Mary Leng, *Mathematics and Reality*, Chaps 5 & 6*

Peter Smith

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1 Introduction

Where have we got to? In Chapter 2 of her book, Leng nails her colours to Quinean naturalism. Let's not now worry again about whether her arguments for starting from a broadly Quinean position in fact have sufficient force to persuade non-believers: there's surely still enough interest in the *conditional* question – *if* you are broadly sympathetic to Quine's project, how should you regard mathematics? In particular, should we go along with some version of an old-Quinean indispensability argument for the truth of at least some mathematics (and then infer from truth to successful reference to mathematical objects)?

In Chapter 3, Leng raises difficulties for Field's attempt to sidestep the indispensability argument by his programme for "nominalizing" physical theories. Again, let's not pause over this, but agree with Leng that *this* supposed route for the Quinean naturalist to avoid a commitment to mathematical entities looks beset by difficulties.

In Chapter 4, Leng takes on what we might call Maddy's challenge. Quinean naturalism, the challenge goes, is surely too narrow in what it counts as serious, truth-tracking, reference-making, cognitive projects. Pure mathematics is every bit as worthy of having an honorific status as physics, and treating a bit of mathematics as answering for its truth to its applicability in actual best science is to get the epistemology all wrong. As Maddy puts it, "the justifications given in mathematical practice differ from those offered in the course of the indispensability defence of realism". Now, Leng agrees with Maddy that the methods of pure mathematics are indeed radically different from the methods of physics. But she construes pure mathematics as typically an enquiry into what logically follows from some assumed axioms,[1] and she thinks that this enquiry can and should remain quite silent on whether the axioms are actually true of anything. So there *is* truth-tracking in pure maths, but it is tracking logical consequences, not what there is.

So pure mathematics, on Leng's view, is a recreational, 'if-then'-ist game. But that still allows an appeal to the indispensability argument to get a grip on *some* mathematics, namely on that mathematics which as a matter of fact is indispensably used

*We are reading Leng's book in the Logic Seminar for the second part of term. It fell to me to introduce discussion of these chapters, and I found myself (very rapidly indeed) writing these notes in trying to get my own thoughts clearer before the meeting. There was an excellent discussion, particularly involving the usual suspects, Tim Button, Luca Incurvati, Steven Methven, Rob Trueman. Tim, especially, had insightful things to say about Yablo which have led to some after-the-event changes. But otherwise these notes stay in a rough-and-ready form, and I'm not going to have time to do better: so caveat lector!

[1]This is, it has to be said, an odd position for someone like Leng who has elsewhere expressed Lakatosian sympathies.

in best science, regimented as tautly and economically as we can. And then – the old-Quinean argument goes – we have no reason to discriminate the maths from the rest of the regimented theory it is embedded in. In confirming our best theory overall, the indispensable mathematical propositions get confirmed holistically along with the rest of our regimented best theory. So these mathematical propositions will have to be regarded – fallibly, of course – as equally part of the truth about the world, and their ontological commitments acknowledged.

After the warm-up exercises of the preceding chapters, Leng's Chapter 5 aims to undermine this central old-Quinean line of thought by criticizing the assumed confirmational holism.

2 Beyond confirmation?

One line of challenge against an over-quick appeal to confirmational holism comes from Sober. The thought is that confirmation is characteristically contrastative: we confirm T' as against some rival T, and it is the differential content of T' as compared with T that should get the credit (so confirmation may be of a theory package T' but, still, different parts of the package might get different levels of confirmational credit in being contrasted with T). So that means, in differentially confirming some physical theory T' over a rival T no special credit will accrue to e.g. a shared mathematical framework of real analysis. In sum, if we are to confirm some mathematical theory component, we need to differentially test theories which essentially contain and lack that component.

But, Leng suggests, even if we accept that constraint on what counts as confirming, we can – seemingly contra Sober – fairly readily conceive putting mathematical components to that test. She borrows an example from Colyvan. Suppose the rival theories are T Newtonian mechanics nominalized a la Field, and T' General Relativity (which we'll suppose can't be nominalized). And the tests confirm T' over T. This confirms T', even by Sober's contrastative standards, and so it gives some differential confirmation to the indispensable mathematical component present in one theory and not the other.

This schematic example seems to conform to Sober's requirement for constrastative confirmation. But Leng's commentary on this is worrying. For she concedes that neither scientists nor mathematicians in practice would regard this sort of thing as confirming the mathematics in question. But no matter, she thinks. For physical scientists, it just isn't what they are interested in (there's a division of labour here); and as for mathematicians, they are interested in what follows from what, and whether their axioms are 'true' isn't really their concern either. This however seems really a bit insouciant, coming from a proclaimed naturalist. If scientists don't care about mathematical 'truth' and mathematicians don't either, then exactly why should *philosophers* care? Isn't a notion of mathematical 'truth' which is beyond the practitioners' ken, disconnected from what interests them, looking embarrassingly like a philosophers' artefact, and hence a fit target for naturalist skepticism? Well, let's for the moment flag the worry, and pass on.

3 A word about modality

Suppose we agree with Leng about how to respond to Sober: the old-Quinean *can* at least in principle find cases where – by his lights – some mathematical theory is differentially confirmed. It is contingent, of course, whether the world is such as to provide confirmation in this way for some patch of mathematics. But it doesn't immediately follow from *that*, of course, that it is a contingent matter whether that bit of mathematics is true.

So you *could* it seems hold that true mathematics is necessarily true – true across all worlds – but it is a local contingent matter which portion of this necessarily true mathematics you can get a confirmatory handle on. Or so Leng seems to allow. For example, we (let's suppose) have been able to confirm a bit of maths by confirming General Relativity; so (by our lights) it is true and hence – the supposition goes – it is true at all worlds, even at those worlds where the physics can be nominalized and the locals get no confirmation. Symmetrically for us. There could be maths which is true here, because indeed true at all worlds, but which is not here confirmable as true.

But now a surely unlovely picture is emerging. Is it that, for any bit of pure maths, it could be confirmed-as-true in some world, and hence is true at all worlds? Then the Quinean distinction between true-because-indispensable and merely-recreational maths has lost its point. Or is that not all maths as played with by pure mathematicians is true, but it can entirely transcend our confirmatory powers to discern which bits are and which bits aren't? Leng, in her remarks about modality, doesn't foreclose this option. But it would seem better for the old-Quinean to bite the bullet, and say mathematics construed as saying *if these axioms, then those consequences* is all happily necessarily true, but that any claim that this axiom holds true or that one doesn't is straightforwardly contingent. I'm not saying that's particularly attractive, but it looks easier to live with than combining an indispensability epistemology with a necessitarian view of maths.

4 Theories and idealization

So far, then, the old-Quinean position is still in play. But Leng now seeks to undermine it by challenging, the confirmational holism that underpins it in a different way to Sober's unsuccessful stab. The claim is that working scientists, by their own standards, often don't treat confirmation of a theory as confirming *all* of it. They discriminate. So a naturalist, honouring scientific standards, shouldn't endorse confirmational holism. On the contrary, scientists' own discriminatory practices prefigure the neo-Quinean discrimination that Leng is going to recommend – namely that we shouldn't take confirmation of a theory (even ideal confirmation of our very best theory) as confirming its mathematical core.

Leng works with two alleged examples. I'll take them in reverse order. She writes:

> There are cases where our theories indispensably posit objects of a particular sort, but where scientists hold back from accepting the existence of such objects until they have some more direct evidence of their existence. Maddy's example is of atomic theory, circa 1900: although this successful theory indispensably posited the existence of atoms, it was only when Jean Perrin's Brownian motion experiments provided some more direct evidence of the existence of such objects that many scientists became convinced of their reality. . . . It seems, then, that indispensable occurrence in a successful theory isn't always enough to convince scientists that they have reason to believe in the objects posited by our theories. (p. 123)

But I fear there is a pun on "success" here. In 1900, the atomic theory was indeed *successful as a 'just so' story*; if you went along with it, you could make some confirmed predictions. But, as I understand the history, lots of theorists took a merely instrumentalist line on the theory and they didn't believe the theory was a *successful description of nature*. They indeed held back from confidently endorsing the existence of atoms –

indispensable to the atomic theory, of course! – *because they held back from endorsing the theory* as more than a rough interim predictive tool on which they weren't prepared to take out any kind of serious bet. So this *isn't* a case where, as Leng later puts it,

> we have . . . [a] case where our observation of scientific practices suggests that the indispensable presence of a theoretical hypothesis in our overall best theory is not on its own considered as good enough reason to believe that hypothesis. (p. 125)

For in 1900, the atomic theory wasn't – at least for the non-believers – part of their "overall best theory". For them, it was either believed definitely-false-even-if-useful, or was still at best a mere speculation. So *this* gives us no model to follow of a case where scientists regard some part of their going *best* theory of the world as thoroughly confirmed but yet still do not believe true some part of it.

So let's turn to Leng's better case, the use of indispensable idealizations. Classical mechanics talks of idealized point particles, frictionless plains and the like. Fluid mechanics idealizes fluids as homogeneous continua. Drop the idealizations and we lose the elegant explanatory power of the theories. But we don't in endorsing the theories believe the idealizations are truth-tracking. Yet these theories are surely part of our "best theory of the world" (by ordinary 'naturalistic' standards of what works best, at any rate). Or so the story goes.

Well, here are three models of what is going on in making such idealizations:

1. The idealizations are convenient myths, and can in principle be cashed out in terms of limits. Thus Quine: "When one asserts that mass points behave thus and so, he can be understood as saying roughly thus: that particles of given mass behave the more nearly thus and so the smaller their volumes." But as Leng argues – correctly, I think – this treatment of idealization is of limited application, and doesn't apply e.g. to the case of fluid mechanics (where indeed the very opposite happens: the smaller and smaller you take volumes of real-world fluid, the less and less like continua they behave).

2. The Suppes/Giere model (also in my *Explaining Chaos*). We should think of 'pure' classical mechanics, say, as describing – truly, of course! – mathematical *abstracta* (e.g. like bundles of trajectories in the phase space of a 'classical pendulum'), and then 'applied' classical mechanics says that real-world behaviour of a swinging pendulum is close enough for long enough to the behaviour in the abstract model. This story is elegantly defended by Giere (and gets more support with fancier examples in my book). But unlike the ur-Quinean story, it does presuppose, it seems, that there are these abstracta to serve as objects of comparison.

   Thus applied classical mechanics says – as it might be – here is the actual solar system, here is a map from the solar system to an abstract orrery involving point particle planets orbiting a point sun in an abstract space, and here's a mathematical theory generating another abstract orrery which close-tracts the one the represents the actual solar system. Accepting all that *seems* to indispensably involve buying the abstracta.

3. But maybe not. Maybe we can still think in terms of the Suppes/Giere model while not really believing in the abstracta it postulates. Is that double-think? Well, Leng puts it like this, reverting to the example of fluid mechanics:

> we should see ourselves as speaking merely figuratively, and not literally, when we adopt the hypothesis that there are such [abstrata], in order to take advantage of the representational value of that hypothesis in allowing us to paint a picture of how things are with real fluids. The fact that we do not have an alternative literally believed account actually available to us, that does not assume the existence of ideal fluids, is not enough to show that it is not some figurative content of our theory of fluid dynamics that is responsible for its success, rather than its literal content (as a theory about the nature of the relation between abstract ideal and concrete real fluids).

But of course, this is, for the moment, just a promissory note of course. However, it gives a promise that Leng aims to make good on in Chapters 7–9. We will have to wait to see how things go!

Note, though, that Leng's own description in the present chapter of the idealization example is over-spun. She talks e.g. of her cases as illustrating "the theoretical utility of adopting hypotheses that are known to be false" (setting us up for the idea that un-true mathematics might still be useful). But that's just propaganda. Unless you are *already* taking a fictionalist line about the mathematics, on the Suppes/Giere model there *are* no false hypotheses when we idealize, properly understanding what we are up to. There are truths about the behaviour of abstract models governed by certain laws (or so defenders of the modelling approach will say), and *more truths* of the kind 'the dynamical behaviour of real world systems *approximates* certain behaviour in the abstract model'.

5 On Yablo on ontology

Leng's Chapter 6 is something of an aside (or at any rate, as John Burgess suggests in his review, arguably out of logical sequence). Here, she responds to Yablo's interesting but untidily written paper 'Does ontology rest on a mistake?'.

You can see why Leng wants to do this, as she shares with Yablo the idea that some theoretical discourse can be metaphorical/figurative/analogical. She shares with him the idea that Kendal Walton's treatment of metaphor and analogy can be recruited to deal with such theoretical fictions. And she wants too to insist that going broadly fictionalist doesn't land us with new ontological commitments as unwelcome as the abstracta we are trying to avoid: rather, as Walton puts it, 'Insofar as statements appearing to be about fictional entities are uttered in pretence, they introduce no metaphysical mysteries.' However, all this is only going to be explained properly in Chapter 7, so we might perhaps have expected an exploration of Yablo's different take on the same ideas to have come later. But be that as it may.

The point of contention, and the prompt for the present chapter, is this: Yablo worries that the naturalist isn't in a position to draw a sharp principled distinction between the non-metaphorical and the metaphorical parts of our best theory. This, in fact, looks far too much like the supposedly sharp internal/external (not to mention analytic/synthetic) distinction that the Quinean naturalist rejects. And if that's right, the neo-Quinean like Leng – who seems committed to drawing just such a sharp distinction between the ontologically committing non-metaphorical content of a theory and its fictional component – is in trouble.

So what is Yablo's key argument (according, at any rate, to Leng)? Well, Yablo observes that – even wearing our scientists' hats – we often say things in a 'make the most of it' spirit:

> I want to be understood as meaning what I literally say if my statement is literally true ... and meaning whatever my statement projects onto ... if my statement is literally false. It is thus indeterminate from my point of view whether I am advancing Ss literal content or not. .... When speakers declare that ... the number of As = the number of Bs ... they are more sure that S is getting something right than that the thing it is getting at is the proposition that S, as some literalist might construe it. If numbers exist, then yes, we are content to regard ourselves as having spoken literally. If not, then the claim was that As and Bs are equinumerous.

But if that's right, there's a circularity problem for any Quinean criterion of ontological commitment. For this tells us that there are numbers (by our own lights) if we quantify over such things in the literally believed part of the story we take to be true of the world; but our talk is only literal only if there are numbers. Yet – according to Yablo – the Quinean account of ontological commitment was the only serious game in town: as he puts it, if questions about our existential commitments had genuine answers, then the Quinean criterion would turn them up. But "it doesn't, so they don't".

Leng responds that it isn't (just) a matter of people's intentions whether they ought to be taken as speaking literally or metaphorically. It is a matter of how their theory logically hangs together. Thus we can ask about the role of what they propose in their overall theory, whether the theory would collapse if qualified with 'it is as if', whether we could have a reasonable account of how the theory can have the successes it does while construed metaphorically, etc.

Which isn't to say that such enquiry is easy to pursue. But, as Sklar has emphasized, it is in fact part of the project of science itself to investigate such matters. Leng mentions the rather boring example of explaining why it is safe to use Newtonian dynamics locally (she says safe 'to assume ... space is Newtonian' but that must be a slip). More interesting would e.g. be attempts to account for the applicability on non-reversible thermodynamics in a world whose base laws are time-symmetric. So Sklar – drawing morals from the practice of science – could insist that we can do better in delineating the literal base from some useful (perhaps practically indispensable) metaphorical add-ons than Yablo's remarks initially allow.

But two comments on this. First, it isn't at all clear that the sort of considerations that Leng mentions here really address Yablo's deeper concerns. His fundamental worry is, as I said, that the non-metaphorical/metaphorical distinction (take-it-literally, allow-it-in-regimented science vs useful myth) is just not a *sharp* one. Leng gestures, perhaps, to how we might begin to go about more carefully placing different bits of the enquiry on a spectrum from ploddingly literal to flight of fancy, but it goes no way to defending the claim contra Yablo that there is a clean distinction which tracks a sharp ontological divide between what there is (by our lights) and what there isn't.

Second, *must* the explanatory base involved in the Sklar-type explaining-away of the use of metaphor be construed literally through and through? If we take that line, then Leng will be in trouble. For she wants essentially mathematical physics to feature as (part of) our best theory of the world, without the mathematical component being read as literally true, and without our having anywhere else to go to explain its success. So, as she recognizes, Leng needs – contra Sklar, I think – to allow parts of theories to count as good-but-metaphorical when there isn't (so to speak) any kind of literal underpinning in the background to explain why the metaphors work as well as they do. Well again, we'll have to see how this works out in Chapter 7.

In the present chapter, however, she seemingly takes a wrong turning. For Leng starts talking about the sort of situation Hacking and Cartwright are interested in, where we are realists about the entities of a theory we don't fully believe. Sure, as Leng says, such cases illustrate how we can "uncover some genuine ontological commitments even from within the midst of theories whose literal truth we may doubt". But this sort of case looks orthogonal to the issues that are going to concern her about mathematical entities. It is one thing to say that we can be committed to Xs (electrons) in the context of a theory which says *false* things about Xs; it is another thing entirely to say that we can fail to be committed to Ys (real numbers) in the context where talk about Ys indispensably features in a theory saying *true* things about Xs. There is just no passage from one case to the other, and Hacking and Cartwright are really just a distraction.

6 Conclusion

Chapter 5 ends with a promissory note – to give an account of how we can use mathematical models Giere-style without believing that the abstracta in the models actually exist, an account which treats them figuratively. Chapter 6 ends with much the same promissory note – to given an account of how parts of theories (the mathematical modelling parts) can count as essentially figurative but indispensable. So all remains to play for in Chapter 7.