Article:

Leng, Mary Catherine orcid.org/0000-0001-9936-5453 (2002) Mathematical Practice as a Guide to Ontology:Evaluating Mathematical Platonism by its Consequences for Theory Choice. Logique et Analyse. pp. 235-348. ISSN 2295-5836

Logique & Analyse **179–180** (2002), 235–248

MATHEMATICAL PRACTICE AS A GUIDE TO ONTOLOGY: EVALUATING QUINEAN PLATONISM BY ITS CONSEQUENCES FOR THEORY CHOICE

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1. *Introduction*

An assumption that lies behind the central project of this paper is that our philosophical account of mathematics should be able to account for actual mathematical practices in a way that recognizes their value. While this may, in the current philosophical climate, seem too obvious to state explicitly, the philosophy of mathematics has not always proceeded with this 'naturalistic' assumption in mind. Accounts of the nature of mathematics have been given which suggest that mathematical practices should be revised to reflect this true nature. However, once we have rejected the idea that philosophical accounts of mathematics may advocate large scale revision to mathematical practices, this naturalistic stance can be used as a bottom line against which to evaluate philosophical claims about mathematics. If the pictures of idealized mathematical practice that these philosophical views present diverge too far from the often more messy realities of actual mathematical practices, then this should count as evidence against these philosophical views.

When it comes to *ontological* claims made by philosophers it is not at all obvious that the naturalistic assumption will help us very much — it is not immediately clear that the claims philosophers make about the existence of mathematical objects will have *any* consequences regarding the way mathematics should be done. In order, then, to evaluate accounts of the ontology of mathematics in the light of mathematical practices, we first need to find out whether any of these accounts have anything to say about these practices.

I shall argue that a variety of different ontological accounts *do* have (at least apparent) consequences for mathematical practice, to the extent that anyone who denies these consequences has some work to do explaining why they don't follow from the ontological picture they favour. Furthermore, I shall argue that Quine's realist account of mathematics has consequences that receive no positive support from, and are even contradicted by, our current mathematical practices. This rather negative conclusion masks a more positive flip side, which I won't have the time to defend here. While I think that ontological accounts such as Quine's that distinguish between literally true and merely consistent theories cannot account for current mathematical practices, I would like to suggest that theories that do not draw this distinction (between merely consistent theories and really true ones) can provide such an account.

I would like to start, then, by considering four different accounts of the existence — or otherwise — of mathematical objects. These are by no means exhaustive, but they do represent the spectrum of attitudes one could have to mathematical statements taken at face value. I claim that all of these theories carry with them an attitude to *consistent candidates* for mathematical theories, and as a result of this attitude they have consequences regarding which theories mathematicians should choose to work on.

Consider, then, the following four ontological theories: fictionalism, Quinean Platonism, Gödelian Platonism and Full-Blooded Platonism. Fictionalism says that no mathematical objects exist, whereas Full-Blooded Platonism says that all possible ones do, but their implications for mathematical practice turn out to be the same. Since both theories claim that all consistent mathematical theories are on a par *from an ontological perspective*, they both see mathematical theory choice as proceeding for reasons that bear no relation to the truth values of the theories under consideration. When a mathematician chooses to work on one consistent theory amongst the many potentially available, this choice will be a practical matter, based on values that are independent of the truth values of the claims of the theory.

The two other accounts under consideration — Quinean Platonism and Gödelian Platonism — differ from this first pair in that they both claim that some, but not all, of the objects referred to in consistent candidates for mathematical theories really exist. Quine's ontological approach is to look to natural science to decide what there is. Taking quantifier commitment as indicative of ontological commitment (since "to be is to be the value of a variable" [14, pp. 1–19]), Quine argues that we are committed to precisely those mathematical entities that are quantified over in theories used in our best science. That means that we have a distinction between merely consistent theories and actually true ones. Amongst consistent candidates for mathematical theories, the theories that are confirmed as really true are those that are indispensable for our current best science. If mathematicians care about the truth of their theories, then, if the Quinean picture is right, they should pay close attention to the use of mathematics in natural science. In particular, according to Quine's account of the scientific confirmation of mathematical theories, we should see mathematicians as choosing which theories to work on based on their application or potential application in natural science.

The view that I am calling 'Gödelian Platonism' is the more traditional version of Platonism that says that there is an independent mathematical realm of objects about which we can make discoveries. According to this view, we can discover what objects there are through intuition, and through quasi-empirical methods. (That is to say, on this view, to the extent that an axiomatic system has consequences that are intuitive, that axiomatic system gets intuitive confirmation.) Like Quine's version of Platonism, according to the Gödelian account, not all consistent candidates for mathematical theories pick out a piece of mathematical reality. This is why Gödel was so keen to get the right answer to the continuum hypothesis rather than sanction all consistent theories as on a par (see [7]). Mathematical theory choice, on this Gödelian view, reflects an assessment of which theories are likely to be *true*, based on intuition. Thus the Quinean and Gödelian versions of Platonism share the thought that mere consistency is not enough for the actual truth of a mathematical theory, but differ in their assessment of how a consistent theory becomes confirmed as literally true.

In what follows I shall concentrate on the Quinean picture, which says that theories are confirmed by their indispensable use in natural science. According to the Quinean view, we should expect mathematicians to choose between rival theories on the basis of their applicability, and to reject as false those theories that have been superseded in scientific applications. In order to assess this ontological picture against the backdrop of mathematical practices, I will consider first of all some examples of cases where mathematical theories have been abandoned. We will see that, in these cases where we might hope to find support for the Quinean picture, the reasons for abandoning the theories in question have not in fact been due to their perceived lack of confirmation in the light of failed applications. Thus the Quinean picture receives no support from these high profile cases of abandoned mathematical theories. Furthermore, and even more problematic for the Quinean view, we will consider a case where Quinean doctrine would predict the abandonment of a mathematical theory, and where the theory in question has remained an important part of mathematics.

2. *Abandoned Theory 1: The Infinitesimal Calculus*

One clear case of mathematical theory choice that might provide support for the Quinean view is the choice of the rigorized calculus, with the Weierstrassian notion of continuity, over the earlier infinitesimal calculus. Since in this example, the immensely successful application of the calculus was the driving force in the move towards rigorization, we might expect that we have a case here of mathematicians taking their cue from natural science when it comes to theory choice.[1] The move from a notion of the continuum

[1] It would, incidentally, be a cheap shot against Quine to note that talk of infinitesimals by mathematicians was abandoned *despite* its successful use in natural science — presumably

that included infinitesimals to our current Weierstrassian conception of the real line, with the epsilon-delta account of continuity, does seem to display the sort of close interrelation between mathematical and scientific reasons for theory choice that would be expected on the Quinean account.

What is important in this case, however, is the difference that the introduction of non-standard analysis by Abraham Robinson makes for our evaluation of the fate of the theory of infinitesimals. Robinson's non-standard analysis revives infinitesimal theory in a rigorous manner, and has been accepted as an important branch of mathematics. The acceptance of non-standard analysis apparently causes problems for the Quinean picture, because on the view of mathematics as continuous with natural science, it would seem that Robinson's work should be seen as the redevelopment of a theory *in competition with* standard analysis. The continuum of non-standard analysis and the continuum of standard analysis are different, and according to the Quinean view the continuum to survive should be the one that is used as part of our best scientific theory. However, while scientists continue to make successful use of standard analysis, without assuming the existence of infinitesimals, this does not devalue the *mathematical* importance of non-standard analysis. In modern mathematics, with rigorous theories of both available, there is no real fight between the continua of standard and non-standard analysis. The results of each theory are valid, and true of their intended models. In Robinson's words,

> ... it appears to us today that the infinitely small and infinitely large numbers of a non-standard model of Analysis are neither more nor less real than, for example, the standard irrational numbers. [16, p. 282]

The acceptance of the non-standard continuum alongside the notion of the continuum appealed to in physics thus causes difficulties for Quine's picture of mathematics as continuous with, and subject to the evaluative methods of, natural science.

3. *Abandoned Theory 2: Quaternions*

A second example of a theory that has been abandoned in natural science in favour of a simpler alternative is Hamilton's theory of the quaternions. The initial development of the system of quaternions was much heralded,

Quine can see the rigorized calculus as an improved candidate for a scientific theory, chosen for reasons of simplicity, etc.

with contemporaries drawing analogies with the development of the calculus. Thus in a review of Hamilton's book on quaternions, Thomas Hill writes excitedly of the practical promise of the theory:

> The discoveries of Newton had done more for England and for the race, than has been done by the whole dynasties of British monarchs; and we doubt not that in the great mathematical birth of 1843, the Quaternions of Hamilton, there is as much real promise of benefit to mankind as in any event of Victoria's reign. (From Thomas Hill's review of Hamilton's book on quaternions, quoted in [4, p. 37])

But although they were of great historical importance, not least for the development of abstract algebra, the quaternions ultimately faltered when it came to applications because, as Morris Kline explains, they

> were not quite what the physicists wanted. They sought a concept that was not divorced from but more closely associated with Cartesian coordinates than quaternions were. [8, p. 785]

As an applied theory, after a short battle for prominence, the quaternions were quickly replaced by quaternion-inspired vector analysis. Indeed, Morris Kline's discussion of this battle suggests just the sort of practical reasons for theory choice that are prominent in Quine's discussions of theory choice in natural science:

> While vector analysis was being created and afterward there was much controversy between the proponents of quaternions and the proponents of vectors as to which was more useful. The quaternionists were fanatical about the value of quaternions but the proponents of vector analysis were equally partisan. On one side were aligned the leading supporters of quaternions such as Tait and, on the other, Gibbs and Heaviside. Apropos of the controversy, Heaviside remarked sarcastically that for the treatment of quaternions, quaternions are the best instrument. On the other hand Tait described Heaviside's vector algebra as "a sort of hermaphrodite monster, compounded of the notations of Grassmann and Hamilton." . . .
>
> The issue was finally decided in favor of vectors. Engineers welcomed Gibbs's and Heaviside's vector analysis, though the mathematicians did not. By the beginning of the present century the physicists too were quite convinced that vector analysis was what they wanted. Textbooks on the subject soon appeared in all countries and are now standard. The mathematicians finally followed suit and introduced vector methods in analytic and differential geometry. [8, p. 791]

In Michael Crowe's *A History of Vector Analysis*, the current value of the quaternions *as an applied system* is reported as minimal:

> ...the consensus now is that the quaternion system is but one of many comparable mathematical systems, and though it is interesting as a rather special system, it offers little value for application. [4, p. 18]

Here we have the choice between two theories being adjudicated by empirical considerations: the theory favoured by physicists and engineers gained prominence, even amongst mathematicians, and the quaternions fell by the wayside. However, the fact that this practical choice in no way affects the evaluation of the quaternions as a *mathematical* theory is apparent given that Hamilton's achievement is in no way devalued in historical accounts. While in Michael Crowe's *A History of Vector Analysis*, the current value of the quaternions *as an applied system* is reported as minimal, there is clearly no suggestion that the theory as a pure mathematical system is in any way deficient.

> ...the consensus now is that the quaternion system is but one of many comparable mathematical systems, and though it is interesting as a rather special system, it offers little value for application. [4, p. 18]

Contrary to the Quinean picture, in historical accounts of Hamilton's development of the quaternions, Hamilton's work is not presented as the formal development of a theory which turned out to be false (since empirical results use, and therefore 'confirm', vector methods over quaternions), but rather as the ingenious solution to a mathematical problem culminating in the introduction of a system of numbers whose results are as true now as they were when Hamilton first presented them.

4. *Abandoned Theory 3: Catastrophe Theory*

My third example is of a theory that was not so much replaced by a competitor, but rather simply abandoned by mathematicians and scientists. Catastrophe Theory, developed during the late 1960s and early 1970s initially as a "mathematical language for biology" [21, p. 32], received a great deal of media attention in the mid-70s. René Thom published his *Structural Stability and Morphogenesis* in 1972, introducing Catastrophe Theory to the mathematical community, and, only four years later, Newsweek reported that Catastrophe Theory was being "hailed as an "intellectual revolution" in mathematics, the most important development since calculus". However, in 1976 *Science* ran the headline, "CATASTROPHE THEORY: THE EMPEROR HAS NO CLOTHES", and by 1977 the *Smithsonian* reported "the death of a theory" [21, p. 72], claiming that Catastrophe Theory was something that could safely be dismissed as unimportant.

The fate of Catastrophe Theory seems initially promising as an illustration of the Quinean thesis of the continuity of mathematics with natural science. I have claimed that Quine's thesis suggests a picture of theory choice where theories die out when they fail to be of use to scientists. In the case of Catastrophe Theory, its fall from grace was indeed due to a lack of important applications. Does the rise and fall of Catastrophe Theory, then, count as evidence for the Quinean thesis that mathematical theories are to be evaluated as a part of natural science? To discover this, we need to take a detailed look at the reasons for the abandonment of this part of mathematics.

Warwick mathematician E. Christopher Zeeman contributed greatly to the perception of Catastrophe Theory as an important and wide-ranging mathematical tool by developing catastrophe theoretic explanations of phenomena ranging from the heartbeat and the nerve impulse to animal aggression and prison riots. However, just as the theory's promise of wide-ranging applications led to its high profile entrance into the public consciousness, it also led to its downfall, as mathematicians began to question the scientific worth of a theory that seemed to explain too much. Catastrophe Theory's most vocal critic, Hector J. Sussmann, began by questioning Zeeman's models in the social sciences, for stock market crashes, prison riots and the effect of public opinion on military policy. Sussmann concluded that these models were "vaguely formulated, ... based on false hypotheses, ... [and] lead to few non-trivial predictions" ([18], quoted in [21, p. 72]). Furthermore, what predictions these models did make more often than not failed to agree with reality. In a more extended critique of the applications of Catastrophe Theory, written with Raphael S. Zahler [19], more of Zeeman's models were criticized, along with the methodology of applied Catastrophe Theory generally. The conclusion most drew from this was that Catastrophe Theory was a flash in the pan, and, save the efforts of a few passionate defenders,[2] the theory has largely died out as an area of serious research.

Since the death of Catastrophe Theory was largely due to a failure in applications, it would seem an excellent example in support of the Quinean picture of mathematics, according to which mathematical theories are confirmed by their empirical successes. Quine's view would certainly predict the death of Catastrophe Theory. A theory that has no applications, or that fails to provide insight into another theory that does have applications, has no empirical confirmation and should be dismissed. However, the story is not

[2] A recent defence of Catastrophe Theory attempted by Alain Boutot [2] is unlikely to win over many new disciples. Insensitive to the precarious position of the evangelical faced with the cynical, Boutot fails miserably in his attempt at a modern, sober and considered look at the virtues of Catastrophe Theory: "Catastrophe theory," he tells us, "does not enable us to escape our fate, but, if it is contemplative, it is enough to assure happiness" [2, p. 195].

as clear cut as it at first seems, for recall that for Quine it is successful applications that confirm the *truth* of a mathematical theory. Yet, despite their damning criticism of the empirical virtues of Catastrophe Theory, Sussmann and Zahler do not see this as speaking in any way against the mathematical worth of the theory, considered apart from its applications. It is Zeeman's attempted applications that are dismissed as "vaguely formulated" and lacking in predictive worth, and not the mathematical claims of Catastrophe Theory. Indeed, while the authors viciously tear apart the claims of Catastrophe Theorists to have found any plausible applications for their subject, they also take pains to "emphasize that the validity or importance of CT from a mathematical standpoint is not at issue here". The authors add that they "are concerned only with evaluating the usefulness of CT for extra-mathematical applications" [19, p. 118]. Clearly they feel that this evaluation should not extend to the theory's virtues as an area of pure mathematics.

The death of Catastrophe Theory was indeed due to its failure to live up to its empirical promise, but in itself this failure was not enough to convince the mathematical community to conclude that the theory was therefore mathematically worthless, having failed to receive confirmation from its applications. Indeed, although the theory did eventually fizzle out without the excitement of immediate applications, and without a solid pure research programme following Thom's classification of the elementary catastrophes, this was clearly not because practitioners saw its failed applications as disconfirmation of its results. There is surely an easy sociological story behind the abandonment of Catastrophe Theory as a field of pure research. As most researchers went into the field keen to work on its potential applications, once it was clear that the theory had little hope for useful immediate applications, those researchers moved away, leaving an underdeveloped and unfashionable pure field of minor interest to potential new practitioners. Furthermore, set aside from its promised applications, its value as an area of pure research looked meagre from a mathematical standpoint. Steven Smale, a Fields medallist like Thom, criticized Catastrophe Theory as a mathematical theory on purely mathematical, aesthetic grounds:

> …as mathematics, it brings together two of the most basic ideas in modern math: the study of dynamic systems and the study of the singularities of maps. Together, they cover a very wide area — but catastrophe theory brings them together in an arbitrary and constrained way. (Quoted in [21, p. 75])

As an applied theory, Catastrophe Theory was abandoned because it failed to come up with its promised goods. However, its death as an area of pure research can be put down to mathematical and sociological reasons. Contrary to the Quinean picture, the lack of empirical confirmation spoke only to the interest and value, and not to the truth or validity, of the theory.

5. *Euclidean Geometry*

The cases I have presented so far are meant primarily to show that no positive evidence for the Quinean view of theory choice in mathematics can be found from some of the most likely candidates — in cases where mathematical theories have been abandoned, this is not because they are seen as falsified in the light of their empirical failures. As an aside, though, I should note that, in favour of my stronger thesis to the effect that theories which put all consistent theories on a par from an ontological perspective can account for mathematical practice, the examples I have given are a good fit with the view that consistency is enough for mathematical acceptability (and that theory choice is then a purely practical matter). If we construe the previous discussion as bearing on the choice between a Quinean versus a FBP-ist or fictionalist view of mathematical theory choice, then it seems that the latter would win support where the Quinean view has failed. The next example I would like to consider is a case that, in my view, conclusively tips the balance.

Historically, the Quinean picture of mathematical truth as essentially empirical does fit somewhat nicely with the way that mathematicians saw Euclidean geometry. Far from being exercises in the free creation of consistent axiom systems, early attempts to derive consequences from the negation of the parallel axiom were part of an attempt to prove its falsity by *reductio ad absurdum*. Euclidean geometry was held to be evidently true of the world, so that early investigations in non-Euclidean systems were part of standard mathematical activity within an area of mathematics that was linked in mathematicians' minds to a concrete physical interpretation. At this stage, in Kline's words,

> . . . the problem of the parallel axiom was not only a genuine physical problem but as fundamental a physical problem as there can be. [8, pp. 880–1]

Even when mathematicians began to suspect that the parallel axiom was independent of the others, geometry was still so closely linked to its physical interpretation that it could be argued that this axiom was known nevertheless to be true, through our intuition of the physical world. Thus, realizing that the axiom could not be proved to be true, in 1763 George S. Klügel (1739–1812) observed that the certainty of the parallel axiom was based on experience [8, p. 867], a very Quinean outlook. This Quinean linkage between the truth of a pure mathematical theory and the truth of its physical interpretation is emphasized by Michael Scanlan when he says that,

> In the past, mathematical practice did not involve a distinction between theory and interpretation. In the eighteenth century, mathematics was seen as the 'abstract' study of certain aspects of nature. [17, p. 13]

Thus the Quinean picture of mathematics as answerable to, and alterable in the light of, changes in our understanding of nature would seem to fit well with this picture of eighteenth century mathematical activity.

Indeed, the Quinean picture has a reasonably easy task in accounting for the history of the development of non-Euclidean geometries. As it became realized that these alternatives existed, they were developed not on the grounds of being internally consistent axiomatic systems, but rather, as Scanlan tells us, as potentially competing descriptions of physical space:

> The mathematicians who originally conceived of non-euclidean geometry, Bolyai, Lobachevsky and to some extent Gauss, seem all to have conceived of the theory as one which is potentially applicable to physical space. ...
>
> The original BL [Bolyai-Lobachevsky, or hyperbolic] geometers saw their results as holding for the case of a single parallel or for the case of multiple parallels. Because of this, the issue facing the pioneers of BL geometry was not strictly speaking consistency but truth. It was the question of whether the possibilities they envisioned of multiple non-intersecting lines were ever realized. [17, p. 15]

Thus, while Quine's picture has difficulties in accounting for the long term co-existence of mutually contradictory theories, the short term rise of non-Euclidean geometries alongside standard Euclidean geometry can be understood as the development of alternative possible theories of space, which would then be tested against experience.

However, this comfortable Quinean position is quickly made difficult by twentieth century developments, which would seem to force a choice between the various geometries. According to the General Theory of Relativity, astronomical space has positive curvature locally (i.e. wherever there is matter). Thus the best geometry to deal with astronomical space is non-Euclidean. Whether "absolute" (empty) space is Euclidean or not is unknown, indeed, it is not at all clear how we might go about answering this question. However, from the perspective of natural science, it is a non-Euclidean geometry which has been shown to best describe the world with which it is concerned.

According to Quine's view of mathematics, then, current science confirms one version of non-Euclidean geometry over its (Euclidean and non-Euclidean) rival theories of space. And as, according to Quine, the only confirmation possible is scientific confirmation, this means that from a mathematical perspective we should set aside the alternatives and stick with our best confirmed geometry of space. However, this is clearly not the attitude taken by modern geometers, whose attitude is typified by H. S. M. Coxeter's view that,

> ... although a geometry may seem more interesting if we can compare it with the real world, its validity as a logical structure is not affected, but depends only on its internal consistency. [3, p. 10]

Similarly, Yuxin Zheng contends that,

> The essential characteristic of modern mathematics is that its objects are not only those forms and relations abstracted directly from experience, but also other forms and relations, (mathematical structures) which are logically possible and are defined on the basis of the forms and relations (structures) we already have. Geometry used to be concerned with the study of the forms and relations of empirical space (and only within the limitations of Euclidean geometry), but now all other similar forms and relations have come within its purview. [22, p. 176]

Although, during the development of non-Euclidean geometry, alternative geometries were thought of as potential rival theories about physical space, in modern mathematics these alternative theories are considered as on a par, as equally separable from experience and as valuable taken alone, independent of a physical interpretation. Clearly there has been a departure from the Quine-friendly outlook prevalent during the early years of non-Euclidean geometry.

In fact, Kline, Zheng and Scanlan all see the history of non-Euclidean geometry as prompting a change in the nature of mathematics. Kline tells us that,

> ... the Greeks, Descartes, Newton, Euler, and many others believed mathematics to be the accurate description of real phenomena and that they regarded their work as the uncovering of the mathematical design of the universe. Mathematicians did deal with abstractions, but these were no more than the ideal forms of physical objects or happenings. [8, p. 1028]

Prior to the mid-nineteenth century at least, the Quinean picture of mathematics as closely entwined with science does seem fairly accurate. But developments since then have led to a change in the content of mathematics, as well as in perceptions of its proper scope:

> ... gradually and unwittingly mathematicians began to introduce concepts that had little or no direct physical meaning. Of these, negative and complex numbers were most troublesome. It was because

> these two types of numbers had no "reality" in nature that they were still suspect at the beginning of the nineteenth century, even though freely utilized by then. The geometrical representation of negative numbers as points or vectors in the complex plane, which, as Gauss remarked of the latter, gave them intuitive meaning and so made them admissible, may have delayed the realization that mathematics deals with man-made concepts. But then the introduction of quaternions, non-Euclidean geometry, complex elements in geometry, n-dimensional geometry, bizarre functions, and transfinite numbers forced the recognition of the artificiality of mathematics. [8, p. 1029]

Whether or not Kline is right that the change led mathematicians to the "correct view of the relationship of mathematics to nature" [8, p. 1028], it is clear that these new developments took mathematics beyond its previous close connection with physical interpretations. Insofar as we are interested in providing a philosophical account of mathematics as it exists today, the Quinean view once again falls short.

6. *Conclusion*

In short, then, the Quinean picture of mathematics as confirmed by its physical applications suggests that mathematicians should be forced to choose between alternative mathematical theories on the basis of their applicability in natural science. While on occasions (in the case of Catastrophe Theory for example) mathematicians have abandoned theories which have failed in application, this is not enough to support the Quinean picture, since, in this case, even the most vocal critics have not seen the failure of applications as speaking against the *truth* of the theory. The death of Catastrophe Theory can be understood purely in terms of a perceived lack of mathematical interest. In general, the examples discussed all speak against the notion that there is any need, beyond the practical, to choose between alternative consistent (or consistent looking) theories. Mathematics can make room for all of them — there is no need to battle for conceptual turf.

Is there any way to defend the Quinean ontological view against these observations of apparently un-Quinean mathematical practices? Well, there are certainly at least a couple of options. First, one could reject the methodological assumption that we started with, and argue that it is appropriate to present a philosophical account of mathematics that is at odds with mathematical practices. While this line of defence could well be attempted, anyone tempted by this view would at the very least have to explain why the current, supposedly misguided, practice of treating consistent theory candidates as ontologically on a par, is successful.

Alternatively, one could reject the claim that mathematicians should be concerned about the literal truth of their theories. Thus, a Quinean might argue that, although science confirms the literal truth of only some of our consistent theory candidates, the property of 'literal truth' is not mathematically interesting – from the perspective of pure mathematics, theories are valuable for reasons other than their genuine truth. In this case, we should expect mathematicians to pay no attention to the question of which of their theories are confirmed as literally true, since this is not a property that matters to them. Again, in this case, the Quinean still has work to do, first in explaining how theories that are not literally true can still be mathematically important, and second in explaining how mathematical methods allow mathematicians to stumble on 'literally true' theories even though they are designed without this value in mind. Finally, the Quinean picture needs to explain what is meant when mathematicians say that their claims are true, if this is something different from the 'literal truth' that is confirmed by applications.

Either way, although there is room to build a defence of the distinction between 'merely consistent' and 'literally true' theories from the Quinean perspective (and the same could be said for the perspective of Gödelian Platonism), observations of mathematical practices suggest, as a default, that all consistent theories are equally good from a mathematical perspective. Explaining this separation of the 'good' from the 'true' is thus vital to the survival of the Quinean programme — but once this distinction is understood, one might be forgiven for wondering what is added by saying, of some mathematical theories, that these ones stand out as 'literally true' whereas their equally good competitors are merely consistent shadows.

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