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INDISPENSABILITY AND PRACTICE*

For some time now, philosophical thinking about mathematics has been profoundly influenced by arguments based on its applications in natural science, the so-called "indispensability arguments." The general idea traces back at least to Gottlob Frege,[1] but contemporary versions stem from the writings of W. V. Quine,[2] and later, Hilary Putnam.[3] Much contemporary philosophy of mathematics (including my own) operates within the parameters of the indispensability arguments; they are called upon to motivate various versions of nominalism, as well as to support various versions of realism. Still, attention to practice, both scientific and mathematical, has recently led me to doubt their efficacy. I shall try to explain these doubts in what follows. If they are legitimate, we will be forced to rethink much of current orthodoxy in the philosophy of mathematics.

I. NATURALISTIC BACKGROUND

An argument based on scientific and mathematical practice can only succeed from a sufficiently naturalistic perspective. Familiar Quinean naturalism counsels us to reject prescientific first philosophy in favor of an approach that begins within our current best scientific theory:

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[1] "... it is applicability alone which elevates arithmetic from a game to the rank of a science," P. Geach and M. Black, eds., *Translations from the Philosophical Writings of Gottlob Frege* (Cambridge: Blackwell, 1970), p. 187. Although Frege speaks of arithmetic, my focus here will be on analysis, broadly construed.

[2] See, e.g., "On What There Is," repr. in *From a Logical Point of View,* 2nd ed. (Cambridge: Harvard, 1980), pp. 1–19; and "Carnap and Logical Truth," repr. in *The Ways of Paradox,* rev. ed. (Cambridge: Harvard, 1976), pp. 107–32.

[3] See, e.g., "What Is Mathematical Truth?" and "Philosophy of Logic," repr. in *Mathematics, Matter and Method,* 2nd ed. (New York: Cambridge, 1979), pp. 60–78, 323–57.

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> . . . naturalism: abandonment of the goal of a first philosophy. It 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 . . . The naturalistic philosopher begins his reasoning within the inherited world theory as a going concern. He tentatively believes all of it, but believes also that some unidentified portions are wrong. He tries to improve, clarify, and understand the system from within. He is the busy sailor adrift on Neurath's boat.[4]

Epistemological studies, in particular, are to be carried out within science, with the help of relevant psychological theories.

From the perspective of this scientific naturalism, a philosopher can criticize scientific practice, but only on scientific grounds, as a scientist might do, for good scientific reasons. This is enough to ratify an appeal to scientific practice in philosophical contexts: because scientific practice can only be questioned on scientific grounds, a conflict between scientific practice and philosophy must be resolved by revising the philosophy. So, for example, if scientific practice holds that p does or does not count as evidence for q, to disagree on philosophical grounds is an offense against naturalism.

As we shall see, however, it is not clear that Quine intends to extend this naturalistic faith in practice to the practice of mathematics. Leaving Quine aside for the moment, we must ask ourselves what the role of the philosophy of mathematics should be. Mathematics, after all, is an immensely successful enterprise in its own right, older, in fact, than experimental natural science. As such, it surely deserves a philosophical effort to understand it as practiced, as a going concern. Indeed, as in any discipline, there remain conceptual confusions in mathematics that might be clarified by philosophical analysis, providing that analysis is sensitive to the realities of actual mathematics. If it is to serve these purposes, a philosophical account of mathematics must not disregard the evidential relations of practice or recommend reforms on nonmathematical grounds.

These are, in my view, proper goals for the philosophy of mathematics. We, as philosophers of mathematics, should provide an account of mathematics as practiced, and we should make a contribution to unraveling the conceptual confusions of contemporary mathematics. So it is against this backdrop that I shall assess the indispensability arguments, or rather, the view of mathematics the indispensability arguments generate. They will be judged by their ability to account for actual mathematics as practiced.

[4] Quine, "Five Milestones of Empiricism," in *Theories and Things* (Cambridge: Harvard, 1981), pp. 67–72; here p. 72.

II. INDISPENSABILITY

The original indispensability arguments were aimed at those who would draw a weighty ontological or epistemological distinction between natural science and mathematics. To those tempted to admit the existence of electrons while denying the existence of numbers, Quine[5] points out that

> Ordinary interpreted scientific discourse is as irredeemably committed to abstract objects—to nations, species, numbers, functions, sets—as it is to apples and other bodies. All these things figure as values of the variables in our overall system of the world. The numbers and functions contribute just as genuinely to physical theory as do hypothetical particles (*ibid.*, pp. 149–50).

But, Quine's opponent insists, the scientific hypotheses in our theory are tested by experiment, and the mathematical ones are not; surely the two can be distinguished on these grounds. To which Quine replies,

> The situation may seem to be saved, for ordinary hypotheses in natural science, by there being some indirect but eventual confrontation with empirical data. However, this confrontation can be remote; and, conversely, some such remote confrontation with experience may be claimed even for pure mathematics and elementary logic. The semblance of a difference in this respect is largely due to overemphasis of departmental boundaries. For a self-contained theory which we can check with experience includes, in point of fact, not only its various theoretical hypotheses of so-called natural science but also such portions of logic and mathematics as it makes use of.[6]

Thus, the applied mathematics is confirmed along with the physical theory in which it figures.

Of course, it is not enough for a piece of mathematics simply to appear in a confirmed scientific theory. For any theory *T*, there is another theory *T′* just like *T* except that *T′* posits a bunch of new particles designed to have no affect on the phenomena *T* predicts. Any experiment confirming *T* under these circumstances would also (in some sense) confirm *T′*, but we do not take this as evidence for the existence of the new particles because they are "dispensable," i.e., there is an equally good, indeed better theory of the same phenomena, namely, *T*, that does not postulate them. The mathematical apparatus of modern physics does not seem to be dispensable in this way; indeed, Putnam has emphasized that many physical hypotheses

[5] "Success and Limits of Mathematization," in *Theories and Things*, pp. 148–55.

[6] "Carnap and Logical Truth," p. 121.

cannot even been stated without reference to numbers, functions, etc.[7]

So a simple indispensability argument for the existence of mathematical entities goes like this: we have good reason to believe our best scientific theories, and mathematical entities are indispensable to those theories, so we have good reason to believe in mathematical entities. Mathematics is thus on an ontological par with natural science. Furthermore, the evidence that confirms scientific theories also confirms the required mathematics, so mathematics and natural science are on an epistemological par as well.

Unfortunately, there is a prima facie difficulty reconciling this view of mathematics with mathematical practice.[8] We are told we have good reason to believe in mathematical entities because they play an indispensable role in physical science, but what about mathematical entities that do not, at least to date, figure in applications? Some of these are admissible, Quine[9] tells us,

> . . . insofar as they come of a simplificatory rounding out, but anything further is on a par rather with uninterpreted systems (*ibid.*, p. 788).

So in particular,

> I recognize indenumerable infinites only because they are forced on me by the simplest known systematizations of more welcome matters. Magnitudes in excess of such demands, e.g. $\beth_\omega$ or inaccessible numbers, I look upon only as mathematical recreation and without ontological rights.[10]

The support of the simple indispensability argument extends to mathematical entities actually employed in science, and only a bit beyond.

The trouble is that this does not square with the actual mathematical attitude toward unapplied mathematics. Set theorists appeal to various sorts of nondemonstrative arguments in support of their customary axioms, and these logically imply the existence of $\beth_\omega$. Inac-

[7] Hartry Field disputes this in his *Science without Numbers* (Princeton: University Press, 1980), and *Realism, Mathematics and Modality* (Cambridge: Blackwell, 1989), but the copious secondary literature remains unconvinced.

[8] Versions of this concern appear in C. Chihara, *Constructibility and Mathematical Existence* (New York: Oxford, 1990), p. 15; and in my *Realism in Mathematics* (New York: Oxford, 1990), pp. 30–1.

[9] "Review of Charles Parsons's *Mathematics in Philosophy*," this JOURNAL, LXXXI, 12 (December 1984): 783–94.

[10] Quine, "Reply to Charles Parsons," in *The Philosophy of W. V. Quine*, L. Hahn and P. Schilpp, eds. (La Salle, IL: Open Court, 1986), pp. 396–403; here p. 400.

cessibles are not guaranteed by the axioms, but evidence is cited on their behalf nevertheless. If mathematics is understood purely on the basis of the simple indispensability argument, these mathematical evidential methods no longer count as legitimate supports; what matters is applicability alone. Here simple indispensability theory rejects accepted mathematical practices on nonmathematical grounds, thus ruling itself out as the desired philosophical account of mathematics as practiced.

So simple indispensability will not do, if we are to remain faithful to mathematical practice. We insist on remaining faithful to mathematical practice because we earlier endorsed a brand of naturalism that includes mathematics. But it is worth noting that even a retreat to purely nonmathematical naturalism (forgetting our commitment to actual mathematical practice) will not entirely solve this problem. From the point of view of science-only naturalism, the applied part of mathematics is admitted as a part of science, as a legitimate plank in Neurath's boat; unapplied mathematics is ignored as unscientific. But even for applied mathematics there is a clash with practice. Mathematicians believe the theorems of number theory and analysis not to the extent that they are useful in applications but insofar as they are provable from the appropriate axioms. To support the adoption of these axioms, number theorists and analysts may appeal to mathematical intuition, or the elegant systematization of mathematical practice, or other intramathematical considerations, but they are unlikely to cite successful applications. So the trouble is not just that the simple indispensability argument shortchanges unapplied mathematics; it also misrepresents the methodological realities of the mathematics that is applied.

There is, however, a modified approach to indispensability considerations which gets around this difficulty. So far, on the simple approach, we have been assuming that the indispensability of (some) mathematical entities in well-confirmed natural science provides both the justification for admitting those mathematical things into our ontology and the proper methodology for their investigation. But perhaps these two—ontological justification and proper method—can be separated. We could argue, first, on the purely ontological front, that the successful application of mathematics gives us good reason to believe that there are mathematical things. Then, given that mathematical things exist, we ask: By what methods can we best determine precisely what mathematical things there are and what properties these things enjoy? To this, our experience to date resoundingly answers: by mathematical methods, the very methods mathematicians use; these methods have effectively produced all of mathematics, including the part so far applied in physical science.

From this point of view, a modified indispensability argument first guarantees that mathematics has a proper ontology, then endorses (in a tentative, naturalistic spirit) its actual methods for investigating that ontology. For example, the calculus is indispensable in physics; the set-theoretic continuum provides our best account of the calculus; indispensability thus justifies our belief in the set-theoretic continuum, and so, in the set-theoretic methods that generate it; examined and extended in mathematically justifiable ways, this yields Zermelo-Fraenkel set theory. Given its power, this modified indispensability theory of mathematics stands a good chance of squaring with practice, so it will be preferred in what follows.[11]

III. THE SCIENTIFIC PRACTICE OBJECTION

My first reservation about indispensability theory stems from some fairly commonplace observations about the practice of natural science, especially physics. The indispensability argument speaks of a scientific theory *T*, well-confirmed by appropriate means and seamless, all parts on an ontological and epistemic par. This seamlessness is essential to guaranteeing that empirical confirmation applies to the mathematics as well as the physics, or better, to the mathematized physics as well as the unmathematized physics. Quine's[12] vivid phrases are well-known: "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (*ibid.*, p. 41).

Logically speaking, this holistic doctrine is unassailable, but the actual practice of science presents a very different picture. Historically, we find a wide range of attitudes toward the components of well-confirmed theories, from belief to grudging tolerance to outright rejection. For example, though atomic theory was well-confirmed by almost any philosopher's standard as early as 1860, some scientists remained skeptical until the turn of the century—when certain ingenious experiments provided so-called "direct verification"—and even the supporters of atoms felt this early skepticism to be scientifically justified.[13] This is not to say that the skeptics neces-

[11] This is more or less the position of my *Realism in Mathematics*.

[12] "Two Dogmas of Empiricism," repr. in *From a Logical Point of View*, pp. 20–46.

[13] I trace the history in some detail in "Taking Naturalism Seriously," in *Proceedings of the 9th International Congress of Logic, Methodology and Philosophy of Science*, D. Prawitz, B. Skyrms, and D. Westerstahl, eds. (Amsterdam: North Holland, forthcoming).

sarily recommended the removal of atoms from, say, chemical theory; they did, however, hold that only the directly verifiable consequences of atomic theory should be believed, whatever the explanatory power or the fruitfulness or the systematic advantages of thinking in terms of atoms. In other words, the confirmation provided by experimental success extended only so far into the atomic-based chemical theory *T*, not to the point of confirming its statements about the existence of atoms. This episode provides no comfort to the van Fraassenite, because the existence of atoms was eventually established, but it does show scientists requiring more of a theory than the sort of theoretical virtues typically discussed by philosophers.

Some philosophers might be tempted to discount this behavior of actual scientists on the grounds that experimental confirmation is enough, but such a move is not open to the naturalist. If we remain true to our naturalistic principles, we must allow a distinction to be drawn between parts of a theory that are true and parts that are merely useful. We must even allow that the merely useful parts might in fact be indispensable, in the sense that no equally good theory of the same phenomena does without them. Granting all this, the indispensability of mathematics in well-confirmed scientific theories no longer serves to establish its truth.

But perhaps a closer look at particular theories will reveal that the actual role of the mathematics we care about always falls within the true elements rather than the merely useful elements; perhaps the indispensability arguments can be revived in this way. Alas, a glance at any freshman physics text will disappoint this notion. Its pages are littered with applications of mathematics that are expressly understood not to be literally true: e.g., the analysis of water waves by assuming the water to be infinitely deep or the treatment of matter as continuous in fluid dynamics or the representation of energy as a continuously varying quantity. Notice that this merely useful mathematics is still indispensable; without these (false) assumptions, the theory becomes unworkable.

It might be objected that these applications are peripheral, that they are understood against the background of more fundamental theories, and that it is in contrast with these that the applications mentioned above are "idealizations," "models," "approximations," or useful falsehoods. For example, general relativity is a fundamental theory, and when space-time is described as continuous therein, this is not explicitly regarded as less than literally true. So the argument goes.

But notice, when those pre-Einsteinians were skeptical of atomic theory, it was a fundamental theory in this sense; it was not proposed against another background as a convenient idealization or mere approximation. The skeptics were bothered, not by such peripheral simplifications, but by what they saw as the impossibility of directly testing the core hypotheses of atomic theory. But consider now the hypothesis that space-time is continuous. Has this been directly tested? As Quine himself points out, "no measurement could be too accurate to be accommodated by a rational number, but we admit the [irrationals] to simplify our computations and generalizations."[14] Similarly, space-time must be regarded as continuous so that the highly efficacious continuum mathematics can be applied to it. But the key question is this: Is that continuous character "experimentally verified" or merely useful? If it is merely useful, then the indispensability argument sketched earlier, the one relying on the role of continuum mathematics in science to support the Zermelo-Fraenkel axioms, or ZFC, cannot be considered conclusive.

I shall not try to answer this question here; to do so would require a more thorough study of the physics literature than I am capable of launching just now. But until such a study is undertaken, until the evidence for the literal continuity of space-time is critically examined,[15] I think the simple observations collected here are enough to raise a serious question about the efficacy of this particular indispensability argument.

IV. FOUNDATIONS OF SET THEORY

My second reservation about the indispensability arguments rests on somewhat less familiar grounds; to reach it, I must review a bit of set theory and (in the next section) a bit of physics.

It is well-known that the standard axioms of contemporary set theory, ZFC, are not enough to decide every naturally-arising set theoretical question.[16] The most famous independent statement is the Cantor's continuum hypothesis (CH), but there are others, some of them more down-to-earth than CH. For example, between the mid-seventeenth and the late nineteenth century, under pressure from both physical and mathematical problems, the notion of func-

[14] "Reply to Charles Parsons," p. 400.

[15] This issue of the continuity of space-time will take an unexpected turn in sect. V.

[16] Kurt Gödel's incompleteness theorem is enough to establish that there are set-theoretic statements that can neither be proved nor disproved from ZFC, but Gödel's later work on the inner model of constructible sets and Paul Cohen's forcing methods yield more: there are statements *which mathematicians have found it natural to ask* which are likewise independent.

tion became more and more general. Around the beginning of our century, various mathematicians undertook to bring order to the wild domain of discontinuous functions. It soon became clear that the complexity of functions could be understood in terms of the complexity of sets of real numbers—e.g., a function is continuous iff the inverse image of every open set is open—and this naturally led to a serious study of the properties of definable sets of reals. Among these, the Borel sets could easily be shown, for example, to be Lebesgue measurable. This result generalized to projections[17] of Borel sets (the analytic sets) and to the complements of these (the coanalytic sets). One more application of projection produces the sets we now call Σ^1_2, but the question of their Lebesgue measurability remained stubbornly unsolved. Sometime later, this question was shown to be independent of ZFC.[18]

In contrast with CH, this question concerns only a limited class of definable sets of reals, sets whose definitions have concrete geometric interpretations, and it involves the intuitive analytic notion of Lebesgue measurability rather than Cantor's bold new invention, the comparison of infinite cardinalities. In other words, it might be said that this independent question, unlike CH, arose in the straightforward pursuit of analysis-as-usual. And there are others of this type.

There is a serious foundational debate about the status of these statements. Despite their independence of ZFC, one might hold that there is nevertheless a fact of the matter, that the statements are nevertheless either true or false, and that it is the burden of further theorizing to determine which.[19] At the other extreme, another might insist that ZFC is all there is to set theory, that a statement independent of these axioms has no inherent truth value, that the study of extensions of ZFC that settle these questions one way or the other are all equally legitimate. For future reference, let me attach labels to crude versions of these positions: let *fact* be the bare assumption that there is a determinate answer to our question, and let the opposing view be *no-fact*.

Now let us pose this foundational question to the indispensability theorist, taking the simple version of indispensability first, for pur-

[17] The projection of a subset of the plane is its shadow on one of the coordinate axes.

[18] I discuss this history in more detail in ch. 4 of *Realism in Mathematics* and in "Taking Naturalism Seriously."

[19] This was Gödel's view, and the one defended in *Realism in Mathematics*.

poses of comparison.[20] Continuum mathematics, including everything from real-valued measurement to the higher calculus, is among the most widely applied branches of mathematics, and at least some of the many physical theories in which it is applied are extremely well-confirmed. Therefore, so the argument goes, we have good reason to believe in the entities of continuum mathematics, for example, the real numbers. In these applications—for example, in the theory of space (or space-time)—we also find quantification over sets of reals, (or equivalently, over regions of space (or space-time)), though particular instances are rarely as complex as Σ^1_2.[21] If we believe in the reals and in those sets of reals definable in our theory, then it seems we should accept the legitimacy of the question: Are Σ^1_2 sets Lebesgue measurable?[22] Thus the simple indispensability theorist should endorse *fact*.

What follows from this for the practice of set theory? Should the set theorist, as Gödel suggests, seek an answer to this legitimate question? Given that our independent question seems, for now, to have no bearing on physical theory, and that it is not settled by the most generous of "simplificatory rounding outs" (i.e., ZFC), the simple indispensability theorist, who uses only the justificatory methods of physical science, has no means for answering it. Furthermore, the exclusive focus on the needs and methods of physical science hints at a lack of interest in any question without physical ramifications. If so, the simple theorist may disagree with *no-fact*, classifying our independent question as one with an unambiguous truth value, without going so far as charging future theorists with the task of answering it. Call this *weak fact*.

Fortunately, these subtle matters of interpretation are beside the point here, because we have already identified the modified indispensability argument as more promising than the simple. Like the simple theorist, the modified indispensability theorist embraces the ontology of continuum mathematics, on the basis of its successful applicability, and thus the legitimacy of our independent question, but she goes beyond the simple theorist by ratifying the set theorist's

[20] For now, I shall ignore the scientific practice objection and take fundamental scientific theory at face value.

[21] One theoretical proposal that involves nonmeasurable sets of reals is I. Pitowsky, "Deterministic Model of Spin and Statistics," *Physical Review D*, XXVII, 10 (15 May 1983): 2316–26. (M. van Lambalgen called this paper to my attention.)

[22] This might be avoided if we took mathematical entities to be somehow "incomplete," an idea of Parsons's which Quine considers in passing. See Quine's "Reply to Charles Parsons," p. 401, and references cited there.

search for new axioms to answer the question. Call this *strong fact*. Although the evidence for or against these axiom candidates will derive, not from physical applications, but from considerations internal to mathematics, the modified theorist sees the past success of such mathematical methods as justifying their continued use.

Either way, then, the indispensability theorist should adopt some version of *fact*. Notice, however, that this acceptance of the legitimacy of our independent question and (for the modified theorist) the legitimacy of its pursuit is not unconditional; it depends on the empirical facts of current science. The resulting mathematical beliefs are likewise a posteriori and fallible.

V. FOUNDATIONS OF PHYSICS

Here the fundamental theories are general relativity and quantum mechanics, and the central problem is to reconcile the two. Three of the fundamental forces—electromagnetic, weak, and strong—have yielded more or less workable quantum-field descriptions, but gravity remains intractable. The mathematics of quantum-field theories has an annoying habit of generating impossible (infinite) values for some physical magnitudes, but the problem has been overcome by a technical trick called "renormalization" in all cases but that of gravity. So the problem remains: How to characterize the gravitational force at quantum distances?

Physicists engage in fascinating speculations on the source of the difficulty: for example, that it arises from the attempt to combine the essentially passive space-time of quantum theory with the dynamic space-time of general relativity.[23] But most important for our purposes is the idea that the fault might lie in our conception of space-time as a mathematical continuum. For example, Richard Feynman[24] writes:

> I believe that the theory that space is continuous is wrong, because we get these infinities and other difficulties . . . (*ibid.*, p. 166).

And Chris Isham:

> . . . it is clear that quantum gravity, with its natural Planck length, raises the possibility that the continuum nature of spacetime may not hold below this length, and that a quite different model is needed (*op. cit.*, p. 72).

[23] See Chris Isham, "Quantum Gravity," in *The New Physics*, P. Davies, ed. (New York: Cambridge, 1989), pp. 70–93, esp. p. 70. I review some of the popular literature on this problem in "Taking Naturalism Seriously."

[24] *The Character of Physical Law* (Cambridge: MIT, 1967).

Here the suggestion is not (as in the previous section) that the continuity of space-time is a "mere idealization," but that it does not belong in our best theory at all!

All this, as I have indicated, is quite speculative; no one yet knows what a reasonable theory of quantum gravity might be like. But the very suggestion that space-time may not be continuous is enough to add previously unimagined poignancy to our earlier conclusion of a posteriority and fallibility. What seemed a rather small concession at the end of the last section—that the grounds for the indispensability theorist's adherence to *fact* could be overthrown by progress in physics—now looms as a real possibility.

Of course, the (potential) falsity of continuum mathematics in its application to space-time would be only part of the story; there are other successful scientific uses for the calculus and higher analysis. But, if science were to change so that all fundamental theories were thoroughly quantized, so that no continuum mathematics appeared there, if all the remaining applications of continuum mathematics were explicitly understood as "approximations" or "idealizations" or "models," then even the modified indispensability theorist would retreat to some version of *no-fact*. The case of quantum gravity should keep us from dismissing this possibility out of hand.

VI. THE MATHEMATICAL PRACTICE OBJECTION

For the modified indispensability theorist, the choice between *strong fact* and *no-fact* hinges on developments in physics (and perhaps the rest of science). We should now ask the impact of this choice: How would the pursuit of our independent question be affected by it? In other words, we want to know if the metaphysical distinction between *strong fact* and *no-fact* has methodological consequences.

If *no-fact* is correct, if there is no pre-existing fact to discover about the Lebesgue measurability of Σ^1_2 sets of reals, then what approach should the set theorist take? Many observers would hold that *no-fact* is the end of the story, that mathematicians are in the business of discovering truths about mathematical reality, and that, if there is no truth to be found, the mathematician should reject the question. From this point of view, all (relatively consistent) set theories extending ZFC are equally legitimate, there is no call to chose between them, and indeed, no grounds on which to do so apart from subjective aesthetic preferences. Once our question is shown to be independent, and developments in science undercut the claim to inherent truth value, there is nothing more of serious import to be said about the Lebesgue measurability of Σ^1_2 sets. Call this *end of the story no-fact*.

This position makes nonsense of the contemporary search for new set-theoretic axioms to settle independent questions like ours. Indeed, in our case, there are two competing candidates: $V = L$ (Gödel's "axiom of constructibility") and MC (the existence of a measurable cardinal). If $V = L$, then there is a non-Lebesgue measurable Σ^1_2 set; if MC, then all Σ^1_2 sets are Lebesgue measurable. Set theorists offer arguments for and against these axiom candidates,[25] and in this debate, MC is strongly favored over $V = L$.[26] If we are not to reject this activity as inconsequential mutterings—an especially unappealing move, given that the original axioms of ZFC are supported by arguments of a similar flavor—we must instead reject *end of the story no-fact.*

But there is another version of *no-fact.* Even if there is no pre-existing fact of the matter to be discovered, the process of extending the axioms of set theory might well be governed by nonarbitrary principles. This idea turns up, not only in the study of set theory, but when ontological decisions are made in other branches of mathematics as well. For example, Kenneth Manders[27] describes the theoretical norms at work in the expansion of the domain of numbers to include the imaginary or complex numbers, and Mark Wilson[28] uncovers the rationale behind the move from affine to projective geometry. In such cases, despite lip service to the notion that any consistent system is as good as any other, mathematicians actually insist that a given mathematical phenomenon is correctly viewed in a certain (ontological) setting, that another setting is incorrect.

One need not assume *fact* to endorse these practices. Even if there is no fact of the matter, no pre-existing truth about the existence or nonexistence of complex numbers or geometric points at infinity or nonconstructible sets, the pursuit of these mathematical topics might be constrained by mathematical canons of "correctness." For our case, one might hold that there is no fact of the matter about the Lebesgue measurability of Σ^1_2 sets, but that there are still good mathematical reasons to prefer extending ZFC in one way rather than another, and perhaps, good mathematical reasons to adopt a theory that decides our question one way rather than another. From this point of view, *no-fact* is just the beginning of the

[25] See my "Believing the Axioms. I–II," *The Journal of Symbolic Logic,* LIII, 2 (June 1988): 481–511, and 3 (September 1988): 736–64.

[26] I discuss part of the case against $V = L$ in "Does V equal L?" in *The Journal of Symbolic Logic* (forthcoming).

[27] "Domain Extension and the Philosophy of Mathematics," this JOURNAL, LXXXVI, 10 (October 1989): 553–62.

[28] "Frege: The Royal Road from Geometry," in *Noûs* (forthcoming).

story; it opens the door on the fascinating study of purely mathematical canons of correctness. Call this *beginning of the story no-fact.*

The question before us is this: What are the methodological consequences of the choice between *strong fact* and *no-fact?* If *strong fact* is correct, the set theorist in search of a complete theory of her subject matter should seek out additional true axioms to settle the Lebesgue measurability of Σ_2^1 sets (and so on). If *end of the story no-fact* is correct, the set theorist left with any interest in the matter should feel free to adopt any (relatively consistent) extension of ZFC she chooses, or even to move back and forth between several mutually contradictory such extensions at will. And finally, if *beginning of the story no-fact* is correct, the set theorist should use appropriate canons of mathematical correctness to extend ZFC and to decide the question.[29]

Obviously, the method prescribed by *strong fact* differs from that prescribed by *end of the story no-fact;* that much is easy. But what about *strong fact* and *beginning of the story no-fact?* Does the pursuit of truth differ from the pursuit of mathematical correctness? In fact, I think it does. Consider, for example, a simple argument that $V = L$ should be rejected because it is restrictive. A supporter of this argument owes us an explanation of why restrictive theories are bad. A *beginning of the story no-fact*-er might say, "because the point of set theory is to realize as many isomorphism types as possible, and set theory with MC is richer in this way."[30] A *strong fact*-er might agree that the world of MC has desirable properties, while insisting that desirability (notoriously!) is no guarantee of truth.[31] Faced with the *beginning of the story no-fact*-er's argument, a *strong fact*-er would reply, "Yes, MC is nice in the way you indicate, but if *V does*

[29] The mathematical canons invoked in *beginning of the story no-fact* could ultimately recommend that several different set theories be accorded equal status. The methodology at work would still be different from that of *end of the story no-fact,* and the range of theories endorsed would almost certainly be narrower.

[30] Spelling out this line of thought precisely is no simple exercise, but I shall leave that problem aside here. The point is just that the *beginning of the story no-fact*-er appeals to some attractive feature of set theory with MC.

[31] Philip Kitcher touches on this point in his reply to Manders, "Innovation and Understanding in Mathematics," this JOURNAL, LXXXVI, 10 (October 1989): 563–4, when he writes, p. 564: "Suppose this is a way in which mathematical knowledge can grow. What kinds of views of mathematical reality and mathematical progress are open to us? Can we assume that invoking entities that satisfy constraints we favor is a legitimate strategy of recognizing hitherto neglected objects that exist independently of us? From a realist perspective, the method of postulating what we want has (in Bertrand Russell's famous phrase) 'the advantages of theft over honest toil.' If that method is, as Richard Dedekind supposed, part of the honest trade of mathematics, is something wrong with the realist perspective?"

equal L, L *does* contain all the isomorphism types possible. What's needed is an argument that $V = L$ is *false*." So *strong fact* will differ methodologically from *beginning of the story no-fact* as well as *end of the story no-fact*.

We have reached this point: a methodological decision in set theory—namely, that between the methodologies proper to *strong fact* and to *beginning of the story no-fact*—hinges on developments in physics. If this is correct, set theorists should be eagerly awaiting the outcome of debate over quantum gravity, preparing to tailor the practice of set theory to the nature of the resulting applications of continuum mathematics. But this is not the case; set theorists do not regularly keep an eye on developments in fundamental physics. Furthermore, I doubt that the set-theoretic investigation of independent questions would be much affected even if quantum gravity did end up requiring a new and different account of space-time; set theorists would still want to settle open questions about the mathematical continuum. Finally, despite the current assumed indispensability of continuum mathematics, I suspect that the actual approach to the Lebesgue measurability of Σ^1_2 sets, to $V = L$ versus MC, is more like that prescribed by *beginning of the story no-fact* than that prescribed by *strong fact*,[32] and I see no mathematical reason to criticize this practice. In short, legitimate choice of method in the foundations of set theory does not seem to depend on physical facts in the way indispensability theory requires.

VII. CONCLUSION

I have raised two doubts about indispensability theory, even modified indispensability theory, as an account of mathematics as practiced. The first, the scientific practice objection, notes that indispensability for scientific theorizing does not always imply truth and calls for a careful assessment of the extent to which even fundamental mathematized science is "idealized" (i.e., literally false). The second, the mathematical practice objection, suggests that indispensability theory cannot account for mathematics as it is actually done. If these objections can be sustained, we must conclude that the indispensability arguments do not provide a satisfactory approach to the ontology or the epistemology of mathematics. Given the prominence of indispensability considerations in current discussions, this would amount to a significant reorientation in contemporary philosophy of mathematics.

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[32] I hope to argue this in some detail elsewhere.