Is the Continuum Hypothesis a definite mathematical problem?

DRAFT 9/18/11

For: *Exploring the Frontiers of Incompleteness* (EFI) Project, Harvard 2011-2012

Solomon Feferman

[t]he analysis of the phrase "how many" unambiguously leads to a definite meaning for the question ["How many different sets of integers do their exist?"]: the problem is to find out which one of the $\aleph$'s is the number of points of a straight line… Cantor, after having proved that this number is greater than $\aleph_0$, conjectured that it is $\aleph_1$. An equivalent proposition is this: any infinite subset of the continuum has the power either of the set of integers or of the whole continuum. This is Cantor's continuum hypothesis. …

But, although Cantor's set theory has now had a development of more than sixty years and the [continuum] problem is evidently of great importance for it, nothing has been proved so far relative to the question of what the power of the continuum is or whether its subsets satisfy the condition just stated, except that … it is true for a certain infinitesimal fraction of these subsets, [namely] the analytic sets. Not even an upper bound, however high, can be assigned for the power of the continuum. It is undecided whether this number is regular or singular, accessible or inaccessible, and (except for König's negative result) what its character of cofinality is.

Gödel 1947, 516-517 [in Gödel 1990, 178] Throughout the latter part of my discussion, I have been assuming a naïve and uncritical attitude toward CH. While this is in fact my attitude, I by no means wish to dismiss the opposite viewpoint. Those who argue that the concept of set is not sufficiently clear to fix the truth-value of CH have a position which is at present difficult to assail. As long as no new axiom is found which decides CH, their case will continue to grow stronger, and our assertion that the meaning of CH is clear will sound more and more empty.

Martin 1976, 90-91 **Abstract:** The purpose of this article is to explain why I believe that the Continuum Hypothesis (CH) is not a definite mathematical problem. My reason for that is that the concept of arbitrary set essential to its formulation is vague or underdetermined and there is no way to sharpen it without violating what it is supposed to be about. In addition, there is considerable circumstantial evidence to support the view that CH is not definite. In more detail, the status of CH is examined from three directions, first a thought experiment related to the Millennium Prize Problems, then a view of the nature of

mathematics that I call Conceptual Structuralism, and finally a proposed logical framework for distinguishing definite from indefinite concepts.

My main purpose here is to explain why, in my view, the Continuum Hypothesis (CH) is [1]*not* a definite mathematical problem. In the past, I have referred to CH as depending in an essential way on the *inherently vague* concept of *arbitrary set*, by which I mean that the concept of a set being arbitrary is vague or underdetermined and there is no way to sharpen it without violating what it is supposed to be about. I still believe that, which is the main reason that has led me to the view that CH is not definite. Others, with the extensive set-theoretical independence results concerning CH in mind, have raised the question whether it is an *absolutely undecidable proposition*, that is, in the words of Koellner (2010), "undecidable relative to any set of axioms that are justified." I prefer not to pose the issue that way, because it seems to me that the idea of an absolutely undecidable proposition presumes that the statement in question has a definite mathematical meaning. There is no disputing that CH is a definite statement in the language of set theory, whether considered formally or informally. And there is no doubt that that language involves concepts that have become an established, robust part of mathematical practice. Moreover, many mathematicians have grappled with the problem and tried to solve it as a mathematical problem like any other. Given all that, how can we say that CH is not a definite mathematical problem?

I shall examine this from three directions, first a thought experiment related to the Millennium Prize Problems, then a view of the nature of mathematics that I call Conceptual Structuralism, and finally a proposed logical framework for distinguishing definite from indefinite concepts.

1. The Millennium Prize Problem Test

Early in the year 2000, the Scientific Advisory Board (SAB) of the Clay Mathematics Institute (CMI) announced a list of seven mathematical problems, each of which if solved

[1] That is not Koellner's view; in fact his main aim in Koellner (2010) is to exposit work of Woodin that it is hoped will lead to a decision as to CH.

would lead to a prize of one million dollars. The list includes some of the most famous open problems in mathematics, some old, some new: the Riemann Hypothesis, the Poincaré conjecture, the Hodge conjecture, the P vs NP question, and so on, but it does *not* include the Continuum Hypothesis. The ground rules for the prize are that any proposed solution should be initiated by a refereed publication, followed by a two-year waiting period "to ensure acceptance of the work by the mathematics community, before the CMI will even solicit expert opinions about the validity or attribution of a presumed solution." (Jaffe 2006, 655) One criterion for the selection of problems by the SAB was that "each of these questions should be difficult and important." (Jaffe 2006, 653). And it was decided that the simplest form of a question was to be preferred "at least whenever that choice seemed sensible on mathematical and general scientific grounds." Finally, "while each problem on the list was central and important…the SAB did not envisage making a definitive list, nor even a representative set of famous unsolved problems. Rather, personal taste entered our choices; a different scientific advisory board undoubtedly would have come up with a different list. … We do not wish to address the question, 'Why is Problem A not on your list?' Rather we say that the list highlights seven historic, important, and difficult open questions in mathematics." (Ibid., 654)

[2]We don't know if CH was considered for inclusion in the list, though I would be surprised if it did not come up initially as one of the prime candidates. In any case, a new opportunity for it to be considered may have emerged since Grigory Pereleman solved the Poincaré conjecture, was awarded the prize and declined to accept it. Let's imagine that the CMI sees this as an opportunity to set one new prize problem rather than let the million dollars go begging that would have gone to Perelman. Here's a scenario: the SAB solicits advice from the mathematics community again for the selection of one new problem and in particular asks an expert or experts in set theory (EST) whether CH should be chosen for that. Questions and discussion ensue.

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[2] There was apparently no animus to the logic community on the part of the SAB concerning the problems it selected; logicians (along with computer scientists) were consulted as to the importance of the P = (?) NP problem.

SAB: Thank you for joining us today. CH is a prima-facie candidate to be chosen for the new problem and your information and advice will be very helpful in deciding whether we should do so. Please tell me more about why it is important and what efforts have been made to solve it.

$\aleph_0$EST: Set theory is generally accepted to be the foundation of all mathematics, and this is one of the most basic problems in Cantor's theory of transfinite cardinals which led to his development of set theory starting in the 1880s. Hilbert recognized its importance very quickly and in 1900 placed it in first position in his famous list of mathematical problems. As to the problem itself, soon after Cantor had shown that the continuum is uncountable in the early 1880s, he tried but failed to show that every uncountable subset of the continuum has the same power as the continuum. That's one form of CH, sometimes called the Weak Continuum Hypothesis, the more usual form being the statement 2 = $\aleph_1$; the two are equivalent assuming the Axiom of Choice (AC). [EST continues with an account of the early history of work on CH beginning with Cantor through Sierpinski and Luzin in the mid 1930s; cf. Moore (1988, 2011).]

SAB: Despite all that work, the quote you give from Gödel [above] says that nothing was learned about the cardinality of the continuum beyond its uncountability and König's theorem. Is that fair? And what's happened since he wrote that in 1947?

EST: Well, in that quote Gödel did not mention a related problem that Cantor had also initiated, when he showed that every uncountable closed subset X of the continuum has the *perfect set property*, i.e. contains a perfect subset and hence is of the power of the continuum; we also say that CH holds of X. The question then became, which sets have that property? That was actively pursued in the 1930s in the Russian school of Descriptive Set Theory (DST, the study of definable sets of reals and of other topological spaces) led by Luzin. The strongest result he obtained, together with his student Suslin, is that all Σ^1_1 (or "analytic" sets) have the perfect subset property. The Σ^1_1 sets of reals are the projections of 2-dimensional Borel sets, and that is the first level of sets in the projective hierarchy, which are generated by alternating projection (giving the Σ^1_n sets) and complementation (giving the Π^1_n sets). The workers in DST were unable to show that the Π^1_1 sets have the perfect set property. The reason for that was explained by

Gödel (1938, 1940) who showed that there are uncountable $\square^1_1$ sets[3] that do not have the perfect set property in his constructible sets model L of ZFC + GCH.

SAB: So the analytic sets would seem to be as far as one could go with the perfect set property.

EST: Actually not, since it could equally well be consistent with ZFC and other axioms that not only $\square^1_1$ sets but *all* uncountable sets in the projective hierarchy have the perfect set property. And that turned out to be the case through a surprising route, namely the use of a statement called the Axiom of Determinacy (AD) introduced in the early 1980s by Mycielski and Steinhaus. That is very powerful because it proves such things as that all sets of reals are Lebesgue measurable, contradicting AC. So set theorists don't accept AD because AC is absolutely basic to set theory and is intuitively true of the domain of arbitrary sets. But set theorists realized that relativized forms of AD could be true and have important consequences.

SAB: Tell me–but first explain what AD is.

EST: Associated with any subset X of 2^N is a two person infinite game G^N_X. Beginning with player I, each chooses in alternation a 0 or a 1. At the "end" of play, one has a sequence s in 2; player I wins if s belongs to X while player II wins if not. AD for X says that one player or the other has a winning strategy for G_X, and AD holds for a class G of sets if it holds for each X in G . Even though AD is not true for the class of all subsets of the continuuum, it could well be true for substantial subclasses G of that. In fact Martin (1975) proved in ZFC that AD holds for the Borel sets and then moved beyond that to study AD for sets in the projective hierarchy. *Projective Determinacy* (PD) states that AD holds for the class of all projective sets. PD turns out to have many significant consequences including that (i) every projective set is Lebesgue measurable, (ii) has the Baire property, and (iii) if uncountable contains a perfect subset. And, in

[3] ZFC consists of the usual Zermelo-Fraenkel axioms of set theory (ZF) expanded by the Axiom of Choice (AC), and GCH is the Generalized Continuum Hypothesis in the form that for all ordinals a , $2^{\aleph_a} = \aleph_{a+1}$.

1989, Tony Martin and John Steel succeeded in obtaining the stunning result that PD holds; their proof was published in the *Journal of the American Mathematical Society* under the title “A proof of projective determinacy” (Martin and Steel 1989).

SAB: That sounds pretty impressive and like real progress. So what you’re telling me is that not only is PD consistent with ZFC but it’s true, though I guess it can’t be true in L from what you told me before.

EST: You’re right about the latter, but something more has to be said about Martin and Steel’s proof to assert that PD is true. They don’t just use the ordinary axioms of set theory; what they actually show (and are quite explicit about) is that PD follows from ZFC plus the assumption that there exist infinitely “Woodin cardinals” with a measurable cardinal above all of them. In fact, Woodin strengthened that to showing that under the same assumption, AD holds in L(R), the constructible sets relative to the real numbers R (cf. Koellner 2010, 205). Incidentally, he also showed that if AD holds in L(R), then it is consistent with ZFC that there are infinitely many Woodin cardinals (ibid.).

SAB: I know what a measurable cardinal is supposed to be since it was introduced by Ulam, and happen to know that Scott showed there are no measurable cardinals in L; on the other hand, lots of large cardinals like those due to Mahlo are consistent with V = L, so the existence of measurables is pretty strong. But what are Woodin cardinals? And when are you going to tell me about the status of CH itself?

EST: First of all, the large cardinals like those of Mahlo, etc., that are consistent with V = L are sometimes called “small” large cardinals, while those like the measurables and beyond that are inconsistent with V = L are called “large” large cardinals. Woodin cardinals are among the latter; they are located in a hierarchy of large large cardinals above measurable cardinals and below supercompact cardinals and are given by a rather technical definition (cf. Kanamori 1994, 471).

SAB: You say the assumption they exist was made by Martin and Steel, so that seems to suggest that their existence hasn’t been proved. Or is it intuitively clear that their existence should be accepted?

EST: Yes and no: there is a linear hierarchy of known large cardinal assumptions increasing in consistency strength containing the measurable cardinals, Woodin cardinals, supercompact cardinals and many more. Now the existence of large cardinals at a given stage proves the consistency of the existence of all smaller large cardinals, so by Gödel's incompleteness theorem, the latter can't be proved without such an additional assumption. And, the individual statements of existence of large cardinals is by no means intuitively evident, but experts in the field have argued why it is plausible to assume their existence. One argument in favor of their assumption is that they serve to answer classical questions about the projective hierarchy and unify the results in a beautiful way. Another, more general, argument is that when natural extensions of ZFC are compared as to consistency strength, they also fall into a linear hierarchy, even when they contradict each other, as do, say, ZF +AC and ZF + not-AC. It has been empirically observed that whenever T_1 and T_2 are two theories in this hierarchy of natural extensions of ZF that have the same consistency strength, that is mediated by a large cardinal assumption of the same strength as both (cf. Steel 2000, 426). In some sense, the linear hierarchy of large cardinals is the backbone of the linear hierarchy of natural extensions of ZF when compared as to consistency strength.

SAB: That doesn't sound very convincing to me as an argument to accept the existence of such large cardinals, but I'll accept the plausibility arguments for the moment. I keep trying to come back to CH itself, which is supposed to be a candidate for the new problem on our list. What can you tell us about that, given all this new work?

EST: Well, now we're getting into speculative territory. Levy and Solovay (1967) showed that CH is consistent with and independent of *all* such large cardinal assumptions, provided of course that they are consistent. So the assumption of even (Large) Large Cardinal Axioms (LLCAs) is not enough; something more will be required.

SAB: Like what?

EST: Some of the experts think that one of the most promising avenues is that being pursued by Woodin (2005a, 2005b) via his strong O -logic conjecture which, if true, would imply that the cardinal number of the continuum is $\aleph_2$ But the explanation of that would take a bit more time (cf. also Koellner 2010, 212 ff).

SAB: Hmm. I won't ask you to explain that to us, or to ask how one would convince oneself of its truth, if even LLCAs are not enough. Anyhow, our time is up, and thank you for all this valuable information and advice.

Next!

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What would be difficult for the SAB as representatives of the mathematical establishment in this imagined interview about whether to add CH to its list is that the usual idea of mathematical truth in its ordinary sense is no longer operative in the research programs of Martin, Steel, Woodin, et al. which, rather, are proceeding on the basis of what seem to be highly unusual (one might even say, metaphysical) assumptions. And even though the experts in set theory may find such assumptions compelling from their experience of working with them and through the kind of plausibility arguments indicated above, the likelihood of their being accepted by the mathematical community at large is practically nil. So if a resolution of CH were to come out of a pursuit of these programs, mathematicians outside of set theory would have no way of judging the truth or falsity of CH on that basis. Thus, given our present understanding of the status of CH, it would not be a good bet for the SAB to add it to its Prize list.

The situation is not at all like that of the case of AC, which was long resisted by significant parts of the mathematics community, but came to be accepted by the vast majority when Zermelo's arguments (and those of others) sank in: namely AC is both a simple intuitively true statement about the universe of arbitrary sets (granted the concept of such) and its use underlies many common informal mathematical arguments previously considered unobjectionable even by the critics of AC (cf. Moore 1982) . Neither of these applies to the extraordinary set theoretical hypotheses in question here.

Of course, none of this by itself establishes that CH is not a definite mathematical problem, but it surely has to give one pause and ask if the concepts of arbitrary set and function that are essential to its formulation are indeed as definite as one thought, despite their ubiquity in modern mathematics. For that we have to dig deeper into the philosophical presumptions of set theory within a view of the nature of mathematical truth more generally. If one is not a formalist, finitist, constructivist, or predicativist, etc., what are the options? The well-known difficulties of platonism have left it with few if any adherents, though it has re-emerged in some forms of mathematical structuralism. Others have retreated to a deflationary view of mathematical truth (e.g., Burgess in Maddy 2005, 361-362, but that leaves us just as much at sea when we ask whether or not CH is true. Still others would put philosophical considerations aside in favor of the judgment of working mathematicians: "mathematics is as mathematics does." Some among those (e.g. Maddy 1997, 2005) combine that with specifically methodological guidelines in the case of set theory, with slogans like "maximize", but that does nothing to tell us why we should accept something as true if it is the result of such.

[4]2. Conceptual Structuralism

2(a) Mathematical structuralism and structuralist philosophies

Mathematical practice has been increasingly dominated by structuralist views since the beginning of the 20th century. Their explicit inception in the 19th century is often credited to Dedekind. But I would argue that mathematicians always regarded their subject matter in structuralist terms, if only implicitly, since their concern throughout was not with *what* mathematical objects "really are", but with how they relate to each other, and how they can be a subject of systematic computation and reasoning. True, one had the successive questions: What is zero?, What are negative quantities?, What are irrational magnitudes?, What are imaginary numbers?, What are infinitesimal quantities?, etc., etc. But the "what" had to do with the problem as to how one could coherently fit systematic use of these successively new ideas with what had previously been recognized as mathematically meaningful. Hilbert is of course famous for his early espousal of an explicit structuralist point of view in his work on the foundations of geometry. The great

[4] This section of the paper is largely taken from Feferman (2009a, 2010a).

contemporary of Hilbert, Henri Poincaré, though known for his emphasis on quite different, more intuitive aspects of mathematics from Hilbert, also voiced a structuralist view of the subject:

> Mathematicians do not study objects, but the relations between objects; to them it is a matter of indifference if those objects are replaced by others, provided that the relations do not change. Matter does not engage their attention, they are interested by form alone. (Poincaré 1952, 20)

In the 20th century the Bourbaki group led the way in promoting a systematic structuralist approach to mathematics, and that has been continued in a specific way into the 21st century by the category theorists led by Saunders Mac Lane.

Structuralist philosophies of mathematics have emerged in the last thirty years as a competitor to the traditional philosophies of mathematics. An early expression is found in the famous article by Benacerraf, "What numbers could not be" (1965). Systematic efforts at the development of a structuralist philosophy of mathematics have been given in Hellman (1989), Resnik (1997), Shapiro (1997) and Chihara (2004), among others; for a survey of some of the leading ideas, see Hellman (2005). Closest to the views here but with quite opposite conclusions re CH is Isaacson (2008).

2(b) The theses of Conceptual Structuralism

[5]My version of such a philosophy, Conceptual Structuralism, is meant to emphasize the source of all mathematical thought in human conceptions. It differs in various respects from the ones mentioned and is summarized in the following ten theses.

1. The basic objects of mathematical thought exist only as mental conceptions, though the source of these conceptions lies in everyday experience in manifold ways, in the processes of counting, ordering, matching, combining, separating, and locating in space and time.

[5] I began developing these ideas in the late 1970s and first circulated them in notes in 1978; cf. Feferman (2009a) 170, fn. 2.

2. Theoretical mathematics has its source in the recognition that these processes are independent of the materials or objects to which they are applied and that they are potentially endlessly repeatable.

3. The basic conceptions of mathematics are of certain kinds of relatively simple ideal world-pictures which are not of objects in isolation but of structures, i.e. coherently conceived groups of objects interconnected by a few simple relations and operations. They are communicated and understood prior to any axiomatics, indeed prior to any systematic logical development.

4. Some significant features of these structures are elicited directly from the worldpictures which describe them, while other features may be less certain. Mathematics needs little to get started and, once started, a little bit goes a long way.

5. Basic conceptions differ in their degree of clarity. One may speak of what is true in a given conception, but that notion of truth may only be partial. Truth in full is applicable only to completely clear conceptions.

6. What is clear in a given conception is time dependent, both for the individual and historically.

7. Pure (theoretical) mathematics is a body of thought developed systematically by successive refinement and reflective expansion of basic structural conceptions.

8. The general ideas of order, succession, collection, relation, rule and operation are premathematical; some understanding of them is necessary to the understanding of mathematics.

9. The general idea of property is pre-logical; some understanding of that and of the logical particles is also a prerequisite to the understanding of mathematics. The reasoning of mathematics is in principal logical, but in practice relies to a considerable extent on various forms of intuition in order to arrive at understanding and conviction.

10. The objectivity of mathematics lies in its stability and coherence under repeated communication, critical scrutiny and expansion by many individuals often working

independently of each other. Incoherent concepts, or ones which fail to withstand critical examination or lead to conflicting conclusions are eventually filtered out from mathematics. The objectivity of mathematics is a special case of intersubjective objectivity that is ubiquitous in social reality.

There is not time to elaborate these points, but various aspects of them will come up in our discussion of two constellations of structural notions, first of objects generated by one or more "successor" operations, and second of the continuum.

Before digging into these structures, I want to address a common objection to locating the nature of mathematics in human conceptions, namely that it does not account for the objectivity of mathematics. I disagree strongly, and in support of that appeal to the objectivity of much of social reality. That is so pervasive, we are not even aware that much of what we must deal with in our daily lives is constrained by social institutions and social facts. In this respect I agree fully with John Searle, in his book, *The Construction of Social Reality*, and can hardly do better than quote him in support of that:

> [T]here are portions of the real world, objective facts in the world, that are only facts by human agreement. In a sense there are things that exist only because we believe them to exist. ... things like money, property, governments, and marriages. Yet many facts regarding these things are 'objective' facts in the sense that they are not a matter of [our] preferences, evaluations, or moral attitudes. (Searle 1995, p.1)

[6]Searle goes on to give examples of such facts (at the time of writing) as that he is a citizen of the United States, that he has a five dollar bill in his pocket, that his younger sister got married on December 14, that he owns a piece of property in Berkeley, and that the New York Giants won the 1991 Superbowl. He might well have added board games to the list of things that exist only because we believe them to exist, and facts such as that in the game of chess, it is not possible to force a checkmate with a king and two knights

[6] Objective facts for others might be that they are not citizens of the United States, or that they are not married, or that they are married in some US States but not in others, or that they have no money in their pockets. Purported facts that might be up for dispute are that one is fishing beyond the 12 nautical mile territorial waters off the coast of Maine.

against a lone king. Unlike facts about one's government, citizenship, finances, property, marital relations, and so on, that are vitally important to our daily welfare, since they constrain one's actions and determine one's "rights", "responsibilities" and "obligations", facts about the structure and execution of athletic games and board games are not essential to our well-being even though they may engage us passionately. In this respect, mathematics is akin to games; the fact that there are infinitely many prime numbers is an example of a fact that is about our conception of the integers, a conception that is as clear as what we mean, for example, by the game of chess or the game of go.

7 Searle's main concern is to answer the question, "How can there be an objective reality that exists in part by human agreement?", and his book is devoted to giving a specific account of the nature of certain kinds of social facts, and of what makes them true. That account is open to criticism in various respects, but what is not open to criticism is what it is supposed to be an account of. That is, we must take for granted the phenomenon of intersubjective objectivity about many kinds of social constructions, be they governments, money, property, marriages, games, and so on. (It should be stressed that we are not dealing with the in many respects antiscientific viewpoint of social constructivism in post-modernist and deconstructionist thought.) My claim is that the basic conceptions of mathematics and their elaboration are also social constructions and that the objective reality that we ascribe to mathematics is simply the result of intersubjective objectivity about those conceptions and not about a supposed independent reality in any platonistic sense. Also, this view of mathematics does not require total realism about truth values. That is, it may simply be undecided under a given conception whether a given statement in the language of that conception has a determinate truth value, just as, for example, our conception of the government of the United States is underdetermined as to the presidential line of succession past a certain point.

2(c) Conceptions of Sequential Generation

The most primitive mathematical conception is that of the positive integer sequence

7 According to Wikipedia, that is now specified up to #17, the Secretary of Homeland Security.

represented by the tallies: |, ||, |||, ... Mathematics begins when we conceive of these as being generated from a fixed initial unit by repeatedly associating with each term n a unique successor Sc(n), without bound; the objects thus generated form the collection N_{++} of positive integers. Implicit in this conception is the order relation m < n, which holds when m precedes n in the generation of N_{+}. Our primitive conception is thus that of a structure (N_{+}, 1, Sc, <), about which certain facts, if we think about them, are evident: that < is a total ordering of N for which 1 is the least element, and that m < n implies Sc(m) < Sc(n). At a further stage of reflection we may recognize the least number principle for the positive integers, that if P(n) is any definite property of members of N_{+} and there is some n such that P(n) then there is a least such n.

[+]As we reflect on a given structure we are tempted to elaborate it by adjoining further relations and operations and to expand basic principles accordingly. In the case of the positive integers N the operation of addition, m + n, is clearly understood through the conception of integers as represented by tallies, for example + is obtained by concatenating the tallies for m and n respectively, so that in particular n + 1 = sc(n). Then we think of m × n as “m added to itself n times”. The basic properties of the + and × operations such as commutativity, associativity, distributivity, and cancellation are initially recognized only implicitly.

[44]As we know, already with these concepts and their basic properties, and derivative ones such as the relation of divisibility and the property of being a prime number, a wealth of interesting mathematical statements can be formulated and investigated as to their truth or falsity, to begin with the existence of infinitely prime numbers, the question of infinity of the twin prime numbers, the questions about perfect numbers, the solvability and unsolvability of various diophantine equations, the theory of congruences, and so on. Incidentally, the least number principle was implicitly applied, for example, by Fermat, in his use of the “method of descent” to establish the non-existence of solutions of $x + y_4 = z$ in positive integers.

[+]The conception of (N, 1, Sc, <, +, ×) is so clear that, again implicitly, at least, there is no question in the minds of (amateur or professional) number theorists--nor should there be any question in our minds--as to the definite meaning of such statements and the assertion that they are true or false, independently of whether we can establish them one way or the other. In other words, in more modern terms, we should accept realism in truth values for statements about this structure; then the application of classical logic in reasoning about such statements is automatically legitimized. Thus, despite the subjective source of the positive integer structure in the collective human understanding, there is no reason to restrict oneself to intuitionistic logic on subjectivist grounds.

I have described in Feferman (2009a) how reflection on the structure of positive integers leads us to conceive of and reduce to it the concepts of the arithmetical structures on the natural numbers N, the integers Z and the rational numbers Q, for which truth in these structures is a definite notion. But it should be emphasized that the general scheme of induction,

$P(0) \wedge \forall n[P(n) \rightarrow P(Sc(n))] \rightarrow \forall n\, P(n)$, needs to be understood in an open-ended sense, by which I mean that it is accepted for

any definite property P of natural numbers that one meets in the process of doing mathematics, no matter what the subject matter and what the notions used in the formulation of P.[8] The question–What is a definite property?–may require in each instance the mathematician's judgment. For example, the property,"n is an odd perfect number", is a definite property, while "n is a feasibly representable number" is not. What about "GCH is true at n"? Returning to the starting point of finite generation under a successor operation, it is also natural to conceive of generation under more than one such operation, Sc_a where *a* is an element of an index collection A. We may conceive of the objects of the resulting structure as "words on the alphabet A", with $Sc_a(w) = wa$ in the sense of concatenation. The mathematics of this conception is simply the theory of concatenation, which is

[8] In Feferman (1996) I introduced the concept of the *unfolding* of an open-ended system T based on axioms in the usual sense of the word and open-ended schemata, to characterize what operations and relations, and what principles concerning, them ought to accepted if one has accepted those provided by T. In Feferman and Strahm (2000) we showed that the unfolding of schematic number theory is of proof-theoretical strength the same as predicative analysis.

equivalent to the theory of natural numbers when A has a single element. Of special interest below is the case that A = {0, 1}, for which we also conceive of the words on A as the finite paths in the binary branching tree, alternatively as the end nodes of such paths.

2 (d). Conceptions of the continuum

The question of the determinateness of CH in its weak form depends on our conception of three things: (1) the continuum, (2) subsets of the continuum, and (3) mappings between such subsets. As to (1), there is in fact no unique concept, but rather several related ones, and part of the problem whether CH is a genuine mathematical problem is that there is a certain amount of conflation of these concepts with a resulting muddying of the picture. I shall be relatively brief about each of these since I have expanded on them in Feferman (2009a).

(i) *The Euclidean continuum*. The conception is semi-structural. Straight lines are conceived of as lying in the plane or in space. They are perfectly straight and have no breadth. Points are perfectly fine; they are conceived of as the objectification of pure locations. A point may lie on a line, but the line does not consist of the set of its points. The idea of arbitrary subset of a line or of the removal of some points from a line is foreign to Euclidean geometry.

(ii) *The Hilbertian continuum*. The conception is structural: one has points, lines, planes, incidence, betweenness and equidistance relations. Continuity is expressed settheoretically following Dedekind.

(iii) *The Dedekind real line*. This is a hybrid (arithmetical/geometric) notion that can be expressed in structural terms relative to the rationals. Continuity is expressed by the Dedekind cut condition.

(iv) *The Cauchy-Cantor real line*. This is again a hybrid structural notion relative to Q. Continuity is expressed by the Cauchy convergence condition, used by Cantor to "construct" the reals.

N (v) *The set 2*, i.e. the set of infinite sequences of 0's and 1's, i.e. all paths in the full binary tree.

(vi) *The set of all subsets of* N, *S(N).*

Not included here are physical conceptions of the continuum, since our only way of expressing them is through one of the mathematical conceptions via geometry or the real numbers.

NNEuclidean geometry aside, so far as CH is concerned, set theory erases the distinctions between the notions in (ii)-(vi) by identifying sequences with sets and by considering only the cardinality of the various continua. But there is also a basic conceptual difference between 2 and S(N), with the elements of 2 pictured as the result of an unending sequence of choices, 0 or 1 (left or right) while the elements of S(N) are the result of sprinkling 1s through the natural numbers in some simultaneous manner and then attaching 0s to the rest. Of course, with each set is associated a sequence as its

characteristic function and vice versa, but that requires a bit of reasoning–the equivalence of these notions is not conceptually immediate.

2(e). Set theoretical conceptions

Setting aside this difference for the moment, let's consider the standard set theoretical account. Sets in general are supposed to be definite totalities A determined solely by

which objects are in the membership relation (∈) to them, and independently of how they may be defined, if at all. That is, two sets A, B which agree as to their members are

identical; in other words the Axiom of Extensionality holds. Suppose given a set A of "individuals"; a subset X of A, in symbols X ⊆ A, is a set every element of which is an

element of A. Then the basic idea of S(A), the "set of all subsets of A" is that it consists of *all* the sets X ⊆ A and only those. From the structural point of view, when we are dealing with S(A) it is through the two-sorted structure (A, S(A), ∈).

When a collection C is a definite totality, the logical operation of quantification over C leads from definite properties to definite properties. In these terms one sees that the terminology, "the set of all subsets of A" is tendentious since it implicitly assumes that

S(A) is a set, and hence that it is a definite totality, so that the operation of quantification over S(A), i.e. ($\forall X \subseteq A$) P(X), has a determinate truth value for each property P(X) that has a determinate truth value for each $X \subseteq A$. This is the usual conception of S(A) in set theory, whether informal or axiomatic. However, one may alternatively consider S(A) to

$_n$be an *indefinite* collection whose members are subsets of A, but whose exact extent is indeterminate. To distinguish the two views of S(A), I will refer to the former as the *settheoretical* one and the latter as the *open-ended* one. The set-theoretical view of S(A) is certainly justified when A is finite; if A has n elements the 2 elements of S(A) can be completely listed (at least in principle). So the only case where the distinction would be significant is when A is infinite, and the first case in which that is met is for A = N, the set of natural numbers.

The set-theoretical view of S(A) is justified by a thorough-going platonism[9], as accepted by Gödel. According to this view, sets in general have an existence independent of human thoughts and constructions, and in particular, for any set A, S(A) is the definite totality of arbitrary subsets of A. A major problem with this view is the classic one of epistemic access raised most famously by Paul Benacerraf (1965). What the platonistic set-theoretical view has going for it, among other things, is the supposed determinateness of statements about S(A) involving quantification over S(A). The open-ended view, on the other hand, rests on conceptual structuralism, for which epistemic access is no problem, but the determinateness of various statements then comes into question.

So, now, on the set theoretical point of view, the power set operation can at least be iterated finitely often and in particular both S(N) and S(S(N)) are definite totalities. The weak form of CH takes the form: for every subset X of S(N) either X is in 1-1 correspondence with N or it is in 1-1 correspondence with S(N). In order for this to be a determinate question, both S(N) and S(S(N)) have to be definite totalities, and for each subset X of S(N) there has to be a definite totality of all possible 1-1 functions whose domain is X and whose range is N or S(N). Prima facie, the assumption of all these definite totalities is only justified on the grounds of platonistic realism. One can try to

9 Or Robust Realism, as Maddy (2005) terms it.

sidestep that by posing the question of CH within an axiomatic theory T of sets, but then one can only speak of CH as being a determinate question relative to T.

In the familiar theories T of sets, not only is it accepted that the power set operation can be iterated finitely often, it is posited that it can be iterated transfinitely often in a cumulative way. This is a *new* idea of sets, not implicit in what has been said so far, which expands the concept of arbitrary set to that of a "universe" V of "all sets" with the usual picture of V as fanning out from the class On of "all ordinals", sliced by the sets V at each a in On, where V_0 is the empty set, each $V_{a+1} = S(V_a)$ and for limit a , V = the union of all $V_{ßaa}$ for ß < a . On this picture, N is represented as a subset of the hereditarily finite sets V_ω, and then $S(N) \subseteq V_{\omega+1}$, and $S(S(N)) \subseteq V_{\omega+2}$. There is no problem to put oneself in the mental frame of mind of "this is what the cumulative hierarchy looks like",

for which one can see that such and such propositions including the axioms of ZFC are (more or less) obviously true. I have taught set theory many times and have presented it in terms of this ideal-world picture with only the caveat that *this is what things are supposed to be like in that world*, rather than to assert that that's the way the world actually is. I am no more uncomfortable doing that than if I were teaching Newtonian mechanics and talking of a world in which space-time is Euclidean, objects are point masses, and we have action at a distance, e.g. under gravitational force, or alternatively if I were teaching relativistic mechanics. And one can see why little more is needed conceptually to develop an extraordinary part of set theory; as in the case of number theory, a little bit goes a long way. The issue is not whether we can think in such terms but, rather, what the philosophical status is of that kind of thought.

How does this look from the viewpoint of conceptual structuralism? According to the ideal-world story that has been told us: whether looked at directly over N or as part of $V_{\omega+2}$, S(S(N)) is a definite totality, quantification over it is well-determined, and CH is a definite problem. But saying that these things are *conceived* as being definite does not *make* them definite. Daniel Isaacson espouses a form of conceptual structuralism in which he tries to make the case for that. He writes:

> The basis of mathematics is conceptual and epistemological, not ontological, and understanding particular mathematical structures is prior to axiomatic

characterization. When such a resulting axiomatization is categorical, a particular mathematical structure is established. Particular mathematical structures are not mathematical objects. They are characterizations. (Isaacson 2008, 31)

So Isaacson makes a very strong demand on conceptions of certain particular structures, namely that they have categorical axiomatizations, i.e. they are exemplified in the conceived structure and any two structures that exemplify them are isomorphic. Now the only axiomatizations in sight that will do this for number theory and set theory are formulated in 2nd order logic where the quantifiers are interpreted as ranging over arbitrary subsets of the domains of the particular structures considered. Certainly no characterizations in 1st order logic will do the trick, by the Löwenheim-Skolem results. And indeed, there are 2nd order characterizations not only of the natural numbers, but of each of the structures mentioned above in which CH is formulated. Moreover, there is such a characterization of $(V_a, \in)$ for a = the first strongly inaccessible ordinal, and that structure is a model of ZFC. All of this is established in ZFC as a background set theory [10](in the last case under the assumption that that a exists).[11] Clear as these results are as theorems within ZFC about 2nd order ZFC, they are evidently begging the question about the definiteness of the above conceptions required for the formulation of CH. Isaacson follows Kreisel (1971) in the argument that CH is a definite mathematical problem because of such categoricity results. But in relying on so-called standard 2nd order logic for this purpose we are presuming the definiteness of the very kinds of notions whose definiteness is to be established. The circularity is so patent, one does not see that any more can be claimed thereby than that the definiteness of those notions may be accepted as a matter of faith. Moreover, the argument via categoricity, which takes structures as objects of a particular kind, vitiates the claim that the issues are conceptual and

[10] Isaacson makes much of the work of Zermelo (1930) in which a general semicategoricity result is proved for ZFC, i.e. ZFC formulated in 2nd order terms, and that was reproved in greater detail by John Shepherdson in his three part paper on inner models for set theory in the *J. Symbolic Logic* 16-18. This takes the form that if M_1, M_2 are any two structures satisfying ZFC, M_1 is isomorphically embeddable in M_2 as a transitive submodel, or vice versa.

[11] Väänänen (2001) has fully deconstructed the facile appeal to "standard" 2nd order logic in this and other foundational arguments.

epistemological, not ontological.

$_N$My own conceptual structuralism forces me to be much more modest. First of all, each of the conceptions (i)-(vi) of the continuum is treated in its own right, not necessarily equivalent to the others in terms of cardinality. Secondly, the intuitive distinction brought up above between sets and sequences is maintained, with sequences having a greater clarity than sets. For example, we have a much clearer conception of arbitrary sequences of points on the Hilbert (or Dedekind, or Cauchy-Cantor) line, or at least of bounded strictly monotone sequence, than we do of arbitrary subsets of the line. And that also makes a difference when we come to the conceptions (v) of 2_N and (vi) of S(N). We have a clearer conception of what it means to be an arbitrary infinite path through the full binary tree than of what it means to be an arbitrary subset of N, but in neither case do we have a clear conception of the totality of such paths, resp. sets. In both cases, our conception allows us to reason that there is no enumeration of all infinite paths through the binary tree, resp. of all subsets of N, simply by the appropriate diagonal argument. But it is an idealization of our conceptions to speak of 2, resp. S(N), as being definite totalities. And when we step to S(S(N)) there is a still further loss of clarity, but it is just the definiteness of that that is needed to make definite sense of CH.

If the concept in general of the totality of arbitrary subsets of a given infinite set A is not definite then how could we sharpen it to make it definite? Well, we could say that we are

talking only about those subsets of A that are defined in a certain way–but then we are no longer talking about *arbitrary* subsets. This holds for any proposed sharpening or

constraint on the concept of being arbitrary in the case of sets. In other words, the concept of the totality of arbitrary subsets of A is essentially underdetermined or vague, since any sharpening of it violates what the concept is supposed to be about. But this is also why (almost all) set-theorists themselves reject Gödel’s Axiom of Constructibility, V = L, that says all sets in the cumulative hierarchy are successively definable in a specific way.

One argument that is made for accepting the definiteness of the continuum according to the set-theoretical point of view is that, as applied to it in its guise as the arithmetical/geometric structure of the real line, it is unquestionably needed to develop modern analysis, and modern analysis is used in an essential way in the formulation of the laws of physical science and derivation of their consequences. It may even be argued that this shows that the real line in the set-theoretical sense is somehow physically exemplified in the real world. However, a number of case studies support the thesis that I advanced in Feferman (1993) that all scientifically applicable mathematics can be

carried out in a completely predicative way–in fact in a certain formal system W that has been shown in Feferman and Jäger (1993, 1996) to be a conservative extension of

Peano Arithmetic (PA). The designation 'W' is in honor of Hermann Weyl, who got predicative analysis underway in his groundbreaking work *Das Kontinuum* (1918), using the open-ended sequential (Cauchy-Cantor) view of the real numbers rather than the Dedekind point of view. Whether or not this thesis will hold for all future applications of mathematics to physical science, what this shows is that the argument from the success of physics to the reality of the set-theoretical conception of the real numbers is completely vitiated.

Nevertheless, one might argue for an intermediate position between that of conceptual structuralism, which rejects the continuum as a definite totality, and the set-theoretical account which not only accepts that but also much, much more. Namely, one may grant as a working apparently robust idea the concept of S(N), but nothing higher in the cumulative hierarchy. This would justify the assumption of Dedekind or Cantor completeness of the real line with respect to all sets definable by quantification over the continuum, thus going far beyond predicative mathematics into the domain of descriptive set theory. In logical terms, that would justify working in a system of strength full 2^{nd} order number theory, or "analysis" as it is justly called. There is no question that all of extant scientifically applicable mathematics can be reduced to working in that and in fact in its relatively weak subsystem denoted Π^1_1-CA (cf. Simpson 1999, 2010).

3. A proposed logical framework for what's definite (and what's not)

According to the finitists, the natural numbers form an incompleted or indefinite totality and the general operations of universal and existential quantification over the natural numbers are not to be admitted in reasoning about them except in bounded form. The intuitionists share the finitists' general point of view about the natural numbers but regard unbounded quantification over them as meaningful though not when combined with classical logic. According to the predicativists, the natural numbers form a definite totality, but not the collection of “all” sets of natural numbers. Nevertheless systems of predicative analysis have been formulated using quantification with respect to variables for such sets within classical logic. Finally, we have the case of set theory, in which according to practically all points of view–including that of the mathematical practitioners from the very beginning to the present as well as both the philosophical defenders and critics–sets in general form an indefinite totality, otherwise the universe of all sets would be a set and thus give rise to Russell’s Paradox. All of this poses questions about the proper formulation of axiomatic systems in which some of the domains involved are conceived to form indefinite totalities, while others, or parts of them, are considered to be definite totalities. There has been much interesting philosophical discussion in recent years about the problems raised by absolute generality (cf. Rayo and Uzquiano 2006) both in its unrestricted sense and more specifically as applied to set theory.

My main concern here is rather to see what precisely can be said about certain ways of construing in formal terms the distinction between definite and indefinite concepts. One way of saying of a statement f that it is definite is that it is true or false; on a deflationary account of truth that’s the same as saying that the Law of Excluded Middle (LEM) holds of f , i.e. one has f ∨ ¬ f . Since LEM is rejected in intuitionistic logic as a basic principle, that suggests the slogan, “What’s definite is the domain of classical logic, what’s not is that of intuitionistic logic.” In the case of predicativity, this would lead us to consider systems in which quantification over the natural numbers is governed by classical logic while only intuitionistic logic may be used to treat quantification over sets of natural numbers or sets more generally. And in the case of set theory, where every set is conceived to be a definite totality, we would have classical logic for bounded quantification while intuitionistic logic is to be used for unbounded quantification. The formal study of systems of the latter kind goes back at least to a Wolf(1974) and

Friedman (1980) and has most recently been carried on in the paper, Feferman (2010b).[12] The general pattern of the studies there is that we start with a system T formulated in fully classical logic, then consider an associated system SI-T formulated in a mixed, semi-intuitionistic logic, and ask whether there is any essential loss in proof-theoretical strength when passing from T to SI-T. In the cases that are studied, it turns out that there is no such loss, and moreover, there can be an advantage in going to such an SI-T; namely, we can beef it up to a semi-constructive system SC-T without changing the proof-theoretical strength, by the adjunction of certain principles that go beyond what is admitted in SI-T, because they happened to be verified in a certain kind of functional interpretation of the intuitionistic logic.

For example, if one starts with S equal to the classical system $KP\omega$ of admissible set theory (including the Axiom of Infinity), the associated system SI-$KP\omega$ has the same axioms as $KP\omega$, and is based on intuitionistic logic plus the law of excluded middle for all bounded (i.e. Δ_0) formulas. But the beefed up system SC-$KP\omega$ proves the Full Axiom of Choice Scheme for sets,

$$\forall x \in a\ \exists y\ f(x,y) \rightarrow \exists r\ [\mathrm{Fun}(r) \wedge \forall x \in a\ f(x, r(x))],$$

where f (x, y,…) is an arbitrary formula of the language of set theory. That in turn implies the Full Collection Axiom Scheme,

$$\forall x \in a\ \exists y\ f(x,y) \rightarrow \exists b\ \forall x \in a\ \exists y \in b\ f(x,y)$$

for f an arbitrary formula, while this holds only for Σ_1 formulas in SI-$KP\omega$.

In addition, SC-$KP\omega$ contains some other non-intuitionistic principles that can, nevertheless, be given a semi-constructive interpretation. The main result in Feferman (2010b) is that all of these systems are of the same proof-theoretical strength, $KP\omega$ = SI$KP\omega$ = SC- $KP\omega$. Moreover, the same result holds when we add the power set axiom

12th See also the slides Feferman (2009b) for the conference in honor of Harvey Friedman's 60 birthday.

Pow as providing a definite operation on sets, or just its restricted form Pow(ω) asserting the existence of the power set of ω .

Now the choice of KPω ± Pow(resp. Pow(ω)) and the associated semi-constructive systems for initial consideration rather than ZFC ± Pow(resp. Pow(ω)) is dictated by the idea that we should only accept the Separation and Replacement schemes for definite properties. And since our idea of quantification as a definite logical operation is that it is only to range over definite totalities, at first sight only those formulas in which quantification is restricted to sets are to be admitted to those schemes. In particular, in view of the discussion at the end of the preceding section it should be of particular interest to investigate what properties and statements are definite in the specific case of the semi-constructive version of KPω + Pow(ω).

In general, given one of the semi-constructive systems SC-T under consideration, one would like to establish that if a formula f (x) is definite in the sense that ∀x[f (x)∨ ¬ f (x)] is provable there, then f is at least equivalent to a Δ_1 formula and hence is absolute for end-extensions. I conjecture that that is the case for the systems of SC-KPω and its extensions by Pow(ω) and even full Pow, but have not yet found a proof. My guess is that that can be done using a detailed analysis of the functional interpretations used in establishing the results about the proof-theoretical strength of these systems in my (2010b) paper.

Similarly, a statement f should be considered definite in this framework for a given semiconstructive system SC-T if LEM holds for f , i.e. f ∨ ¬ f , can be proved there. In pure

intuitionistic systems we have the disjunction property, that if f ∨ ψ is proved then either f or ψ is provable; but we don't have such a result for the semi-constructive systems

considered. For, every arithmetical statement satisfies the LEM in our system but we certainly can't prove or disprove every such statement in the system. But we could hope to show that if LEM is provable for f then it must at least be equivalent to a Δ_1 sentence.

Turning to statements of classical set theory that are of mathematical interest, one would like to know which of them make essential use of full non-constructive reasoning and which can already be established on semi-constructive grounds. My guess is that a great deal–including all of classical DST–can already be done in SC-KPω + Pow(ω).

Turning finally to the Continuum Hypothesis (CH), that, in particular, is certainly meaningful in SCS + (Pow(ω)) and formally it is a definite statement in SCS + (Pow), i.e. one can prove CH $\vee$ -CH there. As with the semi-intuitionists, one can regard the conception of the set of all subsets of ω as clear enough, but not that of arbitrary subset of

[13]*that*, and the meaning of CH depends essentially on quantifying over all such subsets. Presumably, CH is not definite in SCS + (Pow(ω)), and it would be interesting to see why that is so. My own view, voiced elsewhere, is that CH is what I have called an essentially vague statement, which says something like: there is no way to sharpen it to a definite statement without essentially changing the meaning of the concepts involved in it. But to formulate that idea more precisely within the semi-constructive framework, some stronger notion than formal definiteness may be required.

References Benacerraf, P. (1965), What numbers could not be, *Phil. Review* 74, 47-73, reprinted in Benacerraf and Putnam (1983), 272-294. Benacerraf, P. and H. Putnam (eds.) (1983) *Philosophy of Mathematics. Selected* nd*Readings,* 2 edn., Cambridge Univ. Press, Cambridge. Browder, F. E. (1976) (ed.), *Mathematical Developments arising from Hilbert Problems*, Proc. Symposia in Pure Mathematics 28, Amer. Math. Soc., Providence, RI. Chihara, C. S. (2004), *A Structural Account of Mathematics*, Clarendon Press, Oxford. Feferman, S. (1993), Why a little bit goes a long way: Logical foundations of scientifically applicable mathematics, in *PSA 1992*, Vol. 2, Philosophy of Science Association, East Lansing; reprinted as Ch. 14 in Feferman (1998).
Feferman, S. (1996), Gödel's program for new axioms. Why, where, how and what?, in (P. Hajek, ed.) *Gödel '96*, Lecture Notes in Logic 6 (1996), 3-22.

[13] One would not expect CH would to be unique in this respect; any statement that makes essential use of the assumed totalities S(S(N)) would be a candidate for such a result.

Feferman, S. (1998), *In the Light of Logic*, Oxford University Press, New York.
Feferman, S. (2009a), Conceptions of the continuum, *Intellectica* 51, 169-189.
Feferman, S. (2009b) What's definite? What's not?, slides for talk at Harvey Friedman 60th birthday conference, Ohio State Univ., May 16, 2009, http://math.stanford.edu/~feferman/papers/whatsdef.pdf .

Feferman, S. (2010a), Conceptual structuralism and the continuum, slides for talk at Phil Math Intersem 2010 Université Paris 7-Diderot, 6/08/10 (revision of slides for talk at VIIIth International Ontology Congress, San Sebastián, 10/01/08). http://math.stanford.edu/~feferman/papers/Conceptual_Structuralism.pdf .

Feferman, S. (2010b), On the strength of some semi-constructive theories, in Feferman and Sieg (2010), 109-129.

Feferman, S. and G. Jäger (1993), Systems of explicit mathematics with non-constructive µ-operator, I, *Annals of Pure and Applied Logic* 65, 243-263.

Feferman, S. and G. Jäger (1993), Systems of explicit mathematics with non-constructive µ-operator, II, *Annals of Pure and Applied Logic* 79, 37-52.

Feferman, S., Parsons, C., and Simpson, S. G. (2010) (eds.), *Kurt Gödel. Essays for his centennial*, Lecture Notes in Logic 33, Assoc. for Symbolic Logic, Cambridge Univ. Press, Cambridge.

Feferman, S. and W. Sieg (eds.) (2010), *Proofs, Categories and Computations. Essays in honor of Grigori Mints*, College Publications, London.

Feferman, S. and T. Strahm (2000), The unfolding of non-finitist arithmetic, *Annals of Pure and Applied Logic* 104, 75-96.

Friedman, H. (1980), A strong conservative extension of Peano Arithmetic, in (J. Barwise, et al., eds.) *The Kleene Symposium*, North-Holland, Amsterdam, 113-122.

Gödel, K. (1938), The consistency of the axiom of choice and of the generalized continuum hypothesis, *Proc. National Academy of Sciences, USA* 24, 556-557; reprinted in Gödel (1990), 26-27.

Gödel, K. (1940), *The Consistency of the Axiom of Choice and of the Generalized Continuum Hypothesis with the Axioms of Set Theory*, Annals of Mathematics Studies 3, Princeton Univ. Press, Princeton; reprinted in Gödel (1990), 33-101.

Gödel, K. (1947), What is Cantor's continuum problem?, *American Mathematical Monthly* 54, 515-525; errata 55, 151; reprinted in Gödel (1990), 240-251 Gödel, K. (1964). What is Cantor's continuum problem?, revised and expanded version of Gödel (1947), in Benacerraf and Putnam (1964), 258-273, reprinted in Gödel (1990), 254-270.

Gödel, K. (1990) *Collected Works*, *Vol. II: Publications 1938-1974* (S. Feferman, et al., eds.), Oxford University Press, New York. Hellman, G. (1989), *Mathematics Without Numbers*, Oxford University Press, Oxford. Hellman, G. (2005), Structuralism, in Shapiro (2005), 536-562. Isaacson, D. (2008), The reality of mathematics and the case of set theory, in (Z. Novak and A. Simonyi, eds.), *Truth, Reference, and Realism*, Central European University Press, Budapest, 1-64. Jaffe, A. M. (2006), The millennium grand challenge in mathematics, *Notices Amer. Math. Soc.* 53, no. 6, 652-660. Kanamori, A. (1997), *The Higher Infinite*, Perspectives in Mathematical Logic, Springer, Berlin.

Koellner, P. (2010), On the question of absolute undecidability, in Feferman, Parsons and Simpson (2010), 189-225.

Kreisel, G. (1971), Observations on popular discussions of foundations, in (D. S. Scott, ed.), *Axiomatic Set Theory*, Proc. Symposia in Pure Mathematics 13, Part I, Amer. Math. Soc., Providence, RI, 189-198.

Levy, A. and R. M. Solovay (1967), Measurable cardinals and the continuum hypothesis, *Israel J. of Mathematics* 5, 234-248.

Maddy, P. (1997), *Naturalism in Mathematics,* Oxford University Press, Oxford. Maddy, P. (2005), Mathematical existence, *Bull. Symbolic Logic* 11, 351-376. Martin, D. A. (1975), Borel determinacy, *Annals of Mathematics* 102, 363-371. Martin, Donald A. (1976), Hilbert's first problem: The continuum hypothesis, in Browder (1976), 81-92. Martin, D. A. and J. Steel (1989), A proof of projective determinacy, *J. Amer. Math. Soc.* 2, 71-125. Moore, G. H. (1982), *Zermelo's Axiom of Choice: Its origins, development and influence*, Springer, New York Moore, G. H. (1989) *Towards a history of Cantor's continuum problem*, ***The history of modern mathematics, vol. I: Ideas and their reception*** (David E. Rowe and John McCleary, eds.), Academic Press, Boston, pp. 78–121.

Moore, G. H. (2011), Early history of the generalized continuum hypothesis: 1878-1938, *Bull. Symbolic Logic* (to appear).

Poincaré, H. (1952), *Science and Hypothesis*, Dover Publications, New York. (English translation of *La Science et l'hypothèse*, Flammarion, Paris,1902.)

Rayo, A. and G. Uzquiano (eds.) (2006), *Absolute Generality*, Clarendon Press, Oxford. Resnik, M. (1997), *Mathematics as a Science of Patterns,* Oxford University Press, ndOxford. Searle, J. R. (1995), *The Construction of Social Reality*, Free Press, New York. Shapiro, S. (1997), *Philosophy of Mathematics: Structure and Ontology*, Oxford University Press, New York. Shapiro, S., ed. (2005), *The Oxford Handbook of Philosophy of Mathematics and Logic,* Oxford University Press, Oxford. Simpson, S. G. (1999), *Subsystems of Second Order Arithmetic*, Perspectives in Mathematical Logic, Assoc. for Symbolic Logic, Springer, Berlin; 2 edn. (2009), Perspectives in Mathematical Logic, Assoc. for Symbolic Logic, Cambridge Univ. Press, Cambridge. Simpson, S. G. (2010), The Gödel hierarchy and reverse mathematics, in Feferman et al. (2010), 109-127.

Steel, J. (2000), Mathematics needs new axioms, *Bull. Symbolic Logic* 6, 422-433. Väänänen, J. (2001), Second-order logic and foundations of mathematics, *Bull. Symbolic Logic* 7, 504-520. Weyl, H. (1918), *Das Kontinuum. Kritische Untersuchungen über die Grundlagen der Analysis*, Veit, Leipzig. (English translation by S. Pollard and T. Bole, *The Continuum. A Critical Examination of the Foundations of Analysis*, Univ. Press of America, Latham (1987).

Wolf, Robert S. (1974), *Formally Intuitionistic Set Theories with Bounded Predicates Decidable*, PhD Dissertation, Dept. of Mathematics, Stanford University.

Woodin, H. (2005a), The continuum hypothesis, in (R. Cori, et al., eds.), *Logic Colloquium 2000,* Lecture Notes in Logic 19, Assoc. for Symbolic Logic, 143-197.

Woodin, H. (2005b), Set theory after Russell: The journey back to Eden, in (G. Link, ed.), *One Hundred Years of Russell's Paradox: Mathematics, Logic, Philosophy,* de Gruyter Series in Logic and its Applications, de Gruyter, Berlin.

Zermelo, E. (1930), Über Grenzzahlen und Mengenbereiche. Neue Untersuchungen über die Grundlagen der Mengenlehre, *Fundamenta Mathematicae* 16, 29-47.