MISSING SYSTEMS AND THE FACE VALUE PRACTICE[1]

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1. THE QUESTIONS

Scientific textbooks, classroom lectures, and journal articles abound with passages which look just like accurate descriptions of actual, concrete systems 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. Typically, at least, there indeed are no actual, concrete systems in the world around us which fit the descriptions given in such passages; typically, that is, competent practitioners are not so unlucky as to give accurate descriptions of systems in the domain of inquiry by accident. I will call any such passage a *description of a missing system*. That is, a description of a missing system is any chunk of scientific text which has the following three features: (i) it has the surface appearance of an accurate description of an actual, concrete system (or kind of system) from the domain of inquiry, but (ii) there are no actual, concrete systems in the world around us which fit the description it contains, and (iii) that fact is recognised from the outset by competent practitioners of the scientific discipline in question.

A relatively familiar and uncomplicated example, borrowed from chapter 3 of Ronald Giere's *Explaining Science* (1988, pp. 70-71), is that of a passage describing of the simple pendulum. Textbooks in classical mechanics at the intermediate level invariably contain such passages, often accompanied by diagrams. The simple pendulum, we are told, is made up of a mass, and a rod or piece of string by which the mass is suspended from a fixed point; the mass swings back and forth in a plane perpendicular to the ground. It may be said in so many words that the mass encounters no air resistance in its travels, and that there are 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, and one which Giere himself emphasises, that there are no real systems fitting this description (Giere, 1988, p. 78; see also pp. 70-1). 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. Competent physicists, of course, know all of this. A passage we are wont to call a "description of the simple pendulum" is thus a description of a missing system.

(Note that the issue here is not idealization. Some descriptions of missing systems, including descriptions of the simple pendulum, are appropriately called "descriptions of idealized systems," but others are more typically called "descriptions of imaginary systems" or "descriptions of hypothetical systems," and are not seen as descriptions of idealized cousins of any real system—consider, for example, Poincaré's famous description of a spherical, thermally-varying world with an apparently non-Euclidean geometry (1952, pp. 64-68). Whether a given description of a missing system

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[1] This paper has been distilled from parts of Thomson-Jones (2007), a much longer, unpublished paper of the same title which can be found online at http://philsci-archive.pitt.edu/archive/00003519 and http://www.oberlin.edu/faculty/mthomson-jones.

also counts as a description of an idealized system can be both philosophically and scientifically important in the right context, but it will not matter here.[2])

Surrounding descriptions of missing systems is a practice of talking and thinking as though there are systems which fit them perfectly, in the full knowledge that no actual, concrete systems do so. To continue with our example: Even though we know that no "real" pendulum—no actual, concrete, physical, spatiotemporal object found in the world around us—fits the description of the simple pendulum, we tend to talk as though there is such a thing, make claims about it, discuss its properties, and compare it to other systems, in both our scientific and our philosophical discourse. Because the practice of talking and thinking this way involves taking descriptions of missing systems at face value in a certain respect (or at least seeming to do so), I will call it *the face value practice*.

I will rely on the example of the simple pendulum, drawn from a particular branch of physics, repeatedly in what follows, but it is important to bear in mind that both descriptions of missing systems and the face value practice play a central role throughout the sciences, natural and social, and can be found just as easily in journal articles and book-length treatises as in textbooks. Yet textbook descriptions of the simple pendulum and the discourse surrounding them serve as good stand-ins, I think, for descriptions of missing systems and instances of the face value practice wherever they may be found. Accordingly, although we will keep an eye out for points at which differences might matter, my arguments are intended to be quite general.

Two interrelated clusters of questions come to mind immediately when we reflect on these twin aspects of scientific and philosophical discourse. (1) How should we interpret descriptions of missing systems? That is, what sort of account should be given of their semantics and pragmatics? One central question here is whether, even

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[2] See Jones (2005) for my own attempt to clarify our thinking about idealization.

though (by definition) there are no actual, concrete objects in the world around us which fit such descriptions, there are objects of some other sort which do fit them in some way or other—*description-fitting objects*, as we might call them. If so, what sorts of things are they, and in what sense do they fit the descriptions in question? (2) How should we make sense of the face value practice? Can we simply take the utterances we produce when engaged in the face value practice to express claims about a realm of description-fitting objects? If not, are we perhaps indulging a useful fiction when we engage in the face value practice? Or are we instead uttering claims which, though true, do not commit us to the existence of description-fitting objects? That is, will the correct understanding of face value practice talk be uncovered by some paraphrase which eschews even apparent reference to missing systems?

One aim I have in this paper is to make some progress towards answering these questions, for given the central role that descriptions of missing systems and the face value practice play in the sciences, answering them is (in my view) a necessary part of answering more familiar philosophical questions about scientific explanation, confirmation, the testing of theories and models, realism and anti-realism, and so on. Thus, although they are questions partly in semantics and partly in ontology, I am pursuing them here because of what their answers will tell us about the feasibility of various views in the epistemology and methodology of the sciences. To that end, I also want to raise and address a third question, one provoked by the observation that face-value-practice utterances show up not just in the quasi-scientific discourse of the philosophy of science—in a presentation of the content of this or that scientific theory, say—but at the very centre of some answers to the epistemological and methodological questions which concern us. The question is this: (3) Is there a set of plausible answers to the questions in (1) and (2) which legitimates such philosophical uses of the face value practice, and can support the weight of accounts which rely on it in that way?

In section 2, I give some examples drawn from Giere's work of this latter kind of philosophical employment of the face value practice.[3] Then, in section 3, I consider three ways of interpreting descriptions of missing systems, and corresponding interpretations of the face value practice—three sets of answers to the questions in (1) and (2). More specifically, we will discuss the proposals that descriptions of missing systems are straightforward descriptions of abstract objects, that they are indirect descriptions of "property-containing" abstracta, and that they are (in a different way) indirect descriptions of mathematical structures. All three proposals are implicitly present at various places in the literature, and I find all three wanting. None of them provides a satisfactory answer to the questions in (1) and (2), and none of them gives us grounds for a positive answer to question (3). The result is to highlight the importance of developing a satisfactory understanding of descriptions of missing systems and of the face value practice, to put pressure on philosophical accounts which rely on the face value practice, and (more specifically, and by example) to help us assess the viability of certain approaches to thinking about models, theory structure, and scientific representation.

The central puzzle here is to know how to think about descriptions of missing systems, and our tendency to talk as though there are objects which fit them when, by definition, there are no actual, concrete objects in the world around us which do so. The three solutions to that puzzle considered in this paper resolve the tension by supposing that, although there are (actual) description-fitting objects, they are abstracta, and hence not to be found in "the world around us." Elsewhere, I consider solutions which instead suppose that there are description-fitting objects which are possibilia of various kinds, along with further variations on the idea that they might be (actual) abstracta.[4] Some of those solutions emerge from the idea that descriptions of missing systems are implicitly

[3] See Thomson-Jones (2007) for additional examples.

counterfactual; some from the idea that they are little fictions, semantically and pragmatically on a par with descriptive passages from works of ordinary fiction; and some from both. The cumulative effect, I think, should be to incline us to reject the idea that there are description-fitting objects corresponding to descriptions of missing systems—although, admittedly, there are further variants of the little fictions approach which would embrace such objects and which have yet to be explored.

What if we do become convinced that the right answers to (1) and (2) will eschew description-fitting objects? The main point I wish to press on that score in the present discussion is that this conclusion would force us to reject, or at least to significantly reinterpret, many philosophical accounts which themselves engage in the face value practice, just because, taken literally, such accounts rely so heavily on the existence of a realm of description-fitting objects corresponding to descriptions of missing systems. And if we attempt to avoid rejection by reinterpretation, the reinterpretation will have to be so extensive that the views in question will hardly be recognisable; indeed, we will not be able to claim to have gleaned much in the way of philosophical understanding until the interpretive work is done. One of my aims, then, is to provide reasons for thinking that we should not be cavalier about engaging in the face value practice in our philosophical work.[5] The first step, however, is to look at some examples of answers to central questions in the philosophy of science which bring the face value practice into play.

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[4] See Thomson-Jones (2007), sections 3.4-3.6, and (in preparation b).

[5] No doubt there will be some who would wish to insist, to the contrary, that philosophers of science can leave such questions to philosophers of language and metaphysicians, safe in the assumption that any adequate account of our talk about missing systems will legitimate the face value practice. I reply to this line of thought in section 4 of Thomson-Jones (2007).

2. THE FACE VALUE PRACTICE AT WORK

2.1 *Models*

The term 'model' has been come to play an increasingly important role in the philosophy of science over the last forty years or so. One reason for this (though not the only one) is that scientists themselves use the term regularly, describing some of their central theoretical activities as centering on the construction and exploration of "models." So one question philosophers of science have come to be concerned with is:

What are models?

or, more carefully,

What kinds of things are scientists talking about when they talk about models?

I will take the question to allow that scientists may be talking about more than one kind of thing.

Though his main focus is on a different question (see the next subsection), Giere offers an answer to this question in the chapter of *Explaining Science* in which the example of the simple pendulum appears:

> I suggest calling the idealized systems discussed in mechanics texts "theoretical models," or, if the context is clear, simply "models." This suggestion fits well with the way scientists themselves use this (perhaps overused) term. (1988, p. 79)

It is clear from his discussion that Giere intends to count the simple pendulum as a typical example of the "idealized systems" he has in mind (*ibid*., pp. 64-71), and it is thus a good example of a model in his sense (see also *ibid*., pp. 79-80). Thus when Giere talks as though there are models, he is talking as though there is such a thing as the simple pendulum—he is engaging in the face value practice.

2.2 *Theories*

Of course, one of the places in which talk of models plays a central role in contemporary philosophy of science is in the discussion of theory structure. I agree with a growing number of authors that it is important for us to think about models and the practices of modelling quite independently of any interest in theory structure,[6] but a good account of theory structure is surely desirable nonetheless, and the main contender is currently the semantic view, according to which a theory is best thought of as consisting, at least in part, of a collection of models. Any proponent of the semantic view whose conception of models is such that we are engaging in the face value practice in talking as though there are such things will thus need to engage in that practice in offering up her view of theory structure. This is just Giere's situation. In *Explaining Science* and later writings, 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" (1988, p. 85); and we have already seen that Giere's notion of model entangles us in the face value practice.

There is another issue here, too. In making this proposal, Giere seems at first glance to be presenting a version of the semantic view which is quite close to the view which appears in the seminal writings of Patrick Suppes and Bas van Fraassen,[7] an impression which van Fraassen reinforces (e.g., 1987, p. 109), and it has become common practice to cite Giere's writings alongside those of Suppes and van Fraassen without

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[6] See, for example, many of the papers in Morrison and Morgan (1999) and Godfrey-Smith (2006). I take it that Cartwright (1983) is a seminal work in this connection; see essays 7 and 8 in particular.

[7] See, for example, Suppes (1957, ch. 12), (1960), (1967) and van Fraassen (1970), (1980, ch. 3), (1987).

further comment when discussing the semantic view. But as I argue in Thomson-Jones (2006), Suppes and van Fraassen, properly understood, are employing the notion of a *mathematical model*—that is, a mathematical structure used to represent a (type of) system under study—and although we will see in the next subsection that Giere does take the simple pendulum and its ilk to be objects we use for the purpose of representation, we will also see in section 3.1 that he does not seem to think of them as mathematical structures. Given, then, that different notions of model might give rise to importantly different views of theory structure, another 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 writings. It follows from the points I make in section 3.1 that there is not—although, as we will see in section 3.2, it may be possible to arrive at one by modifying Giere's notion of model somewhat, provided we are willing to take the necessary baggage on board.

2.3 *Representation*

Here is a third question, on a topic which has been gaining momentum recently:

How does scientific representation proceed?

This formulation is intended to leave open the further questions of whether scientific representation always proceeds in the same way, and whether scientific representation is different from other kinds.

Building on the proposals we have already mentioned, 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"—missing systems *par excellence*—"are the means by which scientists represent the world—both to themselves and for others. They are used to represent the diverse systems found in the real world…." (1988, p. 80). They represent, Giere proposes, by standing in relations of similarity to real systems, and it is the job of *theoretical hypotheses* to make claims about the respects in and degrees to which this or that model is similar to this or that real system.[8] This is a widely discussed view of how at least some scientific representation proceeds, and it seems to be attractive to many.[9] And to advocate such an account of representation is clearly to engage in the practice we are concerned with: to say that scientists (sometimes) represent real pendula by comparing them to the simple pendulum is, at least on the face of it, to talk as though there is such a thing as the simple pendulum.

3. INTERPRETING THE PRACTICE

So far we have seen examples of a philosopher engaging in the face value practice—the practice of talking and thinking as though there are things which fit the descriptions given in the passages we are calling descriptions of missing systems—and doing so in the process of offering answers to three central philosophical questions about the sciences. The point generalises, and in two ways: there are many other philosophers who engage in the face value practice, and many other questions to which philosophers have offered answers partly by engaging in the practice. Questions about scientific explanation, evidence and confirmation, idealization, the processes of model and theory construction, and realism have all been addressed in ways in which make use of the face

[8] Giere develops and modifies this account in various ways in later writings—e.g., his (2004)—but not in ways which make an important difference here.

[9] Peter Godfrey-Smith endorses the basic picture as capturing *one* sort of scientific representation (2006, sections 1 and 4), although he opts for a development of that basic picture which goes beyond Giere's writings, and is perhaps at odds with it in certain respects (*ibid*., pp. 734-735 and 736-737).

value practice. This is hardly surprising, perhaps, given what we have seen so far, as our answers to those questions will clearly be shaped, in part, by the ways in which we talk and think about models, theories, representation, and idealization. Indeed, Giere (1988) offers an extended account of the epistemology and the methodology of the sciences which takes as a starting point his talk of "theoretical models."

Recall now that the questions I am interested in are these: How should we interpret descriptions of missing systems? How can we make sense of the face value practice? And is there a way of thinking about descriptions of missing systems which legitimates a heavy philosophical reliance on the face value practice, such as Giere's, and does so in a way which can support the weight of the philosophical accounts in question? The first question is clearly the fundamental one, and we now turn to consider three answers to it, returning to the other two questions at regular intervals.

3.1 *Straightforward descriptions of abstract objects*

The simplest approach is to take it that corresponding to a description of the simple pendulum there is in fact an object fitting 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.[10] We can, of course, call this object the simple pendulum. On this approach, then, there is such a thing as the 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. The utterances making up a description of the simple pendulum are true assertions about that object. If this approach is the right one, the face value practice needs no explaining:

[10] Of, if you prefer, by being an entity to which the predicates used in the description apply. I will leave it to those readers who are made nervous by talk of properties to substitute such formulations throughout the following discussion.

talking as though there are objects which fit the description given in such a passage is not a mere manner of speaking.

Of course, this approach faces an obvious difficulty, 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 are frictionless at the point of suspension, or move through an unvarying gravitational field. Indeed, this is part of what makes such a passage count as a description of a missing system. So how could this be a tenable way of reading the description of the simple pendulum?

Here is Giere's way of posing this problem, and his solution to it:

> Mechanics texts continually refer to such things as "the linear oscillator," "the free motion of a symmetrical rigid body," "the motion of a body subject only to a central gravitational force," and the like. Yet the texts themselves make clear that the paradigm examples of such systems [in the world around us] fail to satisfy fully the equations by which they are described. No frictionless pendulum exists, nor does any body subject to no external forces whatsoever. How are we to make sense of this apparent conflict?…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.
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> (1988, p. 78, Giere's emphasis)

In other words, there is such a thing as the simple pendulum, but it is not part of the spatio-temporal world around us—instead, it is 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 the object described 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 for this proposal to address the initial problem posed by the absence of simple pendula from the spatio-temporal world around us, Giere has to be using the term 'abstract' in such a way that it entails non-spatiotemporality (as, indeed, it is most often intended to do). But no non-spatiotemporal object can have the properties ascribed to the simple pendulum, for no object which has, for example, a 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" (*ibid*., Giere's emphasis).[11]

To put the point another way: Any object which had, in the straightforward way of having properties, the properties mentioned in descriptions of the simple pendulum, would perforce be a spatiotemporal object. Thus we cannot say both that there are no simple pendula in the world around us and that there exists an object which has, in the straightforward way of having properties, the properties mentioned in descriptions of the simple pendulum. We must clearly reject the latter claim, and with it the reading of descriptions of missing systems under consideration.

One natural next move is to hold on to the idea that there is such a thing as the simple pendulum, and to suppose that it stands in some relation other than instantiation to the properties mentioned in the textbook descriptions. I will consider two variations on that theme in what follows. But we should pause to note that replacing the straightforward reading with any such alternative will reduce the initial appeal of Giere's account of representation, and may reduce it significantly. Giere claims that taking representation to rest on relations of similarity between models and real systems in the domain of inquiry provides a way of skirting a host of thorny issues facing the philosopher of science, because (he claims) doing so allows us to put aside our worries about truth and correspondence (*ibid*., pp. 79, 81, 82). Scientific representation is not

[11] There is a small gap in my reasoning here—perhaps the simple pendulum, though spatiotemporal, does not inhabit *our* space-time. See section Thomson-Jones (2007), section 3.6, and (in preparation b) for more.

fundamentally a matter of some sort of correspondence between a linguistic object, such as a sentence or a proposition, and the world, or a part of it; instead, it has centrally to do with relations of similarity between two nonlinguistic objects, a model (the sort of thing which is described by the kinds of passages we are considering) and a "real system" (*ibid*., pp. 80-81, and chapters 3 and 4 *passim*). Amongst other things, Giere thinks this picture makes life easier for the would-be scientific realist (*ibid*., chapter 4, esp. pp. 106-107). But the appeal of this picture, its apparent simplicity and clarity, relies crucially on taking similarity to be a straightforward and familiar notion. The most straightforward way of being similar in a respect consists simply in having a property in common. Closely related, and only slightly less straightforward, is the sort of similarity which consists in the having of "nearby" determinates of the same determinable.[12] The heightwise similarity of the two Buddhas on my bookshelf, for example, consists in their having different but nearby heights. And this seems to be just how Giere is thinking of similarity:

> The general form of a theoretical hypothesis is thus: such-and-such identifiable real system is similar to a designated model in indicated respects and degrees. To take a[n]…example:
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> > The positions and velocities of the earth and moon in the earth-moon system are very close to those of a two-particle Newtonian model with an inverse square central force.
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> Here the respects are "position" and "velocity," while the degree is claimed to be "very close." (*ibid*., p. 81)[13]

Thus, on Giere's view, the simple pendulum is similar to the pendulum in the grandfather clock that sits in his living room partly in that they execute similar motions, both moving approximately in accordance with the equation

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[12] As judged by some relevant metric (using the term loosely), and some relevant standard of closeness.

[13] It might be thought that the quotation marks around 'position' and 'velocity' in the last sentence of this quote are functioning as scare quotes, and that Giere does not mean *real* position and *real* velocity. That reading, however, would make little sense of the quotation marks around 'very close.' I take it, rather, that Giere intends the quotation marks as, well, quotation marks.

$$\ddot{x} = -(g/L)x$$

(where x is the horizontal displacement of the bob, L is the length of the pendulum, and g measures the strength of the homogeneous gravitational field acting on the pendulum). The difference is just that the approximation is somewhat less good in the case of the clock pendulum, as there we have frictional forces, air resistance, variations in the gravitational field, and so on (*ibid*., pp. 70-71 and 76-78).[14] 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 ascribed to it 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.[15]

In short, then, much of the appeal of Giere's account of representation relies on our taking his proposal—that the simple pendulum has the properties ascribed to it in textbook descriptions—to be employing the ordinary notion of having a property; but taken that way, the proposal is inconsistent with the obvious fact that there are no actual, concrete objects satisfying the description of the simple pendulum in the spatiotemporal world around us.[16] And as we have seen, it will not help to say, as Giere does, that the simple pendulum is an abstract object: if 'abstract' in part means non-spatiotemporal, we again have an inconsistency, as no object which has (in the ordinary way) the properties in question can be non-spatiotemporal; and if 'abstract' does not in

[14] The equation is still an approximation in the case of the simple pendulum, one which is reasonable only at small angles of swing, as it incorporates the approximation that $\sin\theta = \theta$ (or equivalently, that $\cos\theta = 1$). For the sake of brevity, I will set this point aside at various points in the remainder of the discussion, and say simply that the simple pendulum moves sinusoidally.

[15] Hughes (1997, pp. S329-S330) also notes that abstract objects cannot be straightforwardly similar (as I have put it) to concrete objects in all the respects Giere's account of representation requires. Thanks to Gabriele Contessa for drawing my attention to this.

[16] Modulo the qualification made in n. 11, above.

part mean non-spatiotemporal, then calling the object abstract does not address the problem.[17]

3.2 *Indirect descriptions of abstract objects: property-containing*

The problem with taking descriptions of missing systems to be straightforward descriptions of abstract objects is that nothing which instantiates the properties picked out by the predicates appearing in the descriptions can either be absent from the world around us, or (therefore) be abstract, in more than one standard sense of that term. One revision which suggests itself, then, is to suppose that corresponding to each description of a missing system is an abstract object which involves the properties in question in some way or other, but does not instantiate them.[18]

Paul Teller (2001) recommends an approach of just this sort to the notion of model. In doing so, he is in part seeking to defend Giere's account against my argument for thinking that it is internally inconsistent as it stands.[19] Here is Teller's own presentation of his idea:

> Modelers intend talk of similarity between a concrete system and a model as an abstract object to be understood as a comparison between the model and the properties—perfectly respectable abstract objects—instantiated by the concrete object being compared. Details will vary with one's account of instantiation, of properties and other abstract objects, and of the way properties enter into models. But for this presentation we can express the idea by saying that concrete objects HAVE properties and that properties are PARTS of models. One makes comparisons between the properties, for

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[17] As noted above (n. 9), Godfrey-Smith (2006) embraces many of the essential elements of Giere's picture of representation. Accordingly, I take it that even though Godfrey-Smith classifies the missing systems as imagined concrete entities (*ibid.*, section 4), rather than abstract ones, his account inherits this central problem. Part of the appeal of saying that scientists (sometimes) represent real systems by making claims about the ways in which they are similar to imagined concrete entities comes from the appearance of simplicity in talking about similarity. But imagined concrete cells (say), whatever they are, cannot be similar to real cells in the appealingly simple way and be nonspatiotemporal.

[18] In discussions of the metaphysics of properties some reserve the term 'instantiation' to denote a special relation which is supposed to hold between particulars and Platonic universals. Here and throughout, however, I intend 'x instantiates ϕ' to be just another way of saying that x has ϕ, whatever the correct account of that locution may be.

[19] See Teller (2001), p. 399, n. 13 and the corresponding text. Teller is in part responding to an unpublished paper of mine ("Models and Idealized Systems") which contained the objections to Giere's account found in section 3.1 of the present paper, and which I began circulating in 1997.

> example, the property of having three vertices, that a concrete object has and the properties that occur as parts or components of the representing model.
> (2001, p. 399)

Reframed as a proposal for interpreting descriptions of missing systems, the idea can summarised this way: Begin by assuming that there are properties, and that they are abstract objects. Then suppose that corresponding to any given description of a missing system, there is an object which contains the properties mentioned in the description as parts, but which does not instantiate them. Descriptions of missing systems can then be read as indirect descriptions of such abstract objects, telling us not what properties they have, but what properties they contain as parts.

If there is such an object—call it a *property-containing object*—corresponding to the description of the simple pendulum, then we can, if we like, think of it as *being* the simple pendulum, and we can say that it "fits the description" of the simple pendulum found in our textbooks, albeit in a somewhat loose or indirect sense of the phrase. Furthermore, the utterances we produce when we engage in the face value practice in relation to the description of the simple pendulum can simply be taken to be claims about the property-containing object corresponding to it. This includes face-value-practice utterances made in philosophical contexts. We can count the property-containing object as a model, and we can propose that it be thought of as a component of a theory. We can also put forward an account of scientific representation on which we represent real pendula by focussing on relations of similarity, in a slightly extended sense, between the property-containing object and the real pendula: property-containing object X and concrete object Y are similar in this extended sense when X contains a property which Y has, or when X contains a determinate of some determinable of which Y has another, nearby determinate. We would thus seem to have an object which can play all the various theoretical roles Giere has in mind for the objects he seems to be positing when he engages in the face value practice. And perhaps this way of reading descriptions of missing systems will legitimate the employment of the face value practice in other philosophical cases, too.

It is worth emphasising that these successes come at the price of the two metaphysical commitments mentioned above: to the existence of properties, and to their abstractness.[20] The latter claim, that properties are abstract objects, can mean more than one thing, but note that we must be willing to accept that there are uninstantiated properties if we wish to adopt the property-containing view: no actual pendulum, lest we forget, instantiates the property of perfect rigidity. This precludes us from taking properties to be *in re* universals or, I take it, tropes—views which in any case would sit uneasily alongside the required insistence that the posited property-containing objects are absent from the spatiotemporal world around us—and nudges us in the direction of a full-fledged Platonism. And there is no obvious and uncontroversial reason for thinking that those who rely on the face value practice are antecedently committed even to the existence of properties, let alone to Platonism about them; rather, these are costs incurred by adopting the specific understanding of that practice implied by the property-containing view.

So one question is whether we are happy to embrace these commitments. But even if we are, there is a further objection to the proposal at hand: unless we are given some independent reason to believe in the property-containing objects which lie at the heart of the proposal (in addition to believing that there are properties, and that they are Platonic abstracta), it will seem at least somewhat *ad hoc* to posit them in order to acquire a workable account of descriptions of missing systems and the face value practice, and to underwrite the philosophical accounts which rely on that practice. There are,

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[20] Teller says that the nominalist—the broad nominalist who denies the existence of abstract objects—will not have the problem to which I drew attention in the first place, namely, the problem of making sense of the idea that an abstract object can stand in the sort of relations of similarity to a concrete object which Giere's proposed account of representation requires (2001, p. 399). This is true, of course; the problem is that saying so does not amount to providing a nominalist account of talk about models, or of scientific representation. See Thomson-Jones (2007), section 4, for a more general discussion of this sort of issue.

admittedly, at least two candidates for the job: the view that given any F's, there is a set containing all and only those F's, and the view that given any F's, there exists a thing which is the mereological sum of the F's (where each F is just some entity or other). When combined with the assumption that there are abstract properties, these doctrines give us property-containing objects as sets of such properties and as mereological fusions of them, respectively. The difficulty, of course, is that both doctrines are controversial; either would be an additional and non-trivial commitment.

The metaphysical theses presupposed by the property-containing view cannot be ruled out of court, and nor can either of those we might draw on in an attempt to rebut the charge that the view is *ad hoc*; furthermore, there would be nothing obviously incoherent about combining those various theses. I am thus not claiming that we have here any decisive objection to the property-containing view. I do, however, take the commitments in question to be weighty enough to propel us onwards, in the hope that we might discover a way of interpreting descriptions of missing systems which is available to those who are not willing to acquire those particular pieces of baggage.[21]

3.3 *Indirect descriptions of abstract objects: mathematical structures*

With the philosophy of science of the last thirty to forty years as a backdrop, one way of interpreting the textbook description of the simple pendulum which suggests itself fairly immediately is that such a passage picks out a mathematical structure—a phase space with a set of trajectories defined on it, say. Taking this idea as a starting point, we can go on to see the mathematical structure in question as a description-fitting object, albeit one which "fits the description" not by having the properties explicitly mentioned in the description, but indirectly, by having various corresponding mathematical properties. Instead of having the property of moving sinusoidally, say, the mathematical structure

[21] I count myself amongst that number: that there are properties is the only one of the relevant theses I am happy to accept.

which we are to think of as indirectly fitting the description of the simple pendulum, and which we can identify as being the simple pendulum, if we like, has some mathematical property which corresponds to the property of perfect rigidity, a property which (let us suppose) is the sort of property that an abstract object can have without any trouble. The requisite correspondence between the physical properties denoted (or usually denoted) by the predicates appearing in the description of the simple pendulum and various mathematical properties might be set up by prior stipulation, or by some other means; there will be more to say on that score soon.

Note how this interpretive approach—call it *the mathematical structures view*—is related to the others we have considered: We began with the idea that there is an object which fits the textbook description of the simple pendulum in the most straightforward way possible, namely, by instantiating the properties mentioned in the description. That approach proved unworkable, and so we went on to consider the option of substituting a relation other than instantiation between the object and the properties. Now, instead, we are substituting other properties.

Like the first two approaches we considered, the view that descriptions of missing systems pick out mathematical structures would seem to make simple sense of the face value practice. Provided we are willing to understand description-fitting in a somewhat roundabout way (a willingness we also had to display on the second, property-containing view), we can again say that there are objects fitting the descriptions given, and so face-value-practice utterances can be interpreted as claims about those objects. Taken the right way, moreover—that is, understood as claims about the mathematical properties of various mathematical structures—many of the claims we make by producing face-value-practice utterances will be true on this view. (Not all, of course, if only because people make mistakes.)

Can the mathematical structures approach to understanding descriptions of missing systems and the face value practice support the weight of Giere's accounts of the nature of models, theory structure, and scientific representation—or at least, the accounts we get when reinterpret Giere's original proposals in the terms of that approach? In some large measure, yes. The mere fact that there are description-fitting objects corresponding to descriptions of missing systems on this view leaves us room to say that such passages describe models, and that those models are components of theories (and that there are both models and theories). The resulting picture is, furthermore, one on which models and, in part, theories, are non-linguistic entities, which is clearly a central point for Giere. And finally, if there is a correspondence between various mathematical properties of the mathematical structure picked out by the description of the simple pendulum and the properties mentioned in that description, then the mathematical structure can be used to play just the sort of representational role Giere wants the simple pendulum to play. If we employ theoretical hypotheses of the kind Giere invokes, for example, we might use the mathematical structure in question to represent the real pendulum on my desk as having a certain period of motion by saying something of the form "This real pendulum is very similar to a simple pendulum of such-and-such a specific sort with respect to its period of motion," meaning just that the determinate of the period-of-motion determinable which the real pendulum instantiates is close to the determinate of period-of-motion which corresponds to one of the mathematical properties possessed by the mathematical structure.[22]

Unfortunately, however, the picture of representation we are left with has lost a good part of the appealing simplicity Giere's account seemed to have at first sight. We predicted that this would be a consequence of rejecting the straightforward reading of descriptions of missing systems, but it is worth noting the extent of the loss in this case. Rather than good old ordinary similarity, understood as property-sharing or as the

[22] If we say, as we might, that the mathematical properties possessed by the mathematical structure *denote* the physical properties to which they have been made to correspond, then we have here the "D" part of Hughes's (1997) "DDI" account, applied to mathematical structures.

possession of nearby determinates of the same determinable, the relation between the model and the modelled system which underlies representation is now one of "correspondence" between the mathematical properties of the model and the physical, chemical, biological, psychological, economic…properties of the modelled system. This means that sooner or later we will have to face the question of how such a correspondence is put in place; and this is just the kind of question Giere had (understandably) hoped to lay aside. It is, after all, the question the logical positivists were attempting to answer with their talk of correspondence rules, except that now the problem is to say how mathematical objects and properties, rather than words, might become linked to objects and properties in the domain of inquiry. There are other sorts of answer we might pursue, of course. Perhaps the relations of correspondence derive from a mapping which renders the mathematical structure isomorphic (in some derivative sense) to the modelled system, for example, or partially isomorphic, so that structural similarity of some sort is the crucial relation. But this answer is not unproblematic, either, as much recent (and not so recent) debate makes clear; and more to the point, we have no answer to the question about correspondence on which representation seems as straightforward as Giere's picture initially seemed to make it.

It is also worth noting, separately, that understanding Giere's accounts of models and representation in the terms of the mathematical structures approach yields a notion of model on which models are mathematical structures used to represent systems from the domain of inquiry. The picture of theory structure we arrive at by this route is thus the streamlined version of the Suppes-van Fraassen semantic view I describe in Thomson-Jones (2006), plus theoretical hypotheses.

So far, these are points about the results of reshaping Giere's accounts of models, theory structure, and scientific representation with the resources provided by the mathematical structures approach to interpreting descriptions of missing systems. But we should step back and ask the prior question of whether the mathematical structures view is viable in and of itself (that is, considered as an answer to questions (1) and (2) of section 1). Should we interpret descriptions of missing systems as indirect descriptions of mathematical structures, and understand the face value practice accordingly?

There is a simple reason for thinking that this interpretive picture is off-track: it does not travel well. So, for example, though the idea that descriptions of missing systems pick out mathematical structures might seem natural enough when we focus our attention on textbook descriptions of the simple pendulum, it is surely much less plausible when we consider, say, a presentation of the nuclear model of the cell. Typical passages presenting that model contain no equations, nor any other sort of mathematical talk; they proceed entirely means of diagrams and descriptive prose.[23] Similarly, passages setting out the billiard ball model of gases, or the nuclear model of the atom, understood as models which have transcended and outlived specific proposals about the details (of the dynamics, say), can also be largely or entirely qualitative. As a result, we would have to engage in some fairly unseemly contortions in order to apply the mathematical structures approach to such passages.[24] Yet these examples are not particularly atypical in their lack of any obvious mathematical nature, and what is more, many such passages clearly play important roles in their respective scientific disciplines. It is thus quite implausible that a workable general approach to understanding descriptions of missing systems, or even one which fits all the central cases, can be grounded in the suggestion that they are indirect descriptions of mathematical structures.

[23] See, for example, chapter 5 of Gould and Keeton (1996). I take the example from Downes (1992, p. 145), where it is used to make a related point in a somewhat different context.
[24] See Thomson-Jones (in preparation a) for further argument on this score.

5. CONCLUSION

We have explored three ways of reading descriptions of missing systems as descriptions of abstract objects, and the result is that we now have reason to be unhappy with that idea: the first approach is unworkable, the second comes at the price of some significant additional commitments, and the third has trouble accommodating a certain central kind of case. Taken in combination with the fact that, by definition, descriptions of missing systems do not describe concrete systems in the world around us, these conclusions can only make us nervous about the ideas that there are description-fitting objects at all, and that face-value-practice utterances can be taken as claims, often true, about a realm of such objects.

As a corollary, we have reason to look askance at answers to epistemological and methodological questions which rely on the face value practice in the way that Giere's accounts of models, theory structure, and scientific representation do. We should thus think carefully about whether to continue to engage in the face value practice when offering up accounts of such central topics. My own inclination is to think that we should not: we should learn to do without description-fitting entities corresponding to descriptions of missing systems, and so at least for the purposes of philosophical thinking on these topics, we should eschew the face value practice as misleading and obfuscatory at best.[25] There is room for further exploration here, and as I mentioned earlier, I conduct some of it in other places (Thomson-Jones (2007), (in preparation b)), focussing in particular on the idea that descriptions of missing systems might be little fictions. The results of those investigations so far, however, bolster the case against the idea that there is an attractive way of interpreting descriptions of missing systems which

[25] The contrast here is with scientific theorising, where perhaps the face value practice can be treated as a useful, and harmless, *façon de parler*—an important part of a certain kind of game of make-believe, say. I leave this question open here.

provides us with description-fitting objects capable of doing the theoretical work that accounts like Giere's require. There are other options yet to be explored, and we may get a surprise; but if in the end we do conclude that some approach which shuns description-fitting objects gets the interpretation of descriptions of missing systems right—perhaps some version of the little fictions approach—then we will have to learn to do without such objects in our philosophical work. The important task will then be to say how we should think about models, theory structure, representation and the rest once we take seriously the thought that missing systems are, indeed, not there.[26]

[26] As I mentioned in n. 19, parts of this paper are descended from an unpublished manuscript entitled "Models and Idealized Systems" which I first began circulating some time ago. More recently I presented the current incarnation at three conferences (under the old title, in the first two cases). Thanks to audiences at the Annual Philosophy of Science Conference in Dubrovnik and the Canadian Society for the History and Philosophy of Science meetings in spring 2006, and the workshop "Scientific Models: Semantics and Ontology" in Barcelona in summer 2007. I would also like to thank Jim Brown, Anjan Chakravartty, Gabriele Contessa, Greg Radick, Kyle Stanford, Kate Thomson-Jones, my departmental colleagues at Oberlin, and especially Nancy Cartwright, my commentator in Barcelona, and Adam Toon, for very helpful discussion and correspondence.

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