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The Myth of the Seven

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Mathematics has been called the one area of inquiry that would retain its point even were the physical world to disappear entirely. This might be heard as an argument for platonism: the view that mathematics describes a special abstract department of reality lying far above the physical fray. The necessary truth of mathematics would be due to the fact that the mathematical department of reality had its properties unchangingly and essentially.

I said that it *might* be heard as an argument for platonism, that mathematics stays on point even if the physical objects disappear. However mathematics does not lose its point either if the *mathematical* realm disappears—or, indeed, if it turns out that that realm was empty all along. Consider a fable from John Burgess and Gideon Rosen's book *A Subject with No Object*:

> Finally, after years of waiting, it is your turn to put a question to the Oracle of Philosophy... you humbly approach and ask the question that has been consuming you for as long as you can remember: 'Tell me, O Oracle, what there is. What sorts of things exist?' To this the Oracle responds: 'What? You want the whole list?... I will tell you this: everything there is is concrete; nothing there is is abstract....'
>
> (Burgess and Rosen, 1997: 3)

Trembling at the implications, you return to civilization to spread the concrete gospel. Your first stop is [your university here], where researchers are confidently reckoning validity in terms of models and insisting on 1-1 functions as a condition of equinumerosity. Flipping over some worktables to get their attention, you demand that these practices be stopped at once. The entities do not exist, hence all theoretical reliance on them should cease. They, of course, tell you to bug off and am-scray. (Which, come to think of it, is exactly what you yourself would do, if the situation were reversed.)

Frege's Question

Frege in *Notes for L. Darmstaedter* asks, 'is arithmetic a game or a science?'[1] He himself thinks that it is a science, albeit one dealing with a special sort of logical object.[2] Arithmetic considered all by itself, just as a formal system, gives, in his view, little evidence of this: 'If we stay within [the] boundaries [of formal arithmetic], its rules appear as arbitrary as those of chess' (*Grundgesetze* II, section 89).[3] The falsity of this initial appearance is revealed only when we widen our gaze and consider the role arithmetic plays in our dealings with the natural world. According to Frege, 'it is applicability alone which elevates arithmetic from a game to the rank of a science' (*Grundgesetze* II, section 91).

One can see why applicability might be thought to have this result. What are the chances of an arbitrary, off the shelf, system of rules performing so brilliantly in so many theoretical contexts? Virtually nil, it seems; 'applicability cannot be an accident' (*Grundgesetze* II, section 89). What else could it be, though, if the rules did not track some sort of reality? Tracking reality is the business of science, so arithmetic is a science.[4]

The surprising thing is that the same phenomenon of applicability that Frege cites in *support* of a scientific interpretation has also been seen as the primary *obstacle* to such an interpretation. Arithmetic qua science is a deductively organized description of *sui generis* objects with no connection to the natural world. Why should objects like that be so useful in natural science = the theory of the natural world? This is an instance of what Eugene Wigner famously called 'the unreasonable effectiveness of mathematics'.[5]

Applicability thus plays a curious double role in debates about the status of arithmetic, and indeed mathematics more generally. Sometimes it appears as a *datum*, and then the question is, what *lessons* are to be drawn from it? Other times it appears as a *puzzle*, and the question is, what *explains* it, how does it work?

Hearing just that applicability plays these two roles, one might expect the puzzle role to be given priority. That is, we draw such and such lessons because they are the ones that emerge from our story about how applications in fact work.

But the pattern has generally been the reverse.[6] The first point people make is that since applicability would be a miracle if the mathematics involved were not true, it is evidence that mathematics *is* true. The second thing that gets said (what on some theories of evidence is a corollary of the first) is that applicability is explained in part by truth. It is admitted, of course, that truth is not the full explanation.[7] But the assumption appears to be that any further considerations will be specific to the mathematics involved and the application.[8] The most that can be said in *general* about why mathematics applies is that it is true.

One result of this ordering of the issues is that attention now naturally turns away from applied mathematics to pure. Why should we worry about the bearing of mathematical theories on physical reality when we have yet to work out their relation to mathematical reality? And so the literature comes to be dominated by a problem I will call *purity*: given that such and such a mathematical theory is true, what makes it true? Is arithmetic, for instance, true in virtue of (a) the behavior of particular objects (the numbers), or (b) the behavior of ω-sequences in general, or (c) the fact that it follows from Peano's axioms? If (a), are the numbers sets, and if so which ones? If (b), are we talking about actual or possible ω-sequences? If (c), are we talking about first-order axioms or second?

Some feel edified by the years of wrangling over these issues, others do not. Either way it seems that something is getting lost in the shuffle, viz., applications. Having served their purpose as a dialectical bludgeon, they are left to take care of themselves. One takes the occasional sidelong glance, to be sure. But this is mainly to reassure ourselves that as long as mathematics is true, there is no reason why empirical scientists should not take advantage of it. That certainly speaks to one possible worry about the use of mathematics in science, namely, is it defensible or something to feel guilty about? But our worry was different: Why should scientists *want* to take advantage of mathematics? What good does it do them? What sort of advantage is there to be taken? The reason this matters is that, depending on how we answer, the pure problem is greatly transformed. It could be, after all, that the kind of help mathematics gives is a kind it could give *even if it were false*. If that were so, then the pure problem—which in its usual form presupposes that mathematics is true—will need a different sort of treatment than it is usually given.

Retooling

Here are the main claims so far. Philosophers have tended to emphasize *purity* over *applicability*. The standard line on applicability has been that (i) it is evidence of truth, (ii) truth plays some small role in explaining it, and (iii) beyond that, there is not a whole lot to be said.[9]

A notable exception to all these generalizations is the work of Hartry Field. Not only does Field see applicability as centrally important, he dissents from both aspects of the 'standard line' on it. Where the standard line links the utility of mathematics to its truth, Field thinks that mathematics (although certainly useful) is very likely *false*. Where the standard line offers little *other* than truth to explain usefulness, Field lays great stress on the notion that mathematical theories are *conservative* over nominalistic ones, i.e., any nominalistic conclusions that can be proved with mathematics can also be proven (albeit often much less easily) without it.[10] The utility of mathematics lies in the *no-risk deductive assistance that it provides to the beleaguered theorist*.

This is on the right track, I think. But there is something strangely half-way about it. I do not doubt that Field has shown us a way in which mathematics *can* be useful without being true. It can be used to facilitate deduction in nominalistically reformulated theories of his own device: theories that are 'qualitative' in nature rather than quantitative. This leaves more or less untouched, however, the problem of how mathematics *does* manage to be useful without being true. It is not as though it benefits only practitioners of Field's qualitative science (it does not benefit Field-style scientists at all; there aren't any). The people whose activities we are trying to understand are practicing regular old platonic science.

How without being true does mathematics manage to be of so much help to *them*? Field never quite says.[11] He is quite explicit, in fact, that the relevance of his argument to *actual* applications of mathematics is limited and indirect:

> [What I have said] is not of course intended to license the use of mathematical existence assertions in axiom systems for the particular sciences: *Such* a use of mathematics remains, for the nominalist, quite illegitimate. (Or, more accurately, a nominalist should treat such a use of mathematics as a temporary expedient that we indulge in when we don't know how to axiomatize the science properly.)
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> (1980: 14)

But then how exactly does he take himself to be addressing our actual situation? I see two main options.

Field might think that the role of mathematics in the *non*-nominalistic theories that scientists really use is *analogous* to its role in connection with his custom-built nominalistic theories—enough so that by explaining and justifying the one he has explained and justified the other. If that were Field's view, then one suspects he would have done more to develop the analogy.

Is the view, then, that he has *not* explained (or justified) actual applications of mathematics—but that is OK because, come the revolution, these actual applications will be supplanted by the new-style applications of which he *has* treated? This stands our usual approach to recalcitrant phenomena on its head. Usually we try to theorize the phenomena that we find, not popularize the phenomena we have a theory of.

Indispensability and Applicability

As you may have been beginning to suspect, these complaints have been based on a deliberate misunderstanding of Field's project.[12] It is true that he asks:

(d) What sort of account is possible of how mathematics is applied to the physical world?

(Field, 1980, vii)

But this can mean either of two things, depending on whether one is motivated by an interest in *applicability*, or an interest in *indispensability*.

Applicability is, in the first instance, a *problem*: the problem of explaining the effectiveness of mathematics. It is also, potentially, an *argument* for mathematical objects. For the best explanation may require that mathematics is true.

Indispensability is, in the first instance, an *argument* for the existence of mathematical objects. The argument is normally credited to Quine and Putnam. They say that since numbers are indispensable to science, and we are committed to science, we are committed to numbers. But, just as applicability was first a problem, second an argument, indispensability is first an argument, second a problem. The problem is: How do nominalists propose to deal with the fact that numbers have a *permanent* position in the range of our quantifiers?

Once this distinction is drawn, it seems clear that Field's concern is more with indispensability than applicability. His question is:

(d-ind) How can applications be conceived so that mathematical objects come out dispensable?

To *this*, Field's two-part package of (i) nominalistically reformulated scientific theories, and (ii) conservation claims, seems a perfectly appropriate answer. But we are still entitled to wonder what Field would say about:

(d-app) How are actual applications to be understood, be the objects indispensable or not?

If there is a complaint to be made, it is not that Field has given a bad answer to (d-app), but that he doesn't address (d-app) at all, and the resources he provides do not appear to be of much use with it.

Now, Field *might* reply that the indispensability argument is the important one. But that will be hard to argue. One reason, already mentioned, is that a serious mystery remains even if in-principle dispensability is established. How is the Fieldian nominalist to explain the usefulness-without-truth of mathematics in *ordinary*, quantitative, science? More important, though, suppose that an explanation can be given. Then *indispensability becomes a red herring*. Why should we be asked to *demathematicize* science, if ordinary science's mathematical aspects can be understood on some other basis than that they are true? Putting both of these pieces together: The point of nominalizing a theory is not achieved unless a further condition is met, given which condition there is no longer any need to nominalize the theory.

Non-Deductive Usefulness

That is my first reservation about Field's approach. The second is related. Consider the kind of usefulness-without-truth that Field lays so much weight on; mathematics thanks to its conservativeness gives no-risk deductive assistance. It is far from clear why *this particular form* of usefulness-without-truth deserves its special status. It might be thought that there is no other help objects can give without going to the trouble of existing. Field says the following:

> if our interest is only with inferences among claims that don't say anything about numbers (but which may employ, say, numerical quantifiers), then we can employ numerical theory without harm, for we will get no conclusions with numerical theory that wouldn't be valid without it... There are other purposes for which this justification for feigning acceptance of numerical theory does not apply, and we must decide whether or not to genuinely accept the theory. For instance, there may be observations that we want to formulate that we don't see how to formulate without reference to numbers, or there may be explanations that we want to state that we can't see how to state without reference to numbers... *if such circumstances do arise, then we will have to genuinely accept numerical theory if we are not to reduce our ability to formulate our observations or our explanations*
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> (Field, 1989: 161–2, italics added).

But, *why* will we have to accept numerical theory in these circumstances? Having just maintained that the *deductive* usefulness of *X*s is not a reason to accept that *X*s exist, he seems now to be saying that *representational* usefulness is another matter. One might wonder whether there is much of a difference here. I am not denying that deductive usefulness is an important nonevidential reason for making as if to believe in numbers. But it is hard to see why representational usefulness isn't similarly situated.[13]

Numbers as Representational Aids

What is it that allows us to take our uses of numbers for deductive purposes so lightly? The deductive advantages that 'real' *X*s do, or would, confer are (Field tells us) equally conferred by *X*s that are just 'supposed' to exist. But the same would appear to apply to the representational advantages conferred by *X*s; these advantages don't appear to depend on the *X*s really existing either. The economist need not believe in the average family to derive representational advantage from it ('the average family has 2.7 bank accounts'). The psychiatrist need not believe in libido or ego strength to derive representational advantage from them. Why should the physicist have to believe in numbers to access new contents by couching her theory in numerical terms?

Suppose that our physicist is studying escape velocity. She discovers the factors that determine escape velocity and wants to record her results. She knows a great many facts of the following form:

(A) A projectile fired at so many meters per second from the surface of a planetary sphere so many kilograms in mass and so many meters in diameter will (will not) escape its gravitational field.

There are problems if she tries to record these facts without quantifying over mathematical objects, that is, using just numerical adjectives. One is that, since velocities range along a continuum, she will have to write uncountably many sentences, employing an uncountable number of distinct adjectives. Second, almost all reals are 'random' in the sense of encoding an irreducibly infinite amount of information.[14] So, unless we think there is room in English for uncountably many semantic primitives, almost all of the uncountably many sentences will have to be infinite in length. At this point someone is likely to ask why we don't drop the numerical-adjective idea and say simply that:

(B) For all positive real numbers M and R, the escape velocity from a sphere of mass M and diameter 2R is the square root of 2GM/R, where G is the gravitational constant.

Why not, indeed? To express the infinitely many facts in finite compass, we bring in numbers as representational aids. We do this despite the fact that what we are trying to get across has nothing to do with numbers, and could

be expressed without them were it not for the requirements of a finitely based notation.

The question is whether functioning in this way as a representational aid is a privilege reserved to existing things. The answer appears to be that it isn't. That (B) succeeds in gathering together into a single content infinitely many facts of form (A) owes nothing whatever to the real existence of numbers. It is enough that *we understand what (B) asks of the non-numerical world*, the numerical world taken momentarily for granted.[15] How the real existence of numbers could help or hinder that understanding is difficult to imagine.

An oddity of the situation is that Field makes the same sort of point himself in his writings on truth. He thinks that 'true' is a device that exists 'to serve a certain logical need'—a need that would also be served by infinite conjunction and disjunction if we had them, but (given that we don't) would go unmet were it not for 'true'. No need then to take the truth-predicate ontologically seriously; its place in the language is secured by a role it can fill quite regardless of whether it picks out a property. It would seem natural for Field to consider whether the same applies to mathematical objects. Just as truth is an essential aid in the expression of facts not about truth (there is no such property), perhaps numbers are an essential aid in the expression of facts not about numbers (there are no such things).[16]

Our Opposite Fix

To say it one more time, the standard procedure in philosophy of mathematics is to start with the pure problem and leave applicability for later. It comes as no surprise, then, that most philosophical theories of mathematics have more to say about what makes mathematics true than about what makes it so useful in empirical science.

The approach suggested here looks to be in an opposite fix. Our theory of applications is rough but not non-existent. What are we going to say, though, about pure mathematics? If the line on applications is right, then one suspects that arithmetic, set theory, and so on are largely untrue. At the very least, then, the problem of purity is going to have to be reconceived. It cannot be: In virtue of what is arithmetic true? It will have to be: How is the line drawn between 'acceptable' arithmetical claims and 'unacceptable' ones? And it is very unclear what acceptability could amount to if it floats completely free of truth.

Just maybe there is a clue in the line on applications. Suppose that mathematical objects 'start life' as representational aids. Some systems of mathematicalia will work better in this capacity than others, e.g., standard arithmetic will work better than a modular arithmetic in which all operations are 'mod *k*', that is, when the result threatens to exceed *k* we cycle back down to 0. As wisdom accumulates about the kind(s) of mathematical system needed, theorists develop an intuitive sense of what is the right way to go and what is the wrong way. Norms are developed that take on a life of their own, guiding the development of mathematical theories past the point where natural science greatly cares. The process then begins to feed on itself, as descriptive needs arise w.r.t., not the natural world, but *our system of representational aids as so far developed.* (After a certain point, the motivation for introducing larger numbers is the help they give us with the mathematical objects already on board.) These needs encourage the construction of still further theory, with further ontology, and so it goes.

You can see where this is headed. If the pressures our descriptive task exerts on us are sufficiently coherent and sharply enough felt, we begin to feel under the same sort of external constraint that is encountered in science itself. Our theory is certainly answerable to *something*, and what more natural candidate than the *objects* of which it purports to give a literally true account? Thus arises the feeling of the objectivity of mathematics qua description of mathematical objects.

Some Ways of Making As If[17]

I can make the above a bit more precise by bringing in some ideas of Kendall Walton's about 'making as if'. The thread that links as-if games together is that they call upon their participants to pretend or imagine that certain things are the case. These to-be-imagined items make up the game's *content*, and to elaborate and adapt oneself to this content is typically the game's very point.[18] At least one of the things we are about in a game of mud pies, for instance, is to work out who has what sorts of pies, how much longer they need to be baked, etc. At least one of the things we're about in a discussion of Sherlock Holmes is to work out, say, how exactly Holmes picked up Moriarty's trail near Reichenbach Falls, how we are to think of Watson as having acquired his war wound, and so on.

As I say, to elaborate and adapt oneself to the game's content is typically the game's very point. An alternative point suggests itself, though, when we reflect that all but the most boring games are played with *props*, whose game-independent properties help to determine what it is that players are

supposed to imagine. That Sam's pie is too big for the oven does not follow from the rules of mud pies alone; you have to throw in the fact that Sam's clump of mud fails to fit into the hollow stump. If readers of 'The Final Problem' are to think of Holmes as living nearer to Windsor Castle than Edinburgh Castle, the facts of nineteenth-century geography deserve a large part of the credit.

A game whose content reflects the game-independent properties of worldly props can be seen in two different lights. What ordinarily happens is that we take an interest in the props because and to the extent that they influence the content; one tramps around London in search of 221B Baker street for the light it may shed on what is true according to the Holmes stories.

But in principle it could be the other way around: we could be interested in a game's content because and to the extent that it yielded information about the props. This would not stop us from playing the game, necessarily, but it would tend to confer a different significance on our moves. Pretending within the game to assert that BLAH would be a way of giving voice to a fact holding *outside* the game: the fact that the props are in such and such a condition, viz., the condition that makes BLAH a proper thing to pretend to assert. If we were playing the game in this alternative spirit, then we'd be engaged not in *content-oriented* but *prop-oriented* make-believe. Or, since the prop might as well be the entire world, *world-oriented* make-believe.

It makes a certain in principle sense, then, to use make-believe games for serious descriptive purposes. But is such a thing ever actually done? A case can be made that it is done all the time—not perhaps with explicit self-identified games like 'mud pies' but impromptu everyday games hardly rising to the level of consciousness. Some examples of Walton's suggest how this could be so:

> Where in Italy is the town of Crotone? I ask. You explain that it is on the arch of the Italian boot. 'See that thundercloud over there—the big, angry face near the horizon', you say; 'it is headed this way'.... We speak of the saddle of a mountain and the shoulder of a highway.... All of these cases are linked to make-believe. We think of Italy and the thundercloud as something like pictures. Italy (or a map of Italy) depicts a boot. The cloud is a prop which makes it fictional that there is an angry face... The saddle of a mountain is, fictionally, a horse's saddle. But our interest, in these instances, is not in the make-believe itself, and it is not for the sake of games of make-believe that we regard these things as props... [The make-believe] is useful for articulating, remembering, and communicating facts about the props—about the geography of Italy, or the identity of the storm cloud... or mountain topography. It is by thinking of Italy or the thundercloud... as potential if not actual props that I understand where Crotone is, which cloud is the one being talked about.[19]

A certain kind of make-believe game, Walton says, can be 'useful for articulating, remembering, and communicating facts' about aspects of the game-independent world. He might have added that make-believe games can make it easier to reason about such facts, to systematize them, to visualize them, to spot connections with other facts, and to evaluate potential lines of research. That similar virtues have been claimed for metaphors is no accident, if metaphors are themselves moves in world-oriented pretend games. And this is what Walton maintains. A metaphor on his view is an utterance that represents its objects as being *like so*: the way that they *need* to be to make the utterance 'correct' in a game that it itself suggests. The game is played not for its own sake but to make clear which game-independent properties are being attributed. They are the ones that do or would confer legitimacy upon the utterance construed as a move in the game.

The Kinds of Making as If and the Kinds of Mathematics

Seen in the light of Walton's theory, our suggestion above can be put like this: numbers as they figure in applied mathematics are *creatures of existential metaphor*. They are part of a realm that we play along with because the pretense affords a desirable—sometimes irreplaceable—mode of access to certain real-world conditions, viz. the conditions that make a pretense like that appropriate in the relevant game. Much as we make as if, e.g., people have associated with them stores of something called 'luck', so as to be able to describe some of them metaphorically as individuals whose luck is 'running out', we make as if pluralities have associated with them things called 'numbers', so as to be able to express an (otherwise hard to express because) infinitely disjunctive fact about relative cardinalities like so: The number of *F*s is divisible by the number of *G*s.

Now, if applied mathematics is to be seen as world-oriented make-believe, then *one* attractive idea about pure mathematical statements is that:

(c) They are to be understood as *content*-oriented make-believe.

Why not? It seems a truism that pure mathematicians spend most of their time trying to work out what is true according to this or that mathematical theory.[20] All that needs to be added to the truism, to arrive at the conception of pure mathematics as content-oriented make-believe, is this: that the mathematician's interest in working out what is true-according-to-the-theory is by and large independent of whether the theory is thought to be *really true*—true in the sense of correctly describing a realm of independently constituted mathematical objects.[21]

That having been said, the statements of at least *some* parts of pure mathematics, like simple arithmetic, are legitimated (made pretense-worthy) by very general facts about the non-numerical world. So, on a natural understanding of the arithmetic game, it is pretendable that $3 + 5 = 8$ because if there are three FS and five GS distinct from the FS, then there are eight (F v G)s—whence construed as a piece of world-oriented make-believe, the statement that $3 + 5 = 8$ 'says' that if there are three FS and five GS, etc. For at least some pure mathematical statements, then, it is plausible to hold that:

(W) They are to be understood as *world*-oriented make-believe.

Construed as world-oriented make-believe, every statement of 'true arithmetic' expresses a first-order logical truth; that is, it has a logical truth for its metaphorical content.[22] (The picture that results might be called 'Kantian logicism'. It is *Kantian* because it grounds the necessity of arithmetic in the representational character of numbers. Numbers are always 'there' because they are written into the spectacles through which we see things. The picture is *logicist* because the facts represented—the facts we see through our numerical spectacles—are facts of first-order logic.)

There is a third interpretation possible for pure-mathematical statements. Arithmeticians imagine that there are numbers. But this a complicated thing to imagine. It would be natural for them to want a codification of what it is that they are taking on board. And it would be natural for them to want this codification in the form of an *autonomous* description of the pretended objects, one that doesn't look backward to applications. As in any descriptive project, a need may arise for representational aids. *Sometimes* these aids will be the very objects being described: 'For all *n*, the number of prime numbers is larger than *n*.' Sometimes though they will be *additional* objects dreamed up to help us get a handle on the original ones: 'The number of prime numbers is $\aleph_0$.'

What sort of information are these statements giving us? Not information about the concrete world (as on interpretation (W)); the prime numbers form no part of that world. And not, at least not on the face of it, information about the game (as on interpretation (c)); the number of primes would have been aleph-nought even if there had been no game. 'The number of primes is $\aleph_0$' gives information about the prime numbers as they are supposed to be conceived by players of the game.

Numbers start life as representational aids. But then, on a second go-round, they come to be treated as a subject-matter in their own right (like Italy or the thundercloud). Just as representational aids are brought in to help us describe other subject-matters, they are brought in to help us describe the numbers. Numbers thus come to play a double role, functioning both as representational aids and things-represented. This gives us a third way of interpreting pure-mathematical statements:

(M) They are to be understood as prop-oriented make-believe with numbers etc. serving *both* as props and as representational aids helping us to describe the props.

One can see in particular cases how they switch from one role to the other. If I say that 'the number of primes is $\aleph_0$,' the primes are my subject-matter and $\aleph_0$ is the representational aid. (This is clear from the fact that I would accept the paraphrase 'there are denumerably many primes'.) If, as a friend of the continuum hypothesis, I say that 'the number of alephs no bigger than the continuum is prime', it is the other way around. The primes are now representational aids and $\aleph_0$ has become a prop. (I would accept the paraphrase 'there are primely many alephs no bigger than the continuum'.)

The bulk of pure mathematics is probably best served by interpretation (M). This is the interpretation that applies when we are trying to come up with autonomous descriptions of this or that imagined domain. Our *ultimate* interest may still be in describing the natural world; our *secondary* interest may still be in describing and consolidating the games we use for that purpose. But in most of pure mathematics, world and game have been left far behind, and we confront the numbers, sets, and so on, in full solitary glory.

Two Types of Metaphorical Correctness

So much for 'normal' pure mathematics, where we work within some existing theory. If the metaphoricalist has a problem about correctness, it does not arise there; for any piece of mathematics amenable to interpretations (C), (W), or (M) is going to have objective correctness conditions. Where a problem *does* seem to arise is in the context of theory-*development*. Why do some ways of constructing mathematical theories, and extending existing ones, strike us as better than others?

I have no really good answer to this, but let me indicate where an answer might be sought. A distinction is often drawn between *true* metaphors and

metaphors that are *apt*. That these are two independent species of metaphorical goodness can be seen by looking at cases where they come apart.

An excellent source for the first quality (truth) without the second (aptness) is back issues of *Reader's Digest* magazine. There one finds jarring, if not necessarily inaccurate, titles along the lines of 'Tooth Decay: America's Silent Dental Killer', 'The Sino-Soviet Conflict: A Fight in the Family', and, my personal favorite, 'South America: Sleeping Giant on Our Doorstep'. Another good source is political metaphor. When Calvin Coolidge said that 'The future lies ahead', the problem was not that he was *wrong*—where else would it lie?—but that he didn't seem to be mobilizing the available metaphorical resources to maximal advantage. (Likewise when George H. Bush told us before the 1992 elections that 'It's no exaggeration to say that the undecideds could go one way or another.')

Of course, a likelier problem with political metaphor is the reverse, that is, aptness without truth. The following are either patently (metaphorically) untrue or can be imagined untrue at no cost to their aptness. Stalin: 'One death is a tragedy. A million deaths is a statistic.' Churchill: 'Man will occasionally stumble over truth, but most times he will pick himself up and carry on.' Will Rogers: 'Diplomacy is the art of saying "Nice doggie" until you can find a rock.' Richard Nixon: 'America is a pitiful helpless giant.'

Not the best examples, I fear. But let's move on to the question they were meant to raise. How does metaphorical *aptness* differ from metaphorical *truth*? David Hills (1997: 119–120) observes that where truth is a semantic feature, aptness can often be an aesthetic one: 'When I call Romeo's utterance apt, I mean that it possesses some degree of poetic power... Aptness is a specialized kind of beauty attaching to interpreted forms of words... For a form of words to be apt is for it... to be the proper object of a certain kind of felt satisfaction on the part of the audience to which it is addressed.'

That can't be all there is to it, though; for 'apt' is used in connection not just with *particular* metaphorical claims but entire metaphorical frameworks. One says, for instance, that rising pressure is a good metaphor for intense emotion; that possible worlds provide a good metaphor for modality; or that war makes a good (or bad) metaphor for argument. What is meant by this sort of claim? Not that pressure (worlds, war) are metaphorically *true* of emotion (modality, argument). There is no question of truth because no metaphorical claims have been made. But it would be equally silly to speak here of poetic power or beauty. The suggestion seems rather to be that *an as-if game built around pressure (worlds, war) lends itself to the metaphorical expression of truths about emotion (possibility, argument).* The game 'lends itself' in the sense of affording access to lots of those truths, or to particularly important ones, and/or in the sense of presenting those truths in a cognitively or motivationally advantageous light.

Aptness is *at least* a feature of prop-oriented make-believe games; a game is apt relative to such and such a subject-matter to the extent that it lends itself to the expression of truths about that subject-matter. A particular metaphorical *utterance* is apt to the extent that (a) it is a move in an apt game, and (b) it makes impressive use of the resources that game provides. The reason it is so easy to have aptness without truth is that to make satisfying use of a game with lots of expressive potential is one thing, to make veridical use of a game with arbitrary expressive potential is another.[23]

Correctness in Non-Normal Mathematics

Back now to the main issue: what accounts for the feeling of a right and a wrong way of proceeding when it comes to mathematical theory-development? I want to say that a proposed new axiom *A* strikes us as correct roughly to the extent that a theory incorporating *A* seems to us to make for an *apter game*—a game that lends itself to the expression of more metaphorical truths—than a theory that omitted *A*, or incorporated its negation. To call *A* correct is to single it out as possessed of a great deal of 'cognitive promise'.[24]

Take for instance the controversy early in the last century over the axiom of choice. One of the many considerations arguing *against* acceptance of the axiom is that it requires us to suppose that geometrical spheres decompose into parts that can be reassembled into multiple copies of themselves. (The Banach–Tarski paradox.) Physical spheres are not *like* that, so we imagine, hence the axiom of choice makes geometrical space an imperfect metaphor for physical space.

One of the many considerations arguing in *favor* of the axiom is that it blocks the possibility of sets *X* and *Y* neither of which is injectable into the other. This is crucial if injectability and the lack of it are to serve as metaphors for relative size. It is crucial that the statement about functions that 'encodes' the fact that there are not as many *Y*s as *X*s should be seen in the game to *entail* the statement 'encoding' the fact there are at least as many *X*s as *Y*s. This entailment would not go through if sets were not assumed to satisfy the axiom of choice.[25] Add to this that choice *also* mitigates the paradoxicality of the Banach–Tarski result, by opening our eyes to the possibility of regions too inconceivably complicated to be assigned a 'size', and it is no surprise

that choice is judged to make for an overall apter game. (This is hugely oversimplified, no doubt; but it illustrates the kind of consideration that I take to be relevant.)

Suppose we are working with a theory *T* and are trying to decide whether to extend it to $T^* = T + A$. An impression I do *not* want to leave is that T^*'s aptness is simply a matter of its expressive potential with regard to our original *naturalistic* subject matter: the world we really believe in, which, let's suppose, contains only concrete things. T^* may also be valued for the expressive assistance it provides in connection with the *mathematical* subject matter postulated by *T*—a subject-matter which we take to obtain in our role as players of the *T*-game. A new set-theoretic axiom may be valued for the light it sheds not on concreta but on mathematical objects already in play. So it is, for instance, with the axiom of projective determinacy and the sets of reals studied in descriptive set theory.

Our account of correctness has two parts. Sometimes a statement is correct because it is true according to an implicitly understood background story, such as Peano Arithmetic or ZFC. This is a relatively objective form of correctness. Sometimes though there is no well-enough understood background story and so we must think of correctness another way. The second kind of correctness goes with a statement's 'cognitive promise', that is, its being suited to figure in especially apt pretend games.

Our Goodmanian Ancestors

If mathematics is a myth, how did the myth arise? You got me. But it may be instructive to consider a meta-myth about how it might have arisen. My strategy here is borrowed from Wilfrid Sellars in *Empiricism and the Philosophy of Mind.* Sellars asks us to:

> Imagine a stage in pre-history in which humans are limited to what I shall call a Rylean language, a language of which the fundamental descriptive vocabulary speaks of public properties of public objects located in Space and enduring through Time.
>
> (Sellars, 1997: 91)

> What resources would have to be added to the Rylean language of these talking animals in order that they might come to recognize each other and themselves as animals that *think*, *observe*, and have *feelings* and *sensations*? And, how could the addition of these resources be construed as reasonable?
>
> (Sellars, 1997: 92)

Let us go back to a similar stage of pre-history, but since it is the language's concrete (rather than public) orientation that interests us, let us think of it not as a Rylean language but a *Goodmanian* one. The idea is to tell a just-so story that has mathematical objects invented for good and sufficient reasons by the speakers of this Goodmanian language: henceforth *our Goodmanian ancestors*. None of it really happened, but our situation today is as if it had happened, and the memory of these events was then lost.[26]

First Day, Finite Numbers of Concreta.

Our ancestors, aka the Goodmanians, start out speaking a first-order language quantifying over concreta. They have a barter economy based on the trading of precious stones. It is important that these trades be perceived as fair. To this end, numerical quantifiers are introduced:

$$\exists_0 xFx =_{df} \forall x(Fx \rightarrow x \neq x)$$
$$\exists_{n+1} xFx =_{df} \exists y(Fy \ \& \ \exists_n x(Fx \ \& \ x \neq y))$$

From $\exists_n x$ ruby(x) and $\exists_n x$ sapphire(x), they infer 'rubies-for-sapphires is a fair trade' (all gems are considered equally valuable). So far, though, they lack premises from which to infer 'rubies-for-sapphires is *not* a fair trade'. If they had infinite conjunction, the premise could be:

$$\sim(\exists_0 xRx \ \& \ \exists_0 xSx) \ \& \ \sim(\exists_1 xRx \ \& \ \exists_1 xSx) \ \& \text{ etc.}$$

But their language is finite, so they take another tack. They decide to make as if there are non-concrete objects called 'numbers'. The point of numbers is to serve as measures of cardinality. Using **S** for 'it is to be supposed that *S*', their first rule is:

(R1) If $\exists_n xFx$ then *$n =$ the number of Fs*, and if $\sim\exists_n xGx$ then *$n \neq$ the number of Gs*[27]

From $(\#x)Rx \neq (\#x)Sx$, they infer 'rubies-for-sapphires is not a fair trade'. ('The number of *F*s' will sometimes be written '$(\#x)Fx$' or '$\#(F)$'.) Our ancestors do not believe in the new entities, but they pretend to for the access this gives them to a fact that would otherwise be inexpressible, viz., that there are (or are not) exactly as many rubies as sapphires.

Second Day, Finite Numbers of Finite Numbers.

Trading is not the only way to acquire gemstones; one can also inherit them, or dig them directly out of the ground. As a result some Goodmanians have more stones than others. A few hotheads clamor for an immediate redistribution of all stones so that everyone winds up with the same amount. Others prefer a more gradual approach in which, for example, there are five levels of ownership this year, three levels the next, and so on, until finally all are at the same level. The second group is at a disadvantage because their proposal is not yet expressible. Real objects can be counted using (R1), but not the pretend objects that (R1) posits as measures of cardinality. A second rule provides for the assignment of numbers to bunches of pretend objects:

(R2) *If $\exists_n xFx$ then $n = (\#x)Fx$*, and *If $\sim \exists_n xGx$ then $n \neq (\#x)Gx$*

The gradualists can now put their proposal like this: *every year should see a decline in the number of numbers *k* such that someone has *k* gemstones.* The new rule also has consequences of a more theoretical nature, such as *every number is less than some other number.* Suppose to the contrary that *the largest number is 6.* Then *the numbers are 0, 1, 2, ..., and 6.* But *0, 1, 2, ..., and 6 are seven in number.* So by (R2), *there is a number 7*.

Third Day, Operations on Finite Numbers.

Our ancestors seek a uniform distribution of gems, but find that this is not always so easy to arrange. Sometimes indeed the task is hopeless. Our ancestors know some sufficient conditions for 'it's hopeless', such as 'there are five gems and three people', but would like to be able to characterize hopelessness in general. They can get part way there by stipulating that numbers can be added together:

(R3) *If $\sim \exists x(Fx\&Gx)$, then $\#(F) + \#(G) = \#(F \vee G)$*.

Should there be two people, the situation is hopeless iff *$\sim\exists n \ \#(\text{gems}) = n + n$*. Should there be three people, the situation is hopeless iff *$\sim\exists n \ \#(\text{gems}) = ((n + n) + n)$*. A new rule:

(R4) *If $m = \#(G)$, then $\#(F) \times \#(G) = \#(F) + \ldots + \#(F)$* ($m$ times).

allows them to wrap these partial answers up into a single package. The situation is hopeless iff *$\sim\exists n \ \#(\text{gems}) = n \times \#(\text{people})$.*

Fourth Day, Finite Sets of Concreta.

Gems can be inherited from one's parents, and also from their parents, and theirs. However our ancestors find themselves unable to answer in general the question 'from whom can I inherit gems?' This is because they lack (the means to express) the concept of an ancestor. They decide to make as if there are finite sets of concreta:

(R5) For all $x_1, \ldots, x_n$, *there is a set y such that for all z, $z \in y$ iff $z = x_1 \vee z = x_2 \vee \ldots \vee z = x_n$*.[28]

Ancestorhood can now be defined in the usual way. An ancestor of b is anyone who belongs to every set containing b and closed under the parenthood relation. Now our ancestors know (and can say) who to butter up at family gatherings: their ancestors.

Fifth Day, Infinite Sets of Concreta.

Gemstones are cut from veins of ruby and sapphire found underground. Due to the complex geometry of mineral deposits (and because miners are a quarrelsome lot), it often happens that two miners claim the same bit of stone. Our ancestors to decide to systematize the conditions of gem discovery. This much is clear: Miner Jill has discovered any (previously undiscovered) quantity of sapphire all of which was noticed first by her. But how should other bits of sapphire be related to the bits that Jill is known to have discovered for Jill to count as discovering those other bits too? One idea is that they should *touch* the bits of sapphire that Jill is known to have discovered. But the notion of touching is not well understood, and it is occasionally even argued that touching is impossible, since any two atoms are some distance apart. Our ancestors decide to take the bull by the horns and work directly with sets of atoms. They stipulate that:

(R6) If F is a predicate of concreta, then *there is a set y such that for all z, $z \in y$ iff Fz*,

and then, concerned that not all sets of interest are the extensions of Goodmanian predicates, boot this up to:

(R7) whatever $x_1, x_2, \ldots$ might be, *there is a set containing all and only $x_1, x_2 \ldots$*[29]

Next they offer some definitions. Two sets S and T of atoms *converge* iff given any two atoms x and y, some s and t in S and T respectively are closer to one another than x is to y.[30] A set U of atoms is *integral* iff it intersects every set of atoms converging on any of its non-empty subsets. A set V of atoms all of the same type—sapphire, say—is *inclusive*, qua set of sapphire atoms, iff V has as a subset every integral set of sapphire atoms on which it converges. The sought after principle: Miner Jill can lay claim to the contents of the smallest inclusive set of sapphire atoms containing the bit she saw first.

Sixth Day, Infinite Numbers of Concreta.

Numbers have not yet been assigned to infinite totalities, although infinite numbers promise the same sort of expressive advantage as finite ones. Our ancestors decide to start with infinite totalities of concreta, like the infinitely many descendants they envisage. Their first rule is:

(R8) If $\forall x(Fx \rightarrow \exists! y\,(Gy \,\&\, Rxy))$ then *$\#(F) \leq \#(G)$*.

This is fine as far as it goes, but it does not go far enough, or cardinality relations will wind up depending on what relation symbols R the language happens to contain. Having run into a similar problem before, they know what to do.

(R9) For each x and y, *there is a unique ordered pair $< x, y >$*

(R10) *If $p_1, p_2, \ldots$ are ordered pairs of concreta, then there is a set containing all and only $p_1, p_2, \ldots$*

A set that never pairs two right elements with the same left element is a function; if in addition it never pairs two left elements with the same right element, it is a 1–1 function; if in addition its domain is X and its range is a subset of Y, it is a 1–1 function from X into Y.

(R11) *If there is a 1–1 function from $\{x: Fx\}$ into $\{x: Gx\}$, then $\#(F) \leq \#(G)$*.

How many infinite numbers this nets them depends on the size of the concrete universe. To obtain a *lot* of infinite numbers, however, our ancestors will need to start counting abstracta.

Seventh Day, Infinite Sets (and Numbers) of Abstracta.

The next step is the one that courts paradox. (R7) allows for the unrestricted gathering together of concreta. (R10) allows for the unrestricted gathering together of a particular variety of abstracta. Now our ancestors take the plunge:

(R12) *If $x_1, x_2, \ldots$ are sets, then there is a set containing all and only $x_1, x_2, \ldots$*

Assuming a set-theoretic treatment of ordered pairs, the sets introduced by (R12) already include the 1–1 functions used in the assignment of cardinality. Thus there is no need to reprise (R9); we can go straight to:

(R13) *If there is a 1–1 function from set S into set T, then #(S's members) ≤ #(T's members)*

(R12) will seem paradoxical to the extent that it seems to license the supposition of a universal set. It will seem to do that to that extent that 'all the sets' looks like it can go in for '$x_1, x_2, \ldots$' in (R12)'s antecedent. 'All the sets' will look like an admissible substituend if the *de re* appearance of '$x_1, x_2, \ldots$' is not taken seriously. But our ancestors take it *very* seriously. Entitlement to make as if there is a set whose members are $x, y, z, \ldots$ depends on *prior* entitlements to make as if there are each of $x, y, z, \ldots$. Hence the sets whose supposition is licensed by (R12) are the well-founded sets.

Much, Much Later, Forgetting.

These mathematical metaphors prove so useful that they are employed on a regular basis. As generation follows upon generation, the knowledge of how the mathematical enterprise had been launched begins to die out and is eventually lost altogether. People begin thinking of mathematical objects as genuinely there. Some, ironically enough, take the theoretical indispensability of these objects as a *proof* that they are there—ironically, since it was that same indispensability that led to their being concocted in the first place.

Worked Example

An oddity of Quine's approach to mathematical ontology has been noted by Penelope Maddy (1997). Quine sees math as continuous with 'total science' both in its subject matter and in its methods. Aping a methodology he sees at work in physics and elsewhere, Quine maintains that in mathematics too, we should keep our ontology as small as practically possible. Thus:

> [I am prepared to] recognize indenumerable infinites only because they are forced on me by the simplest known systematizations of more welcome matters. Magnitudes in excess of such demands, e.g., beth-omega or inaccessible numbers, I look upon only as mathematical recreation and without ontological rights. Sets that are compatible with [Godel's axiom of constructibility V = L] afford a convenient cut-off...
>
> (1986: 400).

Quine even proposes that we opt for the 'minimal natural model' of ZFC, a model in which all sets are constructible *and* the tower of sets is chopped off at the earliest possible point. Such an approach is 'valued as inactivat[ing] the more gratuitous flights of higher set theory...'(Quine, 1992: 95).

Valued by whom? one might ask. Not actual set theorists. To them, cardinals the size of beth-omega are not even slightly controversial. They are guaranteed by an axiom introduced already in the 1920s (Replacement) and accepted by everyone. Inaccessibles are far too low in the hierarchy of large cardinals to attract any suspicion. As for Gödel's axiom of constructibility, it has been widely criticized—including by Gödel himself—as entirely too restrictive. Here is Moschovakis, in a passage quoted by Maddy:

> The key argument against accepting V = L ... is that the axiom of constructibility appears to restrict unduly the notion of an *arbitrary* set of integers
>
> (1980: 610).

Set-theorists have wanted to *avoid* axioms that would 'count sets out' just on grounds of arbitrariness. They have wanted, in fact, to run as far as possible in the other direction, seeking as fully packed a set-theoretic universe as the iterative conception of set permits. All this is reviewed in fascinating detail in Maddy (1997); see especially her discussion of the rise and fall of Definabilism, first in analysis and then in the theory of sets.

If Quine's picture of set theory as something like abstract physics cannot make sense of the field's plenitudinarian tendencies, can any other picture do better? Well, clearly one is not going to be worried about multiplying entities if the entities are not assumed to really exist. But we can say more. The likeliest approach if the set-theoretic universe is an intentional object more than a real one would be (A) to articulate the clearest intuitive conception possible, and then, (B) subject to that constraint, let all heck break loose.

Regarding (A), *some* sort of constraint is needed or the clarity of our intuitive vision will suffer. This is the justification usually offered for the axiom of foundation, which serves no real mathematical purpose—there is not a single theorem of mainstream mathematics that makes use of it—but just forces sets into the familiar and comprehensible tower structure. Without foundation there would be no possibility of 'taking in' the universe of sets in one intellectual glance.

Regarding (B), it helps to remember that sets 'originally' came in to improve our descriptions of non-sets. E.g., there are infinitely many *Z*s iff the set of *Z*s has a proper subset *Y* that maps onto it one-one, and uncountably many *Z*s iff it has an infinite proper subset *Y* that *cannot* be mapped onto it one-one. Since these notions of *infinitely* and *uncountably many* are topic neutral—the *Z*s do not have to meet a 'niceness' condition for it to make sense to ask how many of them there are—it would be counterproductive to have 'niceness' constraints on when the *Z*s are going to count as bundleable together into a set.[31] It would be still more counterproductive to impose 'niceness' constraints on the 1–1 functions; when it comes to infinitude, one way of pairing the *Z*s off 1–1 with just some of the *Z*s seems as good as another.

So: if we think of sets as having been brought in to help us count concrete things, a restriction to 'nice' sets would have been unmotivated and counterproductive. It would not be surprising if the anything-goes attitude at work in those original applications were to reverberate upward to contexts where the topic is sets themselves. Just as we do not want to tie our hands unnecessarily in applying set-theoretic methods to the matter of whether there are uncountably many space–time points, we don't want to tie our hands either in considering whether there are infinitely many natural numbers, or uncountably many sets of such numbers.

A case can be made, then, for (imagining there to be) a *plenitude* of sets of numbers; and a 'full' power set gathering all these sets together; and a plenitude of 1–1 functions from the power set to its proper subsets to ensure that if the power set isn't countable, there will be a function on hand to witness the fact. Plenitude is topic-neutrality writ ontologically. The preference for a 'full' universe is thus unsurprising on the as-if conception of sets.

NOTES

I am grateful to Jamie Tappenden, Thomas Hofweber, Carolina Sartorio, Hartry Field, Sandy Berkovski, Gideon Rosen, and Paolo Leonardi for comments and criticism. Most of this chapter was written in 1997 and there are places it shows. For one thing, a lot of relevant literature is simply ignored. Also various remarks about the state of the field were truer then than they are now (which is not to say they were particularly true then). My own views have changed too. Where the chapter speaks of

'*making* as if you believe that *S*', I would now say '*being* as if you believe that S, but not really believing it except possibly per accidens' (see Yablo, 2002a). Related to this, mathematical objects may exist for all I know. I do not rule it out that '2 + 3 = 5' is literally true in addition to being metaphorically true, making it a twice-true metaphor along the lines of 'no man is an island'. I also do not rule it out that '2 + 3 = 5' is a maybe-metaphor, to be interpreted literally if so interpreted it is true, otherwise metaphorically. (Compare 'Nixon had a stunted superego', to use Jamie Tappenden's nice example.) I think that the existence issue can be finessed still further, but the margin is too small to contain my proof of this.

1. Beaney (1997: 366).
2. I am pretending for rhetorical purposes that Frege is still a logicist in 1919.
3. Geach and Black (1960: 184–7)
4. He speaks in *Notes for L. Darmstaedter* of 'The miracle of arithmetic'.
5. See Wigner (1967).
6. I am ignoring the Quine/Putnam approach here, first because Quine and Putnam do not purport to draw lessons from *applicability* (but rather indispensability), second because they do not purport to draw *lessons* from applicability. They do not say that we *should* accept mathematics given its applications; they think that we already *do* accept it by virtue of using it, and (this is where the indispensability comes in) we are not in a position to stop.
7. To suppose that truth alone should make for applicability would be like supposing that randomly chosen high quality products should improve the operation of randomly chosen machines. This seems to be what the Dormouse believed in *Alice in Wonderland*; asked what had possessed him to drip butter into the Mad Hatter's watch, he says, 'but it was the BEST butter'. The best record of what I had for breakfast won't help science any more than the best butter will improve the operation of a watch.
8. Thus Mark Steiner (1998): 'Arithmetic is useful because bodies belong to reasonably stable families, such as are important in science and everyday life' (25–6). 'Addition is useful because of a *physical* regularity: gathering preserves the existence, the identity, and (what we call) the major properties, of assembled bodies' (27). 'That we can arrange a set [e.g., into rows] without losing members is an empirical precondition of the effectiveness of multiplication . . .' (29). 'Consider now *linearity*: why does it pervade physical laws? Because the sum of two solutions of a (homogeneous) linear equation is again a solution.' (30). 'The explanatory challenge . . . is to explain, not the law of gravity by itself, but the prevalence of the inverse square . . . What Pierce is looking for is some general physical property which lies behind the inverse square law, just as the principle of superposition and the principle of smoothness lie behind linearity' (35–6).
9. At least, not at this level of generality.

10. Some have questioned this claim, alleging a confusion of semantic conservativeness with deductive conservativeness. I propose to sidestep that issue entirely.
11. Field has pointed out to me that there are the materials for an explanation in the representation theorem he proves en route to nominalizing a theory. This is an excellent point and I do not have a worked out answer to it. Let me just make three brief remarks. First, we want an explanation that works even when the theory cannot be nominalized. Second, and more tendentiously, we want an explanation that doesn't trade on the potential for nominalization even when that potential is there. Third, the explanation that runs through a representation theorem is less a 'deductive utility' explanation than a 'representational aid' explanation of the type advocated later in this paper.
12. Deliberate now, anyway; it started out as an innocent misunderstanding. Thanks to Ana Carolina Sartorio for straightening me out on these matters.
13. Representational usefulness will be the focus in what follows. But I don't want to give the impression that the possibilities end there. Another way that numbers appear to 'help' is by redistributing theoretical content in a way that streamlines theory revision. Suppose that I am working in a first-order language speaking of material objects only. And suppose that my theory says that there are between two and three quarks in each *Z*-particle:

    (a) $(\forall z)[(\exists q_1)(\exists q_2)(q_1 \neq q_2 \,\& q_i \varepsilon z \,\& (\forall r_1)(\forall r_2)((r_1 \neq r_2 \,\& r_j \varepsilon z) \rightarrow (r_1 = q_1 \text{etc.}))]$.

    Then I discover that my theory is wrong: The number of quarks in a *Z*-particle is between two and *four*. Substantial revisions are now required in sentence (a). I will need to write in a new quantifier '$\forall r_3$'; two new non-identities '$r_1 \neq r_3$' and '$r_2 \neq r_3$'; and two new identities '$r_3 = q_1$' and '$r_3 = q_2$.' Compare this with the revisions that would have been required had quantification over numbers been allowed—had my initial statement been

    (b) $(\forall z)(\forall n)(n = \#q\,(q \varepsilon z) \rightarrow 2 \leq n \leq 3)$.

    Starting from (b), it would have been enough just to strike out the '3' and write in a '4.' So the numerical way of talking seems better able than the non-numerical way to efficiently absorb new information. Someone might say that the revisions would have been just as easy had we helped ourselves to numerical quantifiers $(\exists_{\geq n} x)$ defined in the usual recursive way. The original theory numbering the quarks at two or three could have been formulated as

    (c) $(\forall z)[(\exists_{\geq 2} q) q \varepsilon z \,\& \neg(\exists_{\geq 4} q) q \varepsilon z)]$.

    To obtain the new theory from (c), one need only change the second subscript. But this approach only postpones the inevitable. For our theory might be mistaken in another way: rather than the number of quarks in a *Z*-particle being two or three, it turns out that the number is two, three, five, seven, eleven, or... or ninety-seven—that is, the number is a *prime* less than one hundred. If we want to write this in the style of (c), our best option is a disjunction about thirty times longer than the original. Starting from (b), however, it is enough to replace '$2 \leq n \leq 3$' with 'n is prime & $2 \leq n \leq 100$.' True, we could do better if we had a primitive 'there exist primely many...' quantifier. But, as is familiar, the strategy of introducing a new primitive for each new expressive need outlives its usefulness fairly quickly. The only really progressive strategy in this area is to embrace quantification over numbers.

14. It is not just that for every recursive notation, there are reals that it does not reach; most reals are such that no recursive notation can reach them.
15. This point is also stressed by Balaguer. I first heard it from Gideon Rosen in 1990. He suggested defining the nominalistic content of a math-infused statement *S* as the set of worlds *w* such that *w* is indiscernible in concrete respects from some *w** where *S* is true.
16. Field does remark in various places that there may be no easy way of detaching the 'material content' of a statement partly about abstracta:

    > the task of splitting up mixed statements into purely mathematical and purely non-mathematical components is a highly non-trivial one: it is done easily in [some] cases [e.g., '2 = the number of planets closer than the Earth to the Sun],' but it isn't at all clear how to do it in [other] cases [e.g., 'for some natural number n there is a function that maps the natural numbers less than n onto the set of all particles of matter,' 'surrounding each point of physical space-time there is an open region for which there is a 1–1 differentiable mapping of that region onto an open subset of R^4.']
    >
    > (Field, 1989: 235)

    He goes on to say that:

    > the task of splitting up all such assertions into two components is precisely the same as the task of showing that mathematics is dispensable in the physical sciences.
    >
    > (Field, 1989: 235)

    This may be true if by 'mathematics is dispensable' one means (and Field does mean this) 'in any application of a mixed assertion. . . . a purely non-mathematical assertion could take its place' (235). But in *that* sense of dispensable—ideological dispensability, we might call it—truth is not dispensable either; there is no truth-less way of saying lots of the things we want to say. It appears then that ideological indispensability has *in the case of truth* no immediate ontological consequences. Why then is it considered to argue for the existence of numbers?
17. This section repeats some of Yablo (1998).
18. Better, such and such is part of the game's content if 'it is to be imagined. . . . *should the question arise*, it being understood that often the question *shouldn't* arise' (Walton, 1990: 40). Subject to the usual qualifications, the ideas about

make-believe and metaphor in the next few paragraphs are all due to Walton (1990, 1993).

19. Walton (1993: 40–1).
20. The theory might be a collection of axioms; it might be that plus some informal depiction of the kind of object the axioms attempt to characterize; or it might be an informal depiction pure and simple.
21. The intended contrast is with true-according-to-some-other-theory.
22. See Yablo (2002b).
23. Calling a figurative description 'wicked' or 'cruel' can be a way of expressing appreciation on the score of aptness but reservations on the score of truth. See in this connection Moran (1989).
24. Thanks to David Hills for this helpful phrase.
25. Thanks here to Hartry Field.
26. Earlier versions of this chapter had a fourteen-day melodrama involving functions on the reals, complex numbers, sets vs. classes, and more besides. It was ugly. Here I limit myself to cardinal numbers and sets.
27. *F* and *G* are predicates of concreta.
28. *n* here is schematic.
29. One might wonder how our ancestors acquired plural quantifiers, and whether they wouldn't have saved themselves a lot of trouble by acquiring them earlier.
30. Crucially for this definition, *x* and *y* can be material *or spatial* atoms. Our ancestors hold that all point-sized spatial positions are occupied by points of space; material atoms cohabit with some of these but not all.
31. Except to the extent that such constraints are needed to maintain consistency.

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