1972: sect. 4.3.

are not open to the same objection: typically, an open-sentence token consists of particular marks on paper, which exist at a particular place in the universe and at a particular time. Furthermore, to say that an open-sentence of a particular sort is constructible is not to imply or presuppose that any such open-sentence token actually exists or, indeed, that anything exists. Constructibility quantifiers do not carry ontological commitments as do the quantifiers of standard extensional logic.

What does 'possible' mean?

In the phrase 'it is possible to construct', the term 'possible' needs some explanation. There are, of course, many different kinds of possibility: logical, metaphysical, physical, epistemological, technological, to name just a few. Epistemological possibility is concerned with what is known. Thus, to say that ϕ is epistemologically possible for agent X is to say that ϕ is not logically precluded by what X knows, that is, ϕ is logically compatible with everything X knows. To say that ϕ is physically possible is to say something like: ϕ is logically compatible with all the physical laws of our universe. The possibility talked about in the Constructibility Theory is what is called 'conceptual' or 'broadly logical' possibility—a kind of metaphysical possibility, in so far as it is concerned with *how the world could have been*. Every purely logical truth is necessary in this sense, but the set of conceptual necessary truths will include much more. What are called "analytic truths" (such truths as 'All bachelors are unmarried') are also held to be necessary in this broadly logical sense.[3] As a rough guide, Graeme Forbes suggests that 'it is possible that P' should be taken to mean: "There are ways things might have gone, no matter how improbable they may be, as a result of which it would have come about that P" (Forbes, 1985: 2). Another way of expressing this sort of possibility is to take it to mean: the world could have been such that, had it been this way, P would have been the case. There are many different systems of modal logic, but the type of system that is generally believed to correctly formalize the logical features of this broadly logical sense of necessity is S5.[4]

[3] See Plantinga, 1974: ch. 1, sect. 1 and Forbes, 1985: ch. 1, sect. 1, for more examples and discussion.

[4] Thus, Kit Fine writes "S5 provides the correct logic for necessity in the broadly logical sense": Fine, 1978: 151. For an interesting discussion of the development of S5 modal logic, see Kneale and Kneale, 1962: ch. 9, sect. 4. See Chihara, 1998 for a discussion of a variety of S5-type systems of modal logic.

What '(Cϕ)Ψϕ' does not mean

'(Cϕ)Ψϕ' ('It is possible to construct an open sentence ϕ such that ϕ satisfies Ψ') does not mean that *one knows how* to construct such an open-sentence or that one has a method for constructing such an open-sentence. Hence, the constructibility quantifier is not at all like the intuitionist's existential quantifier. Furthermore, it does not mean that one can always, or even for the most part, determine what particular objects would satisfy such an open-sentence or how one would determine what objects would satisfy ϕ. Nor does it mean that one can determine whether a series of marks, sounds, hand signals, or what have you is or is not such an open-sentence.

It may strike the reader as strange, even objectionable, that I would put forward a theory that asserts the constructibility of open-sentences of various sorts, when it cannot provide the sort of information indicated above. If so, consider the example of Euclid's geometry, which I described earlier: for well over two thousand years, this modal theory was a sort of paradigmatic mathematical system, showing scholars how mathematics should be developed and applied. But notice, Euclid's geometry does not tell us how to recognize straight lines, how to tell if a line is really straight, or if a line really intersects a point. It does not tell us how to construct points, straight lines, or arcs. The important point is this: it doesn't matter that Euclid's geometry does not tell us these things. The usefulness of that kind of modal theory does not depend on its giving us that kind of information. *That's not the way we use that geometry.*[5] Similarly, my Constructibility Theory is not designed to give us information about how to tell what is an open-sentence or what things satisfy any given open-sentence. It is just not that sort theory. It has been designed to provide us with a way of understanding and analyzing mathematical reasoning, in a way that does not presuppose the existence of mathematical entities, as will become apparent in the sections to follow.

The many levels of the Constructibility Theory

The Constructibility Theory is similar to Frege's theory of concepts: just as Frege's hierarchy of concepts is stratified into levels, the open-sentences that are talked about in the Constructibility Theory are of different levels. Thus,

[5] Of course, we do have ways of telling when lines drawn on a sheet of paper are approximately straight, roughly intersect a curve at a point, etc., and one might say that the usefulness of Euclid's geometry theory depends upon our having such ways of telling. But it should be noted that we also have ways of constructing open-sentences, and we have ways of telling, for many actual cases, that something satisfies an open-sentence we have constructed.

consider the following situation. On the desk in my office, there are two pieces of fruit, which I have named 'Tom' and 'Sue'. On the blackboard in my office, I write the open-sentence

x is a piece of fruit on the desk in my office.

Both Tom and Sue satisfy this open-sentence. The desk does not. This open-sentence token is of level 1. Now suppose that I write the open-sentence

There is at least one object that satisfies *F*

in the bottom left corner of my blackboard, where '*F*' is being used as a variable of level 1. This open-sentence token is satisfied by the open-sentence token I constructed earlier—the level 1 open-sentence I first wrote on the blackboard. The second open-sentence I constructed is a second-level open-sentence. Clearly, then, we can go on to construct open-sentences of level 3, 4, 5, and so on.

An objection considered

It might strike a critical reader that, in speaking of level 3, 4, 5, ... open-sentences, I am, in effect, presupposing the natural numbers (or the finite ordinal numbers) and in this way presupposing mathematical objects, thus undermining my attempt to provide an analysis of mathematical reasoning that does not assume the existence of such things. Actually, what I do assume here is the rule we all learn as children for constructing and ordering the arabic numerals—numerals which are being used to talk and to theorize about different levels of open-sentences in a way that enables me to order the things I wish to discuss. Such a use does not require genuine quantification over and reference to mathematical entities.

In the next section, I shall sketch the development of finite cardinality theory within the framework discussed above.

2. Preliminaries of the Constructibility Theory of Natural Number Attributes

I shall use the following to refer to the entities of different levels:

Level 0: *Objects x, y, z, ...*

Level 1: *Properties F, G, H, ...*

Level 2: *Attributes* $\mathscr{F}$, $\mathscr{G}$, $\mathscr{H}$, ...

Level 3: *Qualities* **F**, **G**, **H**, ...

It needs to be emphasized that what I am calling 'properties', 'attributes', and 'qualities' are just open-sentences. Thus, the open-sentences of level 1 that will be talked about in this theory are to be called 'properties' and the upper-case letters '*F*', '*G*', '*H*', etc. are to be used as variables to refer to these open-sentences. Extrapolating from the level 1 case, one can see that the level 2 open-sentences to be talked about will be called 'attributes' and the script upper-case letters '$\mathscr{F}$', '$\mathscr{G}$', '$\mathscr{H}$', etc. will be used as variables to refer to these second-level open-sentences. Thus, properties, attributes, and qualities are not being used to refer to universals or abstract entities of some sort, as is generally the case in philosophical works. I use this terminology simply to facilitate our keeping distinct and ordered the open-sentences of different levels I shall be talking about.

Quantifiers

(a) Quantifiers containing occurrences of level 0 variables are just the standard quantifiers of first-order logic.
(b) Quantifiers containing occurrences of level 1 or higher variables are constructibility quantifiers.

Relations

All the open-sentences to be talked about in this theory will be monadic open-sentences, that is, such open-sentences as '*x* is a human' that contain occurrences of only one variable. The reason I restrict the theory to just monadic open-sentences is simplicity: it makes the task of formalizing the theory so much easier.

Of course, we will need relations if we are to develop arithmetic in the Fregean way. So how am I to get relations in a system that deals only with monadic open-sentences? There is a similar problem in set theory. How does one get relations in set theory? As far as mathematics is concerned, a relation can be taken to be a set of ordered pairs. That is, everything one needs to do with relations in mathematics can be done by taking a relation to be a set of ordered pairs.[6] In mathematics, the taller-than relation among the set of human beings can be taken to be just the set of all ordered pairs of humans $<x, y>$ such that *x* is taller than *y*. So the problem of defining relations in set theory reduces to the problem of defining ordered pairs in terms of sets.

[6] Some might think that only binary relations can be defined to be sets of ordered pairs. Isn't a ternary relation to be defined to be a set of ordered triples? But the ordered triple $<x, y, z>$ can be defined to be the ordered pair $<x, <y, z>>$. Clearly, an ordered *n*-tuple can be defined, in that way, to be an ordered pair.

Kuratowski's solution to this problem is now widely used. He proposed that we take the ordered pair $<x, y>$ to be the set $\{\{x, x\}, \{x, y\}\}$. When $<x, y>$ is so defined, then it can be proved in set theory that $<x, y> = <z, w>$ iff $x = z$ and $y = w$. And that is the crucial condition we want ordered pairs to satisfy.

In what follows, I shall follow the above set-theoretical practice, by defining relations à la Kuratowski.

Couples

A couple $\{x, y\}$ is a property that is satisfied by only the objects *x* and *y*.

Example: '$x = $Tom v $x = $Sue' is a couple {Tom, Sue}.

Notice that I have given here an example of a monadic open-sentence: only one variable (i.e. '*x*') occurs in the open-sentence—of course, there are two *occurrences* of this one variable. Notice also that I say 'a couple' (instead of 'the couple'), because it is possible to construct indefinitely many different such couples. Thus, '*x* is the very same person as Tom or *x* is the very same person as Sue' is also a couple {Tom, Sue}.

Ordered pairs

An ordered pair $<x, y>$ is an attribute that is satisfiable by all and only couples $\{x, x\}$ and $\{x, y\}$ that could be constructed.

Note: An ordered pair is an open-sentence satisfied by other open-sentences—it is not satisfied by the objects *x* and *y*.

Example: The open-sentence

F is a couple {Tom, Tom} or *F* is a couple {Tom, Sue} is an ordered pair $<$Tom, Sue$>$.

Relations

A relation **R** is a *quality* that is satisfiable only by ordered pairs such that if an ordered pair $<x, y>$ could be constructed that satisfied **R**, then **R** is satisfiable by every ordered pair $<x, y>$ that could be constructed.

It can be seen that there is nothing mysterious about relations. They are things we can literally construct, say by writing them down on paper or on the blackboard.

Example: '$\mathscr{H}$ is an ordered pair $<x, y>$ such that x is married to y' is a quality which is a relation corresponding to the intuitive relation of *being married to*. Thus, any ordered pair < Hillary Clinton, Bill Clinton > that one constructed would satisfy the above-mentioned relation.

A notational definition

If **R** is a relation, then

'x**R**y' means$_{def}$ 'an ordered pair $<x, y>$ satisfies **R**'.

Notice that, as the term 'relation' has been defined, if an ordered pair $<x, y>$ satisfies some relation **R**, so that x**R**y, then every ordered pair $<x, y>$ that is constructible would also satisfy **R**.

In the next section, many of the definitions will look exactly like those given in Frege's theory of cardinal numbers.

Relation **R** *correlates F to G*

iff, for every x that satisfies F, there is a y that satisfies G such that

x**R**y;

and for every y that satisfies G, there is an x that satisfies F such that

x**R**y.

Relation **R** *is a one-one relation*

iff, for every x, y, and z,

if x**R**y and x**R**z, then $y = z$;

if x**R**y and z**R**y, then $x = z$.

F is equinumerous with G

iff, it is possible to construct a one-one relation that correlates F to G.

Example: Let us suppose that the objects under consideration here are people. Then consider the open-sentences:

J: 'x is the junior senator from some state.'

S: 'x is the senior senator from some state.'

and let **R** be the open-sentence:

$\mathscr{H}$ is an ordered pair $<x, y>$ such that x is the junior senator from a state in which y is the senior senator.

It can be seen that **R** is a one-one relation correlating property J with property S. Hence, J is equinumerous with S.

Compare, in the context of this example, the Fregean truth conditions for the assertion that J is equinumerous with S with the constructibility truth conditions for that same assertion. In the Fregean case, in order that the assertion be true, there must exist some abstract entity—a one-one relation—something that does not exist in physical space, which cannot be detected by any scientific instruments. In the constructibility case, in order that the assertion be true, it must be possible to construct an open-sentence of a certain sort—and in this case, I can (indeed, I have) constructed an open-sentence of the required sort.

The developments in the following section deviate somewhat from the Fregean developments. This is because we don't have objects to serve as the cardinal numbers. In this theory, instead of cardinal numbers, we have cardinal number attributes.

3. CARDINAL NUMBER ATTRIBUTES

Cardinal number attribute of a property

An attribute $\mathscr{N}$ is a *cardinal number attribute of property F* iff $\mathscr{N}$ is satisfiable by all and only those properties equinumerous with F.

Example of a cardinal number attribute of a property:

G is equinumerous with 'x is the senior senator from some state'.

Hume's Principle

It is a straightforward matter of proving from the above a constructibility version of what has come to be known as "Hume's Principle":

F is equinumerous with G iff it is possible to construct cardinal number attributes of both $\mathscr{F}$ and $\mathscr{G}$, and for any $\mathscr{F}$ that it is possible to construct which is a cardinal number attribute of F, and for any $\mathscr{G}$ that it is possible to construct which is a cardinal number attribute of G, $\mathscr{F}$ and $\mathscr{G}$ are satisfiable by exactly the same properties.[7]

Cardinal number attribute

An attribute $\mathscr{N}$ is a *cardinal number attribute* (or cardinality attribute) iff it is possible to construct some property F such that $\mathscr{N}$ is a cardinal number attribute of property F.

The next step is to define the *natural number attributes*. Thus, I first define:

A *zero attribute* is any cardinal number attribute that is satisfied by '$x \neq x$'.

Then the following five definitions yield what is desired:

(1) *The immediate predecessor relation* P

$\mathscr{M}$ **P** $\mathscr{N}$ iff it is possible to construct a property F which is such that some object x satisfies it & $\mathscr{N}$ is a cardinality attribute of F & it is possible to construct a property G such that $\mathscr{M}$ is a cardinality attribute of G and G is satisfiable by all (and only those) objects different from x which satisfy F.

(2) P-*hereditary qualities*

A quality **Q** is **P**-hereditary iff, for all cardinality attributes $\mathscr{M}$, $\mathscr{N}$, if $\mathscr{M}$ satisfies **Q** and $\mathscr{M}$**P**$\mathscr{N}$, then $\mathscr{N}$ satisfies **Q**.

(3) P-*descendants*

A cardinality attribute $\mathscr{N}$ is a **P**-descendant of cardinality attribute $\mathscr{M}$ iff $\mathscr{N}$ satisfies every **P**-hereditary quality that $\mathscr{M}$ satisfies.

(4) *Natural number attributes*

A natural number attribute is any **P**-*descendant of a zero attribute*.

Comment: the above definition of the natural number attributes takes as its model not Frege's definition of 'natural number' given in *The Foundations of Arithmetic* (Frege, 1959), but rather Russell's definition (see, for example,

[7] There is a great deal of discussion in the recent Fregean literature of this principle. For an enlightening overview of these discussions, see William Demopoulos's introduction to Demopoulos, 1995. There is the question as to whether Frege thought that arithmetic might be founded on Hume's Principle instead of his Axiom V. See, in this regard, Heck, 1995 and Heck, 1997. For criticisms of this view, see Nomoto, 2000. It should also be noted that Michael Dummett has strenuously argued against the use of the term 'Hume's Principle' to apply to principles of the sort given above. In Dummett, 1998, he writes: "The term 'Hume's Principle' is productive of intellectual as well as historical confusion, and its use should be resolutely avoided" (387).

chapter 3 of *Introduction to Mathematical Philosophy* (Russell, 1920)). The principal difference is in the definition of 'P-descendant'. Russell's definition of the ϕ-ancestral relation for parameter ϕ (the converse relation of the ϕ-descendant relation) implies that every object x is a ϕ-ancestor of itself—something that Frege wished to avoid since his aim was to capture the intuitive idea of "following in the ϕ-series". Frege did not want his analysis to require that every object follow itself in such a ϕ-series, so his definition of the ϕ-ancestral has an extra clause in the antecedent that is not found in Russell's definition.[8] Russell chose an analysans that gave a less accurate fit with the analysandum in order to have a simpler definition of 'natural number'. The difference is only minor, but I believe that it results in a more easily grasped definition.

Specific natural number attributes

We cannot define the specific natural number attributes in the way Frege defined the specific natural numbers. Recall how Frege proceeded. From the point of view of set theory, Frege proceeded essentially as follows:

For any set α, $C\alpha$ is to be the set of all sets equinumerous with α.[9] Then,

$1 = C\{x \mid x = 0\}$

$2 = C\{x \mid x = 0 \vee x = 1\}$

$3 = C\{x \mid x = 0 \vee x = 1 \vee x = 2\}$

etc.

We cannot take this route since we don't have any objects to function as the "numbers" in such a definition. So this is how I define the specific natural numbers:

A natural number attribute is:

a *one attribute* iff it is possible to construct a zero attribute which immediately precedes it;

a *two attribute* iff it is possible to construct a one attribute which immediately precedes it;

a *three attribute* iff it is possible to construct a two attribute which immediately precedes it,

etc.

[8] See Frege, 1959: 92 for Frege's definition.
[9] For purposes of simplicity of exposition, I overlook the fact that in most standard set theories, there is no such set as $C\alpha$. In simple type theory, there is such a set of the appropriate type.

Thus, instead of defining specific natural numbers as Frege did, here we define what attributes would be specific natural number attributes.

Compare how simple cardinality statements would be analyzed in the present system in contrast to the way such statements were analyzed by Frege. The statement

The number of moons of the earth is one

was analyzed by Frege to be:

The cardinal number of the concept *is a moon of the earth* = one

whereas, in the present system, we get:

It is possible to construct a one attribute which 'x is a moon of the earth' satisfies

Addition

What we define here are the "addition attributes": suppose that $\mathscr{M}$ and $\mathscr{N}$ are natural number attributes and that it is possible to construct properties F and G such that F satisfies $\mathscr{M}$, G satisfies $\mathscr{N}$, and no object satisfies both F and G. Then an $\mathscr{M}+\mathscr{N}$ attribute is any attribute satisfied by all and only those properties satisfied by any object that satisfies either F or G.

Contrast the above definition of addition with the usual recursive definition found in typical first-order developments of Peano arithmetic:

For any natural number x, $x+0=x$;

For all natural numbers x and y, $x+y'=(x+y)'$

(where x' = the successor of x).[10]

The above recursive definition is perfectly adequate in the context of attempting to study the mathematical features of a type of structure, since it yields all the usual theorems regarding addition. But this sort of definition tells us nothing about how addition is connected to our use of cardinality theory in common everyday situations in which addition plays a role. It does not aid us in assessing the often-heard philosophical view (to be discussed in Chapter 9) that seven plus five equals twelve is refuted by the fact that seven gallons of alcohol mixed with five gallons of water does not yield twelve gallons of mixture. The above definitions are directed at quite another project: picking out the type of structure with which arithmetic deals. And it is

[10] See Russell, 1920: 6 for a brief discussion of this definition in the context of characterizing the Peano system.

perfectly adequate for giving us all the usual theorems regarding addition. But it is not the sort of definition that would be satisfactory for the project I have in mind, which is to gain insights into the way arithmetic is applied in science and everyday life.

Analogues of the above recursive definition of addition are among the following theorems, which can easily be proved:

(1) If $\mathscr{M}$ and $\mathscr{N}$ are natural number attributes, then it is possible to construct an $\mathscr{M}+\mathscr{N}$ attribute and every such attribute is a natural number attribute.
(2) If $\mathscr{N}$ is a natural number attribute and $\mathscr{M}$ is a zero attribute, then every $\mathscr{N}+\mathscr{M}$ attribute is an $\mathscr{N}$ attribute.
(3) If $\mathscr{N}$ is a natural number attribute, and $\mathscr{M}$ is a one attribute, then $\mathscr{N}\mathbf{P}(\mathscr{N}+\mathscr{M})$.
(4) If $\mathscr{M}$ and $\mathscr{N}$ are natural number attributes, $\mathscr{O}$ is an $\mathscr{M}+\mathscr{N}$ attribute, $\mathscr{T}$ is a one attribute, $\mathscr{S}$ is an $\mathscr{N}+\mathscr{T}$ attribute, and $\mathscr{F}$ is an $\mathscr{M}+\mathscr{S}$ attribute, then $\mathscr{O}\mathbf{P}\mathscr{F}$.

Specific sums

Specific sums corresponding to such theorems of arithmetic as '$7+5=12$' can be easily proved. Thus, it can be proved that:

If $\mathscr{M}$ is a seven attribute, $\mathscr{N}$ is a five attribute, $\mathscr{O}$ is a twelve attribute, then any $\mathscr{M}+\mathscr{N}$ attribute is an $\mathscr{O}$ attribute.

I shall not go on to develop the usual arithmetical theory of the "multiplication attributes", since this can be done in a straightforward way. Let us turn instead to a constructibility version of the theory of natural numbers, which I shall call 'Peano's theory', whose axioms consist of constructibility versions of the following.

Peano's axioms[11]

(P1) Zero is a natural number.
(P2) The successor of any natural number is a natural number.
(P3) If the successors of two natural numbers are equal, then the numbers themselves are equal.

[11] When Peano introduced his axioms in 1889, he included among them identity axioms. It is common today to give only the five axioms that concern the natural numbers. See in this regard Gillies, 1982: 67.

(P4) Zero is not the successor of any natural number.
(P5) The Principle of Mathematical Induction.

A constructibility version of (P1) can be given as:

(P1*) Any zero attribute is a natural number attribute.

Given the above definitions of the relevant terms, this assertion is obviously true. So let us turn to the following constructibility version of (P2):

(P2*) If $\mathscr{M}$ is a natural number attribute and $\mathscr{M}\mathbf{P}\mathscr{N}$, then $\mathscr{N}$ is a natural number attribute.

To see that this assertion is true, notice that, since $\mathscr{M}$ is a natural number attribute, it must satisfy every **P**-hereditary quality that a zero attribute satisfies. Since $\mathscr{M}\mathbf{P}\mathscr{N}$, $\mathscr{N}$ must also satisfy every **P**-hereditary quality that a zero attribute satisfies.

(P3*) If $\mathscr{M}\mathbf{P}\mathscr{U}$, $\mathscr{N}\mathbf{P}\mathscr{V}$, and, further, any property satisfies $\mathscr{U}$ iff it satisfies $\mathscr{V}$, then any property satisfies $\mathscr{M}$ iff it satisfies $\mathscr{N}$.

To prove this, I assume the following:

(1) $\mathscr{M}\mathbf{P}\mathscr{U}$ and $\mathscr{N}\mathbf{P}\mathscr{V}$.
(2) Any property satisfies $\mathscr{U}$ iff it satisfies $\mathscr{V}$.

It follows from (1) that

(3) it is possible to construct a property F & there is an object x such that x satisfies F & $\mathscr{U}$ is a cardinality attribute of F & it is possible to construct a property G such that $\mathscr{M}$ is a cardinality attribute of G and G is satisfied by all (and only those) objects different from x which satisfy F

and

(4) it is possible to construct a property F & there is an object x such that x satisfies F & $\mathscr{V}$ is a cardinality attribute of F & it is possible to construct a property G such that $\mathscr{N}$ is a cardinality attribute of G and G is satisfied by all (and only those) objects different from x which satisfy F.

Since it is possible to construct a property F with the features described in (3), let us suppose that:

(5) property F^* has been constructed which is such that there is an object x such that x satisfies F^* & $\mathscr{U}$ is a cardinality attribute of F^* & it is possible to construct a property G such that $\mathscr{M}$ is a cardinality attribute of G and G is satisfied by all (and only those) objects different from x which satisfy F^*.

We can assume that:

(6) x^* is an object such that x^* satisfies F^* & $\mathscr{U}$ is a cardinality attribute of F^* & it is possible to construct a property G such that $\mathscr{M}$ is a cardinality attribute of G and G is satisfied by all (and only those) objects different from x^* which satisfy F^*.

Since we know that it is possible to construct a property G that has the features described in (6), let us assume that:

(7) property G^* has been constructed which is such that $\mathscr{M}$ is a cardinality attribute of G^* and G^* is satisfied by all (and only those) objects different from x^* which satisfy F^*.

Similarly, applying the same three steps to (4) *mutatis mutandis*, we obtain:

(8) property $G^\#$ has been constructed which is such that $\mathscr{N}$ is a cardinality attribute of $G^\#$ and $G^\#$ is satisfied by all (and only those) objects different from $x^\#$ which satisfy $F^\#$.

From (2) and (5), we can infer that:

(9) $\mathscr{V}$ is a cardinality attribute of F^* and that F^* is equinumerous with $F^\#$.

From (7), (8), and (9), we can infer that:

(10) G^* is equinumerous with $G^\#$.

Hence,

(11) any property that would satisfy $\mathscr{M}$ would also satisfy $\mathscr{N}$ and conversely.

From this, we can infer (P3*).

(P4*) It is not possible to construct an attribute $\mathscr{M}$ and a zero attribute $\mathscr{N}$ which is such that $\mathscr{M}\mathbf{P}\mathscr{N}$.

A proof can take the form of a *reductio ad absurdum* as follows:

Suppose that attributes $\mathscr{M}$ and $\mathscr{N}$ have been constructed such that:

(1) $\mathscr{N}$ is a zero attribute and $\mathscr{M}\mathbf{P}\mathscr{N}$.

Then, we can infer:

(2) it is possible to construct a property F & there is an object x such that x satisfies F & $\mathscr{N}$ is a cardinality attribute of F & it is possible to construct a

property G such that $\mathscr{M}$ is a cardinality attribute of G and G is satisfied by all (and only those) objects different from x which satisfy F.

But it is absurd to suppose that it is possible to construct a property F which is such that, for some object x, x satisfies F & $\mathscr{N}$ is a cardinality attribute of F.

(P5*) is the following constructibility version of the Principle of Mathematical Induction:

If

(i) **Q** is a quality satisfied by some zero attribute,

and

(ii) for all cardinality attributes $\mathscr{M}$, $\mathscr{N}$ that it is possible to construct, if $\mathscr{M}$ satisfied **Q** and $\mathscr{M}\mathbf{P}\mathscr{N}$, then $\mathscr{N}$ would also satisfy **Q**,

then

every natural number attribute would satisfy **Q**

Proof: Suppose that $\mathscr{C}$ is a natural number attribute. Then $\mathscr{C}$ must satisfy every quality that is **P**-hereditary and that is satisfied by a zero attribute. Since from (ii) we can infer that **Q** must be **P**-hereditary, we can conclude that $\mathscr{C}$ must satisfy **Q**.

Instead of continuing an exposition of the Constructibility Theory, I shall now take up the objections to my Constructibility Theory advanced by Shapiro and Resnik in their respective books on structuralism.

4. Shapiro's Objections to the Constructibility Theory

Recall that Shapiro's grounds for adopting his *ante rem* structuralism were twofold: on the one hand, he maintained that his realistic version of structuralism was the simplest and most straightforward ontological position to take. On the other hand, he raised objections to his nominalistic rivals, claiming among other things that the nominalistic views were, in some sense, *equivalent* to the realistic positions, and hence did not have any epistemological advantages. In this section, I shall evaluate Shapiro's objections to his nominalistic rivals, concentrating for the most part on his objections to my Constructibility Theory.

Let us consider an objection to my views that Shapiro first raised in his paper "Modality and Ontology" and that he repeated with some minor changes in his book on mathematical structuralism.[12] Shapiro begins his objection by providing his readers with a way of translating the sentences of the Constructibility Theory into the sentences of a set-theoretical version of simple type theory.[13] His instructions are: replace all the variables of the Constructibility Theory that range over level n open-sentences with variables of simple type theory that range over type n sets ($n = 1, 2, 3, \ldots$); then replace the symbol for satisfaction with the symbol for membership; and replace the constructibility quantifiers with ordinary existential quantifiers. (Shapiro, 1997: 231). He sums up as follows:

> [T]o translate from Chihara's language to the standard one, just undo Chihara's own translation. *The two systems are definitionally equivalent.* From this perspective, Chihara's system is a notational variant of simple type theory. An advocate of one of the systems cannot claim an epistemological advantage over an advocate of the other. (Shapiro, 1997: 231, italics mine)

In the article, Shapiro is even more blunt and pointed in stating his objection, writing:

> From this perspective, Chihara's system is a notational variant of simple type theory (or non-cumulative set theory, with finite ranks). To be crude: here, it seems, *ontology is reduced by envisioning that we have changed the shape of a few symbols of the regimented language.* (Shapiro, 1993: 468, italics mine)

My Constructibility Theory is thus made to appear as if were an utterly trivial rewriting of the set-theoretical version of simple type theory—a mathematical innovation which has no more value than a new printing (in a slightly different font) of an old book developing simple type theory. These are very serious charges that Shapiro has leveled against my views—charges which, if sustained, would be a devastating refutation of my philosophical position.

But before attempting any sort of evaluation of Shapiro's charges, we should investigate in more depth the logic of Shapiro's reasoning. What grounds does he have for concluding that my system is a mere notional variant of simple type theory? To see the underlying rationale for his inferences, we need to see what his general strategy is for rejecting all the anti-realist views that he takes up in his works.[14] Basically, his overall goal is to show that

[12] The paper cited is Shapiro, 1993. The book is Shapiro, 1997.

[13] The version of simple type theory Shapiro has in mind is, I believe, essentially the set theory described by Quine in Quine, 1963: sec. 36.

[14] The other anti-realists attacked in this way by Shapiro are Hartry Field, Geoffrey Hellman, and George Boolos.

[t]he epistemological problems facing the anti-realist programmes are just as serious and troublesome as those facing realism. Moreover, the problems are, in a sense, *equivalent* to those of realism. (Shapiro, 1993: 456).

Here, then, is how Shapiro claims to establish these conclusions:

In each case, the structure of the argument is the same. I show that there are straightforward, often trivial, translations from the set-theoretical language of the realist to the proposed modal language, and vice-versa. The translations preserve warranted belief, at least, and probably truth ... The contention is that, because of these translations, neither system can claim a major epistemological advantage over the other. (Shapiro, 1993: 457).

Notice that, according to this scenario, one must come up with a translation scheme *tr* that preserves warranted belief; that is, for any sentence ϕ of the anti-realist language, belief in ϕ is warranted iff belief in $tr(\phi)$ is warranted.

In the quote from Shapiro's book, it was claimed that the two theories in question are "definitionally equivalent". What is definitional equivalence and how is this condition relevant to the question as to whether the Constructibility Theory is a mere notational variant of simple type theory? Here's the definition of the technical term 'definitional equivalence' that Shapiro gives in his article:

Two theories T, T' are said to be *definitionally equivalent* if there is a function f_1 from the class of sentences of T into the class of sentences of T' and a function f_2 from the class of sentences of T' into the class of sentences of T, such that (1) f_1 and f_2 both preserve truth (or theoremhood if the theories do not have intended interpretations) and (2) for any sentence ϕ of T, $f_2f_1(\phi)$ is equivalent in T to ϕ, and for any sentence ψ of T', $f_1f_2(\psi)$ is equivalent in T' to ψ. (Shapiro, 1993: 479)

He then proposes to use definitional equivalence "as an indication that the intended structures, and thus the ontologies, of different theories are the same" (Shapiro, 1993: 479).

Before subjecting Shapiro's reasoning to critical examination, it should be noted that some logicians have questioned the appropriateness of Shapiro's conditions for *definitional equivalence*.[15] In order to avoid a lengthy investigation of the concept of definitional equivalence here, I shall regard Shapiro as simply putting forward a stipulative definition of the term 'Shapiro equivalence', with no suggestion that he is attempting to capture some pre-existing notion of definitional equivalence. Thus, we can take Shapiro to be claiming that if two theories A and B are shown to be Shapiro equivalent, then we have an "indication" that A and B have one and the same ontology.

[15] This was done by Albert Visser at my presentation of an earlier version of this material at the *One Hundred Years of Russell's Paradox* conference help in Munich, June 2001. And others there seemed to agree.

Surprisingly, Shapiro claims to have "shown" that the Constructibility Theory, under certain (plausible) conditions, "is [Shapiro equivalent] to a standard "realist" theory", and he concludes that "the intended structure—and the ontology—of [the Constructibility Theory] is the same as that of the corresponding realist theory, and is not to be preferred on ontological grounds" (Shapiro, 1993: 479). I say 'surprisingly', because nowhere either in his article or in his book is there anything like a proof of this Shapiro equivalence. As I noted earlier, he does provide a translation scheme for translating each sentence of the Constructibility Theory into a sentence of simple type theory; and he supplies us with a method of translating back again. But he must do more than that to establish Shapiro equivalence. To justify this conclusion, one must show that the method of translation given in the earlier quotation preserves truth (or at least theoremhood). Not only has Shapiro failed to provide a proof of this crucial element, he has not even given a plausibility argument supporting the belief that truth or theoremhood is preserved by the translation procedures. What has happened here? Has Shapiro simply forgotten that this extra condition needs to be fulfilled? That is hard to believe.

Recall that Shapiro also claimed that his method of translating from the language of the Constructibility Theory to the language of simple type theory preserves warranted assertability. Has he provided a proof of this claim? Again, no such proof is to be found in either Shapiro's article or his book.

Why is Shapiro so sure that the Constructibility Theory is a mere notational variant of simple type theory? Since there is nothing remotely like a proof of this doctrine, I can only wonder at his confidence at affirming such a position. Perhaps he believes that if he can prove that the Constructibility Theory is Shapiro equivalent to simple type theory, then that would be tantamount to a proof that the Constructibility Theory is a notational variant of simple type theory. And perhaps he believes that he has good grounds for thinking the Constructibility Theory is Shapiro equivalent to simple type theory.

If theory A is Shapiro equivalent to theory B, does it follow that A and B are notational variants of one another? Not at all. Suppose theory A is a first-order theory about Mr Jones's cats and theory B is a first-order theory about

Mr Smith's dogs. And suppose that *A* and *B* are Shapiro equivalent (the domain of *A* consists of the cats of Jones, and the domain of *B* consists of the dogs of Smith; what *A* says about the number of males Jones has, *B* says about the number of males Smith has—and it just happens to be the case that both theories are true). Theory *A* is no mere notational variant of theory *B*, for *B* is a theory about dogs, not cats. One could not explain the barking emanating from Smith's house by appealing to theory *A*.[16]

I suggest that we examine Bertrand Russell's Theory of Types, as it was developed in *Principia Mathematica*,[17] for some insights into the plausibility of Shapiro's claims.

Russell's theory of types

Principia Mathematica was the culmination of over thirteen years of very intensive research by Russell and his colleague Alfred North Whitehead. It was supposed to provide the world with Russell's solution not only to the paradox that bears his name, but also to a whole class of similar "vicious-circle" paradoxes containing such antinomies as the Cantor and the Burali-Forti.[18] The authors claimed that "the theory of types, as set forth in [*Principia Mathematica*], leads both to the avoidance of contradictions, and to the detection of the precise fallacy which has given rise to them" (Russell and Whitehead, 1927: vol. 1, p. 1). It was hoped that *Principia Mathematica* would provide mathematicians with a solid paradox-free foundation for all their theories by showing how classical mathematics could be derived within its formalized logical system.

The Constructibility Theory is similar to Russell's theory of types in two important respects: (1) it is a "no-class" theory just as Russell's theory is; and (2) the mathematical developments in its formal theory are carried out in a kind of simple type theory, in the way that the mathematical part of Russell's formal theory is.[19] So it is not surprising that much of what Shapiro says about the Constructibility Theory can be also said of Russell's theory of types. Here is how this might be done.

[16] David Kaplan suggested an example such as the above at the *One Hundred Years of Russell's Paradox* conference.

[17] Russell and Whitehead, 1927.

[18] See Chihara, 1973: ch. 1 for more details.

[19] Of course, there are significant differences. For one thing, there is not even an apparent reference to sets in the Constructibility Theory. Talk instead is of the constructibility of open-sentences that function like sets.

As is well known, set theory is developed in *Principia Mathematica* even though no sets are supposed to be within the range of any of its quantifiers. Mathematics is developed within *Principia Mathematica* in a theory that is only about propositional functions and individuals—these are the only kinds of things that are talked about in the theory and this is why it is called a "no-class" theory. However, there are abbreviating definitions by means of which certain sentences of the theory get transformed into sentences that look like sentences of set theory.[20] Mathematics is developed in *Principia Mathematica* essentially using only those sentences that, by means of these abbreviations, look and function like sentences of set theory. As Quine has remarked: "Once classes have been introduced, [propositional functions] are scarcely mentioned again in the course of the three volumes" (Quine, 1966*d*: 22).

Now the actual type theory of *Principia Mathematica* is that horror of students: the very complicated ramified theory of types.[21] However, once the Axiom of Reducibility is accepted, the mathematical part of the system—the set-theoretical part—gets simplified, so that it operates very much like the simple theory of types.[22] Indeed, we can distinguish a sort of sub-theory of *Principia Mathematica*, which formalizes the mathematics of this theory. For purposes of reference, let us call this theory 'RST' (for 'Russell's simple theory of types'). RST has as its sentences the sentences of *Principia Mathematica* that get transformed by its abbreviating definitions into straightforward sentences of the standard set-theoretical version of simple type theory (henceforth to be called 'ST'). We have in effect, then, a translation function f_1 that takes each sentence of RST into a sentence of ST, and it would be a simple exercise to come up with a translation function f_2 that would take each sentence of ST into a sentence of RST.[23] The assertions of RST are just those sentences of the theory that are theorems of *Principia Mathematica*. I suggest we investigate whether RST is a mere notational variant of ST.

The fact that one can translate from RST to ST and back again is no guarantee that the two are notational variants of one another. If RST is just a notational variant of ST, then each sentence of RST should have the same

[20] See Chihara, 1973: ch. 1, sect. 4.

[21] See Chihara, 1973: ch. 1 for a characterization of Russell's ramified theory of types.

[22] How this comes about is explained in Chihara, 1973: ch. 1, sect. 9.

[23] For purposes of simplicity of exposition, I have skipped over the fact that the rules of translation cannot be simply read off the abbreviating definitions of *Principia*, for as Gödel has noted (in Godel, 1964*a*: 212), the authors of that logical work were sloppy about the syntax of their system and a certain amount of work would be required to make the translation rules logically adequate.

meaning as its set-theoretical correlate. After all, it is only the notation that is supposed to be different. So consider the following sentence of RST:[24]

[1] $(\exists F)((x)(F!x \leftrightarrow x = x) \;\&\; (\exists y)F!y)$

This sentence translates into the set-theoretical statement:

[2] $(\exists y)y \in V_1$

where V_1 is the universal set of individuals. But it is highly implausible to suppose that the former has the same meaning as the latter, for the "no-class" sentence logically implies a whole host of other sentences of *Principia Mathematica* (many of which are not sentences of ST) that are not implied by the corresponding set-theoretical sentence. For example, the former sentence of RST logically implies:

[3] $(\exists F)(x)(F!x \leftrightarrow x = x)$

which is not implied by [2]. The set-theoretical sentence says that the universal set of type 1 has a member, and how can that sentence imply that there is a predicative propositional function of order 1 that is satisfied by every individual that is identical to itself?

Thus, the mere fact that one can translate the sentences of one theory into the sentences of another so as to preserve certain mathematically relevant relationships between the sentences does not give one the right to say that the two theories are mere notational variants of one another. Much more needs to be shown.

Consider, then, the question as to whether the above two theories are Shapiro equivalent. To be able to answer this question in the affirmative, one would have to have convincing grounds for thinking that the translation functions preserve at the very least theoremhood (since it is questionable that *Principia Mathematica* has a standard interpretation).[25] So is the translation of

[24] For purposes of perspicuity and simplicity of exposition, I have used a simplified and more contemporary notation. The diligent student can reconstruct an accurate version. The exclamation mark in the above formula [1] is used to indicated that the variable 'F' ranges over *predicative* propositional functions of order 1. For details, see Chihara, 1973: 21.

[25] In his 1918 lectures (Russell, 1956), Russell described the language of *Principia Mathematica* as "a language which has only syntax and no vocabulary whatsoever. . . . [it] aims at being that sort of language that, if you add a vocabulary, would be a logically perfect language" (198).

every theorem of ST a theorem of RST? Take the Axiom of Infinity and the Axiom of Choice (which Russell called "the multiplicative axiom"). These are fundamental axioms of simple type theory. Their status is no different from all the other axioms. But the corresponding statements of *Principia Mathematica* are treated very differently. The authors do not attempt to justify the acceptance of these statements, as they do for the case of the Axiom of Reducibility.[26] Instead, they assert that the Axiom of Infinity, like the Axiom of Choice, "is an arithmetic hypothesis which some will consider self-evident, but which we prefer to keep as a hypothesis, and to adduce in that form whenever it is relevant".[27] And in a later work, Russell wrote: "As to the truth or falsity of the axiom [of choice] in any of its forms, nothing is known at present" (Russell, 1920: 124). Thus, the Axiom of Infinity and the Axiom of Choice are not treated as genuine axioms of the theory. Not surprisingly, we find the authors of *Principia Mathematica* asserting such things as "if the Axiom of Infinity is true", such and such follows.

Making such conditional assertions is perfectly consistent with the authors' practice of proving theorems having these "axioms" as antecedents. Thus, when Russell and Whitehead encountered a theorem of standard mathematics requiring the Axiom of Choice for its proof—say Zermelo's Well Ordering Theorem—what they proved in *Principia Mathematica* was a conditional sentence of the form

$$AC^* \rightarrow WO^*$$

where AC^* is the sentence of RST that is abbreviated by the Axiom of Choice of ST and WO^* is the sentence of RST that is abbreviated by the Well Ordering Theorem of ST.[28] In short, the Well Ordering Theorem is not really a theorem of RST.[29] Of course, the Well Ordering Theorem is a theorem of ST, so it can be seen that theoremhood is not preserved by the translation scheme described above and that the two theories are not Shapiro equivalent after all.

[26] In a section entitled "Reasons for Accepting the Axiom of Reducibility", they claim that "the inductive evidence in its favor is very strong" (Russell and Whitehead, 1927: vol. I, p. 59). No such reasons are produced in favor of accepting the other two axioms.

[27] Russell and Whitehead, 1927: vol. II, p. 203. In Russell, 1920: 117, Russell describes the axiom as "convenient, in the sense that many interesting propositions, which it seems natural to suppose true, cannot be proved without its help; but it is not indispensable, because even without those propositions the subjects in which they occur still exist, though in a somewhat mutilated form". Later in this work, Russell writes: "It is conceivable that the multiplicative axiom in its general form may be shown to be false" (130).

[28] Russell and Whitehead, 1927: vol. I, pp. 536–42. See also Carnap, 1964: 34–5.

[29] It would be grotesque to claim to have proved Fermat's Last Theorem (FLT) in first-order Peano arithmetic on the basis of a proof of the theorem: FLT $\rightarrow$ FLT.

Is the Constructibility Theory Shapiro equivalent to simple type theory?

Consider now Shapiro's claim that my Constructibility Theory is Shapiro equivalent to simple type theory. As I said earlier, the Constructibility Theory is much closer to *Principia Mathematica* than it is to the set-theoretical version of simple type theory. Like *Principia Mathematica*, the Constructibility Theory has no axiom that corresponds to the Axiom of Choice. So we have some reason for thinking that the Constructibility Theory is not Shapiro equivalent to simple type theory, just as we had strong grounds for concluding that RST is not Shapiro equivalent to ST. Furthermore, if the Constructibility Theory is not Shapiro equivalent to simple type theory, then it certainly cannot be a mere notational variant of that theory.

It should now be obvious that the case Shapiro has assembled in support of his claim that the Constructibility Theory is a mere notational variant of simple type theory is very weak indeed. All that he has done is to provide us with a method for translating sentences of the Constructibility Theory into sentences of simple type theory and a method for translating sentences of simple type theory into sentences of the Constructibility Theory, these translations preserving certain mathematically significant relationships. As the above discussion of Russell's Theory of Types shows, there is much more that must be established in order to conclude that the two theories are mere notational variants of one another. We can have such methods of translation, even when a sentence in one theory is quite different in meaning and logical significance from the sentence of the other theory into which it is translated.

I shall now provide additional grounds for concluding that the Constructibility Theory is no mere notational variant of the standard set-theoretical version of simple type theory.

The mystery of infinitely many different empty sets

Let us begin our evaluation of Shapiro's objection by comparing the Constructibility Theory with the simple theory of types regarding the existence of null sets. Some of you may wonder at my use of the plural of the word 'set' in the previous sentence. Is there not at most one null set? Can there be more than one? But in simple type theory, there is a null set of type 1, a null set of type 2, a null set of type 3, ... Quine has expressed his distaste for this feature of the set theory, writing:

> One especially unnatural and awkward effect of the type theory is the infinite reduplication of each logically definable class. ... This reduplication is particularly strange in the case of the null class. One feels that classes should differ only with respect to their members, and this is obviously not true of the various null classes. (Quine, 1938: 131)

What distinguishes the null set of type 1 from the null set of type 2? They have exactly the same members, namely, none at all. So how can they be different?

Now the Constructibility Theory is not subject to this paradoxical feature of simple type theory. What corresponds in the Constructibility Theory to the statement of simple type theory, 'There is a null set of type 1', is the statement, 'It is possible to construct an open-sentence of level 1 that is true of no objects'. The latter statement can be seen to be true, since one can construct, for example, the open-sentence

$$x \neq x$$

which is an open-sentence of level 1 that is true of no object. The statement of the Constructibility Theory that corresponds to the set theoretical statement that there is a null set of type 2 is the statement that it is possible to construct an open-sentence of level 2 that is not true of any open-sentence of level 1. This statement can be seen to be true, since one can construct, for example, the level 2 open-sentence

> *F* is a level 1 open-sentence that is satisfied by an object that does not satisfy *F*.

There is no mystery about how there can be such distinct open-sentences. The open-sentence of level 1 constructed above is easily distinguished from the open-sentence of level 2 that was constructed. The mystery of infinitely many distinct null sets simply does not arise for the Constructibility Theory.[30] The fact that the Constructibility Theory does not give rise to a mystery that bedevils the simple theory of types is evidence that these two mathematical theories are not mere notational variants of each other.

Differences in how the two theories are confirmed

Another reason for questioning Shapiro's thesis that the Constructibility Theory is a notational variant of simple type theory can be found by comparing the grounds we have that support the truth of the two theories. If the two theories are mere notational variants of one another, then any

[30] Cf. Chihara, 1980: 27.

confirmation of one would automatically be a confirmation of the other. Now one consequence of the axioms of the Constructibility Theory is this:

(4) It is possible to construct an open-sentence of level 1 that is not satisfied by any object.

By Shapiro's translation scheme, the corresponding sentence of simple type theory is the sentence:

(5) There exists a set of type 1 of which nothing is a member.

If the two theories in question are mere notational variants of one another, then the grounds supporting the one belief should be the same as the grounds supporting the other. But among the grounds I have for my belief in the former is the fact that I have literally constructed an open-sentence of the kind in question. By a law of modal logic ("actuality implies possibility"), this implies the truth of (4). But that law of modal logic does not allow me to infer (5) from my construction: the fact that I have constructed some open-sentence does not imply, in modal logic, that the null set of type 1 exists. Thus, we get a kind of direct verification of the constructibility statement that one cannot get from the set-theoretical case. After all, how does one justify the belief that there exists a null set of type 1? If one justifies the belief at all, it will be on very general grounds—no doubt involving Quinean considerations about what is our best scientific theory of the world. Nothing like that is needed in the open-sentence case.

Of course, if the two theories in question were indeed mere notational variants of one another, then one would expect all the justifications of the axioms of one theory to be translatable into justifications of the translated versions of that axiom. But it is hard to see how the kind of modal justifications of the axioms of the Constructibility Theory I give in my book can be translated into justifications of the axioms of the set-theoretical version of simple type theory. Thus, consider the Abstraction Axiom of the lowest level. It tells us that for any condition on x expressible in the language of **Ct**,

$$- - - x - - -$$

it is possible to construct a level 1 open-sentence that is satisfied by all and only those objects that satisfy the condition.[31] But any condition on x expressible in the language of **Ct** is an open-sentence of the sort required that

[31] For purposes of simplicity here, I am considering only the case in which the condition in question contains no occurrences of any free variable other than x.

it is possible to construct. So it obviously is possible to construct an open-sentence that is satisfied by all and only those objects that satisfy the condition. But this modal justification is not directly translatable (certainly not by the simple method of translation that Shapiro gives) into a set-theoretical justification of (this case of) the set-theoretical version of the Abstraction Axiom.

I conclude that Shapiro's thesis that the two theories in question are mere notational variants of one another is not only unfounded, but its credibility is overwhelmingly undermined by the considerations cited above.

5. Other Objections Shapiro Has Raised

Shapiro's objection to my use of possible worlds semantics

As I noted in the beginning section of this chapter, the kind of possibility with which I am concerned in the Constructibility Theory is that of the "conceptual" or "broadly logical" sort, the system of derivations for which is widely believed to be formalized in S5 modal logic. The semantics of an S5 system is generally given in terms of possible worlds. The overarching idea behind such a semantical system is this. Imagine that there is a huge totality of universes (or "possible worlds") such that anything that could happen does happen in at least one of these universes. Then, to say that $\Diamond\phi$ (it is possible that ϕ) is to say that there is a possible world w such that ϕ is true in w. To say that $\Box\phi$ (it is necessary that ϕ) is to say that, for every possible world w, ϕ is true in w. Using these ideas, one can devise model-theoretical definitions in terms of which the fundamental logical notions of validity, semantical consistency, soundness, and completeness can be defined. One can define, within this framework, what an "interpretation" of the language is, the idea being that the interpretation will supply:

a non-empty set **W** to represent the set of possible worlds;

a member **a** of **W** to represent the actual world;

a function **U** to assign to each member **w** of **W** a set of elements to represent "the universe of **w**" (the set of things that exist in **w**);

a function to assign to every pair, whose first element is a unary predicate F of the language and whose second element is a member **w** of **W**, a subset of what **U** assigns to **w** to represent the extension of F in the world represented by **w**;

(and similarly for the case of binary, ternary, . . . predicates).

One can then define what it is for a sentence of the language to be true under an interpretation, and in terms of this relative notion of truth, one can define the above-mentioned logical notions of validity, semantical consistency, soundness, and completeness. Standard versions of derivational systems of S5 quantificational modal logic can then be formulated and proved to be sound and complete.[32]

Consider now the constructibility quantifier. This functions as a kind of logical constant and is taken to be a primitive of my system in much the way 'is a member of' is a primitive of set theory. However, it was important to provide readers of *The Worlds of Possibility* (Chihara, 1998) with explanations of how this kind of quantifier functions and also of how inferences involving this primitive are to be made. Since the semantics of S5 type modal systems was already well known, I chose to explain the basic features of the constructibility quantifier in terms of the possible worlds idea, using the kind of set-theoretic semantical system that was already in use in modal logic. However, as Shapiro notes explicitly, I warned the reader that my possible worlds semantical explanations should not be taken to be literal descriptions of how constructibility quantifiers function. I said: "[T]his whole possible worlds structure is an elaborate myth, useful for clarifying and explaining the modal notions, but a myth just the same" (Chihara, 1990: 60).

It is on this point that Shapiro jumps in with an objection, writing:

> If the structure really is a myth, then I do not see how it explains anything. One cannot, for example, cite a story about Zeus *to explain a perplexing feature of the natural world such as the weather*. . . . In everyday life, a purported explanation must usually be true, or approximately true, in order to successfully explain. (Shapiro, 1997: 232–3, italics mine)

Here, Shapiro seems to be confusing two very different sorts of explanations:

(A) scientific explanations of natural phenomena,

and

(B) explanations of ideas or the meaning and use of expressions.

Now Shapiro can certainly make a plausible case that the former, type (A) explanations, must be true, or approximately true, in order to successfully explain some natural phenomenon, although, as Bas van Fraassen has emphasized, "There are many examples, taken from actual usage, which show

[32] See Chihara, 1998: ch. 1 to see how this is done.

that truth is not presupposed by the assertion that a theory explains something" (van Fraassen, 1980: 98), and I can think of cases in which one makes use of imaginary states of affairs in order to explain some complicated natural process. However, I shall not contest Shapiro's case as it applies to type (A) explanations, since the objection only goes through if it also applies to type (B) explanations. Instead, I shall argue that, first, my explanations of how the constructibility quantifiers function, using possible worlds semantical ideas, were type (B) explanations; second, that type (B) explanations are not precluded from using myths or imaginary things and states of affairs.

Clearly, my explanations of how the constructiblity quantifier functions was a type (B) explanation. I was introducing, in my logical system, a novel logical constant. So a type (B) explanation was called for. Besides, philosophers only rarely put forward scientific explanations of natural phenomena. So why should one suppose that I was offering a type (A) explanation?

Now if one is explaining or clarifying some idea or one's use of a term or phrase, what is to prevent one from making use of a myth or purely imaginary things and situations? Why could one not use a blatantly false theory in explaining an idea, if the logical features of the false theory are well understood? Let me give you an example. For many years, intuitionism was a very puzzling and confusing theory to many classical mathematicians: the intuitionistic restrictions of mathematical operations seemed arbitrary or simply unintelligible. Then a number of interpretations of intuitionistic ideas were given within the framework (and using the concepts) of classical mathematics. These interpretations made intuitionism intelligible to many classical mathematicians, even though the classical theory in terms of which intuitionism was being explained was, to many intuitionists, false. From the point of view of the intuitionist, a false theory that was well understood was being used to explain a true theory that was not understood at all. Similarly, I wanted to make the logic of my Constructibility Theory intelligible to mainstream philosophers and logicians, who were familiar with possible worlds semantics, even though I was convinced that all this talk of possible worlds was fictional.[33] By proceeding in the above way, I expected those familiar with modal logic to learn how to use my constructibility quantifiers.

Take the imaginary world Flatland of Edwin Abbott.[34] This is a world in which all things in it are portrayed as existing and moving about as flat

[33] See Chihara, 1998 for a detailed expression of my attitude towards possible worlds semantics.

[34] The reader can find a short description of Abbott's work, as well as some selections from *Flatland*, in Newman, 1956: 2383–96.

objects on a two-dimensional plane. The inhabitants of Flatland turn out to be such geometric objects as triangles. Trying to imagine how the inhabitants of Flatland would view a three-dimensional object, say a sphere, entering their world and passing through it has been thought to shed light on certain aspects of relativity theory.[35] Ian Stewart has suggested that the scientific purpose of the work was "serious and substantial": "Abbott's sights were focused not on the Third Dimension—familiar enough to his readers—but on the Fourth Dimension. Could a space of more than three dimensions exist?" (Stewart, 2001: vii).

Is one to react to Flatland by claiming, as Shapiro has, that no myth can be used to explain anything? Surely the philosophical literature is filled with examples in which imaginary situations and worlds are used to explain various ideas and conceptual possibilities. Anyone who has worked in modal logic and the philosophy of necessity can come up with countless examples of the use of such imaginary situations and states of affairs to explain some principle, concept, or doctrine.[36] And surely those who have studied Wittgenstein's writing can cite abundant examples in which imaginary societies or tribes (i.e., "myths") are effectively used to explain the author's ideas or his use of some term.[37] Indeed, such devices are so common in the writings of analytic philosophy that few contemporary philosophers, I dare say, would accept Shapiro's thesis on explanations.

The constructibility quantifiers

Closely related to the "notational variant" objection I discussed earlier is another that Shapiro raises against my constructibility quantifiers. By reasoning that brings to mind this "notational variant" objection, Shapiro argues that my constructibility quantifiers are no different in logical meaning from standard existential quantifiers. More specifically, Shapiro's contention is that "the constructibility quantifier has virtually the same semantics as the ordinary existential quantifier." Here is how Shapiro reasoned:

> As he [Chihara] describes the system, in a given model, the variable ranges are dutifully distributed into different possible worlds, but this fact plays *no role* in the definition of satisfaction in the modal system he develops. Every object (and every predicate) is rigid and world-bound, and each constructibility quantifier ranges over all objects (of appropriate type) in all worlds. Thus, the worlds do not get used anywhere—just the objects in them. In short, the (mythical) semantics that Chihara develops is just ordinary *model theory*, with some irrelevant structure thrown in. . . . Thus, my contention is that the constructibility quantifier has virtually the same semantics as the ordinary existential quantifier. (Shapiro, 1997: 233).

[35] This was proposed in an anonymous letter published in the 12 February 1920 issue of *Nature*.

[36] Even a cursory perusal of my book (Chihara, 1998) will yield numerous examples of the sort being described.

[37] See, for example, Wittgenstein, 1958 and Wittgenstein, 1953.

As in the previous cases, this is a very harsh criticism. But is it accurate? First of all, is it true that all the objects in the modal system are "world-bound"? Where did Shapiro get that idea? I make it clear that an object, thing, or person in one world can appear in many possible worlds. For example, I write: "If, in one possible world, someone makes certain speech sounds, and if in another possible world, *this same person* makes the very same sounds, we are not forced to conclude that the very same open-sentence token was produced" (Chihara, 1990: 60, italics added). Furthermore, when one examines the semantics of the language in which Ct is formulated, one finds no requirement, explicit or implicit, that the objects in each world must occur in only one world. So Shapiro's claim that the objects in the modal system are all world-bound is simply false.[38]

In describing the formal language in which Ct is formulated, I make it clear that I have omitted certain logical features of the language for purposes of facilitating the exposition of the system. For example, I make it clear that the vocabulary of the language in question has been simplified so as to contain only the non-logical constants that express satisfaction and identity. Of course, to use the theory to validate inferences involving the use of mathematics that we routinely make in science and everyday life, one would need predicates other than satisfaction and identity. But since these other predicates would not be needed to describe the axioms of the theory, I dropped them from consideration in the exposition. This is why I wrote: "In a fuller exposition, I would include an infinite number of predicates" (Chihara, 1990: 56).

For similar reasons, I dropped from consideration all existential quantification over the type n entities ($n > 0$). But in a fuller exposition of the language, I would certainly include a discussion of existential quantification of that sort. I would definitely want to express in the language, such thoughts as:

[1*] An open-sentence ϕ exists which is such that exactly two objects satisfy ϕ.

[38] I think Shapiro was misled by a suggestion I made that one could avoid a certain problem by treating the open-sentence tokens in each possible world as "world-bound". See Chihara, 1990: 60–1. Of course, this suggestion was never intended to apply to all the objects in each possible world, but only to the open-sentence tokens.

Thus, in a fuller exposition, the language would contain the following sentence:

[1] $(\exists x_1)(\exists x)(\exists y)(Sxx_1 \mathbin{\&} Syx_1 \mathbin{\&} (z)(Szx_1 \rightarrow (z=x \vee z=y)))$.

Now as the semantics of the language makes clear, from this sentence, one could infer that it is possible to construct an open-sentence which is such that exactly two objects satisfy it. That is, one could infer:

[2] $(Cx_1)(\exists x)(\exists y)(Sxx_1 \mathbin{\&} Syx_1 \mathbin{\&} (z)(Szx_1 \rightarrow (z=x \vee z=y)))$.

But from [2], one cannot infer [1]—which shows clearly that constructibility quantifiers are not just existential quantifiers. These logical differences should make evident, even to those unfamiliar with possible worlds semantics, that the worlds do play an important role in the semantics of the constructibility quantifiers.

In other words, the apparent similarities that led Shapiro to his conclusions are due to the fact that the constructibility quantifiers were being explained and discussed in a very simplified and impoverished context—this for didactic and heuristic purposes.

In claiming that "the constructibility quantifier has virtually the same semantics as the ordinary existential quantifier", Shapiro seems to have overlooked or ignored completely a whole chapter of the book (chapter 2) which is devoted entirely to the possible worlds semantics of the constructibility quantifier. Now, a constructibility statement does not, strictly speaking, entail that an *entity* that satisfies some condition can be brought into existence: the construction of an open-sentence may consist simply in the waving of a flag in various ways. However, within the set-theoretical setting in which possible worlds semantics is developed, the constructibility of something is *represented* by there being some element of the appropriate type in a possible world. For example, I give an example of a language with constructibility quantifiers for which possible worlds interpretations (called '**K***-interpretations') are defined. In this language, there are two kinds of variables: starred variables to be used in constructibility quantifiers and unstarred variables to be used in ordinary existential quantifiers. There are two kinds of things talked about in the language, when it is given a **K***-interpretation: 0-things, which are the kind of things that concern the existential quantifier, and the 1-things, which are the kind of things that are said to be constructible. Truth under a **K***-interpretation is defined in such a way that:

(1) the truth (under a **K***-interpretation) of an existential statement depends only upon what 0-things exist in the actual world;

(2) the truth (under a **K***-interpretation) of a constructibility statement depends upon what 1-things exist in all the possible worlds.

Thus, in discussing the truth conditions for existential statements, I wrote:

> [A]s far as what is relevant to truth under a **K***-interpretation, the 0-things from all possible worlds other than the actual world are not significant: the domain of the standard [existential] quantifiers can be regarded as the set of things in the actual world.. . .[On the other hand] the constructibility quantifier can be regarded as ranging over the totality of 1-things from all the possible worlds. (Chihara, 1990: 35).

This semantical definition reflects the fact that existential statements (using the standard existential quantifier) assert the actual existence of something, that is, the existence of something in the actual world, whereas constructibility statements do not—they only assert that something (an open-sentence, say) could exist, that is, that something of the appropriate sort exists in some possible world (not necessarily the actual world). Clearly, the distinction between the actual world and the other worlds in **W** is used, and plays a significant role, in the semantics of constructibility quantifiers. So it is hard to see how the differences (between how truth under an interpretation is defined for the existential and the constructibility statements respectively) are compatible with maintaining, as Shapiro does, that "the constructibility quantifier has virtually the same semantics as the ordinary existential quantifier".

Shapiro's second objection to my use of possible worlds semantics

As I have emphasized several times already, the possible worlds semantics I gave, in explaining how the constructibility quantifier functions and how the deductive system of the Constructibility Theory operates, was not used to define the meaning of the logical and modal terms of the system: it was set forth primarily as a didactic device—a kind of model to facilitate transmitting to the reader an understanding of certain logical features of my system—or as a heuristic instrument for presenting or investigating modal situations in a perspicuous manner. But Shapiro thinks that the modal notions I use, without the aid of the sort of model theory (and hence set theory) in terms of which my theory is described, are simply not determinate and precise enough to develop mathematics in the purely modal way I advocate. Thus, he writes:

> I submit that we understand how the constructibility locutions work *in Chihara's application to mathematics* only because we have a well-developed theory of logical possibility and satisfiability. Again, this well-developed explication is not *primitive or pretheoretic*. The articulated understanding is rooted in set theory, via model theory.

Set theory is the source of the precision we bring to the modal locutions. Thus, *this (partial) account of the modal locutions is not available to an antirealist....*In short, we need some reason to believe that, when applied to the reconstruction of mathematics, constructibility quantifiers work exactly as the model-theoretic semantics entails that they do. (Shapiro, 1997: 232, italics mine)

There are several things that should be said in response to this objection. Consider the second sentence, in the quotation above, 'Again, this well-developed explication is not primitive or pretheoretic'. Here, Shapiro seems to be taking the word 'primitive' to mean essentially what 'pretheoretic' means. Thus, since I characterize my constructibility quantifiers as 'primitive', Shapiro concludes that they are "pretheoretic".

Here is what Shapiro says that indicates how he came to attribute to me the view described above.

Clearly, constructibility quantifiers are established parts of ordinary language, and competent speakers do have some grasp of how they work. For example, we speak with ease about what someone could have had for breakfast and what a toddler can construct with Lego building blocks. Moreover, there is no acclaimed semantic analysis of these locutions, model-theoretic or otherwise, as they occur in *ordinary language*. These observations seem to underlie Chihara's proposal that the locutions are "primitive." We use them without a fancy model-theoretic analysis. (Shapiro, 1997: 232).

In this quotation, Shapiro is contrasting his own view of how the constructibility quantifier could obtain sufficient precision to be applicable to mathematics (by the use of model theory) with the view he attributes to me according to which I supposedly maintain that my constructibility quantifier is a pretheoretic, "primitive" notion of ordinary language—it supposedly is a notion that is unrefined by the techniques and results of model-theoretic semantics.

Shapiro's understanding of my position is partially correct: I do hold that there are constructibility quantifiers which are "established parts of ordinary language" and I do think "competent speakers do have some grasp of how they work." But I do not maintain that the constructibility quantifiers of ordinary language are identical to the constructibility quantifiers of Ct—they are, as I see it, very similar in grammatical structure and function, but not completely identical.

Consider the relational expressions 'is a member of', which is also an established part of ordinary language. We say such things as:

John is a member of the class of 2008.

Tracy is a member of the gang that has been terrorizing the neighborhood.

A member of the pack of wolves seen in the neighborhood has just killed a chicken.

I believe that such uses of the expression have led some mathematicians to believe that school classes, gangs, packs, bunches, and flocks are really sets.[39] Certainly, the expression 'is a member of' of ordinary language has much in common, both logically and grammatically, with the term used in set theory. But it would be a mistake to suppose that school classes, gangs, packs, and so on should be *identified* with the sets spoken of by mathematicians. A pack of wolves, for example, is not thought to go out of existence just because some member of the pack is killed: "That same pack has returned to ravage the countryside", it might be said, even though a member of the pack has been shot. No mathematician would believe that a set which has as members all but one member of a set *A* is identical to *A*. In other words, the mathematician may use, in his or her theorizing, an expression that is a part of ordinary language but give it a special sense that is subtly different from that of the expression in ordinary usage. And these differences may very well be the result of sophisticated theoretical analysis and mathematical reasoning. Thus, when Zermelo formulated his axiomatization of set theory, he was not merely setting down in axiomatic form ideas already implicit in our ordinary use of such expressions as 'is a member of'. It would be the height of implausibility to suppose that Zermelo's axiom of choice was implicit in the ordinary everyday use of the expression 'is a member of'.

Shapiro thinks that my constructibility quantifier is a "pretheoretic" notion because of such passages as the following:

It needs to be emphasized however that the semantics of possible worlds, which is to be used here, is brought in merely as a sort of heuristic device and not as the foundations upon which the mathematics of this work are to be based: *the constructibility quantifiers are primitives of this system*, and the Platonic machinery of Kripkean semantics is used to make the ideas comprehensible to those familiar with this heavily studied area of semantics. (Chihara, 1990: 25, italics added)

Shapiro seems to think that, in calling the constructibility quantifier a "primitive" of my system, I am implying that this logical constant of my system is primitive, in the sense of pretheoretic, crude, unrefined, rudimentary, untutored, or naive. However, as I used the term 'primitive', it was as a relative

[39] See, for example, Halmos, 1960: 1.

term—relative to the system in which it is being used: a notion that is a primitive of one system may occur as a defined notion in another system. The membership relation is a primitive of set theory; it is a defined term in *Principia Mathematica*. Frege emphasized that not every term in one's theory can be defined: some notions in the theory must be undefined. In Frege's system, *concept* and *extension of a concept* are primitive notions; whereas *zero* and *successor* are defined. In setting up a formal system, one chooses the terms that are to be primitives and the terms that are to be defined terms.[40] It can be seen that I was using the term 'primitive' in the passage quoted above in the sense of "primary", "assumed as a basis", or "undefined and original". There is no suggestion that the constructibility quantifier is a rudimentary, untutored, or naive pretheoretic notion. Thus, although ordinary, non-philosophical speakers of ordinary English do say such things as "It is possible to construct houses made entirely of ice", it should be kept in mind that the kind of possibility expressed in that double-quoted sentence should not be assumed to be the kind of "conceptual" or "broadly logical" possibility that is required in my constructibility quantifier. I would certainly not claim that such a heavily philosophically studied notion of possibility is a "pretheoretic" or preanalytic notion of ordinary language.

What about Shapiro's suggestion that, without the use of possible worlds semantics (and hence set theory), the logic of the constructibility quantifier would not be sufficiently determinate and precise to carry out the sort of reasoning required to develop mathematics within the Constructibility Theory? At least part of Shapiro's reason for maintaining such a position is his mistaken belief that, in calling the constructibility quantifier a "primitive" of my system, I was taking this logical constant to be a crude "pretheoretic" notion of ordinary language. As I noted earlier, a primitive of a system can be a highly refined, widely studied, and carefully analyzed theoretical term of a sophisticated system. For example, the terms 'set' and 'is a member of' are primitives of Zermelo's 1908 axiomatization of set theory.[41] The terms are not given model-theoretic analyses or definitions. Yet it would be wrong to classify them as crude "pretheoretic" terms of ordinary language. For Zermelo's axiomatization was made as the result of a careful and deep study of

[40] Some readers may find it enlightening to ponder the sorts of consideration that go into choosing what is and what is not to be a primitive of one's system. See in this regard, Chihara, 1998: 79–81.

[41] Zermelo, 1967: 201. There is no suggestion that these terms should be taken to be crude or unrefined concepts of the theory.

the works of practicing mathematicians—especially their use of set theory—at least partially in response to the various antinomies of mathematics that had been discovered and also partially in response to objections that had been made by eminent mathematicians to his proof of the well-ordering theorem.[42] Essentially the kind of theorizing, analysis, clarification, and theoretical concept formation that went into Zermelo's axiomatization can be carried out for the case of modal reasoning as well. Model theory is not essential to such a process. It is a process of theorizing that philosophers have been carrying out since the time of Aristotle, long before model theory was invented. I can find nothing in what Shapiro has argued that precludes developing the logic of the constructibility quantifier to a sufficient degree of precision to carry out the sort of development described in Part I of this chapter.

Let us now investigate the question as to whether one can have a sufficiently sophisticated understanding of the logic of the constructibility quantifier, without making any appeals to model-theoretic notions or other aids from possible worlds semantics, to make the sort of applications to mathematics that is the concern of Shapiro in the quotation above. Let us consider the development of finite cardinality theory given in Sections 2 and 3 of this chapter. Notice that the exposition and discussions of this theory (including all the proofs of theorems) are given without any appeals to any model-theoretic notions or to results from possible worlds semantics. Deductions and inferences are made using modal reasoning and without any mention of set theory or set-theoretical results. Yet, it can be seen that standard theorems of number theory can be obtained within this system. Those who think, with Shapiro, that such "applications [of the Constructibility Theory] to mathematics" can only be made with the aid of set theory should try to find some specific theorem I have proved that requires, in their opinion, a hidden appeal to set theory. This would provide both sides of the dispute with a definite example to investigate, to see if at some point in the reasoning, there is a surreptitious appeal to some model-theoretic result. In the absence of any such specification, I find unconvincing Shapiro's rather unspecific objection that somewhere in my development of mathematics, I need set theory to carry out the reasoning.

It might be objected that I do appeal to Frege's development of finite cardinality theory in developing my constructibility version of the theory, and since Frege's development is Platonic in nature (presupposing as it does

[42] See, for details of Zermelo's motivation for his axiomatization, Bach, 1998.

the existence of extensions of concepts), one can argue that my version itself presupposes the existence of abstract mathematical objects. In answer to such an objection, it should be noted that Frege's theory of cardinality is only used as a sort of model—there is no theorem of Frege's whose truth is presupposed anywhere in my development of number theory. There is clearly no obstacle to using various Platonic theories (such as set theory) in this way.

Can a nominalist make use of model theory?

Shapiro relates that Burgess once argued, at a convention, that it is far-fetched for someone who learned much about logical possibility from a high-powered course in mathematical logic, using a text like Shoenfield's *Mathematical Logic* (Shoenfield, 1967), to go on to claim a primitive pretheoretic status for this notion. Shapiro adds: "The same goes for constructibility assertions" (Shapiro, 1997: 238 n.). This last point lies behind the previously quoted claim of Shapiro's that set theory is the source of the precision we bring to the modal locutions and that "this (partial) account of the modal locutions is not available to an antirealist".

It can be seen that the crucial presupposition of this criticism is the following "illegitimacy thesis":

> It is illegitimate for Chihara to make use of set theory in explicating or clarifying the logic of the constructibility quantifier.

Why should one accept the illegitimacy thesis? In stating his objection, Shapiro does not supply any explicit argument supporting this thesis. But it is not difficult to see what he is thinking. He knows that:

(1) I do not think that the axioms of set theory, as standardly (Platonically) understood, are literally true assertions.

He also believes that:

(2) one cannot use an untrue theory in explaining or explicating anything.

So he concludes that I cannot make use of set theory in explicating or clarifying the logic of the Constructibility Theory.

But as I argued earlier, I reject (2) completely, so this line of reasoning can also be seen to be unconvincing. After all, what is wrong with, say, using set theory or model theory as an aid to the conceptual clarification of some of the finer points of the constructibility quantifiers or of the broadly logical notion of possibility? Certainly if the use I make of set theory presupposes that its theorems, as standardly (Platonically) understood, are literally true assertions,

then a legitimate complaint can be lodged against me. But that I have used set theory in that way has by no means been shown.

Can model theory be legitimately used by the anti-realist as a tool in clarifying, explicating, and investigating the logical features of the constructibility quantifier, without presupposing the existence of mathematical objects? I shall respond to this question in Chapter 9.

6. RESNIK'S OBJECTIONS

Resnik raises four main objections to my theory. In what follows, I shall take them up in the order in which they are given.

Resnik's induction objection

The first is the objection that I presuppose mathematics in justifying the axioms of the Constructibility Theory—the suggestion being that I thereby presuppose the existence of mathematical entities in providing this justification. The basis for this objection is the fact that I use a form of mathematical induction in my justifications of the axioms of the Constructibility Theory (Resnik, 1997: 61–2).

Resnik considers a possible reply. I might argue, he suggests, that the principle of induction I use should itself be understood as a derived principle of the Constructibility Theory, so that the inductive principle I use can be understood as concerned merely with the constructibility of open-sentences. To this, Resnik replies:

> But this would call for a further constructibility theory—a metaconstructibility theory—since he is trying to justify his initial constructibility theory. It would then be fair to ask for the justification of this metatheory. Presumably, Chihara would use induction to justify this theory, and we then would press him to eliminate it. ... Chihara should be obliged to stop the regress at some point and give a neutral, non-mathematical justification of his system. (Resnik, 1997: 61–2)

I do not wish to contest the claim that the principle of induction I use is, in some sense, a variation on the principle of induction used in number theory. Nor do I wish to deny that this principle is, in some sense, mathematical in nature. What counts as mathematical is somewhat vague and little is to be gained by arguing about such matters. The crucial question we need to ponder here, however, is whether or not the use of this principle ontologically commits one to the existence of mathematical entities. So let us consider in more detail the way this principle is used.

I start with a specification of a rule for constructing the arabic numerals, '1', '2', '3', ... Now suppose that the following two things are proved:

[1] Every open-sentence ϕ of the level given by the numeral '1' is such that ϕ satisfies condition F.

[2] All arabic numerals n and m that it is possible to construct and all open-sentences ϕ and θ that it is possible to construct are such that if m immediately follows n according to the specification mentioned above and if ϕ satisfies condition F, then θ satisfies F.

The principle of inference I employ, then, allows one to infer that it is not possible to construct an open-sentence ϕ of any level such that ϕ does not satisfy F. Now such a rule of inference is not a rule about mathematical objects. There is no mention of abstract mathematical objects or any quantification over mathematical objects. Call it a mathematical principle if one likes (because of its similarity to the familiar principle of number theory), but calling it 'mathematical' should not blind us to the fact that it is basically a modal principle—a principle not about abstract mathematical entities but about what it is possible to construct.[43]

There is no need to appeal to a metatheoretic version of the Constructibility Theory to justify the use of this principle. Nor it is necessary to appeal to the standard principle of mathematical induction to see that this modal principle is valid—its validity can be grasped directly.[44]

Resnik on the Axiom of Choice

Resnik's second objection concerns the fact that I do not include the Axiom of Choice among the axioms of the Constructibility Theory. He comments: "[The axiom] is now part of standard mathematics, and is required for some theorems that are employed throughout science. Thus it should be given a correlate in his system" (Resnik, 1997: 62).

There are two reasons why I did not regard the absence of the Axiom of Choice from the list of axioms of the Constructibility Theory as a serious problem. First of all, I was confident that no use of mathematics in the empirical sciences requires that the Axiom of Choice be true. As will become apparent in later chapters of this work, I believe that the axiom in question is not the kind of axiom that needs to be true, especially when it is literally

[43] For a more detailed discussion of my use of this nominalistic form of mathematical induction, see Chihara, 1973: ch. 5, sect. 2.

[44] Cf. Chihara, 1973: 178–81.

construed, to be useful in applications. In any case, if Resnik wishes to push this line of attack, he should point to a specific application of mathematics which necessitates the *truth* of the axiom. Secondly, I intended the mathematics of the Constructibility Theory to be that of *Principia Mathematica*, which is generally regarded as a formalization of the classical analysis adequate for all applications in the sciences. Now as I mentioned earlier, Russell and Whitehead proceeded in *Principia* without justifying the Axiom of Choice. As I pointed out earlier, whenever they needed to prove a theorem ϕ that depended upon the Axiom of Choice, they proved in *Principia* a theorem of the form:

$$\text{CHOICE} \rightarrow \phi$$

—something I could do easily in the Constructibility Theory. Notice that if, for some reason, scientists find it useful or convenient to formulate one of their theories in terms of a kind of set-theoretical structure in which Choice holds, it would be a simple matter to apply to such structures the theorems of the Constructibility Theory proved in the above way as dependent upon Choice.

Resnik's objection to my justification of the Abstraction Axiom

Resnik describes the constructibility version of the Abstraction Axiom in the following way:

In a simplified form it postulates that for any object y and any condition '... x ... y ...' formulated in the constructibility theory, an open-sentence is constructible that is satisfied by just the (constructible) things w that are such that ... w ... y .... Of course, '... w ... y ...' is an open sentence. But it does not verify Chihara's axiom because it does not *mention* the object y. The letter 'y' occurs in it as a free variable. (Resnik, 1997: 63)

Now suppose that the variable 'y' refers to an object k. Here is what I wrote:

Then it is reasonable to maintain that there is some possible world in which the language of this theory is extended to include a name of the object k. Then the formula expressing the condition in question can be converted into the required open-sentence by replacing all free occurrences of ['y'] by the name of k, and surely it is possible to do this. (Chihara, 1990: 66)

There are two parts to Resnik's objection to this reasoning, the first of which concerns my talk of a possible world. He realizes that my talk of possible worlds is not intended to be taken literally and that this use of words is only a heuristic device, but he objects that I am unjustifiably using intuitions about possible worlds in my justification of the axiom.

The second part of Resnik's objection is specifically directed at the passage in the quotation above expressing the idea that in some possible world there would be an extension of the language of the theory which includes a name of *k*. Now, Resnik remembers a passage in my book in which, in answer to the question 'What would it be like for a token of a type to exist in a possible world?', I reply: "Here, we can imagine a possible world in which some people . . . do something that can be described as the production of the token" (Chihara, 1990: 40). He infers from this that what it means to say that some token of a certain sort is constructible is that it is possible for people (that is, humans) to do the constructing. With such a restricted view of constructibility (constructibility by humans), Resnik then can go on to respond to my justification of the Abstraction Axiom by claiming:

> [I]t seems to me that there may be physical objects that it is not humanly possible to name, simply because it is not humanly possible to identify them with sufficient precision. They might be too small, too fast, or too fleeting. (Resnik, 1997: 64)

But here, Resnik has simply drawn the wrong conclusion from the example quoted above in which some people do the constructing of a token. It is clear from the context from which the quote is taken that the example was brought in to explain *what it would be like for a token to exist at a world*—the example makes it clear that it need not be the case that there be some entity (the token) that exists at the world. It could be sufficient that some intelligent being performs some act (say, waves a flag in a certain way). The point being made was that "we need not concern ourselves with questions about the ontological status of tokens: in particular, we need not worry ourselves over whether a series of hand signals is or is not an entity, or whether it is a physical object of some sort" (Chihara, 1990: 40). Thus, it would be a mistake to infer from this example that what 'it is possible to construct' means in the Constructibility Theory is: it is possible *for humans* to construct. I never intended any such restricted interpretation, and the quotation Resnik cites in his book does not justify any such interpretation.

It should be clear, in any case, from the S5 modal system used in the formalization of the Constructibility Theory and from the justifications of the axioms of the theory given in the book, that the constructibility quantifier '(C*x*)' ought not to be understood in the restricted way Resnik understand it.

I suspect that Resnik, deep down, realizes that I had no such understanding of the constructibility quantifier, because immediately following his

objection to my proof of the Abstraction Axiom, he says: "[I]f Chihara simply means that for any object *k* it is logically possible that some being tokens a name for it, then his intuition seems more plausible" (Resnik, 1997: 64). He then continues: "In the end, then, Chihara's epistemology amounts to the epistemology of logical possibility." So perhaps even Resnik did not take the above objection to be a serious one. Of course, what I meant by the passage in question was that, for any object *k*, it is possible, in the 'conceptual' or 'broadly logical' sense of "possibility" discussed earlier in this chapter, that some being tokens a name for it.

Let us now return to the first part of this objection in which Resnik complains about my appeal to intuitions about possible worlds. I simply brought in talk of possible worlds as a heuristic device to aid the reader to call up the modal intuitions needed. Possible worlds semantics is frequently used as a sort of heuristic aid—much as we use Venn diagrams in assessing reasoning with categorical syllogisms. For example, one can see that a certain modal argument is invalid, by constructing a possible worlds diagram from which one can specify a structure in which the premises are true and the conclusion is false. Resnik seems to think that this would be an illegitimate use of possible worlds semantics, since the anti-realist does not believe in possible worlds and yet is using his or her intuitions about worlds to make these inferences. But it takes very little sophistication in modal reasoning to see that such a possible worlds diagram can be converted into a specification of the meanings of the predicates and a description of *how the world could have been* such that the premises would be true and the conclusion false.[45] In other words, such diagrams can be used to give the essential elements needed to specify how the premises could be true and the conclusion false—all this without making use of any intuitions about what possible worlds exist.

Similarly, we need no intuitions about possible worlds to follow the train of thought about the Abstraction Axiom that is the target of the first part of Resnik's objection (although, we do need intuitions about what is possible). The first sentence in the above quote can be understood to say: "Then it is reasonable to maintain that there could have been beings who extended the language of the Constructibility Theory to include a name of the object *k*." All my talk about possible worlds can easily be translated into talk about what could have been the case, as anyone familiar with the modality in question could have inferred.

[45] The reader can obtain a clear idea of how this can be done by studying how this is done for the modal sentential logic in Chihara, 1998: ch. 6.

Resnik's skepticism about modality

All of the above is only a preliminary to his most fundamental objection to my Constructibility Theory: Resnik has doubts about modality in general. First, he is skeptical that we have the epistemological means to know complicated modal facts of the sort required by the Constructibility Theory. Second, he is even skeptical that any of the theorems of the Constructibility Theory are true, since he doubts that there are any modal facts at all—he is a "non-cognitivist" when it comes to modal statements, believing that no modal statement has a truth value.

I shall take up first his doubts that there are any modal facts. Here's how he states his position: "I doubt that there are any modal facts to be known—even when the modality is that of logical possibility" (Resnik, 1997: 64). When it comes to logical possibility, he expressed his doubts this way:

> The view I am proposing is a restrained *logical non-cognitivism:* sentences of ordinary language that seem *categorically* to attribute logical necessity or other logical properties and relations actually perform other functions, and are neither true nor false. (Resnik, 1997: 167)

What are these "other functions" that such categorical sentences perform? Here's the sort of analysis Resnik provides. Suppose that we assert that Frege's axioms are contradictory or inconsistent. According to Resnik's analysis, our utterance informs our audience that we expect Frege's axioms "to be treated in a certain way": we show that we are confident that our audience can see for themselves that Frege should "retract or qualify" his axioms (Resnik, 1997: 168). A second function of such statements is to express a certain commitment: in this case, to the falsity of the axioms.

I am skeptical of this analysis. When I tell a class of students that Frege's axioms are inconsistent, I certainly do not intend to show the class that I am confident that they can *see for themselves* that Frege should retract or qualify his axioms. It has been my experience that most students who are not mathematics majors are simply not able to carry out the sort of derivation needed to see that the axioms are inconsistent.

Resnik's non-cognitivist analysis of this case seems to be the result of the sort of strategy adopted by some phenomenologists regarding statements "about physical objects": find some implications of the analysandum—for the modal non-cognitivist, implications that are non-modal—and then claim that the function of an utterance of the sentence is to inform the audience of these implications. One problem with this strategy is that the implications one comes up with may hold only in certain contexts or with certain kinds

of audiences, when no such context dependency is to be found in the analysandum.

Imagine J. B. Rosser announcing to some of his colleagues that Quine's system of axioms of the first edition of *Mathematical Logic* is inconsistent.[46] Is this an announcement that shows that he is confident that his audience can see for themselves that Quine should retract or qualify those axioms? Surely not. It is questionable that Rosser would have expected even someone who had worked extensively with that set theory to have been able to see for himself or herself that Quine should retract or qualify his axioms.

Resnik admits that some statements in which modal terms occur may have truth values: these are statements in which modal terms occur in non-categorical contexts. Here's an example of such a non-categorical statement:

> [@] Any theory implying a falsehood is false.

Resnik suggests that such statements should be understood "as tacitly referring to the norms that govern (or ought to govern) our inferential practices" (Resnik, 1997: 169). Thus, [@] is rendered by Resnik as:

> [@′] Any theory from which we may infer a falsehood is false.

In this context, implication is analyzed by Resnik in terms of what we may infer.

I find this analysis, too, to be highly questionable. [@] and [@′] clearly do not mean the same things, as can be seen from the fact that [@], as ordinarily understood by anyone with even a bit of logical training, does not presuppose or make tacit reference to a system of inference rules, whereas (as Resnik himself suggests) [@′] does. Furthermore, consider how one might justify one's belief in [@]. From the standard intuitive explication of implication, one can prove (trivially) that [@] holds, whereas a proof of [@′], depending upon the particular system of inference rules one is tacitly referring to, will very probably be relatively substantial. I do not see how [@′] can be a plausible rendering of [@]. In any case, Resnik admits that he has no systematic method for dealing with such non-categorical examples and that they pose a "serious, but not fatal, difficulty" for his position (Resnik, 1997: 169–70).

Returning to Resnik's non-cognitivist position on categorical modal statements, let us examine some specific examples of modal statements that mathematicians have made. Here is how Fermat stated what became known

[46] See Quine, 1963: 302 for references and more details on what Rosser proved.

as his "Last Theorem":

It is impossible to divide a cube into two cubes, a fourth power into two fourth powers, and in general any power except the square into two powers with the same exponent.[47]

Evidently, Fermat did not accept Resnik's doctrine that there are no modal facts.

Now Resnik could respond to this objection by claiming that Fermat's statement of his "theorem" just means "There is no solution to . . ."—a straightforward existential statement which poses no difficulty for his views.

I see substantial problems with such a reply. First, this response conflicts with Resnik's non-cognitivism. Thus, notice that Fermat's statement seems to categorically attribute an impossibility and hence that, according to Resnik's non-cognitivism, it actually performs other functions, and is neither true nor false. But surely, under the suggested synonymy, Fermat's statement is true.

Second, the reply cannot be that the modal statement of Fermat's is *necessarily equivalent* (or even "logically equivalent") to the existential statement, since first, that very statement of equivalence is a modal statement, and second, the equivalence implies that Fermat's modal statement is true (which contradicts his thesis that no categorical modal statements are true). No, the reply must be that what appears to be a modal statement is actually an existential statement—that Fermat actually made not a modal statement but rather a straightforward negative existential statement.

Such a position seems to me to be wildly implausible. Suppose that Fermat had said: "It is impossible for even God to divide a cube into two cubes, a fourth power into two fourth powers, and in general any power except the square into two powers with the same exponent." What would that modal statement mean, according to the supposed Resnik position? Or: "Not even Descartes could divide a cube into two cubes, a fourth power into two fourth powers, and in general any power except the square into two powers with the same exponent." Fermat would have been willing to make both assertions. But are all these apparently distinct modal statements one and the same negative existential statement? That would be linguistically very implausible. What these examples highlight is the fact that the above-suggested response to my objection rests upon a substantial (and counterintuitive) linguistic thesis. If Resnik were to make the suggested response, the burden of proof would be on him to provide convincing linguistic evidence supporting the

[47] Dorrie, 1965: 96.

underlying linguistic thesis. I doubt very much that he could come up with what is required.

Continuing the theme of whether mathematicians make modal claims, most mathematicians would affirm that Gauss was able to prove a special case of Fermat's "theorem", namely that it is impossible to divide a cube into two cubes.[48] Of course, such an affirmation strongly suggests that Gauss proved that the statement 'It is impossible to divide a cube into two cubes' is true—something that conflicts with Resnik's anti-modalist position.

I noted in Chapter 1 that Euclid's geometry was modal in character. It is not surprising, then, that many of the solutions to ancient geometric problems that were given in the nineteenth and twentieth centuries are stated modally. For example, the theorem that answered the ancient problem of whether it is possible to square the circle is stated:

It is impossible to draw with a compass and straightedge a square that is equal in area to a given circle.[49]

Resnik's position on categorical modal statements implies that this statement has no truth value. But how can that be if it has been proved? How can this theorem have no truth value if it has survived hundreds of years of attempts to find a compass and straightedge method of squaring the circle? Do we not have some evidence of the truth of the statement?

Consider the following problem posed in 1891 by Edouard Lucas:

How many ways can n married couples be seated about a round table in such a manner that there is always one man between two women and none of the men is ever next to his own wife?[50]

This modal problem was solved by several mathematicians, who gave an effective procedure for obtaining the number. Notice that the solution can be tested for relatively small n by actually making arrangements of possible seating charts. If the tests for $n = 6, 7, 8, \ldots, 100$ are all found to correspond to the answers provided by the solution, won't this result provide some evidence that the solution is correct?

Resnik thinks that such statements as 'It is possible to construct an open-sentence token that is true of my gold pen, my left thumb, and the moon' lack truth values. But I have actually constructed an open-sentence token that is

[48] Ibid.

[49] The theorem followed from the proof given in the nineteenth century of the transcendence of π. See Dorrie, 1965: 136.

[50] Dorrie, 1965: 27. A detailed discussion of this problem is given there.

true of my gold pen, my left thumb, and the moon, and that shows, according to standard modal logic, that the modal statement in question is true. I am much more confident of the truth of the modal statements I have listed here than I am of the many paradoxical and bizarre consequences of Resnik's principle of the nonsensicality of trans-structural identity (thesis [2]).

A view opposing Resnik's skeptical doubts may be appropriate here. Putnam vividly expresses such an opposition:

> From classical mechanics through quantum mechanics and general relativity theory, what the physicist does is to provide mathematical devices for representing all the *possible*—not just the physically possible, but the mathematically possible—configurations of a system. Many of the physicist's methods (variational methods, Lagrangian formulations of physics) depend on describing the actual path of a system as that path of all *possible* ones for which a certain quantity is a minimum or maximum. . . . It seems to us that 'possible' has long been a theoretical notion of full legitimacy in the most successful branches of science. . . . It seems to us that those philosophers who object to the notion of possibility may, in some cases at least, simply be ill-acquainted with physical theory, and not appreciate the extent to which an apparatus has been developed for *describing* 'possible worlds'. (Putnam, 1979: 71)

How do we know the axioms of the Constructibility Theory?

Let us now consider Resnik's contention that we lack the epistemological means to know complicated modal facts of the sort required by the Constructibility Theory. Resnik's contention is based upon his enumeration of the means we have for "determining logical possibilities": (1) modal logic; (2) inferences from what is actually the case to what is possibly the case; and (3) logical intuition (Resnik, 1997: 64). The first two, he tells us, are "probably too weak to provide all the knowledge" that is required (Resnik, 1997: 64). As for the third, he has little confidence in logical intuitions, "since often even the intuitions of professional logicians conflict" (Resnik, 1981: 64).

Before tackling this objection head on, a few preliminary points may be useful:

[1] Why should the fact that the intuitions of professional logicians sometimes conflict lead one to lose confidence in logical intuitions? True, there are some rather spectacular cases of disagreements among logicians about such basic logical principles as the law of excluded middle. But it is worth noting that these disagreements concern only the application of the "law" to infinite totalities—there is no disagreement when dealing with finite totalities.[51] Furthermore, the disagreement is frequently based not upon a conflict of "logical intuitions", but rather on highly theoretical reasoning, perhaps involving theories of how we learn the logical connectives.[52] Besides, the cases of agreement among the logical intuitions of professional logicians far outweigh the cases of disagreement. After all, we don't lose confidence in our intuitions about the grammaticality of strings of English words just because fluent speakers of English sometimes disagree about whether a particular string of English words is or is not grammatical. It is striking that even the intuitionists do not advocate abandoning all our logical intuitions.

[2] I am uncomfortable applying the word 'know' to my beliefs about the axioms of the Constructibility Theory, since it is not clear to me just what is required in order to know such things. I see no compelling reason to claim to know that these axioms are true. It is sufficient that we have plausible grounds for believing the axioms. In this respect, my epistemological position vis-à-vis the axioms of the Constructibility Theory is surely no worse than that of the Platonic set theorists vis-à-vis the axioms of set theory: even those Platonists who rely upon indispensability arguments to support their ontological beliefs do not claim to know that the axioms of ZF are true.

As for Resnik's threefold classification of our means for determining possibility, he simply omits what is perhaps the most fruitful means: theoretical reasoning. Just as one can theorize about physical laws and principles, about logical laws and principles, and about grammatical laws and principles, one can also theorize about modal laws and principles. One can construct theories about possibility and test these theories, for example, to see if our logical, linguistic, and scientific theories come out as expressing possibilities or to see if the theory conflicts with any theories determined to be possible by the methods of (1), (2), and (3) described by Resnik. One can also theorize about what it would be possible for intelligent beings to do, given that these beings have such and such capacities and such and such a language, by extrapolating from what we humans are able to do. Much of the justifications of the axioms of the Constructibility Theory are of this theoretical sort. I see no reason why such reasoning should be considered to be inappropriate for the aims I have in mind.

[51] See, for example, Brouwer, 1967: 336, which is quite explicit about this.

[52] Recall the argument in favor of rejecting excluded middle that Dummett once gave (which was discussed in Chapter 3).