What is Naturalism in Mathematics, Really?

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Penelope Maddy's *Naturalism in Mathematics* is perhaps the most definitive contemporary statement on its topic, lucidly written and technically well-informed. The book's central theme, as Maddy states it on p. 28, is how a 'final consensus' might be 'rationally achieved' on 'new axiom candidates', or, as she also puts it, 'experimental hypotheses', for set theory. Her position might best be characterized as qualifiedly Quinean. She lays stress on pragmatic factors in theory choice and on the testing of hypotheses by their consequences.

The book is divided into three parts: The Problem; Realism; and Naturalism. It is the inadequacies she uncovers in her earlier Realism[1] that prompt Maddy's move here to her version of Naturalism. In §1 below I shall take each chapter in turn, summarizing what it covers. §1 is intended to be a fair exposition of the project in its own terms, with only an occasional critical comment. (Readers familiar with the book could therefore proceed directly to §2.) In §2 my task is a more critical one, in both senses of the word. I shall offer some opposing thoughts on some of the most important points on which one might take issue with Maddy, both as an interpreter of the historical record and as a writer dealing with contemporary philosophical problems. I take up eight issues, not necessarily in the order in which they first arise in the text: restrictiveness; naturalism; extrinsic ver-

*Review essay for *Philosophia Mathematica* on Penelope Maddy's *Naturalism in Mathematics*, Clarendon Press, Oxford, 1997. I am grateful to Maddy for clarifying certain aspects of her position in personal correspondence, and saving me from some unproductive misunderstandings. I also owe thanks to Julian Cole, Robert Kraut, Stewart Shapiro and an anonymous referee for helpful comments on an earlier draft.

[1]P. Maddy, *Realism in Mathematics*, Oxford University Press, 1990.

sus intrinsic justifications; bivalence; unification; indispensability; Zermelo's 'pragmatism'; and the evolution of the notion of mathematical function.

1. Exposition.

Chapter I.1, 'The Origins of Set Theory', argues that modern set theory resulted from a confluence of Frege's philosophical investigations and Cantor's mathematical ones. Maddy discusses Frege's ill-fated Axiom V (of Unlimited Comprehension), Russell's paradox, the Vicious Circle Principle and the theory of types (both ramified and simple). She endorses Quine's criticism of the Axiom of Reducibility as 'self-effacing: if it is true, the ramification it is meant to cope with was pointless to begin with.'[2]

Maddy explains how Cantor happened upon his notions of ordinal and cardinal numbers while trying to treat of 'derived sets' of real numbers. She describes how the subsequent attempt to provide for sets as mathematical objects, carried out mainly by Zermelo, led to the conception of the cumulative hierarchy of sets.

Chapter I.2, 'Set Theory as a Foundation', rehearses how one can definitionally reduce different branches of classical mathematics to set theory.

> The force of set theoretic foundations is to bring (surrogates for) all mathematical objects and (instantiations of) all mathematical structures into one arena—the universe of sets—which allows the relations and interactions between them to be clearly displayed and investigated. (p. 26)

Maddy stresses, correctly, that set theoretic foundationalism lays no claim to giving the essence or intrinsic nature of the individual mathematical objects (the 'positions within a structure') for which set theory provides surrogates—precisely because there are so many different set theoretic *structures* ensuring surrogacy (the famous Benacerraf point).

Maddy discusses Quine's 'ontological reduction' briefly, but without making it clear whether she endorses the Quinean view. She rightly asks 'if neither metaphysical insight nor ontological economy is forthcoming [from set theoretic reduction or surrogacy of classical mathematics], what is gained by the exercise?' (p. 26) While implicitly conceding the antecedent, she answers her own question in a positive spirit. The answer, she says,

> lies in mathematical rather than philosophical benefits ... Mathematics is profoundly unified by this [set theoretically reductive]

[2] Quine, as quoted on p. 12.

> approach; the interconnections between its branches are highlighted; classical theorems are traced to a single source; effective methods can be transferred from one branch to another; the full power of the most basic set theoretic principles can be brought to play on heretofore unsolvable problems; new conjectures can be evaluated for feasibility of proof; and ever stronger axiomatic systems hold the promise of ever more fruitful consequences. As the desired mathematical payoffs can be achieved by this modest version of set theoretic foundations, I will assume no more than this in what follows. (pp. 26–8)

The way that mathematics is 'profoundly unified' by taking set theory as its foundation is impressive despite two caveats to which Maddy draws attention. First, as we know from the work of Gödel—and as Zermelo did not realize in 1908—one could never prove, within one's chosen foundational theory, that that very theory was consistent. Secondly, although set theory is 'just one theory' with (perforce) its own methods of proof, one ought nevertheless to be mindful of the different casts of thought of combinatorialists, analysts, algebraists and others, whose variety of approaches and resources of conceptualization might be unduly restricted were one to insist on some uniformity of proof-methods in addition to the *ontological* unification afforded by set theory itself.

Chapter I.3, 'The Standard Axioms', discusses the justificatory status of the various axioms of ZFC. Maddy deals with a loose distinction between 'extrinsic' and 'intrinsic' justifications. A justification of an axiom is extrinsic when it adduces for consideration what follows from the axiom, or adverts to pragmatic considerations of simplicity, economy and so on. By contrast, a justification is intrinsic when it appeals to our intuitions, and/or grasp of the concepts involved, without an eye to the axiom's consequences. The distinction between these two types of justification will be employed in her case for naturalism. The leading idea is that the choice of new axioms for set theory might admit only of extrinsic justifications.

There is an interesting but brief discussion of the *limitation of size* doctrine (pp. 43–9), which ends with the widely accepted conclusion that it is impossible to restrict Unlimited Comprehension in a principled way without adverting to 'measures of size' that themselves make sense only against the background of what sets happen to exist. Maddy maintains that the limitation of size doctrine is itself a consequence of the iterative conception of set. It follows that any justification resting either directly on the iterative conception, or appealing to limitation of size, will count as intrinsic. Maddy discusses each axiom of ZFC in turn, giving both intrinsic and extrinsic justifications.

Chapter I.4, 'Independent Questions', gives a nice history of the Continuum Hypothesis, and an explanation of the broad features of Gödel's proof of its consistency relative to ZFC. By contrast, Maddy's explanation of the central idea behind Cohen's proof of the relative consistency of ¬CH is very terse (p. 66). She then moves on to a very informative discussion of Borel sets, projective sets, Lebesgue measurability, and important mathematical conjectures involving these notions that are independent of ZFC.

Chapter I.5, 'New Axiom Candidates', begins with a brief examination of the Axiom of Constructibility before moving on to large cardinal axioms and the axiom of determinacy. The reader is treated to a clear exposition of inaccessibles, hyperinaccessibles and Mahlo cardinals (the so-called 'small' large cardinals). The discussion then moves to measurable cardinals (which contradict $V = L$) and supercompact cardinals. The aspirations of large cardinal theorizing are capped off by inconsistency, as shown by Kunen, when one postulates the existence of a cardinal that is the critical point for a non-trivial elementary embedding of V into V. Just short of that, certain 'extremely large, large cardinal axioms have been formulated by examining Kunen's proof and positing cardinals that seem as large as possible without allowing the argument to go through.' But alas, 'soon after the invention of forcing, the hopes that large cardinal axioms would settle the full CH were dashed by Levy and Solovay.'[3]

What, then, about determinacy? Full determinacy contradicts the axiom of choice, but the more limited axiom of Projective Determinacy (PD) leads to 'an elegant and nearly-complete theory of the properties and behavior of projective sets of reals.' (p. 80) But 'the trouble is that PD, in itself, seems too specialized, too opaque, to serve as a basic axiom for set theory.' Maddy quotes Martin and Steel:

> Because of the richness and coherence of its consequences, one would like to derive PD itself from more fundamental principles concerning sets in general, principles whose justification is more direct.

The 'culminating theorem' of Martin and Steel is that the existence of a supercompact cardinal (SC) implies PD. Hence PD cannot settle CH, since

[3]The paper in question here is A. Levy and R. Solovay, 'Measurable cardinals and the continuum hypothesis', *Israel Journal of Mathematics* 5, 1967, pp. 234–48.

SC, by the result of Levy and Solovay, cannot. So CH remains elusive and intractable, one whole century after Hilbert pronounced it the Most Wanted fugitive from mathematical law.

Chapter I.6, '$V = L$', briefly surveys some of the things Gödel wrote in arguing *against* accepting the so-called Axiom of Constructibility. It implies CH, and Gödel thought CH was false. $V = L$ is a 'minimizing' principle—'restrictive, limiting, minimal, and ... these things are antithetical to the general notion of set.' (Thus Maddy at p. 84, in summarizing a sampling of set theorists' views on the matter. There is near unanimity, then, on an *in*trinsic argument.) But 'there are also more clearly extrinsic arguments against $V = L$ ... its numerous detractors clearly hold that its merits are far outweighed by its demerits.' (pp. 84–5) The rest of Maddy's book is devoted to resolving in the negative the 'legitimate mathematical question' whether the universe of sets is constructible, 'on the grounds that $V = L$ is restrictive, in some sense or other.' And, although she does not note this, Maddy's explication of these grounds would count as extrinsic by her own lights.

Chapter II.1, 'Gödelian Realism', takes as its point of departure the problem of assigning a truth-value to CH, rather than to $V = L$. Maddy shows how Gödel saw an analogy between the choice of scientific hypotheses (to explain our perceptual experiences) and our choice of mathematical axioms (to explain our mathematical intuitions).

Chapter II.2, 'Quinean Realism', aims to find some justification for assuming a mind-independent world of mathematical objects. Quine's pragmatism sees only the constructible sets as *needed* for physical theorizing. Unlike Carnap, Quine does not see the existence of abstracta such as sets and numbers as a 'pseudo-question', or a merely pragmatic policy-question as to whether one should adopt one linguistic framework rather than another. Rather, commitment to any kind of entity—be it physical or mathematical—is incurred, for Quine, by the existential quantifications made within one's overall theory of the world. (This, indeed, is the true font of Quinean naturalism—see §2.2 below.) The choice of theory is constrained only by our *empirical* data (that is, our observational reports about observable objects), and by pragmatic desiderata such as simplicity, economy and so on. On Quine's view, mathematics is a fully-fledged part of science, inextricably bound up with physical hypotheses, and not merely part of the linguistic framework within which scientific questions about what 'really' exists might be raised and answered. Quine is willing to apply Occam's Razor just as readily to the abstract realm as to the physical—even if this is contrary, in the case of set theory, to the desire to *maximize* that flows from the iterative conception. Maddy is alive to this point, but only much later in her exposition (p. 131):

> crudely, the scientist posits only those entities without which she cannot account for our observations, while the set theorist posits as many entities as she can, short of inconsistency. ... Quine counsels us to economize, like good natural scientists, and thus to prefer $V = L$, while actual set theorists reject $V = L$ for its miserliness.

Thus the ontologically wary pragmatist believes $V = L$, even if his set theoretic colleagues have found intrinsic, conceptual reasons for thinking that there is more to V than is ever dreamed of in L.

Chapter II.3, 'Set Theoretic Realism', amounts to a one-page statement of Maddy's strategy in her earlier book *Realism in Mathematics.* (p. 108):[4]

> The compromise goes like this. Take the indispensability arguments to provide good reasons to suppose that some mathematical things (e.g. the continuum) exist. Admit, however, that the history of the subject shows the best methods for pursuing the truth about these things are mathematical ones, not those of physical science.

Chapter II.4, 'A Realist's Case against $V = L$', seeks to develop the case without appealing to the existence of measurable cardinals. Maddy clearly wishes to keep open the option of giving an extrinsic argument later on for the existence of measurables, by appealing to Scott's result that the existence of a measurable cardinal contradicts $V = L$. She gives an interesting account of the principle of Mechanism in the history of physics, drawing on Einstein and Infeld's 1938 monograph *The Evolution of Physics.* We see how the Field Conception came to displace Mechanism, as anomalies cropped up for the latter and explanatory successes accrued to the former. Maddy's suggestion is that, analogously, mathematicians progressed from a principle of Definabilism to a 'much broader understanding of the nature of

[4] The 'compromise' Maddy is referring to here is between the Gödelian and Quinean versions of set-theoretic realism, not a compromise struck by the naturalist co-opting certain features of such realism. I am grateful to Maddy for clarifying her meaning in context here, and mention this lest any other reader fall prey to that misunderstanding. Maddy subsequently finds the compromise inadequte, in that the indispensability considerations, she argues, do not vouchsafe the ontological conclusions sought by the set-theoretic realist.

functions' (p. 122). It is an underdeveloped analogy at best; Maddy demonstrates little organic connection, or even explanatory co-operation, between the two areas. The needs of physical theorizing impinged on the development of the notion of mathematical function on two occasions, but with cancelling effects: in the mathematical treatment of a vibrating string; and in the solution of Fourier's heat equation. For the former case, Euler liberalized the then-dominant Definabilist conception; but his 'new' functions were to be subsumed under the Fourier expansions introduced for the second case, thereby reviving Definabilism with a broader conception of what definitions would be available. Further developments away from the Definabilist conception, however, were prompted by mathematical, not physical, considerations in the foundations of the calculus.

Maddy provides a detailed survey of these shifts in mathematical attitude towards the nature of functions, beginning with Descartes and Fermat, and moving on to Bernouilli *père*, Leibniz, Euler, D'Alembert, Bernouilli *fils*, Fourier, Dirichlet, Riemann, Darboux, Du Bois-Reymond, Baire, Borel, Lebesgue, Zermelo, Hadamard, Poincaré, and Bernays. The fully liberalized conception of a function as an arbitrary single-valued correspondence, whether or not it is definable, led to mathematical advances that would not be sacrificed once they had been accomplished. Zermelo's Axiom of Choice in 1904 postulated the existence of functions for which one need not be able to specify any rule. In the ensuing dispute in 1905 over Zermelo's axiom, Lebesgue posed the crucial question: *Can we prove the existence of a mathematical object without defining it?*

Lebesgue answered this question negatively, and both Zermelo and Hadamard opposed him. Hadamard appealed to the lesson of history: 'the essential progress in mathematics has resulted from successively annexing notions which ... were 'outside mathematics' because it was impossible to describe them.' This challenge to Definabilism culminated in the Combinatorialism of Bernays. In a lecture in 1934 Bernays explained Combinatorialism as exploiting 'an analogy of the infinite to the finite':[5]

> There are [n^n] functions [which assign to each member of the finite series 1, 2, ... , n a number of the same series], and each of them is obtained by n independent determinations. Passing to

[5]P. Bernays, 'On platonism in mathematics', in P. Benacerraf and H. Putnam, eds, *Philosophy of Mathematics*, 2nd edn., Cambridge University Press, 1983, pp. 258–71; at pp. 259–60. Maddy's text had *nn* in place of n^n, and I incorporate her correction from personal correspondence.

> the infinite case, we imagine functions engendered by an infinity of independent determinations which assign to each integer an integer, and we reason about the totality of these functions. ... Sequences of real numbers and sets of real numbers are envisaged in an analogous manner.

Maddy summarizes thus (pp. 127–9):

> So, according to Combinatorialism, there is one function from reals to reals for every way of making $2^{\aleph_0}$ independent assignments of a real to a real. These assignments are taken to exist, on analogy with the permutations [*sic*][6] of 1, 2, ... , n, regardless of whether or not we have a rule to determine them. ... Thus Combinatorialism deposed Definabilism, much as the Field Conception replaced Mechanism. ... Under these circumstances, the deep and widespread resistance to adding $V = L$ as a new axiom seems perfectly rational.

Chapter II.5, 'Hints of Trouble', is a short segué to a consideration of the role of continuum mathematics in natural science.

Chapter II.6, 'Indispensability and Scientific Practice', challenges the Quinean indispensability argument for a mathematically realist view at least towards those structures or entities involved in the mathematical theorizing that is applied by natural scientists. By means of the interesting case history of the gradual, and finally decisive, acceptance by the scientific community of the reality of atoms, Maddy shows that existential commitments have to pass particularly demanding muster. The early explanatory successes of Dalton's atomic theory were not enough. The Laws of Definite and of Multiple Proportions, Gay-Lussac's Law of Combining Volumes, Boyle's Law and Charles's Law were all explained by atomic theory. So were the phenomena of isomerism, and Dumas's Law of Substitution. When Cannizzaro distinguished between molecule and atom in 1858, and Avogadro's hypothesis was confirmed in independent ways, the atomic theory's explanation of all these things was still not enough to convince the scientific community at large of the reality of atoms. Something more was needed—something by way of 'independent' confirmation of their existence. This came from the phenomenon of Brownian motion, where 'directly' observable movements of tiny particles were 'directly' explained in terms of molecular collisions, and

[6] Maddy overlooks the fact that Bernays's n^n functions from $\{1, \ldots, n\}$ to $\{1, \ldots, n\}$ will not all be one-one. Permutations, by definition, are one-one.

led to another determination of Avogadro's number, in decisive agreement with the results of other methods. Maddy concludes (p. 143) 'Perhaps the notion that all existence claims are on the same footing—the 'univocality of "there is" ' as Quine calls it—is inaccurate as a reflection of the function of scientific language.' The challenge is to explain how certain experiments license a switch from a fictional use of 'there are atoms' to a literal use.

> [T]he case of atoms makes it clear that the indispensable appearance of an entity in our best scientific theory is not generally enough to convince scientists that it is real. If we still hope to draw conclusions about the existence of mathematical things from the application of mathematics in science, we must be more attentive to the details of how mathematics appears in science and how it functions there.

So the question will be: does science make ineliminable and non-idealizing use of the mathematical continuum in theorizing about natural phenomena? Maddy notes that there are at least two kinds of idealization in science—idealization for causal isolation (frictionless planes, and so on), and idealization for simplification (continuous ideal fluids and so on). Idealization prevents one from drawing ontological conclusions from the applied mathematical context in question. (p. 146) Maddy also draws attention to the opinion of Feynman, who confessed bluntly:[7] 'I rather suspect that the simple ideas of geometry, extended down into infinitely small space, are wrong.' Feynman also had misgivings over whether the fine structure of time is continuous. Maddy's cautious and 'sorry' conclusion (pp. 153–4) is that

> a space-time continuum is not something we can take as established. ... given the state of current natural science, a responsible indispensability argument ... seems unlikely to support the existence of more than a few (if any) mathematical entities [fn] and these few cannot be expected to guarantee determinate truth-values to the independent questions of set theory.

Chapter III.1, 'Wittgensteinian Anti-Philosophy', is a chapter that Maddy encourages 'philosophers ... with a technical bent, [who] tend to be unsympathetic to the style and content of the late [*sic*] Wittgenstein ... to skip over.' (p. 161) The aspect of the later Wittgenstein's philosophy that is

[7] *The Character of Physical Law*, MIT Press, 1967, at p. 166.

most relevant to Maddy's concerns (not that she endorses it—see pp. 169–70) is his hostility to the conception that pure mathematicians tend to have of the kind of mathematics they pursue—the kind that might, for all they know, be forever devoid of any application. Wittgenstein insists[8] 'It is the use outside mathematics, and so the *meaning* of the signs, that makes the sign-games into mathematics.' He offers no argument as to why the signs used in pure mathematics should be beholden to applications in natural science in order to be *meaningful*. As Maddy observes, 'This Wittgensteinian line of thought has damaging consequences for set theory.' (p. 168) Indeed it does; the Wittgensteinian line of thought here is unjustified and irresponsible. It transgresses Wittgenstein's own stricture to 'leave mathematics as it is'. No pure mathematician would tolerate having his or her discourse so easily divested of meaning, simply for want of an application in natural science.

The lesson or 'clue' that Maddy, curiously, detects from her excursus into the later Wittgenstein is that we should '[excise] all traditional philosophy, and in its place, [pay] careful attention to the details of the practice [of higher mathematics] itself.' Would that Wittgenstein himself had done so. In this crucial respect, his naturalism is the true ancestor of Quine's—see §2.2 below.

Turning her attention in Chapter III.2 to 'A Second Gödelian Theme', Maddy provides a reading of two of Gödel's important discussions (of Russell's vicious circle principle, and of Cantor's continuum hypothesis) according to which Gödel is moved by entirely *mathematical* considerations to draw the philosophical morals he does. Gödel always stresses the importance of being able to provide a foundation for all of classical mathematics. Foundational systems that cannot do this are ruled out. Thus Maddy draws the conclusion (pp. 174–6) that

> ...the support for the assumption of the existence of sets lies ... in the requirements of ordinary mathematical practice ... On this reading of Gödel, his argument for the legitimacy of [the question of the truth or falsity of] CH actually bypasses his philosophical realism altogether; its working parts are all drawn from mathematics itself. If a Wittgensteinian anti-philosopher were to 'treat' Gödel's discussion by excising the 'misguided' philosophy, nothing essential would be lost; the residue would be

[8] *Remarks on the Foundations of Mathematics*, revised edition, ed. G. H. von Wright, R. Rhees, and G. E. M. Anscombe, MIT Press, 1978; at V, §2.

> an inventory of ordinary mathematical considerations, just the sort of thing the anti-philosopher recommends to our attention.

In Chapter III.3, 'Quinean Naturalism', Maddy draws her characterization of naturalism from the writings of Quine. Naturalism, according to Quine, is 'the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described.'[9] Moreover, naturalism

> sees natural science as an inquiry into reality, fallible and corrigible but not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method.[10]

Maddy herself is aware of the inadequacies of Quinean naturalism as far as mathematics is concerned. In Chapter III.4, 'Mathematical Naturalism', she tries to effect a synthesis of Quine's position with 'the insights we've gained from Wittgenstein and Gödel' (p 182). Maddy's leading proposal is that we should adopt

> a mathematical naturalism that extends the same respect to mathematical practice that the Quinean naturalist extends to scientific practice. ... mathematics is not answerable to any extra-mathematical tribunal and not in need of any justification beyond proof and the axiomatic method.

(We shall return to this definitional template in §2.2 below.)

Chapter III.5, 'The Problem Revisited', introduces the two maxims that Maddy sees as driving set theorical endeavours. Maddy calls them UNIFY and MAXIMIZE. The first is expressed as follows: '[I]f your aim is to provide a single system in which all objects and structures of mathematics can be modelled or instantiated, then you must aim for a *single*, fundamental *theory* of sets.' (pp. 208–9; my emphases.) The maxim UNIFY, then, requires one to settle every set theoretical statement one way or the other: that is, *aim for one complete theory.* Although its inspiration is *prima facie* ontological, the maxim's express goal is merely theoretical completeness—a condition that we know, by Gödel's first incompleteness theorem, will forever elude us, providing we obey the other overriding maxim of 'CONSISTENCY—our

[9] W. V. O. Quine, 'Things and their place in theories', in *Theories and Things*, Harvard University Press, 1981, pp. 1–23; at p. 21.

[10] W. V. O. Quine, 'Five Milestones of Empiricism', reprinted in *Theories and Things*, pp. 67–72; at p. 72.

legitimate preference for consistent theories' (p. 230). It is left to the second maxim, MAXIMIZE, to ensure a plenitude of objects and structures:

> ...the set theoretic arena in which mathematics is to be modelled should be as generous as possible, the set theoretic axioms from which mathematical theorems are to be proved should be as powerful and fruitful as possible. Thus the goal of founding mathematics without encumbering it generates the methodological admonition to MAXIMIZE. (pp. 210–11)

The question, of course, is: How does one tell, formally, when a theory is maximizing? Maddy's answer is at best partial. In Chapter III.6, 'A Naturalist's Case against $V = L$', she attempts to provide a formal criterion for telling when a theory is (too) *restrictive*. Non-restrictiveness will be a necessary, but by no means sufficient condition for maximizing. Maddy is able at least to show that (on her formal explication) ZFC plus the Axiom of Constructibility ($V = L$) is restrictive, and that the latter axiom therefore should not be adopted.

2. Critical discussion.

2.1. Restrictiveness. Maddy's definition of the notion 'T is restrictive' is the heart of her original contribution in the book. Over pp. 220–24 she gives a sequence of definitions building up to it. The formal notion, however, does not accomplish much. For as Maddy concedes (p. 225) her definition of restrictiveness 'classifies as restrictive certain theories that don't seem restrictive and ... fails to classify as restrictive certain theories that do seem restrictive.' That is, her criterion admits false positives and false negatives, respectively. So it will 'need supplementation by informal considerations of a broader character', such as whether an inner model is 'optimal' (p. 227) or whether a theory (such as ZFC extended by the claim that $0^\dagger$ does not appear in any transitive set model of ZFC) 'leans hard in [the] direction' of inconsistency. (p. 229) Such considerations are so dependent on the refined intuitions of expert practitioners that it is hard to see how any account that has to resort to them can count as fully naturalized (in the normal philosophical sense of this term—see §2.2 below). And even if this shortcoming can be remedied, by providing a revised formal definition admitting (as far as one can tell) no false positives or false negatives, still one can point out that the criterion of restrictiveness cannot be the whole story needed. Having a formally specifiable negative filter on extensions of ZFC is not enough to convince one that our eventual *positive* choices of new axioms to append to ZFC can be explained by a genuinely naturalistic model, whether in the usual sense or in Maddy's sense.

2.2. NATURALISM. I have three criticisms to offer of Maddy's naturalism. They concern, respectively, her accommodation of the *a priori*; an internal tension in her account; and her quietism about classical methods.

Maddy's naturalism, one could argue, is no such thing; it is rather, on closer examination, an *a priori* investigation, involving conceptual analysis, considerations of intended interpretation, and non-empirical striving for reflective equilibrium. In a way that would find agreement with Plato, Descartes, Leibniz and Kant, Maddy emphasizes the *a priori* nature of mathematics as its defining trait. She divorces mathematics from natural science in general, and insists that one seek to understand mathematics on its own terms. As a result, her naturalism (about mathematics) is leached of the original philosophical significance that a Quinean naturalism would have had for mathematics. How is this so?

As Steven Wagner and Richard Warner wrote in their editorial introduction to a recent collection of essays on naturalism,[11] 'we take naturalism to be the view that only natural science deserves full and unqualified credence.' This is echoed by the definition of naturalism offered in *The Oxford Companion to Philosophy*, edited by Ted Honderich.[12] Naturalism is

> the view that everything is natural, i.e. that everything there is belongs to the world of nature, and so can be studied by the methods appropriate for studying that world. ... In metaphysics naturalism ... insists ... that the world of nature should form a single sphere without incursions from outside by souls or spirits, divine or human, and without having to accommodate strange entities like non-natural values or substantive abstract universals. (p. 604)

Thus, *prima facie*, naturalism would incline one to a nominalistic view of mathematics. Quine, however, accorded mathematical entities the status of scientific posits. Mathematical entities were on an ontological par with theoretical entities such as fundamental particles. This was because of the way hypotheses quantifying over them were integrated into an holistic web of mathematical-and-scientific belief. That web of belief faced the tribunal

[11] S. Wagner and R. Warner, eds., *Naturalism: A Critical Appraisal*, University of Notre Dame Press, 1993; at p. 1.

[12] Oxford University Press, 1995.

of experience as a whole. One could not factor out the special contribution of mathematics; nor did one have any intuitive intellectual access to such things as numbers and sets, independently of the role they would play in scientific theorizing about nature overall.

So for Quine, nominalism is averted as a consequence of naturalism by integrating mathematics into natural science, and not according mathematics any separable status as an intellectual practice.

Maddy's anti-Quinean apostasy is precisely to accord mathematics such separable status. Yet almost all other aspects of her philosophical temperament appear to remain Quinean. Her leading proposal from p. 182 was quoted earlier. Her definitional template for mathematical naturalism admits the following substitutions, yielding peculiar brands of 'naturalism':[13]

> a theological naturalism that extends the same respect to theological practice that the Quinean naturalist extends to scientific practice. ... theology is not answerable to any extra-theological tribunal and not in need of any justification beyond prayer, religious indoctrination and the study of scripture.

If that seems unbearable,[14] the Quinean ought still to be troubled by

> a semantic naturalism that extends the same respect to semantic practice that the Quinean naturalist extends to scientific practice. ... semantics is not answerable to any extra-semantic tribunal and not in need of any justification beyond introspection on one's own grasp of meaning and the method of meaning-postulates.

Maddy says her plan 'is to construct a naturalized model of [mathematical] practice'. (p. 193) But this does not mean a model adverting only to the patterns of neurological firings in the brains of mathematicians, and the causal story behind their jottings and their tappings on keyboards—which is what one might expect, given the other characterizations of naturalism quoted above. Rather, she is out to account for the 'actual justificatory

[13]These substitutions are mine. Maddy anticipates only the objection that would issue from 'astrological' naturalism; she does not say anything about theological or semantic naturalism.

[14]For further interesting discussion of the difficulties, on Maddy's conception of naturalism, in distinguishing between theological and mathematical naturalism, see J. M. Dieterle, 'Mathematical, Astrological and Theological Naturalism', *Philosophia Mathematica* 7, June 1999, pp. 129–35.

structure of contemporary set theory' and to argue for the claim that 'this justificatory structure is fully rational'. Any *a priorist* foundationalist would be in her debt were she to succeed.

Maddy wants to identify the *goals* of set theoretic practice, and '[elaborate] means-ends defences or criticisms of particular methodological decisions.' (p. 197) She aspires thus to straddle the boundary between the descriptive and the normative. Moreover, she allows no room for any rational critique of the goals that the community of mathematicians might set for themselves. She provides no criterion by means of which one might distinguish between (i) the goals to be served by the practice as a whole (for example: *boost the self-esteem of schoolchildren*; or *develop an appropriate mathematics for non-continuous spacetime, in case it is needed for the theory of quantum gravity*, and (ii) the intra-theoretic goals to be pursued within the practice (for example: *solve the continuum hypothesis*; or *classify the simple finite groups*).

It is worth pointing out that mathematicians *within* the mathematical community will often take issue with the direction of their discipline, its fundamental assumptions, its fundamental goals, and its own criteria for judging excellence and importance of work in various areas of mathematics. Just such a debate in under way at present, for example, over the issue of large cardinal axioms, and how best to convince 'core mathematicians' of their importance for 'ordinary' mathematical problems.[15] The mathematical community itself should not be thought of as monolithic, producing self-regulatory norms commanding community-wide consensus within, and impervious to criticism from without. Some mathematicians are very philosophically minded, and allow philosophical considerations to influence their choice of problems and methods. Maddy writes as though, from her naturalistic perspective, professional mathematicians would have to be allowed to be a law unto themselves. But, first, this exaggerates the degree of unanimity on important issues even among professional mathematicians. Secondly, it downplays the legitimate interest, of those who are *not* members of that professional community, in the body of norms that govern the mathematicians' own practice. For, as Maddy herself points out (pp. 204–5), mathematics is 'staggeringly useful, seemingly indispensable, to the practice of natural science'. One might add also: to engineering and technology; to medical

[15] This emerged clearly at the ASL panel discussion on the topic 'Does Mathematics Need New Axioms?', at the University of Illinois at Urbana-Champaign on June 5, 2000. The panellists were Solomon Feferman, Harvey Friedman, Penelope Maddy and John Steel.

diagnostics; to the financial markets; to actuarial science; and to a host of other areas of human activity in which *everyone's* interests and concerns are engaged. So ramifying would be the consequences, both theoretical and practical, of any lapse on the part of professional mathematicians into bad practices resulting from erosion of their historically hard-won norms, that even those *outside* the community of professional mathematicians have a permanent and legitimate concern in the nature of the norms governing the latters' practice. In a word: mathematicians cannot be allowed to be a law unto themselves. What they do is too important, and ought to be subject to outside constraints designed to protect everyone's interests.

The 'naturalistic' model that Maddy proposes is beset with an internal tension. On the one hand, the phenomena being modelled are intrinsically semantic and norm-governed. One is modelling 'practice', 'debates', 'arguments', 'discourse', 'utterances', 'testimony', 'reports'. (p. 199) In doing so, one is descrying the theorists' goals, as well as 'the underlying justificatory structure of the practice' in which 'sound and persuasive arguments' may be implicit. (p. 200) On the other hand,

> . . . suppose mathematicians decided to reject the old maxim against inconsistency—so that both '2+2=4' and '2+2=5' could be accepted—on the grounds that this would have a sociological benefit for the self-esteem of school children. This would seem a blatant invasion of mathematics by non-mathematical considerations, but if mathematicians themselves insisted that this was not so, that they were pursuing a legitimate mathematical goal, that this goal overrides the various traditional goals, *I find nothing in the mathematical naturalism presented here that provides grounds for protest.* [My emphasis][16]

According to Maddy, 'our fundamental naturalistic impulse' is the 'conviction that a successful practice should be understood and evaluated on its own terms.' (p. 201) The footnoted concession emphasized in the last quotation reveals, however, that the uncritical quietism essential to Maddy's

[16] Fn. 9, p. 198. Maddy (personal correspondence) assures me that certain other offbeat mathematical ideas were slips of the pen. In footnote 16 of p. 27 of the hardback edition, we are told that we can '[erect] an equilateral right triangle on the unit length of a line'; and on p. 197 she tells us 'I want to prove that P is false, so I'll assume not-P and derive a contradiction.' But with what justice, on her own account, would Maddy be able to claim that she was in error here, if she suddenly found herself surrounded by mathematicians unanimously agreeing on such things?

'naturalistic' methodology has reduced that methodology to absurdity. Interestingly, it is not only the *a priori* foundationalist, shocked at the thought that mathematicians might propose to have 2+2 being equal both to 4 and to 5, who is rendered impotent; it is also the Quinean re-distributor of truth-values! For Maddy tells us (p. 204) that 'the scientific naturalist [has] reason to refrain from criticizing mathematical methods'. Astrology, she says, 'is subject to scientific correction in a way that pure mathematics is not'. Implicit in these comments is a rejection of the Quinean view that no statement—not even a mathematical one—is immune to revision as we try to adjust the whole web of belief to the tribunal of experience. Maddy, by contrast, has conferred on mathematical statements an extraordinary immunity to revision, even when (as in the example of her footnote) we would want to say that the whole community of mathematicians had taken leave of its collective senses.

Such is the price of buying into Quineanism without its unrelenting, naturalizing *holism*. For Quine, it is the evidential holism in our theory of nature that truly naturalizes other areas of thought, such as mathematics. Natural science *as a whole* has to be understood in its own terms. To be anti-holistic, and separate mathematics off from science as a whole, as Maddy does, is to divert the springs of naturalism at their very source. And to apply the Quinean dictum about science as a whole to a self-contained mathematics shorn thus from its connections with the theory of nature is to lapse into two intimately related kinds of original sin: *a priorism* in mathematics; and the accompanying belief that mathematics will be immune to revision in the light of any human sensory experience.

Section III begins with a statement of Maddy's naturalism: '[I]f our philosophical account of mathematics comes into conflict with successful mathematical practice, it is the philosophy that must give.' (p. 161) But, it may be asked, is there such a thing as a philosophically neutral criterion of *successful* mathematical practice? How could one identify such practice (especially as *successful*) before engaging in any kind of philosophical reflection on its role within the wider body of human thought? Why is this uncritical *quietism* about the norms of a particular practice being dignified with the label 'naturalism'?[17]

[17] In personal correspondence, Maddy claims 'there is nothing in my mathematical naturalism that insists on (or is quietistic about) classical logic and mathematics. It leaves room for any and all arguments against these classical forms and for whatever else you like, as long as those arguments are mathematical, not philosophical.' And there's the rub. The philosophical, meaning-theoretic arguments of the Dummettian anti-realist against

Consider someone who is indisputably a naturalist, in that she is an atheist and materialist, committed to the methodological precepts of natural science in accounting for the workings of the perceivable world. Such a naturalist could believe that analysis of the meanings of logico-mathematical expressions is best pursued by inquiring after certain central patterns of correct inferential use of those expressions. Our naturalist could believe that one outcome of such analysis is a reflective equilibrium in which not all of classical logic and mathematics is sacrosanct and beyond philosophical criticism. Thus our naturalist, whose naturalism is genuine, might not like to see the term 'naturalism' hijacked for what is really only quietism about the canons of classical logic and mathematics.

2.3. Extrinsic versus intrinsic justifications. Recall that the leading idea behind the distinction between extrinsic and intrinsic justifications was to allow for the possibility that some new axioms of set theory might admit only of extrinsic justifications, rather than force themselves upon us as true when we confine ourselves to reflecting on their conceptual ingredients. Intrinsic justifications are supposed to exploit only the conceptual ingredients in the claims being justified; while extrinsic justifications are supposed to advert to the consequences of those claims. But are the classes of intrinsic and extrinsic justifications really disjoint? An inferential meaning theorist[18] might wonder why Maddy calls extrinsic (and by implication, non-intrinsic) those justifications that advert to derivable consequences. In so far as entailments help constitute meanings, considerations of what follows from what are at the heart of a properly conducted meaning analysis. Thus what Maddy is inclined to call an extrinsic justification could be claimed to be nothing more than an exercise in conceptual grasp. Why should not deductive explorations be on an equal footing with reflective intuition? In general, why should not any path to reflective equilibrium in the foundations of mathematics be claimed to be wholly concept-driven?

It transpires that each axiom (or axiom scheme) of ZFC can lay claim to an *intrinsic* justification, even though Maddy does not point this out by

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classical logic and in favor of intuitionistic logic are hereby ruled out. The result will almost certainly be quietistic, since classical reasoning is so deeply entrenched in mathematics. Why forbid an external fulcrum of philosophical detachment from the practice, and the force of non-mathematical (such as meaning-theoretic) considerations, when seeking proper leverage on normative issues such as correct choice of underlying logic?

[18]In *The Taming of The True* (Clarendon Press, Oxford, 1997) I seek to provide an inferential meaning theory for the main logico-mathematical operators. Such an account focuses on introduction and elimination rules for expression-forming operators.

way of summary of her own discussion. Nor does she seem to notice that the only argument (which she apparently endorses) *against* the Anti-Foundation Axiom[19] is one that, by her lights, counts as intrinsic (p. 61). She appears to be more concerned to stress the use of extrinsic justifications whenever they appear in the literature.

Maddy's contrast between extrinsic and intrinsic justifications can be applied to clarify her discussion of the axiom of constructibility. In her final chapter, 'A Naturalist's Case Against $V = L$', Maddy does not happen to mention extrinsic or intrinsic justifications. Her criterion of restrictiveness, however, involves definitions adverting to various *consequences* of the theories under consideration. Among such consequences, for example, are 'everything has property φ'; 'everything below some inaccessible rank has property φ'; 'if a set has property φ then so do all its members'; and 'there are non-φ isomorphism types'. Thus the considerations marshalled in her case against $V = L$ are extrinsic, in her sense of this term, even though she does not explicitly draw attention to this fact.

The case study may therefore be construed as a plausibility argument for the claim that at least one axiom candidate can be decided upon (in this case, negatively) by bringing important extrinsic consideration to bear. Admittedly this is only a case study. Given, however, that there would appear to be a sufficiently strong *conceptual* case against $V = L$, one might wonder how such a case study (however adeptly pursued, and however strong and subtle the extrinsic considerations for the same conclusion might be) can conduce to a naturalistic view about higher set theory. What the naturalist is offering us might be interesting; but the *a priori* foundationalist needs something more before being moved to regard it as more than an afterthought, or codicil to the real conceptual work that already settles the status of the axiom to his or her satisfaction. The metaphilosophical thesis of the naturalist needs to be something as strong as 'There is at least one axiom candidate whose acceptance or rejection is motivated solely by extrinsic considerations.' Gödel himself ventured the conjecture that this might one day prove to be true; but offered no concrete example of such an axiom. Maddy takes a careful look at one important axiom ($V = L$) but the results of her study do not bear out Gödel's surmise.

2.4. BIVALENCE. Gödel's realism is very much a Platonic affair (concerning the existence of abstract objects) and offers no argument for the move from

[19] This axiom has been used by Aczel and others in place of Foundation for their work in situation semantics.

the existence of those objects to the claim that all mathematical statements about them are determinately truth-valued. Let us call this latter claim Bivalence. Now it is well-known that the intuitionist does not accept Bivalence. There is a defensible brand of intuitionism that nevertheless takes the objects of mathematics to be mind-independent, and the provision of proofs to be a rule-governed activity, not susceptible to the creative whims or idiosyncracies of individual mathematicians. That is to say, mathematical thought is still constrained by 'what is out there', and the individual mathematician is *not* free simply to 'make things up' as one goes along.

The brand of intuitionism in question was expressed as follows by Crispin Wright:[20]

> ... someone could hold both that it is correct to think of the natural numbers as genuine objects ... and that there are decisive objections to the realist's way of thinking about the truth or falsity of statements concerning such objects. ... someone might be persuaded of the reality—the *Selbstständigkeit*—of the natural numbers but ... reject the realist conception of the meaning of statements about them.

The 'realist conception' at issue is that Bivalence holds.

Elsewhere I have emphasized and developed further the essential point here—that ontological realism can be combined with semantic anti-realism.[21] Such an 'ontologically realist' intuitionist would be in full agreement with Gödel's sentiment, quoted by Maddy: 'the objects and theorems of mathematics are as objective and independent of our free choice and our creative acts as is the physical world.'[22] Note how Gödel here talks of *theorems*, not of *truth values of statements* in general. He does *not* commit himself to anything as classically full-blooded as, say, 'the objects, and *a determinate truth-value for each and every statement*, of mathematics are as objective and independent of our free choice and our creative acts as is the physical world.' Yet it is this stronger reading that would be needed by anyone looking to Gödelian realism for a route to the conviction that CH has a

[20] *Cf.* C. Wright, *Frege's Conception of Numbers as Objects*, Aberdeen University Press, 1983, pp. xviii–xx.

[21] *Cf.* N. Tennant, *Anti-Realism and Logic*, Clarendon Press, Oxford, 1987, in ch. 2, 'Scientific v. Semantic Realism', pp. 7–12.

[22] K. Gödel, 'Some basic theorems on the foundations of mathematics and their implications' (The Gibbs Lecture, Brown University, 1951), in S. Feferman *et alia*, eds., *Collected Works, vol. III*, Oxford University Press, 1995, pp. 304–323; at p. 312, n. 17.)

determinate truth-value, possibly independent of our means of coming to know what that truth-value is.

As the reader will be able to check, every quotation that Maddy adduces from Gödel's writings (with one exception—see the next footnote) expresses no more than what an 'objective intuitionist' would readily concede.

The fundamental (and required) justificatory move is never made to determinacy of truth-value, whether for CH or across the board.[23] Interestingly, the very paragraph that Maddy quotes from Gödel in order to show how 'it is the mathematical considerations, not the philosophical ones, that are decisive', seems to embody decisively *philosophical* insights into the source of mathematical truth and the insufficiency of ontological realism to secure realism about truth-value. The paragraph in question is

> However, the question of the objective existence of the objects of mathematical intuition ... is not decisive for the problem under discussion here [i.e. the meaningfulness of the continuum problem]. The mere psychological fact of the existence of an intuition which is sufficiently clear to produce the axioms of set theory and an open series of extensions of them suffices to give meaning to the question of the truth or falsity of propositions like Cantor's continuum hypothesis.[24]

Thus Gödel, it would appear, grounds the *meaningfulness* of CH in the faculty of mathematical *intuition*, which is relied on to produce appropriate axioms to settle the matter. Those intuitions might be only partly 'of objects'; they might concern also the workings of our mathematical concepts, and the meanings of our mathematical vocabulary. The brute independent

[23] In personal correspondence, Maddy draws attention to her quotation (on p. 89) from Gödel: '... if the meanings of the primitive terms of set theory as explained on page 262 and footnote 14 are accepted as sound, it follows that the set-theoretical concepts and theorems describe some well-determined reality, in which Cantor's conjecture must be either true or false.' ('What is Cantor's continuum problem?' (1964), in S. Feferman *et alia*, eds., *Collected Works, Vol. II*, Oxford University Press, 1990, pp. 254–70; at p. 260.) She comments 'This clearly makes the move to determinacy of truth value for CH.' But I maintain that it does not. Rather, it simply *states* determinacy; it does not *infer* it from claims of existence. A 'well-determined reality' is by definition one in which every proposition has a determinate truth-value—*a fortiori* one in which CH has a determinate truth-value. What is still missing is an argument from the mere objective existence of mathematical objects, and perhaps the obtaining of certain primitive relations among them, to the determinacy of truth-value of *every proposition about those objects*, regardless of those propositions' logical (quantificational) complexity.

[24] 'What is Cantor's continuum problem?', *loc. cit.*, p. 268.

existence of those mathematical objects, could it somehow be secured, would not go far enough to settle the continuum hypothesis. Something more is needed: the contact of the mind, through its faculty of intuition, with that realm of objects. Such contact must issue in linguistically expressed *axioms*, and they in turn will settle the question of the truth-value of CH by means of appropriate *proofs*. One is tempted to ask: Where is the 'naturalism' in all of *this*?

2.5. UNIFICATION. The unification of mathematics that is afforded by set theory amounts, on Maddy's account, to the following: (i) set theory aspires to an ontology so rich that every conceivable mathematical object or structure could be assigned at least one surrogate within that ontology; (ii) every statement φ of classical mathematics can be translated (*modulo* appropriate definitions of the primitives involved in φ) into a set-theoretic statement $\varphi^{\in}$; and (iii) for every proof of a classical mathematical theorem φ there exists a set-theoretic proof of $\varphi^{\in}$.

There are at least two missing ingredients that would appear to be needed in order to advance from reductive *chutzpah* to truly profound unification. The first would be a convincing demonstration that certain innocent-looking problems in 'ordinary' mathematics are solvable only on the basis of set-theoretic principles controlling the behavior of the cumulative hierarchy 'way above' the likes of $V_{\omega+\omega}$, where (surrogates for) all the objects talked about in such problems reside.[25] The 'ordinary mathematical universe' $V_{\omega+\omega}$ is but a tiny chunk at the bottom of the cumulative hierarchy. This pinpoints the special interest of Friedman's recent work on finite independence results.[26] This work shows that set theory is important not simply because it accommodates classical mathematics under suitable reduction, but because certain simple problems that one would unreflectively reckon to be solvable, if at all, 'in their own terms', turn out to require the postulation of certain large cardinals normally thought of as the esoteric concern of pure set theorists.

The second missing ingredient would be some kind of assurance that the variety of proof methods in the different branches of classical mathematics could be *homologously accommodated* by the methods of proof available in the foundational theory (in this case, set theory). One could impose this sort of adequacy requirement, *and* as far as one can tell, set theory actually meets it. This would enable one to stress further the significance of

[25] See H. Friedman, 'Higher Set Theory and Mathematical Practice', *Annals of Mathematical Logic*, vol. 2, no. 3, 1971, pp. 325–57.

[26] 'Finite Functions and the Necessary Use of Large Cardinals', *Annals of Mathematics* 148, no. 3, 1998, pp. 803–93.

set theoretic foundations. On Maddy's construal, a piece of mathematical reasoning in some synthetic branch of classical mathematics, from a set of axioms Δ to a theorem φ, needs only to be matched by some proof or other, from the axioms of ZFC, of the set theoretic translation $\varphi^{\in}$ of φ. But this is overly modest. Why not insist further that one should be able to take the 'synthetic' proof of φ from Δ, and 'homologously unfold it' into the sought set-theoretic proof of $\varphi^{\in}$? Meeting such a requirement would of course provide no guarantee that ordinary mathematicians could eschew their synthetic methods in favor only of strictly set-theoretic ones; for the requirement (stated now in the converse direction) is only that one should somehow be able to 'condense back' the set theoretic proof so as to obtain the original synthetic proof. But the latter might still have to be sought and discovered in its own, appropriately compiled, terms.

2.6. INDISPENSABILITY. What is the force of the indispensability argument? Maddy answers (p. 159)

> My guess is that the practice of set theory, the methods set theorists actually use to pursue the independent questions, would be unaffected, no matter how these issues [of scientific applications of continuum mathematics] might turn out. ... mathematics, like natural science, seems not to be conducted as it would if the presuppositions of our indispensability theorist were correct.

Maddy takes her case-study of atomic theory and the slow acceptance of the reality of atoms as showing that natural science does not have its existential claims arbitrated in the way that the Quinean indispensability theorist thinks they are: namely, by the criterion of quantificational commitment within a unifying physical theory, and with a univocal sense for 'there exists'. It is very tempting, however, to respond to her case study of atomic theory, and her anecdotal surmises about the practice of set theorists, with two suggestions.

The first suggestion is that the pragmatic naturalist should seek a little further for the more exigent nature of clinching commitment to new natural kinds. What exactly is the logical structure of the explanatory hypotheses whose use in generating explanations finally induces one to recognize the reality of the natural kinds over which they quantify? And what sorts of explanations must one be in a position to provide, before such recognition is regarded as justified? It must, surely, have something to do with how explanatory and predictive hypotheses logically relate to observable evidence. What is the nature of that relation? There may well be a logically interesting story to tell here, on a par with a finally satisfactory criterion of cognitive significance.[27]

The second suggestion is that on the mathematical side one should have the intellectual courage to admit the stark and obvious fact that the discipline is a wholly *a priori* one, whose standards of commitment are intuitive, logical and conceptual. *Even if* spacetime is grainy, so what? As far as the abstract *mathematical* continuum is concerned, the truth-value of the continuum hypothesis, if it is somehow fixed, will be so quite independently of what mathematics is needed for the empirical description of our contingent world of concrete objects. It is no wonder at all that set theorists would not care whether Feynman's successors opt for non-continua to describe spacetime. It is genuinely irrelevant, both conceptually and logically, to mathematical propositions and how we properly come to establish their truth-values.

2.7. Zermelo's 'pragmatism'. Maddy contends that the ontology that results from the formation of ranks in the cumulative hierarchy of sets is, in effect, that of the cumulative theory of types. The impression of seamless confluence, or conceptual convergence, between Fregean and Cantorian developments is underwritten further by her (unargued) claim that Zermelo's pioneering investigations of 1908[28] were in pursuit of a purely pragmatic end—that of 'selecting a simple, efficient, and powerful set of axioms from among the jumble of controversial and mutually exclusive set theoretic principles being debated at the time.' (p. 19)

Maddy does not remark, however, on the fact that the confluence of type-theoretic and set-theoretic thinking is more of a *transference*—from the linguistic (i.e., the stratification of type theory) to the ontological (i.e., the ranks of the cumulative hierarchy of sets). It could also be objected that she makes Zermelo out to be much more of a methodological naturalist than he might have been. In his 1908 paper Zermelo wrote (at pp. 210–11)

> The further, more philosophical, question about the origin of these principles and the extent to which they are valid will not be discussed here. I have not yet been able to prove rigorously that my axioms are consistent. ... But I hope to have done at

[27] *Cf. The Taming of The True, op. cit.*, ch. 11, 'Cognitive Significance Regained', pp. 355–402.

[28] E. Zermelo, 'Investigations in the foundations of set theory, I', reprinted in J. van Heijenoort, *From Frege to Gödel: A sourcebook in mathematical logic*, Harvard University Press, 1967, pp. 200–15.

> least some useful spadework hereby for subsequent investigations in such deeper problems.

It is clear that Zermelo saw his task as that of formulating certain set theoretic principles as formal axioms and then demonstrating their power by deriving various important theorems. As Gregory Moore argues in his book on the Axiom of Choice,[29] Zermelo's primary motivation in formulating the axiom was to formalize his informal proof of the well-ordering theorem, so as to defend the informal proof against various critics. Zermelo's paper reveals very little of whatever sort of thinking might otherwise have resulted in his specific choice of axioms. His words quoted above amount to a 'limitation of space' apology. They invite the inference that his method of discovery (of suitable axioms) might have had just as much to do with philosophical introspection and analysis as it might have had to do with logical manipulations within a body of sought theorems upon which a certain deductive organization had to be imposed. But *even if* the sole inspiration for Zermelo's choice of axioms lay in certain 'pragmatic' insights as to what would logically afford what, there is the further question as to how one *explains* the fact that in our informal reasoning about sets as mathematical objects we happen to proceed in such a way as is best elucidated by that particular choice of first principles. Why *do* we choose the starting points that we do?

2.8. THE EVOLUTION OF THE NOTION OF A MATHEMATICAL FUNCTION. $V = L$ was formulated only after Bernays's statement of combinatorialism. One is led to wonder whether, had Gödel's definition of L come much sooner after Zermelo's formulation of AC, matters might have turned out differently in the annals of mathematics. The trouble with appealing to history in seeking extrinsic justifications is that one can lose sight of whatever *a priori* or conceptual insights and reasons might have been driving the evolution of the mathematical thought that one chronicles.

Maddy's own approach to this branch of intellectual history seems to be premissed on the view that, if thinking has evolved this way, then there must have been some good, justifying reason for it to have done so. But to concede even *that* much is to allow there to be a deeper, *a priori* justification underlying what purports to be a merely pragmatic one (appealing to the history of mathematical practice). One could, however, go even further than Maddy's implicit view, and claim that clearer intuitions and a sharper grasp of abstract concepts will eventually get the upper hand in mathe-

[29] G. Moore, *Zermelo's Axiom of Choice: its Origins, Development, and Influence*, Springer Verlag, New York, 1982, chs. 2–3; esp. pp. 158–9.

matics, because of its own internal norms. Understand it as the kind of *a priori* enterprise that it is, and one thereby has the essentials of an explanation of why mathematicians' thinking about functions, say, has taken the various turns that Maddy chronicles. Perhaps Combinatorialism, allowing both indefinable and impredicatively defined functions to exist, is a sort of conceptual attractor in the phase space of mathematical thought. *Any* community of rational creatures engaged in mathematical investigations might have to end up where we are now, with the Combinatorial conception firmly entrenched. Their starting points might be different, and the history of their thought might be subjected to different perturbations of deviance (predicativism, definabilism, finitism, and so on); but, perhaps, as the debates are worked out, and the dust settles, it is always the combinatorial conception that must shine through.