Avner Ash
Boston College

Justin Clarke-Doane
Columbia University
IAS, Princeton

Intuition and Observation[1]

The motivating question of this paper is: 'How are our beliefs in the theorems of mathematics justified?' This is distinguished from the question 'How are our mathematical beliefs reliably true?' We examine an influential answer, outlined by Russell, championed by Gödel, and developed by those searching for new axioms to settle undecidables, that our mathematical beliefs are justified by 'intuitions', as our scientific beliefs are justified by observations. On this view, axioms are analogous to laws of nature. They are postulated to best systematize the data to be explained. We argue that there is a decisive difference between the cases. There is agreement on the data to be systematized in the scientific case that has no analog in the mathematical one. There is virtual consensus over observations, but conspicuous dispute over intuitions. In this respect, mathematics more closely resembles stereotypical philosophy. We conclude by distinguishing two ideas that have long been associated -- realism (the idea that there is an independent reality) and objectivity (the idea that in a disagreement, only one of us can be right). We argue that, while realism is true of mathematics and philosophy, these domains fail to be objective. One upshot of the discussion is that even many questions of fundamental physics fail to be objective in roughly the sense that the question, 'Is the Parallel Postulate is true?', understood as one of pure mathematics, fails to be. Another is a kind of pragmatism. Factual questions in mathematics, modality, logic, and evaluative areas go proxy for non-factual practical ones.

1. Informal and Formal Proof

How do we know mathematical truths? The obvious answer is: on the basis of proofs (Rachels [1998, 3])! For example, we know Fermat's Last Theorem on the basis of the proofs of Wiles and Taylor-Wiles, and know the 4-dimensional Poincaré Conjecture on the basis of Freedman's proof. But what is a mathematical proof? To most philosophers, it is a *formal* proof: a finite sequence of formulas, each of which is either an axiom or follows from a previous formula by a rule of formal inference, the last line of which is the formula proved. Mathematical proofs are

[1] Thanks to Barry Mazur, Will Cavendish, Michael Harris, Juliette Kennedy and two anonymous referees for helpful comments.

not remotely so finicky.[2] A formal proof of practically any mathematical conjecture of interest would be incomprehensible to human beings. Mathematical proofs are closer to (informal) arguments that convince experts that *there exists* a formal proof. Such arguments are sometimes sketches, or outlines, of formal proofs. But they are not so in general.

Why think that formal proof is the final arbiter of mathematical validity? Because, on one hand, a formal proof of (a formal statement of) a conjecture is widely agreed to establish that conjecture, even if the proof is deemed otherwise non-ideal. For example, the proofs of the Four Color Theorem and the Kepler Conjecture make apparently ineliminable use of computers, given the limitations of human computing ability with respect to time, patience, and accuracy. These proofs are unsatisfying insofar as they cannot be 'surveyed'. But practically no one denies that the theorem follows (or, at least, follows in classical logic – see Section 11).[3] On the other hand, the independence results concern *formal* proofs, but are widely agreed to settle whether the conjecture in question is (informally) provable. For example, the proof that $\mathrm{Con}(ZFC) \rightarrow \mathrm{Con}(ZFC + CH)$ and $\mathrm{Con}(ZFC) \rightarrow \mathrm{Con}(ZFC + {\sim}CH)$ is an argument that there exists no *formal* proof of (a formal statement of) the *Continuum Hypothesis* (*CH*) or its negation from (a formal statement of) *Zermelo-Fraenkel* set theory with the *Axiom of Choice* (*ZFC*), if that theory is (formally!) consistent. This establishes that anyone seeking to (informally) prove the *CH* or ~*CH* (from accepted principles) is doomed to fail. At best, they might identify new axioms that mathematicians come to accept, along with the others.[4]

It is a good question how mathematicians know that there is a formal proof on the basis of the (informal) arguments that convince them. Most mathematical theorems have no (concrete) formal proofs to back them up (though some in the interactive theorem proving community have begun to rectify this situation). But even if this question were answered, two puzzles would remain. First, how do mathematicians know what formally follows from what? Second, how do they know the axioms? We focus on the second question, but return to the first in Section 11.

A preliminary point: logicians partition axioms into *foundational* and *structural* ones (Shapiro [1997, 41 & 50]). Outside of set theory, arithmetic, and real analysis in some treatments, mathematicians tend to use axioms to describe anything that satisfies certain conditions, not to characterize an antecedently given collection of things. The question of correctness (over and above consistency) only arises in connection with foundational axioms. So, we focus on them.

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[2] Indeed, formal proofs are at best rational reconstructions of actual mathematical reasoning. See Ash & Gross [2008], Harris [2012], and Mazur [2012].

[3] For a contemporary illustration, consider the purportedly almost complete formal verification in *Lean* of Holmes' proof of Con(*NF*). If this is indeed completed, then it will establish the (relative) consistency of Quine's system, even if no one besides Holmes can follow the proof.

[4] One might think that proofs by diagram show that not all informally valid proofs can be formally verified. But, again, informal proofs are not, in general, outlines of formal ones. So, in order for this to be the case, there would need to be a diagrammatically provable conjecture that either lacks a formal analog or whose proof does. Whether there is such a thing depends on what 'formal analog' should mean. This is outside the scope of the present paper.

2. Knowing the Axioms

There is a familiar narrative according to which the axioms of mathematics are *self-evident* (Greene [2013, 184]). Historically, this story is associated with Euclid's *Elements* and Descartes's clear and distinct ideas. In the 20th century, self-evident axioms were often deemed *analytic* – truths that one can know merely in virtue of their linguistic competence (Coffa [1982], Singer [1994, 8]). Perhaps it is just part of what we mean by 'set' that every set occurs at some level of the cumulative hierarchy, for example — i.e., that the *Axiom of Foundation* is true (Boolos [1971, 498], Shoenfield [1977, 327]).[5] One who lacks an even defeasible disposition to believe Foundation must lack competence with the predicate '$\in$' (and concept of membership).

However, even if it were analytic that all sets are well-founded, it is not analytic that there are no non-well-founded set-like things. If we were worried that some sets fail to be well-founded, then, under the assumption that Foundation is analytic, we should just worry that there are shmets – where it is no part of the concept of shmet that all shmets are well-founded. The claim that there are no shmets is like the claim that there are no superstrings. This is not analytic on anyone's view. But the claim that there are no shmets is just the 'transposition' of the claim that there are no non-well-founded sets, under the assumption that Foundation is analytic. Of course, there might be sets *as well as* shmets.[6] But then the question of whether all sets are well-founded would be like the question of whether two straight lines intersecting another so as to make less than a 180° angle on one side must intersect on that side. It depends on what you mean by 'line'! If 'line' is interpreted to mean Euclidean line, then yes. If it is interpreted to mean, say, hyperbolic line, then no. There is no metaphysical, as opposed to semantic, question to dispute.[7]

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[5] We use 'true' throughout in the ordinary Tarskian sense (as per Benacerraf [1973]). So, e.g. 'There is an inductive set' is true just in case there is an inductive set. Mathematicians, of course, frequently use the term differently. One often calls the conclusion of a proof 'true' in ordinary mathematical practice when one thinks that the proof is correct by the ordinary standards that are regulative there, independent of one's belief in the literal truth of its conclusion. Ordinary truth, not 'mathematical correctness', is what is relevant to the comparison with empirical science.

[6] In the development of Warren [2020], it is apparently analytic that there are shmets as well as sets.

[7] Shoenfield himself writes that 'It is...possible that there is a completely different analysis of the notion of a set, and this might lead to quite a different set of axioms [1985, 324].' But he does not seem to appreciate the deflationary methodological ramifications of this possibility. Boolos is bolder. But Incurvati responds: 'we cannot believe in… Foundation on the...grounds that the iterative conception is, as Boolos suggested...'the only natural and (apparently) consistent conception of set we have [2014, 206].' He continues, '[Aczel's] graph conception [for example]… emerges as a candidate to be the conception of set embodied by a set theory centered around the idea of a set being depicted by a graph…[I]f sets are what is depicted by an arbitrary graph, then most of the axioms of *ZFA* are justified [2014, 205].' Bar-Hillel, Fraenkel, & Levy note, 'To an extreme Platonist the question of the consistency of a system of set theory is not really a central foundational problem. He will discard a theory at the point when it turns out not to fit the 'true facts' about the universe, even if it is consistent. To put it more strongly, an extreme Platonist, like a physicist, will prefer an inconsistent theory some parts of which give a faithful description of the real situation to a demonstrably consistent theory which gives 'wrong information' about the universe [1973, 326].'

Perhaps the axioms of mathematics are self-evident, but not analytic. Many reject the analytic/synthetic distinction on independent grounds (Quine [1951]). But they still allow that some claims are self-evident – or, anyway, initially plausible or intuitive. Consider the proposition that *here is a piece of paper*. This is not analytic. It is conceivable that we are brains in vats. But we are presented to – perceptually – as being in a world, with external objects, including pieces of paper (Pryor [2000]). Do we not enjoy analogous experiences in connection with pure mathematics (Bealer [1999], Bengson [2015], Chudnoff [2013], Huemer [2005])?[8] Set theory presents to us – albeit intuitively, and not perceptually – as being about a cumulative hierarchy, where every set has a Choice function, sets under definable functions also form sets, and so on. Gödel writes,

> [D]espite their remoteness from sense experience, we do have a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true [1947, 483-4].

One problem with Gödel's proposal is that, even if *some* mathematical propositions 'force themselves on us as being true', these do not include most of the axioms of standard mathematics (Boolos [1999, 130-131], Maddy [1988a]).[9] For example, notwithstanding Foundation, sets that contain themselves, or those with infinitely-descending chains of membership, are important in some parts of mathematics, linguistics, and computer science (Aczel [1988, Introduction], Moss [2018]). They also let us preserve something of the naive concept of set, according to which there is a universal set, complements of arbitrary sets, and sets of all sets with *n* members (Quine [1937] & [1969]). Meanwhile, it is not beyond dispute that Foundation is even coherent (Ferrier [Forthcoming], Potter [2004, Sec. 3.3], Rieger [2011, 17–18]). What, after all, does it *mean* to say that an object, like *V*, is 'built in stages' via repeated applications of the *Powerset* and *Union* axioms? What kind of priority is involved? Not temporal priority.[10] But it cannot be 'metaphysical' possibility either. There is no possible world in which a set exists but its powerset fails to.[11] Perhaps there is a hyperintensional respect in which earlier stages 'ground' or

[8] The conception of intuition common to these authors is that an intuition that *P* is an experience as of *P* (not just dispositions to judge that *P*) which defeasibly justifies one in believing that *P*. The key difference between intuition and observation, on this conception, is the content that is experienced. Ironically, given that these authors all accept an a priori/a posteriori distinction, one of us has argued from this conception to a new basis for skepticism about that distinction (Clarke-Doane [2020, Section 3.6 & 3.9]).

[9] *Cf.* Boolos:'I am by no means convinced that any of the axioms of infinity, union, or power [set]...force themselves upon us or that all the axioms of replacement that we can comprehend do….[T]hat there are doubts about the power set axiom is of course well known….[T]here is nothing *unclear* about the power set axiom….But it does not seem to me unreasonable to think that…it is not the case that for every set, there is a set of all its subsets [1999, 130–1, italics in original].'

[10] An anonymous referee requested an argument for this. Two immediate problems are that it is unclear whose construction would be canonical and how anyone could complete a transfinite construction in a finite time. Note also that, in light of the Relativity of Simultaneity, there may be no Lorentz invariant fact as to what sets existed if they were constructed in time. But at least what sets of *events* exist is something all observers should agree to!

[11] On the orthodox interpretation, anyway. We return to different interpretations of 'possible' in Section 11.

'constitute' later ones? Perhaps. But whether such hyperintensional relations are well-founded is itself doubtful (Cameron [2008, 4–5], Dixon [2016], Rabin & Rabern [2016]).

The more promising intuition-based epistemology of mathematics draws an analogy between the axioms of mathematics and the laws of empirical science. Russell hoped to 'prove' all of mathematics in logical as well as epistemic senses of that term (Russell [1903/2022], Russell & Whitehead [1910/1963]). He hoped to begin with indubitable 'laws of logic' (somewhat like Frege) and deduce.[12] Unfortunately, he soon discovered the paradox that bears his name, according to which, even the seeming banality that every predicate has an extension (perhaps empty) must be false. He further realized that his project required axioms that practically no one claimed were self-evident, like the axioms of *Infinity*, *Reducibility*, and Choice. He concluded,

> We...believe the premises [axioms] because we can see that their consequences are true, instead of believing the consequences because we know the premises….But the inferring of premises from consequences is the essence of induction; thus the method in investigating the principles of mathematics is really an inductive method, and is substantially the same as the method of discovering general laws in any other science [1907/1973, 273–274].[13]

Despite his remark about axioms 'forcing themselves upon us', Gödel agrees. He writes,

> [Russell] compares the axioms of…mathematics with the laws of nature and logical evidence with sense perception, so that the axioms need not…be evident in themselves, but rather their justification lies (exactly as in physics) in the fact that they make it possible for these 'sense perceptions' to be deduced….I think that…this view has been largely justified by subsequent developments, and it is to be expected that it will be still more so in the future [1944/1990, 121].

[12] These laws included, of course, what we would today regard as substantial mathematics, since the (higher-order) logic that Russell used is incomplete.

[13] Russell's changing view on the epistemological status of mathematics seems to be related to his abandonment of logic in favor of metaphysics, epistemology and social philosophy at the height of his logical powers. He recalls,

> I wanted certainty in the kind of way in which people want religious faith. I thought that certainty is more likely to be found in mathematics than elsewhere. But I discovered that many mathematical demonstrations, which my teachers expected me to accept, were full of fallacies, and that, if certainty were indeed discoverable in mathematics, it would be in a new field of mathematics, with more solid foundations than those that had hitherto been thought secure. But as the work proceeded, I was continually reminded of the fable about the elephant and the tortoise. Having constructed an elephant upon which the mathematical world could rest, I found the elephant tottering, and proceeded to construct a tortoise to keep the elephant from falling. But the tortoise was no more secure than the elephant, and after some twenty years of very arduous toil, I came to the conclusion that there was nothing more that I could do in the way of making mathematical knowledge indubitable [1952/2023].

Russell's mature proposal is that the *epistemological* priority of mathematical principles typically precedes their *logical* priority.[14] We deduce theorems from axioms. But we are justified in believing the axioms because we are justified in believing the theorems that they imply, rather than the other way around (contra the aspirations of Frege's logicism). Assuming that our mathematical 'sense perceptions' are revisable, Russell's method is, accordingly, just Rawls' method of *reflective equilibrium* – the method of 'testing theories against judgments about particular cases, but also testing judgments about particular cases against theories, until equilibrium is achieved [Blackburn 2008, 312].'[15] We begin with 'intuitions', or plausibility judgments. We then abduct to the best axiomatization of those judgments. Explanatory considerations may lead us to amend some of the judgments with which we began. Likewise, compelling intuitions may 'disconfirm' an otherwise attractive theory. We vacillate between considerations of plausibility and systematicity until (ideally) we achieve harmony between the two.[16]

We can illustrate the method with reference to *AC*. *AC* says that every set has a Choice function. It implies a range of plausible propositions. For example, *AC* implies that the union of countably many countable sets is countable, and that any two sets either have the same cardinality or one has greater cardinality—that is, the *Principle of Cardinal Comparability* (*PCC*). But it also implies what are widely agreed to be counterintuitive results, like the *Banach–Tarski Paradox* and that the set of real numbers is well-orderable (it is relatively consistent even with *ZFC* that there is no *definable* well-order on the reals). Perhaps these results show that we ought to reject *AC*. In that case, we might adopt a weakening of it, like *Dependent* or *Countable*, Choice, or (given the consistency of sufficiently large cardinals) the *Axiom of Determinacy* (which happens to imply Countable Choice). But maybe *AC*'s implications instead show that our intuitions about, say, mathematical space, need to be revised. What exactly they do show depends on which alternative would facilitate equilibrium among our beliefs.[17]

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[14] 'Typically' because this need not always be so. For instance, the axioms of Euclidean geometry (which Frege [1900/1997], unlike Russell, denied were provable from 'logic' alone) seem to be more like a compendium of our intuitions about space than a post facto axiomatization of still more concrete intuitions. But Russell [1907/1973] explicitly takes the axioms of arithmetic to be 'inductively' supported. Similarly, Weyl reminds us that 'A set-theoretic treatment of the natural numbers such as that offered by Dedekind may indeed contribute to the systematization of mathematics; but it must not be allowed to obscure the fact that our grasp of the basic concepts of set theory depends on a priori intuition of iteration and of the sequence of natural numbers ([1918/1994, 109]).

[15] See Rawls [1974, 8] for one characterization of the method.

[16] *N.B.*: Ordinary mathematicians sometimes use the word 'intuition' for a kind of mental state that differs from the one appealed to by Gödel and those searching for new axioms. Intuitions in this sense are reflective of mathematicians' knowledge of the rules of the practice, not of their conviction as to the literal truth of a mathematical claim in dispute. Conspicuously, an expert may have an 'intuition' as to how to solve a problem. By contrast, intuitions in the sense of impressions as to the literal truth of (non-logical) mathematical statements (e.g., *PCC*, mentioned in the next paragraph) are the ones that are relevant to the comparison with empirical science. (By 'non-logical', we mean not intuitions about what follows from what. We return to logical intuitions in Section 11.)

[17] What precisely constitutes equilibrium is a notorious problem (Quine & Ullian [1978], Daniels [2016]). In particular, we do not assume that harmony is ever achieved. Maybe it is a regulative ideal. This will not be important.

3. Justification and Reliability

Russell's epistemology has considerable appeal. It has been widely adopted by those searching for new axioms to settle undecidables, like the Continuum Hypothesis (Woodin [2010, 1], Kennedy & van Atten [2004] & [2009], Koellner [2006], Maddy [1988b], Martin [1998], Steel [2014]). How do we know the axioms of mathematics? In the way that we know the laws of nature! The difference is that in science, laws are postulated to systematize observations, while in mathematics laws (axioms) are postulated to systematize intuitions. But observation and intuition play the same epistemic role. They both present the world to us as being a certain way. They are the data that we try to 'save'.[18]

Unfortunately, there is a complication. At most the *justification* of our mathematical beliefs is analogous to the justification of our empirical scientific beliefs. It does not follow that the *reliability* of our mathematical beliefs parallels that of our scientific ones. Benacerraf notes,

> without an account of *how* [mathematical propositions] 'force themselves upon us as being true,' the analogy with sense perception and physical science is without much content….[T]here is a *superficial* analogy….[W]e 'verify' axioms by deducing consequences from them concerning areas in which we seem to have more direct 'perception' (clearer intuitions). But we are never told how we know even these, clearer, propositions [1973, 674, italics in original].

To see the problem, consider again how we know that *here is a piece of paper*. Knowledge is justified and reliable belief (where specifying the relevant sense of 'reliable' is the Gettier Problem). We can explain the justification of our belief by citing its perceptual evidentness. It seems to us that *here is a piece of paper*, and we have no reason to doubt appearances. But what about its reliability (Field [1989, Introduction])? Although we are far from having a complete theory, an evolutionary account of how we came to have a reliable mechanism for perceptual belief, and a neurophysical account of how that mechanism works such that it is reliable, suggests itself (Schechter [2010]). This contrasts with the mathematical case. We can explain the justification of our belief that, say, $1 + 1 = 2$ by citing its intuitive evidentness. But what explains the *reliability* of this belief? '$1 + 1 = 2$' is about *numbers*. It says that the plus function maps the number *1* and itself onto *2*. How could that have any bearing on our ancestors' reproductive fitness? More relevant are *(first-order) logical truths* about things in our ancestors'

[18] Compare Gödel: 'I don't see why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them… [1973/1907, 273–274].' (We of course use 'data' in its Latin sense, not in a statistical one. We restrict ourselves to fundamental, or basic, givens, as is standard in these discussions. Note that, in keeping with the analogy to observation, the data in the mathematical case concern the likes of numbers and sets, not *our experiences as of* numbers and sets. The data in the scientific case (with the exception of the psychological sciences) concern pointers, planets, and particles, not our *experiences as of* pointers, planets, and particles.)

environments – like that if there is ‘exactly one’ lion to the left, and ‘exactly one’ lion to the right, and no lion to the left is to the right, then there are ‘exactly two’ lions to the left or right (where the phrases ‘exactly one’ and ‘exactly two’ are abbreviations for constructions out of ordinary quantifiers and the identity sign, and do not refer to numbers) (Clarke-Doane [2012]).

Indeed, even if there were a theory of how we came to have a reliable mechanism for mathematical belief, it would remain to explain how that mechanism works such that it is reliable. We have no established psychophysics of number perception – not to be confused with the perception of cardinalities of (very small) collections, as with the Approximate Number Sense (*ANS*). Numbers are not like hands, deflecting photons that stimulate our optic nerves![19]

The upshot is that while the *justification* of our mathematical beliefs may be explicable in analogy with the justification of our scientific beliefs, the reliability of those beliefs is not.

4. Science and Mathematics

What makes science work? Part of the story is that it relies on observation. Observation is shared. We disagree about evolution, but we agree on the observations that are supposed to support it. Well, not exactly. For one thing, ‘all’ is too strong. Observations can be impaired. Not *everyone* perceives what we do. Also, observation is theory-laden. In claiming to observe a fossil, we betray commitment to a theory according to which the world is old enough for the object in question to have fossilized. This is not common ground in all contexts. Nor could we avoid every such difficulty by appealing to more primitive kinds of judgment. Even claims about what we *seem* to see presuppose a theory to which some object (Churchland [2014, Sec. 2.5]).

Nevertheless, there is characteristic conformity about the data that our scientific theories aim to systematize. It is remarkable that a believer in dark matter can agree in the ways that matter for experiment with supporters of *MOND* (Milgrom [2002]) even though the former believes that a quarter of the universe is made of a substance whose existence the latter denies. The dispute between advocates and detractors of dark matter is about what best accounts for our observations, not over our observations themselves. While observation is theory-laden, it is, on the whole, shared.

If mathematics is like science, at least *at the level of justification* (even if not at the level of reliability), then intuition should play an analogous role. It should be the common ground, for all

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[19] Presumably we know this much, despite not even knowing what numbers would be, or whether the question of what they would be is somehow ill-formed (Benacerraf [1965]).

practical purposes, against which theorists with very different conceptions of mathematical reality can test those conceptions.

Prima facie, it does just that. Mathematics constantly surprises, sometimes undercutting hopes or expectations in the process. It does not submit to our wishes like a lump of clay. But the picture is complicated on inspection. It is clear that there are rules to which the overwhelming majority of mathematicians implicitly agree, whose violation is a matter on which they also generally concur – independent of any position that they may harbor on the nature of mathematical reality.[20] Ultimately, those rules answer to a formal system like (first-order) *ZFC*, as per Section 1. We say 'ultimately' because there is a division of cognitive labor. Algebraic geometers can usually rely on others in their field to check the validity of a proof, disregarding questions of logic and set theory. But if questions are raised about background set-theoretic (or category-theoretic) assumptions, then outside experts may be consulted. In principle, the matter may face the tribunal of logicians who determine whether the theorem can be proved in *ZFC*.[21]

The agreement in practice among mathematicians is almost unique in the academy. But agreement in practice does not amount to agreement in theory unless there is also agreement on axioms (and inference rules). There is agreement in practice in chess (or baseball, or bridge, or…) too. But there is no corresponding theory about the world to which all parties would subscribe.[22] Generally speaking, mathematicians follow the rules that they have been taught without seriously inquiring as to their correctness (or into what it might mean to say that an axiom or inference rule is correct) (Martin [1998, 219]). This is no indictment of mathematicians, notwithstanding Frege [1980/1884, 11]. Arguably, mathematicians bracket foundational questions *in order to* prove theorems. If they had to first settle the question of what axioms (and inference rules) are correct, then they might never get started (Easwaran [2008]).

However, for purposes of arguing that there is an epistemic parity between intuition and observation, it matters what theorists believe, not what they do. Since attempting to determine what axioms are true (or what logical rules are truth-preserving) is not a normal mathematical activity, we must turn to the (small!) minority of theorists who concern themselves with foundational matters. Do intuitions play the role in the foundations of mathematics that

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[20] The current case of Mochizuki's claimed proof of the *ABC* conjecture may be an exception. Kiran Kedlaya writes that 'it is potentially *unknowable* whether Mochizuki's proof is correct….[I]n principle one could try to translate the proof into a formalized system (e.g. *Lean*) and then verify mechanically the correctness of the proof. However, if one does end up confirming the existence of a correct proof, this still leaves the question of authorial intent: does the formal proof actually correspond to what Mochizuki had in mind?....I wouldn't be surprised if we ended up with a proof that meets community standards but requires substantial work beyond what is in his papers…In that case, whose proof is it anyway?…And is the original attempt correct, incomplete, or something else?' [Personal correspondence, quoted with permission]
[21] We do not suggest that this is a common occurrence!
[22] One might think that all parties would agree on how you are *permitted* to move chess pieces, and what moves are *possible*, in various circumstances. But any such theory (construed literally) would involve commitment to evaluative and modal facts, which we will see in Sec. 10 and 11, are at least as controversial as mathematical ones.

observations play in fundamental physics? Are they common ground against which disparate theories are tested?

5. The Problem of Conflicting Intuitions

In a word: no. Among those (very few) theorists who concern themselves with foundational questions, there is infamous disagreement. Fraenkel, Bar-Hillel and Levy write that there is ‘far-going and surprising divergence of opinions and conceptions of the most fundamental mathematical notions, such as set and number [1973, 14].’ And Bell and Hellman remark,

> Contrary to the popular (mis)conception of mathematics as a cut-and-dried body of universally agreed upon truths…as soon as one examines the foundations of mathematics one encounters divergences of viewpoint...that can easily remind one of religious, schismatic controversy [2006, 64].

It is conceivable that all such disagreement concerns the best systematization of our shared mathematical intuitions, not the contents of those intuitions themselves. But a cursory glance at the literature tends to undercut this hypothesis. The following quotations are representative:

> There is almost a continuum of beliefs about the…world of set theory [Cohen 2005].

> Let me try to be as accurate, explicit, and forthright about my belief about the existence of κ [= the least ordinal greater than all f(i), where $f(0) = \aleph_0$ and $f(i + 1) = \aleph_{f(i)}$] as I can…I…think it probably doesn’t exist….I am also doubtful that anything could be provided that should be called a *reason* and that would settle the question [Boolos 1999, 121, italics in original].

> Without any doubt the most problematic axiom of set theory is the axiom of choice....The current situation with *AC* is that the contestants have agreed to differ [Forster Forthcoming, 72].[23]

> The set-theoretical axioms that sustain modern mathematics are self-evident in differing degrees. One of them – indeed, the most important of them, namely…the so-called axiom of infinity – has scarcely any claim to self-evidence at all [Mayberry 2000, 10].

[23] Although Forster is a logician, it should not be thought that his assessment only reflects that of logicians with interest in foundations. For instance, Villani writes, ‘The axiom of choice…is absolutely not constructive; youngsters like me want to get rid of it…[Cartier et al. 67]’, to which Cartier responds, ‘In my generation too, the axiom of choice was far from being uncontroversial, van der Waerden was reluctant to use it in his book..and…[a]s for Deligne, he clearly admitted that he uses the axiom of choice out of convenience, but that he does not believe in it [*Ibid*., 67].’

> *ZF* does not embody a philosophically coherent notion of set. There is a coherent constructivist position….There is also a coherent anti-constructivist position ….But *ZF* is an uneasy compromise between these two: it pays lip-service to constructivism without…meaning it…. Only a non-well-founded [set] theory can…be shown to modify the naive conception as much as, but no more than, is required… [Rieger 2018, 17-18].[24]

> Set theorists say that $V = L$ has implausible consequences…. [They] claim to have a direct intuition which allows them to view these as so implausible that this provides 'evidence' against $V = L$. However, mathematicians [like me] disclaim such direct intuition about complicated sets of reals. Many say they have no direct intuition about all multivariate functions from N into N [Friedman FOM, 5.25.00]![25]

> I have had numerous opportunities to discuss the alternatives in set theory with mathematicians from other disciplines. I am always surprised at how rapidly and with what certainty they express their opinion. Their opinions are divided and reflect, in my view, a pre-existing orientation [Jensen 1995, 401-2, n. 18].

Of course, the mere existence of disputes like those above does not *imply* that theorists have conflicting intuitions [Stratton-Lake 2020]. But the face-value interpretation of the debates is that opposed parties do often have different intuitions. To borrow Jensen's language, the disagreements are reflections of 'deeply rooted differences in mathematical taste' [1995, 401].[26]

6. Theory Ladenness

Perhaps, however, while disagreement in mathematics does turn on conflicting intuitions, controversial mathematical intuitions are analogous to controversial observations, not to *here is a piece of paper*. They are theoretical. Disagreement over abstruse matters of set theory, like whether there are more subsets than L allows, is like disagreement over the extent to which a borderline biopsy sample harbors dysplasia. The latter is an *observational* matter over which there is controversy.

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[24] Compare [Potter, 2004, p. 36]: '[I]n an attempt to make the history of the subject read more like an inevitable convergence on the one true religion, some authors have tried to find evidence of the iterative conception quite far back in the history of the subject.'
[25] Similarly, Devlin thinks that $V = L$ 'is … a natural axiom, closely bound up with what we mean by 'set' …. [and] tends to decide problems in the 'correct' direction [1977, 4].' On the virtues of $V = L$, see especially Jensen [1995].
[26] Compare Mogensen discussing moral disagreement: 'Diagnosing a clash of intuitions…will typically involve attempting a careful hermeneutic reconstruction of the underlying dialectic, designed to reveal that the dispute rests ultimately with certain…premises that one side finds intuitive and the other does not. Any such reconstruction is bound to be controversial…. [W]hereas many philosophers agree that some questions boil down to...differences in intuition, there is considerable disagreement as to exactly which questions those are [2016, 24].'

The problem with this response is that, in the empirical case, we can contrast observational judgments of, e.g., the degree of dysplasia with ‘practically bedrock’ observational judgments about the overt appearance of the biopsy sample (‘...not very circular’). The contrast is vague (Hanson [1958, Ch. 1]). There is no bright line between observations that serve as a common arbiter of scientific dispute and those that fail to. However, in the mathematical case, not even a vague distinction suggests itself. No claims are off limits from serious dispute. Consider:

> It will be recognized…that in any wording [the Least Upper Bound Axiom] is false (Weyl as quoted in Kilmister [1980, 157]).[27]
>
> The reason for mistrusting the induction principle [of arithmetic] is that it involves an impredicative concept of number….A number is conceived to be an object satisfying every inductive formula [Nelson 1986, 1].
>
> I am a platonist…[but] I deny even the...Peano axiom that every integer has a successor….[Zeilberger 2004, 32-3].
>
> This standard concept of even the potential infinite is no less dubious than our standard concept of the actual infinite…[Benardete 1964, 2010].
>
> I have seen some…go so far as to challenge the existence of 2^{100}…[Friedman 2002, 4].
>
> There are no such things as mathematical objects. And so….[even s]entences like ‘4 is even’ are not true [Balaguer 2018].

Of course, remarks like the above often reflect intuitively philosophical commitments. Weyl and Nelson, who evidently lack an intuition in support of the *Least Upper Bound* and (unrestricted) *Induction*, axioms, respectively, subscribe to more or less radical versions of predicativism, the philosophical doctrine that it is not coherent to define an object in terms of a totality to which it belongs.[28] It might be objected that the unsullied among us share the intuition that every non-empty set of real numbers with an upper bound has a least upper bound, and that for *any* predicate, *F*, in the language of arithmetic, if *0* satisfies *F* and *n+1* satisfies it whenever *n* does, then all natural numbers satisfy *F*. Only those corrupted by philosophy lack this good sense.

[27] Note that Weyl accepts classical logic here. So, his objection is different from that of the intuitionists (a group Weyl later joined). See Weyl [1918/1994]. Indeed, none of the quotations are from those who explicitly reject classical logic. But some seem to be committed to non-classical logic,. For instance, the *Successor Axiom*, rejected by Zeilberger (below) and sometimes, it seems, by Nelson ([1986, 176] & [2013]), is a (classical) logical truth in a language with the successor sign. By the axioms of identity, $Sx = Sx$. By *Existential Introduction*, $\exists y(y = Sx)$. And by *Universal Generalization*, $\forall x \exists y(y=Sx)$. So, Zeilberger, at least, would seem to be committed to a free logic.

[28] Predicativism was introduced in Russell [1906] and Poincaré [1906], although not in the radical form advocated by Nelson.

There are three problems with this objection, however. First, who knows what intuitions we would have if all philosophical considerations were bracketed. How could one even form a view as to whether, for example, *AC* is true bracketing the philosophical question of whether sets depend on us for their construction? (If they do, how could one hold that there is a well-order on the reals – assuming that we cannot construct the *On* of *V*, and, hence, *L*?) Second, insofar as we can make sense of such questions, mathematical disputes often appear to turn on untutored intuition, not philosophy. Consider the intuitions that are traded in debates over the Axiom of Foundation, for example. The key question is whether there are sets that contain themselves or sets with infinitely descending chains of membership. The standard view is that such sets are 'pathological' (Boolos [1971, 491], Maddy, [1988a]). But, as Rieger (above) remarks, others have a very different reaction. Again, the dispute over Foundation appears to be more indicative of 'deeply rooted differences in mathematical taste' than philosophical priming [1995, 401].

Finally, it is irrelevant to the contrast whether intuition explains theory, or theory explains intuition. Either way, intuition varies in a way that observation does not. *If we could check philosophical theories independently of our intuitions*, then this difference might have epistemic import. But, as critics of reflective equilibrium have long stressed, we do not seem to be able to.

7. Heretics

Maybe an appeal to poll numbers would reestablish a parity between observation and intuition. There are always heretics, including geniuses. Perhaps mathematicians with apostate intuitions are in the overwhelmingly *minority*. And maybe *this sociological fact* affords (defeasible) evidence that the heretics are 'hallucinating'. For instance, Koellner writes of the proposed axiom, *Projective Determinacy* (*PD*), which is inconsistent with the *Axiom of Constructibility*, *V*=*L*, that '[*PD*] has gained wide acceptance by the set-theorists...who know the details of the constructions and theorems involved in the case that has been made for *PD*', apparently citing this as evidence for *PD* and against *V* = *L* [2013, 21-22].[29]

However, this rejoinder does not stand up to scrutiny. First, it is not clear that there are *any* prominent scientists with heretical observations. One can be a fine mathematician – as, e.g., Nelson was – while having wildly heterodox 'tastes' (in Jensen's sense). But it is hard to see how one could make major contributions to any empirical science while doubting the observational analog of the totality of exponentiation (rejected by Nelson). Of course, there are scientists who are colorblind or otherwise impaired. But there is an *independent* account of what is inaccurate about their observations, as evinced by the fact that they are convinced of their

[29] The sense in which *V* = *L* says that all sets are 'constructible' is *not* the sense in which constructivist mathematicians (i.e., intuitionists), like Brouwer, say this (to say nothing of 'ultra-intuitionists' like Yessenin-Volpin). *L* has the same ordinals as *V*, and takes them as given prior to the 'construction' process.

observational limitations. A colorblind scientist believes that she is colorblind, not that the rest of the scientific community hallucinates wavelengths! By contrast, a prevalent and exasperating feature of clashes in intuition is that, as Forster writes, 'the contestants have agreed to differ.'

Second, the *relevant* group to poll would presumably consist of theorists who actually work on the disputed axioms and intimately related topics -- not merely those who 'who know the details of the constructions and theorems involved in the case' for the standard axioms.[30] However, as Russell underscores, specialist knowledge tends to turn 'something so simple as not to seem worth stating' into something that admits of reasonable debate [1918, 514]. Forster quips,

> [F]or people who want to think of foundational issues as resolved…[standard axioms provide] an excuse for them not to think about [them] any longer. It's a bit like the role of the Church in Medieval Europe: it keeps a lid on things that really need lids [Forthcoming, 15]!

In other words, *among the relevant sample group*, skeptics about Choice are unlikely to be in the overwhelming minority, although they may well be among mathematicians more generally – simply because most of the ones who understand *AC* and its implications uncritically adopt it.

The final problem with appeal to poll numbers is that it is unclear why they would matter. Arguably, poll numbers matter in the empirical case because we should expect ourselves to be reliable detectors of facts about our surroundings. Insofar as empirical science is an extrapolation from everyday environmental inquiry, we should predict that scientists would converge toward the truth (and insofar as science floats free of observational check, as with *GUT*s, or *TOE*s, we should be unsurprised by intractable disagreement). Of course, this prediction is premised on a scientific theory – evolutionary theory. It has no force in connection with disagreement over evolution itself. But *assuming* the truth of evolutionary theory, we have the following contrast. There is no reason to think that true mathematical intuitions – as opposed to true scientific observations – would be popular *independent of their contents*. (*Given* the truth of *AC*, say, and an argument that it was likely for us to *believe AC*, we of course have reason to think that the intuitions that we do have, which happen to be true, would be popular. But that is not reason to believe that *true intuitions per se* would be (Clarke-Doane [2020, 5.5])). The mathematical intuitions that we have often appear to owe themselves to where we went to graduate school, who we studied with, and other factors that are irrelevant to the mathematical truth.[31] Martin remarks, 'For individual mathematicians, acceptance of an axiom is probably

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[30] Devlin draws a similar distinction when he writes: 'Currently I tend to favour *V=L*….At the moment I think I am in the majority of informed mathematicians, but the minority of set theorists ... [1981, 205].' Chow similarly remarks, 'You can stick to your guns, seek out like-minded people, develop theories, and evangelize. What I don't think you can do is to judge your efforts by how popular they seem to be [FOM 7.25.2023].'

[31] Indeed, Koellner, quoted above, studied with the most prominent advocate of *PD*, Hugh Woodin. (Would he have the same intuitions concerning 'simple' sets of reals had he studied with Jensen?)

often the result of nothing more than knowing that it is a standard axiom [1998, 218].' And Cohen laments, 'the attitudes that people profess towards the foundations [the question of what axioms are true] seem to be greatly influenced by their training and their environment [1971, 10].'

8. A Misleading Contrast?

It might be thought that the apparent contrast between intuition and observation owes itself to a sleight of hand. In the mathematical case, we cited foundational theorists. But in the scientific case, we referred to the likes of (ordinary) physicists. Arguably, the latter take a similar attitude toward the Standard Model of particle physics, General Relativity, or inflationary cosmology, as ordinary mathematicians take toward *AC*. If what matters is belief, not practice, then we should compare foundational theorists in mathematics to *foundational theorists* in science. However, controversy in the foundations of science – where questions of literal truth and falsity are at issue – seems to be as prevalent as disputation in the foundations of mathematics. Carroll recalls,

> At a workshop attended by expert researchers in quantum mechanics….Max Tegmark took an…unscientific poll of the participants' favored interpretation of quantum mechanics….The Copenhagen interpretation came in first with thirteen votes, while the many-worlds interpretation came in second with eight. Another nine votes were scattered among other alternatives. Most interesting, eighteen votes were cast for 'none of the above/undecided.' And these are the experts [2010, 402, n. 199].

Disputes about the interpretation of quantum mechanics are not about the meanings of words – notwithstanding the term 'interpretation'. They are about the reality (or lack thereof) behind the Schrodinger Equation (or its relativistic surrogate) and the Born Rule. Is there really a state vector? Does it collapse? Does consciousness have anything to do with this? Attempts to answer these questions can make different experimental predictions. For instance, the Ghirardi–Rimini–Weber (*GRW*) 'interpretation' predicts collapse in circumstances that the Copenhagen Interpretation does not. So, disagreement about the interpretation of quantum mechanics is disagreement about the non-semantic world, like disagreement in the foundations of mathematics.

But while the foundations of physics also exhibits 'divergences of viewpoint...that can easily remind one of religious, schismatic controversy', this does not conflict with the thesis that intuition varies in a way that observation does not.[32] Disputes over the interpretation of quantum mechanics are still over how best to systematize the experimental record, not the record itself. In this way, they are more like the dispute over dark matter, and unlike debates over the axioms of mathematics. For example, all interpretations agree that the Bell Inequalities have been violated. But the Everett interpretation explains this by denying that experiments have unique outcomes, the Bohmian and *GRW* interpretation do this by postulating action-at-a-distance, and superdeterminists allow that the choice of measurement is correlated with the measured system.

Is there any other way to argue that the data that is systemized in mathematics is as uniform as the data that is systematized in science? Only, it seems, if one could argue that mathematics answers to observations to the exclusion of intuitions. For instance, if the data of mathematics were limited to the observed outputs of computers, or observations of collections of things (as per Mill [1882/2009]), then mathematics would be on a par with empirical science at the level of justification. It would *be* an empirical science at the level of justification. Actually, Quine points out a more promising way that mathematics could be an empirical science. He writes,

> Objects at the atomic level and beyond are posited to make the laws of macroscopic objects, and ultimately the laws of experience, simpler….Moreover, the abstract entities which are the substance of mathematics...are another posit in the same spirit. Epistemologically these are myths on the same footing with physical objects…neither better nor worse except for differences in the degree to which they expedite our dealings with sense experiences [1951, 42].

On one reading,[33] Quine is explaining how our belief in typical theorems of mathematics, like *Euclid's Theorem*, could be empirically justified, even though there is no useful sense in which we observe infinitely-many prime numbers. Our belief in that theorem may be like our belief in gluon confinement – a theoretical postulate 'to be vindicated…by the indirect systematic contribution which [it] make[s] to the organizing of empirical data in the natural sciences [1958, 4].' But not even Quine suggests that *all* of mathematics can be so vindicated. He writes,

> I recognize indenumerable infinities only because they are forced on me by the simplest known systematizations of more welcome matters. Magnitudes in excess of such

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[32] Rickles writes, 'There exist very many ways to 'make sense' of quantum mechanics: Copenhagen, modal, relative-state, many-worlds, many-minds, Bayesian, Bohmian, Qbist, spontaneous collapse, etc. However, what one person considers to be a perfectly rational account, another might consider to be outright lunacy. There is an almost religious fervour concerning the holding of a particular stance on quantum mechanics: 'the church of Everett' versus 'the church of Bohm*'* [2016, 132].'

[33] Putnam writes that, as he prefers to formulate it, the 'argument was never intended to be an 'epistemology of mathematics.' If anything, it is a *constraint* on epistemologies of mathematics from a scientific realist standpoint [2015, 63, emphasis in original].' In other words, the indispensability of at least some mathematics to science shows that one cannot be a realist about science without being a realist about some mathematics. It does not explain the justification of our belief in mathematics. We will invoke a weakening of this constraint in Section 11.

> demands, e.g., $\beth_\omega$ or inaccessible numbers, I look upon only as mathematical recreation and without ontological rights [1986, 400].[34]

The existence of '[m]agnitudes in excess of such demands, e.g., $\beth_\omega$' is implied by *ZFC*. So, Quine is rejecting standard axioms of mathematics (presumably, the *Axiom of Replacement*).

In sum, even if a fragment of mathematics is justified by observation, the rest of mathematics must be justified differently. If that justification derives from intuition, as we have assumed, then the data supporting scientific laws is uniform in a way that the data supporting mathematical axioms is not.

9. Mathematics and Philosophy

We have been discussing analogies between mathematical and scientific knowledge. We have supposed that the justification of mathematical axioms is similar to that of scientific laws. Following Russell, axioms are postulated to systematize mathematical intuitions, as laws are postulated to systematize observations. But the analogy misleads in a way that has not been sufficiently appreciated. Mathematical disagreement characteristically bottoms out in disputes over the data to be systematized, while scientific disagreement tends not to. The view that 'mathematics begins with a small number of shared, self-evident assumptions' (Greene 1995, 401) is indeed a cartoon. But mathematics is not just like science even at the level of justification.

Where does that leave mathematics in the science-humanities continuum? *Insofar as mathematics is theoretical* – making claims on the world, like the sciences – it leaves mathematics on a par with philosophy. This includes modality (the theory of possibility and necessity), (meta)logic (the theory of what follows from what), and value (the theory of what is morally, intellectually, or otherwise good or obligatory). It is a familiar point that, with respect to *reliability*, our philosophical and mathematical beliefs may be alike. Bealer writes,

> The lack of…an explanation [of our reliability] in the case of [philosophical] intuitions makes a number of people worry about relying on intuitions. (This really is just Benacerraf's worry [quoted above] about mathematical knowledge.) [1999, 52, n. 22]

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[34] Many hold that Quine was too sanguine about how much mathematics is indispensable to empirical science. Feferman argues that Weyl's system, *W*, exhausts what is indispensable for empirical science, and that '*W* [itself can be] treated in an instrumental way, its entities outside the natural numbers are regarded as 'theoretical' entities, and the justification for its use lies in whatever justification we give to the use of *PA* [1992, 451].' Field goes so far as to maintain that we can avoid 'all appeal to mathematical entities in explanations when the chips are down: it must be possible, for instance, to develop theoretical physics without any appeal to mathematical entities [1989, 6].'

The problem of explaining the reliability of our philosophical intuitions closely resembles the problem of explaining the reliability of our mathematical intuitions. Neither an evolutionary account of how we came to have a reliable mechanism for philosophical belief, nor a neurophysical explanation of how that mechanism works such that it is reliable, is forthcoming. So, if our philosophical beliefs are reliable, this fact also requires a different kind of explanation.

What has been less widely recognized is that the *justification* of our mathematical beliefs seems to be parallel to the justification of our philosophical beliefs as well. In both cases, we are left to apply the method of reflective equilibrium to 'data points' *which are not generally accepted*. For any two mathematicians with positions on foundational questions, the equilibria that they approach will commonly appear to be mutually inconsistent, as in philosophy.[35] Lewis writes,

> Our 'intuitions' are simply opinions: our philosophical theories are the same. Some are commonsensical, some are sophisticated; some are particular; some general; some are more firmly held, some less….[A] reasonable goal for a philosopher is to bring them into equilibrium. Our common task is to find out what equilibria there are that can withstand examination, but it remains for each of us to come to rest at one or another of them… [1983: x-xi].

It is easy to miss this similarity between mathematics and philosophy on account of their divergent aims. Mathematicians want standards by which their work can be judged. The reality (or lack thereof) behind those standards is secondary, even personal. By contrast, philosophers want to know the reality behind the standards that are regulative in an area. While an oversimplification, Kreisel is not far off when he suggests that philosophy seeks 'reasons *for* axioms' while mathematical 'practice is concerned with deductions *from* axioms' [1967, 191, italics in original].[36]

We submit that this similarity between mathematics and philosophy may be characteristic of apparently a priori, or what, following Williamson [2005], we will call 'armchair', areas.[37] *Armchair disagreement tends to bottom out in disputes about the data to be systematized, while empirical scientific disagreements tend not to*. This difference illuminates why disagreements over what axioms to accept, how the world could have been, what follows from what, and what we ought to do, are intractable in a way that disagreements over whether there are gravitons

[35] 'Appear' for reasons that will emerge in Section 11.
[36] One reason that this is an oversimplification is that, ideally, at least, 'Self-criticism feeds back into mathematics and transforms it [Bevan & Paseau, 446].'
[37] The term 'armchair' is neutral on whether the relevant areas are a priori, or whether there is even a useful a priori / a posteriori distinction. As Quine [1951] argued, mathematics, logic, and fundamental ontology may merely be especially abstract branches of the empirical scientific project whose results we are particularly reluctant to amend..

seems not to be. We agree on the data that is pertinent to the latter question to an extent that we do not agree on the data that is pertinent to the former.[38]

10. Illustration: Modal Metaphysics

Consider modal metaphysics, the theory of how the world could have been. 'Applied' modal claims certainly turn in part on observation, like applied mathematical ones. For instance, assuming the *Necessity of Identity* (*NI*), $\forall x \forall y[x = y \rightarrow \Box(x = y)]$, if the mind (consciousness) is identical to the body (a physical property), then it necessarily is. The actual identity of a physical property, like *being a synchronized 35-75 hertz neural oscillation in the sensory areas of the cortex*, is an empirical matter. But basic modal principles, like *NI* itself, do not seem to admit of observational check. Disputes over them appear to bottom out in clashing intuitions, like debates over *AC* or the Axiom of Foundation. The following quotations are illustrative.

> The necessity of identity follows if we have (i) in every world 'a' and 'b' refer to what they actually refer to *and* (ii) in every world 'a' and 'b' co-refer. But we have no right to the latter claim….It follows from…(i) and the necessity of identity; but of course one can't rely on that without begging the question [Cameron 2006].[39]

> Lewis thinks this 'unrestricted composition'...is…necessary….[But w]hy suppose that…it is impossible for the world to have different principles governing the part-whole relation [Nolan 2005, 36]?

> [T]he metaphysically possible…is constrained by the laws of nature [Edgington 2004, 1].

> [C]ould this table have been made from a completely different block of wood?.... [T]hough we can imagine making a table out of another block of wood…identical in appearance with this one, and…in this very position in the room, it seems to me that this is not to imagine this table as made of wood…but rather it is to imagine another table, resembling this one…made of another block of wood…[Kripke 1980, 113-114].

> In order to provide a decisive argument for absolute motion using [Newton's] bucket (or globes) [thought experiment] we would have to empty the universe of all other matter. Though we might think we can do this in thought experiments…our intuitions might differ [and] how are we to decide between them in such cases [Rickles 2016, 46]?

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[38] Insofar disagreement over the existence of gravitons *does* appear intractable, this is arguably because the parties to it disagree over *armchair* questions like the extent to which we are justified in believing in whatever entities are referred to in the best explanation of our observations, i.e., 'inference to the best explanation' (Harman [1965]).
[39] For defenses of modal logics with contingent identity, see Wilson [1983], Girle [2017, 7.4, 8.5, and 8.6], Gibbard [1975], and Priest [2008, Ch.17].

> [T]he criteria to which one appeals in…reflection [on the essence of things] are sufficiently obscure to leave me, at least, with an embarrassingly large number of undecided cases….The existence of such cases does show the distinction [between essential and accidental attributes] is a good deal less clear than essentialists are wont to suppose [Cartwright 1968, 626].

Cartwright's lament resembles Friedman's remark that 'mathematicians [like me] disclaim… direct intuition about complicated sets of reals'. Different theorists have different intuitions, and some lack relevant intuitions altogether. But intuitions are the data to which modal theories, like set theories, respond. How are we to theorize absent agreement on the data to be 'saved'?

Modal metaphysicians could follow the lead of (ordinary) mathematicians, and adopt some generalizations (and inference rules), thereafter bracketing arguments against them (leaving those to the 'shphilosophers').[40] This would generate the appearance of metaphysical progress, as more results were 'proved'. But metaphysicians could only do this in bad faith. The sense of proof in question would be logical, not epistemic. What the 'proofs' showed about, e.g., whether the mind is identical to the body would remain controversial among those who concerned themselves with questions of literal truth. And, yet, as Williamson emphasizes, 'our task', in modal metaphysics, is precisely 'in a scientific spirit to build and test theories that codify putatively true generalizations of the sort at issue, to find out which are true [2013, 423].'

11. Disagreement, Objectivity, and Practice

Disagreement in intuition has long motivated skepticism about philosophical questions. Mogensen writes,

> Moral disagreement is widespread. Some disagreements are shallow, but others seem deep: they apparently cannot be traced to ignorance of relevant non-moral facts or failures of epistemic rationality, reflecting instead fundamental differences in people's moral intuitions….[W]e ought to suspend judgment when we encounter disagreements of this kind [2016, 1].

In light of the above, one might be tempted to argue from disagreement to skepticism about mathematics, modality, logic, and 'armchair' questions generally. Even in the case of logic, Williamson reminds us that 'If we restricted it to uncontroversial principles, nothing would be left [2012].' For example, paracomplete logicians reject the *Law of the Excluded Middle* (*LEM*), which says that, for any *P*, (*P* v ~*P*) is a logical truth. And paraconsistent logicians allow that

[40] It is arguable that this is what certain segments of the metaphysical community have actually done.

contradictions – claims of the form (*P* & ~*P*) – can, as a matter of logical possibility, be true. Unfortunately, armchair skepticism cannot be contained. It engenders empirical scientific skepticism. There is no way to be agnostic about mathematics, possibility, logic *and* every kind of value, while maintaining any belief of interest about the empirical world (Nagel [1997]). Even fictionalists, like Field [1980], require knowledge of possibility and (meta)logic in order to make the case that mathematics is conservative over 'nominalistic' theories, or that – at the appropriate energies – the world is just like the Standard Model says it would be if (ignoring gravity and dark matter and energy) there were mathematical entities (Rosen [2001, 75]).

There is an alternative to skepticism about armchair areas, however. Instead of amending our epistemology, we could amend our metaphysics. Mackie writes,

> Disagreement on questions of history, biology, or cosmology does not show that there are no objective issues for investigators in these fields to disagree about. But such scientific disagreement results from speculative inferences or explanatory hypotheses based on inadequate evidence, and it is hardly plausible to interpret moral disagreements in the same way [1977, 37].[41]

By 'objective' Mackie means mind-and-language independent. Again, we cannot give up on the mind-and-language independent truth of mathematics, logic, and so forth without giving up on the mind-and-language independent truth of the scientific theories that presuppose them.[42] However, there is another sense of 'objective' in which we could give up on the objectivity of armchair facts, including (pure) mathematical ones, while retaining belief in their mind-and-language independence – i.e., while retaining commitment to armchair *realism*.

Consider the ordinary platonist's conception of pure (rather than applied) geometry. Platonists do not believe that the truth of the Parallel Postulate depends on our minds or languages. That postulate would have been true (of Euclidean space) even if no one had existed. But it is *as if* its truth depended on our conventions. Where a relativist claims that the Parallel Postulate is true according to the framework of Euclidean geometry, and false according to the framework of, say, hyperbolic geometry, a platonist claims that that the Parallel Postulate (syntactically individuated) is true of the $lines_{Euclidean}$ and false of the $lines_{hyperbolic}$. Rather than switching frameworks on which to relativize, the platonist switches kindred subjects. In both cases, the methodological consequences are the same: 'the conflict between the divergent points of view…disappears …..[B]efore us lies the boundless ocean of unlimited possibilities [Carnap 1937/2001, XV].' There is enough *mind-and-language independent reality* to go around.

[41] See Leiter [2009] for a related argument.
[42] Of course, some do give up on this. Dummett [1976] argues from intuitionistic logic to a kind of physical antirealism. But we assume realism – to be shortly distinguished from *objectivism* – about the physical world throughout this article.

This suggests the following proposal. Maybe *armchair questions generally are like the Parallel Postulate question, according to the mathematical platonist*.[43] Does every set have a Choice function? All sets_{ZFC} do, but some $\text{sets}_{ZF + AD}$ do not. Could Kripke's table have been made of a different piece of wood? This is $\text{possible}_{\text{Logical}}$, but not $\text{possible}_{\text{Metaphysical}}$. Does 'the moon is made of green cheese' follow from 'snow is white and snow is not white'? It $\text{follows}_{\text{Classical}}$ but does not $\text{follow}_{\text{Paraconsistent}}$. And so on. While each of these claims is true independent of us, its truth is like that of the Parallel Postulate, for the platonist. A claim 'just like' its negation is also true.

How does this view help with the problem of mathematical, and more generally, armchair knowledge? If *armchair pluralism* (as we will call the resulting view) is correct, then intuitions about *AC* may vary in the banal sense that our intuitions about the Parallel Postulate do. Different intuitions may be about different subjects. Intuitions supporting *AC* may be about one class of set-like universes, while intuitions against it may be about another. The fact that our intuitions of Euclidean space diverge from our intuitions of hyperbolic space does nothing to call into question intuition's status as a *justifier*. The same may be true of our intuitions as to whether *AC*. It would be if our intuitions fixed the contents of our set theoretic beliefs – that is, if those contents were *determined* by what 'follow from, the…intuitions' that we have (Balaguer [2001, 90]).

Armchair pluralism also suggests an explanation of the *reliability* of our beliefs. Had we had an intuition in favor of *~AC* – as it seems that we could have easily – we would have believed something true about a different subject (again, if the 'cooperative metasemantics' alluded to in the last paragraph is correct). We would have had beliefs about sets* rather than sets per se. Similarly, had we had the intuition that not everything follows from a contradiction, we would have had beliefs about paraconsistent consequence relations, not logical relations (assuming that we actually pick out a non-paraconsistent consequence relation with the term, 'consequence'). All of these are real. So, there is no question of who is really right. The classicist is right of classical consequence and the paraconsistent logician is right of paraconsistent consequence!

Armchair pluralism, thus, invites a kind of *pragmatism*. The only non-semantic questions (i.e., questions that are not just about what we happen to mean by words) in armchair debates are *practical*. Instead of asking whether every set has a Choice function, we ask the practical

[43] Objectivist realists often note that there are important differences between the Parallel Postulate and set-theoretic undecidables like the Continuum Hypothesis (*CH*) (Kennedy & van Atten [2004, Sec. 5.1]). For example, the Parallel Postulate is undecidable in Hilbert's second-order axiomatization, but second-order *CH* is decidable. The significance of this difference is doubtful, though. The sense in which *CH* is 'decidable' is that, given a full semantics for second-order logic, $ZFC_2 \vDash CH$ or $ZFC_2 \vDash \neg CH$. *CH* is not decided in the usual, syntactic sense (second-order logic is incomplete). Also, second-order logic assumes a metatheory – some set theory or something just as epistemically (if not ontologically) obscure. Even if Quine was wrong that second order logic is 'set theory in sheep's clothing', knowledge of second-order consequence is hardly more intelligible than knowledge of mathematics.

question of *whether to use* a concept of set that satisfies the Axiom of Choice (for a purpose).[44] Instead of asking whether the mind could have been different from the body, we ask whether to use a concept of possibility that satisfies the Necessity of Identity. And so forth. Since divergent theories are all true – albeit of subtly different subjects – truth factors out. It is as if armchair claims did not admit of truth at all. Intractable armchair questions get traded for practical ones.[45]

It might be worried that armchair pluralism robs armchair questions of interest. But this assumes that their intellectual value depends on their answering to a unique and independent reality. Why should that be? Even if there were 'one true *V*', for example, this may be less interesting than universes of our imagination – as it presumably is, according to advocates of 'large' large cardinals, if $V = L$ is true. Metaphysics does not settle questions of intellectual value. This is just an application of Hume's point that one cannot derive an (in this case, epistemic) 'ought' from an 'is'.

It remains to say how armchair pluralism applies to questions of value themselves. It radicalizes Hume's dictum. If evaluative pluralism is correct, then there is a property answering to Mill's utilitarianism alongside a property answering to Kant's deontology -- and a property answering to every other such theory (as in the pure geometric case). Thus, as in set theory, modality, or logic, there is no useful question of whether, e.g., we *really* ought to kill the one to save the five (in some concrete situation). We $\text{ought}_{\text{utilitarian}}$, and ought $\text{not}_{\text{deontological}}$! Instead, we need to ask *whether to use* a utilitarian or deontological concept of ought. But there is an intimate connection between *this* practical question and a question of fact from the target domain. Evaluative facts, unlike mathematical, modal, or logical ones, are supposed to tell us what to do. So, what happens when we replace the practical question of whether to use a given concept with the factual question of whether we *ought* to use it? The argument reapplies! We $\text{ought}_{\text{utilitarian}}$ to use $\text{ought}_{\text{utilitarian}}$, and $\text{ought}_{\text{deontological}}$ to use $\text{ought}_{\text{deontological}}$ (and so on for every concept of ought).[46] The question of *which concept to use* remains open. This cannot be the question of which concept we *ought* to use on pain of triviality. (We ought to use ought, ought* to use ought*, and so forth.) Hence, the facts, *even the evaluative ones* (if there are any), fail to settle what to do.[47]

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[44] We speak of concepts for readability. One who is skeptical of them, like Quine, is free to replace our talk of concepts with talk of (formal) theories (set theories, modal theories, evaluative theories, and so on).

[45] A distinct argument for armchair pluralism about mathematics, in particular, comes from the history of mathematics. Unlike physics, chemistry, or biology, mathematical theories of the past (even going back to antiquity) do not seem to be jettisoned in light of newer theories. They are absorbed into the body of accepted mathematics through a process of reinterpretation and widening of the relevant axiomatic system. See Crowe [1975]. A liability of this argument is that it may trade on an equivocation with 'theories'. *Sentences* are certainly preserved. But what matters is whether the *propositions expressed by them* are. This is more far less straightforward.

[46] Or, at least, for any self-sanctioning concept of ought. 'Self-hating' concepts of ought, ought**, such that we ought** not to use ought** are presumably not attractive guides to decision.

[47] How do we decide what to do if knowledge of the facts fails to settle this? We decide via practical reasoning. For approaches to the logic and semantics of practical reasoning, non-factually construed, see Blackburn [1984], Gibbard [2003], Schroeder [2012], and Silk [2015].

12. Implications for Physics

Before concluding, let us illustrate some consequences of armchair pluralism for fundamental physics. Just as armchair anti-realism engenders scientific anti-realism, armchair pluralism leads to scientific pluralism. The physical world becomes *perspectival* to a surprising degree.

Why is this the case? After all, the ordinary platonist's pluralism about pure geometry does not commit them to pluralism about physical space(time). It merely shows that, of the myriad pure geometries, at most one of them precisely models our universe. The reason is that the set theoretic, logical, and modal cases are different from pure geometry in a crucial respect. Different geometries can all be realized in a single metatheory, like some set theory. But every set theory, plus a logic and modal theory, *is* a metatheory, with implications for all reasoning.

Consider the intersection of a family of sets of points in spacetime, $\cap x$.[48] Does it exist? In order to prove that it does, we must form $\{y: y \in \cup x \,\&\, (\forall z)(z \in x \rightarrow y \in z)\}$. This requires the (restricted) *Comprehension Axiom* schema, which says that, for any set, z, and any formula, Φ, there is a set containing those members of z that satisfy Φ. That is, $(\forall z)(\exists y)(\forall x)(x \in y \longleftrightarrow (x \in z \,\&\, \Phi))$ [where y is not free in Φ]. The existence of the union of the sets in x, $\cup x$, is given by the *Union Axiom*, that for any set, z, there is a set, $\cup z$, containing the members of members of z. In symbols: $(\forall z)(\exists y)(\forall x)(x \in y \longleftrightarrow (\exists w)(w \in z \,\&\, x \in w))$. Finally, the points that get collected in the first place are represented by ordered quadruples, understood as sets, constructed out of natural numbers – whose existence is given by the *Axiom of Infinity* – by way of repeated applications of Comprehension and the *Powerset Axiom*, that, for any set, z, there is a set containing the subsets of z, *P*(z), written $(\forall z)(\exists y)(\forall x)(x \in y \longleftrightarrow (\forall w)(w \in x \rightarrow w \in z))$. But not all set theories countenance Infinity and Powerset. For example, Kripke–Platek set theory lacks the Powerset, and sometimes Infinity, axioms. So, whether $\cap x$ exists – what might have seemed to be an objective physical question – depends on the set theory that we use to check.[49]

The logical and modal cases are similar. Let *T* be a regimented physical theory, such as General Relativity or the Standard Model.[50] Does *T* imply *O*, where *O* is some observation statement that has been refuted (i.e., we have that ~*O*)? This depends on whether *T* is (classically) consistent.

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[48] This example is a variation on that of Clarke-Doane [2022, 2.1].

[49] A simpler argument with a more complicated conclusion appeals to the Banach-Tarski paradox. Is it physically possible to cut a (solid) ball into a finite number of pieces, and, using only rigid motion, construct two balls of the same size? Yes, relative to *ZFC*, and no relative to *ZF* + *AD*. Or, to take an example from the mathematical physics literature, 'the spacetime manifold *M* is partially ordered by the chronology relation, and any timelike curve *g* on *M* is simply ordered by the same relation. Therefore, *on the strength of the maximum principle* [which is equivalent to *AC*] there is a subset *h* of *M*, which is simply ordered by the chronology relation, contains *g* and is maximal with respect to these properties of *M*….[T]he set *h* is a timelike curve [Heller 1986, 64, italics added].'

[50] We indulge here in the fiction that there is a provably consistent axiomatization of quantum field theory (in four dimensions) with interactions.

If ~Con(*T*), then *T* implies everything, including *O*. So, prima facie, one way that it could fail to be objective whether *T* has been refuted is if it failed to be objective whether Con(*T*) – and, if Con(*T*), then *T* did not imply *O*. We say 'prima facie' because, if Con(*T*), then ~Con(*T*) is only true in a nonstandard model where 'proof' and 'theory' are interpreted non-standardly so that, perhaps, ~Con(*T*) does not say *of T* that it is *inconsistent*. (There is a similar prima facie argument that there would be no objective fact as to whether a physical computer computing a Turing uncomputable function would eventually halt.) But there is another way that it could fail to be objective whether *T* implies *O*. It may fail to be objective *what logic is correct*, as it is if armchair pluralism is true. In that case, if *O* is classically, but not intuitionistically, derivable from *T*, then the question of whether *T* has been refuted by *O* would be like the question of whether the Parallel Postulate is true, understood as one of pure mathematics for the platonist.

Finally, consider a quantum spin system, *S*, with eigenstates $|\uparrow>$ and $|\downarrow>$. Could *S* have been in the indeterminate state of being *neither* $|\uparrow>$ *nor* $|\downarrow>$ *nor* $a|\uparrow> + b|\downarrow>$, for all complex numbers, *a* and *b*? What about the contradictory state $|\uparrow>$ *and* $|\downarrow>$? As a matter of metaphysical, a fortiori physical, possibility, the answer to both of these questions is, of course, 'no'. But if the kind of possibility at issue is paracomplete, allowing for the possibility of indeterminacies, then the state, *neither* $|\uparrow>$ *nor* $|\downarrow>$ *nor* $a|\uparrow> + b|\downarrow>$, will be in *S*'s state space. If it is paraconsistent, then the state $|\uparrow>$ *and* $|\downarrow>$ will be in that space. And if *First-Degree Entailment* (*FDE*) governs the possible states of the system, then *neither* $|\uparrow>$ *nor* $|\downarrow>$ *nor* $a|\uparrow> + b|\downarrow>$ as well as $|\uparrow>$ *and* $|\downarrow>$ will all be possible states of the system, *S*.[51] In general, if there is no objective fact as to which kinds of possibility are real, then there is no objective fact as to the state space of a physical system.

To be clear: this does not mean that we should expect to observe indeterminate or contradictory states, anymore than the existence of metaphysically possible but physically impossible states in which a projectile accelerates above *c* shows that we should expect to observe those (or, indeed, the existence of metaphysically *impossible* states shows that we should expect to observe them). One question is how the world could have been. This is not objective, on the current view. Another question is how it *will* be – which is objective, modulo pluralism about set theory.

Although these implications may seem hard to swallow, the alternative is arguably more so. It is widely conceded that not all aspects of our best theories reflect objective features of the world. Those theories include a representational apparatus that we bring to the table. For instance, the world itself may not decide the fine details of spacetime structure. But if that just means that, e.g., whether ∩x above exists depends on our beliefs, then we seem to be committed to the view that spacetime would be amorphous if none of us believed otherwise.[52] Armchair pluralism allows that all of the facts of spacetime are equally independent of us. The sense in which some

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[51] For details of how to combine non-classical and modal logics, see Priest [2014, Sec. 11].
[52] A tame variation on this idea is due to Grünbaum [1973], who argues that spacetime is metrically amorphous.

of them are not objective is just that reality is rich enough to witness any conception of them that we might have had. Which conception is 'right' is a practical question, not settled by the facts.[53]

13. Armchair Pluralism

We have argued that intuitions vary in a way that observations do not, and that this favors an anti-objectivist, but realist, conception of armchair areas – armchair pluralism. Armchair reality is so rich that any set-theoretic, modal, logical, or evaluative theory that we might have adopted is true of its intended subject. While we can always ask what is packed into the concepts of set, possibility, implication, and obligation that we find ourselves with, this autobiographical exercise is of no *methodological* import. It just raises the question of whether to use our present concepts rather than alternatives.[54] The only non-semantic questions are *practical*: which concepts to use?

The foundations of armchair pluralism require in-depth treatment. Three questions are pressing.

First, with respect to what subjects exactly is pluralism tenable? Pluralism itself would seem to be an armchair theory. It is a product of philosophy, which we have arrived at by reasoning. Moreover, our views on pluralism seem to share the contingent profile of our mathematical, modal, logical, and moral ones. So, the above argument would appear to show that whether pluralism is correct *itself* lacks an objective (even if not independent) answer (because there are subtly different senses of 'objective' giving intuitively opposite verdicts – as with the Parallel Postulate). Is that so? Or is armchair pluralism itself objectively (and independently) correct?[55]

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[53] It might be thought that one could appeal to the concept of *naturalness* in the sense of Lewis [1983] and Sider [2011] in order to give epistemic (or 'theoretical') content to the claim that one conception of set, possibility, consequence, or value right. But either the claim that some such conception is natural is itself evaluative (with implications for which is *good* or which *ought* to use) or not. If it is, then the problem of pluralism rearises vis-à-vis naturalness. Conception$_1$ may be natural$_1$ but not natural$_2$. What could the claim that naturalness$_1$ is *real* naturalness amount to if not just something about what we happen to mean by the word, 'naturalness'? Alternatively, the claim that some conception is natural is not normative. But then it is neither here nor there from the methodological standpoint of which to use, for familiar is/ought reasons. See Clarke-Doane [2020, Sec. 6.6].

[54] In other words, if armchair pluralism is correct, then the '[m]any set theorists, including Gödel' who 'believe that conceptual analysis will eventually lead to an idea of a set so clear and distinct that the answer to the continuum question will become apparent' [Huber-Dyson 1991, 9] are misguided *even if* their belief is true.

[55] It might be thought that there are subtly different senses of (counterfactually) 'independent' giving rise to different concepts of realism as well. But if these senses correspond to more or less inclusive classes of worlds with respect to which we evaluate the counterfactuals, then realism is not thus ambiguous if the following principle is true.

> *Counterfactual Absoluteness* (Clarke-Doane [2019, Section 7]): Suppose that $\forall P\ ([N+]P \rightarrow [N]P)$, but not conversely. Then, if $(A\ [] \!\rightarrow B)$ is non-vacuously true with respect to a model, $N = <D, S, V, @>$, where $w \in D$ just in case w is N-possible, then $(A\ [] \!\rightarrow B)$ remains true with respect to a model, $N^+ = <D', S', V', @>$ where $w \in D'$ just in case w is N^+-possible, whenever N is a submodel of N^+ (where N and N^+ are models of propositional conditional logic, where S is a class of relations, one for each formula of the language, D is a domain, V is a valuation, and @ is the actual world).

Second, how liberal should our pluralism about a subject be? It is natural to be as liberal as we can consistently be. But, roughly, Gödel's Second Incompleteness Theorem says that it is consistent to say false things about consistency (if weak theories of arithmetic are consistent).[56] Is a more stringent criterion needed? Even if not, logical pluralism generates different versions of the consistency requirement, depending on the logic.[57] So, in order to know how rich armchair reality is, we must know how rich logical reality is. In [Clarke-Doane 2019], one of us argues that logical reality is *indefinitely extensible*. Given a logic, we can always intelligibly weaken it (but the 'trivial logic', according to which nothing follows from anything, is unintelligible as an all-purpose reasoning device). And, yet, as the locution 'can...weaken it' suggests, indefinite extensibility is normally taken to be a modal notion. We cannot take it that way if modal reality -- the way the world *could have been* -- is itself indefinitely extensible too.

Finally, what kind of framework exactly is armchair pluralism? It cannot be a theory in the formal sense. Any formalization of it would assume a metatheory of its own which would wrongly take itself to be maximal. Is it a truth that one can 'show but not say'? Or is it an attitude, not apt for representing the world?[58] If our intuitions are not generally 'seriously inadequate and badly distorted' (Mackie [1977, 75]), each of these questions has an answer.

Counterfactual Absoluteness says that once one finds a witness for the antecedent of a true counterfactual in some *N*-possible world, one cannot flip its truth-value by ascending to a more absolute modality, *N*+. It is, therefore, a relativized version of Nolan's 'Strangeness of Impossibility' condition (Nolan [1997]). If counterfactuals like 'had our mathematical beliefs been different, the truths would have been the same' are non-vacuously true, then they remain true as we add more worlds to our model. So, mathematical realism's truth is indifferent to this variation.

[56] 'Roughly speaking' because, again, one could argue that, e.g., *ZF* + ~Con(*ZF*) asserts the 'shinconsistency' of the 'Sh*ZF* axioms', not the inconsistency of the *ZF* axioms. See, e.g., Azzouni [Forthcoming].

[57] Note that armchair pluralism does not countenance inconsistent mathematics (in the sense of Mortensen [2022]) just because it encompasses paraconsistent logics. Logics are about what follows from what, not what (non-metalogical) claims are (actually) true(though of course the claim that *S* is valid implies that *S* is true). The claim that any contradiction is actually true is a further commitment on which paraconsistent logics are silent.

[58] See Clarke-Doane [Forthcoming]. In the set-theoretic case, pluralism (or something like it) is often treated as an unformalizable theory (Balaguer [1995, 6]). See also Hamkins [2012, 417]. Shelah writes, 'My mental picture is that we have many possible set theories….I do not feel 'a universe of *ZFC*' is like 'the Sun', it is rather like 'a human being'...[2003, 211].' But Shelah does not go on to say what the force of calling something a 'mental picture' is. (Cohen's method of forcing was the impetus for pluralism among set theorists. Bell [1981, 358] writes, 'The techniques [Cohen] invented have led to an enormous proliferation of essentially different models of set theory and the rise of a 'relativistic' attitude toward the set-theoretical foundations of mathematics. This attitude involves abandoning…the idea that mathematical constructions should be viewed as taking place within an 'absolute' universe of sets with fixed and predetermined properties. Instead, one works in suitably chosen *models* of set theory having the properties required to carry out the construction in question (italics in original).' However, for reasons discussed here, we do not see an *epistemological* difference between axioms whose independence is proved via forcing and the others, like Foundation or Infinity. We are also skeptical of a metaphysical/semantic difference. See Clarke-Doane [2012].)

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