THE CONTINUUM HYPOTHESIS: INDEPENDENCE AND TRUTH-VALUE

BY

THOMAS S. WESTON

S.B., Massachusetts Institute of Technology

1966

SUBMITTED IN PARTIAL FULFILLMENT

OF THE REQUIREMENTS FOR THE

DEGREE OF DOCTOR OF

PHILOSOPHY

at the

MASSACHUSETTS INSTITUTE OF

TECHNOLOGY

June, 1974

Signature of Author ______________________
Department of Philosophy,
March 27, 1974

Certified by ______________________
Thesis Supervisor

Accepted by ______________________
Chairman, Departmental Committee
on Graduate Students

ABSTRACT

THE CONTINUUM HYPOTHESIS: INDEPENDENCE AND TRUTH-VALUE Submitted to the Department of Philosophy, M.I.T., in partial fulfillment of the requirements for the degree of Doctor of Philosophy.

In the Introduction, Cantor's continuum hypothesis (CH) is stated, and the history of attempts to prove it is reviewed. The major problems of the thesis are stated: Does the CH have a truth-value, and if so, how could we discover what it is, given the proofs of its independence from the most widely accepts set theories? By way of a prima facie case that CH has no truth-value, several versions of formalist accounts of truth in mathematics are described.

Chapter I examines the claim that the independence proofs for CH are only possible because of inadequacies in first-order formalized set theory. Various strategies for repairing or generalizing formal systems are examined and it is shown that no so far proposed strategy based on the usual axioms of set theory excapes independence proofs. The latter part of the chapter discusses G. Kreisel's claim that the CH is "decided" in second - order formulations of Zermelo-Fraenkel set theory. It is argued that (a) second-order logic is of no heuristic value in discovering a proof or refutation of CH, and that (b) Kreisel's semantic arguments that CH is "decided" are sound only if a much simpler argument that does not used second-order logic is also sound. This simpler argument is investigated in the second chapter.

In Chapter II, the question of the truth-value of CH is examined from the point of view of various sorts of realism. A notion of "minimal realism" is formulated which forms the basis of subsequent discussion. It is argued that this form of realism is plausible, but that it cannot be sufficiently clarified at present to exclude the possibility that there may be alternative equally natural interpretations of set theory, some according to which the CH is true, and others according to which it is false. Various historical cases are examined to show that the possibility of multiple natural interpretations has been a major difficulty in other problems in mathematics. The model construction techniques of Godel and Cohen and their generalizations are examined to see if they provide equally plausible interpretations of set theory, and it is concluded that they do not. In the final section, it is argued that although we cannot conclusively rule out multiple interpretations of set theory, it is very implausible that the portion of "the universe" of sets which concerns the CH has multiple interpretations, and that the fact of independence results does not increase the plausibility of this view.

In Appendix A, various types of plausibility arguments in mathematics are sketched, and the controversies over the plausibility of the CH and the axiom of choice are reviewed and compared. It is concluded that the axiom of choice should be regarded as well established, but no existing arguments show that CH or its negation are plausible.

Three technical appendices prove results used in the text and summarize axiomatic theories discussed there.

The author gratefully acknowledges the patient help of his committee, Professors Richard Cartwright and George Boolos. Invaluable advice and moral support were also provided by Professors Richard Boyd and Hillary Putnam.

Thesis Committee: Professor Richard Cartwright, Chairman
Professor George Boolos

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INTRODUCTION

What is the Continuum Hypothesis ?

(CH) "Every infinite set of real numbers can be put into one-to-one correspondence either with the set of natural numbers or with the set of all real numbers."

This is, in essence, the continuum hypothesis, first proposed by Georg Cantor in 1878.[1] In the presence of the axiom of choice, which will be assumed throughout most of this thesis, CH is equivalent to the Aleph Hypothesis: (AH) $2^{\aleph_0} = \aleph_1$

That is, the cardinal of the continuum is the first uncountable cardinal.

Cantor himself accepted the AC, or rather its equivalent, the statement that every set can be well-ordered,[2] so this distinction was unnecessary for him. In the sequel, we shall distinguish CH from AH only in Appendix A, where the status of AC is discussed.

Since we will be discussing the CH in the context of set theory, we will always mean by 'the continuum' the set of all sets of finite ordinals, $\mathbb{P}(\omega)$, to which the real numbers are isomorphic in a natural and familiar way.

The History of the Continuum Question

At the time when he proposed CH, Cantor promised that "an exact solution to this question" closed by his "theorem" would come later.[3] There followed a long series of unsuccessful attempts to deliver on this promise, but none of his attempts at proof succeeded.[4]

In 1904 and again in 1905, Konig claimed to have shown that CH is false, but these arguments were faulty and were withdrawn.[5]

In 1900, Hilbert had listed the continuum problem first on his famous list of outstanding unsolved mathematical problems.[6] In 1925, he announced a proof of CH which also proved faulty. The only correct partial result from this collection of faulty proofs in Konig's "lemma" that $2^{\aleph_0}$ is not the limit of countably many smaller cardinals.[8]

In 1938, Kurt Godel was able to prove that if Zermelo-Frankel set theory (ZF)[9] is consistent, then it remains so on addition of AC and CH. Godel accomplished this by showing that the sets satisfying the apparently restrictive condition of constructibility[10] form an interpretation satisfying ZF and AC and CH.[11] The relative consistency of $\sim$CH with the axioms of ZF cannot be shown by an analogous method,[12] however, and this result was only obtained 25 years later by Paul Cohen.[13] His completion of the proof that CH is independent of the axioms of ZF, even if the axiom of choice is assumed, confirmed a conjecture of Skolem made in 1922.[14] Both Godel's and Cohen's results extend to the generalized continuum hypothesis (GCH) that $2^{\aleph_\alpha} = \aleph_{\alpha+1}$ for all α[15]. CH has since proved to be independent of ZF even when any of "large cardinal" axioms are added to it.[16]

Present Status

It is evident that CH is a "simple" question, in the sense that it can be simply stated in familiar terms. It represents a proposed solution to a natural problem, that of the position of the cardinal of the continuum in the series of $\aleph$'s, the infinite cardinal numbers. It has proved sufficiently interesting to attract some of the most talented mathematicians of this and the last generation. In addition to its independence of ZF and most other set theories and unlike the axiom of choice, which is also independent of ZF, there are no compelling plausibility considerations which favor either CH or its negation.[17] This suggests that its solution, if indeed this will be possible, will require quite new principles of a type which can not now be envisioned.

This unsettling situation has provoked a variety of alarmed responses among set theorists and philosophers of mathematics. It is asserted by some that CH lacks a truth value at all, or that the independence results have instituted a state of crisis in set theory comparable to that in geometry provoked by the proof of independence of the fifth postulate, which may result in set theory dividing into mutually exclusive types, some accepting CH and some its negation.

None of these assertions is accepting by the present writer, but they deserve answers, and giving those answers is the subject of this thesis. Specifically, we hope to answer the following questions:

(1) Do the independence results give any reason to believe that CH is neither true nor false?

(2) If so, are these reasons compelling?

(3) If not, is it reasonable to expect that CH or its negation will actually be shown to be true?

In the sequel, we will say that a sentence is determined if it has a truth-value, and that it is decided (w.r.t. some contextually-definite theory) if it is either provable or refutable (in the theory).

CH is not determined: A prima facie case.

By way of motivation for the study of questions (1) through (3), it is useful to state a prima facie case that CH is not determined, that is, arguments which pretend to establish this claim, but which may be either faulty or answerable.

Since some of the queasy feeling aroused by the independence proofs is undoubtedly due to sympathy with formalist views, discussion will here be confined to that general kind of view. Later, in Chapter II, we will agree to reject formalism and give a prima facie case from a realist position.

We consider two variations of formalist doctrine. According to the first, we can construe formalism as a stingy theory of truth, or at least of having a truth value. That is, a sentence concerning some area of mathematics will be determined if and only if it is decided in the formal system which is most preferable on grounds of utility or elegance and which encompasses the given area of mathematics.[18]

Since ZF or a ZF-based theory would undoubtedly be the most preferable theory in this sense and since no presently available or foreseeable considerations lead us to prefer such a theory in which CH is decided, CH is not determined.

The second version of formalism differs from the first in refusing to speak of truth in mathematics at all, but only of the most preferable or acceptable theory. This view accords better with the statements of most of those who have titled themselves formalists, particularly with such characteristic statements as: "any mention or purported mention of infinite totalities is, literally, meaningless."[19] It is, of course, difficult to see how meaningless sentences can be determined. It may still be possible to select from among these meaningless sentences some which have truth-like properties, such as being incorporable into a preferred formal theory, but this seems to be excluded for CH and its negation just as on the previous view.

On either view of formalism, then, CH is not determined, and on the latter, not even "determined" as to acceptability for incorporation into the preferred theory. This is so provided that it is really the case that no formulation of set theory like those presently entertained decides CH. It has, however, been doubted whether every reasonable formulation does fail to decide the CH. Clearly refuting or establishing this is the first order of business before more subtle considerations are taken up. This is the subject of the first chapter. There I hope to establish that there is no escape from independence results via reformulations based on presently accepted axioms.

In the second chapter, I explore the three questions (1) through (3) from a realist point of view. Plausibility arguments for CH and AC are taken up in Appendix A, and Appendix B cummarizes the formal theories discussed in the text.

Chapter I

Introduction

Godel and Cohen stated their independence results for ZF, but their methods extend to virtually every formal set theory so far developed. (We shall have more to say about this infra). Thus, in the logician's sense of 'theory', that is, a set of sentences of a given--usually artificial--language which is closed under specified inference rules, the CH is independent of virtually all set theories. If it happens however, that some existing theory is not subject to the independence methods, or even if it seems likely that such a theory may yet be developed, even the formalist may hope for a solution to the continuum problem.

If some system in which CH is decided were to be developed, and we were able to find good reasons to believe this theory true (formalists may substitute 'good reasons to accept it'), then this would surely count as solving CH. But the development of such a system seems not only sufficient but necessary--for, the continuum problem cannot be counted solved unless there is a clear and disciplined presentation of a proof from principles whose truth is at least probable. It has been the case up to now that any single correct mathematical proof can be incorporated--by a natural translation--into some formalized system, and it is extremely difficult to see how an argument which was incapable of such incorporation could be counted as reliable. Thus the question of whether an actual solution of the continuum question is presently foreseeable reduces to the question of whether we have or are close to having a formal system of set theory (satisfying the stated conditions) which decides CH.

In this chapter, we will consider two kinds of claims:

(1) There actually exist acceptable formal theories which decide CH, and

(2) Although we do not now have the kind of theory in (1), there are sufficient resources in present mathematical practice beyond its formal theories that is reasonable to hope for a solution to the continuum question on the basis of what we already know--even if we do not know that we know it.

It is the contention of this writer that both these claims are false. A modified version of (2), which holds that we have reason, based on the current state of mathematical practice, to believe that CH is <u>determined</u> will be defended in Chapter II. Claim (1) is, in effect, made by Professor Kreisel and perhaps others. These views will be discussed later in this chapter.

For the present, we will concern ourselves with (2). An advocate of (2) must maintain that ZF and the other formal theories to which Godel-Cohen independence techniques for CH apply do not fully represent "set theory", where 'theory' is taken not in the logician's sense, but as an human institution with its accompanying beliefs, practices and techniques. If we are correct in maintaining that a condition on clarity of a proof of CH or its negation is the possibility of incorporation into an acceptable formal system, then the advocate of (2) must maintain that some formal system can more adequately represent at least a portion of set theory which deals with cardinals. Mostowski, for example, has held that

> ... the identification of the Godel-Bernays system with the intuitive set theory is not justified. This at any rate is my point of view. We need new axioms to codify the intuitive set theory. The disquieting fact is that we do not know where to look for them.[1]

Adherents of most philosophical positions about set theory would agree with Mostowski that new axioms are to be sought, but what makes his position a version of (2) is his claim that these new axioms are already present (implicitly?) in "intuitive set theory". It is not an easy matter to make a case that such axioms exist which decide CH if "intuitive set theory" is identified with the present institution of set theory, since axioms so far proposed, e. g., "large cardinals", do not decided CH when added to ZF.

We will, however, be concerned first with a more radical version of (2) and its accompanying "diagnosis" of the independence results. This version, held by Paul Bernays and others, maintains that presently accepted axioms decide CH if <u>properly understood,</u> but that these principles are incompletely captured in formal systems of set theory. For example, Bernays claims that

> ... the results of Paul J. Cohen on the independence of the continuum hypothesis do not directly concern set theory itself, but rather the axiomatization of set theory; and not even Zermelo's original axiomatization, but a sharper axiomatization which allows of strict formalization.... in model theory one might argue that the original question has not been answered, as Cohen's proof applies only to a <u>formalized</u> axiomatization of set theory.[2]

Bernays also states that the inadequacies which appear in formal systems of set theory are quite general and apply to other theories:

> From our experiences with non-standard models, it appears that a mathematical theory like number theory cannot be fully represented by a formal system: and this is the case not only with regard to derivability, as Godel's incompleteness theorem has shown, but already with regard to the means of expression. For a full representation, we need an open succession of formal systems.[3]

These views, and some related ones of other authors,[4] will be discussed in the next few sections; we give them the descriptive title 'informalist'. In what follows, we will consider informalists to fault formalized theories for failing to represent adequately mathematical theories, and to maintain that this accounts for the independence results. We do _not_ want to include in the characterization of informalism the related notion that formal theories cannot adequately represent some mathematical _structure_ such as the universe of sets or the series of natural numbers. This position will be alluded to in what follows, but will receive a fuller discussion in Chapter II.

Our characterization of informalism is still incomplete on one crucial point: if formal theories do not adequately represent set theory, does any thing else do better? Bernays suggests, in a passage quoted above, that Zermelo's "original axiomatization" is better, and other authors are more explicit:

> ... Zermelo's system, despite its nonformal character, comes far closer to the true Cantorian set theory.[4]

This suggests that the notion that some theories expressed in a natural language--in Zermelo's case, German--are better than formal systems of set theory. This interpretation is born out by an examination of Bernays list of specific defects of formal systems, which have already quoted. He claims that (a) the "means of expression" of formal systems are inadequate; that (b) "derivability" relations are not faithfully represented in formal systems; and that (c) the defects in (a) and (b) can be remedied by "an open succession of formal systems". The context suggests that remedy (c) is only intended to apply to arithmetic. For ZF, he suggests that if "strictly formal methods ... are transgressed" by restating axioms with an "unrestricted concept of predicate", then the means of expression of the resulting theory are no longer inadequate.[5] We will take up this specific suggestion in due course, but it is clear that Bernays regards this as a case where natural language description of principles of set theory is superior to "strictly" formalized ones.

Bernays' ground for (a) is the existence of non-standard models for formal theories. He argues for (b) on grounds of Godel's Incompleteness theorem. As a preliminary to discussion of specific suggestions of informalists for improvements on ZF, we will examine Bernay's claims (a) - (c). We will accept (b) with reservations and, in part, (c), while demurring to (a).

Fixing Up Formal Theories:

Up to now, we have used the term 'formal system' without explanation, meaning by it first-order systems of the usual sort and perhaps others. It is useful at this point, however, to be a little more precise. Until further notice, we will take a formal system to be have a specified syntax, rules of inference and axioms, and to satisfy the condition that we can effectively tell whether a given formula is well-formed and whether a given sequence of formulas is a proof of the system. In practice, we shall always substitute 'recursively' for 'effectively' ; that is, we assume Church's thesis.

If this notion of formal system is adopted, it follows from well-known results of Godel, as strengthened by Rosser, that any formal system which is consistent and sufficiently strong to express and prove the usual properties of "plus" and "times" in arithmetic has an undecided sentence.[5a] For the case of first-order arithmetic, this sentence can be taken to be of the form (x) Fx where 'x' is a number variable and 'F' is a recursive--<u>i.e.</u>, a relatively "simple"--predicate. Furthermore, Godel's proof clearly shows that, if arithmetic is consistent, this sentence is <u>true</u>--that is, it is <u>true of</u> the natural numbers. Since we can prove, in a way which is satisfactory according to usual mathematical practice, that arithmetic is consistent, we can prove, in mathematical practice, that the Godel sentence of any of the usual first-order formulations of arithmetic is true, although it cannot be proved in that system. Presumably, this is Bernay's point (b), that "derivability" relations in arithmetic are not adequately represented in formalized arithmetic. Doubtful points remain, however, for the mathematical practice proofs alluded to, for example Gentzen's[6], are conducted in a vocabulary richer than that of the usual first-order systems of arithmetic. It is less than evident that Godel's results could form the basis of an argument that proof resources in mathematical practice which use the same vocabulary as first-order formal arithmetic are more potent than the resources of the formal theory, especially since the only known proofs of some arithmetic sentences use notions from complex-variable theory. On the other hand, if we consider formal theories with a richer vocabulary, we will still be able to obtain Godel's result, but it is less than obvious that we will be able to prove the consistency of this theory in mathematical practice.

The salvagable point here is that we do know that "sufficiently strong" formal theories are incomplete, but we do not know that any true mathematical sentences are unprovable by all mathematical practice methods. If we did know that this was the case at present, we would surely attempt to develop new methods to remedy this defect. So formal theories are faulty in a way which we hope mathematical methods generally are not, and which we would attempt to remedy if it should turn out that way.

It is plain that there is a superficial analogy between the Godel-Rosser incompleteness results for arithmetic and stronger systems, and the Godel-Cohen independence theorms for formal set theories, in that both are incompleteness results. Evidently Bernays feels that the analogy is more than superficial, and Cohen hints at this as well.[7] Since it is possible to remedy the incompleteness of formal arithmetic by introducing what Bernays calls "an open succession of formal systems", it is worth while to examine briefly how this works and to see if the same or similar devices may remedy the "defect" of the independence results for formal set theory. If this were possible, it would be a strong point in favor of the informalist position, but in fact the prospects for a "remedy" along this line are poor. For definiteness, we will concern ourselves in the discussion which follows with the system of first-order arithmetic S described in Mendelson's book.[8]

Two Devices for "Completing" Arithmetic

The first device to be discussed in the so-called "ω-rule". By way of motivation, we look more closely at the undecided sentence of S given by Godel's proof. Godel's basic device is to assign every formula and finite sequence of formulas a code number in a systematic way, so that the various metamathematical notions such as "is a well-formed formula", "is an axiom", "is a proof of", etc, can be represented by numerical predicates and relations. For example, let Prf(x, y) be the number-theoretic relation which holds when x is the code number of a proof of the formula with code number y. Let m be a code number for $(x)\text{-Prf}(x,y)$. Then Godel's result is that $(x)\text{-Prf}(x,\overline{m})$, a formula which says "I am not provable", is true, hence unprovable in S, provided S is consistent. It happens, however, that each of the instances of this formula, $\text{-Prf}(\overline{0},\overline{m})$, $\text{-Prf}(\overline{1},\overline{m})$, ... are theorms of the system. This suggests that if we add a new rule of inference to S which would allow us to infer $(x)F(x)$ from $F(\overline{0})$, $F(\overline{1})$, $F(\overline{2})$, ..., then we could prove the recalcitrant sentence, and hopefully render our system complete. This does happen, in fact, and our new rule is called the ω-rule.[9]

Our reason for considering such devices in the first place, however, was the alledged inadequate representation of a mathematical theory by its formal counterpart. On this criterion, the ω-rule fails for reasons other than incompleteness of the resulting quasi-formal system, for it requires that some proofs be infinitely long, and this is surely a non-trivial idealization of the actual activities of mathematicians. There is, however, another device for completeing formal arithmetic which obviates this particular difficulty.

This new device relies on the fact that provability in S can be represented by Prf (x, y). It is possible to make use of this relation to prove that every instance of some single free-valiable formula F, $F(\overline{0})$, $F(\overline{1})$, etc., is provable in S, and then, by analogy with the ω-rule, permit the inference to $(x)F(x)$. That is, one adds the rule that:

(*) From $(x)(\exists y)\,\mathrm{Prf}\,(y, I(\overline{f}, x)$, infer $(x)F(x)$

where f is the code number of F, and $I(f,x)$ represents a function whose value is the code number of the x<u>th</u> numerical instance of the formula whose code number is f.

If (*) is added to S, the Godel sentence $(x)\text{-}\mathrm{Prf}\,(x, \overline{m})$ becomes provable. However, the resulting system has another undecided sentence which can be decided in turn by an analogue (**) of (*) with the predicate Prf* which describes provability in the system with rule (*). This system is, in turn, incomplete, and so on. In this way, one can generate an "open succession of formal systems" with the property that every true sentence and no false one can eventually be proved in a system of the sequence.[10] The theorems of the sequence thus correspond to the so-called "true arithmetic", the set of sentences of S true in the standard interpretation. These same results may be obtained if instead of the rule (*) and its counterparts, a schema of axioms:

$$(*') \quad (x)(\exists y)\,\mathrm{Prf}\,(y, I(\bar{f}, x)) \supset (x)\,F(x)$$

is added to S and similarly for succeeding stages. Since (*') "says" that a sentence $(x)\,F(x)$ of number theory holds if all its numerical instances are proved in S, it is evidently true (i.e. all instances are true) if S is consistent, and similarly for succeeding stages.

Difficulties still remain, however, about representing number-theoretic proofs in this fashion. In order to formulated the Prf-like predicates far out in the sequence of theories, one must be able to represent certain characteristics of the progression of theories either by the apparatus of ordinal numbers, or by some system of notations for ordinals, such as Kleene's system O. But either choice requires information either not expressible in S (ordinals) or not provable in S (facts about notations in O).[11] So provability in formal number theory cannot be fixed up by this device to correspond to truth in number theory without using more powerful theory for which a "correspondence problem" arises all over again. As before, we are entitled to make use of this fact to show a lack of correspondence between provability in formal theories and mathematical practice only by making the attractive but incompletely warranted assumption that all true sentences of arithmetic are provable in mathematical practice.

Despite the stated defects of the second sort of remedy for incompleteness of S, however, it does yield completeness, and it is evident that the added axioms are true (or the added rules sound), so this gives some justification for Bernays' claim (c) that derivability defects of formal systems are not present in an "open succession" of them. But unfortunately, these devices do not decide CH when applied to formal set theory. This application can be accomplished by translating sentences of arithmetic into set theory in the usual way and then adding either an ω-rule or "succession" rules applied to these arithmetic translations.[12] The sad fact is that, on the reasonable hypothesis that there are well-founded models for, say, ZF, there are models for ZF in which the arithmetic portion is the same but which give different truth-values to CH. Hence neither CH nor its negation can be a consequence of any of the newly provable arithmetic sentences.

<u>Other ways to "fix" ZF</u>:

Even though the devices which complete arithmetic fail for ZF, it is worth asking whether some analogous devices may work. We cannot, of course, cover all "analogous devices", but some partial negative results are available.

The pecular feature of arithmetic which makes a sound ω-rule possible is the existence, for each element of the intended model, of a term--a numeral--which denotes it. Hence we can say that if F (t) holds for every term in a certain class, then (x) F (x). Since no formal system which satisfies our effectivity requirements can have more than countably many terms, and there are uncountably many sets, we cannot construct a sound analogue to the ω-rule for ZF.[12a]

If there are any well-founded models for ZF, however, there is a model for ZF which satisfies the following requirement: There is a sequence of formulas with one free variable $\{A_n(x)\}_{n\in\omega}$ such that every element of the model satisfies exactly one of the $A_n(x)$. Using the definite description operator (ix), we can construct the required class of terms $T = \{ (ix)A_n(x) \}_{n\in\omega}$. It may be that an "$\omega$-rule" based on this class of terms would decide CH, since it is in fact true in the model, but this would tie us to a model which is certainly unnatural, since it is countable.

A more promising idea is to attempt to construct a succession of systems based on ZF using notions more powerful than arithmetic ones. Takeuti[13] has had partial success in such a project. The technical details for this system are formidable, so we confine ourselves to a somewhat impressionistic description of the main result. Takeuti begins with ZF plus the so-called axiom of constructibility (treated in the next chapter). He then constructs a transfinite hierarchy of "types" with the sets as the lowest type. In turn, this type theory serves as the basis for a sequence of theories, where each theory is obtained from its predecessor by the addition certain new axioms. These axioms make use of formula '$\mathrm{Prov}_T(x)$' which say that the formula with code number x is provable in the system T. If T is a theory in the sequence, the succeeding theory T' is obtained by adding $\mathrm{Prov}_T(\bar{m})$ for each code number m of a formula provable in T and $-\mathrm{Prov}_T(\bar{m})$ for each m which codes a formula unprovable in T.

Unlike the succession of theories based on arithmetic, this sequence begins with a theory including enough resources to prove all the required information about the succession characteristics of the sequence of theories. Takeuti is able to obtain a completeness result for an important class of sentences of ZF, viz., the class of first-order formulas of ZF with quantifiers relativized to constructible sets. Takeuti shows that a formula in this class is provable if and only if it true. He also seems hopeful that this result may be extended to all first-order sentences of ZF, but it is evident that this cannot be done by any straightforward method, since Takeuti's argument makes strong use of features of the constructible sets very probably not shared by the entire universe of sets, such as the existence of a definable well-ordering of all sets. Takeuti's partial result is of no help on the CH, however, since it was already known that CH is true when relativized to the constructible sets.

Our conclusion must be that the devices which "repair" the incompleteness of arithmetic, and their analogues, fail to perform the same service for ZF, at least as far as is presently known or seems likely to be. If we are to continue an informalist program of seeking liberalizations of formal systems which will not suffer from the defect of incompleteness, we must follow out another suggestion of Bernays', involving the "unrestricted notion of predicate" in set theory. Before proceeding to this however, we still must deal with Bernays' claim (a) that formal theories are deficient in their "means of expression" because they must allow of non-standard interpretations.

Non-Standard Models and Expressibility

It is the conventional wisdom in mathematical ontology that there are few if any contexts in which it makes any mathematical difference which of a class of isomorphic structures we take to be the subject matter of a mathematical theory. In accordance with this usual line of thinking, we shall speak somewhat inaccurately and mean by 'standard model of theory T' either some one isomorph of the class or the class itself, depending on the context.

Incomplete theories are guaranteed to have models other than the standard one(s), because there must be non-isomorphic models of each of its incompatible completions. When we are dealing with complete quasi-theories of the sort described in previous sections, whose theorems constitute, say, true arithmetic, we must use other methods to show the existence of non-standard models. Standard theorems of model theory, such as the so-called Upward Löwenheim-Skolem Theorem, guarantee that true arithmetic has uncountable, hence non-standard, models. It turns out, however, that it also has countable ones.[14] If as Bernays seems to maintain in the passage quoted above, the existence of non-standard models of sentences of formal languages shows that they fail to express adequately the sense of their natural language counterparts, then the "open succession" of systems would fail on the same grounds as the incomplete theory S. So if Bernays claim (a) that the "means of expression" of S are inadequate were supported by the existence of non-standard models, the "open succession of systems" would fare no better. This writer, however, sees no reason to grant this.

No doubt any set of sentence of an artificial or natural language which are true under some standard interpretation can be reinterpreted so that they are also true according to completely unintended ones as well. It is possible to lay it down that 'Quine is a philosopher' is to be interpreted in such a way that 'Quine' refers to Nixon and 'is a philosopher' has the usual sense of 'is a politician'. But the fact that such a reinterpretation is possible does not show that 'Quine is a philosopher' does not say that Quine is a philosopher. It merely shows that these words might have been understood in a way in which they are not. The simple structure of the usual first-order languages and the existence of a model theory for them makes the existence of a large variety of possible interpretations which preserve grammatical categories of "predicate", "individual constant", etc., stick out like a sore thumb. But there is no philosophically significant reason and likely no reason at all why such a model theory should not eventually be available for natural languages as well.

Logicians sometimes study theories with no intended interpretation, but it is far more common to study a theory because of the interest of its intended interpretation. For theories of this latter type, the intended interpretation of the constants, function symbols, etc., is invariably explained in the same way as the natural language technical terms of any part of mathematics or natural science, by explaining the intended sense or "pointing to" the intended reference in some natural language the speaker and his audience both understand. If these explanations and supplimentary examples and audience guesses are inadequate for determination of meaning or reference in one case, they are also defective in the other.

Such explanations may, in fact, be vague or otherwise incomplete, and further investigation may show that several incompatible refinements of them lead to different interpretations, none obviously more correct than the others. (We give some examples of this in Chapter II). But for at least one of the formal theories which Bernays alledges to be defective in the "means of expression", that is, S, possible ambiguities in the standard interpretations of '0', '+', 'x', etc., have not been detected and we have not the slightest reason to believe that any will come to light.

This writer believes that the existence of "the" standard interpretation of the '$\in$' of set theory is a little less clear cut, a matter which we look into further in Chapter II. It is clear, however that the existence of undisputedly unintended interpretations of ZF goes no farther in showing that the "means of expression" of that theory are inadequate than in the case of S.

In summary, we have found that one of the alledged defects of formal languages--the "means of expression"--is non-existent. The other defect, incompleteness, is real enough, but the evidence is not conclusive that mathematical practice fares any better. The devices which remedy this defect for arithmetic fail to do so for ZF, so our diagnosis of incompleteness must be somewhat different for the two cases. We have yet to follow out Bernays' suggestion that enriching formal set theory to permit an "unrestricted" concept of predicate would decide CH. This topic, examined from several sides, will concern us for the rest of this chapter.

"Unrestricted Concept of Predicate": Background

Historically, the notion of predicate enters set theory with the comprehension principle, that the extension of every predicate is a set. This principle was proved inconsistent by Russell, but all modern set theories preserve it in some less general form.

In Zermelo's set theory of 1908[15], the principal ancestor of ZF and the cluster of related theories with which we will be almost exclusively concerned, the comprehension principle is replaced by a short list of its instances--the powerset, infinity, pairing, nullset, union, and choise axioms--and by a modified comprehension principle called the axiom of separation [Aussonderungsaxiom]. In its earliest version, this axiom states that

> Whenever the propositional function G(x) is definite for all elements of a set M, M possesses a subset M_G containing as elements precisely those elements x of M for which G(x) is true.[17]

Zermelo's explanation of the key term 'definite' in this definition is:

> A question or assertion is said to be definite if the fundamental relations of the domain [i.e., membership], by means of the axioms and the universally valid laws of logic, determine without arbitrariness whether it holds or not. Likewise, a "propositional function"

> [Klassenaussage] G(x), in which the variable term x ranges over all individuals is said to be definite if it is definite for <u>each</u> <u>single</u> individual x of the class R. Thus the question of whether $a \epsilon b$ or not is always definite, as is the question of whether $M \subseteq N$ or not.[18]

The new feature of the separation axiom which distinguishes it from the comprehension principle is the limitation to subsets of some set. Other than the limitation as the definiteness, the "propositional function" G(x) is unrestricted in the sense in which Bernays uses that term. As we shall see, Bernays (and others) allege that (a) later formulations of the separation axiom have introduced restrictions on G(x) and that (b) these restrictions are responsible for the independence results. It will be our aim to take on this (two-part) claim by reviewing the history of the notion of definiteness since Zermelo in order to see what restrictions, if any, have been introduced and what affects they may have had.

Zermelo later admitted that the explanation of definiteness which we have quoted leaves much to be desired: it was just for this reason that the "more restrictive" accounts of definiteness were proposed. From his own original account, about all one can conclude is that his intention was to exclude "propositional functions" with borderline cases among the members of the set M. That is, he excludes cases in which it is impossible to determine "without arbitrariness" whether G(x) holds, not because our knowledge is inadequate, but because no answer is correct.

In his original paper--from which the earlier quotations are taken--Zermelo gives examples of definite propositional functions but none of non-definite ones. These he supplied, however, in a much later paper, lisiting "green colored set" and "smallest irrational number which can be defined in a finite number of words of an arbitrary European language", and, in general, "every sentence which includes extrasystematic [systemfremde] relations."[20] These examples suggest that a reasonable way of avoiding non-definite propositional functions is to restrict the language in which set theoretic proofs may be carried out so as to exclude the "extra-systematic" terms. This has proved to be the basic technique of later set theorists in their accounts of definiteness.

Skolem's Approach

Of the two main strategies for incorporating a more precise account of definiteness into set theory, the work of Skolem is representative of the treatment which eventually resulted in ZF.

On his account, a proposition is definite if it is a "finite expression constructed from elementary propositions of the form $a \varepsilon b$ or $a = b$" by means of negation, conjuction, disjunction, and universal and existential quantification.[21] A similar proposal was made independently by A. Fraenkel, which he used to proved the independence of the axiom of choice from a version of ZF.[22]

Skolem regarded his notion as "completely clear" and "sufficiently comprehensive to carry out all ordinary set theoretic proofs",[23] and it is clear that each of the applications of separation in Zermelo's 1908 paper is justified in Skolem's version of definiteness.[24]

Skolem's idea is preserved in the moderm formulation of the Replacement (and Foundation) axiom schema(ta) of ZF:

Replacement: $(z_1)\cdots(z_n)(x)((u)(u\epsilon x \supset (\exists! v)F(u, v, z_1, \ldots, z_n)) \supset$
$(\exists y)(v)(v\epsilon y \equiv (\exists u)(u\epsilon x \,\&\, F(u, v, z_1, \ldots, z_n)))$

where F is a well-formed formula of the language of ZF, and the free variables of F are among $u, v, z_1, \ldots, z_n$.

The axiom of separation is a easy consequence of replacement, so it need not be listed separately among the axioms of ZF. That is, for any formula $G(v, z_1, \ldots, z_n)$ with the free variables shown, one need only consider the instance of replacement with

$$u = v \,\&\, G(v, z_1, \ldots, z_n)$$

in place of F. The separation axiom,

Separation: $(z_1)\cdots(z_n)\ (x)(\exists y)\ (v)(v\epsilon\, y \equiv v\epsilon\, x \ \ \&\, G(v, z_1, \ldots, z_n))$

is then a consequence by quantifier logic.

Evidently, the conditions imposed on G which are inherited from those in the replacement schema are exactly equivalent to those in Skolem's proposal. Few would argue that experience with ZF has not substantiated his claim that his account is "adequate for all ordinary set theoretic reasoning."

Zermelo's Reaction to Skolem and Fraenkel

In 1929, Zermelo wrote a short paper on definiteness,[25] which attacked Frankel's approach and presented a new account of his own. He seems to have been unaware of Skolem's related work.[26]

Zermelo was critical of the Skolem-Fraenkel account on two grounds. First, he held that Fraenkel's "constructive" procedure of "admitting only special forms of propositional functions" "fundamentally contradicts the purposed and essence of the axiomatic method."[27] He offers almost nothing by way of explanation for this indictment of constructive methods. He says only that Fraenkel's "genetic" procedure concerns only the "origin or generation [Erzengung] of the sentence, rather than the sentence itself, and yields no certain decision in involved cases". Whatever is at stake in this objection, and I do not pretend to be clear about it, exactly similar arguments could be raised against Zermelo's own proposal, which we will consider shortly.

Zermelo's second objection is far clearer but no more decisive. Fraenkel states that definite functions to be used in the separation axiom are those which can be formed "by a prescribed application (repeated only a finite number of times, of course, ...)" of the five fundamental operations,[28] as in Skolem's proposal. Zermelo objects that this version "depends on the concept finite number, whose clarification is still a principle task of set theory."[28a] Zermelo offers no argument for the claim that the notion of finite number is in need of clarification or needed it in 1929, nor can I see any reason to believe that this is so. In any case, it is not strictly true that the Fraenkel-Skolem formulation makes use of finiteness, even though Fraenkel did include the word 'finite' in a parenthetical remark. In fact, both writers are simply giving recursive definitions, and the clauses of such definitions, as for example

"If F, G are definite, so is their conjunction,"

say nothing about numbers at all.[29]

It is true, of course, that the expressions which result are all finite. This is an important fact because languages with expressions of infinite length will fail to meet our effectivity requirement for formal systems except is the quite special case where well-formedness and axiomhood can be determined from some finite initial portion of each expression. But we still need not say anything about finiteness when describing the language when it contains only the usual sort of syntax.

A further noteworthy point is that, like the first, this objection also applies to Zermelo's own new account of definiteness, which we have yet to describe.

Restriction on Definite predicates and Independence

Despite the inconclusiveness of Zermelo's attack on the Fraenkel-Skolem proposal, his opinion that it was too restrictive, admitting only "certain special forms of propositional functions"[30] anticipates the objections of Bernays and other writers.

As an example of Bernays' view, consider the following remarks which he made in reply to criticism of the paper we cited at the beginning of this chapter:

> For the applicability of Cohen's result it does not, of course, matter . . . whether axiomatic set theory is presented as a formal system or not, and even less in what particular system it is formalized. What the applicability does depend on is the mentioned sharper axiomatization by which strict formalization becomes possible, that is, the Fraenkel-Skolem delimitation of Zermelo's concept '<u>definit</u> <u>Eigenschaft</u>' [definite property].[31]

In a similar vein, R. Smullyan states that

> Zermelo's system, despite its nonformal character, comes far closer to the true Cantorian set theory. Of course, there is no harm in laying down formal axiom which force at least the first-order properties into the picture, but there is no reason to identify all properties with those properties whose existence is forced by the axioms.[32]

Smullyan calls attention to the system VBI, which we shall shortly consider. He regards this system as having axioms for "second-order properties", but believes that it too is over-restrictive:

> [VBI] should not be thought of as the whole Cantorian set theory, since there is no reason to identify properties with second-order properties either.[33]

As previsouly announced, our aim is to examine attempts to lift the supposed restrictions introduced by Fraenkel and Skolem, and to see if any of these attempts produce a system in which it is possible to decide the CH. We will survey six proposals. The first three are von Neumann's and two which derive from it, Zermelo's later proposal and VBI. The fourth, J. Friedman's STC is an extension of von Neumann's system in a richer language. Next, we will treat takeuti's NTT which is based on ZF in a richer language. Finally, we will consider Kreisel's proposal to reformulate ZF with a second-order underlying logic. All of these systems are described in some detail in Appendix B; the text will concern only basic ideas. We will begin by describing the modern version of von Neumann's system, VBG (for von Neumann-Bernays-Godel set theory).

Before proceeding, however, it is in order to ask why we should have to consider additional technical proposals, rather than sticking to Zermelo's system as Smullyan suggests. The reason is that because of the vagueness of Zermelo's account of definiteness, we simply cannot tell whether independence results can be carried out or not. We have Zermelo's word for it that the Frankel-Skolem account departs from his intentions, but it is far from clear just which features of their account are deficient or what definite properties they exclude. Plainly we must have some of this sort of information if we are to be able to evaluate the informalist explanation of the independence of CH. As I see it, the cash value of the informalist position must be some reason to believe that theories with a richer supply of definite properties would decide CH. The conclusion which I will draw is that a survey of existing proposals provides no such reasons. We will return to this question after the specific proposals have been examined.

Von Neumann's System

Von Neumann's system was the first of a long series of attempts to clarify the separation priciple by providing axioms for definite properties rather than treating the predicates which express them.

In his original system,[34] Von Neumann left unspecified the relation between the definite properties, which he called "type II objects", and the sets of his systems. Bernays subsequenctly proposed a system in which no "type II object" is a set, but every set has a corresponding "type II object" which represents it; the converse, however, does not hold.[35]

A tidier system is VBG, in which every set is a "type II object"-- now called a class--but some classes are not sets. Classes cannot be thought of as definite properties, however, but only as surrogates for them. Classes are "surrogates" because they are extensional w.r.t. the "exemplification" relation. That is, classes which are "exemplified" by the same objects are identical, and it is a commonplace that this is, in general, false of properties. It might be maintained that definite properties are extensional, and hence that classes need not be regarded as surrogates, but as definite properties themselves. If the non-extensionality of properties generally is as important as the conventional wisdom would have it, however, it is implausible that it should apply only to indefinite ones, since these are simply properties without border-line cases or such as are (perhaps) expressed by nonsense predicates such as 'green-colored set'. Since classes are extensional, it is perhaps most reasonable to regard them as the extensions of definite properties.

In VBG, the "set" axioms are the same as those for ZF, except that the two schemata of replacement and foundation are each replaced by a single axiom involving quantification over classes:

Repl: $(F)(x)(\mathrm{Func}(F) \supset (\exists y)(v)(v \in y \equiv (\exists u)(\langle u, v\rangle \in F \,\&\, u \in x)))$

Found: $(X)(X \neq \emptyset \supset (\exists u)(u \in X \,\&\, u \cap X = \emptyset\ )$

In these formulas, upper-case Roman letters are class variables; 'Func' is a formula which says that F is a functional class of ordered pairs. In addition to these two axioms, an axiom schema of class existence is added which assures the "definite property representing" function of classes:

Comp: $(X_1) \ldots (X_n)\ (\exists Y)\ (x)\ (x \in Y \equiv G(x, X_1, \ldots, X_n))$

This "comprehension" schema says, in essence, that the extension of a well-formed formula with one free set variable is a class.

This bald statement of the class comprehension principle needs the following important qualification for VBG, that the formula G may contain no bound class variables. If this restriction is lifted so that classes may be defined by quantifying over classes, the system which results is the so-called Kelley-Morse system,[36] which we will call VBI (for VonNeumann-Bernays-Impredicative). Smullyan's remarks about VBI give us reason to be particularly interested in this system, and we will return to it shortly.

As for VBG, the exclusion of bound class variables in Comp has the consequence that VBG is equivalent in strength to ZF in the sense that a formula of VBG without class variables is a theorem of VBG iff it is a theorem of ZF.[37] On the other hand, the apparatus of class quantification in VBG permits a slightly greater expressive power. For example, we will shortly define a function defined on all sets--the so-called rank function--which can be proved to exist in VBG, but not in ZF. In ZF, it is possible to prove the existence of each portion of this function which is a set, so each application of the rank function to sets can be expressed in ZF in a rather less convenient way. The price of this greater technical convenience of VBG is, however, the philosophical difficulty of interpreting class quantification; we will treat this problem in the next chapter.

As one would expect, lifting the restriction on Comp to yield VBI results in a somewhat greater proof-theoretic strength of that theory. In particular, we can formulate a truth definition for VBG in VBG, but it cannot be proved in VBG that all the theorems of VBG are true unless VBG is actually inconsistent. In VBI, on the other hand, we can prove this fact about VBG. The proof uses a formula $(\exists X)C(X, y)$, where C contains no bound class variables, to define a class A:

$$A = (\iota Y)(x)\ (x \in Y \equiv (\exists X)C(X, x)).$$ [37a]

If this class could be defined in VBG, the proof would go thru within that system; so if VBG is consistent, A is a class which can be defined in VBI but in VBG. There are, in fact, many more of these, but in order to look into the matter of the additional definite properties required by VBI, we need a short digression.

Natural Models for ZF, VBG & VBI

There is a class of "natural" models of the theories which we have been dealing with which informalist writers and others are particularly and rightly concerned to study and characterize. Although we will reserve most model-theoretic questions for the next chapter, it is useful to describe them here. For the time being, we forget any possible doubts about whether there is must one "universe" of sets and simple speak about the sets, distinguishing them occasionally form other structures which may be models of the theories we discuss.

It is a theorem of VBG that there is a function defined on all ordinals which has the universe of sets as its range, and decomposes this universe into levels or ranks, each containing the previous ones. This rank function R may be defined by transfinite induction as follows:

$$R(0) = \emptyset$$

$$R(\alpha') = P(R(\alpha)), \text{ where } P \text{ is the operation "powerset of"}$$

$$R(\lambda) = \bigcup_{\alpha<\lambda} R(\alpha), \text{ where } \lambda \text{ is a limit ordinal.}$$

When we are thinking of "the" universe of sets with the structure imposed by R, we will speak of the "cumulative type structure" (CTS). If x is a set, we call the least ordinal α such that $x \in R(\alpha + 1)$ the rank of x.

It is natural to ask whether some initial portion of the CTS, the sets of rank less than some ordinal $\varkappa$, form a model for VBG. The answer to this question is "yes" if there exist ordinals of a certain "large" size, called inaccessible ordinals. An ordinal $\varkappa$ is said to be (strongly) inaccessible if it is a regular cardinal and if, for all $\overline{\overline{\alpha}} < \varkappa$, $2^{\overline{\overline{\alpha}}} < \varkappa$, where regular ordinals α are those which are not the sum of fewer than α ordinals less than α. By this definition, ω is inaccessible. For reasons which we will briefly indicate in the next chapter, it is plausible that there are inaccessibles $> \omega$, but this cannot be proved in VBG or VBI. Since we will be interested only in such inaccessibles, we include the condition "$> \omega$" in the definition of inaccessibility.

The fact which makes inaccessibles in this sense interesting for the present discussion is that if $\varkappa$ is inaccessible, the sets in $R(\varkappa + 1)$ form a model M for VBG and VBI, with the 'ϵ' symbol interpreted as the "real" ϵ relation restricted to $R(\varkappa + 1)$.[38] In this model, the "sets" are the elements of $R(\varkappa)$, and the proper (non-set) classes are elements of $R(\varkappa + 1) - R(\varkappa)$, the subsets of $R(\varkappa)$. It is plain that M is a natural model from the point of view of the separation axiom. That is, separation is used to prove the existence of various subsets, but the construction of M guarantees that every "actual" subset of any set in the "universe" is (the extension of) a definite property.

In order to have a "rank model" of the set portion of VBG or VBI, however, it is not necessary to include all of the subsets of sets of the model. Instead, we need only include certain subsets which are definable in a way which parallels the class comprehension axiom. That is, we call a set S first-order definable over a rank structure $R(\varkappa)$ (or more generally, over any transitive set of sets) if there is a formula of ZF, $F(x, z_1, \ldots, z_n)$, which is true in the structure $R(\varkappa)$

of all and only the x's which are in S, where $z_1, \ldots, z_n$ are given some fixed interpretation in $R(\varkappa)$. We call the set of all such sets S: Fodo $(R(\varkappa))$. It then happens that $R(\varkappa) \cup \mathrm{Fodo}(R(\varkappa))$ is a model for VBG, but not VBI.[39] This shows that not all of the subsets of $R(\varkappa)$ need have been included in M to obtain a model of VBG, since $\mathrm{Fodo}(R(\varkappa))$ is smaller (in the sense of cardinality) than $R(\varkappa + 1) - R(\varkappa)$. It also permits us to gauge the effect of the restrictions on the Comp axiom of VBG, because we can show that the axioms of VBI require more of $R(\varkappa + 1) - R(\varkappa)$ than $\mathrm{Fodo}(R(\varkappa))$ to be added to $R(\varkappa)$ to obtain a model. In fact, it is easy to show that if $R(\varkappa) \cup B$ is a model for VBI, and $B \subseteq R(\varkappa + 1)$, then B must contain at least as many "classes" not in Fodo $(R(\varkappa))$ as there are in Fodo $R(\varkappa)$ itself.[40] On the other hand, B need not contain all of $R(\varkappa + 1) - R(\varkappa)$,[41] so VBI does not have sufficient power to "force" all of the "classes" in $R(\varkappa + 1)$ to "exist".

VBG and VBI Compared:

The preceeding discussing makes clear that with respect to the natural models given by the function R, VBI can prove the existence of substantially more definite properties than VBG, although neither theory can prove the existence of all the "classes" of a natural model. The additional strength of VBI carries with it some attendant risks, however, in that the more generous class comprehension axioms may introduce some inconsistency.

It is easy to show that VBG is extremely conservative in its class existence principles, and that it represents a quasi-constructive approach to class existence in a way in which VBI does not. The technical fact which justifies this claim is that the predicative comprehension schema of VBG can be replaced by one purely existential axiom and six additional axioms which permit constructions from classes previously shown to exist. The existential axiom asserts the existence of a class of all ordered pairs $<x, y>$ such that $x \in y$. The other axioms guarantee the existence of the intersection, compliment, cross-product, etc., of any classes. Since all these operations are of a familiar, elementary, and constructive kind, accepting VBG commits us only to classes which can be proved to exist by a finite sequence of operations which we have not the slightest reason to believe would lead to any inconsistency. This intuitive argument for confidence in the class existence assumptions of VBG is, or course, born out by the previously cited "equivilence" of ZF and VBG.

Such a conservative interpretation of class existence assumptions will plainly not be possible for VBI, since any axiomatization of that theory must include at least one class existence axiom with bound class variables. This axiom would exclude the "ordered generation" scheme just described for VBG because it would define a class by reference to (that is, quantification over) the whole totality of classes, not just previously constructed ones. It has not, in fact, even been shown that VBI is finitely axiomatizable.

On the other hand, it would be a mistake to over-estimate the risks of VBI, since it is known that if VBG plus the assertion that there exists an inaccessible ordinal is consistent, so is VBI.[42] The prevailing opinion is that the likelihood that such a theory is inconsistent is small.

Despite its extra "richness" in definite properties, the final outcome of our discussion of VBI is rather anti-climactic. Despite the hope of Smullyan and conjectures by others,[43] Tharp and Solovay[43a] have (independently) shown that CH(and even GCH) is independent of VBI, even if AC is added, so that the first step of our attempt to cash in informalist objections does not succeed.

<u>Zermelo's proposal:</u>

As we noted above, Zermelo was dissatisfied with the Fraenkel-Skolem proposal because it (allegedly) makes use of finiteness and "yields no certain decision in involved cases." His own offering was an axiomatization of definite propositional functions as follows:

Let some class B of objects be given: then the "fundamental relations" of the theory in question, in our case, the membership relation, are definite for all "value combinations" from B. <u>I. e.,</u>

(1) $D(x \in y)$ for all arguments x, y from B, where $D(p)$ is to be read "p is definite".

Next, he states that "definiteness carries over to compound propositions",[44] that is,

(2) If $D(p)$, then $D(\bar{p})$, where $\bar{p}$ is the negation of p,

and

(3) If $D(p)$ and $D(q)$, then $D(p \cdot q)$ and $D(p \vee q)$, where $p \cdot q$, $p \vee q$ are the conjunction and disjunction of p and q, respectively.

For quantification, he states that

(4) If $D(f(x, y, z, \ldots))$ for all "value combinations" from B, then $D((x)(y)(z) \ldots f(x, y, z, \ldots)$ and $D((\exists x)(\exists y)(\exists z) \ldots f(x, y, z, \ldots))$.

Zermelo also lays down an axiom for "second order" quantification of propositional functions, that is

(5) "If $D(F(f))$ for all definite functions $f = f(x, y, x, \ldots)$, then also $D((f)F(f))$ and $D((\exists f)\, F(f))$".[45]

Finally, Zermelo includes a restrictive clause which "concerns not so much the particular 'definite' propositions p as their totality P".[46] This axiom states that

(6) The collection P of definite propositions must contain no "proper subsystem [echtes Untersystem] P_1"[47] which satisfies (1) - (5).

Zermelo asserts that this characterization avoids the use of finiteness, but we have already seen that the Fraenkel-Skolem idea does so as well. In a reply to Zermelo's paper,[48] Skolem asserted that the only difference between Zermelo's new notion and his own is the second-order quantification clause (5) and the restrictive clause (6).[49] Skolem criticized (5) as unclear, unjustified, dangerously close to the paradoxes, and not clearly more extensive than his own proposal.[50] Against (6), he argued that whether or not finiteness needs to be clarified by set theory, the notion of "subsystem" certainly should be.[51]

This later objection is certainly well taken, but it we substitute classes for definite properties (or "propositional functions") in (1) - (5), the result is the class comprehension principle of VBI. Even if troublesome points remain in the interpretation of classes in VBI, it is clear from our previous discussion that the risks of antinomies in VBI are small and that VBI does provide for more definite properties than the Skolem-like principle in VBG.

VBI does not, of course, contain anything corresponding to the restriction (6). Indeed, it could not, for (6) is essentially a metatheoretical statement not expressible in VBI itself. We must conclude that VBI is at least as rich in definite properties as Zermelo's proposal, so that the independence results cannot be eliminated by adopting his characterization.

<u>"Superclass" Theories</u>:

So far, the accounts of definite properties, propositions, functions, etc., which we have considered amount to small modifications of the basic class comprehension principle of VBG. We now turn to a group of theories which take the axioms of VBG or ZF as a basis and add a heirarchy of "superclasses" which can be used in Replacement, Foundation, etc., just as are classes.

In VBG and VBI, proper classes are distinguished from sets by "non-elementhood". That is, a class is proper just in case it is not a member of any class. This criterion plays a role in the avoidance of the paradoxes such as Russell's, since there can be no classes of proper classes at all, much less a class of non-self-membered classes.

Several authors[52] have developed variations of VBG which countenance new entities which can contain proper classes as elements, and it is these added entities we will call "superclasses". The particular proposals which we will discuss are J.I. Freidman's theory STC, and G. Takeuti's NTT(for Nodal Type Theory), beginning with the former.

To form STC, Freidman adds a hierarchy of "extended" sets, indexed by ordinals and "extended ordinals", to the sets and classes of VBG. Level zero extended sets are simply sets, and level zero extended proper classes are simply proper classes in the usual sense. Extended sets of level one contain sets and classes of level zero, and the collection of level one extended sets is a level one proper class. In general, for each ordinal (or "extended ordinal") β there is a level β consisting of extended sets and classes, each level containing all earlier levels.[53]

Friedman formulates STC by adding to the language of VBG a new predicate M(x), interpreted as "x is an extended set", and he shows that STC can be interpreted[54] in VBG+I0, VBG augmented by the assertion that there exists an inaccessible ordinal.[55] In fact, VBG+I0 is equiconsistent with the theory VBG+ "there is a class M satisfying the hierarchy axioms of STC".[56]

In effect, adding "higher type" or "super" classes to VBG in the manner of STC is equivalent to adding sets farther out in the rank hierarchy of sets. This should not come as any great surprise if we recall that in the natural rank models for VBG, the "proper classes" are simply the sets of highest rank. This phenomenon also bears out an observation of Godel (in a slightly different context) that "the following could be true: Any proof for a set theoretical theorm in the next higher system above set theory . . . is replaceable by a proof from . . . an axiom of infinity".[57]

STC does just this: it erects a "higher system above set theory" in the form of extended sets and classes. The fact of the equivalence of this procedure with the assertion that the CTS itself is "long" -- that is, an appropriate axiom of infinity such as I0--gives just the result we would expect about CH. That is, since virtually all of the axioms of infinity, including I0, fail to decide CH or even GCH,[58] these statements are both independent of STC.[59]

Nodal Type Theory:

In formulating NTT, Takeuti makes use of the rough equivalence between "higher type" classes and higher rank sets in natural models which we have already noted. In the simplest case, the $R(\varkappa + 1)$ model of VBG, the proper classes are members of $R(\varkappa + 1) - R(\varkappa)$. For example, in the formula (X)F(X), where X is a class variable, and the set variables are regarded as ranging over $R(\alpha)$, (X)F(X) may be rewritten as $(x)(x \varepsilon R(\alpha + 1) \supset F(x))$. Takeuti considers a generalization of this idea in which the reinterpretation of (X)F(X) is $(x)(x \varepsilon R(f(\alpha)) \supset F(x))$, where f is some function of α, perhaps different from $\alpha + 1$. In Takeuti's treatement, the function f is replaced by a formula $A(\alpha, \beta)$ functional in its first argument. The variable X which is reinterpreted in this way is then labeled with the formula $A(\alpha, \beta)$ as a superscript: $(X^A)F(X^A)$. Such variables are then classified as to "degree". The degree of a set variables is zero, and if $A(\alpha, \beta)$ is a formula in the ordinary (first-order) language of ZF, X^A has degree 1. If $A(\alpha, \beta)$ involves variables of degree higher than 0, say at most n, then the degree of X^A is n+1. The types or levels of the "superclasses" of NTT are thus labeled by formulas of an expanded language based on the language of ZF, as opposed to STC's indexing of levels by ordinals and "extended ordinals".

Besides this difference in formulation of the hierarchy of superclasses, NTT has a completely new feature having no counterpart in STC, the notion of "nodal" class of ordinals. This notion may be explained as follows. Consider the first-order language L_{ZF} whose only non-logical symbol is 'ε' and let T_α be the "theory of $R(\alpha)$", the set of all sentences of ZF true in $R(\alpha)$. Takeuti reasons that since the number of distinct T_α's is bounded but the class of ordinals is not, some theory T must appear infinitely often in the series of T_α's. Takeuti conjectures that there is only one theory which will appear "overwhelmingly often", and he wishes to "define the absolute set theory to be this theory".[60]

In order to make precise the (quasi-measure-theoretic) notion of "overwhelmingly often", Takeuti introduces the notion of a "nodal" class of ordinals. The fundamental property of nodal classes is intended to be that for any closed sentence φ, "φ is true" will be equivalent to "the class of α's such that $\varphi^{R(\alpha)} \in T_\alpha$ is nodal", where $\varphi^{R(\alpha)}$ is the relativization of φ to the rank $R(\alpha)$.

The explanation of the previous paragraph was confined to formulas of ZF; in NTT, Takeuti extends the same idea to the language including typed variables X^A, and also adds a new one-place predicate N(X), read "X is a nodal class" (or superclass). The result is that NTT is a extremely powerful theory compared to ZF. This power may be illustrated in the following way:

We have already noted that the existence of inaccessible ordinals cannot be proved in ZF. In NTT, however, it is a theorem that the inaccessibles form a nodal class. In other words, there are overwhelmingly many inaccessibles according to NTT.[61]

From one point of view, this is an extremely implausible result, since an inaccessible is an ordinal which is enormously larger than any of its predecessors, and it is difficult to believe that "almost all" ordinals have this property. There is, however, as rationale by which we may justify retaining the nodal notion despite this feature. This rationale is that the nodal axioms of NTT may be thought of as very strong axioms of infinity. Such axioms as that of measurable cardinal (MC) are known to imply the existence of many inaccessibles. In particular, ZF + MC implies that there are κ inaccessibles less than the least measurable cardinal κ.[62]

This interpretation of the consquences of the nodal axioms conforms with Takeuti's stated intentions of presenting a theory in which "the creation of ordinals is endless, and therefore there is no absolute universe of our set theory".[63]

The way in which such "large infinity" properties are incorporated into NTT may be seen in this way: A reflection principle is a principle which states that for any formula φ of some given language of set theory, there is an $R(\alpha)$ such that φ is true of all sets just in case $\varphi^{R(\alpha)}$ is true in $R(\alpha)$. Depending on the given language and the additional conditions imposed, reflection principles are equivalent to various strong axioms of infinity. (Reinhardt has even obtained a principle which is equivalent to MC.)[64] In NTT, one has an extremely strong reflection principle. This principle is simply the fundamental property of nodal classes, that if φ is a closed sentence of NTT, and $\varphi^{R(\alpha)}$ is φ relativized to $R(\alpha)$ in a special way given in NTT, then

$$\varphi \Leftrightarrow N(\{\alpha/\varphi^{R(\alpha)}\}).$$

In other words, φ is true just in case it is "reflected" in almost all $R(\alpha)$'s.

NTT & CH:

Complete independence investigations for NTT are not yet available, but the partial ones so far obtained suggest that CH may well be independent of NTT.

The first result is that if NTT is consistent, so is NTT + "Every set is constructible".[65] It is, or course, already known that this implies the consistency of CH and GCH with NTT. The second partial result concerns NTT^1, the restricted version of NTT which results from excluding variables of degree greater than 1. Takeuti credits R. Solovay with showing that NTT^1 can be interpreted in "a system" in which there are measureable cardinals.[66] It is known, however, that for ZF, there are non-constructible sets if there is a measurable cardinal.[67] If this result carries over to NTT^1, "Every set is constructible" cannot be a theorem of NTT^1. Thus, very provisionally we have some reason to believe (subject to future research) in the independence of "Every set is constructible" from NTT^1. This does not, or course, settle the independence of CH even from NTT^1, but it represents a first step in independence theorems for NTT.

NTT and Informalism:

We have seen that of all of the proposals about definite properties which we set out to investigate, only NTT could possibly decide CH. Our incomplete knowledge about NTT is not really an obstacle to evaluation of the program we had set out, however. That program was to determine whether enriching set theory with definite properties might settle CH. The reason that NTT has this status is that although it does have an enriched apparatus for "superclasses", it clearly has new set theoretic axioms as well. As we saw at the outset, informalism is only interesting doctrine if it is construed as claiming that the standard axioms, properly formulated, would decide CH. But NTT goes beyond standard axioms to such as extent that we should not regard a proof that NTT implies CH, in the unlikely event that one should be produced, as a vindication of the informalist position. Nevertheless, NTT is worthy of consideration, for it gives some indication of the complexities of definite property notions which may yet be investigated.[68]

Our investigation of axiomatic systems which may be richer in definite properties than ZF has been confined to theories with the same fundamental conception of the structure of the universe of sets as ZF--the CTS. The reason for this is that almost every one regards this class of theories as the most plausible ones in the field. There are, however, other theories, in particular Quine's NF and ML. We will discuss these theories briefly in the next chapter, but we mention here that no independence results have been published for these theories. There are two reasons for this, as I see it. The first is that although there was considerable interest in ML and NF twenty years ago, investigators in foundations are no longer concerned with them. The second reason is that since no clear notion of the structure of the universe for ML--comparable to the CTS for ZF--is known, the Godel-Cohen results cannt be carried over to ML in any obvious way. Godel's and Cohen's costructions make essential use of the idea of the CTS in the construction of models. Both mimic the rank structure in their models. No such simple principle about construction of models for ML is yet known. Finally, we will argue in the next chapter that neither ML nor NF is plausible, so that a proof or refutation of CH in either theory would not settle the continuum question.

There remains one important approach to the problem of definite properties which we must consider, the formulation of ZF in second-order logic. This is the subject of the concluding sections of this chapter.

Why Consider Second-Order Logic?

We are about to begin a discussion of second-order logic (SOL) which will occasionally take us out o f set theory altogether, so it is important to justify this digression at the outset. We will first provisionally characteriza SOL, and then consider two compelling reasons for studying it in connection with CH.

As a working definition, we will say that a theory is formulated in SOL if, in addition to the usual apparatus of a first-order theory, it contains variables for predicates (or relations) and permits quantification of them. With such a definition, we can easily show continuity of SOL with our earlier concerns with definite properties. The connection is simply this: If we think of predicate constants as "standing for" (we do not attempt to be precise about what "standing for" means) properties or relations, then in binding predicate variables with quantifiers, we are presumably quantifying over properties. On this rationale, we are automatically studying (definite?) properties if we study this sort of logic.[69] This is a conclusion which would have pleased Zermelo, since he made it clear that he would have preferred to study definite properties as a general problem of logic, not one concerning only set theory.[70]

Our second and more compelling reason for spending effort on SOL as applied to set theory is G. Kreisel's repeated claim that CH is decided in second-order formulations of ZF.[71] To be fair, Kreisel does not mean this in the particular sense of 'decided' which we have adopted, <u>i.e.,</u> proved ot refuted. A characteristic formulation in his own words is that "CH is (provably) <u>not</u> independent of the full (second-order) version of Zermelo's axioms".[72]

It is my contention that these assertions of Kreisel's could hardly be more misleading, and that his evaluation of the significance of the mathematical facts on which he bases his claim is incorrect.

It is my intention in the remainder of this chapter to show just why this is so, and to consider what value, if any, second-order logic may have for further research on the CH.

The reader should be warned that although I will be attacking Kreisel's position on this particular point, I am in agreement with him on important points which will be treated in Chapter II. The aim of the present discussion is to clear away matters which I believe obscure those points.

In the sections which follow, we will treat first the question of a more precise characterization of "second-orderness". We will then consider a few of the differences between first- and second- order theories, and see what advantages the latter may have over the former. Finally we will see in just what sense CH is "decided" in second-order ZF and consider what importance would be attached to this result.

Arithmetic: A Second-Order Example:

Following our working definition of second-order theory, we could formulate arithmetic as follows: Our language contains, besides logical constants, the constant '0' , the function symbol 'S' ("successor") and the function symbols '+' and 'x'. We include one-place predicate variables, and adopt the usual (first-order) axioms except that the induction schema is replaced by a single axiom with predicate quantification:

(1) $(P)\ ((P(0)\ \&\ (x)\ (P(x) \supset P(S(x))\ )\ )\ \supset (x)\,P(x)\ ).$

We call the resulting theory S^2.[73] A possible model M for S^2

can then be defined as the sextuple $\langle \underline{N}, \underline{C}, \underline{+}, \underline{x}, \underline{S}, \underline{0} \rangle$. $\underline{N}$ is the first-order universe of "natural numbers'; $\underline{C}$ is the second-order universe, consisting of subsets of $\underline{N}$ (not necessarily all of them). We also have $\underline{0} \in \underline{N}$, $\underline{S} : \underline{N} \rightarrow \underline{N}$, $\underline{+} : \underline{N} \times \underline{N} \rightarrow \underline{N}$. We will deal later with the question of just how much of the powerset of $\underline{N}$ is to be in $\underline{C}$.

As an alternative formulation of "second-order" arithmetic, $S^{2'}$, we consider the following theory. The language of $S^{2'}$ contains only first-order quantification, with non-logical constants 'O', 'S', '+', 'x' as before, and two new one-polace predicate symbols 'N', 'C', read "is a number" and "is a set" respectively. Finally, we add a two-place predicate symbol 'ϵ' , interpreted, as usual, as membership. The induction axiom for $S^{2'}$ may be stated:

(2) $(x)((C(x) \& 0 \in x \& (y)((N(y) \& y \in x)) \supset S(y) \in x \supset (y)(N(y) \supset y \in x))$.

Of course (2), together with the other usual Peano axioms will not yield a viable formulation of arithmetic. We will need some axioms of class existence, an extensionality axiom for 'ϵ' , axioms which state that every thing is either a number or a set but not both, and axioms to settle similar technical matters.[74] A possible model for $S^{2'}$ will be of the form $\langle \underline{U}, \underline{N}, \underline{C}, \underline{+}, \underline{x}, \underline{S}, \underline{0}, \underline{\epsilon} \rangle$ where $\underline{U} = \underline{N} \cup \underline{C}$, $\underline{C} \subseteq P(\underline{N})$, and the other elements have the obvious interpretations.

Is $S^{2'}$ a second-order system ? If we stick by our provisional definition, the answer must be "no", for $S^{2'}$ does not permit quantification over predicates. But there is a rationale for answering "yes", and abandoning the restrictions of the provisional definition. It is plain that nothing we have said so far about the possible models for S^2 and $S^{2'}$ rules out our taking the possible models for S^2 to be exactly those obtained from $S^{2'}$ structures by omittting the first and last elements.

There are clearly other intimate relations between S^2 and $S^{2'}$, most easily summarized as follows: If M is the standard model for S^2, where $\underline{N}$ is the set of natural numbers, $\underline{C}$ is the powerset of $\underline{N}$, etc., and M' is the $S^{2'}$ structure obtained from M by adding the set $\underline{\epsilon} = \{ \langle x, y \rangle \mid x \epsilon \underline{N} \ \& y \epsilon \underline{C} \ \& x \epsilon y \}$ as the last element of the tuple and $\underline{U} = \underline{N} \cup \underline{C}$ as its first, then

(3) $M \models P(x)$ iff $M' \models N(x) \ \& C(y) \ \& x \epsilon y$, where the interpretation of the predicate variable P is the same as that of y.

Of course there are also marked differences between our two "versions" of arithmetic. The difference between individual and predicate variables is marked by variable style in S^2 and by the predicates C(x) and N(x) in $S^{2'}$. As we have seen, this gives rise to a few additional axioms in $S^{2'}$ which concern C(x) and N(x).

Evidently, $S^{2'}$ must also have some axioms guaranteeing the existence of various sets, but we have listed no comparable "comprehension" axioms for S^2. This distinction is about to disappear, however, because as we have formulated S^2, there is no way to show that specific (first-order) predicates may be substituted in (1), the induction axiom for S^2. This defect must be remedied by stating further logical axioms for the second-order portion of S^2, as is done, for example in Church's book.[75] Such axioms have the form of a schema, the closure of

(4) $(P)F(P) \supset F(\Psi)$,

where Ψ is a formula satisfying certain restrictions we will mention later. It is plain that transposing (4) and prenexing the resulting predicate quantifier will yield a schema whose form is (the closure of)

(5) $(\exists P)\,(G(\Psi) \supset G(P))$,

which is evidently a kind of comprehension schema for predicates, but counted among the logical axioms of S^2.

The point that I wish to extract from this pair of examples is that there is a strict parallel between theories which are second-order in our original, provisional sense, and those which are what we would ordinarily consider first-order theories with set theoretic notions included. In later sections, we will show that the parallelism exhibited in these arithmetic examples also exists between second-order formulations of ZF and VBI.

For the present, we will confine our discussion of SOL to the original sense of that term, but the reader is requested to keep in mind that our remarks could be recast interms of the first-order-cum-set theory sort of SOL exemplified by $S^{2'}$. The following section will be concerned with the model theoretic and metalogical properties of SOL, as compared with general properties of first-order logic (FOL).

Properties of SOL:

So far, I have been deliberately vague about how much of the powerset of the first-order universe of a possible model in SOL must be included in the second-order universe (that is, the range of the second-order variables). There are three commonly considered conditions for such possible models, and as one would expect, the properties of the resulting model theory for SOL depends critically on which one is chosen.

The first condition is that the SO universe should include only the finite subsets of the FO one. The result is called _weak second-order logic,_ and we will not consider it here. The second possible condition is to require that the SO universe be identical with the powerset of the FO universe. This is the notion which Kreisel has in mind. We shall call such structures '*-structures', or, where appropriate, '*-models'.

The third possibility is that one may require the SO universe to contain sufficient subsets of the FO universe to satisfy the substitution schema (4). We shall call such structures (models) general structures (models). Plainly, every *-structure is a distinct general structure, but the converse does not hold. The properties of SOL when general structures are considered are not significantly differently from those of FOL, but striking differences appear when we restrict our attention to *-structures. We list a few interesting properties of SOL for possible models of this type--call it '*SOL'.

I. S^2 is Categorical

It is well-known that no first-order theory with any infinite models is categorical--that is, has all of its models isomorphic to one another. But let us consider S^2. Let $a_0, a_1, a_2, \ldots$ be the sequence of (necessarily distince and without other predecessors) denotations of the numericals of S^2: '0', 'S(0)', 'S(S(0))', etc., w.r.t. some model $M = \langle \underline{N}, \underline{C}, \ldots \rangle$ of S^2. Let $A = \{ a_i \mid i \in \omega \}$. Since $\underline{C} = \mathbb{P}(\underline{N})$ and $A \subseteq \underline{N}$, $A \in \underline{C}$. So A is within the range of the universal quantifier (P) in the induction axiom (1). Since $\underline{0} \in A$ and $x \in A \supset S(x) \in A$, all numbers are in A, i.e., $N = A$. Clearly we can show this for all *-models of S^2, so they are all isomorphic.[76]

II. *SOL is not Semantically Complete

We say that a system consisting of a class of logical axioms, inference rules and possible models is semantically complete if any sentence φ (of an appropriate language) can be proved from a given set of sentences Γ by means of the axioms and rules just in case the given sentence φ is true in every model of Γ. It is well-known that FOL is complete in this sense.[77]

It is an easy consequence of the result in I. above that *SOL cannot be semantically complete. The Godel-Rosser incompleteness theorem shows how to obtain a sentence φ of S^2 which is true in the standard interpretation of S^2, but unprovable by the usual rules and axioms. Since all *-models of S^2 are isomorphic, φ is true in all of them, even though it is unprovable. Hence *SOL is not semantically complete w.r.t. the usual axioms and rules.

This result may be generalized as follows: Given any effective set of axioms true in all *-structures (of some particular language type) and any effective set of sound and effective rules for SOL, *SOL is not semantically complete w.r.t. these rules and axioms. This remarkable result, that no acceptable set of rules and axioms can prove all the "semantic consequences" of every *SOL theory will be the basis of my critique of Kreisel's position on CH and SOL. That is, it can be shown--under "common sense" assumptions--that CH has the same truth value in all *-models of second-order ZF, and also that CH can be neither proved nor refuted in SO ZF either by the usual rules or by any other acceptable set of rules. We will return to this point below.

In addition to semantic incompleteness, other embarrassing properties of *SOL may be easily proved using the ideas of the previous few paragraphs. For example, there are extensions of S^2 which are consistent (i.e., no contradiction is a theorem) but which have no *-models.[79] We will mention a few such properties below in connection with SO ZF.

General Structures & SOL

The remarkable properties of *SOL disappear if we do not confine attention only to *-structures but admit all general structures. The categoricity result I. can no longer be obtained, for the proof of I. used the fact that $A \varepsilon \underline{C}$, but if $\underline{C} \neq \mathbb{P}(\underline{N})$, we have no guarantee of this. Hence, I. cannot be proved by this method. In fact, one can show that I. is simply false for general models, because Henkin[78] has shown that SOL is complete for general models, so the result of II. is false for such structures, and we have shown that I. implies II.

Second-Order Set Theory

Having considered some basic properties of SOL, it is now appropriate to describe second-order ZF and state the relevant results about it.

As before, we will consider only one-place quantified predicates, and we will have 'ε' as the only non-logical constant.[80] We call ZF^2 the theory stated in such a language whose axioms are the usual first-order axioms of ZF with Replacement and Foundation replaced by:

Found: $(P)((\exists x)Px \supset (\exists x)(Px \;\&\; (y)(y \varepsilon x \supset {\sim}Py))$

and the translation into primitive notation of:

Repl: $(P)((x)(y)(z)(P(\langle x, y\rangle) \;\&\; P(\langle x, z\rangle) \supset y = z) \supset$
$(x)(\exists y)(z)(z \varepsilon y \equiv (\exists u)(u \varepsilon x \;\&\; P(\langle u, z\rangle))))$.[81]

For convenience, we may also state the axiom of Separation:

Separation: $(P)(x)(\exists y)(z)(z \, \varepsilon \, y \equiv z \, \varepsilon \, x \ \& \ Pz)$

We also have available the logical axioms for SOL, including the substitution schema (4) given earlier. With the aid of (4), it is easy to show that all instances of first-order Foundation and Replacement are theorems of ZF^2.

A possible model of ZF^2 is a triple $M = \langle U, B, R \rangle$, where $B \subseteq \mathbb{P}(U)$, $R \subseteq U \times U$, and M satisfies the logical axioms of ZF^2. If $B = \mathbb{P}(U)$, we shall speak of *-structures and models, as before, and use the term '$*ZF^2$' when we are considering only models in this sense.

<u>Comparing VBI and ZF^2</u>

In an earlier section, we suggested that S^2 and $S^{2'}$ ought to be regarded as variant formulations of the same theory, despite the fact that $S^{2'}$ is technically a first-order theory. In this section, we describe a similar relation which holds between ZF^2 and VBI. Let E be the following formula of ZF^2:

$$(F)(G)((\mathbf{x})(F(\mathbf{x}) \equiv G(\mathbf{x})) \supset F = G).$$

E is an axiom of extensionality for predicate variables; it is plainly true in all general or *-structures, so the theory $ZF^2 + E$ is consistent if ZF^2 is. The precise relation between VBI and ZF^2 is given by the following proposition, which is proved in appendix C:

<u>Proposition:</u> If φ is a formula of VBI and φ^* is the formula of ZF^2 which results from replacing every atom of φ of the form $x \, \varepsilon \, X$ by the predicate variable $X(x)$, then VBI $\vdash \varphi$ iff $ZF^2 + E \vdash \varphi^*$.

Since CH contains no class or predicate variables, CH = CH*. It is thus an easy corollary of Tharp's and Solovay's independence results for VBI that CH is not decided in ZF^2, since it is not decided in the extension ZF^2+E.

I know of no mention of the above proposition in print, but the proof is simply quantification theory, and it would be surprising if Kreisel were unaware of it. Clearly, he has another sort of result in mind, which we describe in the following section.

A "Categoricity" Result for ZF^2

'Categoricity' appears in shudder quotes in the title of this section because it is unlikely that the *-models of ZF^2 actually fall into a single isomorphism class. In fact, if there is a measurable cardinal κ, there are at least κ non-isomorphic *-models of ZF^2.[81a] We can prove, however, that all the *-models of ZF^2 are closely related in a sense which will be described below.

First, we note that all *-models are well-founded. The first-order theory ZF has, or course, an axiom which "says" that the ϵ-relation is well-founded, but there are models of that theory--which of course satisfy this axiom--which are not well-founded.[82] It is plain, however, that a *-structure satisfies the S0 foundation axiom iff it is actually well-founded. Using this fact, and the so-called Isomorphism Theorem[83] for first-order theories, we may infer that all *-models of ZF^2 are isomorphic to an ϵ-model, that is, a model of the form $\langle U, B, R\rangle$, where U consists only of sets, and R is the restriction of the ϵ-relation to U.

By a more involved argument, which we outline in Appendix D, we can give a much more detailed characterization of the *-models. Before stating this result, we must make a technical reservation. It is entirely possible that there are no *-models of ZF^2 at all, even if the axioms of this theory are true under the intended interpretation, since the collection of all sets cannot itself be a set, hence not a model of anything. We will argue in the next chapter that this technical point has more than technical significance, but for the present, we will ignore this point and refer to the CTS itself as a model. We can now state the promised result:

<u>Proposition:</u> Every *-model of ZF^2 is isomorphic to the entire CTS or to a natural model with first-order universe $R(\kappa)$, where κ is inaccessible.

It follows immediately from this proposition that if there are no inaccessibles, then $*ZF^2$ is categorical. We may also state a related result for Z^2, the theory obtained from ZF^2 by omitting the replacement axiom.

<u>Proposition:</u> Every *-model of Z^2 is isomorphic either to the entire CTS or to a natural model with first-order universe $R(\alpha)$, where α is a limit ordinal greater than ω.

From this second proposition, we obtain an independence proof of the second-order replacement axiom from Z^2, since there are non-inaccessibles which are limit ordinals greater than ω, e.g., $\omega + \omega$.

Either proposition illustrates the mathematical facts on which Kreisel bases his case that CH is "decided" in ZF^2 (or Z^2), for the CH concerns only sets in $R(\omega+3)$, the initial portion of the CTS out to $\omega+3$. Since all *-models of either theory (are isomorphic to models which) contain these sets as their initial portion, the CH has the same truth value in all of them.[84] Thus, unless we have some cause to challenge the "categoricity" argument for $*ZF^2$, we have proved that CH is determined, although it is not decided in ZF^2.

As with the categoricity proof for S^2, however, the two propositions stated above fail to hold if we include consideration of general models. It is not even the case that such models must be well-founded. Even if well-foundedness could be quaranteed, the rest of the argument would not succeed, as the reader may see by examining the proof sketch in Appendix D.

<u>Significance of the "Categoricity" of $*ZF^2$</u>

At first glance, the results of the previous two sections provide an inviting solution to the problem of this thesis. That is, we have been able to show that despite the fact that CH is undecided in ZF^2, it is determined provided we accept one natural sense of 'model' for SOL. In the view of this writer, however, this "solution" is defective on two counts:

First, I will argue that $*ZF^2$ is a system of questionable value as a practical foundation for set theory, in that it conceals significant features more clearly presented in, say VBI.

Second, I will offer an argument to the same conclusion as Kreisel's --that CH is determined--using a proper subset of his premises, one which I believe to be far less problematic.

In order to deal properly with the first objection, I propose to consider briefly Kreisel's reasons for profering *SOL, in general and in set theory. Later, we will return to the second point.

Claimed Advantages of *SOL

Although Kreisel gives no compact defence of *SOL, he makes three main claims in various articles:[85] (1) *SOL is more natural to the ordinary mathematician and to earlier writers on foundations: (2) SO axioms provide the evidence for FO schemata of accepted theories; (3) second-order formulations are heuristically fruitful in finding interesting theories which may ultimately be cast in first-order form. We will deal with these points in order.

As for (1), Kreisel is plainly right in claiming that some earlier researchers intended to give categorical formulations of their theories.[86] Dedekind gives a categoricity proof for his formulation of artithmetic, which, on account of the Skolem-Lowenheim Theorem, would be impossible if the underlying logic were first-order.[87] Similarly, the first-order formulation of Peano's original five axioms, stated in terms of "zero" and "successor", is not sufficient to prove the existence of "plus" and "times" functions.[88] One must either include these two notions as primitives, add some simple set theory--or, employ second-order logic.

Kreisel cites one further, more problematic case, claiming that Zermelo himself employed SOL for his "categoricity" results on ZF.[89] While it is not unreasonable to read SOL into Zermelo's paper,[90] it does need to be read in; Zermelo himself specifies no underlying logic. Kreisel's interpretation of Zermelo's argument has not gone unchallenged. Shepherdson, in his refinement of Zermelo's results,[91] maintained that Zermelo was simply mistaken, and set out to correct the "logical shortcomings" of his proof and produce a related first-order theorem.[92] A reasonable guess as to Kreisel's reasons for reading Zermelo's proof as second-order is that otherwise his result is false.

Kreisel's conclusions from such cases as these is that categoricity is a (necessary) adequacy condition for "set theoretic foundations as <u>originally</u> intended" by earlier logicians.[93] Apparently, Kreisel agrees that this "original intention" was correct, for he regards the fact that no infinite mathematical structure may be categorically characterized by a first-order theory as "establishing the <u>inadequacy</u> of 'first-order' foundations".[94]

Besides faithfulness to the hopes of earlier investigators, and the possibility of categoricity, Kreisel suggests one other sense in which SOL is more natural. He argues that mathematical practice proofs of the kind given by, say, Bourbaki, are best understood as having an (implicit) SOL logic.[95] Several other writers have also expressed the view that SOL is closer to mathematical practice,[96] but Kreisel's own argument is extremely tenuous. He cites Bourbaki's practice of extreme care in stating hypotheses, while making no mention of the set comprehension axioms needed for the proof. This procedure would be "extremely unscientific", he claims, unless second-order logic is intended.[97]

Such an argument leaves us free to conclude that even Bourbaki may be somewhat unscientific, but it is more plausible that they simply have no interest in the detailed formulation of the intuitive set theory in which they work. This writer sees no reason to regard such an attitude as unscientific, but more importantly, Kreisel himself is concerned to defend "informal rigour" against "positivisitic" or "doctrinare" challenges.[98] If refinements do need to be read into even Bourbaki's carful proofs, I see no reason to read in SOL rather than set theory.

As a final defense of the naturalness of SOL, Kreisel argues that the notion of validity in the sense of truth in all *-structures corresponds to "intuitive validity". His argument to this effect is a rather self-defeating analogy to the first-order case. For that case, he makes use of the Godel completeness theorem to argue that first-order sentences are "intuitively valid" iff they are valid in the usual model-theoretic sense, and he states that "one would expect"[99] such a result for SO sentences as well. Kreisel plainly does expect such a result,[100] but his reasons are not entirely clear. It may be that he is relying on the fact that the definition of SO consequence "uses exactly the same basic notions as that of first order consequence."[101] In any case, his first-order argument makes use of the completeness theorem, which is false for *-structures in SOL, and Kreisel gives no indication of how a second-order argument may be constructed which avoids this embarassing fact.

The outcome of this series of arguments (of Kreisel's) seems to this writer to be inconclusive at best. The intentions of the earlier writers mentioned may not be realizable. Categoricity is a nice property of theories, but we have seen how categoricity arguments for theories formulated in SOL lead directly to incompleteness results. Even if we could establish the naturalness of SOL formulations of mathematical practice proofs, it is not clear that this is important. Kreisel's remarks about intuitive validity seem to count against, not for, *SOL. We turn next to evidential and heuristic arguments for SOL.

Evidential Superiority of *SOL

Kreisel's view on this matter is most clearly put in connection with the induction axiom (SOL) or schema (FOL) in arithmetic: "A moments reflection shows that the evidence for the first order axiom schema derives from the second order schema [sic.]"[102]

Kreisel does not elaborate an argument, but his intent here is plain enough. Ones reason for believing that all instances of the first-order schema are true is that one believes that induction works for all properties, hence in particular, the ones defined by formulas in the schema. This is a sensibl e enough view, and I am not concerned to dispute it. I only wish to note that whatever force this argument has for the SO induction axiom of S^2, it also has for the set theoretic formulation in $S^{2'}$, which, we have noted, is a first-order theory. $S^{2'}$ even has the advantage of offering at least a minimal characterization of the sets (or properties) involved.

The point may be put concisely this way. A SO axiom may be "evidentially superior" to a schema of its first order instances without being superior in this respect to other first-order formulations of "the same" axiom with set (or class) variables. In the particular case of set theory, it is plainly absurd to maintain, say, that ZF^2 is "evidentially superior" to VBI.

Heuristic Value of SOL

Kreisel offers two specific examples of the heuristic value of SOL, and one additional one is suggested by his remarks. We will not consider the first example, because he himself notes that "quite simple reflection principles" are superior in that case for the "discovery of new axioms".[103]

The second example is the argument that CH is "decided" in *SOL which we have already cited. Presumably, what *SOL aides us in discovering by this procedure is that CH has a truth value, but at this point, we are still in the process of evaluating this "discovery". In another context,[104] Kreisel suggests that the SO categoricity results suggests that properties other than those definable in the usual (FO) language of set theory should be investigated in new attacks on the continuum problem. As we have seen, a number of people regard this approach as fruitful, some no doubt inspired by the SOL categoricity.[105]

The third case, suggested by Kreisel's comments,[106] concerns the previously mentioned case of the plus and times functions for arithmetic. The existence of these functions may be proved in SO arithmetic formulated in the meager language containing only "zero" and "successor" as non-logical constants. But the first-order theory in this language is so weak that it is decidable.[107] As we mentioned in the earlier discussion, a satisfactory FO theory may be obtained without set theory only by adding plus and times as primitives. Kreisel seems to believe that such additional technical complexity of the first-order axioms make it easier to discover the SO ones first, and then concoct the first-order theory:

> The general principle which distinguishes the discovery of the basic axiomatic systems here considered [arithmetic, analysis, and set theory] from technical ones like the axioms of group theory is this:
> We have a second order (categorical) axiomatization of a mathematical structure, and then pass to a first order system either (i) by using a schema or (ii) by using a many sorted calculus and giving explicit closure conditions for the "sets".[108]

Taken as an historical thesis, this view is not unreasonable, but the question we must ask is whether following such a scheme may help solve the continuum problem. If the course we have considered in the bulk of this chapter and which Kreisel endorses[109]--better characterization of definite properties--is to lead to a solution, *SOL is unlikely to help. The reason for this is well put by Mostowski, who maintains that

> ... we obtain no additional information on the status of the continuum problem if we pass from the first order logic to the second order one.... We need axioms which will characterize the notion of an arbitrary predicate. The solution of the continuum problem depends essentially on the choice of these axioms. But is the problem of their choice in essence not the same as the problem of finding suitable axioms for the notion of a set?[110]

The proof-theoretic equilavence of VBI and ZF^2 gives clear support to Mostowski's statement that "we obtain no additional information" about CH in the passage from FOL to SOL. The second-order logic which interests Kreisel, *SOL, is <u>worse</u> off than FOL, in the sense that rather than providing additional information about the CH (or anything else), crucial restrictive conditions are <u>concealed</u> in the definition of *-models. As we have noted, the axioms of ZF^2 (or VBI) which concern class existence, the substitution schema (or class comprehension schema, respectively)are <u>not</u> sufficient to replace the condition on *-structures that the SO universe be the powerset of the FO universe. Without such a condition, the "categoricity" result for ZF^2 evaporates. But if the condition is justifiably imposed, an exactly parallel one could perfectly well be stated for VBI or even VBG. That is, one could require that the only models for VBG to be considered are those isomorphic to an ϵ-model M in which the "classes" (elements X of M for which $M \models (\exists Y)(Y \in X))$ are exactly the actual subclasses of the universe of M. Such models are called "supercomplete", and VBG and VBI are "categorical" w.r.t. supercomplete models in exactly the same sense in which $*ZF^2$ is "categorical".[111]

Mostowski states--and this writer agrees-that what one needs are principles, axiomatically presented, which better characterize all--or more of--the actual subclasses of an ϵ-model. It is absolutely no help in doing this to make "*-ness" or supercompleteness part of the definition of 'possible model'. If anything, choosing a SOL formulation, which makes the class existence schemata part of the logical apparatus of the theory obscures this effort, because the problem to be solved is, as Mostowski notes, "in essence" a set-theoretic one. Such interesting new attempts to characterize classes as Takeuti's notion of "nodal class of ordinals" could be stated in SOL terms, but this is plainly less clear than adding to the VBI comprehension schema. The clearly warranted conclusion is that as far as heuristic value for the "definite property" problem goes, SOL (with or without the '*') is inferior to first-order theories.

Further Disadvantages of $*ZF^2$

The conclusion I wish to draw from the disscussion of the previous few sections is that the advantages of SOL, if any, are slight, and in crucial matters related to the continuum problem, definitely outweighted by disadvantages. Further disadvantages of *SOL in general were cited in the case of S^2, specifically the lack of a completeness theorem and consequent undesirable properties. We may cite as an example the existence of consistent extensions of S^2 with no *-models [112]

Similar examples could be cited for set theory. Several have been collected in a paper by A. Levy,[113] who attributes them to Mostowski.[114] Levy's examples turn on the non-equivalence of predicate quantified statements and the schemata of their instances.

For example, he shows that there is a certain schema of theorems of ZF^2, $\sim\Psi(\varphi)$, which, when replaced by the predicate quantified statement $(P) \sim\Psi(P)$, yields a consistent theory with no $*$-models. As one might expect, this result turns on a trick about truth definitions. $\sim\Psi(P)$ says that P is not a truth set for ZF^2, and by Tarski's Theorem, we have $\sim\Psi(\varphi)$, for all φ. $(P) \sim\Psi(P)$ is actually false, however, because the truth set for ZF^2 must actually be contained in any $*$-model of ZF^2, even though it is not defined by any φ. A similar construction yields a consistent extension of ZF^2 with more $*$-models than ZF^2.[115]

Levy concludes that these examples "show the inadequacy for general use of the notion of standard model introduced in § 2 [i. e., $*$-models]".[116] One might reply, however, that the examples simply illustrate the fact, repeatedly noted by Kreisel,[117] that SO sentences need not be equivalent to the schemata of their instances. Rather than showing the unsuitability of the $*$-structure notion for "general use", Levy's examples merely add to our list of unhappy features of *SOL.[118]

A Better Argument that CH is Determined

At the outset of our critique of Kreisel's claims for SOL, I promised a simpler argument that CH is determined. In this section, we deliver on that promise.

First, we must recall that all our discussion of *SOL, including the technical arguments of Appendix D, have rested on a proposition perhaps too obvious and fundamental to be called a premise.

This proposition is that, contrary to formalist views, the terms 'set', 'is a member of', and the like, have a settled interpretation, and further that this interpretation is that given by a single mathematical structure, the CTS. We explicitly noted such an "assumption" in our earlier discussion of natural models,[118a] and Kreisel introduces his discussion of SOL with a similar statement: "This section takes the precise notion of set (in the sense of the cumulative type structure of Zermelo) . . . as starting point. . ."[119] Using this "assumption", we can put our argument that CH is determined as follows:

Consider the initial portion of the CTS, $R(\omega+3)$. Since CH concerns only this portion of the CTS,[119a] if we let $M = \langle R(\omega+3), \in \restriction R(\omega+3) \rangle$ then we have the following:

> There is a unique intended interpretation of the terms of set theory and a structure M such that:
>
> (1) The CH is true under the intended interpretation iff $M \models CH$, and
>
> (2) The CH is false under the intended interpretation iff $M \models \sim CH$, and
>
> (3) $M \models CH$ or $M \models \sim CH$.
>
> ---
>
> (4) Therefore, there is a unique intended interpretation of the terms of set theory such that CH is either true or false under that interpretation

In the (three part) premise of (A), (1) and (2) simply express (for CH) the correspondence between truth under the intended interpretation and the technical notion of satisfaction in M. (3) is a trivial theorem of model theory. It is plain that the argument (A) is valid, and given principles Kreisel accepts, it is also sound. Consequently, Kreisel's long detour thru the morass of *SOL is simply mathematical obfuscation of a philosophical triviality.

The Critical "Assumption"

The reader should be warned that by using the term 'assumption', I do not mean to imply that the proposition in question is doubtful or unsupported. I do mean to suggest that this proposition deserves examination. One reason for this is that it has been repeatedly denied by recent writers on the significance of the independence proofs (not all of them formalists), and denied on the grounds of the independence of CH in particular. It is this view which we will examine in the next chapter. Our final conclusion will be that (1) - (4) does really settle the question of whether CH is determined, provided only that a minimal realism is adopted.

CHAPTER II

REALISM AND THE CONTINUUM QUESTION

Setting the Problem

At the end of the last chapter, I outlined an argument to the effect that CH is determined, which is restated here:

(A)

> There is a unique intended interpretation of the terms of set theory and a model M given by this interpretation such that:
> (1) The CH is true under the intended interpretation iff $M \models CH$, and
> (2) The CH is false under the intended interpretation iff $M \models \sim CH$, and
> (3) $M \models CH$ or $M \models \sim CH$.
>
> ---
>
> (4) Therefore, there is a unique intended interpretation of set theory according to which the CH is either true or false.

The single premise of the argument (A) has the form of the existential generalization of the conjunction of the three clauses (1) - (3). In the discussion which follows, we will occasionally speak loosely and refer to (1), (2) and (3) as premises of (A). Argument (A) is easily shown to be valid by quantification theory, and our examination of its soundness will serve to organize the discussion of this chapter. In a later section, we will consider another argument that CH is determined whose premises do not have the explicitly existential character of those of (A), and will there justify our concentration on (A).

It is plain that (A) will be totally unacceptable from the point of view of several widely discussed views in the philosophy of mathematics. In the introduction, we outlined formalist views which either reject the notion of truth in mathematics altogether, or identify it with provability in a preferred formal system. For the first of these two views, all the premises of (A) are nonsense. For the second, the satisfaction relation "M $\models$" must be replaced by assertions about provability in some formal system, in which case (3) will be false.

Likewise, (A) cannot be accepted by the Intuitionists of the Dutch school, if only for the reason that (3) would be for them an unjustified appeal to the principle of the excluded middle.

The fact that our argument is unacceptable on either intuitionist or formalist views does not, however, oblidge us to give a general evaluation of these doctrines in order to continue our discussion of the main question, that of whether CH is determined. We need not do this because in one way or another, this question is already settled from the point of view of these two positions.

From a formalist point of view, the question is settled in the negative by the technical results outlined in the previous chapter. For intuitionists, moreover, the question does not even arise, because they reject entirely the greater part of set theory, in particular that part necessary to <u>state</u> the CH, namely, cardinal arithmetic beyond $\aleph_0$.[1]

The positions that remain to be considered, and which may be reasonably be said to be assumed both in Kreisel's argument and in (A), are those which are generally call "realist", or, perjoratively, "platonist". The realism implicit in (A) has two aspects: first the assertion that there exists a unique intended interpretation of set theory, and second, that for CH, truth under that interpretation coincides with satisfaction in a certain mathematical structure (or group of related structures) M.

The project of the present chapter is to investigate the soundness of the argument (A) , assuming some form of realism. We need just enough realism to make good sense both of the assertion that there is a unique intended interpretation of set theory (and the structure or structures M) and of the notion of satisfaction of CH in M, but we also need enough leeway in our assumptions about realism to be able to challenge the assertion that there is indeed a unique such interpretation. Just why this leeway is needed will be seen below, when examine the arguments that there may be more than one candidate for the "intended interpretation" of set theory, so that the premise of (A) might be false.

There are a number of philosophers of this and recent generations who have been concerned with mathematical truth and existence, and who have called themselves realists. The ideal procedure for our present discussion would be to simply take one of these announced positions or the common part of several of them and state it as an assumption on which our examination of (A) will rest. I believe, however, that the discussion below will show that this "ideal" procedure will not work in the present case, because none of the usual views are both sufficiently clear and reasonably plausible so as to serve as a basis for our investigation. Both the advocates and the opponents of such views describe them in language which is vague, impressionistic, metaphorical, or down-right paradoxical. As a result, it will be necessary to defer our discussion of the argument (A) until two subsidiary matters are taken up. The first is a brief review of realist positions in mathematics, and the second, which is a natural outgrowth of the first, is a review of theories of mathematical truth. After these two digression, I will present my own version of a minimal realist position which I hope will be both clear and plausible and which will not beg the questions which we must ask about the premises of (A).

Realist positions in Mathematics:

Several writers whose views are discussed elsewhere in this thesis are characterized by themselves or by others as realist. K. Godel, for example, has stated his view that

> The set-theoretical concepts and theorems describe some well-determined reality, in which Cantor's conjecture must be either true or false.[2]

In an earlier statement, Godel expresses a similar idea:

> Classes and concepts may, however, also be conceived as real objects, namely classes as "pluralities of things" or as structures consisting of a plurality of things and concepts as the properties and relations of things existing independently of our definitions and constructions.
>
> It seems to be that the assumption of such objects is quite as legitimate as the assumption of physical bodies and there is quite as much reason to believe in their existence. They are in the same sense necessary to obtain a satisfactory system of mathematics as physical bodies are necessary for a satisfactory theory of our sense perceptions and in both cases it is impossible to interpret the propositions one wants to assert about these entities as propositions about the "data", i. e., in the latter case the actually occuring sense perceptions.[3]

Other phrases used by Godel to express the same view include 'exist objectively'[4], 'objective existence'[5], 'real objects'[6], and 'real content which cannot be explained away'[7]. Godel also quotes (one of) Russell's view(s) :

> Logic [including set theory--TSW] is concerned with the real world just as truly as zoology, although in more abstract and general features.[8]

This remark of Russell's recalls a much earlier statement of a realist view by Joseph Fourier:

> The analytical equations ... are not restricted to those properties of figures, and to those properties which are the object of rational mechanics; they extend to all general phenomena. There cannot be a language ... more worthy to express the invariable relations of natural things. Considered from this point of view, mathematical analysis is as extensive as nature itself. ... It brings together the phenomena the most diverse and discovers the hidden analogies which unite them.[9]

R. Smullyan, who styles his own views as realist, closely follows Godel's language. He states his position in the following terms:

> We can describe the realist view point as follows: There is a well-defined mathematical reality of sets, and in this reality, the continuum hypothesis is definitely either true or false.[10]

As a final example of a clearly realist position, we must deal briefly with Frege's ontological views. He divides the entities within the subject matter of mathematics into two kinds, objects and functions:

> I count as objects everything that is not a function, for example, numbers, truth-values, and ... courses-of-values [classes].[11]

Clearly, Frege counts sets, the entities which interest us here, as objects. If we could find in Frege's account (a) a clear characterization of mathematical entities and (b) a clear distinction between objects and functions, it might aid us in explaining realism in set theory. Unfortunately, as in the case of the philosophers discussed earlier, we do not find these features in Frege's work, but we do find suggestive and conscientiously pursued attempts.

As for (a) , Frege develops his own views by contrast to and criticism of two rival positions. One is formalism, the view which "regards signs as the subject matter of this science [mathematics]".[11a] Frege regards himself as having "definitely refuted" this view;[11b] the subject matter of arithmetic, for example, is not numerals, but "what they stand for",[11c] namely numbers. One reason why this was not obvious to formalists was the "very widespread" tendency "not to recognize as an object anything that cannot be perceived by means of the senses".[11d] But this is simply a mistake; one must distinguish what is "objective" from what is

> handlable or spacial or actual [wirklich]. The axis of the earth is objective . . . but I should not call [it] actual in the way the earth itself is so.[11e]

The second sort of view which Frege is concerned to refute is the notion that mathematical entities are ideas:

> If number were an idea, then arithmetic would be psychology. But arithmetic is no more psychology than, say, astronomy is. Astronomy is concerned, not with the ideas of the planets, but with the planets themselves, and by the same token the objects of arithmetic are not ideas either.[11f]

Frege's own view on the nature of mathematical objects may best be put negatively, by contrast to the two views that the subject matter of mathematics consists of signs and that it consists of the products of "a psychological process".[11g] Mathematical objects are not either of these, but they are not physical either. As he says of "thoughts" (propositions), which are in the same ontological boat as mathematical objects, "[they] are by no means unreal [unwirklich], but their reality is quite a different kind from that of [physical] things."[15]

Most of the above discussion mentions objects only, but it applies equally to functions. For example, Frege remarks that although he takes numbers to be objects, he could have taken them to be concepts (functions whose values are truth values).[11h] It is clear that the functions in question are, like mathematical objects, neither physical nor mental, but still "objective".

On Frege's view, neither 'object' nor 'function' can be defined,[11i] both being simple to "admit of logical analysis".[11j] The best that can be done is to give "hints" of what is meant by this distinction.[11k] He provides two sorts of hints, one a metaphor, and the second a clearer grammatical criterion. The metaphor is given a typical expression in the following passage:

> I am concerned to show that the argument does not belong with the function, but goes together with the function to make up a complete whole; for the function by itself must be called incomplete, in need of supplementation, or 'unsaturated'. And in this respect functions differ fundamentally from numbers.[11ℓ]

The idea of the metaphor of "unsaturatedness" is that a function is something such that when it is "completed" with one or more objects (the arguments), the result is an object (the value of the function).[12] Frege gives some indication that he had the notion of a function as a set of ordered pairs, but he regarded his own more arcane notion as "logically prior".[11m]

Besides the metaphorical description of a function, Frege gives a grammatical criterion:

> A concept is the reference of a predicate; an object is something that can never be the whole reference of a predicate, but can be the reference of a subject.[11n]

This criterion also has shortcomings, however. It appears to work only for certain functions (i. e., concepts), and it admits of some exceptions.[11o] Further, since Frege maintains that "the singular definite article always indicates an object",[11p] the reference of the subject of the sentence 'the concept man is not empty' is not a concept, but an object which "represents" the concept.[11q] What the relation is between this object and the concept represented is very obscure. Generally speaking, the obscurity of the object function distinction is the subject of a great deal of critical complaint by Frege's commentators.[13]

****

As a summary of the views we have sketched here, we can list three key characteristics: (a) mathematical truths are "about" "mathematical objects"; (b) these objects are "real"; and (c) they are not mental. Frege is particularly explicit about this last clause, but is clear from passages not quoted that all the other authors cited also subscribe to it.

Quasi-Realist Views

Several authors whose views we will be oblidged to discuss bear some similarity to those reviewed in the previous section, but differ in one or more important particulars from them.

Kreisel, for example, distinguishes three sorts of "realism", but he discusses them is a sufficiently hypothetical tone that it is unclear which of these he may accept. He says, for example, that

> The contact [of the analysis of mathematical experience] with mathematical realism is, or course, the assumption that there are basic elements with the properties assumed in the analysis, that is, the existential assumptions of set theory are valid.[16]

Kreisel clearly accepts this much "realism"; he later distinguishes three views, "strong", "weak", and "strict" forms of realism:

> The realist position is here taken in its strong form, namely as involving the existence of sets of high ordinal, and not merely as involving the existence of some mathematical objects [weak realism].[17]

"Strict" realism he takes to be the view that

> all mathematical notions are built up from the notion of set by means of logical definitions in the language of pure set theory ($\in$).[18]

This view he regards as implausible.

The way in which Kreisel's version of realism is clearly different from those of the previously cited authors is that he appears uncertain whether mathematical objects may not be mental, or, as he says, "objects not external to ourselves".[19] He suggests that analysis of the nature of "mathematical evidence" may make the view that mathematical objects are indeed "not external to ourselves" more plausible.[20] It appears, then that Kreisel rejects characteristic (c) of realism as listed in the previous section. In traditional terminology, his view is therefore closer to "conceptualism" than to "realism".

Another author with a similar view is P. Bernays. Bernays claims to detect a "tendency ... which consists in viewing the objects [of mathematics] as cut off from all links with the reflecting subject".[21] He endorses this tendency to a limited extent, because he believes that such a position is necessary to justify the excluded middle in mathematical reasoning.[22] The position he prefers he calls "restricted platonism"

> which does not claim to be more than, so to speak, an ideal projection of a domain of thought. But the matter has not rested there. Several mathematicians and philosophers interpret the methods of platonism in the sense of conceptual realism, postulating the existence of a world of ideal objects containing all the objects and relations of mathematics. It is this absolute platonism which has been shown untenable by the antinomies, particularly by those surrounding the Russell-Zermelo paradox.[23]

We will have occasion to discuss Bernays' argument in this last sentence in a later section.

Finally, we must consider Cantor's position, which, paradoxically, amounts to an argument that conceptualist and realist positions amount to the same. There are, he says, two senses of 'actuality'; in the first sense,

> We may regard the whole numbers as 'actual' in so far as they, on the ground of definitions, take a perfectly determined place in our understanding, are clearly distinguished from all other constituitents of our thought, stand in definite relations to them, and thus modify, in a definite way, the substance of our mind.[24]

In the second sense, we may ascribe "actuality" to our conceptions

> insofar as they must be held to be an expression or an image of processes and relations in the outer world, as distinguished from the intellect.[25]

Cantor held, however, that "because of the unity of the All, to which we ourselves belong" the first kind of actuality always implies the second.[26] It would be pointless, even if it were possible, to clearly explain Cantor's metaphysical argument here, but the outcome of the argument is not hard to see: in mathematics, we need only consider the first sort of "actuality". For example,

> in the introduction of new numbers, it is only obligatory to give such definitions of them as will afford them such a definiteness, and, under certain circumstances, such a relation to the older [natural] numbers, as permits them to be distinguished from one another in given cases. As soon as a number satisfies all these conditions, it can and must be considered as existent and real in mathematics. In this I see the grounds on which we must regard the rational, irrational, and complex number as just as existent as the positive integers.[26a]

Evaluation of Realist & Quasi-Realist Views

In an earlier section, we introducted our discussion of realist position, and characterized them as "vague, impressionistic, metaphorical or paradoxical". I think that our subsequent summaries of positions, both "realist" and "quasi-realist", bear out this perjorative description. I do not mean to imply, however, that any of the characterizations are senseless, that the reader or this writer has no idea what is meant by them, or that the remarks have no value as methodological guides for the development of various strains in mathematics. The philosophical remarks of intuitionist mathematicians, for example, are notoriously obscure,[27] but this has not prevented the development of a substantial body of intuitionist mathematics.

I do wish to claim, as previously announced, that none of the positions reviewed is both plausible and sufficiently clear to provide a basis for evaluation of the argument (A). It is simply not evident what we may infer from the statement that sets are "real objects" or that there is a "well-defined mathematical reality of sets". It will be our project in the next few sections to attempt to give a minimal account of realism which will serve our purposes in evaluating (A). We will do this by formulating realism in terms of the notion of truth, and then defending a notion of truth with properties we can use in evaluating (A).

Minimal Realism

We will formulate our version of realism only for set theory; we state it as follows:

(B) (a) Most of the statements of set theory (including existential ones) which secure wide acceptance among mathematicians acquainted with set theory, in particular most of the axioms and theorems of ZF, are true under at least one natural interpretation, and (b) these statements need not be reinterpreted as facons de parler in order to be regarded as true.

At this point, it may well be unclear why we have chosen to call position (B) realism; that will be clearer after we have discussed theories of mathematical truth. It is clear, however, that (B) is a reasonable assumption on which to base a discussion of whether CH has a truth value, for (B) simply states that most of the statements for which we have proofs acceptable by current standards are true. If this is not taken as a starting point, then it is idle to consider whether statements which are known to be unprovable (according to these same standards) are true.

Although (b) is listed separately, I believe that discussion below of natural interpretations will show that it is already implied by the "at least one natural interpretation" clause of (a). It will clarify matters, however, to give an illustration by way of explanation of (b).

In a 1905 paper,[28] Russell outlined three methods which might avoid the paradoxes of set theory, among which is a theory he called the "no classes theory". This method amounts to a translation procedure for eliminating any proported reference to sets, and so, Russell believed, developing a foundation for mathematics without the assumption that sets exist.[29] This is the sort of position we have sought to rule out by clause (b). Russell later developed his theory in some detail, but without the ontological economies he had hoped for.[30]

We now turn to a discussion of theories of truth in mathematics, so that we may show the connection between (B) and the views we have styled "realist" and then prepare for an evaluation of (A).

Theories of Truth in Mathematics

Loosely speaking, we will be discussing four theories of truth, although one of them is not an entire account of truth, but deals only with quantified sentences, and another (conventionalism) is only a loose collections of slogans. The remaining two theories are two versions of Tarski's ideas, one that of Tarski himself, and another based on his work.

Theory I: Conventionalism:

Empiricist writers of this century, especially before the influence of Tarski's ideas became widespread, maintained that truths of mathematics are such because they merely record rules "which govern the use of language".[31] Another formulation puts is this way:

> The validity of mathematics ... derives from the stipulations which determine the meaning of the mathematical concepts, and ... the propositions of mathematics are therefore essentially "true by definition."[32]

It is held to be a consequence of such views that the truths of mathematics need not be regarded as "truths about the world",[33] "say nothing about any actual thing",[34] and convey no "factual information."[35]

As to the ontological implications of such a view, various advocates disagree. The main attraction of conventionalist theories for empiricists has been that they seem to provide an account of mathematics which makes it possible to explain how one can come to know mathematical truths which is consistent with their general epistomological view. One might suppose that elementary statements, for example, might be knowable simply by reflection on the meaning of the terms they contain. This emphasis on the epistomological aspects of mathematics has tended to overshadow ontological questions. Does the truth of '2 + 2 = 4' imply that numbers exist? Some maintained that this is a typical "pseudo-question", while others answered 'no'.[36] The most sophistocated formulation is Carnap's; he maintained that one should answer either 'yes' (for trivial reasons), hold that "it is not a theoretical but a practical question, a matter of decision rather than assertion",[37] or say that it is a pseudo-question , depending on exactly how the question is meant.[38]

Happily, we need not decide just what sort of ontology fits best with conventionalist accounts of truth because we can simply reject those accounts. In doing so, we rely on the arguments of Quine,[39] who convincingly argues that no reasonable construal of conventionalism can be right even about logic.

Quine's argument may be summarized as follows: While it may not be necessary to a conventionalist thesis that someone has ever explicity laid down the conventions in question, we must be able to imagine that this be done:

> In dropping the attributes of deliberateness and explicitness from the notion of linguistic convention, we risk depriving the latter of any explanatory force and reducing it to an idle label. 38a

To see whether the making of deliberate and explicit conventions for the truth of logic could be possible, Quine traces out a program of convention making for the logical truths--those sentences whose truth depends (in a sense he makes precise) only on the occurences and arrangement of the basic logical vocabulary: 'not', 'if ... then', 'every'. The single condition of adequacy to be met by this (imaginary) process of convention-making is that the results conform to the present ordinary usage of the logical vocabulary, which we must temporarily assume we do not understand.

If there were only finitely many sentences about which conventions were to be made, the process would be simple. We would only need to run thru an appropriate list and tick off the ones to be made true so as to conform to ordinary usage. Unfortunately, we need to make conventions about infinitely many sentences, and since human beings cannot simply run thru infinite lists, we must do this by adopting by convention some finite number of principles which together determine the truth values of all the logical sentences. The crucial difficulty which arises here is that both in stating the principles and inferring particular sentences, we must use both the locial vocabulary whose meaning is to be fixed by the conventional assignment of truth values to the logical sentences, and the logical principles we are seeking to make true by convention. In Quine's words, "The difficulty is that if logic is to proceed mediately from conventions, logic is needed for inferring logic from conventions."[38b]

Thus, there is no non-vacuous sense of 'convention' according to which logic is true by convention. To this conclusion, we need only add two remarks: (a) conventionalists typically regard mathematics as part of logic;[40] and (b) any plausibility conventionalism may have for mathematics is certainly removed if it won't work for logic.

It may seem that we have neatly dispensed with conventionalism, but such is not quite the case, because although the particular formulations quoted earlier cannot be correct, there are formulations which make use of Tarski's ideas which are very close in spirit to the statements cited here. We will examine such a position shortly.

Tarski's Views

Just about all contemporary philosphical views on truth, as well as the mathematical theory of models, derive in some manner from Tarski's work. Several interpretations may be put on Tarski's ideas, and I will maintain that the version that Tarski himself accepted is not plausible. In discussing Tarski's views, I will rely heavily on the arguments of a recent paper by Hartry Field, "Tarski's Theory of Truth",[41] to which the reader is referred for a more detailed treatment.

As Field shows, Tarski's theory may be regarded as characterizing truth for a language L (which, for simplification, we may take to be a first-order one of the usual sort) in terms of the three additional semantic notions of (1) what an individual constant c_i refers to, (2) what a predicate (i.e., predicate letter) applies to, and (3) which n+1-tuples of objects fulfill an n-place function symbol.[42] For convenience, we will use the term 'primitive denotation' to cover all three of these notions. We will call T the Tarskian theory which explains truth in terms of primitive denotation.

Thus formulated, the interest which T holds for us depends on what account of primitive denotation we can supply. If we could produce a clear and explanatory account of primitive denotation for mathematics such that only things which "really exist" can be referred to, we would have made progress in giving a clear account of a realist point of view. We have no such account to offer in general or for mathematics but (in a later section) we will suggest a line enquiry which at least deserves to be followed up. First, however, we review Tarski's own view.

Tarski's Theory of Primitive Denotation

Since it was Tarski's objective to "not make use of any semantical term if I am now able previously to reduce it to other [non- semantic] concepts",[43] he offered a further characterization of referring, fulfillment and application exemplified by the following treatment of the case of reference:

(C) (e) (a) (e is a constant that refers to a iff (e is 'c_1' and a is $\overline{c_1}$) or (e is 'c_2' and a is $\overline{c_2}$) or . . .)

where 'c_1', 'c_2' . . . are all the individual constants of L, and $\overline{c_1}$, $\overline{c_2}$, . . . is a list of the translations of these constants into English (or any other language which we happen to be using).

Field emphasises the triviality of (C) as a "theory" of reference by comparing it will the following "theory" of valence in chemistry:

(D) (E) (n) (element E has valence n iff (E is potassium and n = + 1) or (E is sulphur and n= -2) or . . .)

where the '. . .' indicates that (D) is to be filled out with a complete list of the elements and their valences. Plainly, (D) is not a theory of valence, any more than (C) is a theory of reference for L. Both (C) and (D) do perform one function, however; they allow us to eliminate the therms 'refers to ' or 'has valence' from many, perhaps all, contexts, provided that the lists are correct and complete. (C) thus answers to Tarski's stated aim of not using a semantic term "if I am not able to previously reduce it to other concepts".

Field argues that (1) the translation list used in (C) for eliminating the term 'refers to', and the counterparts of (C) for 'fulfills' and 'applies to' are of no philosophical importance,[44] and (2) that such accounts of the notion of primitive denotation saddle Tarski's account with very considerable practical and methodological drawbacks.[45] To these conclusions, he attaches only the proviso that some *other* treatment of primitive denotation which is compatible with physicalism be possible.[46] As will be seen, I fully agree with this view.

Tarski's addition to T for the case of reference may be put in a somewhat more general form as follows: Translate the constants of L into some language you understand, say L'; let the translation of 'c' be 'glurg', and let 'swarp' be some term of L'. Then 'c' refers to swarp just in case glurg = swarp, where this identity is understood as a statement of L'. This may strike one a circular or at least unhelpful , for we started to define truth for L and ended with an account which depends on understanding statements of some other (and richer) language L'. Tarski's procedure is really a special case of this general method, for his translation is not a systematic or explanatory account, but simply a list. But is some truly explanatory account possible for some appropriate language L'? Certainly many philosophers think not.

Carnap, for example, is easily able to adapt the Tarskian account, T + (C), to his conventionalist framework. In his view, the choice of the language L' and the translation into it are just as much a matter of simple convenience as the choice of the language L of our initial interest. The evident triviality of (C) as an account of reference (which Carnap adopts) makes it possible for him to countenance semantic notions and still maintain that "the admissibility of entities of a certain type... as designata [i.e., as things referred to -- tsw] is reduced to the question of the acceptability of the linguistic framework for those entities",[47] and this "question of acceptibility" is a purely pragmatic one.[48]

Field believes that one important reason that Tarski's translation "theory" of reference has come to be accepted as that nontrivial theories of reference with which philosophers are familiar, such as Russell's business about "logically proper names", are patently absurd. Field holds out hope for some sort of causal theory of reference, such as that sketched by Kripke.[49] Field summarizes this view as:

> According to such [causal] theories, the facts that 'Cicero' denotes Cicero and that 'muon' applies to muons are to be explained in terms of certain kinds of causal networks between Cicero (muons) and our uses of 'Cicero' ('muon'): causal connections both of a social sort (the passing of the word 'Cicero' down to us from the original users of the name, or the passing of the work 'muon' to laymen from physicists) and of other sorts (the evidential causal connections that gave the original users of the name "access" to Cicero and gave physicists "access" to muons). I don't think that Kripke or anyone else thinks that <u>purely</u> causal theories of primitive denotation can be developed (even for proper names of past physical objects and for natural kind predicates); this however should not blind us to the fact that he has suggested a kind of factor involved in denotation that gives new hope to the idea of explaining the connection between language and the things it is about.[50]

It is just this hope which I share, but there is a special problem for such a "causal" account of reference for mathematics. In fact, it is widely maintained that a causal theory of reference for <u>mathematics</u> is absurd. The argument is roughly that from the point of view of realism, mathematical objects are objects, but <u>non-physical</u> ones. (Clearly this the point of view of the realist writers we have quoted such as Frege and Godel). But, so the argument runs, it is absurd to hold that non-physical objects could enter into causal relations.[51] Similar arguments have been given against causal theories of mathematical <u>knowledge:</u> one has to believe in magic to believe that people can stand in causal relations to mathematical objects.[52]

Plainly, there is something right about such arguments. If (a) mathematical objects are non-physical and (b) we cannot causally interact with non-physical objects, then (c) we cannot causally interact with mathematical objects. Realist view such as Frege's and Godel's are clearly committed to (a), and (b) sounds plausible in any case, so they are committed to (c) and causal theories of reference and knowledge are plainly impossible on their views. But outside of mathematics, we have at least a sketch of a causal theory of reference, and more than a sketch of a causal theory of knowledge[53a] which, in this writer's view, are the most promising accounts of these notions. No such promising accounts of knowledge and reference are available for the realist positions espoused by Frege and Godel, and the remarks these authors make on these subjects are either uninformative or really do sound like magic.[53b]

For other realist position of the sort suggested by Russell and Fourier, that mathematics deals with the most general properties of the world, the argument (a) - (c) does not clearly apply, however. For example, suppose that I come to know--by looking -- that there are three oranges in the bowl on my table. According to causal accounts of knowledge, I come to know this by interacting causally with something: the bowl of oranges or a state of affairs of which it is a part, or the fact that the bowl contains three oranges. It does not follow, of course, that I interact with the number three, and I am not sure exactly what should be said about the relation between the property and the number. If the Russell-Fourier suggestions can be followed out, it is far from obvious that we will be committed to (a). It this should seem far-fetched, I think it can be made to appear less so by comparison with an example from physics.

Mass is certainly a physical quantity, if we are to come to know anthing about it or refer to it, then on causal accounts of these notions, we must causally interact with it. But mass (or perhaps a measure of mass) is a relation between physical objects (or systems) and real (or rational) numbers--or so it is represented in formalizations of mechanics.[53c] Such an entity certainly <u>sounds</u> like an abstract object, but it is also a physical one. If we can causally interact with such entities, it is certainly not absurd that we can causally interact with the abstract objects of mathematics; such at least is the force of the present analogy.

Status of the Theory T:

I would be the first to admit that the remarks about reference to mathematical objects at the end of the last section suffer from the same deficiencies as those I pointed out for standard realist and quasi-realist accounts: vagueness, metaphorical phraseology, etc. It is not my object to try to convert these remarks into a clear theory, because I do not see now to do it. I believe, however, that the treatment we have suggested with prove sufficiently clear for some applications to be made later in this chapter, and to indicate that not-trivial theories of primitive denotation for mathematics within the frameowrk of a Tarskian account of truth may yet be possible.

For the present, it is enough to indicate what portion of Tarski's theory we will accept, namely T, the portion without the "list" accounts of primitive denotation, plus whatever account of primitive denotation we are able to provide when needed. We accept T for the following reason: it is the only theory in the field which is at all plausible. This is important for two reasons. First, we need to make it clear that truth corresponds to the technical notion of satisfaction--based on the Tarskian account--for clauses (1) and (2) of the argument (A). Second, if we are to show any connection between our statement of minimal realism (which is cast in terms of truth) and the ontological views of the previous sections, a Tarskian account of truth, supplemented by appropriate accounts of primitive denotation, seems to me to be the best and perhaps the only way to do this. Conventionalist accounts of truth seem to provide no clues about the ontological questions of mathematics: the remaining theory which we will discuss--substitutional quantification--seeks to be "ontologically neutral", even though I doubt that it succeeds.

To establish that T is indeed the most plausible, we appeal to the general good opinion that philosphers (and model theorists) have of it and to Field's discussion in particular. It remains to show that the other theories are unacceptable. We have already done this for conventionalism, and there remains only one view to be discussed. This is the so-called theory of substitutional quantification (SQ).[54] According to SQ, truth for all atomic sentences is assumed to be taken care of in some fashion. The peculuar feature of SQ is that it defines truth for quantified sentences not by means of the Tarskian notion of satisfaction, but as follows:

'(x)F(x)' is true iff 'F(t)' is true for every closed term 't'.

Truth for existential sentences is defined in a similar fashion. (It is assumed, of course, that the language in question contains countably many closed terms so that the above definition does not reduce to the propositional case by finite conjuction of the F(t)'s).

J. Wallace[55] has argued that even for the simple case of truth in arithmetic, any adequate definition of SQ permits a definition of satisfaction to be constructed, so that in some sense SQ presupposes the Tarskian account. In fact, however, much more conclusive considerations weigh against SQ. I have shown elsewhere[56] that for any first-order language for ZF which meets the requirements of SQ and is capable of any reasonable sort of translation into English, ZF can prove about itself that its own quantification is not substitutional. The idea of this proof is to formulate ZF in any of the appropriate languages and add a truth predicate 'True(x)' with axioms unobjectionable to an advocate of SQ. It is then easy to show that the resulting theory is consistent if ZF is, and that it contains a sentence F(x) not containing the truth predicate, such that " '$\sim$(x)F(x)' is true" and "for every closed term t, 'F(t)' is true" are both theorems.

Reference, fulfillment and application in model theory

Despite our suggestions that other accounts of primitive denotation than the "list" translation are possible, the usual practice in the mathematical theory of truth--that is, model theory--may be described as a translation procedure. One does not, however, simply pick any language which we understand. The language L' into which the translations are made is a standard one, and the essential features are more or less settled. As we have illustrated in our discussion of natural models in the last chapter, this language is the language of set theory, either VBG or (what comes to the same) ZF augmented by class terms.[57]

For example, for the language of number theory S, which contains '+', 'x', '′', and '0' as non-logical constants, we may characterize, say, reference w.r.t. the standard model as follows:

The referent of '0' = $\emptyset$ (the empty set)

and for application of the successor function, we have:

The function symbol '′' applies to $\langle x, y \rangle$ iff
$$x \in \omega \;\&\, y \in \omega \;\&\; y = x \cup \{x\}$$

The treatment of '+' and 'x', and for other theories, predicates and relations, is similarly routine.

The fact that model theory contains a standard, accepted account of primitive denotation is significant or not depending on why set theory has the special role it does.

It has been claimed that set theory constitutes the most fundamental theory in mathematics, to which other theories may be "reduced" in a sense parallel to the reduction of macrophysics to quantum mechanics. For the present, this claim is obviously true: the only other candidate for "most fundamental theory" status is category theory,[58] and it remains to be seen whether category theory is an alternative to set theory, an alternate form of set theory, or reducible to set theory.[59]

The fact that set theory is the background theory for model theory raises some special problems for our enquiry, since we wish to study the model theory of set theory itself. Using set theory to study set theory need not be automatically pointless, for we are not attempting some sort of epistomological reduction of set theory to something else more secure. On the contrary, we are assuming a large body of accepted set theory to be true, according to our statement (B) of minimal realism. For rather well-known technical reasons, which we will shortly review, present set theory will not in fact aid us in giving an account of primitive denotation for terms of set theory which will be of much help in settling the question of uniqueness of interpretation which is our main interest. It will be useful, however, for us to review the conventional wisdom about truth--i.e., primitive denotation--characterizations for set theory, before seeing what other account of primitive denotation for set theory may be possible.

Set-Theoretic Accounts of Primitive Denotation of 'ϵ':

Since the only non-logical symbol of ZF--the theory which is our central concern--is 'ϵ', we will consider the possibility of an account of application for 'ϵ' within ZF.

For statements in the language of ZF, a possible model is an ordered pair $\langle U, R \rangle$ of the "universe" U and a subset R of UxU, which interprets the application of 'ϵ' as follows:

$$\text{For all } x, y \in U,\ \langle U, R \rangle \models x \in y \text{ iff } \langle x, y \rangle \in R.$$

The usual set theoretic version of order pairs make them out to be sets of higher rank than their elements, i.e., $\langle x, y \rangle =_{df} \{\{x\}, \{x, y\}\}$, so that if the rank of U is α, (i.e., if α is the least ordinal such that $U \in R(\alpha + 1)$), the rank of $\langle U, R \rangle$ can be as high as $\alpha + 5$. A somewhat more involved definition of ordered pair can be given such that if $x, y \in R(\alpha)$, then $\langle x, y \rangle \in R(\alpha)$, provided that α is infinite.[60] Thus, in any case, the rank of any possible model $\langle U, R \rangle$ is at least as high as the rank of U. This fact, which is hardly interesting for most first-order theories, brings out a special problem if we attempt to give an account of application for 'ϵ' which covers all sets. If we are to construe the universe of sets as a possible model $\langle U, R \rangle$ in the usual way, then it must be a set, hence a member of the "universal set" U, and hence of lower rank than U, which contradicts our previous rank calculations.

This argument presupposes that there can be a set of all sets, which raises other problems as well. For instance, such a set would presumably be a member of itself, which contradicts the axiom of foundation. Even if this axiom is omitted, which is sometimes done, difficulties about the existence of such a set remain, for every set has a powerst of larger cardinality (Cantor's theorem) but the powerset of the set of all sets is surely that universal set itself (or at any rate, contained in it), which contradicts the irreflexiveness of the of the "greater than" relation among cardinals, which can be proved in ZF.

To these two familiar arguments, we can add others based on the traditional paradoxes; each of these can be used to show that there can be no set of all sets in any of the ZF-based set theories discussed in Chapter I. For example, supposed that there is a set S of just those sets which are not members of themselves. Then, by Russell's simple argument, S both is and is not a member of itself, so there is no such set. But by the axiom of foundation, the universe consists of just those sets which are not members of themselves, so there is no universal set. Similarly, if the axiom of foundation is not assumed, we can still use the axiom of separation to show that if there were a universal set, then the S defined above would also be a set, which leads to a contradiction in the same manner as before. Thus we can give an extraordinarily simple argument based on Russell's paradox, which use very modest assumptions (either foundation or separation) to show that there is no set of all sets. We may conclude that not only is the existence of a universal set incompatible with ZF, VBG, etc., but that modification of these systems to obtain a system compatible with the existence of such a set requires that one give up quite fundamental principles of those theories.

Application for 'ϵ' in Other Set Theories

There are several set theories which we have not yet considered in which it is a theorem that there is a set of all sets. These are the theories, NF (New Foundations) and ML (Mathematical Logic), invented by Quine.[61] It would serve no particular purpose to give a thorough exposition of these systems; an outline of them appears in Appendix B. For the sake of completeness in our discussion of set-theoretic accounts of application, we will briefly examine the question of whether the difficulties so far treated in defining application for 'ϵ' in ZF and its relatives are lessened for NF or ML.

The first problem, about the rank of the universe $\langle U, R \rangle$ is easily settled, for no notion of rank similar to that for ZF is available in either NF or ML. That is, we cannot arrange the universe into well-ordered cumulative levels such that the members of a set which first appear at a level has only members which first appear at lower levels. This is because the universal set is a member of itself, hence a member whose "rank" could not be lower the that of the set itself.

The difficulty about Cantor's theorem is obviated in NF because that theorem obtains in NF only for the so-called "cantorian" sets--sets cardinally similar to the set of their unit sets.[62] The set of all sets is, however, not cantorian. Non-cantorian sets also have other special properties. For example, the axiom of choice also fails for non-cantorian sets, as is shown by the fact that the predecessors of the cardinal of the universal set are not well-ordered, a strikingly counterintuitive result.[63]

The features of NF which we have noted are also reflected in ML, which bears a relation to NF comparable to that which VBI bears to ZF. In ML, all sets--including the universal set--are cantorian, but the one-one correspondences which guarantee this are, in some cases, proper classes. The axiom of choice is probably not inconsistent with ML, but only in a form which guarantees a choice <u>class</u> for every exclusive set of sets. The cardinal numbers are well-ordered, but in some cases the required maps are again proper classes. Cantor's theorem divides into the two statements that (a) every set is cardinally less than the set of its subsets and (b) every class is cardinally less than the class of its subclasses. The latter statement is a theorem of ML, but the former is not--it fails for the universal set. Clearly, classes in ML do not play the auxiliary "formula substitute" role which they do in VBG, but are essential for natural and important theorems of ML whose counterparts in ZF, VBG, etc., involve only sets.

As to the model theory of these theories, NF is easily disposed of, as it has no models in which both the integers and the ordinals are well-ordered (under their respective order relations) and in which the equality relation is interpreted as actual identity.[63a] No such dramatic result is known for ML, but it is easily seen that ML has no advantage over ZF and VBG in the problem of explaining application of 'ϵ'. First, since quanitifcation over proper classes is essential to important theorems of ML, it is no advantage to have a universal <u>set</u> U--one needs a universal <u>class</u> containing all classes as its members, but there is no such class according to ML.

Second, and more important, our assumption (B) of minimal realism includes the truth of accepted set theoretic statements under at least one natural interpretation. We have not yet discussed naturalness conditions for interpretations of set theory, but it is plain that the description we have given of the CTS provides at least a start for ZF. For ML, we lack <u>any</u> useful or natural picture of an intended interpretation. Even if (B) is a perfectly reasonable assumption for ZF, the same cannot be said for ML. This being the case, it is hard to see the point of explaining application for ML or for ZF within ML.

<u>Further Attempts to Explain Application of 'ϵ'</u>:

Our difficulties have centered on the fact that the intended interpretation $\langle U, R \rangle$ cannot be a set. One natural course of action is to concede this and explore the possibility of treating it as a proper class. This will not, I contend, lead to any advance in explanatory clarity, but it is worth looking into, both to cover all the possibilities, and specifically to provide a basis of some later discussion.

We will consider several possible accounts of proper classes, beginning with the notion that proper classes are collections of some sort, but ones too large to be sets. This intuitive idea of proper classes departs from the point of view we took in the last chapter, where we suggested that classes are definite properties of sets, or surrogates for them. That interpretation is a bit contrived for the classes of VBG, but even there we can regard classes as obtained (metaphorically) by ignoring differences between properties with the same extension--perhaps by "weeding out" all properties but one with a given extension--and ignoring the distinction between a definite property whose extension is a set and that extension.

Fortunately, we need not resort of such metaphorical selecting and ignoring in order to accomodate the property interpretation to VBG; Bernays has already developed a version of VBG which does nicely.[64] In his version, identity of classes is not even expressible, but is replaced by the relation "having the same extension". A distinction is maintained between a set and the class having the same "members". If we prefer to hold that classes or at least proper ones are properties, then Bernays' version should be counted as more precise, or at least more revealing in its account of classes, although standard VBG may still be preferred for its less cumbersome apparatus.

While the class-as-definite-property interpretation has historical support--we outlined it in the last chapter--it is more popular today to regard classes as large collections. This idea is certainly encouraged by features of standard VBG, since it is a theorem that a class is proper iff it is cardinally similar to the universal class (class of all sets). So all proper classes are the same size, and (by the replacement axiom) are all cardinally larger than any set.

Cantor's Proper Classes

The view that proper classes are distinguished from sets by size is an old one--about the same vintage as the property interpretation. In 1905, Russell suggested that sets may be distinguished from "non-entities" by size.[65] Cantor had anticipated such a distinction even earlier (1899) in a letter to Dedekind.[66] Cantor called his proper classes "inconsistent multiplicities." These are "multiplicities" which can not "be thought of without contradiction as 'being together', so that they can be gathered together into <u>one</u> thing' ".[67]

This language contrasts with Cantor's earlier "definition" of a set as "any collection into a whole M of definite and separate objects m of our intuition or our thought".[68] His examples of "inconsistent multiplicities" include the multiplicity of all ordinals, and that of all cardinals.[69]

Surely any explanation of the collection of all sets as an inconsistent multiplicity, if it is to be taken at all seriously, can only convince us either that there is no such entity, or that we have no reason to believe there is, for how can something which "cannot be conceived as '<u>one</u> thing' " be conceived of as something at all? That is, if we construe "the universe", the U in $\langle U, R \rangle$, as an inconsistent multiplicity, how can this provide an explanation of application according to some unique interpretation?

Generally, it is wise not to put too much weight on such semi-psychological considerations as what may or may not be conceived. But according to Cantor's peculuar view as explained in the section on "quasi-realism", these considerations are important because our conceptions must be "an expression of ... processes and relations in the outer world".[70] This invites us to consider whether the collection of all sets may actually be somehow inconsistent in "the outer world". That is, we should consider whether the assumption that all sets are "gathered together" into a single collections leads to a contradiction.

Such a claim is suggested by Russell,[71] and actually stated by Bernays:

> The antinomies [he says] bring out the impossibility of combining the following two things: the idea of the totality of all mathematical objects and the general concepts of set and function; for the totality itself would form a domain of elements for sets, and arguments and values for functions.[72]

We must, therefore, give up "absolute platonism", which is the view that there is a "totality of all mathematical objects".[73] But plainly, giving up absolute platonism is not the only possible course, even if Bernays' argument is accepted; we could, for example, give up the idea that every "mathematical object" is eligible for set membership or function argumenthood. This is the move suggested by maintaining that not every collection is a set, and only collections which are sets can be elements or arguments.

In order to show that some contradiction follows from the assumption that there is a collection of all sets, we would need some principles of "collection theory", and it seems reasonable to suppose that certain principles of set theory would serve. Indeed, one might reasonably maintain that the only principles of collection theory for which one can have any good grounds now are those of set theory, since set theory is a kind of rational reconstruction of the notion of collection, the only one which has been well investigated.

Assuming we have some principles of collection theory, adapted from set theory, it is extremely unlikely that the notion that there exists a collection of all sets leads to a contradiction, unless we are to assume at the outset that every collection is a set and thus beg the question. For example, if we attempt to carry over the version of Russell's argument which we used previously to the present case, we can easily show that there can be no collection of all collections; but the situation is more complicated for sets. For example, if we supposed that there is a collection of all sets, then, by a separation axiom for collections, we infer that there must be a collection of all non-selfmembered sets. If we could assume that this collection were a set, we could derive the familiar contradiction that it must be both a member of and not a member of itself. We cannot, without question begging, make this assumption, so our attempt to show that proper classes are more than metaphorical "inconsistent multiplicities" is blocked.

One further suggestion deserves examination, at least as an attempted interpretation of Cantor, if not on its own merits. In VBG, no proper class (and in Bernays' variant, no class) can be a member of any set. By Cantor's definition, any "separate and definite objects" can be members of a set, so presumably a (proper) class ought not to be regarded as a "separate, definite object". Whatever exactly this means, "objects" which are not "separate and definite" certainly <u>sound</u> like the sort of thing which could not be "conceived as <u>one</u> thing", which is Cantor's description of "inconsistent multiplicities". Taking "multiplicity which cannot be a member of any set" as a formal counterpart to "inconsistent multipicity", however, is unlikely to lead to a suitable argument that the assumption that such multiplicities exist leads to an actual contradiction. The reason is that we have shown that if there are inaccessibles, there are models of ZF in which the "inconsistent multiplicities" of the model are simply sets of higher rank than any set in the model. Similarly, we may cite the equi-consistency result for ZF and VBG, which says that predicatively definable proper classes can be added to ZF without incurring inconsistency, provided ZF is consistent. Since the "universal" class is such a class, efforts to use Cantor's suggestions to show that a universal class must be somehow inconsistent lead nowhere.

Proper Classes as Definite Properties (Again)

Having made no notable progress in clarifying application of 'ϵ' by means of the notion of "large collection", it is in order to see whether we can do any better by construing proper classes as properties. I think that we cannot, on account of the familiar difficulties about identity of properties ably put by Quine:

> The positing of attributes [i.e., properties] is accompanied by no clue as to the circumstances under which attributes may be said to be the same or different. This is perverse, considering that the very use of terms and the very positing of objects are unrecognizable to begin with except as keyed in with idioms of sameness and difference.... The lack of a proper identity concept for attributes is a lack that philosophers feel impelled to supply; for, what sense is there in saying that there are attributes when there is no sense in saying when there is one attribute and when two? [74]

It is not my intention in quoting Quine either to endorse the doctrine suggested here about when "positing" of objects is "recognizable" or to grind his axe about properties, viz., that talk of them ought to be dispensed with. I do believe, however, that he is properly skeptical about the philosophical theories so far advanced to clarify the "circumstances under which attributes may be said to be the same or different". No doubt some similar skeptical motive prompted Bernays to develop his variant of VBG with its inexpressibility of identity of classes and his avoidance of quantification over them. Virtually all putative explanations of identity of properties so far offered have made use of notions at least as much in need of clarification (if indeed this is possible) as the notion of property identity itself, such as "synonomy of expressions" or "possible worlds" or "necessary truth".

One partial exception to this evaluation is the suggestion that a property is the "sense" of a predicate expression (open sentence with one free variable). The explanation given by advocates of this view of what the "sense" of such an expression is ordinarily makes use of such notions of "possible world", etc., but one suggested criterion of identity of properties--that is, when two predicate expressions have the same sense--is clear: Two predicate expressions have the same property as their sense iff they are logically equivalent. That is, $A(x)$ and $B(x)$ express the same property iff '$(x)(A(x) \equiv B(x))$' is logically valid. This very narrow criterion is faulted by those who would make use of some broader notion of necessary equivalence, and by those who would permit synthetic identity of properties, but in any case, it will not do for VBG. In that theory, it is a theorem that the class of sets and the class of all non-self-membered sets are identical, but '$(x)(x=x \equiv -x \in x)$' is not logically valid. If we were to expand the language of Bernays' variant to introduce identity of classes, the same difficulty would appear there.

As stated earlier, I do not wish to conclude from these difficulties about identity of properties that property talk should be dispensed with. There are two reasons, however, why the lack of clarity about identity in this context is a problem for us. First, we are concerned to determine whether there is a unique universe of sets, and if "the" universe is a property and we don't know how to count properties, we won't be very clear about even how to say that "the" universe is unique. Second, the separation principle, used to "count" subsets, uses the notion of property. In fact, the only way we succeeded in "counting" definite properties in our discussion in the first chapter was to speak always of their extensions. So, for the present at least, the property interpretation of classes provides no clearer explanation of application of 'ϵ' w.r.t. a unique intended interpretation that previous attempts.[75]

It should be plain from the discussion of this and the previous section that cavalierly responding to the discovery that there is no set of all sets by saying "let the universe be a proper class" leads to no advance whatever in explaining truth or specifying interpretations for set theory--quite the reverse. At the very least, some sufficiently strong theory of classes will be needed for this task, and that theory must have a clear interpretation in turn. So far, our discussion of proper classes shows them to be less well understood than sets.

The Universe Described in English

So far, we have failed to give a satisfactory account of primitive denotation for set theory within either set theory or its extension by addition of proper classes. The main obstacle to such an account has proved to be the impossibility of describing an intended interpretation $\langle U, R \rangle$ within set theory, and the obscurity of considering it a proper class. Thus we have failed both to give an adequate account of truth of set-theoretic sentences required for (B) and to specify a unique interpretation required for (A).

Nothing of what we have said here is new, and realists have attempted to deal with these familiar difficulties we have reviewed. Bernays made use of facts we cited to reject a form of realism, "absolute platonism", although we were unable to accept his argument as conclusive. Other philosophers sympathetic to realist views have had another response. Kreisel argues that a description of "the" intended interpretation in non-technical language and the reliance on an intuitive notion of truth under such an interpretation is adequate to settle the question of uniqueness of interpretation. We briefly review his account.

Kreisel's view that the intended interpretation of set theory may be profitably described in English is shown in the following passage:

> In time, the remarkably clear and general cumulative type structure was isolated, consisting of the objects generated by iterating the operation: $X \to X \cup P(X)$, where $P(X)$ denotes the powerset of X, and taking set theoretic unions at limits....
>
> <u>This structure was the result of analysis</u>: not complicated formal constructions, but the description (in words:) of the cumulative type structure constituted the decisive foundational advance. I, for one, would not claim that this, or any other general notion is the <u>naive</u> notion of set....
>
> The familiar axioms known as ZERMELO'S, hold for the cumulative heirarchies obtained from the empty set by iterating the power set construction to any limit ordinal;....
>
> Objectively, at the present stage of knowledge, the <u>natural problem</u> is to develop the theory and find out more about the structure.[76]

Kreisel claims that description "in words" of the CTS, as in the quoted passage, is adequate "to avoid an endless string of ambiguities to be resolved by further basic distinctions."[77] Apparently, he believes this because the discription of the CTS "in words" "provides a coherent <u>source</u> of axioms",[78] both for Zermelo initially and for us at present. The axioms beyond ZF he has in mind are various strong axioms of infinity.[79]

I find this argument from the fruitfulness of non-technical descriptions of CTS inconclusive , for, as we have noted before, horribly confused remarks such as Brouwer's can be a fruitful source of interesting principles. Kreisel, however, plainly believes that we are in a position to deny the "(alleged) <u>bifurication or multifurcation of our notion of set of the cumulative hierarchy</u>... [by] asserting the properties of our intuitive conception of the cumulative type structure. "[80]

As for our understanding of truth in such a structure, Kreisel is perfectly willing to consider models which are proper classes.[81] He also defends the notion of intuitive validity, that is, truth in an "arbitrary structure",[82] including proper classes. He thinks that set theoretic accounts of truth have increased our knowledge about this notion, "but that doesn't mean that [it] was vague before".[83]

Plainly, Kreisel is asserting that despite the lack of a completely satisfactory theory of primitive denotation, set theoretic or otherwise, we do understand the intended interpretation of set theory and truth under that interpretation sufficiently well to accept the premises of (A), and hence its conclusion. In one passage, however, he introduces a curious qualification to his claims about the uniqueness of CTS:

> ... unless one has theoretical or empirical reasons against naive judgement, in particular against [CTS], the precise notion of set ... is a foundation for [Zermelo's] axioms.[84]

It should be clear to the reader from our repeated use of the phrase "at least one natural interpretation for set theory" that I think there may well be "reasons against naive judgments" in this case. Other writers we will discuss--in particular, Mostowski--advance "theoretical ... reasons" based on the independence results for the claim that there actually are several equally natural interpretations for set theory. I do not find these reasons conclusive, but I do think that this a serious question.

To show that the question of multiple interpretations is a serious one, we will follow two lines of enquiry. We will examine descriptions of the CTS of the sort diven by Kreisel for possible ambiguities. We will then review several important cases in the history of mathematics in which ambiguities and unclarity were responsible--in part--for failure to resolve controversial questions. Later, we will consider--but reject--arguments that the ambiguities in the interpretation of set theory are not merely possible, but actually exist. Before we can go into these questions, however, we need to digress to provide a basis for questioning the truth or clarity of any part of set theory.

Set Theory vs. Number Theory

At the outset, we must deal with a feeling that some readers must have at this point, that in questioning the uniqueness of the of the intended interpretation of set theory, we are exhibiting a sort of skepticism about set theory which is no more justified about set theory than it would be about number theory.

It is my view that it would indeed be wrong to doubt that number theory has a unique intended interpretation, but that there are important differences between set theory and number theory which bear both on the certainty of our beliefs in their respective theorems and on the clarity of the interpretation of those theorems.

The argument that there are such differences rests on a certain methodological rule of thumb, which I think is obvious, but which deserves a brief discussion. The rule of thumb is this:

(E) A theoretical question of a scientific discipline has not been settled with certainty if no candidate answer has obtained at least near unanimous consent of the professional practitioners of that discipline.

(E) is, or course, only a rule of thumb; for any particular question, it may be that decisive arguments exist which are known to some, but have not yet been generally recognized; but if the question is an important one, it is worth determining whether such arguments do in fact exist.

Persistent disagreement among professional practitioners of a discipline requires an explanation. Plainly such an explanation must make reference to the plausibility of the various views held; we cannot always explain such disagreements by maintaining that is just a shortcoming of the advocates of one view that they fail to perceive its total implausibility. But ---and this is the point--we are not entitled to be certain of the truth of a proposition if it is incompatible with another which is plausible.

Our case for differences between number theory and set theory is thus partly sociological; it depends on which theories actually secure acceptance and by whom. One question in set theory which now, by our rule of thumb, has no answer presently deserving confident belief is that of the nature of, and how to characterize, definite properties. As we have already suggested--and will argue below--this question is central to our understanding of other even more fundamental notions of set theory, such as powerset.

In contrast, we find almost no disagreement in number theory. The theory may be formulated in various ways: formally or informally, first or second order, with various primitives, with or without the ω- rule. It is plain, however, that there is a single intended interpretation which is the subject matter of these various formulations. This standard model must be isomorphic to the series of numerals, must be a progression, must have order type ω, all of which conditions are, of course, equivalent.

The only possible difficulty with this rosy picture is the existence of intuitionistic number theory. If I correctly understand their position, the intuitionists take themselves to be studying the same structure as classical number theorists, but how they could _know_ this to be the case if numbers are mental constructions is unclear. Their methods for studying the subject matter of number theory differ from the usual ones, but to an unknown degree, since their formulations are vague and they hold a different interpretation of the basic logical vocabulary.

If we are to abide by our rule of thumb, the existence of this body of intuitionist theory may render some propositions of number theory less certain than they would otherwise be. The effect is certainly slight, however, for despite wide interest in intuitionist views, few are actually persuaded that their restrictive standands of proof are correct. Thus the "sociological" evidence for the plausibility of intuitionist views is very weak.

Alternative Set Theories

In contrast to the virtual unaniminity in number theory, we find a quite different situation in set theory, which can be traced historically to the paradoxes discovered at the turn of the century. Of course, it is not simply that people found some paradoxes seventy years ago which gives set theory a different character from arithmetic; this historical fact would not show anything about the present case. An example which shows this clearly is the paradoxes of 18th century analysis, which were more closely tied to fundamental methods and questions than those of set theory. As Berkeley analyzed a typical differentiation argument:

> herein is a direct fallacy: for, in the first place, it is supposed that the abscisses z and x are unequal, without which no one step could have been made; and in the second place, it is supposed they are equal; which is a manifest inconsistency.[85]

We can now see that Berkeley's charge of inconsistency was correct; but it would be ludicrous to maintain that since this situation once obtained in analysis, this discipline is now less secure.

The situation we find in set theory is that the paradoxes[86] have had a lasting effect in the form of a large variety of alternative set theories embodying various incompatible features. Besides Cantor's and Frege's theories, there are more than a dozen theories which are still studied and many of them are still defended as plausible. Four of these theories trace their ancestry to Frege, simple and ramified type theory, and Quine's NF and ML. A larger number derive from Zermelo, his own, ZF, VBG (two versions), VBI, STC, TT, NTT, and Ackermann's. New theories have recently been published by H. Friedman,[87] Y. Moschovakis,[88] L. Tharp,[89] and P. Hajek.[90]

Although ZF and its relatives are the set theories receiving the most attention--and deserving it--this was not the case, to judge from the journals, as little as 20 years ago, when one finds more attention given to Quine's theories.

It should be noted that some of these non-ZF-based theories are not simply put together out of *ad hoc* principles thought to avoid paradoxes, but have a certain intuitive support. (That is, intuitive support for someone who is familiar only with "naive" set theory; whether the principles of alternatives theories are intuitively natural for someone already trained to work with ZF is another question). For example, Quine's ML takes the idea that sets should be arranged in types--this is embodied in the stratification condition on the definition of sets[91]-- and adds the notion that every set should have a complement, so that "large" and "small" sets are naturally related. This last principle is certainly plausible; (again, to the non-initiate of ZF). Naively, it is very hard to see why no set should have a complement, as is the case in ZF.

We have already noted an important difference, however, between the relative intuitive support for ZF and other theories such as ML. ML is based on principles which are plausible given the "naive" notion of set and which seem to avoid paradoxes. ZF, on the other hand, is based on a intuitive idea of how the entire universe is structured. Once one accepts this intuitive picture, the particular axioms of ZF seem obviously true. Thus, it has been possible for several authors to given either an intuitively convincing argument for each axiom (Shoenfield,[92] Kreisel[93]), or to derive the axioms from a few primciples about ranks (Scott[94]). This is important because (a) we have some reason to believe ZF consistent because we have an idea what a model for it "looks like" and (b) as Kreisel suggests, it helps to find new axioms.

One additional competitor to ZF needs to be mentioned: category theory. Several people have proposed category theory as an alternative "working foundation" for mathematics,[95] but that is not the suggestion I wish to consider here. In particular, such a notion would not tend to know that there is anything suspect about ZF; it might just not turn out to be the most general or convenient working basis for mathematics. The suggestion I wish to consider is that some of the notions in category theory are alternative set theoretic ones, in particular the so-called "large categories" such as the category of all categories. Category theorists treat such objects not, like the collection of all sets in ZF, as simply "too big" to exist (i.e., be a set), but as to big to have a cardinality. Consequently, one talks about these large categories, but does not make cardinality arguments about them.

It is possible that this situation may be representable in some ZF based theory such as STC by introducing new objects which are not sets (extended ordinals) to be the cardinals of the collections which are not sets, but it should be strongly emphasized that this strategy represents an important departure from the basic notion of ZF that all the collections one wishes to make use of are obtained by iteration of powerset through all the ordinals.[96]

The conclusion which I want to draw from these comparisions of ZF with non-ZF-based theories of current interest is not that we simply know nothing in the areas in which these theories conflict.

On the contrary, I believe--as do most working set theorists--that by far the most plausible account of set theory is represented by ZF and its relatives. But on some matters it will be useful or necessary to evaluate in just what respects this group of theories <u>is</u> superior to alternatives and to elucidate some notions by comparing their treatment in alternative theories. We can then apply our rule of thumb to specific propositions of set theory. That is, we can argue that some specific questions are not conclusively settled simply because we have theories in the field which give different answers to them.

<u>Natural Interpretations of ZF</u>

I have suggested above that (a) some theorems of ZF are less certain than those of number theory, and that (b) it is not certain that there is a single intended or most natural interpretation of ZF. In the previous section, a case was made for (a), but we have yet to do the same for (b). The idea of this section is to try to show how non-isomorphic but equally natural interpretations of ZF might arise. We will do so by analyzing the sort of description of CTS given by Kreisel "in words".

We proceed by assuming that there is no ambiguity in the notion of ordinal number, and discuss the iteration of the powerset operation only. If our assumption about the ordinals is unjustified, then the case for possible ambiguities in the notion of set will only be strengthened. The power set of a set x must consist of all and only its subsets, i.e., (PS) $$P(x) = (\imath w)\,(y)(y \in w \equiv y \subseteq x)\ .$$

One thing to be noted immediately is that the metaphor we have used, of the universe being "constructed" or "generated" by iteration of powerset through the ordinals, is seriously misleading. Such a metaphor suggests a predicative construction, where elements of one level are defined by quantification only over elements of earlier levels. Powerset is not, however a predicative notion, as an examination of the definition (PS) quickly shows. The powerset of x contains all subsets of x--that is, sets which are contained in x. This is simply because the quantifier (y) in (PS) ranges over sets. But the relevant sets are in general of higher rank than x itself, so it is misleading to think of the set of higher rank as themselves "constructed" by the powerset operation. What this powerset will contain depends on what sets contained in x of higher rank than x "already exist". The significance of this fact is suggested by the following intuitive picture: it is conceivable that there should be "subcollections" of x which are not sets. One fact which shows that this is indeed conceivable is that in alternative set theories such as ML, there are "subcollections" of some sets which are not sets. In particular, the class of all non-self-membered sets is contained in the set of all sets, but is not itself a set. This shows that in ML, the separation principle fails even for definite properties expressible in the language of ML.

We are, of course, not dealing here with a notion of the structure of the universe which is appropriate for ML, but merely using the comparison with that theory to show that it is not crazy to suggest that some "collections" contained in a set may not be sets. There is, however, a theory TSS (for Theory of Semi-Sets) recently developed by the Prague set theorists which is based on ZF and which has this same feature.[97] TSS was developed to accomodate Vopenka's formulation[98] of Cohen's independence methods, and it is not clear that the intended interpretation of this theory is the standard structure of sets. Rather, it is a convenient method for independence proofs which do not deal explicity with models of set theory. Nevertheless, one author has given an intuitive justification of TSS not depending on construction of nonstandard models of ZF.[98a] If the axiom "every semiset is a set" is added to TSS, the result is ZF minus Foundation.[99]

ZF and ZF-based theories other than TSS are, of course, firmly committed to the separation principle, that for every set x and every definite property G there is a subset of x consisting of just those elements having G. Because of the importance of this principle to the theories which have inherited it from Zermelo's, it is plain that which collections contained in x are subsets of it depends on what definite properties there are, and this is a controversial matter receiving radically different treatments even in theories which take the CTS as motivation.

The matter comes down to this: In all natural interpretations of ZF, the powerset of a set x contains all and only the subsets of x, but which "candidate subsets" (subcollections) are actually subsets might differ in two interpretations, provided that the ranges of definite properties associated with the two interpretations are distinct. Of course, we would want to chose as most natural the interpretation which arises in this way which has the maximum number of associated definite properties. But it is entirely possible that this criterion may not dictate a choice between the two. With the present chaotic range of treatments of definite properties, it is possible that there are incompatible ways of choosing maximum ranges of them.

What we are implicitly appealing to in the argument of the previous paragraph is a principle about the CTS which might be called the principle of maximum width:

(PMW) Every subcollection of a set is a subset of it.

PMW certainly seems a natural condition on natural interpretations. Godel indicates approval of a slightly more general principle:

> I am thinking of an axiom which ... would state some maximum property of the system of all sets.... Note that only a maximum property would seem to harmonize with the notion of set [of the CTS].[100]

The context of Godel's remarks indicates that he had something like PMW in mind.[101]

Given that PMW is a plausible and important intuitive notion, the question is how to formulate it in a useful way. Our earlier argument about definite properties essentially uses the separation principle as a formulation of PMW, viz.,

(PMW1) For all definite properties G, $\{y \mid y \text{ has } G \ \& \ y \in x\} \in P(x)$.

Another way to formulate the intuitive idea of PMW is to simply assert that there are "many" subsets of any infinite set, in some appropriate sense of 'many'. The idea of this approach is that if we can say that there are "many" subsets in a sufficiently strong sense, this may have the same effect as saying that all candidate subsets are actual subsets. The problem is now to talk about "many" subsets. A leading principle proposed by G. Takeuti is to formulate the notion of "many" in terms of cardinality:

(PMW2) "The cardinality of a powerset of an infinite set is very big with respect to the notion of cardinal."[102]

There are two difficulties with this approach. The first, noted by Takeuti himself,[103] is that the notion of cardinal of a set x makes use of quantification over sets of higher rank than x, so it already presupposes some notion of rank. Some of Takeuti's refinements of PMW2 do assert that the universe is wide in some sense, since they contradict the axiom of constructibility,[106] a "minimum width" principle.[107]

The second difficulty is that it is hard to see why principles derived from PMW2 should be plausible. Takeuti certainly thinks so,[105] and others state similar opinions.[105a] Their reasons for this attitude--and the resultant rejection of CH, which is incompatible with any reasonable version of PMW2--are hardly clear, however.

Some strong versions of PMW2 formulated by Takeuti are surely to be rejected, since they contradict the axiom of choice.[108]

It is still possible, however, that we may find reasons for adopting some version of PMW2, and then we will have another point of view from which we can see how equally natural CTS-like structures can arise: they may satisfy different, incompatible version of PMW2.

Whether it is actually plausible, as opposed to just conceivable, that different equally natural interpretations of ZF which are not isomorphic may be arrived at in these or other ways, or whether the independence of CH adds to this plausibility is quite another question. I will argue that the CH does not make this situation more likely, but the sort of argument given here is sufficient to suggest that such questions are worthy of further investigation.

Historical Cases of Unclarity of Mathematical Notions

The argument of the previous section might be taken as a kind of cartesian demon, a rationale for challenging what we think--or at least Kreisel thinks--we know. But our object is not merely to raise skeptical objections about mathematical knowledge, but real ones. To show that it is at least sometimes reasonable to question whether our notions are clear--that is, susceptible to only one reasonable interpretation. I will review several historical cases where unclarity in some fundamental notions was partly responsible for failure to resolve important questions. The special advantage of historical cases is that we can now look back and use a more developed theory to characterize views in a clearer way than could the participants in these past controversies. Since we cannot look into the future, we lack this advantage in set theory.

The first example is from the history of set theory itself. Roughly, I claim that about 1900, the notion of set was unclear in at least the sense that what sorts of reasoning about sets is legitimate and in particular what sorts of putative definitions or descriptions of sets actually define sets was enormously unclear. These difficulties and a number of incompatible strategies for resolving them were described in a 1905 paper of Russell.[109] This paper contains, in outline, most of the devices for avoiding the paradoxes subsequently incorporated into set theories we have listed in previous sections. I claim that a survey of set theory up to the time when Russell wrote, as contained in Van Heijenoort's collection,[110] shows that Russell was correct in maintaining that the difficulties would not be resolved "until the fundamental logical notions employed are more thoroughly understood".[111] It is clear that Russell included both the notion of set and that of definite property among these "notions of logic".[112]

Russell saw the outstanding problem of set theory as that of determining which one free variable sentences ("norms") define sets (are "predicative"), which is roughly the problem of which properties are definite.[113] At the same time that Russell was puzzling over which set definitions are proper, a group of prominent French mathematicians were debating the issue of whether every set must be definable, a position almost no one would now defend. This controversy was provoked by Zermelo's announcement of the axiom of choice. The debate, recorded in the so-called "cinq lettres" correspondence, is discussed in appendix A.[114]

Although I claim that the naive notion of set seventy years ago is an example of an unclear notion,[115] one which made it impossible at the time to answer such questions as "is there a set of all ordinals?", I do not claim that this shows that the notion of set is now unclear. After all, a lot of progress has been made in seventy years, and the particular question just cited can now be answered in the negative with considerable confidence.

The Vibrating String in the 18th Century

We will go over this famous controversy in outline, focusing on the part played by the notion of "arbitrary function".[116]

In 1746, Jean D'Alembert published a memoir on the vibration of a stretched string, whose motion be described by what is essentially the well-known wave question:

$$\text{(W)} \qquad \frac{\partial^2 \varphi}{\partial t^2} = c^2 \frac{\partial^2 \varphi}{\partial x^2}$$ [117]

This equation is valid provided the amplitude of vibration $\varphi(x, t)$ is vanishingly small, where t is the elapsed time and x is the distance from one end of the string. Brook Taylor had already shown in 1713 that, in effect,

$$\text{(T)} \qquad \varphi(x, t) = B \sin(2\pi t) \cos(2\pi x / L)$$

is a solution to (W), where L is the length of the string and B is a constant. D'Alembert sought the general solution to (W), recognizing that (T) is not the only possible one. He argued that this general solution has the form

$$\varphi(x, t) = A(ct+x) - A(ct-x) \tag{L}$$ [118]

where A(u) is a function called the "generating curve". D'Alembert proceeded to calculate A(u) for given initial position of the string Y(x) and initial velocity V(x). He claimed, however, that the problem could be solved only for rather special initial V(x) and Y(x):

> ...one obtains the solution of the problem only for cases where the different shapes of the vibrating string may be included in one and the same equation. In all other cases it seems impossible to give [$\varphi(x, t)$] a general form.[119]

It follows from this restriction that Y and V be given by an "equation" that they must both be odd periodic functions of period 2L. The restriction to initial conditions given by "equations" thus has the astounding consequence that for such simple initial shapes as triangular ones, the problem cannot be solved.

The term 'equation' occurs here for an important reason. For D'Alembert, as for virtually all 18th century analysts,[120] functions must be given by an equation,[121] although the operations to be permitted in forming these equations were not precisely delimited. Roughly, D'Alembert's notion of function coincides with the modern notion of analytic function of a real variable, that is, function which can be expanded in a power series of its derivatives at every point. Such functions must have continuous derivatives of all orders. He assumes, without a hint of proof, the key property of such functions, that if they agree on any finite interval, they agree everywhere. The restriction to such functions turns out to account for the cases of the string problem where D'Alembert had contended solution was impossible.[122]

D'Alembert's memoir prompted an immediate reply by Leonhard Euler,[123] repeating D'Alembert's arguments, but without his restrictions on the initial shape and velocity.[124]

> These ... curves ... are equally satisfactory, whether they are expressed by some equation or whether they are traced in any fashion, in such a way as not to be subject to an equation.[125]

Thus Euler admitted non-analytic functions as solutions to (W), both as mathematically and physically significant. This departure from the conventional wisdom of the time has been called, rather extravagantly, "the greatest advance in scientific methodology in the entire century".[126] In any case, it was an important development. D'Alembert refused to the end of his life to admit "mechanical" (i.e., non-equation-defined) functions, either as solutions to physical problems or in mathematics generally.[127] He regarded this restriction as entirely obvious, while never giving any reason for it.[128] This question, and not any intrinsic interest of vibrating strings, was the scientific basis of the bitter controversy over the string problem which was to last the rest of the 18th century.

Euler's notion of function may be described in modern terminiology as "continuous function with piecewise continuous slope and curvature"[129] but it should not be supposed that his own conception of the matter was clear. His notion of an "arbitrary function" as "any curve..., irregular or traced at will"[130] was, if anything, less clear than D'Alembert's, although more general. Unlike D'Alembert, however, "no error results from Euler's failure to supply precise definitions".[131] While errors on Euler's part did not result from imprecision, it is clear that the length and complexity of the controversy was partly due to imprecision on all sides. The problem of the vibrating string was not finally laid to rest until the modern notion of function as a many-one relation finally emerged in the 19th century.

The real extent of the controversy on the nature of "arbitrary functions" in the string problem is not sufficiently indicated in the exchange we have described so far. Of the many other participants, two deserve mention. The first is Daniel Bernoulli, who had worked on the string problem before D'Alembert. Bernoulli's memoir replying to Euler and D'Alembert is full of ironic references to their "abstract calculations".[132] His view was that Taylor had already proved that the only solutions were sinusoidal, but he generalized this notion to included sums of sines, i.e., trigonometric series.[133] He held that

> All these new curves and new kinds of vibration given by Messrs. D'ALEMBERT and EULER are absolutely nothing else than a mixture of several kinds of TAYLOR's vibrations.[134]

Bernoulli's observations can now be seen to be correct in a certain sense; the functions concerned can be expanded in trigonometric series, although Bernoulli did not prove this.

Euler was sure that this was not possible, but his argument was fallacious.[135] Bernoulli's "contribution" to the debate was to abuse Euler and D'Alembert for their "abstract analysis", which, "if heeded without any synthetic examination of the question proposed, is more likely to surprize than enlighten".[136] Bernoulli goes so far as to suggest that the issue between Euler and D'Alembert is merely a verbal dispute, "an issue ... concerning the meaning of certain terms".[137]

The remaining participant of note is Lagrange. His 1759 memoir attacks Euler's integration of non-analytic functions: "it seems indubitable that conclusions drawn from the rules of the differential and integral calculus are always illegitimate ... if this law [of continuity] is not assumed".[138] Nevertheless, he believed Euler's result to be correct,[139] and tried to establish it by other methods, which were grossly defective.[140] D'Alembert detected the errors, but Lagranage was never able to remove them completely.[141] Later, as D'Alembert's political influence in the Berlin Academy increased, Lagrange came aroung to his point of view, D'Alembert meanwhile having made some concessions on the operations to be admitted in equations.[142] The new operations, such as $(\sin(\pi x))^{p/q}$, give rise to non-analytic functions. D'Alembert seems in the end to have given up the notion that all derivatives of a function must exist, allowing some to be infinite.[143]

Non-Euclidean Geometry

There is a third incident in the history of mathematics which we must consider because it is much discussed in connection with the independence results in set theory. This incident is the discovery of the independence of Euclid's Fifth postulate of the other axioms of geometry. Cohen,[144] Mostowski,[145] Kreisel,[146] and Suppes,[147] among others, all cite this discovery as an important historical precedent. Kreisel is anxious to draw distinctions between that case and the independence of CH; the others urge that the comparison is most apt. Mostowski, for example, compares the current situation in set theory with the development of a variety of notions of "spaces" in modern geometry:

> Probably we shall have in the future essentially different notions of set just as we have different notions of space, and will base our discussions of sets on axioms which correspond to the kind of sets which we want to study.[148]

Since the fifth postulate is regarded by these authors as an important parallel to our own case, we cannot avoid discussing it, but I doubt very much that this example really shows what Mostowski says it shows. We do now have different notions of "spaces", but the fact remains that under the intended interpretation as a question about <u>space</u>, the question has--subject to future developments in physics--been settled.

Up until the 19th century, almost no one doubted that the parallels postualte was true,[149] and when it was discovered that it was independent of the (perhaps more evident) remaining axioms, Lobachevsky[150] and (probably) Gauss,[151] two of the co-discovers of this fact, immediately regarded it as an astronomical question, and proposed experiments to settle the question. As Lobachevsky put it:

> The fruitlessness of the attempts made, since Euclid's time, for the space of 2000 years, aroused in me the suspicion that the truth, which it was desired to prove, was not contained in the data themselves [i. e., the other axioms]; that to establish it the aid of experiment would be needed, for example, of astronomical observations, as in the case of other laws of nature.[152]

My own view is that Lobachevesky and others who have and do regard 18th century geometry as about physical space (and the parallels postulate as false under this intended interpretation) are entirely correct.[152a] There are, however, two main competing views: (a) geometry is about space, but not physical space,[152b] and (b) geometry is (or may be construed as) about physical space, but which geometry physical space has is entirely a matter of convenient stipulation.[152c] Advocates of (b) differ as to which stipulation ought to be made, and hence on the truth value of the parallels postulate.[152d] This not the place to discuss these rival views, but I do want to make the following observations on the condition that my view announced above is correct.

Our two previous historical cases concerned unclarity of key notions, but I see no reason to regard this as such a case. In particular, I see no reason to suppose that the notions of "point", "straight line", "parallel", etc, which are central to Euclid's geometry, were or are unclear, or that any unclarity there may have been was responsible for the difficulties in determining whether the parallels postulate is true.

The Historical Cases Summed Up

Our announced intention in reviewing examples from the history of mathematics was to show that unclarity of fundamental notions not only occurs, but it matters. Have we shown this? One reason for answering "no" is that each of the questions we have raised has now been answered with reasonable certainty. There is no set of all ordinals, there are non-analytic solutions of the wave equation, and, subject to the view of geometry adopted above, the parallels postulate is false.

Leaving the matter here, however, ignores the crucial point, that naturalness of an interpretation can change with time. The clearest example of this is the notion of function. We have give Euler credit in the string controversy for his more nearly correct, although vaguer, notion of function. Euler's notion is shown to be natural by making possible the solution of a class of physical problems. But D'Alembert's notion was natural too-it just wouldn't do for the new problems. In effect, Euler acknowledged the naturalness of his opponents conception by the definition he gave in a textbook published two years after his first memoir on the string problem: a function of a "variable quantity" is "any analytic expression whatsoever made up from that variable quantity and from numbers or constant quantities".[153] From the point of view of modern analysis, however, neither Euler nor D'Alembert had what is now that most natural notion of function.[154]

Euler's greater generality counted in favor of his interpretation, but the case of geometry shows that the most general notion need not be the most natural interpretation of a given theory. If our interpretation of geometry is correct, the parallels postulate is a proposition about space, but there are far more general notions: differentiable manifold, metric space, topological space, etc. This point has relevance for set theory because it could well turn out that there is a natural interpretation of ZF given by an extreme "maximum width" principle which contradicts the axiom of choice. As we argue in Appendix A, AC is a plausible principle, so a more general (wider) interpretation need not be the most natural.

The question we must now turn to--at last--is whether any of the recent discoveries in set theory make it plausible that the notion of set is now unclear in the specific sense we have been employing-- equally natural CTS-like structures.

The Independence Results and Multiple Interpretations

A number of writers make definite claims that the notion of set is vague or imprecise. Mostoswki, for example, maintains that the "intuitive notion of set is too vague to allow us to decide whether the axiom of choice and the continuum hypothesis are true or false."[155] It is clear from his discussion that he is claiming that the present notion, not just the naive one, is vague. His example of a more precise notion is that of constructible set, which we will discuss momentarily. Mostowski predicts that set theory will "split" into a "multitude of set theories",[156] and his view is shared by a number of others: A. Robinson,[157] L. Kalmar,[158] P. Suppes,[159] and, at one time, Cohen.[160] Although some of these defend "multifurcation" of set theory on formalist grounds--after all, if set theory doesn't answer to anything, isn't one consistent new axiom as good as another? --the interest of Mostowski's position is that he claims that there are or will be distinct intuitive notions of set to go with such divergent theories.[161] Further, Mostowski makes it clear that it is the independence results for the CH and related statements that has lead him to such a position, specifically the fact that it is relatively consistent with ZF to assume that the continuum has any one of an enormous variety of cardinalities.[162]

Mostowski claims that the models of Godel for ZF+AC+GCH and of Cohen for ZF+AC+ ~CH give "various possibilities which are open to us when we want to make more precise the intuitions underlying the notion of a set".[163] He also makes it clear that there are incompatible was of doing this "precising" which give different truth values for CH.[164] I will now proceed as follows in discussing this suggestion: we will examine the models which Godel and Cohen use in their independence proofs in sufficient detail to see if there is indeed reason to believe that they represent essentially different natural notions of set, or at least suggest how to develop such notions. Since Cohen's work uses Godel's ideas, we begin with Godel's notion of constructible set.

The Constructible Universe

The constructible sets may be defined by a method analogous to the ranks, where the powerset operation is replaced by the operation $x \to C(x)$, where $C(x)$ is the set of sets first-order definable over x. We define a function M by transfinite induction as follows: $M(0) = \emptyset$; $M(\alpha +1) = M(\alpha) \cup C(M(\alpha))$; $M(\lambda) = \bigcup_{\alpha < \lambda} M(\alpha)$.

Let $L = \bigcup_{\alpha} M(\alpha)$. The proper class L is called the constructible universe. Godel proved that if ZF is consistent, then L is a "model" for ZF+AC+GCH, hence this latter theory is consistent if ZF is. Actually, L is not quite a model for ZF since it is a proper class. If there are any standard models for ZF, and there are arguments which make this plausible,[165] then there are standard models of ZF which consist only of constructible sets, and which, of course, satisfy AC+GCH. The same proof shows that it is consistent with ZF to assume that the universe consists only of constructible sets (written V = L). Cohen showed that it is also consistent with ZF to assume that not every set or even every set of integers is constructible, even if the AC and GCH are added. I. e., if ZF is consistent, then so is ZF+AC+GCH+V≠L.[166] ZF+V=L is not consistent with some of the "large cardinal" axioms (measurable cardinal, Ramsey cardinal). In fact, these axioms are sufficient to yield a set of integers which is not constructible and has a relatively simple definition in second-order number theory.[167]

Since V = L is consistent with ZF, the notion of constructible set is one of Mostowski's candidate interpretations for a more precise notion of set. The great difficulty with this is that most workers in the field find V=L implausible; I claim it follows that it is not a plausible way to "make precise the intuitions underlying the notion of set" either. We review some of the evidence.

There are several common points of view from which it is natural or imperative to disbelieve V=L. In the first place, one would expect an informalist of either of the types discussed in Chapter I to reject V=L. Essentially the axiom says that the universe consists of just those sets got by taking the ordinals as given and adding the minimum of sets assuming that the definite properties are just those expressed by first-order formulas. That is, L is the common part of all standard "models" (allowing "models" to be proper classes) which contain all the ordinals. Also, the common part of <u>all</u> standard models is itself a model which satisfies V=L. This is the so-called minimal model M; it is, or course, countable.[169] All the informalist positions we considered, however, deny that all definite properties are expressible by first-order formulas of set theory, so they would presumably also deny that all such properties can be expressed by a such formulas with constants in some $M(\alpha)$. Interestingly, we can construct an analogue to L using the notion of "second-order definable over". This also yields a "model" of ZF, the class of "hereditarily ordinal definable" sets. Both the CH and its negtaion are consistent with ZF+AC+ "every set is hereditarily ordinal definable", and it is consistent with ZF that the constructible sets are exactly the second-order constructible sets.[170] This gives another sense in which we do not get any help in deciding CH by using second-order properties of sets.

A second position closely related to the informalist objections is that of Godel, the inventor of the axiom. Godel raises two objections against V=L. The first is that L gives a narrow notion of set, and he wishes a wide one, as we have seen.[171] Godel's second objection is that V=L implies the continuum hypothesis, which he finds implausible because of a number of its consequences in topology.[172] I dispute this claim in Appendix A.

Thirdly, the advocates of the more ambitious "large cardinal axioms" are oblidged to reject V=L, since it is incompatible with them.[173] Since a number of people find some of these axioms plausible, they have conclusive reasons to reject V=L. For my own part, I see little reason for accepting or rejecting these axioms--see Appendix A.

The fourth, and perhaps the most common position is simply that there is no good reason to believe that V=L is true.[174] Indeed, there are practically no defenses of V=L in the literature. The only one known to me is a UCLA Carnap Prize Essay by R. Van Zuyle.[175] He recommends V=L on the grounds that (1) we could never know if it were false; (2) it "solves" outstanding problems, and (3) it gives a more predicative structure to the universe of sets.

As for (1), this is simply false unless we could never have any reason to believe the large cardinal axioms which contradict it, a possibility he does not discuss. For (2), we need some reason to believe that the problems "solved" by assuming V=L are decided correctly if such solutions are to could in its favor. In considering (3) we make two points. First, V=L departs from predicativity in an important respect by making use of ordinals not previously "constructed" in the definition of L. That is, we do not have $(\alpha)\ (\alpha \in M(\alpha))$. In thus taking the ordinals as "given", we in fact take all of L as "given". The precise result is that the theory of ordinals can be given an axiomatic development in such a way that a definition of set can be given within that theory and all the axioms of ZF plus V=L can be proved. The usual development of the theory of ordinals within set theory gives the converse result as well. So, in a precise sense, the deductive strengths of set theory and ordinal theory are the same.[176] Secondly, if V=L really did give a predicative notion of set, that would be reason to believe that it is false, since such basic portions of mathematics as analysis cannot, as far as we know, be developed on a predicative basis.[177] What we are left with in (3) is the claim that a notion which is "sort of" predicative is for that reason a more plausible notion of set, and Van Zuyle provides no argument for this.

It is fair to conclude that "refining our intuitions" about the notion of set to conform to V=L can only lead to a less plausible notion of set than that which we have at present. It remains to examine Cohen's models of the negation of CH to see if they provide a natural notion of set.

Cohen's Constructions

Let us describe informally the idea of Cohen's construction of a model for $ZF+AC+2^{\aleph_0} = \aleph_2$.[178] We start with the minimal standard transitive model M. Since M is countable, and there are uncountably many sets of integers, there are plenty of sets of integers not in M. But the cardinal $\aleph_2$ in M (written $\aleph_2^M$) is actually countable. Cohen's strategy is to add $\aleph_2^M$ new sets of integers and a set which is a one-one correspondence between $\aleph_2^M$ and the continuum of M, $P(\omega)^M$. We will then have a structure in which $2^{\aleph_0} = \aleph_2$. If this structure is to be a model of ZF, we have to add certain other sets, in particular, all sets constructible from the "new" sets using ordinals in M. In order to have $2^{\aleph_0} = \aleph_2$ in the resulting structure, we have to check that we do not add enough new sets to change the cardinals of M, say be adding a one-one correspondence between $\aleph_1^M$ and $\aleph_2^M$. Cohen showed that the new structures resulting from M in this way will be models of ZF if the "new" sets are of a special sort, called generic sets. The structures resulting from adding generic sets and sets constructible from them to M are called "Cohen extensions" of M. Cohen's methods for obtaining generic sets of integers and proving that the resulting Cohen extensions form models of ZF with the same cardinals apply to other models as well as M, provided they satisfy certain conditions discussed below.

It is plain that the models we have considered so far cannot be considered natural interpretations of ZF since they are countable. Cohen's original techniques for obtaining generic sets requires that the starting model be countable, a requirement satisfied by M of the previous paragraph. Since we are interested in models which also satisfy AC, the Cohen extensions of M are countable as well.[179] Other writers have developed generalizations of Cohen's techniques which, together with plausible hypotheses, imply that there are also <u>un</u>countable models of ZF+AC+ ~CH, in fact, models of cardinality as large as desired, although this cannot be proved from ZF+"ZF has a standard model" alone.[180] These other techniques, however, still use facts about the starting model (and hence the Cohen extensions) which show that they are <u>not</u> natural interpretations of ZF. These include such characteristics of the starting model and the extension as having "uncountable cardinals" which are actually countable. Such hypotheses could be eliminated in general only if it could be shown that there are uncountable cardinals less than the cardinal of the continuum, but this is the negation of the continuum hypothesis!

This last statement needs to be somewhat more delicately put. In the techniques of Cohen and their generalizations, we need to show (1) that there exists a Cohen extension containing a one-one correspondence between the continuum and some cardinal $\aleph_\alpha$ greater than $\aleph_1$ of the starting model; (2) that $\aleph_\alpha$ is still greater than $\aleph_1$ in the extension (if the extension has this property, call it a "nice" extension). It is a consequence of a theorem of Solovay and Martin[181] that we can't use Cohen's techniques to prove that every model M has a nice extension unless the $\aleph_\alpha$ of M is actually less than $2^{\aleph_0}$. Of course, the CH says that such an $\aleph_\alpha$ of M must be actually countable.[182]

So again with the more general techniques of other authors, both the starting model and the extension have such obviously "unnatural" properties as that some of their "uncountable" cardinals are actually countable, and the construction technique depends on our knowing this.[183] This is true unless we know that the CH is actually false or--to avoid begging the question against Mostowski--that there is a natural interpretation of "set" according to which the CH is actually false. If we knew that there were any such interpretation, however, we would be interested in that, and not the tricky models conjured up with Cohen-style techniques.

Despite the failure of Cohen constructions to provide natural interpretations of ZF+AC+$\sim$CH, there is a natural "width" principle which has developed out of Cohen techniques. This is the new axiom proposed by Solovay and Martin.[184] This axiom, which they call "A", says that sets generic w.r.t. the various substructures of the universe which cardinality less than that of the continuum are already in the universe, so that the process of "adding" new sets to a model which does not contain them to obtain a pathological extension cannot be carried out. It thus seeks to counteract the very feature of the starting model of ZF which made the construction of the extension possible--that it did not contain all sets of integers.

Axiom A does not imply, but is implied by CH (since if the CH is true, all the relevant substructures are countable, and the existence of generic sets for countable structures is unproblematic). Axiom A itself implies many of the "interesting" consequences of CH. Specifically, 48 of the 82 consequences of CH which Sierpinski proves in <u>Hypothese du Continu</u>[185] are also consequences of A and the remainder become so when "denumerable" is replaced by "having cardinality less than that of the continuum".[186]

This brief discussion should indicate that A has some intuitive support as a "maximum width" principle, asserting that models of ZF+A must already contain those sets which would be artificially added by Cohen techniques. This will not help Mostowski's case, however, for it is perfectly plausible to construe this axiom as a suggestion about how to characterize the "actual" universe, not as representing a distinct notion of set. I think that is is fair to conclude that Mostowski's suggestion that the models constructed in the independence proofs give us-- or are likely to give us--essentially different intuitive conceptions of "set" simply does not stand up under examination.

<u>Undecided Questions: Do They Show Unclarity?</u>

In the previous section, we showed that the models of Godel and Cohen do not themselves provide natural interpretations of ZF at all, much less natural interpretations according to which the truth values assigned to CH differ. A different but related claim by Mostowski must also be examined, that the notion of set is shown to be "vague", "unclear", or in need of "clarification"[187] by the mere fact that a "multitude of seemingly very simple questions" (including CH) have not been decided by "two or three generations of mathematicians."[188]

Analyzing this claim requires us to do two things: (1) to analyze the claim that the notion of set is unclear, and (2) to see why the fact the "seemingly very simple" questions resist solution should tend to show that the notion of set is unclear. For (1), we have bee considering throughout this chapter the possbility of multiple equally natural interpretations. Not only is this a reasonably clear formulation of "unclarity", it is also the sort of thing Mostowski himself has in mind. The conclusion that he wants to draw from the alleged unclarity of the notion of set is that set theory can be expected to split into a multitude of different theories, only the "common part" of which "could claim a central place in mathematics". This common part, he worries, may not "contain all the axioms needed for a reduction of mathematics to set theory".[189]

Accepting the formulation of unclarity in terms of multiple interpretations, we can now put the question in (2) as: "Even if the independence proofs themselves do not provide alternative natural interpretations, don't they give us reason to believe there must be such interpretations?"

The method I will follow in attacking Mostowski's contention is to draw an analogy with number theory. That theory has its own "apparently simple" unsolved problems which have resisted solutions for generations, but no one, to my knowledge, draws the conclusion from this fact that the notion of natural number is unclear, nor should they.

The longevity of the outstanding problems of elementary number theory and the enormous effort expended in attempted solution of them is quite remarkable. Goldbach's conjecture, Fermat's "Last Theorem", and other questions in the theory of Diophantine equations[190] have been the object of study for centuries, with no reason to believe that solutions are near. Fermat's "Theorem", proposed in 1637, states that the equation: $x^n + y^n = z^n$ has no solutions for positive integral values of x, y, z if n is greater than 2. This conjecture has been verified up to fairly large n, but the general case remains unproved. Goldbach's conjecture, proposed in 1742, is that every even positive integer is the sum of two primes; it also remains unproved, although a similar conjecture, that every "sufficiently large" odd integer is the sum of three primes, was finally proved by Vinogradov in 1937.[191] It is not known that any of these open questions are actually undecided by the usual axioms of number theory. The possbility that they may be independent is suggested by the fact that some of the attempted solutions, and Vinogradov's proof, use methods from the theory of complex variables which cannot be expressed in elementary number theory and for which no elementary replacement is known. If either Goldbach's or Fermat;s conjecture is false, however, it cannot be independent, since any counter-example would be a true numerical formula without quantifiers, hence provable in elementary number theory.[192]

The fact that the conjectures are not <u>known</u> to be independent of formal number theory does not destroy the usefulness of the analogy with set theory. Since all obvious methods and many subtle ones have already failed, solutions of the outstanding problems of number theory will undoubtly require discovery of quite new principles of number theory--not necessarily principles independent of Peano's axioms, but still principle entirely novel w.r.t. present number-theoretic practice. Of course, we can be certain that any novel principles of set theory which decide CH will be unprovable in ZF (if that theory is consistent). Thus, we are certain to have some problems about the justification of any such principles which we might not have for principles which decide the outstanding problems of arithmetic. In neither case, however, is the immediate problem one of justification of available plausible principles which decide the open questions. As Mostowski puts it:

> We need new axioms to codify the intuitive set theory. The disquieting fact is that we do not know where to look for them.[193]

Actually the situation is not quite this bleak. A number of new principles have been suggested, but they either don't decide CH (the large cardinal axioms[194] and other principles we have discussed, such as "All sets are ordinal definable"[195]), or they are implausible (V=L, axiom of determinateness[196]) or they are simply suggestions to be investigated whose plausibility is unknown (Takeuti's width principles). We are plainly far from a solution to the continuum question, but it is not the case that we have no ideas.

Of course, once we have a really plausible principle for set theory which decides CH, we will still have the problem of giving a real justification for it. Just how hard this will be, we cannot tell until we have a suggestion whose justification is sought. In comparison with unsolved problems of past set theory which were eventually solved, the longevity of the CH as an open question is not particularly astounding. Cantor promised a proof that every set could be well-ordered in 1883,[197] but Hilbert still listed the well-ordering of the continuum as an outstanding problem of mathematics in 1900.[198] Zermelo gave a proof of the well-ordering theorem in 1904,[199] but this proof used the axiom of choice, which was not generally accepted for another generaltion.[200] Hence a problem independent of principles accepted when it was proposed, took more than fifty years to solve, but it <u>was</u> solved.

I conclude that set theory and number theory are analogous in this important respect, that they both have outstanding problems that are superficially elementary, that is, they can be simply stated in familiar terms. In number theory, this situation has rightly not led people to believe that there is some unclarity in the notion of natural number. Of course, we know that it will take a new idea to decide CH, but that is the case for number theory as well. We know that any such new idea for set theory will give rise to problems of justification that we may not have for number theory, but the case of the axiom of choice shows that this is not always impossible.

<u>A Final Word on Independence and Multiple Interpretations</u>

As we have noted, open problems resisting solution for centuries have not in fact raised doubts about clarity of arithmetic notions. It is interesting to note that the formal independence results for certain pathological sentences had no such effect either. One reason for this is that the independent sentences are interesting <u>only</u> because of their independence, and are obviously true.

Hypothetically, we may ask what would have happened if some independently interesting sentence had turned out to be independent? Would people have been as upset about it as about the independence of the CH? As it happens, there is a real occurrence which closely resembles our hypothetical one.

After Dedekind, the actual inventor of "Peano's" axioms,[201] had published his formulation of arithmetic,[202] Hans Keferstein, an influential German mathematician, published a critique. Keferstein suggested what amounted to abandoning the induction principle (although this was not obvious).[203] Dedekind wrote to Keferstein to justify his axioms:

> How did my essay come to be written? ... it is a synthesis constructed after protracted labor, based on a prior analysis of the sequence of natural numbers just as it presents itself, in experience... for our consideration. What are the mutually independent fundamental properties of the sequence N, that is, those properties that are not derivable from one another, but from which all others follow?[204]

Dedekind answered this question by listing the axioms on which he and Keferstein agreed: N is a set, every number has a successor, distinct numbers have distinct successors, and 1 has no predecessors. He then states that

> I have shown in my reply ..., however, that these facts are far from being adequate for completely characterizing the nature of the number sequence N.[205]

Keferstein was finally persuaded by these arguments,[206] and rightly so. His own axioms were inadequate because they failed to characterize a well-understood structure, which "presents itself... for our consideration". Given a bit of set theory, Dedekind's axioms do charcterize this structure.

In constrast, we do not find the same confidence on the part of set theorists that they are studying a given structure. There are several explanations for this. One is that a good many set theorists are formalists or are sympathetic to that view. A second is that, as we have noted, there is a variety of incompatible set theories, and it is rational to hesitate in choosing among them. Third, the structure of any theory which hopes to encompass the whole of mathematics must be enormously more complex than number theory. Given a small fregment of set theory, the natural number series and truth in that structure can be characterized quite easily. We have seen that the same cannot be said for the CTS. We were unable to give an unexceptionable characterization of either the CTS itself or truth in the whole structure.

As I have argued, this third point (and indirectly, the second) give some reason to doubt that there is a unique intended interpretation of ZF. I have also argued in the previous section that the fact of the independence results does not itself tend to show this.

There is, however, a way in which the independence results bear on uniqueness of interpretation. As the Dedekind-Keferstein controversy suggests, grave methodological worries should not be provoked by independence proofs if one is confident that a "given" structure is being studied. Of course, the independence results for CH would be sensational in any case, because they are profound technically and deal with interesting questions. But the considerable disaffection with set theory as a working basis for mathematics evidenced by Mostowski, Suppes and others, and even the conversion to formalism by Cohen,[206] and others,[207] shows that set theorists are not confident of a unique CTS. As Kreisel says,

> ... several recent results in logic, particularly the independence results for set theory, have left logicians bewildered about what to do next: in other words, these results do not 'speak for themselves' (to these logicians). I believe the reasons underlying their reaction necessarily also make them suspicious of informal rigour.[208]

Kreisel takes these "underlying reasons" to be "pragmatic" and "positivist" objections (including formalism) and the belief that intuitive notions are unreliable.[209] As I have argued, methodological queasiness over set theory cannot be put down entirely to bad philosophy of mathematics, but I do agree with Kreisel that this is probably the main reason for is prevalence among working set theorists.

Argument (A) Reconsidered

So far, our attempts to show that the premises of (A) are true, and consequently, that CH is determined, have not succeeded. These attempts proceeded by assuming a realist position (B), formulated in terms of truth, and then trying to develop a notion of truth sufficient to justify the premises of (A). Having failed to produce such a notion of truth for all sets, however, there is another course open: to explain truth only for the portion of the universe that involves the CH. This represents a major retreat from (B), for there we assumed the truth of most widely accepted theorems of ZF, (under some interpretation) and some of these concern set of arbitrarily high rank, for example, the statement "every set has a rank". Of course, the fact that we were unable to supply the account of truth for all the statement assumed true in (B) does not make that assumption unwarranted; it simply doesn't help to explain realism in set theory. In any case, we now confine our attention to truth in a small initial portion of the universe.

As CH is usually formulated, i. e., $2^{\aleph_0} = \aleph_1$, it concerns sets of moderately high rank, since $\aleph_1 = \omega_1$, and ω_1 has rank ω_1. Since we are assuming AC, we can give an equivalent formulation using sets of lower rank:

(C) $(x)(x \subseteq P(\omega) \supset (\exists f)$ ("f is a 1-1 correspondence between x and ω" or "f is a 1-1 correspondence between x and $P(\omega)$"))

It is easy to compute how much of the CTS this statement need be taken to quantify over. All members of ω are members of $R(\omega)$, so ω and all its subsets are members of $R(\omega+1)$. So $P(\omega) \subseteq R(\omega+1)$, and $P(\omega) \in R(\omega+2)$. The correspondences $\underline{f}$ are sets of ordered pairs of subsets of ω, or of such subsets and integers. With a suitable definition of ordered pair, all of these pairs have rank no higher than $R(\omega+2)$. The correspondences themselves thus have rank $R(\omega+3)$. A standard model $M = \langle R(\omega+3), \in\restriction R(\omega+3)\rangle$ containing all such correspondences then has rank slightly higher than $\omega+3$. Since M is a set, we would have no particular problem about explaining primitive denotation w.r.t. M if we could satisfy ourselves about its uniqueness. Our strategy for considering this question of uniqueness will be to analyze the first $\omega+3$ iterations of the powerset operation. Before doing so, however, I want to digress to consider another natural argument that CH is determined which seems to avoid dealing specifically with interpretations and structures, as does argument (A).

An Argument that CH is Determined:

Consider the formulation (C) of CH given in the previous section. Then we have

(1') The CH is true iff ...

and

(2') The CH is false iff it is not the case that...

where the '...' is replaced by the sentence (C). In addition, we have

(3') ... or it is not the case that ...

(with the same replacement for '...') is a logical truth. It follows that

(4') The CH is true or the CH is false.

This seems a simple solution to our problem, but in fact it will not do. Our assent to (1') and (2') is surely not reasonable if the terms and quantifiers of (C) are given just <u>any</u> interpretations. 'ω' had better denote ω and '$P(\omega)$' the powerset of ω; the range of the function quantifier had better be all 1-1 correspondences of the appropriate type, etc. But suppose that there are equally natural interpretations of any of these notions; then the argument (1') - (4') will not show that CH is determined, but it can fail to do so in several different ways, depending on exactly how we understand the premises.

If we regard 'the CH' in (1'), (2') and (4') as the name of a particular proposition, (1') and (2') may be true on some perfectly good interpretations and not on others. If, on the other hand, we regard 'the CH' as simply a name of the sentence (C), then (1') - (3') and hence (4') will be true under <u>each</u> interpretation. But the fact that (4') is true under each natural interpretation does <u>not</u> show that CH is determined, for we have no guarantee that the same disjuct of (4')is true under each appropriate interpretation. So, in one way or another, we cannot avoid talking about natural interpretations of set theory; considering the fact that we have not been able to give a clear account of truth for the whole universe, let us return to the analysis of $R(\omega+3)$.

<u>Analyzing $R(\omega+3)$</u>

The first ω iterations pose no problem. All the sets thus obtained are hereditarily finite: that is, they and all their members, members of members, etc, are finite. There is a natural isomorphism of all such sets with the set of natural numbers ω, so we need have no worries about uniqueness this far.

The situation is more complicated for the next iteration, to $R(\omega+1)$, but we can still make a good case for uniqueness. We have already noted that the continuum $P(\omega)$ is a subset of $R(\omega+1)$. In fact, we can use the natural isomorphism of $R(\omega)$ and ω to construct a natural isomorphism between $P(\omega)$ and $R(\omega+1)$. The importance of this manuver is that $P(\omega)$ is, or is naturally isomorphic to, the continuum of real numbers R. Next to the natural number series, this is the most studied structure in mathematics and it is of central importance to physics. Both of these facts give reasons to believe that R (and hence $R(\omega+1)$) has been uniquely identified.

To argue this point, our first consideration is a "sociological" examination of alternative theories of the continuum of the same kind we made for set theory and number theory. There are two main types of alternatives to standard real number theory. The first is the so-called non-standard analysis.[210]

In this approach, one adds non-Archimedean field, i.e., a field with elements x such that $x \neq 0$ and for all positive natural numbers n, $|x| < \frac{1}{n}$. The theory of analysis based on this formulation of the real numbers is not, or course, exactly that of standard analysis, but it is plain that non-standard real number theory still makes use of R, although it adds other elements as well. In particular, if we consider a language for the theory of real numbers with a constant to name each real, then the non-standard models of this theory are exactly the elementary extensions of R. In fact the most interesting models of non-standard analysis are obtained by simply starting with R and using ultraproduct constructions to obtain non-standard structures with the same cardinality. Thus non-standard analysis is equivalent to standard analysis in the sense of elementary equivalence, but also in the stronger sense of elementary embedding (in our large cardinality language). Every non-standard model of this sort contains an isomorphic copy of R.[210a]

The second (group of) alternative(s) is the range of "constructive" treatments of real numbers, including the intuitionist ones. In standard analysis, one can treat reals as equivalence classes of convergent sequences of rationals, $\{x_n\}_{n\in\omega}$. Constructive analysis imposes various restrictions on such sequences, usually in the form of a recursive function f(m, n) such that $(n)(m)(|x_n - x_m| < \frac{1}{f(n,m)})$.[211] The idea of this formulation is thus to study the reals by means of sequences of rational approximations, with "constructive" restrictions on the type of reasoning permitted. Despite these restrictions, it is easy to see that the same structure is being studied. It is obvious (speaking as a "classical" mathematician) that every real number has an infinite decimal expansion. The finite initial portions of such expansions give a sequence of rationals satisfying the stated restriction, i.e., $|x_n - x_m| < 10\cdot 10^{-\max(m,n)}$. Constructive analysis based on this theory of the reals does indeed have somewhat different theorems (even when they sound the same as classical ones[212]); whether any such formulation of analysis is adequate for physics is yet to be seen.

There is a second kind of reason for confidence in the adequacy of our knowledge of R to guarantee uniqueness. This is the central role of that structure is physics and, indeed, in natural science generally. Instants of time, locations in space, and a host of physical quantities take a continuum of values. In maintaining this, I am taking a realist interpretation of physics; I will not defend such a view here--it has been extensively argued elsewhere.[213]

The fact of the physical realization of the continuum does have a particular significance for us, however, for this reason: We saw that there are special difficulties with causal theories of primitive denotation for mathematics. But if causal theories can be developed for theoretical entities at all, it is surely not absurd that human beings could causally interact with continuous physical quantities, such as energy, momentum, etc.[214]

<u>Up to the Next Rank, $R(\omega+2)$</u>

It is possible to continue our analysis to the next rank, $R(\omega+2)$, but I shall not do this, for two reasons. The first is that we could only do so by the method used for $R(\omega+1)$ with some decrease of confidence in uniqueness. The objects of established mathematics which appear at this level are the real-valued functions. Historically, the notion of "arbitrary function" of this type was only clarified by the development of set theory. The second reason is that we can show by Cohen- style methods that there is a fairly natural proposition about members of $R(\omega+1)$ which is independent of ZF.

This proposition is that $\aleph_1^L$, the first uncountable cardinal of the constructible universe, is countable. If V=L, this proposition is false; if there are measurable cardinals, it is true. Addison[215] has shown that "$\aleph_1^L$ is countable" can be given a formulations quantifying only over real numbers. The significance of this fact is that we have an independent question only a few ranks below that of the continuum question, whose independence cannot be attributed to multiplicity of natural interpretations. This is our final reply to Mostowskian arguments against uniqueness.

Conclusion

At the beginning of this chapter, we cited a simple, obviously valid argument (A) to the effect that CH is determined. I do not claim to have shown conclusively that the premises of this argument are true. But I believe that it has been shown that they are plausible given to the modest realism of (B), and that none of the objections to these premises hold water. I conclude that it is rational to be confident that CH has a truth value. Only future research can show how hard it will be to find out what it is.

APPENDIX A

Introduction

This appendix will be concerned with the plausibility---that is, the probable truth---of several principles which are independent of ZF. The principal ones which will concern us will be CH and AC, but others will come up in the course of discussing these two. Other plausibility arguments, concerning the axiom of constructibility and Martin's axiom, will be found in Chapter II. We will omit any systematic treatment of "large cardinal" axioms, making a few general remarks at the end.

Plausibility Arguments

It is beyond the scope of this appendix to attempt a systematic account of plausibility arguments in mathematics, but we must give an indication of what sort of arguments are to be taken to fall in this category.

Generally, we will take a plausibility argument to be an argument for the truth of a mathematical proposition which is not deductively valid, and therefore falls short of the standards of mathematical proof. We are, of course, only interested in those arguments which actually give or may be thought to give some reason to believe the conclusion. We will consider the following, general types and some others not easily labeled:

(A) Analogy to or generalization from known cases, for example, generalizations from theorems about finite sets to infinite ones. Such generalizations or analogies may be refuitable or there may be incompatible variants which are equally supported by analogy. We will find such cases in discussing AC and the axiom of determinateness.

(B) Arguments based on consequences in the natural sciences. Such argument have played an important historical role in the history of analysis[1], but set theory is sufficiently removed from physics that such arguments will prove to be of limited usefulness in discussing AC and CH. It is useful to describe one hypothetical example of an argument of this sort which actually gives quite good _a posteriori_ reasons for believing a mathematical proposition. Suppose that we are unable to determine whether a system of differential equations S has a solution for any parameters in some region Q, but we can prove that if S has a solution for _any_ set of parameters in Q it has one for all of them. Suppose as well that we know how to build an electrical circuit which, if it works in the intended way, is described by S. Such an assumption will no doubt involve our having sufficient reason to believe a substantial body of current physical theory. If we can build such a circuit with parameters known to lie in Q---although their exact values may be unknown---and it works, then we have shown that S has solutions for all of Q.[2] Such "experimental" verification of mathematical propositions are not entirely hypothetical, for similar verifications are in fact performed for engineering problems using analogue computers.

(C) "Geometrical" or "intuitive" arguments. Such arguments are by far the most difficult to describe or evaluate, if only because of the substantial volume of conflicting philosophical views on the epistemic importance of "intuition" in mathematics. Kant and the Dutch and French intuitionists have maintained versions of the thesis that one is justified in accepting a mathematical result only if one has had certain "intuitions." These authors suppose "intuitions" to be a type of, or akin to, preceptions. A number of leading figures in set theory have also supposed that "intuition" or "intuitions" provide the ultimate basis for accepting the axioms of set theory. Zermelo, in particular, stated flatly that set theory is a science " resting ultimately on intuition".[3] A general discussion of such issues is too ambituous a project to take up here, but I believe it possible to admit some examples of arguments "based on intuition" without adjudication of the larger questions. For, as a practical matter, it is useful or even necessary to attempt to visualize mathematical structures which one wishes to study, and to use this visualization as an aide to understanding existing results and suggesting new ones. For example, in studing the real line, one may imagine it as consisting of an extremely large number of tiny objects, such as grains of sand. Imagining selections or transformations of these objects may then convey information about their mathematical counterparts.

I believe that it can be argued successfully that "intuition" of this sort is very useful indeed in the ways mentioned, without opting either for the positions of the intuitionist schools or those like Zermelo's. Detailed studies of the history of analysis such as Boyer's [4] make it clear that intuitive notions did in fact play an enormous role in the development of analysis before and during the foundational work of Cauchy,[5] Weierstrauss and Cantor which finally gave definitive formulations to the fundamental notions and proof methods in that field.[6]

It may be denied that this role was either a necessary or a useful one. In fact, one of Boyer's main contentions is that reliance on intuitive notions retarded the precise formulation of the concepts of limit, derivative, etc. But far from being incompatible with the claim that "intuition" can be a reliable guide <u>before</u> fundamental notions receive a definitive formulation, this contention tends to support it. The fact is that reliance on intuitive notions did not make the practice of analysis impossible, even if it retarded further development of that field.

During an extended period--several hundred years---in which the difficulties in analysis were at least as great as those in naive set theory before resolution of the paradoxes, a rich theory of great practical ultilty was developed and the overwhelmingly greater part of this theory remained intact after the precise formulations were finally developed. It seems eminently reasonable to maintain that the influence of "intuition" in early analysis was generally beneficial and, at the time, necessary to research.

That "intuition" has played this role in analysis does not decisively refute the contention of philosophers such as Quine who have maintained that "intuition is bankrupt" in set theory,[7] but the fact of the utility of "intuition" in a field with strong logical and historical ties with set theory suggests that intuition may play a role here as well. This is really beside the point we will need, however, because the "intuitive" arguments which we will examine will actually be about consequences of AC and CH in analysis itself.

Despite the previous argument to the effect that "intuitive" arguments can be of some value, this writer doubts that propositions supported only by considerations of types (A) or (C) can be regarded as well established. Persuasive arguments of type (B) can probably be constructed only in a relatively small range of cases. All three sorts of arguments are often put to a different purpose, that of suggesting new directions for investigation. No doubt it is easier to establish the conclusion that such and such is worth looking into than the stronger assertion that such and such in probably true.

THE AXIOM OF CHOICE

Origin and Early Controversies

The axiom of choice states that for every set S of mutually exclusive non-empty sets, there exists a set containing exactly one member of each set in S. Although several earlier authors mention related principles,[8] the axiom was first introduced by E. Zermelo, who used it in his proof that every set can be well-ordered.[9] This proof met a number of objections against AC, mainly from the French intuitionists. Borel, in particular, maintained that reasoning which presupposed, as he initially thought AC did," an arbirtary choice made a non-denumerable infinity of times" is "outside the domain of mathematics".[10] The publication of Borel's remarks provoked a series of exchanges among French mathematicians, the so-called Cinq lettres, in which Borel, Baire and Lebesgue attacked the "principle of Choice", and Hadamard defended it.[11]

In this debate, the main issue which emerged was whether the sets whose existence is guaranteed by the axiom might fail to be definable. Poincaré pronounced the special case of the well-ordering theorem for the continuum "devoid of meaning or false or at least not proved"[12] on just such grounds.[13] All of these opponents of the AC objected as well to other aspects of Cantorian set theory which are now hardly controversial. Borel, for example, regarded Cantor's theory of cardinality as having little "intrinsic value", although it " served as a guide to more serious reasoning."[14]

Well-ordering and the Continuum Hypothesis

One consequence of the well-ordering theorem which particularly concerns us is that the continuum can be well ordered. For otherwise, $P(\omega)$ has no cardinal at all in the sense we have been employing (von Neuman's) and in the alternative sense of cardinal of Frege-Russell, it would turn out that the cardinal of $P(\omega)$ is not equal to and not less than any $\aleph_\alpha$, so is larger than any $\aleph_\alpha$ to which it can be compared under the "greater than" relation for cardinals.[15] Thus in either case, the Aleph Hypothesis (AH) that $2^{\aleph_0} = \aleph_1$, which, lacking AC, we must momentarily distinguish from CH, cannot be true if the continuum cannot be well-ordered. For von Neumann Cardinals, it doesn't even make sense" since '$2^{\aleph_0}$' does not denote, and for Frege-Russell cardinals, it is about as false as it could be. The continuum hypothesis proper, that there are no cardinals properly between $\aleph_0$ and $2^{\aleph_0}$, fares just as badly. For von Neumann cardinals, it still doesn't make sense. For Frege-Russell versions, CH will be false if any $\aleph_\alpha$ with α greater than 0 is comparable with $2^{\aleph_0}$.. Otherwise, we will have even less hope for solving the continuum question than with AC.

Since the continuum and the series of natural numbers are perhaps the two most important structures in all of mathematics, the inapplicability of Cantor's theory of $\aleph$s to one of them would substantially reduce the interest and practical importance of that theory. This should certainly count as an undersirable consequence of a failure of AC radical enough to result in the non-well-orderability of the continuum.

Later controversies about AC

Four years after his first proof of the well-ordering theorem, Zermelo published a new proof and a set of replies to objections to his first paper, dealing in particular with objections to AC. Zermelo maintained that AC meets the same criteria as other axioms in that it is "intuitively evident and necessary for science.[16] In support of this contention, he cites implicit use of the principle in the work of earlier invertigators, including Cantor.[17] As we shall see below, what is essentially Zermelo's view has become the dominent one, but other developments were to come first.

The objections of the French intuitionists to AC were, in effect, objections to non-constructive existence arguments, that is, they were of a general, methodological character. Later objections were based on supposedly implausible consequences of the axiom. In 1905, Vitali proved that AC implies the existence of a set of real numbers which is not Lebesgue measureable.[18] This result can also be obtained on the weaker hypothesis that the continuum can be well-ordered (WOC).[18] So, by the remarks of the previous section, 'Every set of reals is Lebesgue measureable' (LM), implies not-CH, since it implies not-WOC. LM has proved to be an attractive, if not particularly plausible, hypothesis.

In 1924, Tarski and Banach cast the existence of a non-measurable set in a striking form. They showed that, assuming AC, the unit sphere can be decomposed into a small finite number of "pieces," which can be "reassembled" by translation and rotation into two unit spheres.[20] At first glance, at least, this seems an absurd result---that an object can be cut into "pieces" which when rearranged yield two objects the same size as the first. This appearant absurdity can be at least partially explained away by pointing out that the "pieces" (sets of points) in question are very far from corresponding to any intuitive notion of "piece." They are, for example, neither open nor closed sets, nor sets formed from finite or countable unions or intersections of such sets, or any other sets even remotely resembling pieces cut with scissors. The result might still be troubling if one only had to apologize for lack of correspondence with intuitive notions in connection with AC. This is not the case. There are a number of "paradoxical" results in elementary analysis which require similar explaining away.[21] One such well known example is Peano's "space-filling curve", a continuous curve which touches every point of an open rectangle in the plane.[22] This result is certainly counter-intuitive, but no one has seriously proposed that it shows some serious inadequacy in the notions involved or in the principles---which do not include AC--used to prove it. Single examples are still occasionally used, however, to argue that AC is implausible. Fairly recently (1947), Borel used the standard non-measurable set construction minus the measure-theoretic terminology to obtain a decomposition of the unit interval of the real numbers into countably many sets which are all translations of one another. This example forces us, he claims, to either give up the AC or to modify the notion of "Euclidean equilivence" (i.e., "being translations of one another" for sets). The former course is the one he recommends.[23] Such arguments are now, however, rather unusual.

Present status of AC

Discussion of the present status of the axiom can be organized under two heads suggested by Zermelo's remark that it is "intuitively evident and necessary for science." We will first review in just what sense AC may be necessary for science, arguments which are broadly speaking of type (B).

Several authors have reviewed elementary analysis to see to what extent AC may be necessary for standard results. Fraenkel and Bar-Hillel list a number of propositions of the type taught in beginning analysis courses whose proofs require AC.[24] A more careful survey has been made by Rosser, who claims that AC for countable sets (CAC) is necessary to develop to theory of Lebesgue measure.[25] CAC is not, however, sufficient to prove the existence of a non-measurable set.[26]

Outside analysis, numerous useful principles and some standard theorems of topology and algebra are equilivent to AC,[27] and some propositions weaker or apparently weaker than AC have no known proofs from principles better established than AC.[28]

The role of AC in set theory itself is especially important, particularly in such basic areas as the theory of cardinality. Comparability of all Frege-Russell cardinals is equilivent to AC,[29] and only sets which can be well-ordered have von Neumann cardinals.

We have already seen what havoc is wrecked in this area if crucial consequences of AC are unavailable.

Apart from the theory of cardinals, it should be remembered that AC is implied by the (apparent) restriction of the notion of set either to constructible or to ordinal definable sets, as we have already noted elsewhere. That AC holds of such semi-constructive notions suggests that the objections of the French intuitionists on the ground of definibility may not have been well taken even if their general prospective is accepted.

We may summarize this section as follows: AC is "necessary for science" in the sense that it is necessary for a great deal of mathematical science that most mathematicians accept. Although there are some who are willing to give up those portions of contemporary mathematics dependent on AC, they generally also reject other portions of "classical" mathematics as well. Many more accept the attitude of Hilbert that AC "rests on a general logical principle which is necessary and indispensable for the first elements of mathematical inference."[30]

AC and Physical Science

Given that AC is essential for some fundamental sorts of mathematics, it is interesting to ask whether the truth of some of this same mathematics may not be a necessary condition for the truth of parts of physical theory which we have good reason to believe true. If this were the case, we might construct a rather loose argument of type (B) for the truth of at least some weakened version of AC. The ever increasing volume of mathematics required (at least in a practical sense) for contemporary physics suggests that this might be the case.

There are counter-indications, however, in the development of "constructive" analysis, including a version of Lebesgue measure theory, by E. Bishop[31] and, independently, by a group of Soviet analysts,[32] without using AC or other "non-constructive" principles. The work of these writers contain theorems which sound like the standard theorems of classical analysis, but often this is deceptive, for some of them are very far from meaning the same as their classical counterparts.[33] Getting to the bottom of this particular question is plainly beyond the scope of the present inquiry.

"Intuitive Evidence" of AC

The question of the types and strength of plausibility arguments which may support AC is complicated by the following fact: much of the widespread agreement to accept AC is undoubtedly the result of its indispensibility for results of considerable elegance and utility of the types already noted. Many of these results now have a place in the training of young mathematicians such that their unattainability would now seem counter-intuitive in itself. Consequences of AC formely adduced as evidence against it, such as the Banach-Tarski theorem, tend now to be treated in elementary courses and texts as amusing curiosities, and not mentioned at all in more advanced treatements.

Cohen has drawn the conclusion from such facts as these that the axiom has won acceptance out of "philosophical opportunism",[34] by which he means accepting a principle for it aesthetic, financial or other pleasing consequences.[35] Those who, like Cohen, believe that it is at least extremely problematic whether we have grounds for believing AC to be true might profitably ask themselves whether the situation is any better for other powerful axioms of ZF, say Replacement or Powerset.

Each of these is independent of the remaining axioms of ZF, and one would be hard put to give a conclusive argument for their truth in every case which made no appeal to utility or elegance of resulting mathematics. Eliminating Powerset entirely would undoubtedly leave us too little mathematics for physics, but such an arguement will certainly not do as a justification for <u>all</u> applications of Powerset, especially in conjunction with Replacement. Why, for example, must there exist a powerset of $\mathbb{R}(\omega+\omega)$? As far as one can see now, sets this "far out" in the ranks have no physical significance and play no role in classical mathematics.[36] The ordinal $\omega+\omega$ itself cannot even be proved to exist without Replacement. Restricting Powerset or abandoning Replacement would be inelegant (and perhaps <u>therefore</u> implausible) in the former case and inconvenient in the latter, but other considerations are not easily forthcoming.
It is possible that, if pressed, Cohen might concede that the acceptance of Powerset and Replacement is opportunistic as well,

since he is defending a formalist position. But this leaves no room for objection to AC in particular.

Cohen is right in this much, that the "satisfactoriness" of the resulting set theory as a working basis for the rest of mathematics has favorably influenced peoples' attitudes about AC. While it is true that not every reason for accepting an axiom is a reason to believe it true, we will only reduce ourselves to an impotent skepticism if we label arguments based on "satisfactoriness" as mere opportunism, since we can hardly do better for other axioms.

For realist positions, at least, why the most satisfactory theory--in the stated sense for set theory, or others for others areas of science---should be the most plausible remains an unsolved question in the "theory of science". For formalists ones, the question of probable truth of such theories does not arise. In any case, we will side-step all such problems, and inquire whether any alternative to AC may yield an equally satisfactory set theory, using whatever plausibility arguments may be available.

<u>Alternatives to AC--Their Relative Plausibility</u>

Few principles yielding any hope as satisfactory alternatives to AC have been proposed by its opponents. In 1927, Church listed two alternative "assumptions", but suggested no arguments to support them.[37] Nor can this writer see any reason to find them attractive.

AC and its equilivents such as the mulplicative axiom, which says that the cartesian product of arbitrarily many non-empty sets is non-empty, are generalizations of principles provable in ZF for finite sets. Until fairly recently, AC represented the only really attractive such generalization, but there are now others in the field. To describe them, we need to explain some notions from the theory of games.

The axiom of determinateness

We can describe informally a positional game of perfect information as follows. Two "players" alternately "choose" successive elements of a sequence from a set M of possible "moves". In making his "choices", each "player" is assumed to know the sequence of "moves" up to that point. This is the "perfect information" clause. Player I moves first, and if the sequence is infinite of order type greater than ω, he also "chooses" at limit ordinals. Player I "wins" if, when the sequence is completed, it is a member of a previously designated set of winning sequences P; otherwise, player II "wins". The game thus described is designated $G^{\alpha}_{M}(P)$, where α is the sequence order type. We also consider the modifications to $G^{\alpha}_{M}(P)$ in which (a) at his "turn", the first "player" may choose any finite or empty sequence of elements of M (written $G^{\alpha}_{M}(P)^{*}$) or (b) each player may choose a finite, non-empty sequence from M (written $G^{\alpha}_{M}(P)^{**}$).

If both M and the sequence length are finite, it is a theorem of ZF that one player can always "win" the game $G^{n}_{M}(P)$, no matter what choices his opponent makes and what sequences are in P.

In this case, $G^n_M(P)$ is said to be determined for all P, written D^n_M, and similiarly for other games. This determinateness theorem will not tell us which player has the perfect strategy, for that will depend on the particular set of sequences P.

The most obvious generalization of this theorem to the infinite case is D^α_M for all α and all M. This statement is inconsistent with ZF,[38] as are a number of other ambitious generalizations, including $D^\omega_{\omega_1}$, $D^{\omega_1}_2$,[39] $D^{\omega *}_{\omega_1}$, $D^{\omega **}_{\omega_1}$.[40] Several other generalizations are more interesting. "For all M, D^2_M" is equilivent to AC.[41] D^ω_2, which is equivilent to D^ω_ω, and inconsistent with ZF+AC, is called the axiom of determinateness (AD).[42]

Whether AD is consistent with ZF (without AC) is not known, and if true, it will be practically impossible to prove, since Con(ZF+AD) implies Con(ZF+AC+ "there is a measurable cardinal").[43] Thus, proving Con(ZF+AD) will require principles stronger than the most "outlandish" currently considered principles. In fact, AD implies that $\aleph_1$ is measurable.[44]

AD was origionally proposed by Steinhaus and Mycielski in 1962,[45] and Mycielski has studied its consequences extensively. It must be said that some of these consequences dealing with the properties of the continuum of the kind studied in descriptive set theory are extremely interesting. We list a few here:

(1) Every set of real numbers is Lebesgue measureable.[46]

(2) The AC holds for every denumerable set of disjoint non-empty sets whose union has power less than or equal to $2^{\aleph_0}$.[47]

(3) Every non-denumerable subset of the Cantor set has a perfect subset.[48]

(4) ω_1 is regular but there is no choice set for the Lebesgue decomposition of the real line.[49]

(5) D^{ω}_{ω}, $D^{\omega *}_{\omega}$, $D^{\omega **}_{\omega}$, $D^{\omega *}_{2}$, $D^{\omega **}_{2}$.[50]

Comments on (1) - (5): As we have already remarked, (1) implies not-CH, and also the falsity of the Tarski-Banach theorem. Mycielski shows that AD implies in addition that $2^{\aleph_0}$ and $\aleph_1$ are actually incomparable.

(2) is a special case of CAC which Mycielski claims to be "sufficient for most applications of the axiom of choice in analysis."[51] In support of this contention, he shows that (2) implies the countable additivity of Lebesgue measure.[52]

(3) is a particular case of the AH, which we must here again distinguish from CH. Since every non-empty perfect set has cardinality $2^{\aleph_0}$,[53] (3) states that every uncountable subset of the Cantor set has cardinality $2^{\aleph_0}$. If $2^{\aleph_0}$ and $\aleph_1$ were comparable, the Cantor set would have to have a subset of power $\aleph_1$, and we would have $2^{\aleph_0} = \aleph_1$, but also, we do not have comparability.

As for (4) and (5), (4) is one of Church's alternatives, and (5) lists generalizations of AD (that is, D^{ω}_{2}) to countable M's and the '*' and '**' modifications of positional games.

Plausibility of AD

Mycielski evidently regards these "many interesting implications" of AD as real advantages of ZF+AD over the "'sad facts' following from the axiom of choice, such as, e.g., paradoxical decompositions of the sphere."[54] He had also wished to obtain an "infinitistic rule of De Morgan," but was unable to find a consistent formulation of AD with this consequence.[55]

In a later paper, Mycielski proposed another axiom to supplant AD, which we shall call AD', which also implies (1)-(5). AD' is $D_R^{\omega^*}$, where R is the set of real numbers. According to Mycielski, AD' recommends itself because "the consistency of this new form is a conjecture which is much better founded"[56] than that of the old AD. The basis for this claim is that an important special case of AD' is a theorem of ZF augmented by a version of AC implied by AD'.[57] This special case is that the game G_{R*}^{ω} (P) is determined if P is an analytic set of sequences.[58] It should be noted that the consistency of AD' would be as difficult to prove as that of AD, since it still implies consistency of ZF+AC+" there is a measureable cardinal".

Mycielski makes it clear that he has arrived at AD--and AD'-- by casting about among incompatible conjectures, trying to find one which will not be plainly inconsistent with ZF. It is in order to ask what guiding principles might govern this selection. Mycielski and Steinhaus offer the following "intuitive justification" for their choice:

> Suppose that both players I and II are infinitely clever and that they know perfectly well what P is, then, owing to the complete information during every play, the result of the play cannot depend on chance. [AD] expresses this.[59]

The great difficulty with this "justification" is that it suggests no relevant difference between AD or AD' and other generalizations from the finite case, such as $D^{\omega}_{\omega_1}$, which are inconsistent with ZF. Why, for example, should increasing the cardinality of possible moves to $\aleph_1$ make the outcome "depend on chance"?

Strangely, Mycielski and Steinhaus regard AD as false in the "classical universum of sets," because they regard AC as true in this "universum." They propose that their axiom be considered as "a restriction of the classical notion of a set leading to a smaller universum, ... which reflects some physical intuitions which are not fulfilled by the classical sets."[60] They cite the Tarski-Banach theorem as an example of not "fulfilling physical intuitions."[61]

The view that both AC and AD might be true in different "universa" closely resembles positions defended about CH which we have discussed in Chapter II. Mycielski and Steinhaus do not, however, suggest that the "universum" appropriate to AD contains "sets" which are not in the classical one. They state that the notion of set appropriate to ZF+AD is a restriction of the classical notion.

Taking this attitude lets Mycielski and Steinhaus "off the hook" as far as justifying their axiom as against the powerfully supported AC, but it also confines AD to the status of a curiousity since they admit that it is false according to the "classical", that is, the usual notion of set. On the other hand, Mycielski's and Steinhaus' clear intention is to propose a new axiom of mathematics to be used by mathematicians not interested in the curiousities of foundational research, and to accept the "sad fact" of the failure of AC as a consequence of this recommendation.

The philosophical attitudes which best accord with such recommendations are either to hold that the question of the truth (as opposed to the utility) of the new axiom does not arise--a formalist attitude--or to maintain that AD or AD' is, or may be true. That AD might be true under a reinterpretation of set theory aides neither view. Since we reject formalist positions under the assumptions of the present work, and indeed Mycielski and Steinhaus do not suggest that this is their view either, we are left with the latter view. But here we find little reason to accept determinateness. The only positive advantages cited for AD are the Lebesgue measurability of all sets of reals, and similar propositions, and we have already noted the highly unpleasant consequences of such results. Mycielski and Steinhaus suppose that these consequences to be advantages of AD because they are "interesting" and because they contradict the Tarski-Banach theorem. But I have already argued that this theorem is no more paradoxical than theorems of analysis not requiring AC.

A final unsettling fact is the very real possibility that either AD or AD' or both will turn out to be inconsistent with ZF.

We know that this particular problem cannot arise with AC.

Since Mycielski and Steinhaus published their work, set theorists have shown a good deal of interest in the technical consequences of AD and its variants.[62] It has been shown, for example, that various nice properties provable for sets at some level of the analytical heirarchy can be proved for all sets at some higher level in the heirarchy by assuming AD.[63] Other investigations have been carried out on the relation between various versions of determinateness and large cardinal axioms. Martin, for example, has shown that if there is a measurable cardinal, then every Σ^1_1 (analytic) set is determined,[64] a result parallel to that obtained by Mycielski for AD'.[65] Davis has proved in ZF+AC that certain simple Borel sets are determined.[67]

All these results are technically interesting, but they do not bear directly on the plausibility of AD or AD'. They merely show that these hypotheses are mathematically interesting.

There is one other reason to regard AD as an interesting proposal, however. Since both it and AC are supported by "generalization" arguments from finite cases not obviously differing in strength, we are oblidged to disregard such arguments for AC. The case for AC must then rest on an appreciation of its consequences of all kinds, that is, on the "satisfactoriness" of ZF+AC.

THE CONTINUUM HYPOTHESIS

In contrast to the extensive literature arguing for or against AC or presenting plausible or implausible consequences of it, much less of either kind of material is available for CH. As we noted in the Introduction, a number of earlier authors, including Cantor, Konig, and Hilbert were sufficiently confident of the truth of CH not only to attempt to prove it, but to announce that they had done so. As it began to seem that CH might not be a theorem of ZF and, simultaneously, more consequences of CH became known through the work of the Polish school of set theorists, some authors began to suggest that CH might be false. Lusin seems to have been the first to deny CH outright proposing an hypothesis whose "certitude appears to me to be beyond doubt"[68] which obviously implies not-CH.

Modern writers who have expressed opinions about CH almost uniformly disbelieve it. Godel has given extensive arguments against it which we will examine below.[69] Cohen maintains that the point of view "which may eventually be accepted is that CH is obviously false."[70] Cohen supports this contention by stating that since the continuum is "generated by a totally new and more powerful principle, namely the Powerset axiom", it is "unreasonable to expect that any description of a larger cardinal which attempts to build up that cardinal from ideas deriving from the Replacement axiom can never reach [the cardinal of the continuum]."[71] Cohen presents no arguments for adopting this attitude toward the Powerset axiom, but merely expresses the hope that "future generations will see the problem more clearly and express themselves more eloquently."[72] In what follows, we will be mainly concerned with arguments against CH presented by Godel, and with certain plausible consequences of CH not mentioned by him.

Consequences of CH

In the main, the consequences of CH are neither as far-reaching nor as striking as the consequences of AC examined in the previous section. Generally, CH plays the role of a simplifying assumption in otherwise less manageable situations. This is hardly surprizing, since CH is, in an obvious sense, the simpliest hypotheses about the position of $2^{\aleph_0}$ in the series of $\aleph$'s. This simplifying role is exploited in a specific way to obtain a number of the consequences of CH which will be cited below: "The continuum hypothesis permits intricate constructions because in a well-ordering of R of type $\aleph_1$, any segment is countable and so easy to handle."[73] GCH also plays a simplifying role in areas requiring intricate calculations with cardinals. In model theory, it is common to be able to prove powerful results on the assumption of GCH, and not even severely restricted versions of these without it.[74] One cannot convert this simplifying role of (G)CH into a plausibility argument in its favor without a liberal dose of wishful thinking or some analysis of the plausibility of the particular results obtained. Prefering the latter, we now turn to specific examples which are interesting either for their own sake or because they are cited by Godel as implausible. Most of these propositions are contained in Sierpinski's compendium of

consequences and equilivents of CH, Hypothese du Continu.[75]
We give these in the form of a list of theorems of ZF+CH, followed by commentary.

Non-Measureble Sets of Reals

(1) There is a set of real numbers which is not Lebesgue measureable.

We have already seen that (1) is a consequence of AC as well as of $2^{\aleph_0} = \aleph_\alpha$, for any $\alpha \geq 1$. CH, or the much weaker hypothesis that $2^{\aleph_0}$ is not greater than or equal to any weakly inaccessible cardinal yields:

(2) (Ulam) Every finite-valued measure defined on all subsets of R which is zero for sets consisting of a single point vanishes identically for all sets of reals.[76]

It follows that no modification of Lebesgue measure will change the situation in (1). Although it is a consequence of ZF that Lebesgue measure can be extended to any particular set of reals,[77] CH implies that

(3) (Banach) There is a countable family of sets of reals to which Lebesgue measure cannot be extended.[78]

All but perhaps the last of these assertions can already be obtained from ZF+AC, so if we accept AC they will be irrelevant to the plausibility of CH. If AC is held to be in doubt, they amount to minor drawbacks of CH. Ulam's construction in the proof of (2) will appear in a more interesting context in the proof of proposition (11).

Relations of Measure and Categroy

(4) (Erdos) Assuming CH, there is a one-one mapping f of R onto itself such that $f = f^{-1}$ and $f(E)$ is of first category if and only if E has measure zero.[79]

Both "measure zero" and "first category" are ways of making precise the notion of a very "small" or "sparse" set of reals, the former from measure theory and the later from topology. Although neither class of sets in contained in the other, there are wide-ranging analogies between their properties and many close parallels between the main results for each sort of "small" set.[80] In view of (4), CH permits us to assert a kind of "duality principle" which gives a precise sense to this parallelism:

(5) True propositions (expressed in a language appropriate for the field of sets of reals) about sets of reals remain true when 'measure zero' and 'first category' are interchanged.[81]

Existence of "Sparse" Sets

The following are consequences of ZF+CH, where E is the interval $[0,1]$:

(6) (Sierpinski) There is a set of reals X of the power of the continuum whose intersection with every perfect set A is of first category on A.[82]

(7) (Lusin & Sierpinski) There is a set of reals Y of the power of the continuum whose image under every continuous mapping of R into itself has measure zero.[83]

(8) (Sierpinski) There is a set of reals Z of the power of the continuum such that for any infinite series of reals $\{a_n\}_{n \in \omega}$, Z

can be covered by a set of intervals such that the length of the nth interval is a_n.[84]

(9) (Sierpinski) The sets of reals X, Y, & Z of (7), (8) &(9) can be chosen so that they coincide except for countably many points with every translation of themselves along the real line.[85]

(10) (Lusin) There is a set of reals W of the power of the continuum which is covered except for countably many points by every dense set of intervals.[86]

(11) (Ulam) For each subset M of E, of the power of the continuum there is a countable family a_i of decompositions of M into continuum many sets with the following properties:

(a) for all i, $a_i = \{A^i_x\}_{x \in R}$ and $M = \bigcup_{x \in R} A^i_x$

(b) for all i, $A^i_x \cap A^i_y = \emptyset$ if $x \neq y$

(c) for all i, x, A^i_x has the power of the continuum

(d) for any increasing countable sequence $\{i_j\}_{j \in \omega}$ and any countable sequence $\{x_j\}_{j \in \omega}$, $M - \bigcup_j A^{i_j}_{x_j}$ is at most countable.[87]

The "Implausibility" of (6) - (11)

Godel asserts that propositions (6) - (11) are "highly implausible,"[88] and thus "indicate that Cantor's conjecture will turn out to be wrong."[89] He also claims that (10) and (11) remain implausible when 'power of the continuum' is replaced by 'power $\aleph_1$'.[90]

Beyond the general remark that (6) - (11) assert "an extreme rareness of the sets concerned", Godel does not explain why these propositions should be considered implausible. In order to construct an argument in aide of his claim, we will need some "intuitive" explanation of (6) - (11) which indicates why they might be thought implausible.

Each of (6) - (11) can be seen to have a slightly paradoxical character in that each asserts the existence of a set large in cardinality--the power of the continuum--but smal in various other senses,and also "uniformly spread out" in various senses. In the following paragraphs, we explain why each of these propositions can be characterized in this way.

As to (6), sets of the first category are intuitively "small", but (6) also requires that X be of first category on every perfect set A contained in R. Since perfect sets can themselves be small (e. g., first category on R), (6) requires that X be a "small" portion of a number of "small" sets. On account of (9), X must also be "uniformly dispersed", i. e., that any translation ("sliding") of X along the real line will cover all but a countable portion of it.

In (7), Y is required to be "small" not only in the sense of having zero measure, but also to remain "small" under any continuous "deformation" (continuous function). Continuity is a non-trivial condition here, because since Y has the power of the continuum, it can be "deformed" into a set of any desired measure by a non-continuous function. Y is required by (9) to be "uniformly spread out" in the same sense as was X.

In (8), Z must have zero measure because the total length of the intervals, $\sum_{n=1}^{\infty} a_n$ can be made arbitrarily small. Since Z is uncountable, some interval must always cover uncountably many points of Z. This would seem to require that much of Z must be concentrated in a small area of the real line. But on account of (9), Z must still be spread out.

In (10), at most countably many points of W can lie outside any dense set of intervals of R, but there are such dense sets whose compliment is uncountable, e.g., the compliment of the Cantor set. W must therefore be both very small and very spread out.

In (11), each a_i is a decomposition of M, say [0,1] itself, into continuum many disjoint sets of the power of the continuum, but the union of any countable selection of at most one of these disjoint sets from each a_i omits at most countably many points from M. The fact that <u>any</u> such countable selection has this property suggests that most A^i_x's must be spread out along M. Since $\bigcup_{x \in R} A^i_x = M$ for each i, and the A^i_x are disjoint for each i, most of the A^i_x must be small. (11) is equilivent to the existence of a family a_i' satisfying (a), (b), and (c) of (11) and the following condition:

(d'): for each x, M - $\bigcup_{i=1}^{\infty} A^i_x$ is countable.

Certainly the plausibility of this assertion (call if (11')) is about the same as that of its equivilent (in ZF) (11). (11') is an immediate consequence of CH plus the following theorem of ZF.[92]

(12) If M is a set of reals of power $\aleph_1$, there is a family of decompositions of M into $\aleph_1$ sets A^i_α of power $\aleph_1$ such that

(a) $M = \bigcup_{\alpha < \omega_1} A^i_\alpha$

(b) $A^i_\alpha \cap A^i_\beta = \emptyset$ if $\alpha < \beta < \omega_1$.

(c) $M - \bigcup_{i=1}^{\infty} A^i_\alpha$ is at most countable for all i, all $\alpha < \omega_1$.

It is difficult to see why (12) should be any more or less plausible thatn (11) or (11'). But we have already noted that Godel finds (11) implausible even if CH is not assumed and M is taken to have cardinal $\aleph_1$. Thus we find that if we grant Godel's judgement, we infer that a theorem of ZF -- that is, (12), is implausible. This may be so, but it certainly removes any objection to similar consequences of CH.

Explaining away implausibility

In so far as the "intuitive" translations of (6) - (11) given in the previous few pages are accurate, I believe that these propositions may be counted implausible or at very least, surprizing.

Godel insists that these "implausible" consequences of CH cannot be explained away in the same way as Peano's space filling curve or other "highly unexpected and implausible"[93] geometrical results not requireing CH (or AC). This method of "explaining away" is the one we have already noted in connection with the Tarksi-Banach theorem, pointing to the "lack of agreement between our intuitive geometrical concepts and the set theoretical ones occuring in the theorems."[94]

Since Godel does not say why he considers (6) - (11) "highly implausible," it is difficult to conjecture what would count as an adequate reply to his claim. If one found a wide-spread evaluation of (6) - (11) as implausible and no supporting argument for this claim, then one could hardly explain this away by citing lack of agreement between geometrical and set-theoretic concepts, because (6)- (11) contain no commonsense geometrical terms at all. But I doubt that many will react this way to (6) - (11) without any discussion which connects these propositions with more "intuitive" notions. Suggesting such a connection was the intent of the previous few pages discussion, and Godel himself has already opened the door to this by his characterization of (6) - (9) as "asserting an extreme rareness of the sets concerned."[95] It is, of course, open to object that the approximate translations are implausible but that they inadequately represent the sense of (6) - (11). But this simply amounts to allowing the defense of inadequate correspondence to intuitive notions or it leaves us unable to evaluate (6) - (11) until some improved explanation is available. Godel has not supplied this, and I am unable to do so either.

Cardinality questions and plausibility

Since the analogues of (6) - (11) requiring the small spread out sets to be only countable are straight forward theorems of ZF, the "power of the continuum" clauses in these propositions are vital to any implausibility arguments. But it is just this notion of the cardinality of sets of reals which it is difficult to describe in intuitive terms.

There are far less arcane examples which present the same problem. It is obvious from the countable additivity of Lebesgue measure that there are countable sets of reals of measure zero. It is not so obvious that there are also uncountable zero sets.[96] Perhaps one expects sets of large cardinality to correspond more nearly to familiar geometrical "parts" of the continuum. The standard example of such a set--the Cantor discontinuum--is not the sort that a neophyte would think of in a few minutes time. It turns out that there are only $2^{\aleph_0}$ countable sets, but there are $2^{2^{\aleph_0}}$ sets of measure zero, so that the obvious examples are actually a minority of the sets of small measure. It is hard for this writer to see the relevant differences in the plausibility of (6) - (11) and such familiar examples examples except their familiarity.

Expert opinion is not all on the side of Godel in this connection. Solovay and Martin, who believe as Godel does that CH is false, confess that they "have virtually no intuitions at all about" the problematical propositions.[97] They see no immediate hope of supporting or rejecting their own axiom,[98] which also implies (6), (7) and (8).[99]

Hypotheses incompatible with CH

Gödel offers one additional reason for finding CH implausible, that "as against the numerous plausible propositions which imply the negation of the continuum hypothesis, not one plausible proposition is known which would imply the continuum hypothesis."[100] Godel does not cite any of these numerous propositions, and I find only one in the papers cited in his notes. This is Lusin's continuum hypothesis:

(13) $$2^{\aleph_0} = 2^{\aleph_1}$$

which is implied in turn by:

(14) Every set of reals of cardinality $\aleph_1$ is the compliment of an analytic set.

Lusin says that the "certainly of [14] appears to me beyond doubt."[101] but his reasons for saying this are obscure. This much is clear, that they derive from his constructivist reinterpretation of the notion of analytic compliment. According to this reinterpretation, "real" analytic compliments are distinguished from "ideal" ones, and the latter eschewed.

Since Lusin admits that the problem:

(15) are there <u>any</u> analytic compliments of power $\aleph_1$?

has resisted more conventional solution,[102] (14) is apparently among the propositions "which may appear entirely implausible [entierment invraisemblables]" on views other than his reinterpretation. The theorem on which such speculation undoubtedly relies is:

(16) Every analytic compliment (CA) is the disjoint union of $\aleph_1$ Borel sets.[103] If there is a CA set suct that the sets of the decomposition are all at most countable, then (16) implies that the answer to (15) is "yes". Lusin hoped that by restricting the notion of CA set so as to exclude the "ideal" ones, he could not only answer (15) in the affirmative, but establish (14). Since single points are Borel sets, (14) is a special case of the converse of (16). We know now that either answer to (15) is consitent with ZF+AC+CH.[104] If there is a measureable cardinal, then the answer is "no".[105]

If Lusin's (14) is accepted, there are $2^{\aleph_1}$ CA sets. Other arguments show that there are $2^{\aleph_0}$ of these, so (14) implies (13), $2^{\aleph_1} = 2^{\aleph_0}$. No arguments known to this writer other than Lusin's pretend to establish either (13) or (14), and if their is a measureable cardinal, (14) is definitely false.

Since $2^{\aleph_1} > \aleph_1$, (13) implies not-CH, so the negation of (13) is consistent with ZF+AC. Other arguments show that (13) itself is also relatively consistent with this theory.[106]

Plausibility of CH Summed Up:

CH has some plausible consequences, and a number which may or may not be implausible. Not-CH is implied by (13), but this writer finds no reason to regard this proposition as particularly plausible. Godel is undoubtedly correct in claiming that no known plausible hypothesis implies CH.

Each of the prominent set theorists who have ventured an opinion about CH, Godel, Cohen, Solovay and Martin, believes it to be false, although they cite no convincing reasons for this belief. One does find a number of mathematicians who publish results with CH or GCH as an hypothesis, and few who use hypotheses incompatible with CH, but this may be purely a matter of the greater ease of obtaining results with CH. Although the weight of expert opinion seems to be against CH, almost nothing else definite can be said.

OTHER PRINCIPLES

For completeness, we should look briefly at the plausibility arguments for the principal remaining set theoretic axioms which cannot be proved in ZF, the so-called "large cardinal" axioms. Since, as we have already suggested, sets of rank greater than $\omega + \omega$ have no apparent significance outside set theory, these axioms are far more removed from mathematical practice than either AC or CH.

The considerations occasionally urged in favor of these axioms are almost entirely of two kinds. The first is a group of generalization arguments of the following sort. One considers some property P possessed by $\aleph_0$ but not by any finite cardinal. Thus, one can regard such properties as being possessed by comparatively large cardinals. If some cardinal greater than $\aleph_0$ were to possesses this property, it would seem reasonable to regard such a cardinal as large compared to $\aleph_0$. If P is the property of inaccessibility, one obtains in this manner the axiom of inaccessible cardinals, and similarly for measureable and Ramsey cardinals.[107] Presumably, one can now construct some argument for these principles, that since there are "arbitrarily large" cardinals, there should be cardinals large enough to have P.

The second sort of generalization argument applies to those principles, particularly measureable cardinal, which permit results of the same kind as, but of greater generality than, whose available in ZF. For example, one can prove in ZF that every Σ^1_1 set of reals has a perfect subset.[108] In ZF+ 'there is a measureable cardinal', one can prove that every Σ^1_2 set of reals has a perfect subset.[109]

Both of these sorts of arguments are classificable as type (A). No argument of either other type seems possible at present. The feeling of this writer, and evidently some others, is that such considerations are entirely too tenuous to come to any even tentative conclusions.

One additional argument for measureable cardinal deserves mention, which is based on a generalization of the reflection principle. This principle is a set-theoretic version of the Lowenheim-Skolem theorem which says that every sentence $\varphi(x_1, \ldots, x_n)$ of ZF with free variables among $x_1, \ldots, x_n$ is "reflected" in a portion of the universe. That is, there is an ordinal α such that $\mathbb{R}(\alpha) \models \varphi(x_1, \ldots, x_n)$ if and only if $\varphi(x_1, \ldots, x_n)$, if $x_1, \ldots, x_n$ are elements of $\mathbb{R}(\alpha)$.[110] Reinhardt has shown that a similar principle for second-order sentences implies measureable cardinals exist, and conversely.[111] But since the Lowenheim-Skolem theorem for second-order sentences is false, we have no better reason to believe this generalized reflection principle than to believe that measureable cardinals exist.

APPENDIX B

AXIOM SYSTEMS USED IN THE TEXT

I. ZF (Zermelo-Fraenkel set theory). Language: first-order predicate calculus with one two-place relation symbol 'ϵ', read "is a member of". (Identity is included among the logical notions).

Below, we denote this language 'L_{ZF}' ; in the text, it is sometimes called simply "the language of set theory". ZF has the following axioms:

(1) Extensionality: $(x)(y)((u)(u \epsilon x \equiv u \epsilon y) \supset x = y)$

(2) Pairing: $(x)(y)(\exists z)(u)(u \epsilon z \equiv u = x \vee u = y)$

(3) Sumset: $(x)(\exists y)(u)(u \epsilon z \equiv (\exists v)(u \epsilon v \ \& \ v \epsilon x))$

(4) Nullset: $(\exists x)(u) \sim (u \epsilon x)$

(5) Powerset: $(x)(\exists y)(u)(u \epsilon y \equiv u \subseteq x)$

(6) Infinity: $(\exists x)(x \neq \emptyset \ \& \ (u)(u \epsilon x \supset (\exists v)(v \epsilon x \ \& v \subseteq u \ \& v \neq u))$

(7) Foundation (schema): $(z_1) \cdots (z_n)((\exists v) \varphi(v, z_1, \ldots, z_n) \supset$

$((\exists v)(\varphi(v, z_1, \ldots, z_n) \ \& \ (u)(u \epsilon v \supset \sim \varphi(u, z_1, \ldots, z_n))))$.

(8) Replacement (schema): $(z_1) \cdots (z_n)(x)((u)(u \epsilon x \supset (\exists ! v)$

$\varphi(u, v, z_1, \ldots, z_n) \supset (\exists y)(v)(v \epsilon y \equiv (\exists u)(u \epsilon x \ \&$

$\varphi(u, v, z_1, \ldots, z_n)))$.

In (7) & (8), n can be any integer, and φ is any formula of L_{ZF} whose free variables are among those shown such that no clashes of variables result when it is substituted in (7) or (8). ZF is not finitely axiomatizable. Standard reference: P. Suppes, Axiomatic Set Theory, New York, 1972.

II. VBG (von Neumann-Godel-Bernays set theory). The language is the same as L_{ZF} except that there are two styles of variables. Upper-case Roman letters (class variables) are primitive; Lower-case variables (set variables) are understood to be relativized to the defined predicate $M(X) = df\,(\exists Y)\,(X \in Y)$. VBG has the following axioms:

(1) Extensionality: $(X)\,(Y)((u)\,(u \in X \equiv u \in Y) \supset X = Y)$

Axioms (2) - (6) are the same as those of ZF.

(7) Replacement: $(F)\,(x)\,((\langle u, v\rangle)\,(\langle u, w\rangle)\,(\langle u, v\rangle \in F \,\&\, \langle u, w\rangle \in F \supset v=w) \supset (\exists y)\,(v \in y \equiv (\exists u)(\langle u, v\rangle \in F \,\&\, u \in x)))$

(8) Foundation: $(X)\,(X \neq \emptyset \supset (\exists u)\,(u \in X \,\&\, u \cap X = \emptyset))$

(9) Comprehension (schema): $(X_1) \cdots (X_n)\,(\exists A)\,(u)\,(u \in A \equiv \varphi\,(u, X_1 \cdots X_r$

In (9), φ is any formula of the language of VBG which does not contain A or bounded class variables. (9) can be replaced by a finite list of its instances. Standard reference on VBG: Godel, <u>The Consistency of the Continuum Hypothesis,</u> Princeton, 1940.

III. VBG (Bernay's version). Bernays' version of VBG takes L_{ZF} plus the following logical notions: (a) Class variables -- class quantification is not permitted: (b) descriptive set terms $(\text{i } x)(\varphi\,(x), \emptyset)$; and (c) class terms $\{x \mid \varphi\,(x)\}$. Operations (b) & (c) require the following axioms:

(A) $y \in \{x \mid \varphi\,(x)\} \equiv \varphi\,(y)$

(B) $\varphi\,(x) \,\&\, (y)\,(\varphi\,(y) \supset x = y) \supset x = (\text{i } z)\,(\varphi\,(y), \emptyset)$

$(\exists x)\,((\varphi\,(x) \,\&\, (y)(\varphi\,(y) \supset x = y))\ v\ (\text{i } z)\,(\varphi\,(z), \emptyset) = \emptyset$

In (A) and (B), $\varphi(x)$ is any formula in the language of Bernays' VBG, which may include free variables other than x. Of course, clash of bound variables must be avoided in choosing suitable $\varphi(x)$ in (A) and (B). Equality of sets is taken as a logical notion, but equality for classes is defined: $A \text{ eq } B \equiv (x)(x \in A \equiv x \in B)$. The set axioms are the same as (1), (4), (5) and a modified version of (6). Axiom (7) is introduced for sets only:

(7) Foundation: $x \neq \emptyset \supset (\exists y)(y \in x \ \& \ (z)(z \notin y \ \mathrm{v} \ z \notin x)$

The Replacement axiom need not be added, since the class formalism of this variant makes it a theorem. Standard reference: P. Bernays and A. Fraenkel, Axiomatic Set Theory, Amsterdam, 1958.

IV. ZF^2 (Second-order version of ZF). Language: L_{ZF} plus second-order predicate variables of one place. The new logical axioms for second-order quantification are:

(A) $(P)(\varphi \supset \Psi) \supset (\varphi \supset (P)\Psi)$ where P is not free in φ.

(B) $(P)\varphi \supset S_{\varphi}^{P(x)}$ where S is the substitution operation defined in A. Church, Introduction to Mathematical Logic, Vol. I., Princeton, 1956, p. 192, and φ satisfies the substitution restrictions in Church, op. cit. The set axioms of ZF^2 are the same as those of ZF except for (7) & (8), which are replaced by:

(7) Foundation: $(P)((\exists z)\, P(x) \supset (\exists u)\,(P(u) \ \& (x)\,(x \notin u \ \mathrm{v} \sim P(x))$

(8) Replacement: $(P)(x)((\langle u, v\rangle)(\langle u, w\rangle)(P(\langle u, v\rangle) \ \& \ P(\langle u, w\rangle) \supset v = w) \supset (\exists y)(v \in y \equiv (\exists u)\,(P(\langle u, v\rangle \ \& u \in x))$

V. VBI (von Neuman-Bernays-Impredicative set theory). This system is identical with VBG as described in section II above, except the restriction on bound class variables in axiom schema (9) is removed.

VI. STC (J.I. Friedman's set theory of "Extended Sets") The language is that of VBG with an additional one-place predicate $M(x)$, read "x is an extended set". STC has the axioms of VBG plus the axiom of Choice (see end of this appendix for AC). The axioms for the predicate $M(x)$ are:

(10) Subsets_M: $x \in M \supset x \cap Z \in M$: where $M = \{x \mid M(x)\}$ and Z is any class, possibly extended.

(11) Union_M: $x \in M \supset U_M x \in M$; where $U_M x = U(x \cap M)$ and U is "union".

(12) Powerset_M: $x \in M \supset P(x) \in M$; where $P(x)$ is powerset of x.

(13) Pairing_M: $x, y \in M \supset \{x, y\} \in M$

(14) Infinity_M: $(\exists x)\ (x \in M \;\&\; T(x) \in M \;\&$ "x is infinite") ; where $T(x) = x \cup Ux \cup UUx \cup UUUx \ldots$

(15) Universes_M: $\{x \mid T(x) \in M \cup z\} \in V$: where $V = \{x \mid x = x\}$.

(16) Maximality_M: $x \in K_\beta \supset (x \approx M_\beta \;\vee\; (\exists_\gamma < \beta)((T_M(x) \cap M_\gamma \approx M_\gamma)$

where $T_M(x) = x \cup U_M x \cup U_M U_M x \cup \ldots$, '$\approx$' means cardinal similarity,

$K_\beta = \{x \in V - M \mid T(x) \subseteq M \cup UK_\gamma\}$ (a recursive definition)

$M_\beta = \{x \in M \mid T(x) \subseteq M \cup \bigcup_{\gamma < \beta} UK_\gamma\}$

The main facts about STC are given in the text. The only reference is Friedman's paper in Math. Ann. 183(1969), pp. 232-240.

VII. NTT (Nodal Type Theory). The language of NTT is formed from L_{ZF} by the following process: Let $A(\alpha,\beta)$ be a relation on ordinal numbers which satisfies the following sentence C_A: $(\alpha)(\exists! \beta) A(\alpha,\beta) \& (\alpha)(\beta)(A(\alpha,\beta) \supset \alpha \leq \beta)$. If A is a formula of L_{ZF}, the degree of the variable X^A is 1. The degree of a set variable is 0. If n is the maximum of the degrees of variables in a formula A satisfying C_A, then the degree of X^A is n+1. The resulting atomic formulas of the language of NTT are $x \in y$, $X^A \in y$, $x \in X^A$, $X^A \in X^B$. Let $R(\alpha)$ be a rank; we define the relativation of a formula φ to $R(\alpha)$ as follows: For formulas containing no variables of degree greater than 0, $\varphi^{R(\alpha)}$ is defined as usual. For quantification of higher degree variables, we define $\varphi^{R(\alpha)}$ as follows: $(X^A)\varphi(X^A)^{R(\alpha)} =_{df} (C_A \& (\exists\beta)(A(\alpha,\beta) \& (x \in R(\alpha))(\varphi^{R(\alpha)}(x)) \vee (\sim C_A \& (x \in R(\alpha))\varphi^{R(\alpha)}(x))$. The set axioms are axioms are those of ZF (1) - (8). In addition, NTT has a new one place predicate N(X), and the following axioms:

(17) $(X)(N(X) \supset 0 \neq X \subseteq On)$ where X is a variable of any degree, and On is the class of all ordinals.

(18) $(X)(Y)(N(X) \& X \subseteq Y \subseteq On \supset N(Y))$

(19) $(X)(y)(N(X) \supset N(X - y))$

(20) $(X)((x)N(X_x) \supset (\{\alpha \mid (x \in R(\alpha))(\alpha \in X_x)\}))$ where $X_x = X'' \{x\}$.

(21) $(X)\,(N(\{\alpha \mid (x \in R(\alpha))\,(\varphi(x,X)) \equiv \varphi^{R(\alpha)}(x, X \cap R(\alpha)))\}))$

(22) the ω-rule.

In axiom (21), the formula φ is assumed not to contain the predicate N(X). The only reference on NTT is Takeuti's "The Universe of Set Theory", in J. J. Bulloff, et. al., eds., Foundations of Mathematics, New York, 1969.

VIII. NF (New Foundations). The language of NF is L_{ZF} minus identity (which is later introduced by a definition) The axioms of NF are

(1) Extensionality: $(x = y \,\&\, y \in z) \supset x \in z$

(23) Comprehension: $(\exists x)\,(y)\,(y \in x \equiv \varphi)$, where φ is any formula of the language of NF which does not contain the variable x, and which satisfies the following condition (called "Stratification"): every variable of φ can be assigned a natural number such that the same number is assigned to every occurrence of a given variable, and if m is assigned to x and n to y and '$x \in y$' occurs in φ, then $n = m+1$. Standard Reference on NF: W. Quine, Set Theory and its Logic, Cambridge, Ma, 1969 (2nd Edition).

IX. ML(Mathematical Logic). We may conveniently take the language of ML to be that of VBG, without identity, which, as with NF, is to be introduced by definition. As with VBG, we understand lower-case variables to be relativized to elements; i. e., $(x)\,(\cdots$ is to be read: $(X)\,(\exists Y)\,(X \in Y \supset \cdots$. The axioms of ML are then:

(1) Extensionality: $(X = Y \,\&\, X \in Z) \supset Y \in Z$

(24) Sethood (as in NF): $(\exists x)(y)(y \in x \equiv \varphi)$, where φ does not contain the variable x, is stratified, and all free and bound variables of φ are lower-case.

(25) Comprehension: $(\exists Y)(x)(x \in Y \equiv \varphi(x))$; φ need not be stratified. General Reference: see under NF.

X. TSS (Theory of Semi-Sets) The language of TSS is the language of VBG. We take as axioms (1), (2), (6), (9) of VBG. In addition, we take the axiom of separation:

(26) Separation: $(x)(\exists y)(z)(z \in y \equiv z \in x \ \&\ \varphi(z))$; where $\varphi(z)$ does not contain y.

Finally, we need an analogue of the replacement axiom, which it is easiest to describe in words. We say that Y is a semiset if it is contained in some set; $Sm(Y) = df\ (\exists x)(Y \subseteq x)$. Let R be a class of ordered pairs which is one-one and functional. Let Dom(R) be the domain of R: $\{x \mid (\exists y)(\langle x, y\rangle \in R)\}$. We say that R is <u>regular</u> if for all x, $R''\{x\}$ is a semiset. We say that R is <u>nowhere constant</u> if

$$(x)(y)((x, y \in Dom(R) \ \&\ x \neq y) \supset R''\{x\} \neq R''\{y\})$$

If R is regular and nowhere constant, it is called an <u>exact functor</u>. Our new axiom then states that:

(27) If R is an exact functor, then the domain of R is a semiset iff the range of R is a semiset.

If we add to TSS the axiom "Every semiset is a set", the resulting theory is equivalent to VBG minus Foundation. Reference: P. Hajek, "On Semisets", in Gandy and Yates, <u>Logic Colloquium</u> '69, Amsterdam, 1971, pp. 67-76.

XI. Other Principles:

(28) The axiom of choice: $(x)(\exists f)\,(u)((u \neq \emptyset \in x \ \&\ ``\ f$ is a function) $\supset$ $f(u) \in u)$

(29) The axiom of Determinateness--see Appendix A, p. 179.

(30) The axiom of constructibility--see Chapter II, p. 143.

(31) "Every set is ordinal definable": This axioms says that every set x is first-order definable over the rank $R(\alpha)$, for some α.

(32) Measurable Cardinal: Let κ be an uncountable cardinal. Suppose that there is a function μ: $P(\kappa) \to \{0,1\}$ such that for every pair-wise disjoint set of subsets of κ $\{x_i \mid i \in I\}$ such that the cardinality of I is less than κ, the following conditions are satisfied:

(a) $\mu(\bigcup_{i \in I} x_i) = \sum_{i \in I} \mu(x_i)$

(b) $\mu(\kappa) = 1$: $\mu(\{\sigma\}) = 0$ for $\sigma < \kappa$.

Such a function is called a measure on κ. If κ has a measure, it is said to be a measureable cardinal.

(33) Regularity: A cardinal is said to be regular if it is not the union of fewer cardinals of smaller cardinality.

(34) Inaccessibility: A cardinal is said to be (strongly) inaccessible if it is regular and cardinally greater than the powerset of any smaller ordinal.

APPENDIX C

EQUIVALENCE OF VBI AND $ZF^2 + E$

Proposition: If φ is any formula of VBI and φ^* is the formula of ZF^2 which results from replacing every atom of φ of the form $x \varepsilon X$ by the predicate variable $X(x)$, then $ZF^2 + E \vdash \varphi^*$ iff VBI $\vdash \varphi$.

A sketch of the proof: Since all the axioms of VBI except the class comprehension axiom schema are converted into axioms of $ZF^2 + E$ by the $*$-transformation (and conversely for the non-logical axioms of $ZF^2 + E$ under the inverse transformation), we need only prove the following two lemmas which relate the comprehension schema of VBI to the substitution schema of ZF^2:

Lemma 1: If $\varphi(x, x_1, \ldots, x_n, P_1, \ldots, P_k)$ is a formula in the language of ZF^2 whose only free set and predicate variables are as indicated, then $ZF^2 \vdash (x_1) \cdots (x_n)\,(P_1) \cdots (P_k)\,(\exists P)(y)$ $(\varphi(y, x_1, \ldots, x_n, P_1, \ldots, P_k) \equiv P(y))$, where y is alphabetically the first variable not in φ.

Lemma 2: If $\varphi(X)$ is a formula in the language of VBI with X free, and the substitution restrictions (Church, p. 192) for X are satisfied by the VBI formula $\Psi(x, \ldots)$, then VBI $\vdash (X)\,\varphi(X) \supset \varphi(\Psi(x, \ldots)))$.

Plainly, these lemmas permit proof of the remaining axioms of VBI(ZF^2) in ZF^2(VBI). We now sketch proofs of the lemmas.

For lemma 1, the following is the contrapositive of an instance of Church's substitution schema *509n:

$$\varphi(x, \ldots)\ \&(y)((\varphi(y, \ldots) \supset \varphi(y, \ldots))\&(\sim\varphi(y, \ldots) \supset \sim\varphi(y, \ldots))) \supset$$

(1)

$$(\exists P)\ (Px\ \ \&(y)((\varphi(y, \ldots) \supset Py)\ \&(\sim\varphi(y, \ldots) \supset \sim Py))$$

The antecedent is logically equivalent to $\varphi(x, \ldots)$; hence, by quantification theory,

$$ZF^2 \vdash \varphi(x, \ldots) \supset (\exists P)(y)(\varphi(y, x_1, \ldots, x_n, P_1, \ldots, P_k) \equiv Py)$$

On the other hand, if we replace $\varphi(x, \ldots)$ and $\varphi(y, \ldots)$ by $\sim\varphi(x, \ldots)$ and $\sim\varphi(y, \ldots)$, respectively, in (1), we obtain, in a similar manner,

$$ZF^2 \vdash \sim\varphi(x, \ldots) \supset (\exists P)\ (y)\ (\varphi(y, x_1, \ldots, x_n, P_1, \ldots, P_k) \equiv Py).$$

From these two formulas, we need only construct a dilemma to obtain

$$ZF^2 \vdash (x_1) \cdots (x_n)(P_1) \cdots (P_k)\ (\exists P)\,(\varphi(y, x_1, \ldots, x_n, P_1, \ldots, P_k) \equiv Py).$$

For lemma 2, $\varphi(X)$, $\varphi(\Psi(x, \ldots))$ are alike except that '$\Psi(x, \ldots)$' occurs in place of '$x \in X$'. $\varphi(\Psi(x, \ldots)$ satisfies the condition that no free variable of $\Psi(x, \ldots)$ is bound in $\varphi(\Psi(x, \ldots))$ except possibly x or its alphabetic variants which may arise in the substitution of $\Psi(x, \ldots)$ and we assume for the time being that X is not free in $\Psi(x, \ldots)$. Since X is not free in $\Psi(x, \ldots)$, we have

$$(\exists Y)\ (x)\ (x \in Y\ \equiv\ \Psi(x, \ldots))$$

by the comprehension axiom of VBI. Instantiating $(\exists Y)$ to A, we have

$$(x)\ (x \in A\ \equiv\ \Psi(x, \ldots)).$$

Since the only free variable in the formulas $x \in A$, $\Psi(x, \ldots)$ which may be bound in $\varphi(A)$ is x, we can use the Equivalence Theorem of quantification theory (Mendelson, p. 71) to obtain

$$(x)\,(x \in A \equiv \Psi(x, \ldots)) \supset (\varphi(A) \equiv \varphi(\Psi(x, \ldots))).$$

It is then a matter of routine quantifier logic to obtain

(2) $$(X)\,\varphi(X) \supset \varphi(\Psi(x, \ldots))$$

as desired.

In case X is free in $\Psi(x, \ldots)$, we first substitute for it some variable not used in any of the formulas of the proof, and then substitute X for that variable in (2). X does not thereby become bound in the consequent of (2) since the quantifier (X) only binds the antecedent.

APPENDIX D

"Categoricity" of $*ZF^2$

Proposition: Every $*$-model of ZF^2 is isomorphic to the entire CTS or to a natural model with first-order universe $R(\varkappa)$, where $\varkappa$ is inaccessible.

The proof of this proposition is presented here so that the reader may see how it depends on the assumption of "$*$-ness". The argument is a minor modification of an argument for VBG in Shepherdson's paper[1], which in turn relies on Zermelo's work.[2]

As we have remarked in Chapter I,[3] every $*$-model of ZF^2 is Well-founded; that is, every subclass of the FO universe has an ϵ - least element. By the Isomorphism Theorem,[4] every well-founded model of ZF^2 is isomorphic to a transitive ϵ - model, that is, an ϵ -model in which every element of the FO universe is a subset of that universe. Transitive ϵ -models M have the property that for a large class of formulas φ,

$$\varphi(x_1, \ldots, x_n) \quad \text{iff} \quad M \models \varphi(x_1, \ldots, x_n)$$

for all $x_1, \ldots, x_n \in M$. Such formulas are called absolute; all absoluteness results used in this appendix are from Shepherdson.[5] Without loss of generality, we confine our attention to transitive ϵ - models of $*ZF^2$: $M = \langle U, C, R \rangle$.

As before, we let $CTS = \bigcup_{\alpha \in \Omega} R(\alpha)$, where Ω is the class of all ordinals. Within M, we define an analogous rank function $r(\alpha)$

as usual: $r(0) = \emptyset$ & $\alpha > 0 \supset (x \in r(\alpha) \equiv (\exists\beta)(\beta < \alpha \& x \subseteq r(\beta)))$. Since 'x is an ordinal; '$x \in y$', '$\alpha < \beta$', '$x = y$', '$x = \emptyset$', and '$x \subseteq y$' are all absolute, every "ordinal" of M is actually an ordinal, and for all $\alpha \in U$, $r(\alpha) \subseteq R(\alpha)$. I. e., all the "ranks" of M are contained in the "real" ranks. We want to show that for all $\alpha \in U$, $r(\alpha) = R(\alpha)$, i. e., the universe of M coincides with an initial portion of the "real" universe. We prove this by induction on α; that is, let γ be the least $\alpha \in U$ such that for some set a, $a \in R(\gamma)$, but not $a \in r(\gamma)$. γ cannot be 0, since $M \models r(0) = \emptyset$ and '$x = \emptyset$' is absolute. γ cannot be a limit ordinal, since $R(\alpha)$, $r(\alpha)$, are just unions of smaller $R(\beta)$'s and $r(\beta)$'s respectively, if α is a limit. We can assume, therefore, that γ is a successor.

Since $R(\gamma)$ is transitive and $a \in R(\gamma)$, $a \subseteq R(\gamma)$. Since $a \in R(\gamma)$ iff every member of a is in some $R(\beta)$, for $\beta < \gamma$, and $R(\beta) = r(\beta)$ by the induction hypothesis, we also have $a \subseteq r(\gamma)$. We need to show that $a \in U$: γ is a set of M, so by the replacement axiom, $r(\gamma)$ is a set of M. Since M is a *-structure, and $a \subseteq r(\gamma) \subseteq U$, we have $a \in C$. If we let $a = P$, then , by the second-order axiom of separation, $M \models (\exists y)(z)(z \in y \equiv z \in r(\gamma) \& P(z))$. Since $M \models (z)(z \in a \equiv P(z))$ and $M \models a \subseteq r(\gamma)$, $M \models y = a$, so $y = a$, so $a \in U$. We now need $a \in r(\gamma)$.

Since γ is a successor, let $\beta + 1 = \gamma$. By hypothesis, $r(\beta) = R(\beta)$, and since $a \in R(\gamma)$, $a \subseteq R(\beta) = r(\beta)$. Since '$x \subseteq y$' is absolute, $M \models a \subseteq r(\beta)$, and then using the theorem

$ZF^2 \vdash (x)(\alpha)(x \subseteq r(\alpha) \supset x \in r(\alpha + 1))$ and the fact that '$x \in y$' is absolute, we have $a \in r(\gamma)$. This contradicts the hypothesis. Hence every *-model of ZF^2 is (isomorphic to) a model of the form $\langle R(\alpha), P(R(\alpha)), \in \restriction R(\alpha) \rangle$. We refer the reader to Shepherdson for the proof that α must be inaccessible.[6]

FOOTNOTES -- INTRODUCTION

1. "Une contribution a la theorie des ensembles", Acta Math. 2(1883), pp. 327-8. This volume and volume 4 of these same journal contain french translations of Cantor's papers thru the end of 1883. These translations, which were corrected by Cantor himself, will be used here as sources of most of Cantor's work not now available in English.

2. Cantor, Fondements d'une Theorie Generale des Ensembles, (excerpts) loc. cit., p. 395.

3. See note 1.

4. A particularly interesting effort is contained in his letter to Dedekind (1899), translated in Form Frege to Gödel, J. van Heijenoort, ed., Cambridge (Mass), 1967, pp. 113-7. Others are cited in Grattan-Guiness' introduction to "An Unpublished Paper by Georg Cantor: Principien Einer Theorie Der Ordungstypen Erste Mittheilung", Acta Math. 124 (1970), pp. 65-107.

5. König, "On the foundations of set theory and the continuum problem", van Heijenoort, op. cit., pp. 145-9, and editor's introduction, p. 145.

6. ibid., p. 367.

7. "On the infinite', ibid., pp. 369-92.

8. ibid., p. 367.

9. ZF is described in appendix B.

10. This notion is discussed in Chapter II.

11. "The consistency of the axiom of choice and the generalized continuum hypothesis", Proc. Nat. Acad. Sci. 24(1938), pp. 556-7 and "Consistency proof for the generalized continuum hypothesis", ibid., 25(1938), pp. 220-24.

12. That is, it is impossible to prove the consistency of ZF+A by "inner model" methods unless A is true in the minimal (inner) model. See L. Tharp, Notes on Set Theory, mimeograph notes, Massachusetts Institute of Technology, 1967, p. 35.

13. "The independence of the continuum hypothesis", *Proc. Nat. Acad. Sci.* *50*(1963), pp. 1143-8 and "The independence of the continuum hypothesis, II", *ibid.*, *51*(1964), pp. 105-110.

14. "Some remarks on axiomatized set theory", in van Heijenoort, *op. cit.*, p. 299n9.

15. See papers cited in notes 11, 13.

16. Levy and Solovay, "Measureable cardinals and the continuum hypothesis", *Israel Journal of Math.* *5*(1967), pp. 234-248.

17. These matters are taken up in appendix A.

18. This position is modeled on that of H. Curry in "Remarks on the definition and nature of mathematics' in *Philosophy of Mathematics: Selected Readings*, P.Benacerraf and H. Putnam, eds., Englewood Cliffs, 1964, p. 153.

19. A. Robinson, "Formalism '64" in *Logic, Methodology and Philosophy of Science*, Y. Bar-Hillel, Amsterdam, 1965, p. 230.

FOOTNOTES--CHAPTER I

1. Discussion of Bernays' "What do some recent results in set theory suggest?" in I. Lakatos, ed., Philosophy of Mathematics, Amsterdam, 1967. p. 114.

2. Bernays, loc. cit., pp. 109, 111.

3. Ibid, p. 111.

4. See, e.g., Smullyan, "Continuum Problem." in The Encyclopedia of Philosophy, Paul Edwards, ed., p. 209.

5a. Standard treatments of the Godel-Rosser results can be found in E. Mendelson's Introduction to Mathematical Logic, Princeton, 1964, pp. 142-9. The generalized form of this result used here is presented in R. Smullyan's Theory of Formal Systems, Princeton, 1961, Appendix, Theorem E'.

5. Bernays, loc. cit., p. 111.

6. For an exposition of Gentzen's proof, see Mendelson, op. cit., appendix.

7. Cohen, "Comments on the Foundations of Set Theory", in Scott, ed., Axiomatic Set Theory, Providence, 1971, p. 12, where he states that Godel's theorem is the "greatest barrier to any attempt to totally understand the nature of infinite sets", and compares this "great deficiency of all formal systems" with the independance results for CH.

8. op. cit., Chapter III.

9. Rosser, "Godel Theorems for Non-Constructive Logics", JSL 2(1937) .

10. Ferferman, "Transfinite Progressions of Axiomatic Theories", JSL 27(1963 for 1962).

11. Ibid.

12. Ibid. A similar idea is applied to ZF in Sward, "Transfinite sequences of axiom systems for set theory", in Scott, op. cit.

12 a. It might be thought that some argument using the Skolem-Lowenheim Theorem might be constructed to get around this difficulty. That this cannot be done is shown in T. Weston, "Theories whose Quantification cannot be Substitutional", forthcoming in Nous.

13. "Formalization Principle", in Logic , Methodology and Philosophy of Science, III, van Roostselaar and Staal, eds., Amsterdam, 1968.

14. See, for example, Haim Gaifman, "A note on models and submodels of arithmetic", in Conference on Mathematical Logic--London '70, New York, 1972, pp. 128-44 and works cited there.

15. Zermelo, "Investigations in the Foundations of Set Theory, I", in From Frege to Godel, A source book in mathematical logic 1879-1931, ed., J. van Heijenoort, Cambridge, Mass., 1967, pp. 199-215.

16. These axioms are stated in Appendix B.

17. Zermelo, "investigations", loc. cit., p. 202.

18. ibid., p. 201.

19. Zermelo, "Ueber der Begriff der Definitheit in der Axiomatic", Fundamenta Mathematicae, 1929, p. 339.

20. ibid., pp. 341-2.

21. Skolem, "Some Remarks on Axiomatized Set Theory", in van Heijenoort, loc. cit., p. 292.

22. A. Fraenkel, "The Notion 'definite' and the Independence of the Axiom of Choice," in van Heijenoort, op. cit., pp. 284-9. Skolem

later (1929) showed that every proposition definite according to the proposals of Fraenkel in 1922 and 1925 is definite according to his own proposal. See Wang's discussion in his "Survey of Skolem's Work in Logic", Selected Works in Logic by Th. Skolem, Oslo, 1970, pp. 36-7.

23. Skolem, "Some Remarks", loc. cit., pp. 292-3.

24. Zermelo, "Investigations", loc. cit., pp. 210-9, 11, esp. 205.

25. Zermelo, "Ueber der Begriff", loc. cit., p. 340.

26. See Skolem's "Einige Bermerkungen zu der Abhandlung von E. Zermelo: 'Ueber die Definitheit in der Axiomatic' ", Fundamenta Mathematicae, 1930, p. 22.

27. Zermelo, "Ueber der Begriff", loc. cit., p. 342.

28a. ibid.

28. Fraenkel, "The notion 'definite' ", loc. cit., p. 286. The five operations are pairing (taken to include nullset and singleton), powerset, union, and choice set, and (apparently) Aussonderungs itself (?). Other formulations by Fraenkel are outlined in Wang, op. cit.

29. Strictly speaking, this sort of recursion clause applies only to Skolem's proposal, but similar remarks apply to Fraenkel's notion.

30. Zermelo, "Ueber der Begriff", loc. cit., p. 340.

31. Bernays, op. cit., p. 117. This quotation suggest that Bernays has retreated from previous connection of the independence results with general defects of formal systems.

32. Smullyan, op. cit., p. 209.

33. ibid.

34. von Neumann, "An Axiomatization of Set Theory", in Van Heijenoort, op. cit., pp. 393-413.

35. Bernays and Fraenkel, Axiomatic Set Theory, Amsterdam, 1958, Chapter I.

36. see J. Kelly, General Topology, Princeton, 1955, Appendix.

37. Novak, "A Construction for Models of Consistent Systems", Fundamenta Mathematicae, 1950.

37a. Mostowski, "Some Impredicative Definitions in Axiomatic Set Theory", Fundamenta Mathematicae, 1950, pp. 111-124.

38. L. Tharp, Notes on Set Theory, mimeograph, M.I.T., 1965.

39. ibid.

40. The idea of the proof of this fact is that the cardinal of Fodo $(R(\kappa))$ = the cardinal of $R(\kappa)$, but using the class A defined in the text, p. 33, it is possible to define a set of "classes" of the same cardinality as $R(\kappa)$, such that if any one of these "classes" were in Fodo$(R(\kappa))$, A would be as well.

41. The idea here is to start with $R(\kappa)$ and construct a model $M \supseteq R(\kappa)$ of VBI by a "Skolem Hull" construction. M is guaranteed to have cardinality no larger than $R(\kappa)$, but $B = M - R(\kappa)$ cannot be all of $R(\kappa + 1) - R(\kappa)$, since the cardinal of this set is greater than that of $R(\kappa)$.

42. L. Tharp, "Constructibility in Impredicative Set Theory", Ph.D. Thesis, M.I.T., 1965, p. 19.

43. Mendelson, op. cit., p. 206.

43a. L. Tharp, Constructibility in Impredicative Set Theory, Ph.D. thesis, M.I.T., 1965. Solovay's proof is unpublished. R. Chuaqui, "Forcing for the Impredicative Theory of Classes", J.S.L. 37(1972), pp. 1-18, shows the independence of CH from VBI+AC using large cardinal assumptions.

44. Zermelo, "Ueber der Begriff", loc, cit., p. 343.

45. ibid.

46. ibid.

47. ibid., p. 344.

48. Skolem, "Einige Bemerkungen zu der Abhandlung von E. Zermelo: 'Ueber die Definitheit in der Axiomatik' ", Fundamenta Mathematicae 15(1930), pp. 337-41.

49. ibid., p. 337.

50. ibid., pp. 338-9.

51. ibid., p. 338.

52. J.I. Friedman, "Proper Classes as Members of Extended Sets", Math. Ann., 1969, pp. 232-240. Friedman cites A. Oberschelp, "Eigentliche Klassen als Urelemente in der Mengenlehre", Math. Ann., 1964, pp. 234-8. Similar approaches are taken in Montague, Scott and Tarski, An Axiomatic Approach to Set Theory, MS in UCLA Graduate Reading Room, and apparently in G. Takeuti's "On the Axiom of Constructibility", an unpublished manuscript which I have not been able to examine. See R. Solovay, "On the Consistency of Takeuti's TT" (Abstract), J.S.L. 31(1966), p. 691.

53. J.I. Friedman, op. cit., p. 233.

54. A theory T_1 can be interpreted in theory T_2 if the non-logical symbols of T_1 can be defined in T_2 in such a way that the axioms of T_1 become theorems of T_2. If T_1 can be interpreted in T_2 and T_2 is consistent, then so is T_1.

55. J.I. Friedman, op. cit., p. 238.

56. ibid.

57. K. Godel, "Remarks Before the Princeton Bicentennial Conference on Problems in Mathematics, 1946" in The Undecidable, M. Davis, ed., New York, 1965, pp. 84-88.

58. A. Levy and R. Solovay, "Measurable cardinals and the continuum hypothesis, Israel J. Math. 5(1967), pp. 234-248.

59. It should be noted that the axioms of STC include AC; p. 238 of Friedman, op. cit.

60. Takeuti, G., "The Universe of Set Theory", in Foundations of Mathematics, J.J. Bullof, et. al., eds., New York, 1969, p. 80.

61. ibid., p. 84.

62. This result is due to Hanf and Tarski. See Shoenfield, "Measurable Cardinals", in Logic Colloquium, '69, Gandy and Yates, eds., Amsterdam, 1971, p. 26.

63. Takeuti, op. cit., p. 80.

64. See discussion of this result in Appendix A of this work, p. 165.

65. Takeuti, op. cit., p. 88.

66. ibid., p. 102.

67. R. Solovay, "A Δ^1_3 Nonconstructible set of integers", Trans. A.M.S. 127(1967), pp. 50-75.

68. This writer would have proferred to include discussion of Takeuti's earlier system TT, but his unpublished paper on TT has not been available to me (see note 52). Solovay has shown CH to be independent of TT (see note 52).

69. Actually, we will continue to consider classes as surrogates for properties, rather than properties themselves.

70. Zermelo, "Ueber der Begriff", loc. cit., passim.

71. G. Kreisel, "Informal Rigour and Completeness Proofs", in The Philosophy of Mathematics, I. Lakatos, ed., Amsterdam, 1967, pp. 150-1; "Observations on Popular Discussions of Foundations", in Axiomatic Set Theory, Vol. I, D. Scott, ed., Providence, 1971,

p. 195. See also his "Two Notes on the Foundations of Set Theory", Dialectica 23(1969), pp. 93-114.

72. "Observations, etc.", loc. cit., p. 195.

73. See Church, Introduction to Mathematical Logic, Princeton, 1956, Chapt. V. In Church's terminology, we have an applied singulary second-order functional calculus with equality but without propositional variables.

74. For a full treatment of this "second-order" system, see Shoenfield, Mathematical Logic, Reading, MA, 1967, p. 227f.

75. Church, op. cit., p. 297, *509n.

76. Adapted from Henkin, "Completeness in the Theory of Types", J.S.L. 15(1950), pp. 81-91.

77. See E. Mendelson, Introduction to Mathematical Logic, Princeton, 1964, p. 145.

78. Henkin, loc. cit.

79. If φ is the Godel sentence for S^2, then $S^2 + \sim \varphi$ is consistent but has no ω- models (See Mendelson, p. 144). All *-models of S^2 are, however, ω-models.

80. Hence we are formulating ZF^2 as in note 73.

81. The terms $<x, y>$ are to be eliminated in favor of $\{x, \{x, y\}\}$ as for example in Montague, "Set Theory and Higher Order Logic", in Proceedings of the 1963 Meeting of the A.S.L., Crossely & Dummett eds., Amsterdam, 1964.

81a. Here, we are using the Hanf-Tarski theorem that if $\varkappa$ is measurable, then there are $\varkappa$ inaccessibles less than $\varkappa$, and also the fact that if α is inaccessible, then $R(\alpha)$ gives a model for ZF^2. The Hanf-Tarski theorem is discussed in J. Shoenfield, "Measurable Cardinals", in R.O. Gandy and C. M. E. Yates, Logic Colloquium '69, Amsterdam, 1971.

82. The simplest example I know of a model of ZF which is not, in fact, well-founded is given by the ultrapower of any well-founded model over a countably incomplete ultrafiler. The ultrapower is a model of ZF by the fundamental theorem on ultraproducts, but is not well-founded. See T. Jech, Lecture Notes in Set Theory, New York, 1971, pp. 43-7 for terminology and proofs.

83. This theorem states that every well-founded model of the axiom of extensionsality is isomorphic to an ϵ - model (as defined in the text). See Jech, op. cit., p. 27.

84. Appendix D gives a more precise version of this rather impressionistic argument.

85. Besides the "Informal Rigour" and "Observations on Popular Discussions" and "Two Notes" articles already cited (note 71), we will also treat passages in Kreisel's "Appendix II" in Reports of the Midwest Category Seminar, III, S. Mac Lane, ed., New York, 1969, and Section A, "Set Theoretic Semantic Foundations" in G. Kreisel and J. L. Krivine, Elements of Mathematical Logic (Model Theory), Amsterdam, 1967. Kreisel's view is discussed in two reviews of Lakatos, op. cit. J. Cleave, "The Significance of Independence Results in Set Theory for the Foundations of Mathematics", Ratio, 1972, and J. D. Halpern, Review #3196, Math. Rev., 1973. Halpern disputes Kreisel's view, while

Cleave is in general sympathy with it, as is M. Moss in "Kreisel's writings on the philosophy of mathematics", *Logic Colloquium '69*, R. O. Gandy and C. M. E. Yates, eds., Amsterdam, 1971.

86. "Comments on Mostowski's paper", Lakatos, *op. cit.*, p. 101.

87. See R. Dedekind, *Essays in the Theory of Numbers*, New York, 1963, p. 92.

88. "Survey of Proof Theory", p. 326.

89. "Informal Rigour", p. 148.

90. "Grenzzahlen und Mengenbereiche", *loc. cit.*

91. Shepherdson, "Inner Models for Set Theory-II", *J. S. L.* *17*(1952), pp. 225-237.

92. *ibid.*, p. 227.

93. "Appendix II", p. 237.

94. *ibid.*, p. 238.

95. "Informal Rigour", p. 151.

96. Cleave, *op. cit.*, p. 159; Mendelson, *op. cit.*, p. 116, problem 3.

97. "Informal Rigour", p. 151.

98. *ibid.*, p. 140.

99. *ibid.*, p. 157.

100. "Set Theoretic Semantic Foundations", p. 191.

101. "Informal Rigour", p. 150.

102. *ibid.*, p. 148.

103. "Comments on Mostowski's paper", p. 102.

104. *ibid.*, p. 102.

105. Cf. Bernays, *op. cit.*, p. 111.

106. "Survey of Proof Theory", p. 325.

107. *ibid.*, p. 326.

108. *ibid.*, p. 325.

109. "Comments on Mostowski's paper", p. 102.

110. Mostowski, "Comments on Bernays' paper", Lakatos, *op. cit.*, p. 114. For another writer of the same opinion, see L. Kalmar, "On the role of second-order theories", Lakatos, *op. cit.*, p. 104.

111. See Shepherdson, *op. cit.*, part II.

112. see note 79.

113. A. Levy, "Axiom Schemata of Strong Infinity in Axiomatic Set Theory", *Pac. Jour. Math.*, 1960, pp. 222-238.

114. *ibid.*, p. 236.

115. *ibid.*, p. 237.

116. *ibid.*, p. 237.

117. See, *e. g.*, "A survey of proof theory", *loc. cit.*, p. 326.

118. It should be noted that the example cited in n. 79 can be adapted to ZF^2 without worrying about predicate quantifiers vs. instances. One simply adds a sentence which makes the resulting theory consistent but ω- inconsistent.

118a. See above, p. 12.

119. "Informal Rigour", p. 147.

119a. This is shown in Chapter II, p. 92.

FOOTNOTES--CHAPTER II

1. L. E. J. Brouwer, "Intuitionism and Formalism", in Philosophy of Mathematics: Selected Readings, P. Benacerraf and H. Putnam, eds., Englewood Cliffs, 1964, p. 73. Brouwer's exact statement is "this power aleph-null is the only infinite power of which the intuitionists recognize the existence".

2. K. Godel, "What is Cantor's Continuum Problem", in Benacerraf and Putnam, op. cit., p. 259.

3. "Russell's Mathematical Logic", in ibid., p. 220.

4. ibid., p. 221.

5. ibid., p. 223.

6. ibid., pp. 223, 228.

7. ibid., p. 224.

8. ibid., p. 212, quoted from Russell's Introduction to Mathematical Philosophy, London, 1920, p. 169.

9. J. Fourier, The Analytical Theory of Heat, A. Freeman, trans., New York, 1955. The original edition dates from 1822.

10. R. Smullyan, "Continuum Problem", in The Encyclopedia of Philosophy, P. Edwards, ed., New York, 1967, vol. 1, p. 209.

11. G. Frege, The Basic Laws of Arithmetic, M. Furth, trans., Berkeley, 1967, p. 36.

11a. "What is a Function", in P. Geach and M. Black, Philosophical Writings of Gottlob Frege, Oxford, 1960, p. 113.

11b. "Function and Concept", in Geach and Black, p. 22; also ibid.

11c. "Function and Concept", p. 23.

11d. ibid., p. 22

11e. The Foundations of Arithmetic, Oxford, 1959, p.35.

11f. ibid., p. 37.

11g. ibid., p.35.

11h. ibid., p. 80nl.

11i. "Function and Concept", p. 32;"What is a Function", p. 115.

11j. "Function and Concept", p. 32.

11k. "What is a Function," p. 115.

11l. "Function and Concept", p. 24.

11m. ibid., p. 26nl.

11n. "On Concept and Object", in Geach and Black, op. cit., p. 48.

11o. ibid., p. 45.

11p. ibid., p. 45.

11q. ibid., p. 46.

12. see, e.g., ibid., pp. 33-4.

13. see, e.g., Essays on Frege, E.D. Klemke, ed., Urbana, 1968, Part I.

14. G. Frege, The Foundations of Arithmetic, J. Austin, trans., Oxford, 1969, pp. 35,72.

15. G. Frege, "The Thought", in Appendix A of Klemke, op. cit., p. 535.

16. G. Kreisel, "Mathematical Logic: What Has It Done For the Philosophy of Mathematics?", in Bertrand Russell, Philosopher of Century, R. Schoenman, ed., Boston, 1967, p. 213.

17. ibid., p. 219.

18. ibid., p. 219.

19. ibid., p. 222.

20. ibid., p. 223.

21. P. Bernays, "On Platonism in Mathematics", in Bernacerraf and Putnam, op. cit., p. 275.

22. ibid., p. 274.

23. ibid., p. 277.

24. G. Cantor, Grundlagen einer allegemeinen Mannigfaltigkeitslehre, Leipzig, 1883, quoted in P. E. B. Jourdain's introduction to Cantor's Contributions to the Founding of the Theory of Transfinite Numbers, New York, 1955, pp. 67-8.

25. ibid.

26. ibid.

26a. ibid., p. 69.

27. Brouwer, for example, describes his attitude as both anti-metaphysical and neo-Kantian. See, e. g., "Intuitionism and Formalism", loc. cit.

28. "On Some Difficulties in the Theory of Transfinite Numbers and Order Types", Proc. London Math. Soc. 2nd Series 4(1907), pp. 29-53.

29. ibid., p. 47.

30. See Godel's discussion in "Russell's Mathematical Logic", loc. cit., p. 223.

31. A. J. Ayer, Language, Truth and Logic, 2nd, ed., New York, 1946, p. 77.

32. C. G. Hempel, "On the Nature of Mathematical Truth", in Readins in the Philosophy of Science, Feigl and Brodbeck, eds, New York, 1953, p. 151.

33. Ayer, op. cit., p. 73.

34. ibid., p. 79.

35. Hempel op. cit., p. 150.

36. See the summary by Carnap. "Intellectual Autobiography". in P. Schilpp ed., The Philosophy of Rudolf Carnap, LaSalle, 1963, pp. 60-62 and 64-67.

37. R. Carnap, "Empiricism, Semantics and Ontology", in Benacerraf and Putnam, op. cit., p. 240.

38. To have mentioned Carnap's name in this section leaves a slightly misleading impression, for his views on truth are roughly the same as Tarski's; we will discuss them later.

38a. ibid., p. 344.

38b. ibid., p. 343.

39. W. Quine, "Truth by Convention", in Benacerraf and Putnam.

40. For example: "The mathematical calculii are a special kind of logical calculii, distinguished merely by their greater complexity" Carnap, Foundations of Logic and Mathematics, International Encyclopedia of Unified Science, Vol. I., No. 3. Chicago, 1939, p. 29.

41. Journal of Philosophy 69(1972), pp. 347-375.

42. ibid., section I.

43. Tarski, "The Concept of Truth in Formalized Languages", Logic, Semantics, and Matamathematics, New York, 1956; quoted in Field, op. cit., p. 354.

44. Field, op. cit., p. 370.

45. ibid., p. 375.

46. ibid., p. 367.

47. Carnap, "Empiricism, Semantics and Ontology", loc. cit., p. 244.

48. Carnap, ibid., passim.

49. S. Kripke, "Naming and Necessity" in Davidson and Harmon, eds., Semantics of Natural Language, Dordtrect, 1972, pp. 253-355.

50. Field, op. cit., p. 367.

51. Such an argument may be found in H. Putnam, "What is Mathematical Truth? ", unpublished manuscript, Cambridge, MA, 1973.

52. Such arguments may be found in M. Moss, "Kreisel's writings on the philosophy of mathematics", in Logic Colloquium '69, Gandy and Yates, eds,, Amsterdam, 1971, p. 418, and in "Mathematical Truth", by P. Benacerraf, unpublished manuscript, Princeton, N.J., 1971.

53. M. Steiner, "Platonism and the Causal Theory of Knowledge", Journal of Philosophy 70(1973), pp. 57-66, argues a causal theory of knowledge via "mathematical intuition" for realist views like those of Godel.

53a. See A. Goldmann, "A Causal Theory of Knowing", J. Phil. 64(1967).

53b. see Godel, op. cit., p. 271.

53c. See R. Montague, "Deterministic Theories", in Decisions, Values, and Groups, New York, 1962, p. 337.

54. See Dunn and Belnap, "The Substitutional Interpretation of the Quantifiers", Nous 2(1968).

55. J. Wallace, "Convention T and Substitutional Quantification", Nous 5(1971); J. Wallace, "On the Frame of Reference", Synthese 22(1970).

56. T. Weston, "Theories whose Quantification cannot be Substitutional", forthcoming in Nous.

57. For such a treatment of ZF, see Takeuti and Zaring, Axiomatic Set Theory, New York, 1971.

58. See F. W. Lawvere, "The Category of Categories as a Foundation of Mathematics., Proc. Conf. on Categorical Algebra, LaJolla. (1965), New York, 1966.

59. See the articles by Feferman and Kreisel in Reports of the Midwest Category Seminar, III, S. MacLane, ed., New York, 1969.

60. See G. Boolos, "On the semantics of the constructible levels", Zeitschr. Math. Log. 16(1970). p. 140.

61. The main source for the discussion of NF and ML in the text is Quine's Set Theory and Its Logic, 2nd ed., Cambridge, 1969. We will also have occasion to refer to J. B. Rosser and H. Wang, "Non-Standard Models for Formal Logics", J.S.L. 15(1950), pp. 113-129, and H. Curry, Review of Rosser's Logic for Mathematicians, Bull. Amer. Math. Soc. 60 (1954), pp. 266-272.

62. It should be noted that Cantor's theorem was one of the earliest substantial theorems of set theory, and has a simple, natural proof with very modest assumptions on set existence. Its failure for NF is certainly a black mark against that theory.

63. H. Curry, op. cit., p. 267.

63a. Rosser and Wang, op. cit.

64. P. Bernays and A. Fraenkel, Axiomatic Set Theory, Amsterdam, 1958, Part II, Chapter I.

65. Russell, op. cit., p. 43.

66. Cantor to Dedkind, 28 July 1899, Translated in From Frege to Godel: A Sourcebook in Mathematical Logic, 1879-1931, J. van Heijenoort, ed., Cambridge, Ma, 1967, pp. 113-117.

67. ibid., p. 114.

68. G. Cantor, Contributions to the Founding of the Theory of Transfinite Numbers, P.E.B. Jourdain, trans., p. 85.

69. Cantor to Dedkind, op. cit.

70. see note 24.

71. Russell, op. cit., p. 43.

72. P. Bernays, "On Platonsim in Mathematics", Benacerraf and Putnam, op. cit., p. 277.

73. ibid.

74. "Speaking of Objects", in Quine, Ontological Relativity and Other Essays, New York, 1969, p. 19.

75. It is a fairly simple matter to concoct a notion of identity tailored to a given model of VBG or even of ZF which will serve for identity of properties in VBG. One simply requires that '$(x)(A(x) \equiv B(x))$' be true in the model. But what is at stake here is to describe some particular standard interpretation.

76. G. Kreisel, "Two Notes on the Foundations of Set Theory", Dialectica 23(1969), p. 93.

77. "Informal Rigour and Completeness Proofs", in I. Lakatos, The Philosophy of Mathematics, Amsterdam, 1967, p. 144.

78. ibid.

79. ibid., p. 145.

80. ibid., p. 145-6.

81. ibid., p. 168.

82. ibid., p. 153.

83. ibid., p. 154.

84. G. Kreisel and J. Krivine, Elements of Mathematical Logic (Model Theory), Amsterdam, 1967, Appendix A, p. 174.

85. G. Berkeley, "The Analyst", exerpt in D. Struik, A Source Book in Mathematics 1200-1800, Cambridge, Ma., 1969, p. 337. Berkeley's pamphlet was published in 1734.

86. It was once suggested to this writer that Cantor's own views on set theory never lead to paradoxes, but only those of others, such as Frege. It is plain, however, that Cantor recognized the danger of paradoxes, and avoided contradictory assertions only be deciding not count as sets "multiplicities" that one would expect to be such from his earlier characterizations. For example, his 1895 definition characterizes sets as "any collection into a whole M of definite and separate objects m of our intuition or our thought" (see note 68), but four years later, he denied that the "multiplicity" of all cardinal numbers is a set, but not on the grounds that cardinals are not "separate" or "definite",

but because they "cannot be thought of without contradiction as being together" (see note 66). Of course, the contradiction which arises if one does suppose that all cardinals are "together", i.e., form a set, is Cantor's paradox.

87. H. Friedman, "A More Explicit Set Theory", in D. Scott, ed., *Axiomatic Set Theory*, I, Providence, 1971, pp. 49-66.

88. Y. Moschovakis, "Predicative Classes", *ibid.*, pp. 247-264.

89. L. Tharp , "A Quasi-Intuitionistic Set Theory", *J.S.L.* *36*(1971), pp. 456-460.

90. P. Hajek, "On Semisets", in Gandy and Yates, *op. cit.*, pp. 67-76.

91. The stratification condition on set definition in ML is explained in Appendix B.

92. J. Schoenfield, *Mathematical Logic,* Reading Ma, 1967, Chapter 9.

93. Kreisel and Krivine, *op. cit.*, Appendix A.

94. D. Scott, "Axiomatizing Set Theory", *Lecture Notes Prepared in Connection with the Summer Institute on Axiomatic Set Theory,* mimeograph, U.C.L.A., 1967, pp. III-A-1 to III-A-15.

95. See, e.g., Lawevere, *op. cit.*

96. For other attemps to reconcile category theory and set theory, see the articles by Kreisel and Feferman cited in note 59.

97. "On Semisets" by Petr Hajek, in Gandy and Yates, *op. cit.*

98. Petr Hajek, "Sets, Semisets and Models", in Scott, *op. cit.*

98a. "On Semisets", *loc. cit.*, pp. 67-70.

99. *ibid.*

100. Godel, "What is Cantor's Continuum Problem?", in Benacerraf and Putnam, op. cit., p. 266.

101. ibid.

102. G. Takeuti, "Hypotheses on Powerset", Scott, op. cit., p. 439.

103. Takeuti, "The Universe of Set Theory", in Foundations of Mathematics, J. J. Bulloff, et. al., eds., New York 1969.

104. "Hypotheses on Powerset", loc. cit.

105. "The Universe of Set Theory", loc. cit., pp. 76-7.

105a. See the discussion in Appendix A, pp. 186.

106. ibid., p. 77.

107. Godel, op. cit., p. 266. The axiom of constructibility is discussed below.

108. "Hypotheses on Powerset", p. 441; the plausibility of the axiom of choice is defended in Appendix A, pp. 186.

109. Russell, op. cit.

110. van Heijenoort, op. cit.

111. Russell, op. cit., p. 53.

112. ibid., p. 53.

113. ibid., p. 34.

114. See page 169, Appendix A.

115. It might be claimed that Cantor himself was never unclear about the notion of set, but this need to be squared with his evident unclarity in expressing himself about sets, clearly shown in earlier discussion and notes in this chapter. It is interesting that although he gave precise principles for construction of ordinals in 1883 ("Fondements

D'Une Theorie Generale Des Ensembles", _Acta Mathematica_ _2_(1883), pp. 381-408), he only gave axioms for sets in 1899, after he was obviously aware of the paradoxes (Cantor to Dedkind, _loc. cit._). It is also plain that at the time when he announced these axioms and the distinction between consistent and inconsistent "multiplicities", he still expressed himself in a paradoxical way--speaking of things which "cannot be thought of as _one_ thing". (Cantor to Dedekind, _op. cit._)

116. The definitive exposition of the controversy is C. Truesdell's _The Rational Mechanics of Flexible and Elastic Bodies, 1638-1788, Leonhardi Euleri Opera Omnia,_ Series 2, Vol. 10, Part III. See also J. Ravetz, "Vibrating Strings and Arbitrary Functions" in _Logic of Personal Knowledge_, Glencoe, 1961. Struik, _op. cit._, contains selections from the principle memoirs, with commentary and references.

117. Truesdell, _op. cit._, p. 238.

118. _ibid._, p. 240.

119. quoted _ibid._, p. 242.

120. _ibid._, p. 244.

121. _ibid._, p. 243.

122. _ibid._, p. 243.

123. _ibid._, pp.244-250.

124. _ibid._, p. 246.

125. quoted _ibid._, p. 246.

126. _ibid._, p. 248.

127. *ibid.*, p. 244, p. 244 n2.

128. *ibid.*, p. 244.

129. *ibid.*, p. 247.

130. *ibid.*, p. 247 nl.

131. *ibid.*, p. 247 nl.

132. *ibid.*, p. 255.

133. *ibid.*, p. 257.

134. quoted *ibid.*, p. 256.

135. *ibid.*, p. 261 .

136. quoted *ibid.*, p. 255.

137. quoted *ibid.*, p. 256.

138. quoted *ibid.*, p. 265.

139. *ibid.*, p. 266.

140. *ibid.*, p. 269.

141. *ibid.*, p. 275.

142. *ibid.*, p. 279.

143. *ibid.*, p. 280.

144. P. J. Cohen and R. Hersh, "Non-Cantorian Set Theory", *Scientific American*, 1967.

145. A. Mostowski, "Recent Results in Set Theory", in Lakatos, *op. cit.*, p. 94.

146. G. Kreisel, "Informal Rigour", *loc. cit.*, p. 151.

147. P. Suppes, "After Set Theory, What?" , in Lakatos, *op. cit.*, p. 115.

148. Mostowski, *op. cit.*, p. 94.

149. For an early exception, see the position of Proclus (circa 400 A.D.) described in J. L. Coolidge, A History of Geometrical Method's,New York 1963, p. 25.

150. N. Lobachevsky, New Principles of Geometry, Kazan, 1825, quoted in R. Bonola, Non-Euclidean Geometry, New York, 1955, p. 92.

151. D. Struik, A Concise History of Mathematics, New York, 1967, p. 143.

152. Lobachevsky, op. cit.

152a. This view is defended in H. Putnam, "It Ain't Necessarily So", J. Phil 61(1964); H. Putnam, "An Examination of Gruenbaum's Philosophy of Geometry", Delaware Seminar in the Philosophy of Science, Univ. of Delaware, 1962; H. Putnam, "Is Logic Empirical? ", Boston Studies in the Philosophy of Science, Vol. V., Dordrecht, 1968, H. P. Robertson, "Geometry as a Branch of Physics", in J.J.C. Smart, Problems of Space and Time, New York, 1964; A. Einstein, "Geometry and Experience", in H. Feigl and M. Brodbeck, Readings in the Philosophy of Science, New York, 1953.

152b. Cf. S. Barker, Philosophy of Mathematics, Englewood Cliffs, 1964, p. 51; P. F. Strawson, The Bounds of Sense, London, 1966, pp. 282-3.

152c. Cf. H. Poincare, "Non-Euclidean Geometries and the Non-Euclidean World", H. Feigl and M. Brodbeck, op. cit., p. 176; H. Reichenbach, "The Philosophical Significance of the Theory of Relativity", ibid., p. 198.

152d. Poincare held that is true, Reichenbach that it is false; see the references given in the previous note.

153. Introductio in analysin infinitorum (1748); quoted in C. Boyer, A History of Mathematics, New York, 1968, p. 485.

154. The earliest good approximation of the modern notion of function which is known to this writer is that of Lejeune Dirichlet in 1837: "if a variable y is so related to a variable x that whenever a numerical value is assigned to x, there is a rule according to which a unique value of y is determined, then y is said to be a function of the independent variable x."; Dirichlet, Werke(1889-1897), Vol. I, p. 135, quoted in Boyer, ibid., p. 600.

155. A. Mostowski, "Recent Results in Set Theory", in Lakatos, op. cit., p. 89. He cites Godel's similar remark in his 1938 paper on Ac and CH, Proc. Nat. Acad. Sci. U.S. 24(1938), pp. 556f. It is clear from Godel's later writings that he no longer holds this position.

156. ibid., p. 94.

157. A. Robinson, "Comments on Mostowski's Paper", in Lakatos, op. cit., p. 103.

158. L. Kalmar, "On the Role of Second-Order Theories", in ibid., p. 105.

159. P. Suppes, op. cit., p. 115.

160. See Cohen and Hersh, op. cit.; Cohen's "Comments on the Foundations of Set Theory, in Scott, op. cit., pp. 9-16, shows that he later changed his opinion.

161. See passage quoted above, p. 67.

162. Specifically, Easton has shown that if $G(\alpha)$ is any increasing function of ordinals in a countable standard model M of ZF which satisfies: "For all α, $\aleph_{G(\alpha)}$ is not cofinal with $\aleph_\alpha$", then there is a countable standard model with the same cardinals as M which satisfies: "$2^{\aleph_\alpha} = \aleph_{G(\alpha)}$" for all regular cardinals $\aleph_\alpha$. (A cardinal is regular if it is not cofinal with any smaller ordinal). See Easton, "Powers of Regular Cardinals", Ann. Math. Logic 1 (1970), pp. 130-178. Cohen had already shown that if τ is any ordinal not cofinal with ω in a given countable standard model M, then the continuum can have cardinality $\aleph_\tau$. If τ is cofinal with ω, then the continuum can have cardinality $\aleph_{\tau+1}$ in a standard model with the same cardinals as M. See Cohen, Set Theory and the Continuum Hypothesis, New York, 1966, p. 134. Solovay has shown that if it is consistent with ZF that there is a measurable cardinal, then it is consistent with ZF that the continuum is real-valued measureable, i. e., a truly enormous cardinal. See Solovay's proof sketch in mimeo notes for 1967 UCLA conference, section IV-F.

163. Mostowski, op. cit., p. 94.

164. ibid., p. 94.

165. See Cohen, Set Theory and the Continuum Hypothesis, p. 82.

166. ibid., p. 125.

167. R. Solovay, "A Nonconstructible Δ^1_3 Set of Integers," Trans. A.M.S. 1967, pp. 50-75.

168. T. J. Jech, Lectures in Set Theory, New York, 1971, p. 35.

169. Cohen, op. cit., p. 104.

170. Myhill and Scott, "Ordinal Definability", in Scott, op. cit., pp. 271-8; K. McAloon, "Consistency Results about Ordinal Definability", Ann. Math. Logic 2(1971), pp. 449-469.

171. Godel, op. cit., p. 266.

172. ibid., p. 267.

173. See Solovay, op. cit., and Rowbottom, "Some Strong Axioms of Infinity Incompatible with the Axiom of Constructibility", Ann. Math. Logic 3(1971), pp. 1-44. For a large cardinal axiom compatible with V=L, see Silver "A Large Cardinal in the Constructible Universe", Fund. Math. 69(1970), pp. 93-100.

174. See, e.g., Boolos and Putnam, "Degrees of Unsolvability of Constructible Sets of Integers", J.S.L. 33(1968), p. 500.

175. "The Case for V=L", UCLA Philosophy Library MS, 1964.

176. G. Takeuti, "A Formalization of the Theory of Ordinal Numbers", J.S.L. 30(1965), pp. 295-317.

177. See S. Feferman, "Systems of Predicative Analysis", J.S.L. 29 (1964), pp. 1-30.

178. This discussion follows Cohen's, op. cit., pp. 109-12.

179. Cohen shows that the existence of uncountable models for ZF+AC+ ~CH cannot be proved from ZF+"ZF has a standard model" or any consistent extension of this theory compatible with V=L; op. cit., p. 109. If AC is omitted, this result no longer holds, but AC is wanted here for reasons cited in appendix A.

180. In Solovay and Scott's generalization of Cohen's method a generic set (more precisely: a generic filter) is guaranteed if the starting model is countable, but all that is really required is that the intersection of the powerset of the set of forcing conditions with the starting model be countable. These conditions are satisfied for the model of ZF obtained by taking the constructible sets out to the first inaccessible, provided that $\aleph_3^L$, the third "uncountable" cardinal in the constructible universe is in fact countable, since this is the cardinal of the powerset of the forcing conditions in the model. That $\aleph_1^L$, $\aleph_2^L$, $\aleph_3^L$, etc., are countable is consistent with ZF and implied by the existence of measurable or Ramsey cardinals (Solovay, op. cit.). See the exposition of the independence proof for CH in Jech, op. cit. p. 64. Jech assumes the countability of the starting model, but his proof can be modified along the lines indicated here using his lemma 55, p. 61, on the existence of generic sets. The cardinality of the Cohen model thus obtained is the same as the first inaccessible, assuming AC.

181. Solovay and Martin, "Internal Cohen Extensions", Ann. Math. Logic 2(1970), pp. 143-78, Theorem I, p. 149.

182. Cohen's method for proving that an extension is nice requires that the forcing conditions obey the so-called countable chain condition. Solovay and Martin's theorem is about generic sets for forcing conditions satisfying this condition. There exist other methods for showing that an extension is nice, such as the proof given by Schoenfield for Easton's result cited in note 162. This proof

requires that the starting model and its extension be countable for other reasons. See J. Schoenfield, "Unramified Forcing" in Scott, op. cit., p. 374.

183. "depends on our knowing this" again because the powerset of the forcing conditions w.r.t. the starting model must be countable. In Cohen's model for ZF+AC+ ~CH, the forcing conditions are all functions whose domain is a finite subset of $\aleph_0^M \times \aleph_2^M$ and whose range is contained in $\{0, 1\}$. In order that there be only denumerably many such conditions, much less that the powerset in M of these conditions be countable, it is necessary that $\aleph_2^M$ be countalbe.

184. op. cit.

185. New York, 1956; cited in Solovay and Martin, op. cit., p. 173.

186. Solovay and Martin, op. cit., p. 174.

187. Mostowski repeatedly uses these terms, op. cit., pp. 89, 93-5, 106-7.

188. op. cit., p. 106.

189. ibid., pp. 94-5.

190. Diophantine equations are equations of number-theoretic functions for which solutions are sought for positive integer values of the arguments.

191. The history of the attempts to prove Fermat's "Theorem" is reviewed in Th. Got, "A Mathematical Enigma: Fermat's Last Theorem", in F. LeLionnais, ed, Great Currents of Mathematical Thought, New York, 1971, vol. I, pp. 81-91. Goldbach's conjecture is discussed in Boyer, op. cit., p. 500. Vinogradov's proof is summarized in K. K. Mardzanisvili and A. B. Postnikov, "Prime Numbers", in A.D. Aleksandrov, et.

al., *Mathematics: Its Content, Methods and Meaning*, Cambridge, Ma, 1963, Part 4, vol. 2, pp. 199-228.

192. E. Mendelson, *Introduction to Mathematical Logic*, Princeton, 1964, pp. 125, 131, Theorems 3.17 and 3.23.

193. A. Mostowski, "Cohen's Independence Proof and Second Order Formalization", in Lakatos, *op. cit.*, p. 114.

194. A. Levy and R. Solovay, "Measurable Cardinals and the Continuum Hypothesis", *Israel Jour. Math.* 5(1967), pp. 234-248.

195. Myhill and Scott, *op. cit.*, pp. 275,278.

196. The implausibility of the axiom of determinateness is argued in Appendix A.

197. G. Cantor, "Fondements d'une Theorie Generale", *loc. cit.*, p. 395.

198. Boyer, *op. cit.*, pp. 654-5.

199. Zermelo, "Proof that Every Set can be Well-ordered", in van Heijenoort, *op. cit.*, pp. 139-141.

200. See the survey of attitudes toward AC in appendix A.

201. Editor's Introduction to "Dedekind to Keferstein, 27 February, 1890", in van Heijenoort, *op. cit.*, p. 98.

202. R. Dedekind, "The Nature and Meaning of Numbers", in *Essays on the Theory of Numbers*, New York, 1963 (originally published in 1888).

203. Editor's Introduction, *op. cit.*, p. 98.

204. "Dedkind to Keferstein", *loc. cit.*, p. 99.

205. *ibid.*, p. 100.

206. Cohen, "Comments on the Foundations of Set Theory", in Scott, *op. cit.*, p. 13.

207. I understand that this is now Solovay's position.

208. Kreisel, "Informal Rigour", loc. cit., p. 140.

209. ibid., pp. 140, 143.

210. See A. Robinson, Introduction to Model Theory and the Metamathematics of Algebra, Amsterdam, 1963, Section 9.4.

210a. This discussion follows closely that in ibid.

211. See E. Bishop, Foundations of Constructive Analysis, New York, 1967, Chapter II; A. O. Slisenko, Studies in Constructive Mathematics and Mathematical Logic, Part I, New York, 1969, and works cited there; A.S. Troelstra, Principles of Intuitionism, New York, 1969, Section 6.

212. See B. van Rootselaar, review of Bishop, op. cit., Math. Rev. 36 (1968), #4930.

213. See, for example, H. Feigl, "Logical Reconstruction, Realism and Pure Semiotic", Philosophy of Science 17(1950); G. Maxwell, "The Ontological Status of Theoretical Entities", in H. Feigl and G. Maxwell, eds., Minnesota Studies in the Philosophy of Science, Part III, Minneapolis, 1962, pp. 3-27; H. Putnam, "Craig's Theorem", J. Phil. 62 (1965), pp. 251-260; H. Putnam, "What Theories are not", in E. Nagel, P. Suppes, and A. Tarski, eds, Logic, Methodology, and Philosophy of Science, Stanford, 1962; R. Boyd, "Realism, Underdetermination, and a Causal Theory of Evidence", Nous 7(1973), pp. 1-12; R. Boyd, Realism and Scientific Epistemology, forthcoming.

214. According to quantum mechanics, there are certain physical systems in which these quantities are not continuous. This need not bother us, however, We only need some systems in which these quan-

tities are continuous, for example, the energy of a free electron. See R. M. Eisberg, *Fundamentals of Modern Physics*, New York, 1963, Section 8.1.

215. J. Addison, "Some Consequences of the Axiom of Constructibility", *Fund. Math.* *46*(1959), p. 356.

FOOTNOTES - APPENDIX A

1. See, for example, Grattan-Guiness' statement: "in general, [Fourier] took the attitude of his predecessors in justifying mathematics by means of its physical interpretation", The Development of the Foundations of Mathematical Analysis from Euler to Riemann, Cambridge, Mass, 1970, p. 21.

2. This example is due to Richard Boyd in conversation.

3. Zermelo, 1908, p. 190. Godel defends a similar view in his 1964, p. 272: "The mere psychological fact of the existence of an intuition which is sufficiently clear to produce the axioms of set theory and an open series of extensions of them suffices to give meaning to the question of the truth or falsity of propositions like Cantor's continuum hypothesis." For examples of talk about intuition by working set theorists, see the mimeo notes of the UCLA Conference on Axiomatic set theory, pp. III-A-1, 3 (Scott), III-N-1 (Moschovakis), II-1 (Shoenfield) and the article by Pozsgay, "Liberal Intuitionism as a Basis for Set Theory," pp. IV-A-1 to IV-A-7.

4. History of the Calculus and its Conceptual Development, New York, 1959, Chapter VI.

5. Grannan-Guiness, op. cit., Chaps III & IV argues that Cauchy did not in fact play the major role in this work traditionally attributed to him.

6. Cf. Poincaré's statement: "One conformed unconsciously to the model which is supplied to us by the functions considered in mechanics

and one rejected everything which deviated from this model; one was not guided by a clear and rigorous definition, but by a sort of obscure and indistinct intuition." "L'oeuvre mathematique de Weierstrauss," Acta Math. 22, p. 4. Cited in Merz, A History of European Scientific Thought in the Nineteenth Century, London, 1912, reprinted New York, 1965, Vol. II, p. 638.

7. Quine has made such remarks in a number of places. See, e.g., The Ways of Paradox and Other Essays, New York, 1966, pp. 18, 69, 104, 110.

8. Cited in Freaenkel and Bar-Hillel, Foundations of Set Theory, Amsterdam, 1958, p. 47. These earlier authors were Peano, who thought the principle false, and Beppo Levi.

9. Van Heijenoort, op. cit., pp. 139-41. Cantor had promised a proof of this theorem in 1883, Fondements d'une Theorie Generale des Ensembles, Acta Math. II (1883), p. 395.

10. Math. Ann. 60(1905), p. 195, "Quelques remarques sur les principles de la theorie des ensembles."

11. "Cinq lettres sur la theorie des ensembles," Bull. Soc. Math. France, 33 261-273. Reprinted as an appendix to Lecons sur la theorie des functions, by E. Borel, 2nd ed., Paris, 1914. Page references in the following notes are to this edition.

12. Last Essays, New York, 1963, p. 67.

13. Feferman has shown that it is relatively consistent with ZF+AC that the continuum has no definable well-ordering, although, on account of

AC, a well-ordering must exist; "Some Applications of the Notion of Forcing and Generic Sets," Fund. Math., 56(1965), p. 341.

14. Lettre de M. Borel à M. Hadamard, l. c., p. 159.

15. If $P(\omega)$ were cardinaly less than or equal to any $\aleph_\alpha$, it would be a similar to a subset of that $\aleph_\alpha$, all of which are well-ordered.

16. Van Heijenoort, op. cit., p. 187.

17. ibid., p. 188.

18. G. Vitali, Sul Problema della misura dei gruppi de punti di una retta, Bologna, 1905. Cited in A. Kuratowski, Topologie, Warszawa, 1958, p. 59.

19. This can be sharpened to: If the continuum has a Δ^1_n well-ordering, then there is a Δ^1_n non-measurable set of reals. See Silver, "Measurable Cardinals and Δ^1_3 Well-orderings," Ann. of Math., 1971, p. 441.

20. S. Banach and A. Tarski, "Sur la decomposition des ensembles de points en parties respectivement congruentes," Fund. Math. 6 (1924), p. 244.

21. See L. Blumenthal, "A Paradox, A Paradox, A Most Ingenious Paradox," Americal Mathematical Monthly, 47(1940), p. 346 for a selection.

22. Apostel, Mathematical Analysis, Reading, Mass, 1957, p. 396.

23. E. Borel, "Les paradoxes de l'axiome du choix," Comptes Rendues Acad. Sci. Paris 224(1947), pp. 1537-8. That Borel still retained his

constructivist bent 43 years after his first attack on AC is indicated by his characterization of the set whose existence is guaranteed by AC is "mal defini."

24. op. cit., pp. 65-70.

25. Rosser, Logic for Mathematicians, New York, 1953, pp. 511-2.

26. Solovay, "A Model of Set Theory in which Every Set of Reals in Lebesgue Measureable," Ann. of Math., 92(1970), pp. 1-56. This work establishes the consistency of ZF+LM+DC, where DC is a slightly stronger version of AC("dependent choice") which implies CAC. This result requires the hypothesis that ZF+'there is an inaccessible > ω' is consistent, but Solovay believes that this can be weakened to Con(ZF).

27. Zorn's lemma, Tychonoff product theorem, and many others. For an exhaustive list, see Rubin and Rubin, Equilivents of the Axiom of Choice, Amsterdam, 1969.

28. E.g., the boolean prime ideal theorem, which has only recently been shown to be weaker than AC. See Halpern and Levy, "The Boolean Prime Ideal Theorem does not Imply the Axiom of Choice," UCLA Proceedings, Providence, 1971, pp. 83-134.

29. Frankel and Bar-Hillel, op. cit., p. 70.

30. Quoted ibid., p. 60.

31. Foundations of Constructive Analysis, New York, 1967.

32. See, Studies in Constructive Mathematics and Mathematical Logic, New York, 1969, esp. the paper by Demuth and works cited there.

33. See van Rootselaar's review of Bishop in *Math. Reviews* *36*(1968), p. 965.

34. Cohen, "Comments on the foundations of set theory," *UCLA Proceedings*, Providence, 1971, p. 12.

35. *ibid*.

36. See remarks in H. M. Friedman, "Higher Set Theory and Mathematical Practice," *Ann Math Logic* 2(1971), pp. 325-57.

37. "Alternatives to Zermelo's Assumption," *Trans. Amer. Math. Soc*, 1927. His two alternatives, as rephaised by Mycielski, were "ω_1 is a regular cardinal" and "ω_1 is regular, but there is no choice set for the Lebesgue decomposition of the real line." See Mycielski, "On the Axiom of Determinateness," *Fundamenta Math.*, *53*(1964), p.213.

38. Mycielski, *op. cit.*, 1964, p. 217.

39. *ibid.*

40. Mycielski, "On the axiom of determinateness, II", *Fund. Math.* *59* (1966), p. 204. Cited as Mycielski, 1966.

41. Mycielski 1964, p. 216.

42. *ibid.*, p. 207.

43. J. E. Fenstad, "The axiom of determinateness", in Fendstad, *Proceedings of the 2nd Scandinavian Logic Symposium*, Amsterdam, 1971, p. 44.

44. *ibid.*, p. 53.

45. "A Mathematical Axiom Contradicting the Axiom of Choice," *Bull. Pol. Acad. Sci.*, *10*(1962)p.1.

46. "On the Lebesgue Measurebility and the Axiom of Determinateness," by J. Mycielski and S. Swierczkowski, Fund. Math., 54(1964), p. 67.

47. Mycielski 1964, p. 217.

48. ibid., p. 208. The Cantor set is obtained from the unit interval [0,1] by removing the middle third of this interval and then the middle thirds of the remaining parts and so on. It is perfect, that is, closed and every element of it is one of its limit points. It is nowhere dense, that is, the interior of its closure is empty.

49. ibid., p. 213.

50. ibid., p. 207.

51. Mycielski 1964, p. 208.

52. ibid. 219.

53. Fraenkel, Abstract Set Theory, Amsterdam, 1961, p. 173 nl.

54. Mycielski 1964, p. 205.

55. ibid., p. 218.

56. Mycielski 1966, p. 202.

57. ibid., p. 209. This version of AC is (2) modified for disjoint families of power up to that of the continuum, rather than only countable ones, as in (2).

58. The analytic sets of sequences from R are defined as follows. Give R the discrete topology and R^{ω} the product topology. A subset of this space is analytic if it results from a Borel set by "generalized projection." That is, if

$$P = \bigcup_{i} \bigcap_{n,\omega} F_{i_o \cdots i_n}$$

where $i = i_o \ldots i_n$ ranges over all finite sequences from ω, and F is closed.

59. op. cit., p. 1.

60. ibid., p. 2

61. ibid., p. 2.

62. These results are surveyed in Fenstad, op. cit.

63. ibid., p.45.

64. ibid., p. 57.

65. See above, p. 165.

66. Fenstad, op. cit., p. 56.

67. ibid.

68. "Sur les ensembles analytiques nuls," Fund. Math. 25(1935), p. 129.

69. Godel, "What is Cantor's Continuum Problem?" Amer. Math. Monthly, 54(1947), pp. 515-25. Revised version in Benacerraf and Putnam, Philosophy of Mathematics, Englewood Cliffs, 1964, pp. 258-73.

70. Set Theory and the Continuum Hypothesis, Amsterdam, 1966, p. 151.

71. ibid.

72. ibid. Solovay and Martin also express disbelief in CH. See their "Internal Cohen Extenstions," Ann. Math. Logic, 2(1970), p. 177.

73. Solovay and Tennenbaum, "Iterated Cohen extensions and Souslin's problem," Ann. of Math., 1971, p.233.

74. for example, see Vaught, "Löwenheim-Skolem Theorems for Cardinals Far Apart," in Addison, Henkin and Tarski, Theory of Models, Amsterdam, 1963.

75. 2nd ed., New York. 1956.

76. Oxtoby, Measure and Category, New York, 1971, p. 26.

77. ibid.

78. ibid.

79. Oxtoby, op. cit., p. 75. A set is of first category if it is a countable union of sets whose closure has empty interior. A set is of measure zero if it is contained in infinite sets of intervals the sum of whose lengths is arbitrarily small.

80. These results are described in detail in Oxtoby, op, cit., Chaps. 19-21.

81. ibid., p. 75.

82. Fund. Math. 22(1934), p. 274. X is of first category on A if $X \cap A$ is of first category in the relative topology on A.

83. Sierpinski, Hypothese du Continu, p. 49.

84. Sierpinski, "Sur deux ensembles lineairs singuliers," Ann. Scuol. Norm. Sup. Pisa 4(1935), p.43.

85. Sierpinski, Fund. Math. 22(1934), p. 274.

86. Comptes Rendues de l'Acad. Sci. 158(1914), p. 1258.

87. Sierpinski, Hypothèse du Continu, p. 15. The list (6)- (11) omits one consequence of CH contained in the earlier but not the later version of Godel's paper, and one proposition contained in both versions which deals with Hilbert Space.

88. Godel, op. cit., p. 267.

89. ibid., p. 266.

90. ibid., p. 267. Presumably, 'continuum many' is to be replaced by 'uncountably many' in this modification of (11), but his is not certain.

91. Sierpinski, Hypothèse du Continu, p. 18.

92. ibid., p. 20.

93. Godel, op. cit., p. 267-70.

94. ibid., p. 271.

95. ibid., p. 267.

96. There exist uncountable sets of reals (called Lusin sets) such that a subset is of first category iff it is countable. Assuming CH, there is also a set of positive measure whose subsets have measure zero iff they are countable.

97. Solovay and Martin, op. cit., p. 177.

98. Martin's axiom, discussed in Chapter II.

99. Solovay and Martin, op. cit., p. 177.

100. Godel, op. cit., p. 268.

101. "Sur les ensembles analytiques nuls," Fund. Math. 25(1935), p. 129.

102. ibid., p. 125.

103. Sierpinski, General Topology, Toronto, 1952, p. 247.

104. Solovay, "The Cardinality of Σ^1_2 Sets of Real", Foundations of Mathematics, New York, 1971, p. 59.

105. ibid., p. 59.

106. Solovay and Martin, op. cit., p. 144.

107. A cardinal κ is measureble if there is a function $f: \mathbb{P}(\kappa) \to \{0,1\}$ such that f is κ- additive and maps singletons into 0. Such a function is κ- additive if the value assigned to the union of fewer than κ disjoint sets is the sum of the values assigned by f to the individual sets. For the definition of Ramsay cardinal, see Solvovay, "A Δ^1_3 Nonconstructible set of integers, "Trans. A.M.S. 127(1967), Sect. 3.1.

108. Solovay, "On the cardinality of Σ^1_2 sets of reals", op. cit., p. 58.

109. ibid., p. 59.

110. See T. J. Jech, Lectures in Set Theory, New York, 1971, p. 28.

111. Reinhardt, "Conditions on Natural Models of Set Theory," UCLA Set Theory Conference Notes, p. IV-L.

FOOTNOTES-- APPENDIX D

1. "Inner Models for Set Theory-Part II", J.S.L. 17(1952), 225-237.

2. "Ueber Grenzzahlen und Mengenbereiche", Fund. Math. 16(1930), 339-344.

3. page 63.

4. see Chapter I, n. 83.

5. Shepherdson, "Inner Models for Set Theory-Part I", J.S.L. 16(1951), 161-190.

6. Shepherdson, "Inner Models-Part II", loc. cit.