The Continuum Hypothesis is neither a definite mathematical problem nor a definite logical problem[1]

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> Cantor's continuum problem is simply the question: How many points are there on a straight line in Euclidean space? In other terms, the question is: How many different sets of integers do there exist? ... [t]he analysis of the phrase "how many" leads unambiguously to quite a definite meaning for [this] question ... namely, to find out which one of the $\aleph$'s is the number of points on a straight line.
> (Gödel 1947, 515-516, reprinted in Gödel 1990, 176-177)

> 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)

> If you come to a fork in the road, take it. (Yogi Berra)

Abstract: For all intents and purposes, the Continuum Hypothesis (CH) has ceased to exist as a definite mathematical problem for the mathematical community at large. Nevertheless, it is still considered a problem to be pursued by substantial parts of the set-theoretical community, though not in its ordinary mathematical sense but rather in some logical sense. However, I shall argue that the work to date has yet to establish it as a definite logical problem. In an Appendix, my background work on a logical framework for notions of definiteness is sketched in terms of which a recent result by Michael Rathjen that CH is formally indefinite is stated.

1. The two faces of the continuum problem. I want to begin by distinguishing mathematical problems *in the direct,* or *ordinary sense* from those *in the indirect*, or *logical sense.* This is a rough distinction, of course, but I think a workable one that is easily squared with experience. Although the Continuum Hypothesis (CH) in any of its

[1] This is a considerably revised version of Feferman (2011), a lecture that I gave at Harvard University on Oct. 5, 2011, in the series, Exploring the Frontiers of Incompleteness (EFI), organized by Peter Koellner. I am indebted to Peter for his invitation to take part in that program and for his trenchant comments Koellner (2011) on the text of that lecture as well for several useful conversations on the issues involved.

usual forms is *prima facie* a mathematical problem in the ordinary sense, it has become inextricably entwined with questions in the logical (i.e., metamathematical) sense. I shall argue that for all intents and purposes, CH has ceased to exist as a definite problem in the ordinary sense and that even its status in the logical sense is seriously in question. Since axioms for very large cardinals are taken for granted in current programs that aim to settle CH in one way or another, we need to make an extensive excursion through the cases that have been made for those. The paper concludes with an Appendix outlining background work of mine which provides a general framework for explanations of notions of definiteness relative to various philosophically motivated semi-constructive systems; this is used to state a recent result by Michael Rathjen which shows that CH is indefinite relative to a semi-constructive system for classical descriptive set theory and beyond.

Mathematicians at any one (more or less settled) time find themselves working in *media res*, proceeding from an accepted set of informal concepts and a constellation of prior results. The attitude is mainly prospective, and open mathematical problems formulated in terms of currently accepted concepts present themselves directly as questions of truth or falsity. Considered simply as another branch of mathematics, mathematical logic (or metamathematics) is no different in these respects, but it is distinguished by making specific use of the concepts of formal languages and of axiomatic systems and their models relative to such languages. So we can say that a problem is one in the logical sense if it makes essential use of such concepts. For example, we ask if such and such a system is consistent, or consistent relative to another system, or if such and such a statement is independent of a given system or whether it has such and such a model, and so on. A problem is one in the ordinary sense simply if it does not make use of the logical concepts of formal language, formal axiomatic system and models for such. Rightly or wrongly, it is a fact that the overwhelming majority of mathematicians not only deal with their problems in the ordinary sense, but shun thinking about problems in their logical sense or that turn out to be essentially dependent on such. Mathematicians for the most part do not concern themselves with the axiomatic foundations of mathematics, and rarely appeal to logical principles or axioms from such frameworks to justify their arguments. Some rare exceptions, for example, are given by proof by contradiction, the principle of induction on the natural numbers, the least upper bound principle for real numbers, and the Axiom of Choice.[2] When mathematicians speak of axioms those are instead mainly for structural concepts such as groups, rings, topological and linear spaces, categories, etc., etc. Moreover, mathematicians carry out their reasoning without appeal to (and in many cases, even knowledge of) what constitutes systematic logical justification. But most importantly, as long as mathematicians think of mathematical problems as questions of truth or falsity, they do not regard problems in the logical sense relevant to their fundamental aims insofar as those are relative to some axioms or models of a formal language.

There are borderline cases, to be sure. There is no question that the general concepts of set and function, of transfinite cardinal and ordinal numbers and of the alephs are ones that are currently accepted in the ordinary sense. Thus the same holds for the notions, for example, of inaccessible cardinal, Mahlo cardinal, measurable cardinal, etc. On the other hand, the notions of constructible set, elementary embedding, superstrong cardinal, etc., are essentially logical. But some notions that were initially presented in logical form such as weakly and strongly compact cardinals turned out to have equivalents that could be understood without appeal to logical notions. Are the projective sets of reals to be considered in the ordinary sense or the logical sense? In the 1930s the mathematicians of the "semi-intuitionistic" school of descriptive set theory took themselves to be pursuing the mathematics of definable sets of real numbers, but the notions involved certainly can be read as mathematical ones in the ordinary sense. And when one is considering a mathematical problem only in terms of its relations of equivalence or consequence relative to other propositions of the same general character, one is in an intermediate position.

2. The metamorphosis of CH from a mathematical problem to a logical problem. Some problems in mathematics present themselves at the outset as ones in the logical sense, in particular as questions of provability or unprovability from given axioms. Such was the case for the parallel postulate relative to the remaining axioms of Euclidean

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[2] Note also that proof by contradiction, proof by induction and the l.u.b. principle are in practice thought of in an open-ended schematic manner, applicable to arbitrary propositions, properties and sets, resp., without restriction in advance to any particular language.

geometry. The Continuum Hypothesis is perhaps unique in having originated as a problem in the ordinary sense and evolved into one in the logical sense. The transition is highlighted by its having slipped from its position at the turn of the twentieth century as #1 on Hilbert's iconic list of important mathematical problems to its disappearance, well by the turn of the millennium, altogether as a problem in the ordinary sense for the mainstream mathematical community. Rather, it—and much of set theory to boot—came to be treated as questions in the logical sense of independence from one axiomatic system or another or truth or falsity in one model or another. You need only look up the several compendia of mathematical problems on the internet for evidence of the dramatic change; most of these do not even list one problem in set theory, axiomatic or otherwise. For another example, consider the volume, *Mathematics: Frontiers and Perspectives* (Arnold, et al., 2000), said in its Preface to be inspired by the famous list of Hilbert problems; its contributors are a number of leading mathematicians yet there is not one article on set theory in its contents.

And, finally, CH is not on the highly publicized Millennium Prize list of seven famous unsolved mathematical problems, even though *prima facie* it met the board's criteria to be historic, central and important. Of course, there were many more than seven such problems that it no doubt considered as candidates; the board chose not to explain why it picked the ones it did and not others (cf. Jaffe (2006)). In the case of CH, *we* could explain its omission by the fact that the prize for a solution of any one of the problems would be one million dollars, and the board would only want to choose problems whose solution could be expected to be definitive after the usual vetting procedures.

It is thus clear that CH can no longer be considered to be a problem in the ordinary sense as far as the mathematical community at large is concerned. Actually, *de facto* that ceased to be the case in 1904, but its changed status was only gradually recognized in steps especially marked at 1934, 1938 and 1963. Here is the history as traced, for example, by Moore (1989, 2011). The Continuum Hypothesis was first proposed by Cantor (as a theorem!) in 1878 in its "weak" form (WCH) that every infinite subset of the continuum is either denumerable or equivalent to the continuum. It was only in 1882 that he proposed the Well-Ordering Principle and thus arrived at the statement of CH in the form that the continuum has the same power as the set of all countable ordinals, i.e. that its cardinal number is $\aleph_1$. Genuine attempts (among others by Bernstein and König) to prove or disprove WCH or CH as a statement in informal set theory essentially came to an end by 1904. As it happens, in that year Zermelo gave his first proof that the Axiom of Choice (AC) implies the Well-Ordering Principle (WO). The first hint that one has to consider the logical status of CH came with Zermelo's introduction of his system of axioms for set theory in 1908 in order to give an airtight proof that AC implies WO. Continuing against an axiomatic background we have Sierpinski's compendium (1934) of equivalents and consequences of CH. It took Gödel (1938, 1940) to shift it more definitively to a metamathematical problem and then for Cohen (1963) to lodge it there permanently.

Mathematicians who have any idea at all of what's gone on in mathematical logic will know in a general way of Gödel's incompleteness theorems, will accept that the Zermelo-Fraenkel system ZFC of axiomatic set theory with Choice provides a foundation of current mathematics, and that the results of Gödel and Cohen show that CH is independent of ZFC. And for those mathematicians, these facts were sufficient to put the nail in the coffin (if any was still needed) of CH as a problem in the ordinary sense. Most mathematicians, I warrant, are not aware of Gödel's views dating at least from 1947 that CH is a definite mathematical problem and that one will likely need new axioms beyond those of ZFC to settle it. Nor are they aware that one possible source he saw for such lay in the assumption of the existence of very large cardinals. (This is not to say that they may not be aware of the idea of inaccessible cardinals in one form or other.[3]) And it hardly needs saying that they are not aware of the working distinction among set-theorists between "small" (or "weak") large cardinals and "large" (or "strong") large cardinals. It did not register with the mathematical community that by the work of Levy and Solovay (1967), CH is independent of every extension of ZFC by large cardinal axioms of either kind if the extension is consistent at all, the biggest nail in the coffin so far. In other words, if mainstream mathematicians had not already discounted CH as a problem in the

[3] For example, those concerned with the foundations of category theory have in effect assumed the existence of at least one inaccessible cardinal (as, e.g., by Mac Lane) or even of a class of inaccessible cardinals (as, e.g., by Grothendieck).

ordinary sense it would only need these subsequent developments for them to close the lid on that.

So what difference does all this make? Simply it is that for the mathematical community at large, the problem of CH—if it is still considered to be a problem at all—has been consigned to one in the metamathematics of set theory, and so that is where we must now turn to pursue the question of its definiteness.

3. The road to large cardinals and their logical template. The subject of set theory has made enormous progress since Scott (1961) proved that the existence of measurable cardinals implies the existence of non-constructible sets, and since Cohen (1963) introduced the method of forcing to prove the independence of the continuum hypothesis from ZFC. Some idea of that subject's current manifold concepts, methods and achievements can be garnered from the *Handbook of Set Theory* (Foreman and Kanamori, eds., 2010) that runs to almost 2200 pages over three volumes. (And, according to its Preface, that doesn't even include some significant additional developments in set theory, because they are ones that are available in independent expositions.) The informative introduction to the *Handbook* by Kanamori (with extensive historical background) itself runs almost 90 pages. Reading that, one finds soon enough that the work represented in the *Handbook* is of the highest level of mathematical difficulty and technical sophistication. And one also sees that it is almost entirely concerned with set theory as an axiomatic subject for which the methods to be applied are those of mathematical logic. It is true that various of the concepts involved may be understood in ordinary mathematical terms, but the essence of their use lies in their logical properties. In particular, that is the case for the *ultraproduct* construction, whose fundamental theorem due to Los (1955) tells us what the first-order properties are of an ultraproduct of models relative to an *ultrafilter* on the index set, in terms of the properties of the factors and the given ultrafilter. In particular, when all the factors are the same we have an *ultrapower* of a model, and Los' theorem in that case tells us what its first-properties are in terms of the properties of the given model and the given ultrafilter. As I will describe in more detail below, Scott obtained his result by proceeding from an ultrafilter supplied by a measurable cardinal to the corresponding ultrapower of the structure $(V, \in)$ and then applying Los' theorem. And, of course, the very meaning of Scott's result requires the notion of the class L of constructible sets from Gödel (1938, 1940), and that is obtained by iterating the notion of first-order definable subset of a given set through all the ordinals. My point here is to emphasize that contrary to some appearances, we are dealing here with a logical subject through and through.

Cohen's method of forcing and generic extensions of models of set theory opened the floodgates for a veritable deluge of relative consistency and independence results, to begin with his own theorem that it is consistent with ZFC that the cardinality $2^{\aleph_0}$ of the continuum can equal $\aleph_2$; in fact by the same methods, it can be made as large among the alephs as one pleases. On the other hand, since by Gödel's fundamental work of 1938 CH is true in the class L of constructible sets—with which all "small" large cardinals like inaccessibles, Mahlo cardinals, etc., are consistent—one would want to know if the assumption of the existence of large cardinals such as those that are measurable and beyond could settle CH in a definite negative way. But, as already mentioned above, that was soon excluded by the previously mentioned result of Levy and Solovay. Despite that, strong large cardinal numbers (i.e. those whose existence is inconsistent with the statement $V = L$ that all sets are constructible) gained increasing attention and have figured in essential ways in several programs aimed at tying down CH positively or negatively. Necessarily, the full explanation of those programs requires the use of rather technical concepts, and those can only be indicated here. Nevertheless that will be enough to elicit their character of and thereby assess their possible significance for the status of CH.

Independently of these programs, several cases have been made by a number of set theorists for the assumption of the existence of various kinds of very large cardinal numbers, including (in increasing strength) Woodin cardinals, supercompact cardinals, and "huge" cardinals. These are defined and studied in detail in Kanamori (1994) and charted there on p. 471 in comparison with other kinds of large cardinals. Fundamental to that work is the application of the notion of an *elementary embedding* $j: K \to M$ to subclasses K and M of the universe treated as substructures of $(V, \in)$. The elementary embedding relation holds if for any formula $\varphi(x_1,\ldots,x_n)$ of the language of set theory and any $a_1,.., a_n$ in K, we have $K \models \varphi(a_1,\ldots,a_n) \Leftrightarrow M \models \varphi(j(a_1),\ldots,j(a_n))$. We write j: $K \prec M$ in this case, and $K \prec M$ if there exists such an embedding j.

The ideas of Scott's 1961 proof are as follows. Suppose κ is a measurable cardinal and U is a κ-complete ultrafilter on κ given by a two-valued measure on it. Form V^κ/U, whose elements are equivalence classes $[f]$ (or suitable representatives thereof) of functions f: $\kappa \to V$ with respect to the equivalence relation, $f \equiv g \Leftrightarrow \{i : f(i) = g(i)\} \in U$. We obtain an embedding h of V into V^κ/U by sending each $a \in V$ into the equivalence class of the function on κ that is identically equal to a. The relation E interpreting membership on V^κ/U is given by $[f]\ E\ [g] \Leftrightarrow \{i : f(i) \in g(i)\} \in U$. Then h: $V \to V^\kappa/U$ is an elementary embedding by Los' Theorem. It turns out that E is well-founded and so V^κ/U collapses to a transitive class M via a canonical map m. Composing h with m thus yields an elementary embedding j: $V \prec M$; moreover, j is not the identity map. Scott's proof concludes by showing that the assumption $V = L$ leads to a contradiction.

Now, almost all the notions of very large cardinal numbers that have been considered to date are given in terms of non-trivial elementary embeddings j: $V \prec M$ as the basic template. It is shown for these that there is a least ordinal κ such that $j(\kappa) \neq \kappa$, called the *critical ordinal* of j. Then $\kappa < j(\kappa)$, and necessarily κ is at least as strong as a measurable cardinal. By putting successively stronger conditions on j and κ one obtains much stronger large cardinal notions, in particular those mentioned above (cf. Kanamori (1994), secs. 22-24, 26 for details). I have used Scott's proof to motivate the basic template, but that does not seem to me to be intuitive in its own right since it reverses the order of reasoning from existence of a cardinal with special properties to existence of a suitable elementary embedding. In addition, I want to emphasize once more the logical character of the concepts involved. I can't imagine this being presented to the mathematical community as constituting a mathematical notion in the ordinary sense, even if redisguised in the form of ultraproducts or related constructions.[4]

4. Why accept large cardinals? I. The consistency hierarchy. Several cases have been made for the assumption of very large cardinals going back to Gödel (1947). Excellent expositions of these are given, among others, by Steel (2000, 2014) and Koellner (2010a, 2013). To begin with, a quite minimal criterion for any suggested new axioms for set theory is that they be consistent. In the case of large cardinals, an upper bound for this criterion was given by Kunen (1971), who showed that there does not exist any non-trivial elementary embedding j: $V \prec V$, thereby demonstrating the inconsistency of a large cardinal axiom that had been suggested by Reinhardt a few years earlier. The largest strong cardinal notions that have been proposed thus far to exist via the elementary embedding template flirt with disaster by coming close to this one. Of course, by Gödel's second incompleteness theorem, no such can be proved to be consistent by its own means. Moreover, there is nothing on the face of the statement of existence of large cardinals given by elementary embeddings $V \prec M$ that leads one to accept them on intuitive informal grounds. Rather, the cases for such comes from elsewhere. The first of these lies in their observed central role in what is called the *consistency hierarchy* of "natural" axiomatic theories extending ZFC. Closely related to it is the *interpretability hierarchy* of such theories.

The basic relation $T_1 \le T_2$ for the consistency hierarchy holds between recursively axiomatized theories T_1 and T_2 when one can prove their relative consistency, Con(T_2) →

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[4] Kanamori (1994) p. 44 writes that the work leading to Keisler (1962) and, in full exposition, Keisler and Tarski (1964) set the stage for Scott's construction (even though the latter was carried out in 1961). They applied ultrapowers in set theory to recapture earlier results of Hanf showing that weakly compact cardinals (defined in terms of possible compactness properties of infinitary logics) are larger than cardinals in the Mahlo hierarchies. That reworking in turn was motivated by Tarski's constant aim to interest the mathematical community at large in logical results by turning the notions involved into *prima facie* mathematical notions in the ordinary sense. Keisler and Tarski (1964) p. 226 write: "We have been motivated by the realization of the practical fact that the knowledge of metamathematics is not sufficiently widespread and may be defective among mathematicians who otherwise would be intensely interested in the topic discussed, and to a certain extent also by some (irrational) inclination toward puritanism in methods." (The voice is clearly that of Tarski.) However, they add "[a]s will be seen…we do not feel we have been completely successful in our undertaking." Incidentally, despite Tarski's long standing interest in varieties of inaccessible cardinals, he balked at accepting the existence of weakly compact cardinals, saying that "we see at this moment no cogent intuitive reasons which could induce us to believe in the existence of [such cardinals]" or even to make plausible the statement that their existence is consistent with the axioms of set theory (Tarski (1962) p. 134).

Con(T_1), in an elementary way, i.e. in a weak fragment of Peano Arithmetic. Under suitable conditions met in practice, if T_1 is (relatively) interpretable in T_2 then $T_1 \leq T_2$; the converse holds for natural theories met in practice.[5] By the work of Lindström (1997) and others, the lattice of degrees of interpretability is very complicated; in particular, one can construct incomparable theories by Gödelian methods (cf. op. cit., Ch. 7). However, it has been observed that we have comparability of any two "natural" theories extending ZFC, that is, roughly speaking, those that have arisen in the ordinary course of work on set theory.[6] There may be some exceptions to this, however; for example, it is not known whether the existence of strongly compact cardinals is comparable to that of superstrong cardinals over ZFC (cf. Kanamori (1994) p. 471). And there is no precise definition on offer as to what constitutes a natural theory. Finally, the natural theories (except for the few open cases) have been touted as forming a well-ordered hierarchy (cf., e.g. Steel (2000), p. 427 and Koellner (2010a), sec. 4), though of course that can't be otherwise given that at any point in studying it there are only a finite number of theories that have been considered.

The first main argument for accepting the large cardinal axioms is that in a number of leading cases, theories that are based on quite independently motivated and conceptually distinct principles turn out to be equiconsistent, and that that fact is established only via a theory based on some large cardinal axiom. As an example of this phenomenon, Woodin (2011) pp. 452-453 refers to three theories, ZFC + SBH, ZF + AD, and ZFC + "There exist infinitely many Woodin cardinals," of which it has been shown that they are equiconsistent, with the third of these providing the essential link in the proof.[7] Furthermore, Woodin predicts that these theories are consistent, though "[j]ust knowing that the first two theories are equiconsistent does not justify this prediction at all." Rather,

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[5] Simple examples among "non-natural" theories can be given to show that the consistency hierarchy is not the same as the interpretability hierarchy though they agree in practice on natural theories.

[6] Friedman (2007), sec. 7, presents a linearly ordered interpretability hierarchy of natural theories that goes all the way down to EFA (Elementary Function Arithmetic). But he points out that it is not linear among natural algebraic theories, e.g. the theories of linear discrete orderings without end points and linear dense orderings without endpoints are incomparable. Also, Friedman, Rathjen and Weiermann (2013), Cor. 3.15, give examples of natural theories stronger than PA that are incomparable with respect to interpretability.

[7] SBH abbreviates the Stationary Basis Hypothesis; cf. Woodin (2011) p. 450. Shelah (1986) proved that SBH implies that CH is false. AD is the Axiom of Determinacy introduced by Mycielski and Steinhaus (1962); it is inconsistent with ZFC, but may be consistent over ZF without AC.

he claims that "[i]t is through the calibration by a large cardinal axiom in conjunction with our understanding of the hierarchy of such axioms as true axioms about the universe of sets that this prediction is justified." Because of his belief in this claim, Woodin predicts that no inconsistency in these systems will ever be discovered in the next ten thousand years, and that that is "a specific and unambiguous prediction about the physical universe," since the discovery of such an inconsistency would be a physical fact. Indeed, he goes on to predict that "[t]here will be no discovery, ever, of an inconsistency in these theories," and that one can arguably claim that "if this stronger prediction is true, then it is a physical law" (op. cit. p. 453). In my view, these statements of what can constitute physical facts and physical laws are quite idiosyncratic, and are called on tendentiously to bolster the predictions of consistency. There are infinitely many quite elementary theories of which we could say with complete confidence that they are consistent, so on the same grounds that would tell us there are infinitely many special physical laws. No system of physics in the ordinary sense comprehends any of these.

5. Why accept large cardinals? II. Mathematical consequences. The second main claim for the large cardinal axioms, and in particular those that assert the existence of infinitely many Woodin cardinals, is that by their assumption, one has been able to settle a wealth of problems in descriptive set theory (DST) that had been open since the 1930s (cf. Koellner 2013 for an exposition). The subject DST concerned properties of definable sets of real numbers. Of particular interest were the "regularity" properties, namely those of being Lebesgue measurable, having the Baire property, and having the perfect subset property (PSP). This last holds of a subset X of $\mathbb{R}$ if it is either countable or contains a non-empty perfect subset. It was long known in the latter case that then X and $\mathbb{R}$ have the same cardinality, i.e. the Weak Continuum Hypothesis holds for such sets. The regularity properties were first verified for the Borel sets of reals, which are those generated from open intervals by closing under countable unions and complements. Later, attention turned to sets in the projective hierarchy, which are generated from the Borel sets by closure under projection and complementation. This hierarchy begins with the so-called analytic sets (or A-sets), defined as the projections of Borel sets, then continues with the CA sets (= the complements of the A sets), then the PCA sets (= projections of the CA) sets, and so on.

In 1917, Suslin proved that every analytic set has the regularity properties, so the question was naturally raised whether the same holds of sets further on in the projective hierarchy. In a shift to a logical problem, Gödel (1940) showed that it is consistent with ZFC that there are PCA sets that are not Lebesgue measurable, since such sets exist in L. This left open the possibility that one could obtain extensions of the regularity properties to the projective hierarchy under the assumption of axioms inconsistent with $V = L$ (such as the existence of measurable cardinals). Indeed, that proved to be the case in a succession of steps (cf. Kanamori (1994) secs. 12 and 27 for the history) culminating in the result of Martin and Steel (1989) that every set in the projective hierarchy enjoys the regularity properties if there are infinitely many Woodin cardinals. A crucial intermediary in the argument was provided by what is called the Axiom of Determinacy (AD) that had been introduced by Mycielski and Steinhaus (1962). This concerns idealized infinite games G_X associated with each subset X of the real numbers, or—what essentially comes to the same thing—of the set 2^N of all infinite sequences of 0's and 1's. This game has two idealized players, I and II, where at each stage of play, I plays first by choosing a 0 or a 1; player II responds in the same way. The play terminates with a sequence s in 2^N, for which player I wins if $s \in X$ and player II wins if not. The game G_X is said to be determined if one or the other of the players has a winning strategy, i.e. a rule for how to play at each stage in order to win no matter what choices are made by the opposite player; finally, AD says that G_X is determined for each set X. Mycielski and Steinhaus showed that AD implies that every set X has the desired regularity properties. However, that comes at the great cost of contradicting AC, the Axiom of Choice, because that implies the existence of non-Lebesgue measurable sets. Since AC is fundamental to much of set theory (and in particular is essential for the theory of cardinal numbers) and since it is accepted as a basic principle on intuitive grounds, it was natural to ask whether certain weakenings of AD could be consistent with ZFC. Let Γ be any collection of subsets of the reals or of 2^N; by AD^Γ is meant the statement that for each X in Γ, G_X is determined. Two special cases of this have received much attention in the literature. By Projective Determinacy (PD) is meant this statement when Γ is taken to be the collection of all projective sets of reals. A strengthening of that is the statement $AD^{L(\mathbb{R})}$, where $L(\mathbb{R})$ is the collection of all sets definable in the constructible hierarchy L relativized to the set $\mathbb{R}$ of all real numbers as an initial set.

What Martin and Steel (1989) proved is that the existence of infinitely many Woodin cardinals implies PD. Woodin then proved that if there are infinitely many Woodin cardinals with a measurable cardinal above all of them then $AD^{L(\mathbb{R})}$ holds. Furthermore he showed via construction of an inner model that $AD^{L(\mathbb{R})}$ implies the consistency of the third of Woodin's three theories indicated above, namely ZFC + "there are infinitely many Woodin cardinals"); cf. Koellner (2010), p. 205. As summarized there, "[l]arge cardinals are *sufficient* to prove definable determinacy and (inner models of) large cardinals are *necessary* to prove definable determinacy." Though many of the workers in the field (especially those in what is sometimes called "the California school"[8]) regard these results as mutually reinforcing, and though they also consider all the problems of DST that were open since the 1930s settled in just the right (coherent and "effectively complete") way, not everyone among the leaders in set theory by any means goes along with that. In particular, Saharon Shelah writes vis à vis $AD^{L(\mathbb{R})}$:

> (a) Generally I do not think that the fact that a statement solves everything really nicely, even deeply, even being the best semi-axiom (if there is such a thing, which I doubt) is a sufficient reason to say it is a "true axiom". In particular I do not find it compelling at all to see it as true.
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> (b) The judgments of certain semi-axioms as best is based on the groups of problems you are interested in. For the California school, descriptive set theory problems are central. While I agree that they are important and worth investigating, for me they are not "the center". Other groups of problems suggest different semi-axioms at best; other universes may be the nicest from a different perspective.
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> (c) Even for descriptive set theory the adoption of the axioms they advocate is problematic. It makes many interesting distinctions disappear …

Shelah (2003) p. 212

[8] This mainly refers to a group of set theorists (sometimes referred to in the past as the "Cabal") at the University of California campuses of Berkeley, Los Angeles and Irvine as well as at CalTech.

Finally, though the article Martin and Steel (1989) is published in a leading mathematical journal and is entitled, "A proof of projective determinacy", it is stated quite explicitly therein that the result in question is established under the premise that there are infinitely many Woodin cardinals. While PD (and the consequent regularity properties for projective sets) certainly make ordinary mathematical sense, I don't think that the mathematical community at large would at all regard it as having been established *per se* once the issue of the nature of its hypotheses is put front and center. There can be no question that for the same community, the proposed raising of these large cardinal axioms to the status of "truths" alongside the accepted informal principles of set theory is indeed deeply problematic.

6. Is CH a definite logical problem? With all this in place as background we can return to the question of the status of CH as a logical problem. Clearly, it can be considered as a definite logical problem relative to any specific axiomatic system or model. But one cannot say that it is a definite logical problem in some *absolute* sense unless the systems or models in question have been singled out in some canonical way. Two programs that have received considerable attention claim to be based on just that aim. The first is Woodin's approach via what he calls Ω-logic; the second one is called the inner model program. Both take large cardinals for granted to a considerable extent and both have been pursued over a number of years with results of great technical complexity. My concern here is only to indicate the character of each.

For the Ω-logic program I rely mainly on the exposition Koellner (2013a), sec. 4; cf. also Woodin (2001, 2001a) and Koellner (2010). The main results assume that there is a proper *class* of Woodin cardinals. For T an axiomatic theory in the language of set theory and φ a sentence, the relation, *φ is an Ω-consequence of T* (in symbols, $T \vDash_\Omega \varphi$), is defined to hold if for any ordinal α and any forcing generic extension M of the universe V, whenever $M_\alpha \vDash T$ we have $M_\alpha \vDash \varphi$.[9] The idea is to have a notion of consequence that cannot be affected by the method of forcing and thus evades the Levy-Solovay result. Woodin also associates with this semantic notion of consequence a so-called "quasi-

[9] M_α is the set of elements of M of rank less than α.

syntactic" notion, $T \vdash_\Omega \varphi$, which is sound for it. Its "proofs" are taken to be universally Baire sets; one can assign ordinals as length to them, but in every other respect they are unlike proofs in familiar systems such as those for first-order logic or ω-logic, which are simply generated by closing axioms under rules of inference. Nevertheless, Woodin's Ω-Conjecture is the analogous statement of completeness, i.e. it asserts that whenever $T \vDash_\Omega \varphi$ then $T \vdash_\Omega \varphi$.

Using $H(\aleph_2)$ to denote the set of all sets of hereditary cardinality less than $\aleph_2$, let Σ consist of all sentences which express that $H(\aleph_2)$ satisfies a given sentence ψ. In particular, CH is equivalent to a member of Σ. Call an axiom A *good* if ZFC + A is Ω-satisfiable and Ω-complete for all sentences in Σ. The CH-Conjecture states that there is a good axiom and that ¬CH (the negation of CH) is an Ω-consequence of *any* good axiom. The main result that has been obtained thus far in this program is that the CH-Conjecture is a consequence of the Strong Ω-Conjecture, where the latter is obtained from the Ω-Conjecture by adding some rather technical conditions concerning a strengthened form AD^+ of the Axiom of Determinacy for the sets constructible from the set of all subsets of $\mathbb{R}$.[10] There is no proof yet of the Ω-Conjecture, let alone of the Strong Ω-Conjecture.

Turning now to the inner model program, I rely on Koellner (2013a), sec. 6; cf. also Woodin (2011) and, for full technical introductions, Mitchell (2010) and Steel (2010). Inner models are simply transitive submodels of V with respect to the membership relation. The least inner model containing all the ordinal numbers is L, the class of constructible sets. Small large cardinal notions (such as being inaccessible, Mahlo and weakly compact) are those consistent with $V = L$ and thus the corresponding cardinals exist in L if they exist at all. The inner model program proceeded from this to incorporate strong large cardinals into "L-like" inner models. That began with the work of Solovay in the 1960s to show that there are such models that contain a measurable

[10] For further expositions of the Ω-logic program, see Woodin (2001, 2001a) and Koellner (2010, 2013a). Note that Woodin earlier stated that the CH-conjecture is a consequence of the Ω-conjecture; he only realized later that this had to be replaced by the Strong Ω-conjecture.

cardinal (cf. Kanamori (1994), pp. 261ff). To date the results of the program have been successively strengthened to the point that one is able to incorporate a Woodin limit of Woodin cardinals in an L-like inner model. The grand aim for the inner model program is to obtain an L-like inner model that satisfies "all" large cardinal axioms, via an "effectively complete" axiom of the form $V = L[E]$ ('E' for 'extenders'). Such a model would, among other things, satisfy CH. But related axioms of the form $V = L[E]_S$ and $V = L[E]_{(*)}$ have been proposed that would also be "effectively complete" and yet which imply ¬CH, and it is not yet settled how to adjudicate between these, if one is successful in realizing them at all.

The character of these two programs is thus seen to be quintessentially that of the metamathematics of set theory. Note that neither of them starts with a definite question regarding the status of CH relative to specific axioms or models, and neither addresses it as a definite logical problem in the primary sense. The Ω-logic program does certainly consider such a problem, namely whether the Strong Ω-Conjecture follows from ZFC together with the assumption that there is a proper class of Woodin cardinals. If it does, then we have the CH-Conjecture on the same basis. But note that that is not a conclusion about CH itself but rather about its status relative to the Ω-consequence relation. On the other hand, if the Strong Ω-Conjecture does not hold under the given assumptions, there is no definite problem concerning CH at all at hand in this approach. Perhaps one will never know which of these two alternatives holds, but if it turns out to be the second one, that would not exclude pursuit of related questions in which the status of CH reappears in a secondary way.

As for the inner model program, there is no definite problem being pursued because the notion of being "L-like" is not precise, though it appears that those who pursue the program have highly sophisticated technical criteria for what would constitute success. But again, the status of CH would be ancillary to that goal and, as we have seen, could go either way depending on how such criteria are construed in the end.

Thus, for both programs, there is no work to date that establishes CH as a definite logical problem, and the prospects for that to change are rather iffy.

7. The "duck" problem. We saw earlier that for all intents and purposes, CH has ceased to be a definite mathematical problem in the ordinary sense. It is understandable that there might be considerable resistance to accepting this, since the general concepts of set and function involved in the statement of CH have in the last hundred years become an accepted part of mathematical practice and have contributed substantially to the further development of mathematics in the ordinary sense. How can something that appears so definite on the face of it not be? In more colloquial terms, how can something that walks like a duck, quacks like a duck and swims like a duck not be a duck?

Of course there are those like Gödel and a few others for whom there is no "duck" problem; on their view, CH is definite and we only have to search for new ways to settle it, perhaps by other means than large cardinal axioms. For example, one idea is that we need to expand set theory by new concepts and new principles concerning them, in some way analogous to what was done by going outside number theory into analysis or algebra in order to settle classical number-theoretical problems. Some might suggest looking to category theory for such an expansion. However, there are many established ways that have been employed to reduce category theory to set theory, even with the inclusion of such "unlimited" objects as the category of all categories, the category of all functors between any two categories, and so on (cf., e.g., Feferman (2013)). Another suggestion that has been made is to add probabilistic concepts to set theory, as has been done implicitly by Freiling (1986) and that has been convincing to some non-set-theoretical mathematicians.[11] For example Mumford (2000) p. 208 writes that "Freiling used the argument [about throwing darts at the continuum] to motivate a new axiom of set theory which disproves the continuum hypothesis. I believe we should go much further. His 'proof' shows that if we make random variables one of the basic elements of mathematics, it follows that CH is false, and we will get rid of one of the meaningless conundrums of set theory. The continuum hypothesis is surely similar to the scholastic issue of how many angels can stand on the head of a pin: an issue which disappears if you change your

[11] All set theorists whom I have asked about this are dismissive of Freiling's argument.

point of view."[12] I don't know of any specific proposals for such an expansion, and one may be skeptical of such since all currently accepted notions and principles of probability theory have been modeled set-theoretically via measure theory. Nevertheless, the history of mathematics shows that one should not exclude the possibility of genuine, robust conceptual expansion in other ways in the future. On the other hand, this would not be accepted by those who hold to the view that set theory is a universal language for mathematics.[13]

Coming back to Gödel, as I said, there was no "duck" problem for him, since as a Platonic realist about the content of set theory (if not more), there is no question that CH is a definite mathematical problem. And while we are very familiar with his proposals (1947, 1964) to seek its solution via the assumption of new axioms for set theory, in particular strong large cardinal axioms,[14] it is possible he thought, as in the following passage, that one might need to somehow go beyond set theory, though what he says is rather vague:

> [P]robably there exist other [axioms] based on hitherto unknown principles...which a more profound understanding of the concepts underlying logic and mathematics would enable us to recognize as implied by these concepts. (Gödel (1947) p. 520, reprinted in Gödel (1990) p. 182).

For further discussion of Gödel's views, see the very informative introductory note Moore (1990) to the 1947 paper and its 1964 revision.

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[12] In the same passage Mumford writes that Freiling's "stunning result" should be considered on a par with those of Gödel and Cohen.

[13] One argument for the supposed universality of the language of set theory is made on the empirical grounds that as a matter of fact, all definite mathematical notions whether classical or constructive in one way or another have been reduced directly or indirectly to set-theoretical notions. However, this won't work if the notions of set theory themselves, including the cumulative hierarchy V, are regarded as completely definite, since by Tarski's theorem, the notion of truth for the structure $(V, \in)$ is not set-theoretically definable.

[14] There is no evidence that Gödel was aware of the *prima-facie* problem raised by the result of Levy and Solovay (1967) for his proposed use of strong large cardinal axioms to settle CH, at least in the straightforward sense; his view of that would have been quite interesting. We do know that in 1970 Gödel claimed to use axioms of a quite different nature—namely for "scales" of functions—to derive $2^{\aleph_0} = \aleph_2$, and that when his proof was found faulty, modified them to axioms for such that would imply CH; cf. the informative introductory note Solovay (1995) to this problematic unpublished work.

In recent years there has been a retreat from—or sidestepping of—Platonism among those writing in a philosophical vein about the justification for strong large cardinal axioms, for example Martin (1998), Parsons (2008), Maddy (2011) and Koellner (2013). While they each take the language of set theory meaningful on the face of it, it is more difficult to say how CH would be regarded as a definite problem for them, whether mathematical or logical. In any case, their views require an extended discussion that I hope to address on another occasion.

In contrast to all these, I have long held that CH in its ordinary reading is essentially indefinite (or "inherently vague") because the concepts of arbitrary set and function needed for its formulation can't be sharpened without violating what those concepts are supposed to be about. Nevertheless, I have a non-platonistic account in what I call *conceptual structuralism* for why mathematics is what it is, and why things that appear definite for mathematicians may not be so. I have spelled this out most recently in Feferman (2009, 2014). Briefly, mathematics at bottom deals with ideal world pictures given by conceptions of structures of a rather basic kind. Even though features of those conceptions may not be clearly definite, one can act confidently and go quite far as if they are; the slogan is that *a little bit goes a long way*. Nevertheless, if a concept such as the totality of arbitrary subsets of any given infinite set is essentially indefinite, we may expect that there will be problems in whose formulation that concept is central and that are absolutely unsolvable. We may not be able to prove that CH is one such (however, see the Appendix, next, for steps in that direction), but all the evidence points to it as a prime candidate.

Appendix. A proposed logical framework for what's definite (and what's not) according to which CH is provably indefinite.

There are two informal notions of definiteness for which criteria can be given in logical terms; the first of these is the notion of being a definite totality, and the second is the notion of being a definite proposition or property. The first criterion is that a totality is definite if and only if quantification over that totality is a definite logical operation. The second criterion is that a proposition or property is definite if and only the Principle of Bivalence holds for it. These two interact in the following way: internally, quantified variables in definite formulas are restricted to range over definite totalities, and externally, classical logic applies only to definite formulas. In Feferman (2010) I introduced a logical framework in which these two criteria for different philosophical viewpoints as to which totalities are definite and which are not can be represented and investigated by proof-theoretic methods.

For example, according to the finitists, the natural numbers form an "unfinished" or indefinite totality, and quantification over the natural numbers is indefinite, while bounded quantification is definite. According to the predicativists, the natural numbers form a definite totality, but not the supposed collection of arbitrary sets of natural numbers. In the case of set theory, definite totalities in general are identified with sets. Then the universe of all sets itself forms an indefinite totality, for otherwise it would be a set and thus give rise to Russell's Paradox. The question then is, which sets exist? If the set of natural numbers is presumed to exist but not the power set operation on sets then this viewpoint leads to a basic form of predicativity in set theory.[15] Finally, for the original workers in "classical" descriptive set theory, the collection $\mathbb{R}$ of real numbers is also taken to be a definite totality, i.e. the power set of the natural numbers is taken to exist, but not the totality of subsets of $\mathbb{R}$.[16]

As indicated above, the first step in formulating axiomatic systems corresponding to such viewpoints is to restrict quantifiers in the formation of definite properties to the domains that are regarded as definite, namely finite initial segments in the case of finitism, the natural numbers in the case of predicativism, the reals in the case of the descriptive set theory, and sets themselves in the case of predicative set theory. However, quantification over indefinite domains may still be regarded as meaningful, since, for example, closure conditions such as that the union of any set of sets is again a set requires unlimited quantification. The second step is to take intuitionistic logic as the basic system in which

[15] There has been much interesting philosophical discussion in recent years about the problems of absolute generality (cf. Rayo and Uzquiano 2006) both in its unrestricted sense and more specifically as applied to set theory.

[16] This viewpoint is often referred to as "semi-intuitionism", but that should be avoided, because historically the logic employed by this group of workers was entirely classical. Yiannis Moschovakis has suggested to me that they did not explicitly reject assumption of the set of all subsets of the reals; cf. also the last paragraph of Moschovakis (2010).

to reason with formulas in general, though augmented by classical logic for the definite formulas. The resulting systems are thus *semi-intuitionistic*. 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 predicative or descriptive set theory, where only each individual set is conceived to be a definite totality but not the universe of all sets, we would have classical logic for formulas that only allow bounded quantification while intuitionistic logic for formulas in which there is no such restriction.[17]

The general pattern of the studies in Feferman (2010) is that one starts with a system S formulated in fully classical logic but in which certain basic principles may be restricted according to which totalities are definite, and then make the corresponding restriction to an associated semi-intuitionistic system SIS. One then asks whether there is any essential loss in proof-theoretical strength when passing from S to SIS. In the cases that are studied in my paper it turns out that there is no such loss, and moreover, there can be an advantage in going to such an SIS; namely, we can beef it up to a *semi-constructive* system SCS without changing the proof-theoretical strength, by the adjunction of certain principles that usefully go beyond what is admitted in SIS because they happened to be verified in a certain kind of functional interpretation of the intuitionistic logic.

Here I want to describe only the results relevant to questions of definiteness in descriptive set theory. Let S = KP be the basic classical system of predicative set theory without the power set axiom, namely the system of admissible set theory with the Axiom of Infinity. Then the associated system SIS has the same axioms as S but is based on intuitionistic logic expanded by the law of excluded middle for all bounded (i.e. Δ_0) formulas, in other words, S = IKP + (Δ_0-LEM). Now the beefed up system SCS is chosen to further include the Full Axiom of Choice Scheme for sets,

(AC_{Set}) $\quad \forall x \in a\, \exists y\, \varphi(x,y) \rightarrow \exists r\, [Fun(r) \wedge \forall x \in a\, \varphi(x, r(x))]$,

[17] The formal study of such systems for set theory goes back at least to Wolf (1974) and Friedman (1980).

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

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

for φ an arbitrary formula, while that holds only for $\sum_1$ formulas in SIS. In addition, SCS contains some other non-intuitionistic principles that can, nevertheless, be given a semi-constructive interpretation.[18]

The main result in Feferman (2010) is that all of these systems are of the same proof-theoretical strength, namely that S ≡ SIS ≡ SCS. Moreover, the same result holds when we add the axiom Pow(ω) asserting the existence of the power set of ω, in accordance with the philosophical viewpoint of classical descriptive set theory, as well as when we add the full power set axiom in accordance with standard set theory. By the way, I conjecture that all of classical DST can already be carried out in the semi-constructive system T = SCS + Pow(ω) in a straightforward way.[19]

One of the basic considerations leading to these semi-constructive systems in general is that a statement φ is recognized to be *definite* relative to the system just in case LEM holds for it, in other words just in case the system proves $\varphi \vee \neg\varphi$, and otherwise it is *indefinite*. Similarly, a formula $\varphi(x)$ is recognized to be definite relative to the system just in case it proves $\forall x[\varphi(x) \vee \neg\varphi(x)]$. In the case of SCS for S = KP, I conjecture that every such formula is equivalent to a Δ_1 formula and hence is absolute for end-extensions. Moreover, in the case of SCS + Pow(ω), i.e. the system T, the corresponding conjecture is that every such formula is Δ_1 in the power set of ω.[20]

[18] These are suitable forms of Markov's Principle, the Bounded Omniscience Scheme, and the Independence of Premises scheme. By the way, Rathjen (2014) points out that IKP + AC_{Set} proves Δ_0-LEM by the familiar argument due to Diaconescu.
[19] Moschovakis (2010) introduced a different intuitionistic framework as a candidate for formalizing some basic results of classical DST.
[20] Michael Rathjen has informed me that it can be shown by the adaptation of the methods from his paper (2012) that this conjecture holds for sentences provably definite in the system obtained from T by replacing AC_{Set} by Full Collection. The conjecture is unsettled for the stronger system.

We can now finally turn to the question whether CH is definite or not relative to T, where it is clearly meaningful; in other words, is CH $\vee$ ¬CH provable there?[21] I raised that question in the lecture Feferman (2011) of which this paper is a revision, and conjectured there that it is not. Recently, Rathjen (2014) established my conjecture using a quite novel combination of realizability and forcing techniques.[22] Subsequently he informed me that his methods show that if φ is any analytic statement consistent with ZFC then CH is indefinite relative to T + φ. Hence this strengthening applies to all the results of classical DST as well as a number from modern DST such as Borel Determinacy, and even the scheme of Projective Determinacy if consistent with ZFC. Note that this is independent of the fact that—as Friedman (1971) showed in the standard context of axiomatic set theory—any proof of BD (including that eventually given by Martin (1975)) requires at least ω_1 iterates of the power set axiom. Similarly, as we have seen PD requires the existence of iteration through infinitely many Woodin cardinals. T by comparison with these only contains Pow(ω).

No doubt Rathjen's methods can also be applied to show the indefiniteness relative to the same systems of other set theoretical statements of interest at the same level of logical complexity as CH. I do not claim that his work *proves* that CH is an indefinite mathematical problem, but simply take it as further evidence in support of that. Moreover, it shows that we can say some definite things about the concepts of definiteness and indefiniteness when relativized to suitable systems; these deserve further pursuit in their own right.

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[21] Of course, CH is definite relative to SCS + Pow(Pow(ω)).

[22] There are a number of forms of realizability interpretation that have been applied to various systems of set theory based on intuitionistic logic, and that are used to obtain disjunction properties. But difficulties arise as soon as one adds LEM for bounded formulas.

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