(Draft version.)

Missing Systems and the Face Value Practice

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Introduction – the overall structure

My talk today comes in three main parts: first, I'll set out the question I'm interested in; second, I'll say something to illustrate the interest and importance of the question; and third, I'll try to answer it.

The question

Scientific textbooks, classroom lectures, and journal articles abound with passages which look, in many respects, just like attempts to provide accurate descriptions of actual, concrete systems (or kinds of system) from the domain of inquiry of the scientific discipline in question, but which are neither intended nor taken as such by any competent practitioner of that discipline. Many passages of this sort are appropriately called "descriptions of idealized systems," but I also mean to include passages which might be called "descriptions of imaginary systems" or "descriptions of hypothetical systems," but which are not descriptions of idealized cousins of any real system – for example, Poincaré's description of a disk-shaped world in which temperature varies in a particular way with the distance from the centre, and in which all bodies, including measuring rods, expand and contract in a uniform fashion as they move from place to place, resulting in an apparently a Lobachevskian geometry. It will be useful to have a

general label to pick out all the passages I have in mind. Given that the key feature of such passages is that it is recognised from that outset that there are no actual, concrete systems in "the world around us" which fit the descriptions they contain, I'll call them "descriptions of missing systems."

It will also be useful to have a simple example clearly in mind, so here is one taken from chapter 3 of Ron Giere's *Explaining Science*. As Giere notes, a typical textbook in classical mechanics will contain a description of the simple pendulum. This is a system made up of a mass suspended by a rod or piece of string from a fixed point; the mass swings back and forth in a plane perpendicular to the ground. Standard textbook descriptions will say in so many words that the mass experiences no air resistance in its peregrinations, and that the rod or string experiences no frictional forces at the point of suspension. Other features of the system may be ascribed to it only implicitly, in the course of subsequent calculations: the pendulum is of unvarying length, for example, and is immersed in a gravitational field which has the same magnitude and direction at all the points through which it swings. It is a straightforward observation, however, that there are no real systems fitting this description. Every real pendulum encounters air resistance, and frictional forces at the point of suspension; no real rod or piece of string is perfectly rigid; no real pendulum moves through a perfectly uniform gravitational field, and so on. And, of course, competent physicists know all of this.

It is an equally straightforward observation that even though we know that no "real" pendulum – no actual, concrete, physical, spatiotemporal object which can be found in the world around us – fits the description of the simple pendulum, we tend to talk as though there is such a thing as the simple pendulum, to make claims about it, discuss its properties, and compare it to other things, in both our scientific and our philosophical discourse. And that observation generalizes to numerous other descriptions of missing systems found in classical mechanics texts, and to passages in textbooks relating to other branches of physics, in textbooks for other sciences, both

natural and social, and in other sorts of scientific document, such as journal articles and book-length treatises. That is, both scientists and philosophers often engage in the practice of talking and thinking as though *there are objects which fit the descriptions given in such passages*. Because it involves taking the passages in question at face value in a certain respect (or at least seeming to do so), I'll call this *the face value practice*.

Now perhaps in some cases this can be regarded as merely a harmless way of speaking; but not in all, I think. As the examples we will look at in a moment serve to illustrate, there are many cases in which philosophers engage in the face value practice in the process of offering answers to central epistemological and methodological questions about the sciences, and in which the centrality of the face value practice to their accounts – that is, the centrality of talking as though there are objects which correspond to descriptions of missing systems by fitting those descriptions – makes it difficult to see how the philosophers in question could claim to be indulging in a mere manner of speaking. Rather, their accounts seem to presuppose the existence of such objects. And so my question is this: Is there a way of thinking about descriptions of missing systems which legitimates such a heavy reliance on the face value practice, and does so in a way which can support the weight of the philosophical accounts in question? That is, can we make sense of the idea that there are, somehow, objects which correspond to descriptions of missing systems by fitting those descriptions, and can we make sense of that idea in a way that can underwrite the philosophical accounts which seem to rely on it? My answer is that we cannot – at least, not without incurring significant costs.

The face value practice at work

Here are three questions which concern us as philosophers of science. In the interests of time, I'll consider just one set of answers which seem to presuppose that there are objects corresponding to descriptions of missing systems – Ron Giere's.

First question:

What kinds of things *are* models?

Here is the heart of Giere's answer:

> I suggest calling the idealized systems discussed in mechanics texts "theoretical models," or, if the context is clear, simply "models."
> (*Explaining Science*, p. 79)

For Giere, the simple pendulum is a typical model; given that Giere at least talks as though there are models of this sort, he is thus talking as though there is such a thing as the simple pendulum – he is engaging in the face value practice.

Second question:

What sort of structure should we take a scientific theory to have?

Giere proposes that we think of a theory as "comprising two elements: (1) a population of models, and (2) various hypotheses linking those models with systems in the real world." Given Giere's notion of model, he thus engages in the face value practice again in offering his account of theories.

Giere seems to be presenting a version of the semantic view which has much in common with the view which appears in the writings of Patrick Suppes and Bas van Fraassen, an impression which van Fraassen reinforces in more than one place. But Suppes and van Fraassen are working with a notion of model on which a model is a mathematical structure of one sort or another, and so if Giere is working with a sufficiently distinct notion – as we might well take him to be, given that the simple

pendulum counts as typical model for him – the resulting view of theory structure will surely be quite different. So a question which arises at this point is whether there is a distinct and coherent version of the semantic view of theory structure to be found in Giere's work. I will argue that there is not.

Third question:

How does scientific representation proceed?

Giere offers an account of scientific representation which relies crucially on the practice of talking as though there are such things as the simple pendulum, frictionless planes, and so on. On Giere's account, "theoretical models" – that is, the systems described in the sorts of passage we are considering – "are the means by which scientists represent the world—both to themselves and for others." They represent, Giere proposes, by standing in relations of similarity to real systems, and it is the job of so-called *theoretical hypotheses* to make claims about the respects in and degrees to which this or that idealized system is similar to this or that real system. (Giere wisely denies, though, that representation just *is* similarity.) This is a widely discussed view of how at least some scientific representation proceeds, and it seems to be attractive to many. But to advocate such an account of representation is clearly to engage in the face value practice: to say that scientists represent real pendula by comparing them to the simple pendulum is to talk as though there is such a thing as the simple pendulum.

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It's worth emphasising that these are just examples, both of questions and answers. Lots of philosophers engage in the face value practice in doing lots of different kinds of philosophical work on the sciences, and I think that the problems I want to draw attention to arise in many such cases. Peter Achinstein's taxonomy of models and Frederick Suppe's account of the nature and function of idealization in scientific

theorising both make heavy use of the face value practice, for example (two cases I cover in more detail in the paper); and the answers we give to questions about the processes of model and theory construction, scientific explanation, evidence and confirmation, and realism will clearly be shaped, in part, by the ways in which we talk and think about models, theories, representation, and idealization. Indeed, both Giere and Suppe offer extended accounts of the epistemology and the methodology of the sciences which take as a starting point their talk, respectively, of "theoretical models" and "physical systems," and both kinds of talk are paradigmatic examples of the face value practice at work.

Interpreting the face value practice

So the question, again, is this: Can we make sense of the idea that there are, somehow, objects which correspond to descriptions of missing systems by fitting those descriptions, and can we make sense of that idea in a way which can underwrite the philosophical accounts which seem to rely on it? The argument of the rest of the paper can be thought of as a "multi-lemma," designed to show that we cannot. In the full paper I consider six ways of making sense of the idea that there are objects which correspond to descriptions of missing systems in a description-fitting way (or eight, depending on how you count).[1] Today I'll just talk about three of them, including one which emerges from the idea that we might think of descriptions of missing systems as little fictions. And I'll take the description of the simple pendulum as my primary example.

[1] Others: Teller; van Inwagen; possibilia as linguistic objects and as "abstract" representations; the direct counterfactual reading.

1. Straightforward descriptions of abstract objects

The simplest approach is to take it that corresponding to passages of the sort we have in mind, there is an entity which fits the description in the ordinary, straightforward way that things fit descriptions given of them – namely, by having the properties picked out by the predicates used in the description. On this approach, there is such a thing as simple pendulum, and it has all the properties mentioned in the description, just as Salman Rushdie has all the properties mentioned in an accurate description of him. If this is right, the face value practice *per se* needs no explaining: talking as though there are entities which fit the descriptions given in the passages in question is not just a manner of speaking.

There is an obvious problem with this approach, however – namely, that as we have noted, there are no actual, concrete, physical systems in the world around us which fit the description of the simple pendulum – no real pendula are perfectly rigid, or experience no friction at the point of suspension, or move through an unvarying gravitational field, and so on. So how could this be a tenable way of reading descriptions of the simple pendulum?

Here is Giere's solution to this problem:

> I propose that we regard the simple harmonic oscillator and the like as *abstract entities* having all and only the properties ascribed to them in standard texts.
>
> (*Explaining Science*, p. 78, Giere's emphasis)

That is, perhaps the simple pendulum is not to be found in "the world around us" just because it's an abstract object. Descriptions of the simple pendulum are, nonetheless, descriptions of it: it has the properties ascribed to it in those passages. So we are considering the option of reading descriptions of missing systems, like the simple pendulum, as *straightforward descriptions of abstract objects* – straightforward in the sense

that, on this reading, the simple pendulum actually has the properties denoted by the predicates appearing in the description.

Immediately, though, there is a problem with this story, and it is a very simple one: In order to address the fact that there are no simple pendula in the spatiotemporal world around us, Giere has to mean the term 'abstract' in a way which entails non-spatiotemporality. But no non-spatiotemporal object can have the properties ascribed to the simple pendulum, for no object which has, for example, a non-zero length, and behaves in the way the simple pendulum is said to behave in descriptions of it – moving through space over time in a particular way – can be non-spatiotemporal. In other words, given what Giere has to mean by 'abstract,' there is an internal inconsistency in the claim that the simple pendulum is an "*abstract entit*[*y*] having all and only the properties ascribed to [it] in standard texts."

Inconsistency is rarely a good thing, but there is an especially severe problem for Giere's views about representation here. Giere claims that, by taking representation to rest on relations of similarity between two nonlinguistic objects – namely, models and real systems – rather than taking it to involve some sort of correspondence between a linguistic object and the world, we will have found a way to skirt a host of thorny issues facing the philosopher of science, for we can then put aside our worries about truth, and correspondence. Amongst other things, Giere thinks this picture makes life easier for the would-be scientific realist – it is part of Giere's "constructive realism." But the appeal of this picture, its apparent simplicity and clarity, relies crucially on taking similarity to be a straightforward and familiar notion. And the straightforward way of understanding simplicity is in terms of the *sharing of properties* (or, perhaps, in terms of similar things having "nearby" determinates of the same determinable) – indeed, I think it is clear that that is just how Giere is thinking of similarity. But this makes it clear that Giere's account of representation relies for a large part of its appeal on the assumption that the simple pendulum (for example) has the very properties picked out by the predicates

which appear in the passages in question; only if it has those properties can it be similar to real pendula by sharing properties with them (or having "nearby" properties).

In short, then, Giere's account of representation relies on our taking his words quite literally when he says that the simple pendulum has the properties ascribed to it in textbook descriptions of it; but taking that claim literally is inconsistent with addressing the obvious fact that there are no actual, concrete objects satisfying the description of the simple pendulum by saying, as Giere does, that the simple pendulum is an abstract object.

(*Aside*: Paul Teller has responded to my claim that Giere's account is internally inconsistent by suggesting another way of thinking of models like the simple pendulum in his paper "Twilight of the Perfect Model Model." I'd be happy to talk about that in the discussion period.)

2. Indirect descriptions of abstract objects: mathematical structures

Given that we have ruled out the option of interpreting the passages in question as straightforward descriptions of abstract objects, then, we might next consider the possibility that they offer us *indirect* descriptions of abstract objects. One way of doing that is to suppose that there is an object corresponding to the description of the simple pendulum, but that it is a mathematical structure which "fits the description" not by having the properties denoted by the predicates appearing in the description, but by having various corresponding *mathematical* properties. Those are the sorts of property that an abstract object can have without any trouble.

Now this may well yield a workable picture of the semantics of descriptions of missing systems, and it may make sense of the face value practice – after all, on this picture there *is* an entity which fits the description given in the sort of passage we have in mind, provided only that description-fitting is understood in the right, somewhat roundabout way. And perhaps we can go on to say that the entities in question are

models, and represent, and are components of theories. But there are four important things we should note about the implications of understanding descriptions of missing systems in this way:

First, if we take the entities in question to be models, then the resulting notion of model is simply that of a mathematical structure which is used to represent systems from the domain of inquiry. But this is just the notion of model Suppes and van Fraassen had in mind when they presented their formulations of the semantic view – or at least, as I argue elsewhere, it is the notion they had in mind some of the time, and it is the notion they should have had in mind all the time. So there is no new notion of model here.

Secondly, and as a corollary, if Giere's version of the semantic view is taken in this way, then though it may or may not be novel in other respects, is not novel in employing a new notion of model.

Third, the picture of representation we are left with loses a good part of the appealing simplicity Giere's account seemed to have at first sight. The relation between the model and the modelled system which underlies the representation relation is not, after all, good old ordinary similarity, understood as property-sharing (or as the possession of nearby determinates of the same determinable). Instead, it starts to look as though the crucial relation between model and modelled is *structural* similarity; and the project of understanding representation in terms (or partly in terms) of structural similarity is not such an uncomplicated one, as much recent debate in the philosophy of science makes clear.

Fourth, there is a worry about scope: Can we plausibly take *every* description of a missing system which plays an important role in one of the sciences to pick out a corresponding mathematical structure? What about a passage characterising the nuclear model of the cell, say? Or a passage characterising, in largely non-mathematical terms, the billiard ball model of gases? It seems unlikely that we could take such passges to

pick out mathematical structures without engaging in some fairly unseemly contortions; and that is one reason for going on to consider some other options.

Interlude: descriptions of missing systems as little fictions

Let us now turn to consider the question about fiction I mentioned earlier: Might it help to think of descriptions of missing systems in the sciences as little fictions? The idea that some or all scientific representations are fictions is an old and familiar one, at least in some of its guises, and it is also an idea which is attracting a good amount of attention at the moment. There are a number of quite distinct claims, I think, which have been expressed in such terms, and more ideas besides which might be so expressed, and which deserve to be explored, but I will focus on just one question along these lines here (a new one, as far as I know): Might it help to think of descriptions of missing systems in the sciences as semantically and pragmatically on a par with descriptive passages in the sorts of things we ordinarily class as works of fiction? And, relatedly, are the systems described by the passages in question ontologically on a par with fictional characters? Is the simple pendulum the same sort of thing as Emma Bovary?

Let's begin with the utterances which make up the telling of a fiction – call them *fictive utterances*. There is, alas, no consensus on the semantics and pragmatics of fictive utterances, and there are important differences between the accounts which have been offered by Lewis, Searle, Walton, Currie, and others. For our purposes, however, it is enough to note that most accounts agree on at least one thing: in producing a fictive utterance, the author is not saying something true. This means that, if our elaboration of the "little fictions" idea begins and ends with the proposal that the utterances made in producing a description of the simple pendulum, say, are semantically and pragmatically on a par with fictive utterances, we run into two problems. First, we seem to get the result that it's not true that the simple pendulum moves sinusoidally (for

example), and that seems not entirely right – isn't there *some* sense in which that is true? Second, if the propositions expressed by the sentences making up a description of the simple pendulum are not true, then we seem to have no reason to posit an object which "fits the description" given in such a passage. Without such objects, however, it is hard to see how the views of models, theory structure, and representation which Giere has offered could be true – and the same goes for many other philosophical accounts which are presented by engaging in the face value practice, including those I mentioned due to Achinstein and Suppe.

Fortunately, there is more we can say by way of elaborating the "little fictions" idea, and by saying more we can avoid both of the problems just mentioned. (Don't get *too* excited: the news is not all good.) The crucial thing to notice with regard to ordinary fiction is that, in addition to the fictive utterances which make up a telling of fiction F, there are *meta-fictional utterances*: utterances which are (or can be taken to be) *about* F, in one way or another. The last option I'll consider for interpreting descriptions of missing systems is suggested by thinking about a particular kind of meta-fictional utterance.

3. Meta-fictional utterances and counterfactuals

Suppose I say to you, now, "Emma Bovary was a deeply dissatisfied woman," or "Sherlock Holmes lived at 122b Baker Street"; in saying either of these things I would be saying something true. These are utterances which occur *outside the fiction* – not as part of the telling of the story, but after the fact, so to speak.

Now of course there is a puzzle about *how* we can manage to say something true by uttering the words "Emma Bovary was a deeply dissatisfied woman" outside the fiction, especially if we are assuming that the utterances making up the fiction are (at least by and large) not making true claims, and if there is at least some straightforward sense in which Emma Bovary does not exist. The standard solution is to propose that there is a tacit "fiction operator" present as a prefix to such extra-textual utterances:

what I am *really* saying when I utter that sentence outside the fiction is "According to the novel *Madame Bovary*, Emma Bovary was a deeply dissatisfied woman." *That* claim is a claim about what is true *in* a certain story, and it is surely true.

Analogously, then, perhaps the description of the simple pendulum given in the textbook is a little fiction, and although the utterances made in giving the description initially are not true, we can nonetheless say something true by uttering the sentence "The simple pendulum moves sinusoidally" outside the fiction, because then we are really just saying that according to the simple pendulum story, the simple pendulum moves sinusoidally.

This suggestion solves the first of the problems we encountered in taking the fiction approach – the problem of explaining why it seems true to say, for example, that the simple pendulum moves sinusoidally – but we do not yet have an entity to play the sort of roles Giere has in mind. However, we can go on elaborating the present approach in a way that will get us one.

The trick is to note that there is now a further question we can ask: When is a given proposition, P, true in a given fiction F? That is, when is a claim of the form "According to F, P" true? This is a non-trivial issue because a little reflection shows that it will not do to say simply that P is true in F iff some sentence is employed in the telling of F which expresses P. It is presumably true that Emma Bovary had two nostrils (to adapt an example of David Lewis's), even though, to the best of my recollection, Flaubert nowhere says as much.

There is more than one theory of when P is true in F, but a particularly well-known one which seems promising from our point of view is Lewis's, as presented in the paper "Truth in Fiction." Lewis actually offers two alternative analyses, which in his view correspond to two distinct usages, but I can make my points by considering just the first:

> What is true in [or according to] the Sherlock Holmes stories is what would be true if those stories were told as known fact rather than as fiction.
> ("Truth in Fiction," p. 42)

We can now propose that if the story of the simple pendulum had been told as known fact, then it would have been true that there was a concrete, spatiotemporal thing with all the properties ascribed to the simple pendulum in the simple pendulum story. And this suggests taking the simple pendulum to be a merely possible object: something which does not actually exist, but might have.

This move plunges us into the dark heart of the metaphysics of modality, but there is one way of elaborating the notion of a merely possible object which turns out to yield an interesting way of developing the approach we are considering: First (and here comes the bad news), embrace the ontology of Lewisian modal realism. Then take a merely possible object to be an inhabitant of any world other than ours. The simple pendula can then be concrete, spatiotemporal objects each of which has a length, and is perfectly rigid, and moves through an unvarying gravitational field, and experiences no frictional forces, and so on, but each of which lives in a world spatiotemporally and causally unrelated to ours.

If simple pendula are things of that sort, then they fit the description given in the standard textbook passages in the most straightforward way there is; and so we might seem to be in a position to conclude that the practice of talking as though there are objects corresponding to such passages is unproblematic. Furthermore, such concrete possibilia will stand in relations of similarity to concrete systems in the actual world, and similar pairs of objects, one in some other possible world and one here in the actual world, will stand in relations of similarity quite straightforwardly, by sharing properties or by possessing nearby determinates of various determinables. The raw materials needed for Giere's account of representation thus seem to be at our disposal on this approach.

Interestingly, this is an idea which Lewis himself mentions in *On the Plurality of Worlds*:

> Idealisations are unactualised things to which it is useful to compare actual things....The frictionless planes, the ideal gases, the ideally rational belief systems–one and all, these are things that exist as parts of worlds other than our own. The scientific utility of talking of idealisations is among the theoretical benefits to be found in the paradise of *possibilia*. (1986, p. 27)

Paraphrasing Lewis, then, we might be tempted to claim that among the theoretical benefits to be found in the paradise of Lewisian possibilia is that of making sense of the face value practice.

Now the obvious disadvantage of this approach is that tickets to the paradise in question are not cheap: there are some significant ontological commitments to be shouldered, most obviously, but there are commitments in the realms of epistemology and semantic theory, too. Perhaps this is a fair price to pay, however, and perhaps the necessary commitments can be independently motivated – certainly Lewis argues at great length that they can. Needless to say, I won't try to settle those large issues here. Instead, let me point out two more specific problems with the approach.

First a problem we encounter if we try to combine the story so far with Giere's proposal that the objects which correspond to descriptions of missing systems are components of theories. Apart from the fact that there is something odd-sounding about the idea that classical mechanics, say, is made up in part of physical objects from other worlds, there is the difficulty that the objects in question do not *actually* exist. If many of the constituents of a theory such as classical mechanics do not actually exist, then we seem forced into saying that classical mechanics itself does not actually exist – or, to put it another way, that there is no actual theory called "classical mechanics." (*Aside*: In fact, there is a way of avoiding this unpalatable consequence, and it is built into Lewis's machinery, put there by him for other reasons. It would be very ad hoc to adopt it simply to fix the problem I have just sketched, however, and the alternative would be to

motivate it by appealing to Lewis's theories of properties, events, and propositions – hardly a trivial addition to our list of commitments. I can say more about this in the discussion period if you'd like.)

The second more specific problem with the approach we are considering is that some descriptions of missing systems contain logical inconsistencies. One example, as Mathias Frisch has pointed out, is found in textbook treatments of synchrotron radiation in classical electrodynamics. Clearly no logically inconsistent passage can be taken to be a straightforward description of a Lewisian merely possible object. There are ways out here: taking inconsistent passages to describe combinations of Lewisian possibilia, or retreating to a more bluntly disjunctive account of, say, representation, or even the Meinongian postulation of impossible objects. But the awkwardness and lack of coherence in any of the resulting pictures is surely a strike against the idea of taking even *consistent* descriptions of missing systems to pick out Lewisian possibilia.

Conclusion

Where does all this leave us? Well, we have explored several ways of making sense of the face value practice by taking it seriously, so to speak – that is, by supposing that there really are objects fitting the descriptions provided in the passages I have been calling "descriptions of missing systems." Along the way, I hope, it has become clear that the face value practice cannot be treated as unproblematic if we are going to engage in it when offering up accounts of such central topics as scientific representation, the nature of models, and the structure of theories. Furthermore, I think we have seen that it has yet to be shown that there is a trouble-free way of interpreting the practice on which it can support all the uses to which philosophers have put it. My own inclination is to think that we should learn to do without entities corresponding to descriptions of idealized and imaginary systems, and, accordingly, that at least for the purposes of philosophical thinking, we should eschew the face value practice as misleading and obfuscatory. There is a case for that conclusion to be found in the fact that we have encountered the various difficulties detailed above in the attempt to spell out an explicit understanding of the face value practice on which there *are* objects corresponding to descriptions of idealized and imaginary systems. Believing in such objects seems bound to land us in trouble one way or another. The next step is to say how we should understand descriptions of idealized and imaginary systems, and how we should think about representation, models, theory structure, idealization, and the rest, if we insist on doing without them.