

V=L and Maximize

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The problem that interests me is easy to state: what justifies the axioms of set theory? Some observers suggest that decisions on the adoption or rejection of axiom candidates are not made on rational grounds, that they are subject only to psychological or sociological or aesthetic constraints. This may be right, but I think it is too early to concede the point; my working hypothesis is that there are sound arguments to be made on these issues. As a student of the methodology of set theory, my job is to try to isolate and elaborate these sound arguments.

In Zermelo's day, for many of the axioms in his first list, the problem of justification was somewhat less daunting than it is for the candidates in dispute today. Zermelo was faced with a fairly well-developed body of set theoretic lore; the difficulty was that paradoxes and other uncertainties lurked around its edges. His goal was to select some particularly fundamental statements to serve as starting points; this was to be done skillfully, so that the core doctrine could be deduced without the troublesome outliers. The upshot, for many of his axioms, was that certain previously-accepted claims were being promoted to axiomatic status, not that any new claims were being made. The selection of these particular statements for promotion needed justification—presumably on grounds of economy, efficiency, and likely consistency—but the problem of justifying the statements themselves was eased by their previous acceptance.¹

The case of the axiom of choice was different. Choice was not a previously-accepted or uncontroversial claim; what needed justification was not its mere promotion, but the statement itself. With this case, the problem of what justifies an axiom arose in a more pressing and poignant form, as did the prior problem of what sorts of grounds are appropriate for such justifications. The subsequent, fascinating history is familiar, and the outcome is now stable, so I won't rehearse it here. But it is important to note that since then, with the advent of independence results and the subsequent search for new, stronger axioms, the problem of justification has become ever more acute.

Rather than talk in the abstract about the general problem of justifying axioms, I want to focus on one particularly salient case: the axiom of constructibility. The decision on $V=L$ is the first truly momentous one after ZFC, and it is pivotal for the further development of the subject. Despite the fact that adding $V=L$ is a safe, economical, and powerful option, settling many of the lingering

¹ This is not to say that all Zermelo's axioms, even excluding choice, were uncontroversial or that his system was immediately accepted. See Moore [1982], pp. 160-167, for details.

open questions, I think it is fair to say that most contemporary set theorists believe that it should be rejected. An instance of my working hypothesis is the assumption that there are good grounds for this consensus, and what I want to explore here is the question of what those good grounds are.

1 Naturalism

Before I take up this project directly, a few words should be said about the prior question: what sort of arguments are appropriate for the justification of an axiom candidate (or in our case, for the justification of the rejection of an axiom candidate)? The standard method of justification in mathematics is proof, but axioms cannot be proved, so where are we to turn for support? As I've mentioned, some people think we are free to choose the starting points of our proofs in any way we please, that we are unconstrained by any rational considerations (except perhaps consistency). The philosophy behind this view is what I call 'glib formalism', the stance that any relatively consistent extension of ZFC is as good as any other, that only sociology or aesthetics or personal whim prompt us to choose one over another.

Now it seems to me that this stance is not consistent with the practice of set theory; set theorists seriously committed to the project of adding new axioms to ZFC would hardly pursue the subject in the spirit they do if they believed the issue could be properly settled by a statement of personal preference or a vote at an ASL meeting (though, of course, such votes are sometimes taken). Under these circumstances, the attractions of a realistic philosophy are obvious. If set theory is the study of an objectively existing world of sets, much as botany is the study of plants and particle physics the study of the small constituents of matter, then a correct extension of ZFC is one that faithfully reflects the structure of that reality of sets; assuming $V=L$ has the other virtues we expect from axioms—simplicity, generality, power, etc.—we should add $V=L$ to our list of axioms if all sets are constructible; otherwise, we should reject it. The question is as substantial as any scientific question; it is a question about the properties of the objective realm of sets. On this reading, realism is called upon to support the existing practice of taking the decision on new axioms seriously.

Now it is well-known that the task of fleshing out a realistic account of mathematics in general, or of set theory in particular, is a difficult one, fraught with epistemological and ontological obstacles, but I'm not interested in those problems here.² In fact, I think that on closer examination the underlying strategy of justifying mathematical practice by appeal to philosophical realism should fail to satisfy either the mathematician or the philosopher of mathematics. I have various messy metaphilosophical reasons for this belief, but our subject here is $V=L$, so I'll just give a quick sketch of the central idea.³

To see why the mathematician might be wary of the strategy of justifying mathematical practice by appeal to philosophy, consider a set theorist ponder-

² I discuss them at length, and attempt to answer them, in [1990].

³ For more, see [199?b], [199?c] and [199?d].

ing the addition of a new axiom. If it were somehow conclusively demonstrated that realism is not a viable account of mathematics, would he conclude that the question he is pondering is actually a trivial one, to be decided on whim? Probably not; more likely, he would continue his pondering in much the same terms as before. The lack of a suitable metaphysics with which to defend his practice might be an embarrassment, but he is unlikely to suspend the practice for that reason. In other words, his assessment of the value of his undertaking is not, after all, dependent on his belief in the correctness of realism. Realism provided a handy counter to certain challenges, but it wasn't the true justification.⁴

To see where the philosopher might also balk at the attempt to justify practice with metaphysics, consider the situation from the perspective of a philosopher who is a scientific naturalist; that is, suppose she believes that the practice of natural science is not answerable to the epistemic standards of any external, a priori standpoint, that legitimate criticisms of scientific method can only come from within scientific practice itself. Such a philosopher may well feel that the same goes for mathematics, that successful mathematical practices should not bend to criticism developed in psychology departments or philosophy seminar rooms.⁵ Having come so far, our philosopher may well also conclude that considerations external to scientific or mathematical practice cannot justify those practices any more properly than they can criticize them, that justifications must also proceed from within the practice. And this means, in particular, that efforts to justify the set theorist's practice on the basis of a philosophical realism about sets is misguided and ineffective.

Both these thought experiments point toward a metaphilosophical position I call mathematical naturalism: the grounds on which to criticize and/or justify mathematical methods are to be found inside mathematical practice itself; the practice need not answer to, nor can it look for support from, any external standard. So, for example, the mathematical naturalist will come down in favor of impredicative definitions because they make possible a classical theory of real numbers, an extremely fruitful mathematical construction, but the same naturalist will dismiss as irrelevant the as-yet-unresolved metaphysical debates concerning the nature of mathematical existence that have been thought to bear on this topic. Presented with a patently successful mathematical theory, the realist might still protest, 'yes, this theory has all the mathematical virtues I can imagine, but we must also ask: is it true in the objectively existing world of sets?'. The naturalist will see this very protest as the importation of an extra-mathematical standard; if a mathematical theory has all the virtues mathematics requires, then the naturalist holds that no further question is of any methodological significance.

⁴ Cf. Kanamori ([1994], p. 481) on the 'invariance' of mathematics under philosophical change.

⁵ Of course, it isn't really a matter of where the criticisms originate, but of what methods they use: e.g., a criticism of classical analysis based on a mathematician's philosophical intuition about abstract objects is a non-starter, while a logical criticism coming from a philosopher (e.g. Bishop Berkeley) may be quite to the point.

The contrast, then, is this: where the realist will ask—is this axiom candidate true?—the naturalist will ask—would the adoption of this axiom candidate produce a fruitful theory—or better yet—would the adoption of this axiom candidate further the goals of this practice? I don't pretend that the naturalist's question is any easier than the realist's—after all, it involves such tricky notions as 'mathematical fruitfulness' and 'the goals of a mathematical practice'—but I do contend that the naturalist's question provides a more productive focus for our inquiry. While the realist is led off into philosophical worries about the nature of mathematical existence and our access to mathematical truth,⁶ the naturalist concentrates instead on considerations squarely inside mathematics. Judging from historical cases, from Euler's generalization of the notion of function to the contemporary consensus on the axiom of choice, it is these practical considerations about the fruits and goals of the practice itself that are in fact decisive.⁷

Before leaving this meta-topic, let me make one last observation on the character of naturalistic arguments. Given that a naturalistic philosopher brings no special modes of argument from philosophy, every argument she gives must be based on modes of argument available to any mathematician qua mathematician; at best, she will make explicit what is already implicit. Unsatisfying as this may be in dramatic terms, a good naturalistic argument should not strike the practitioner as late-breaking news; at best, it will fall so far short of originality as to qualify as a commonplace. Given this goal, the best confirmation of success would be for the mathematician to shrug and say, 'of course, everybody knows that.' I think there is a non-trivial link between this fact and Wittgenstein's remark that 'Philosophy only states what everyone admits' ([1953], §599), but I won't drag this discussion off into the wilds of Wittgensteiniana.⁸

So, all that said, my goal here will be to sketch an argument against $V=L$ that is naturalistic in this sense. To do this, I might try to exploit any one of various commonly heard complaints about $V=L$. In earlier work, I argued that $V=L$ is worthy of suspicion as an axiom candidate, because it is closely allied with a methodological principle, Definabilism, which recommends that mathematical objects be regarded as definable or constructible in a uniform way, and Definabilism has a bad track record in the history of mathematics.⁹ But grounds for suspicion are not grounds for rejection, and I am after the stronger

⁶ As in my [1990].

⁷ I think the 'anomalies' of [1993] are better understood from a naturalistic than from a realistic point of view. It may seem a stretch to regard the mathematical considerations that influenced opinion in those cases as evidence for the truth of the hypotheses in question, realistically understood, but it seems quite plausible to suppose that they provided good evidence for the mathematical fruitfulness of those hypotheses, good evidence for the efficacy of those hypotheses in pursuit of the goals of the relevant practices.

⁸ I discuss examine this connection in [199?a].

⁹ See my [1993]. There I suggested that an argument for the stronger conclusion, that $V=L$ should be rejected, would require a different philosophical backdrop from the realism presupposed there. Naturalism is the proposed replacement.

conclusion here. The argument I will sketch rests on the common thought that $V=L$ is bad because it is 'restrictive'; to make this go, I will have to explain why restrictiveness is bad, and why $V=L$ is restrictive.

But first, a warning is in order: though I will do my best to present this argument as convincingly as I can, I will conclude with an examination of where it goes wrong. I do this for two reasons. First, the difficulties that arise are rather subtle and suggestive, and I hope that careful attention to their structure might lead to an improvement of the argument. And second, as a naturalistic philosopher, the best input I can get is the reactions of an audience like this one, to the general approach of the argument, to its details, to the possibilities for improvement. So I hope I will be forgiven for this rather unorthodox procedure.

2 Maximize and Unify

To begin, I want to examine what I take to be one among the many of the general goals of set theoretic practice. I will suggest that this goal, once isolated, motivates two methodological maxims, as effective means for the achievement of that goal. Both these maxims are ultimately relevant to the case against $V=L$. First, then, the goal.

Again, as befits a naturalistic argument, the point is quite simple and familiar. Though Cantor's set theory arose out of his work on the uniqueness of trigonometric representations, it wasn't long before he was writing that¹⁰

... pure mathematics ... according to my conception is nothing other than pure set theory.

Zermelo struck a similar theme in the paper containing his first axiomatization:

Set theory is that branch of mathematics whose task is to investigate mathematically the fundamental notions 'number', 'order', and 'function', taking them in their pristine, simple form, and to develop thereby the logical foundations of all of arithmetic and analysis. ([1908b], p. 200)

Since then, the idea that set theory provides a 'foundation' for mathematics has become so much a part of set theoretic orthodoxy as to appear in the opening sentences of its textbooks; for example, Kunen writes:

Set theory is the foundation of mathematics. All mathematical concepts are defined in terms of the primitive notions of set and membership ... from [the] axioms, all known mathematics can be derived. (Kunen [1980], p. xi)

¹⁰ This quotation comes from an unpublished paper of Cantor's written in 1884 (see Grattan-Guinness [1970], p. 84). The translation is from Hallett [1984], p. 125.

Moschovakis, alert to potential metaphysical disputes,¹¹ puts the point more carefully to his beginning students:

... we will discover within the universe of sets *faithful representations* of all the mathematical objects we need, and we will study set theory on the basis of the lean axiomatic system of Zermelo **as if all mathematical objects were sets**. (Moschovakis [1994], p. 34)

The idea is that set theory provides a foundation in the following sense: every classical mathematical object can be represented by a set, and every classical mathematical theorem can be proved from the axioms of set theory.

By providing a framework broad enough to supply instances for all structures of classical mathematics, set theory brings the various subdisciplines into a common arena, so that interrelations are stressed; for example, one can hardly help being stuck by the way versions of the same assumption—the axiom of choice—turn up among the basic premises of so many distinct branches of mathematics. In some cases—e.g., when the points on a line are modelled by the set theoretic reals—set theory provides a more precise account of the structure than had previously been possible. In other cases—e.g., for the algebraic question ‘are all Whitehead groups free?’—the set theoretic setting demonstrates that the question is independent of traditional assumptions, that no proof or disproof on their basis is possible. In still other cases—e.g. questions about the properties of simple sets of reals—set theory has shown how strong new hypotheses can resolve issues that baffled the analysts of the 20s. In light of the benefits, my suggestion is merely that it is one of the continuing goals of set theoretic practice to provide such a foundation.

Before drawing any consequences from this suggestion, let me pause a moment to clarify a few points often raised as objections to set theoretic foundations. The first, and oldest, harkens back to the original foundational programs of Frege and Hilbert, whose aim was to place mathematics on an unshakably secure basis. Seeing the matter in this light, Zermelo, presenting his axioms, writes with some regret:

I have not yet even been able to prove rigorously that my axioms are consistent, though this is certainly very essential; instead I have had to confine myself to pointing out now and then that the antinomies discovered so far vanish one and all if the principles here proposed are adopted as a basis. ([1908b], p. 200-201)

Poincaré phrases the same point as a sharp criticism:

¹¹ E.g., in Benacerraf [1965]. Moschovakis’s way of putting the point allows for the common idea that mathematics studies structures, not things; from this perspective, the job of set theory is to provide instantiations for all mathematical structures. Thus, set theoretical foundationalists can agree with their critic, MacLane, when he writes, ‘a real number is not a Dedekind cut; that cut is just one possible model of a protean idea of the reals’ ([1992], p. 121).

We have put a fence around the herd to protect it from the wolves but we do not know whether some wolves were not already within the fence. (Kline [1972], p. 1186)

And the same objection appears in the work of MacLane, the most prominent contemporary critic of set theoretic foundations:

Now in one sense a foundation is a security blanket: If you meticulously follow the rules laid down, no paradoxes or contradictions will arise. In reality there is now no guarantee of this sort of security; we have at hand no proof that the axioms ZFC for set theory will never yield a contradiction, while Gödel's second theorem tells us that such a consistency proof cannot be conducted within ZFC. ([1986], p. 406)

Since Gödel, as MacLane notes, Zermelo's hope of establishing the consistency of his axioms has been effectively dashed.

What needs emphasizing here is that the contemporary orthodoxy on set theoretic foundations does not claim to present a foundation in the sense of the early researchers, does not claim to base mathematics on unshakable premises. What's claimed is that set theory provides a unified framework in which the objects of classical mathematics can be modelled, the structures of classical mathematics instantiated, and the theorems of classical mathematics proved. Even critics like MacLane are willing to grant this much (see MacLane [1986], pp. 406, 358).

A closely related worry is expressed by Tiles. Logicians have long realized that the principles needed to found mathematics cannot all be plausibly claimed to rest on unsullied mathematical intuition. Russell, in his defense of the Axiom of Reducibility ([1910], pp. 59-60), Zermelo, in his defense of the Axiom of Choice ([1908a], pp. 186-190), Gödel, in his discussion of new set theoretic axioms ([1947], pp. 182-183)—all these thinkers have appealed to so-called extrinsic supports, that is, defenses based on the welcome consequences of an axiom candidate. Tiles claims that such defenses are inappropriate

... to the conception of set theory as providing a logical *foundation* for mathematics. To claim this status for set theory it is necessary to claim an independent and intrinsic justification for the assertion of set-theoretic axioms. It would be circular indeed to justify the logical foundations by appeal to their logical consequences, i.e. by appeal to the propositions for which they are going to provide the foundation. ([1989], p. 208)

But, once again, this critique is only sound if set theory is proposed as a foundation in the epistemic sense, as providing a 'secure given starting point' (Tiles [1986], p. 208). As long as set theory is only proposed—as it is here—as a unifying framework, as a shared, basic ontology, this criticism is also off the mark.

Finally, I suspect that there is another important, generally epistemological objection lingering the background. The most explicit statement of this objection

I can find comes from Mathias, a supporter of set theoretic foundations, as a conjecture on the underlying concerns of his opponents:

Set theory is so rich a theory that it has been claimed for much of this century to be the foundation of mathematics. In ontological terms this claim is not unreasonable; but MacLane resists. I would guess that his reason is not so much that he objects to the ontology of set theory but that he finds the set- theoretic cast of mind oppressive and feels that other modes of thought are more appropriate to the mathematics he wishes to do. ([1992], p. 115)

But again, to claim that set theory provides a unified ontological setting is not to claim that only set theoretic methods should be employed in mathematics, that algebraists, analysts, number theorists, geometers should all become set theorists. To draw a comparison, to say that all entities studied by natural science are ultimately physical, subject to the laws of physics, is not to say that biologists, chemists, botanists, and geologists should all become physicists. I think Mathias puts the point quite well, so I hope I will be forgiven for quoting him at some length:

One of the remarkable things about mathematics is that I can formulate a problem, be unable to solve it, pass it to you; you solve it; and then I can make use of your solution. There is a unity here: we benefit from each other's efforts. ... But if I pause to ask why you have succeeded where I have failed to solve a problem, I find myself faced with the baffling fact that you have thought of the problem in a very different way from me: and if I look around the whole spectrum of mathematical activity the huge variety of styles of thought becomes even more evident.

Is it desirable to press mathematicians all to think in the same way? I say not ... Uniformity is not desirable, and an attempt to attain it, by (say) manipulating the funding agencies, will have unhealthy consequences.

The purpose of foundational work in mathematics is to promote the unity [as opposed to the uniformity] of mathematics. ([1992], pp. 113-114)

Assuming then, that one of the goals of set theoretic practice is to provide a foundation in this sense, what methodological consequences follow? The first is immediate: if your goal is to provide a single system in which the objects and structures of mathematics can be modelled and instantiated, in which all theorems of mathematics can be proved, then you must aim for a single, fundamental theory of sets. This admonition to UNIFY is just the flip-side of another of MacLane's objections; he mentions the independence of CH, the proliferation of models achieved by forcing, the range of new axiom candidates, and concludes

For these reasons 'set' turns out to have many meanings, so that the purported foundation of all Mathematics upon set theory totters. ([1986], pp. 358-359)

The methodological maxim UNIFY simply runs this argument in reverse: if you wish to provide a foundation, you must settle on a unique theory. If some such

maxim were not in force, set theorists would not be motivated to decide the issue of $V=L$; it would be enough to consider alternative set theories with and without it.

The second methodological maxim that springs from the goal of set theoretic foundations is not much more distant. Contemporary pure mathematics is a vastly broad-ranging inquiry, dedicated to the notion that mathematicians should be free to study all and any structures and theories that seem to them of sufficient mathematical interest. If set theory is to found such a discipline, it should not impose any limitations of its own: 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. This desire to found mathematics without incumbering it generates the set theoretic maxim I call MAXIMIZE.

To be a bit more specific, I will focus on a particular aspect of the admonition to MAXIMIZE. Given that set theory is out to provide models for all mathematical objects and instantiations for all mathematical structures, one way in which it should MAXIMIZE is in the range of available isomorphism types. After all, perhaps the most fundamental contribution of set theory to date is its provision of a continuous structure to model the real numbers. The advice to MAXIMIZE isomorphism types is still regrettably vague; I will do my best to clarify it as I apply it.

3 Why $V=L$ is restrictive

My aim now is to argue that $V=L$ is restrictive, but 'restrictive' is a notoriously slippery notion. In the case of the continuum hypothesis, for example, arguments have been offered that purport to show that CH is restrictive, but arguments of similar structure purport to show that not-CH is restrictive!¹² I will be trying to isolate a sense of the term that provides a bit more guidance than this. As the idea is to motivate the case against restrictive theories by appeal to MAXIMIZE, the central claim will be that restrictive theories somehow restrict isomorphism types. But to say this is not to make matters much clearer.

Let me begin, then, with a crude version of the argument that underlies the approach I'll be taking. The idea is simply this: there are things like 0^\sharp that are not in L . And not only is 0^\sharp not in L ; its existence implies the existence of an isomorphism type that is not realized by anything in L .¹³ These facts

¹² See my [1988], pp. 497-498, 500, for summaries.

¹³ Suppose 0^\sharp exists, and consider the structure $(V_{\omega+1}, \epsilon)$. If $(y, S) \in L$ is isomorphic to $(V_{\omega+1}, \epsilon)$, then S is well-founded and extensional on y . These notions are both absolute for L , so L thinks S is extensional and well-founded on y , and so (since L also thinks Mostowski's theorem on transitive collapse), L thinks there is a transitive A such that $(A, \epsilon) \cong (y, S)$. A given function being such an isomorphism is also absolute for L , so the two structures must actually be isomorphic. Thus, $(A, \epsilon) \cong (V_{\omega+1}, \epsilon)$, and they are both transitive, so $A = V_{\omega+1}$. But $0^\sharp \in V_{\omega+1} = A \in L$ and L is transitive, so $0^\sharp \in L$. So L thinks $V \neq L$, which is impossible.

wouldn't carry any weight if $ZFC+\exists 0^\sharp$ were inconsistent—CONSISTENCY is an overriding maxim,¹⁴ as inconsistent theories are useless—but accumulated evidence suggests that $ZFC+\exists 0^\sharp$ is not inconsistent.¹⁵ So it seems that $ZFC+V=L$ is restrictive because it rules out the extra isomorphism types available from $ZFC+\exists 0^\sharp$.

By way of contrast, consider AFA,¹⁶ the anti-foundation axiom (see Aczel [1988]), which guarantees the existence of non-well-founded sets. It might seem that reasoning similar to what I've just rehearsed would classify ZFC as restrictive, because there are non-well-founded sets available from $(ZFC-F)+AFA$ that are not in WF (the class of well-founded sets). But the similarity is an illusion: the new, non-well-founded sets do not realize any isomorphism types that are not realized in WF.¹⁷

To view the matter from another angle, consider the following style of argument in favor of $V=L$: $V=L$ is to be preferred because it makes possible a deep and rich structure theory; we can say a lot about constructible sets, but very little about sets in general. Similarly, in algebra, the study of groups is richer and more productive than the study of arbitrary structures with one binary operation. The goal of mathematics, after all, is these strong structure theories, and $V=L$ is better suited to this goal. This general line of thought is not unknown in the history of mathematics, for example, it seems to have influenced Borel's rejection of arbitrary functions and D'Alembert's earlier rejection of non-differentiable functions.

Notice, first, that this style of argument is entirely naturalistic, that is, it is based on an assessment of the goals of mathematics and the most effective means of reaching them. Let's suppose, as seems plausible to me, that a case could be made in favor of this preference for strong structure theories. Would this additional maxim then count against $ZFC+\exists 0^\sharp$? The answer, it seems to me, is no, because the new isomorphism types gained by $ZFC+\exists 0^\sharp$ are gained, as it were, for free. In moving from $ZFC+V=L$ to $ZFC+\exists 0^\sharp$, we aren't losing anything, because we still have L itself; we can MAXIMIZE without sacrificing the strong structure theory of L . Similar responses could be given to Borel and D'Alembert.

Let me make one last remark about the virtues of this primitive argument against $V=L$ before I attempt to tightening it up. Our two methodological

¹⁴ There have been episodes in the history of mathematics when CONSISTENCY has been (temporarily) sacrificed, but given the motivating concern of axiomatic set theory with the issue of consistency, this is not a likely instance for such a sacrifice.

¹⁵ I have in mind the relative consistency results of Jensen and Solovay [1970], the extended successful work on inner models, and the ordinary inductive evidence that no one has yet derived a contradiction from $ZFC+\exists 0^\sharp$.

¹⁶ John Steel first recommended that I consider this case for contrast.

¹⁷ If A is a set (possibly non-well-founded) and R is a relation on A , then in $ZFC-F$, it can be shown that A is equinumerous with some ordinal α ; let f be a one-to-one correspondence between them. Let S be $\{ \langle f(x), f(y) \rangle \mid \langle x, y \rangle \in R \}$. Then $(A, R) \cong (\alpha, S)$, which is in V . So $(ZFC-F)+AFA$ adds no new isomorphism types. See McLarty [1993] for a related discussion.

maxims—UNIFY and MAXIMIZE—are in obvious tension. Given alternatives like $ZFC+V=L$ and $ZFC+\exists 0^\sharp$, the easiest way to MAXIMIZE would be to allow both theories, to use whichever theory turns out to be more useful in a given situation. But UNIFY counsels against this course. The beauty of this case is that it seems possible to UNIFY—that is, to choose between $ZFC+V=L$ and $ZFC+\exists 0^\sharp$ —while still MAXIMIZING, because the choice of $ZFC+\exists 0^\sharp$ doesn't require the sacrifice of any of the content of $ZFC+V=L$.

So far so good. But I suspect many will have noticed a serious gap in this reasoning: we want a criterion of restrictiveness that applies to theories, like $ZFC+V=L$, but our discussions have centered on a model, namely L . The same switch appeared in the AFA case, when we moved from talk of ZFC to talk of WF. The pressure to make such a switch is understandable, because we are concerned with isomorphism types, and these are realized in models, not theories. To compare theories in these terms, we've actually used interpretations of one theory in another: to interpret $ZFC+V=L$ in $ZFC+\exists 0^\sharp$, we replace a formula φ with φ^L , (that is, φ with quantifiers relativized by the requirement ' $x \in L$ '); to interpret ZFC in $(ZFC-F)+AFA$, we replace φ with φ^{WF} (quantifiers relativized to ' $x \in WF$ '). Without some further explanation, it seems this general style of argument could be turned on its head:¹⁸ consider the theory $ZFC+V=L$ +'there is a transitive model of " $ZFC+\exists 0^\sharp$ "'; call this theory T . Obviously, T proves there is a transitive model of $ZFC+\exists 0^\sharp$; let $\psi(x)$ say that x is in the $<_L$ -least such model. Then the replacement of φ by φ^ψ is an interpretation of $ZFC+\exists 0^\sharp$ in T . But T also proves that any transitive model of $ZFC+\exists 0^\sharp$ is missing some countable ordinals,¹⁹ and hence that there are isomorphism types not realized by anything satisfying ψ . What does MAXIMIZE now recommend?

Intuitively, what's wrong with this pro- $V=L$ line of reasoning is that an interpretation of $ZFC+\exists 0^\sharp$ using ψ is not comparable to the interpretation of $ZFC+V=L$ in L or the interpretation of ZFC in WF. The claim that we can UNIFY while MAXIMIZING in fact rests on the idea that interpreting $ZFC+V=L$ in L somehow 'preserves' that theory, in a way that the proposed interpretation of $ZFC+\exists 0^\sharp$ does not seem to preserve it. To make any progress here, we need some notion of a fair interpretation of one theory in another.

To get at the idea of 'preserving' or 'fairly interpreting' a theory T , consider for a moment the point of view of the T -theorist. Obviously, his intention is to give a theory of V itself. In the cases that interest us, the interpreting theorist will not agree to this, but perhaps the two can settle on an interpretation in a substantial approximation to V . In the history of set theory, two such approximations stand out: the first is Zermelo's 'normal domains' (Zermelo [1930]), now known as 'natural models' or 'standard complete models', that is, V_κ 's for κ inaccessible; the second is proper class inner models, beginning with Gödel's L . To speak crudely, the first compromises on 'tallness' and the second on 'thickness'.

¹⁸ This sort of argument, suggested by Tony Martin, was discussed in a different connection in [199?b].

¹⁹ ' $x=0^\sharp$ ' is Π_2^1 , so by Shoenfield's Absoluteness Theorem, it is absolute for transitive models containing all countable ordinals.

As a first stab at an account of what it is to provide a fair interpretation of a theory extending ZFC, I propose to focus on interpretations in natural or proper class inner models.

Formally, then, suppose that we are working in the language of set theory, that α is a variable that ranges over ordinals, and that φ is a formula with one free variable.

Definition 1 "*T shows φ is an inner model*" iff

- (i) for all σ in ZFC, $T \vdash \sigma^\varphi$, and
- (ii) $T \vdash \forall \alpha \varphi(\alpha)$ or $T \vdash \exists \kappa (\text{Innac}(\kappa) \wedge \forall \alpha (\alpha < \kappa \rightarrow \varphi(\alpha)))$, and
- (iii) $T \vdash \forall x \forall y ((x \in y \wedge \varphi(y)) \rightarrow \varphi(x))$.

This definition allows for natural models, proper class inner models, and truncations of proper class inner models at inaccessible levels (a simultaneous compromise on 'tallness' and 'thickness').

Definition 2 φ is a fair interpretation of T in T' (where T extends ZFC) iff

- (i) T' shows φ is an inner model, and
- (ii) for all $\sigma \in T$, $T' \vdash \sigma^\varphi$.

I've required that T extend ZFC because the fair interpretations all start as models of ZFC.

On this definition, there is a fair interpretation of ZFC+V=L in ZFC+ $\exists 0^\sharp$ (namely, $x \in L$), but there is no fair interpretation of ZFC+ $\exists 0^\sharp$ in ZFC+V=L (because ' $x=0^\sharp$ ' is absolute for fair interpretations). There is also a fair interpretation of ZFC in (ZFC-F)+AFA (namely, ' $x \in \text{WF}$ ').

The next step is to add the idea of providing new isomorphism types.

Definition 3 T' maximizes over T iff there is a φ such that

- (i) φ is a fair interpretation of T in T', and
- (ii) $T' \vdash \exists x \exists R \subseteq x^2 \forall y \forall S \subseteq y^2 ((\varphi(y) \wedge \varphi(S)) \rightarrow (x, R) \not\cong (y, S))$.

If T' also extends ZFC, then clause (ii) can be replaced by the simpler

- (ii)' $T' \vdash \exists x (\sim \varphi(x))$

because (in the presence of Foundation), an extra set is an extra isomorphism type. For good measure,

Definition 4 T' properly maximizes over T iff T' maximizes over T and T does not maximize over T'.

Using this definition, what we observed in the original, informal argument can be rephrased as: ZFC+ $\exists 0^\sharp$ properly maximizes over ZFC+V=L, but (ZFC-F)+AFA doesn't even maximize over ZFC.²⁰

²⁰ Our previous informal argument is not quite enough to establish this second claim, as the definition of fair interpretation now allows for (ZFC-F)+AFA to interpret ZFC in models other than WF. Given that $\text{Con}(\text{ZFC-F}) \rightarrow \text{Con}((\text{ZFC-F})+\text{AFA})$ (see Moschovakis [1994], pp. 259- 262), (ZFC-F)+AFA cannot prove the existence of an

At this point, it's tempting to call a theory 'restrictive' when there is a (probably consistent) theory that properly maximizes over it. But this isn't quite right. For example, ZFC+'there is an inaccessible' properly maximizes over ZFC, but we wouldn't want to call ZFC restrictive on that account, because ZFC can be extended to ZFC+'there is an inaccessible'. It seems reasonable to say that ZFC doesn't go as far as it might, but not to say that it restricts. For T to be restrictive, the maximizing theory, T', should at least contradict T. So,

Definition 5 *definition: T' inconsistently maximizes over T iff T properly maximizes over T and T' is inconsistent with T.*

In fact, even this isn't quite enough. To see this, consider the supposition that there are arbitrarily large measurable cardinals:

$$T = \text{ZFC} + \forall \alpha \exists x (\text{MC}(x) \wedge x > \alpha)$$

This surely seems a generous theory, one whose fair interpretations ought to realize lots of isomorphism types, but it can be maximized over by a theory that seems to restrict:²¹

$$T' = \text{ZFC} + \exists x (\text{Inacc}(x) \wedge x = \sup\{y \mid \text{MC}(y)\})$$

It would surely be counterintuitive to classify T as restrictive on the grounds of T', and I think the reason can be made clear in the present terms. T' provides something that T cannot prove to exist, namely, a set of measurable cardinals with an inaccessible, but not measurable supremum. In that sense, T' is richer than T. But nothing in T precludes what T' provides; that is, T can be extended to:

$$T'' = T + \exists x (\forall y (y \in x \rightarrow \text{MC}(y) \wedge \text{Inacc}(\sup(x)) \wedge \sim \text{MC}(\sup(x))))$$

Now T'' maximizes over T', which suggests, once again, that though T doesn't go as far as it might, it also doesn't restrict. The term 'restrictive' ought to be reserved for theories that actually rule out a certain line of development. So,

Definition 6 *T' strongly maximizes over T iff*

- (i) *T' inconsistently maximizes over T, and*
- (ii) *there is no consistent T'' extending T that properly maximizes over T'.*

Obviously, allowing inconsistent T'' would void the definition. In most interesting cases, we won't be able to prove the consistency of a candidate for T'', so the conclusion to be drawn will be that whatever evidence we have for the consistency of a candidate T'' is also evidence that T' doesn't strongly maximize over T.

Finally, the proposal is:

inaccessible. Now suppose (ZFC-F)+AFA interprets ZFC in L rather than WF. The argument of footnote 16 then shows that (A,R) is isomorphic to a set in WF. If (ZFC-F)+AFA could prove that this set is not in L, it could prove the existence of a non-constructible, well-founded set. To see that (ZFC-F)+AFA cannot do this, modify Moschovakis's construction by beginning from L rather than WF; the result will be a model of (ZFC-F)+AFA+WF=L.

²¹ This example is also due to Tony Martin.

Definition 7 *T* is restrictive iff there is a consistent *T'* that strongly maximizes over *T*.

Once again, we will often be unable to prove the consistency of a candidate *T'*, but evidence for its consistency will be evidence for the restrictiveness of *T*. In our case, we know that no extension of $ZFC+V=L$ will allow a fair interpretation of $ZFC+\exists 0^\sharp$,²² so we can be as confident that it establishes the restrictiveness of $ZFC+V=L$ as we are of the consistency of $ZFC+\exists 0^\sharp$. But we can do better than this by observing that we have the best possible evidence that $ZFC+V\neq L$ is consistent (it is consistent if ZFC is), and thus, even better grounds on which to conclude that $ZFC+V=L$ is restrictive.

In these terms, we can state clear grounds for claiming that $ZFC+V=L$ is restrictive. Furthermore, MAXIMIZE will, at the very least, tell us that restrictive theories should be rejected, so we can also state clear grounds for the consensus against $V=L$. We are left with a wide range of theories that strongly maximize over $ZFC+V=L$, including $ZFC+V\neq L$, $ZFC+\exists 0^\sharp$, $ZFC+MC$, and for that matter, such duds as $ZFC+V\neq L+\sim\text{Con}(ZFC)$.²³ The next step would be to examine the rational grounds for a choice between these.

4 What goes wrong

So, that's the argument that I would like to claim provides a rational grounding for the view that $V=L$ should be rejected. The fact that it goes wrong in more complicated cases may well not surprise those long accustomed to the endless richness and variety of set theory. I suspect this very richness and variety partly accounts for the enduring fascination of set theory over the past 100 years.

In any case, what's gradually emerged is that the proposed criterion for restrictiveness produces both false negatives (theories that seem restrictive, but are not so classified) and false positives (theories that don't seem restrictive, but are so classified). What I'll be reporting is actually just the current state of play in ongoing discussions I've had with several much more knowledgeable persons, especially Tony Martin, John Steel and Sarah Resnikoff.

Let me start with the false negatives. After $V=L$, it is natural to ask what the criterion has to say about various theories of the form $ZFC+'V=\text{the canonical inner model with such-and-such large cardinals}'$, the expectation being that they will be strongly maximized over by theories of the form $ZFC+'the next larger large cardinal exists'$. In fact, this pattern is satisfied for a considerable distance. For example, $ZFC+'there are two measurable cardinals'$ ($ZFC+2MC$) provides a fair interpretation of $ZFC+'V=\text{the canonical inner model with one measurable cardinal}'$ ($ZFC+V=L[U]$). Furthermore, $ZFC+V=L[U]$ implies that there is no (uncountable) inner model of $ZFC+2MC$, so neither it nor any extension of

²² In any extension of $ZFC+V=L$, the only candidates for fair interpretations of $ZFC+\exists 0^\sharp$ are L and models of the form L_κ for κ inaccessible. Neither of these could think $\exists 0^\sharp$, or even $V\neq L$.

²³ Thanks to John Steel for bringing this one up.

it can fairly interpret $ZFC+2MC$. Thus, $ZFC+2MC$ strongly maximizes over $ZFC+V=L[U]$, and the later is restrictive, as seems right.

But, as Steel has noted, trouble arises at the level of two Woodin cardinals, at $ZFC+V=L[E]$, where $L[E]$ is the canonical inner model with two Woodins. Surprisingly, this theory can be extended to provide definable proper class inner models for such theories as $ZFC+$ 'there are five Woodins', $ZFC+$ 'there is a supercompact', and so on.²⁴ Thus, on our definitions, none of these theories strongly maximizes over $ZFC+V=L[E]$. This seems wrong, perhaps for reasons along the following lines: the inner models that the extensions of $ZFC+V=L[E]$ provide for the larger large cardinals are not 'canonical' (e.g. they are not 'iterable'), so to interpret a theory like $ZFC+$ 'there are five Woodin cardinals' in this way is not to 'preserve' it or to interpret it 'without loss'. This example suggests that defining 'fair interpretation' as interpretation in a mere inner model is too weak, a weakness that was masked in the case of $V=L$ because the so-called 'fair' interpretation of $V=L$ in L is, in fact, optimal.

So, it seems that any more worthy successor to the argument proposed here would need a better notion of 'fair interpretation'. I have nothing to offer at this moment, but Martin points out that the prospect of strengthening this definition raises another interesting possibility. Recall that T' maximizes over T if T' provides a fair interpretation of T and an isomorphism type not represented in that fair interpretation. The isomorphism type provision is redundant whenever T' includes Foundation and T' is inconsistent with T , that is to say, in most of the cases that interest us. The only case in which it's done any real work is that of $(ZFC-F)+AFA$. But if 'fair interpretation' were redefined so as to allow for a fair interpretation of $(ZFC-F)+AFA$ in ZFC – which doesn't seem so far-fetched, given that our best understanding of models of AFA is already based on interpretations in terms of ordinary set theoretic graphs – then the extra clause would be doing no work at all and could be eliminated. The connection back to MAXIMIZE would then be simplified: instead of singling out a particular thing we want to maximize—*isomorphism types*—we could rest with the simple thought that if T' fairly interprets T , in the new robust sense, then whatever T provides, T' will preserve. So a better notion of fair interpretation might simplify the over- all argument considerably.

Let's turn now to the false positives. Again, the example is due to Steel. Start, this time, with $ZFC+$ 'there is a measurable cardinal' ($ZFC+MC$) and consider the following theory, T :

$$ZFC+\exists 0^\dagger+\forall\alpha < \omega_1 L_\alpha[0^\dagger] \not\models ZFC (ZFC+\sigma).$$

0^\dagger codes the canonical inner model of $ZFC+MC$, so T provides a truly fair interpretation of $ZFC+MC$. On the other hand, MC implies that if 0^\dagger exists,

²⁴ The trick, as it's been explained to me, is that the canonical model with two Woodins is Σ_4^1 -correct, and inner models of the larger large cardinals can be coded by Δ_4^1 reals. The relevant extension of $ZFC+V=L[E]$ simply adds the claim that the larger large cardinal axiom is true relativized to the formula defining the appropriate inner model.

then there is a countable α such that $L_\alpha[0^\dagger] \models ZFC$,²⁵ that is, $ZFC+MC \vdash \sim \sigma$. Now suppose $ZFC+MC$ has an extension, T' , with a fair interpretation of T . Then T' proves there is an inner model $M \models T$, so in particular, $M \models \sigma$. σ is Σ_3^1 , and thus absolute upwards for models like M , so $T' \vdash \sigma$. But $ZFC+MC \subseteq T'$, so $T' \vdash \sim \sigma$, too. Thus, T' is inconsistent. It follows that T strongly maximizes over $ZFC+MC$, and thus, that $ZFC+MC$ is restrictive. I suppose most people would consider this a misclassification.

The diagnosis of what has gone wrong in this case involves a careful look at the theory T . T is a way of saying that 0^\dagger exists, but is not contained in any transitive set model of ZFC .²⁶ This isn't as strong as saying that $ZFC+\exists 0^\dagger$ is inconsistent, but it is in the same ballpark. Granted, $ZFC+MC$ can't be extended to provide a fair interpretation of this theory, but should it? This line of thought suggests that some theories, perhaps including T , are so poorly constructed that they don't deserve to be fairly interpreted; if so, another theory's failure to do so would not count against it.

Looking back, we realize that among the theories listed as strongly maximizing over $ZFC+V=L$ was $ZFC+V \neq L + \sim \text{Con}(ZFC)$. This didn't cause any discomfort in the case of $V=L$ because there were also other, more attractive theories strongly maximizing over it; at the time, I simply referred to $ZFC+V \neq L + \sim \text{Con}(ZFC)$ as a 'dud' that appears among the range of these theories, presumably one that would not be in the running (for good reasons) when we set out to choose between them. But what if the only theories strongly maximizing over a given theory are duds? Would it still seem reasonable to classify it as restrictive?

Of course, the trick is to specify what counts as a dud and why. I don't have anything worked out to say about this, though I will come back to the problem again in a moment. However, I think it is worth remarking that we need not require that such an account be fully formalizable in set theory. Though the criterion for restrictiveness we have been considering so far is a formal one, the arguments for MAXIMIZE and UNIFY are not, and it seems to me quite unlikely that any complete theory of the various forces that influence theory choice in set theory will be fully formalizable. The point is that an argument can be sound, rational, and legitimately compelling without being formal.

In any case, the false positives I know of do seem to rest on theories that deserve to be called 'duds', for reasons that must for now remain unspecified. But before leaving the topic of unattractive theories entirely, I'd like to call attention to a more subtle worry that concerns me even in the central case in which the criterion does work, that is, in the case of $V=L$. Recall that another of the theories that strongly maximizes over $ZFC+V=L$ is the simple $ZFC+V \neq L$. This

²⁵ If κ is a measurable cardinal and 0^\dagger exists, then V_κ is a model of ZFC to which 0^\dagger belongs. This model has a submodel of the form $L_\kappa[0^\dagger]$ which is also a model of ZFC . $L_\kappa[0^\dagger]$, in turn, has a countable, transitive, elementary submodel containing 0^\dagger . This last must be of the form $L_\alpha[0^\dagger]$ for some countable α .

²⁶ By essentially the argument of the previous note, if 0^\dagger is contained in some transitive set model of ZFC , it is contained in one of the form $L_\alpha[0^\dagger]$ for some countable α .

is surely not a dud in the sense of theories like $ZFC + \sim \text{Con}(ZFC)$, but it is an unattractive theory, nevertheless. It is too weak to settle any of the outstanding open questions; though it does, strictly speaking, provide a new isomorphism type, it tells us nothing about that type, which makes it pretty much unusable. It is as if a physicist were to conclude that the known elementary particles—A, B, and C—do not exhaust the range of such particles, and then propose as a new physical theory the claim 'There are particles that are not of types A, B or C'. Not a good physical theory. Nor is $ZFC + V \neq L$ a good set theory.

This observation leads to an interesting quasi-historical question: when Scott ([1961]) proved that the existence of a measurable cardinal implies the falsity of $V=L$, did this increase confidence in measurable cardinals or decrease confidence in $V=L$? If $V=L$ was justifiably regarded as restrictive at that time, simply due to the unattractive $ZFC + V \neq L$, we would expect Scott's proof to increase the confidence in MC. If, on the other hand, the weakling $ZFC + V \neq L$ is not enough to establish the restrictiveness of $V=L$, then we would expect Scott's proof to decrease confidence in $V=L$, by showing that there is something of interest that is ruled out by it. On this second line of thought, for a theory to count as truly restrictive, the theory maximizing over it must have some attractions of its own: it must imply the existence of an isomorphism type about which something interesting is known, or more generally, it must have other virtues, perhaps linked to other goals of set theoretic practice.

In fact, it seems more plausible to suppose that the evidence of Scott's theorem would do both things at once. On the one hand, $V=L$ was already under legitimate suspicion for its association with Definabilism, so implying its negation would be a plus for MC. On the other hand, MC was already known to enjoy other virtues; e.g., it implies the existence of small large cardinals, which can be regarded as efforts to extend Cantor's successful methods. On these grounds, its clash with MC would count against $V=L$. So the success of MC and the failure of $V=L$ appear intertwined, and more important, considerations other than mere restrictiveness seem to be involved.

But whatever the best analysis of this episode, I think it might well be reasonable to require that the maximizing theory have independent virtues before classifying the maximized theory as restrictive. Again, I would expect these virtues to be linked to other identifiable goals of set theory, e.g., that of providing a rich theory of sets of real numbers, or of extending Cantor's original methods. If a modification of our account of 'maximizing' along these lines proved feasible and desirable, it might well solve the problem of dud theories along the way; presumably, they would not be particularly virtuous. But this is mere speculation.

In sum, then, if we hope for an account of restrictiveness along the proposed lines, we face difficulties with spelling out both aspects of the primitive underlying argument. On the one hand, we need a more robust notion of 'fair interpretation'; we have not fully captured the idea of 'preserving' the maximized theory 'without loss'. On the other hand, we also need a more discerning approach to the idea of a theory 'giving more'; to do this, a theory must be more worthy than those we've called 'duds' and perhaps it must have independent

virtues of its own. I have no clear sense of the prospects for these improvements, though I seriously doubt they will be easy; but I retain the conviction that the consensus against $V=L$ is rationally justified, and that the grounds can be clearly articulated, if perhaps not fully formalized. In any case, I hope it goes without saying that I would welcome your reflections on this or any other line of thought on these matters.²⁷

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