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MISSING SYSTEMS AND THE FACE VALUE PRACTICE

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1. THE QUESTIONS

Scientific textbooks, classroom lectures, and journal articles abound with passages which look just like attempts to provide accurate descriptions of actual, concrete systems from the domain of inquiry of the scientific discipline in question, but which are not intended as such, and not taken as such, by any competent practitioner of that discipline. At least typically, there indeed are no actual, concrete systems in "the world around us" (as we tend to say) 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. When all these conditions are satisfied, I will say that we are dealing with 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 attempt at 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.

Note that the issue here is not idealization. Some descriptions of missing systems are appropriately called "descriptions of idealized systems," but others are more

typically called "descriptions of imaginary systems" or "descriptions of hypothetical systems," and would not usually be regarded as descriptions of idealized cousins of any real system – consider, for example, Poincaré's famous description of a spherical, thermally-varying world (1952, pp. 64-68). Whether a given description of a missing system 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 to our present purposes.[1]

Surrounding descriptions of missing systems is a practice of talking and thinking as though there *are* systems that fit the descriptions given in such passages perfectly, and talking and thinking that way in the full knowledge that there are no actual, concrete systems in the domain of inquiry which do so. Because this practice 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*.

Here is a relatively familiar and uncomplicated example of a description of a missing system, borrowed from chapter 3 of Ronald Giere's *Explaining Science* (1988).[2] Textbooks in classical mechanics invariably contain a passage apparently describing something called the "simple pendulum," often accompanied by a schematic diagram. The simple pendulum 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 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.[3] It is a straightforward observation, however, and one which Giere himself emphasises, that there are no real systems fitting the standard textbook description of the simple pendulum (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.[4] And, of course, competent physicists 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.

It is an equally straightforward observation that 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. When we do these things, we are engaging in the face value practice.

I will employ this example, drawn from a particular branch of physics, repeatedly in what follows, but it is important to bear in mind that descriptions of missing systems play a central role throughout the sciences, both natural and social, and can be found just as easily in journal articles and book-length treatises as in textbooks. Accordingly, my arguments are intended to be quite general.

There are a number of questions which immediately come to mind when we reflect on these twin aspects of scientific and philosophical discourse. First there is a single, simple question about descriptions of missing systems; and then there is a cluster of questions centering on the face value practice, a discursive practice surrounding such descriptions. (1) How should we interpret descriptions of missing systems? That is, what sort of account should be given of their semantics and pragmatics? (2) How should we make sense of the face value practice, the practice of thinking and speaking as though there are objects which fit the descriptions given in descriptions of missing systems? Can the face value practice itself be taken at face value, for example? That is, even though there are no actual, concrete objects in the world around us which fit those descriptions, are there objects of some other sort which do, so that we can take the utterances we

produce when we engage in the face value practice to express claims about *those* objects? If so, what sorts of things are they, and in what sense do they fit the descriptions in question? If not, are we indulging a useful fiction when we engage in the face value practice? Or are we instead saying true things which, properly understood, do not commit us to the existence of a corresponding class of description-fitting objects? That is, is there, so to speak, a plausible nominalistic paraphrase of the sort of talk people engage in when immersed in the face value practice? If so, what is it?

The questions in (2) are clearly related to question (1), and it is worth spelling out some of the connections explicitly. Begin with a piece of terminology: when a view says that, corresponding to a given description of a missing system, there is an object which in some way or other fits the description (despite the fact that, by definition, there is no actual, concrete system in the world around us which does so), I will say that the view posits *description-fitting objects*. Now, if the answer to (1) involves a realm of description-fitting objects – perhaps, most straightforwardly, descriptions of missing systems express strings of truth-apt claims, and the truth conditions of those claims concern the features of the description-fitting objects – this will suggest taking a face value approach to the face value practice itself: it will be natural to see the utterances we produce when we engage in the face value practice as expressing claims about the description-fitting objects. In that case, furthermore, there may be no special obstacle to regarding both descriptions of missing systems and the utterances we produce when engaging in the face value practice as making *true* claims. If, on the other hand, the answer to question (1) does not involve a domain of description-fitting objects, we may be pushed towards giving a useful-fiction account of the face value practice, or a "nominalist" paraphrase of the utterances in question (nominalist just in the sense that it makes no reference to description-fitting objects); we may even come to take seriously the option of abnegating the face value practice altogether. This is not inevitable, however, for as we shall see in sections 3.5 and 3.6, it is possible to combine an answer to (1) which makes no use of

description-fitting objects with an account of the face value practice which relies on them nonetheless.

In section 3, I will proceed by considering a range of answers to question (1) – a number of answers, that is, to the question of how we should interpret descriptions of missing systems. On a coarse-grained way of counting, I will focus on four views altogether: that descriptions of missing systems are straightforward descriptions of abstract objects (3.1); that they are indirect descriptions of abstract objects (3.2, 3.3, and perhaps 3.5); that they are little fictions, semantically and pragmatically on a par with descriptive passages from works of ordinary fiction (3.4-3.6); and that they are implicitly counterfactual (3.6). There is more than one way of filling out each of the last three of these four ideas, however. Accordingly, I will consider variations on each of those three themes. And, as the discussion moves along, I will be exploiting the connections between question (1) and the questions in (2) to consider a range of interpretations of the face value practice.

The list of options I will consider is not logically exhaustive, of course; the hope is rather that it includes a number of the most natural, *prima facie* plausible, or otherwise promising candidates for ways of understanding descriptions of missing systems and the face value practice (though for various reasons the list also includes some options which arguably have none of those features). Certainly the list includes a number of views which have actually been advanced, along with one or two speculative developments of ideas which seem to be in the philosophical air. But there is one notable way in which the list of options I will discuss is skewed towards a certain kind of view: for the most part, I will be focussing on accounts which posit description-fitting objects at some point or other.

The reason for this bias is not that I will be presupposing that a view which posits corresponding objects in answering the questions in (1) and (2) is more likely to be right; it is that there is a third, more specific question I want to focus on in the present

discussion. There are many instances in which philosophers engage in the face value practice in an entirely central way when offering answers to epistemological and methodological questions about the sciences – I will present a number of examples drawn from the work of Giere, Peter Achinstein, and Frederick Suppe in section 2. The more specific question driving this paper is thus: (3) Is there a set of plausible answers to the questions in (1) and (2) which legitimates such uses of the face value practice by philosophers of science, and which can support the weight of their accounts of models, theory structure, representation, and so on?[5]

Interpretations of descriptions of missing systems and of the face value practice – that is, sets of answers to the questions in (1) and (2) – can be divided into those which posit description-fitting objects and those which do not. In section 3 I will provide reasons for thinking that that there is no very attractive interpretation in the first category. My argument will proceed by considering a range of options, and pinpointing the ways in which each of them is wanting. The first interpretation we will consider is internally inconsistent, and can thus be rejected outright; various others are inadequate to the job of underwriting the accounts of models, representation, theory structure, and so on which Giere, Achinstein, and Suppe have put forward; and the rest require us to take on various significant additional philosophical commitments, many of which are quite unappealing. Of course, there will be those who are happy to take on the commitments necessitated by one or another of the interpretations in the third category, and so I do not expect to persuade everyone. Furthermore, the list of interpretations I will consider is not exhaustive, and so it is always possible that some other interpretation which posits description-fitting objects can be used without great cost to underwrite the accounts Giere, Achinstein, and Suppe have put forward. Still, the problems we will run into with the interpretations we do examine should, I think, incline some of us to reject the idea that such philosophical accounts can rest

unproblematically on the assumption that there are in fact description-fitting objects corresponding to descriptions of missing systems.

This conclusion alone, however, obviously does not answer question (3). Might the weight of the philosophical accounts in question be supported by an interpretation of the face value practice, and of descriptions of missing systems, which does *not* posit a realm of description-fitting objects? The answer to this question, I think, is that it depends on the nature of the weight to be supported. Giere's and Achinstein's accounts, it seems to me, commit them to the existence of description-fitting objects which are capable of playing certain theoretical roles (being models, being components of theories, being employed for the purposes of representation, and so on), so that our failure, in the various parts of section 3, to find an interpretation which posits such objects counts against the ultimate viability of their accounts. And I will make the case that Giere and Achinstein are committed to description-fitting objects in the first place simply by considering, at various points, the difficulties they would face if they attempted to do without them. This will involve canvassing (especially in sections 3.4 and 3.6) at least two possible interpretations which do not posit description-fitting objects. Interestingly, however, in section 3.6 we will see that Suppe might be able to legitimate his employment of the face value practice by embracing the second of these more ontologically austere options. It follows that use of the face value practice in the philosophy of science may be not be problematic in every case – although we will also note that interpreting Suppe's employment of the face value practice in the relevant way might alter our understanding of the epistemological and methodological stories he is telling.

Overall, then, although I will not be attempting to settle on final answers to the questions in (1) and (2) in the present discussion, I will offer an answer to (3), and we will make some headway towards answering the two more general questions in the process.

No doubt there will be some who would wish to cut this inquiry off at the pass by arguing that philosophers of science can leave such questions to the philosophers of language and the metaphysicians, safe in the assumption that any adequate account of our talk about missing systems will legitimate the face value practice. I will reply to this line of thought in section 4; by then we will be in a better position to appreciate why such a strategy of deferral will not do. For now I must simply ask for patience on the part of the reader who is inclined to adopt it – or, failing that, recommend some skipping ahead. 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.

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 philosophers noticed 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 been concerned with is:

What are models?

or, more carefully,

What kinds of things are scientists talking about when they talk about models?[6]

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 section 2.2), 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)[7]

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.

Similarly, Peter Achinstein, in a relatively early attempt to give a taxonomy of models in his 1968 book *Concepts of Science: A Philosophical Analysis*, engages in the face value practice in two separate places. First, he introduces the category of "representational models." In Achinstein's terminology, a "representational model" is "a three-dimensional physical representation of an object which is such that by examining it, one can ascertain facts about the objects it represents" (1968, p. 209). Achinstein then divides representational models into two categories: those which represent by reproducing characteristics of the modelled system, albeit typically scaled by some factor, and those which represent in virtue of a structural analogy, without the reproduction of characteristics.[8] An example of the first sort of representational model is an engineer's scale model of a bridge, in which the length, height, and width of the modelled system are represented by the length, height, and width of the model; an example of the second sort is an electrical circuit used to model the behaviour of the air

in the neck of a flask when a sound wave impinges upon it, in which the force on the air due to the sound wave is represented by the varying electromotive force in the circuit, and the displacement of the air is represented by the charge on a capacitor in the circuit.[9] Achinstein calls representational models of the second variety "analogue models" (*ibid.*, pp. 210-211), and towards the end of his initial discussion of representational models he writes:

> An analogue model need not be built but only described. (*The model in such a case is still the three-dimensional object or system described, not the description of it.*) Maxwell's description of tubes containing incompressible fluid is a description of an analogue model of the electric field. He "constructed" his model simply by describing these tubes, not by building them. (*ibid.*, p. 211, emphasis added)

In making room in his taxonomy for unconstructed analogue models, Achinstein is behaving as though there are such things – as though, for example, there is a system of "tubes containing incompressible fluid" which fits Maxwell's description, and stands in various relations of structural similarity to the electric field. He is thus engaging in the face value practice.

Achinstein's taxonomy also contains the category of "imaginary models" (*ibid.*, pp. 218-221). As the label suggests, one of the main features which characterizes imaginary models is that a description given in specifying a model in this category is not regarded as giving an accurate or plausible description of any actual system, or even a reasonable approximation to the truth about any actual system (*ibid.*, p. 220). As an example, Achinstein refers us to Poincaré's description of a spherical 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; once the details are fixed in the right way, the geometry of such a world will seem to its inhabitants to be non-Euclidean (*ibid.*, pp. 218-219, and n.

18). Poincaré clearly did not intend to be providing an accurate characterisation of the actual universe, or any part of it, in describing such a world.[10]

In such a case, Achinstein insists, "[t]he word 'model' may refer either to the object described or to the assumptions made about it" (*ibid.*, p. 220). The difficulty here, of course, is just that it would seem that there *is* no actual object which fits the description in such cases: Poincaré's description of a thermally-varying spherical world is a paradigm case of a description of a missing system. It is thus natural to conclude that there is nothing other than the description itself to which we can affix, or refrain from affixing, the label 'model.' So in speaking as though as though there is such a thing as the world described, a thing which counts as a certain sort of model and must be covered by one's taxonomy of models, Achinstein is again engaging in the face value practice.[11]

So much for the first example of a question which has led some philosophers of science to speak as though there are objects which fit various descriptions of missing systems. Before moving on, however, I should reveal a related agenda of my own which lurks in the background here.

As the discussion so far reminds us, philosophers of science have offered more than one answer to the question "What kinds of things are scientists talking about when they talk about models?". They have also employed the term 'model' for independent reasons, both as a term drawn from mathematical logic and as something more closely approaching a philosophical term of art.[12] The result is that the word 'model' is used in a good number of significantly different ways by different authors, and all too often without clear recognition of that fact. The waters have become sufficiently muddy that we find ourselves needing to ask:

What kinds of things are philosophers of science talking about when we talk about models?

This latter question becomes important, for example, if we want to understand and properly appraise the semantic view of theory structure.[13]

In another paper I lay out my own taxonomy of models, one which is intended to encompass the most common uses of the term 'model' both in the sciences and in the philosophy of science (which is not to say that patterns of usage are the same in those two areas, or that they are the same in the various sciences, or even across the branches of a given science).[14] In brief, the taxonomy is as follows:

1) Truth-making map: A model as a mapping from parts of a language which provides an interpretation for, and makes true, some given set of sentences in that language.
2) Truth-making structure: A model as a (generally) nonlinguistic structure which provides an interpretation for, and makes true, some set of sentences.
3) Mathematical model: A model as a mathematical structure used to represent a (type of) system under study.
4) Propositional model: A model as a set of propositions, the members of which together form a representation of a (type of) system under study.
5) Physical model: A model as an actual physical object or process used to represent a (type of) system under study.

The idea is not that the five *notions* of model involved in setting out the taxonomy are the most commonly used notions of model in the areas in question; it is rather that, on the most common uses, the intended *referents* of the term 'model' fall into one of the five categories making up the taxonomy. In using the term 'model' in the ways I have just described, however, Giere and Achinstein seem, on the face of it, to be picking out kinds of model which do not fit into this taxonomy. In section 3.1, I will argue that on a natural reading of what they say, neither Giere nor Achinstein is employing a coherent notion of

model in those places. We will also see in the subsequent discussion that on at least two alternative readings, Giere and Achinstein would be picking out objects which fall into one of the categories of my taxonomy after all.[15]

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,[16] 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 the face value practice in offering up her view of theory structure. This is just the situation with regard to Giere's view of theory structure. 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 how Giere's notion of model entangles us in the face value practice.

There is another issue here, too. In saying this, 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,[17] an impression which van Fraassen reinforces (1987, p. 109, and 1989, p. 222),[18] and it has become common practice to cite Giere's writings alongside those of Suppes and van Fraassen without further comment when discussing the semantic view. But Suppes and van Fraassen, properly understood, are employing the notion of a mathematical model,[19] 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 purposes 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. I will argue that there is not – although, as we will see in sections 3.3 and 3.6, 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.[20]

2.3 *Representation*

Here is a third question, on a topic which has been gaining considerable momentum quite 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.[21]

We have already seen that Achinstein claims that unconstructed analogue models can be used to represent actual systems from the domain of inquiry – that Maxwell's system of tubes containing an incompressible fluid, for example, can be used to represent the actual structure of the electric field. And 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.[22] This is a widely discussed view of how at least some scientific representation proceeds, and it seems to be attractive to many.[23] 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.

2.4 *Idealization*

A final example:

How should we understand the nature and functions of idealization in science?

Although Giere does not tend to frame his views as providing an answer to this question, we can see that there is at least a partial answer to it implicit in what he says: descriptions of idealized systems (which are descriptions of missing systems, and which are obviously one kind of place in which idealization occurs) serve to pick out corresponding systems – the idealized systems themselves, such as the simple pendulum – which are then used to represent the features and behaviour of real systems via relations of similarity to them. And part of the picture seems to be that by proceeding in this way, rather than by representing real systems more directly, to so speak, we gain something by way of unification – we have a vivid way of drawing out various similarities between the real systems which surround us, and, accordingly, a way of grouping them together.[24]

Frederick Suppe tells a strikingly similar story in the definitive statement of his version of the semantic view of theory structure, *The Semantic Conception of Theories and*

Scientific Realism (1989). The notion of a "physical system" occupies a central role in his story; indeed, the first relatively full sketch of his account begins with this notion (*ibid*., p. 65). An initial characterization of the notion of a physical system proceeds by example:

> [C]lassical particle mechanics is concerned with the behavior of isolated systems of extensionless point-masses which interact in a vacuum, where the behavior of these point-masses depends only on their positions and momenta at a given time. A physical system for classical particle mechanics consists of such a system of point-masses undergoing a particular behavior over time. (*ibid*., p. 65)

As Suppe is quick to emphasize, however, the spatio-temporal world around us contains no such systems; and the same is true, he argues, of the physical systems of thermodynamics, quantum mechanics, the "valence theory of chemical reactions" (which concerns interactions between perfectly pure chemical substances), and the "genetic theory of natural selection," amongst others (*ibid*., pp. 65-66).[25] Suppe does not classify the things he calls physical systems as models, and neither does he seem to regard them as components of theories; nonetheless, they play a central role in his account of the methodology of the sciences, a role which is crucially connected to the function of idealization. "In general," he writes, "a scientific theory has the task of describing, predicting, and (possibly) explaining a class of phenomena," and on Suppe's account it does this by characterizing the behaviour of "physical systems," which amounts to telling us how actual systems would behave under idealized circumstances. So idealizations are built into the characterization of physical systems; and when these are "coupled with an appropriate experimental methodology," we are able to make predictions about and explain the behaviour of systems which "do not meet these idealized conditions" (*ibid*., p. 67). As in Giere's account, then, we represent the behaviour of real systems indirectly, via entities which fit the descriptions of idealized systems we find in textbooks and elsewhere and that is at least one way in which

idealization plays a role in our theoretical activity. (Suppe calls his physical systems "idealized replicas" of real systems (*ibid*., p. 65).) But again, the descriptions of idealized systems in question are descriptions of missing systems, and so Suppe, in offering this kind of account of the function of idealization – and of representation, prediction, and explanation – is engaging in the face value practice.

3. INTERPRETING THE PRACTICE

So far we have seen some central examples of philosophers 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 several 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, the processes of model and theory construction, and realism have all been addressed in ways in which make use of the face 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, both Giere (1988) and Suppe (1989) 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."

Recall now that the specific questions I want to focus on are these: 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? Can we, for example, find some way to make sense of the idea that, corresponding to descriptions of missing systems, there are after all description-fitting objects of some kind, and make sense of that idea in a way that can underwrite the philosophical accounts which seem to rely on it? (The thought behind this second question is in part that although finding description-fitting objects may not be the only way of attempting to legitimate the employment of the face value practice in developing our accounts of modelling, theory structure, representation, and the rest, it would clearly be the most straightforward. I will also argue that it is the only way that would succeed in legitimating Giere's and Achinstein's employment of the face value practice.) I will approach these questions simply by considering, in turn, a number of ways of interpreting descriptions of missing systems.

The questions I am focussing on are partly semantical and partly ontological, but 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. Note also that although I am inclined towards certain ways of interpreting descriptions of missing systems, and the face value practice, a lot of what I say will be independent of the correctness of those inclinations. The argument of the rest of the paper can be thought of as a "multi-lemma," designed to show that some views about models, theory structure, representation, idealization, and so on, are in trouble of one sort or another on a significant number of plausible (and implausible) understandings of the practice on which they rely. Some views may be able avoid most of the trouble: as we will see in section 3.6, we can interpret Suppe in such a way that his employment of the face value practice is relatively innocuous (although not without a certain shift in our understanding of the account he is offering). But our exploration of the difficulties which Giere, Achinstein, and Suppe all face in varying degrees will serve a dual purpose. First, it will lead to critiques of their particular views, critiques which should be of interest in their own right – especially, given the shape of current debates, that of Giere's views.

Secondly, it will serve as a cautionary tale about the employment of the face value practice, and give us a better sense of certain pitfalls we should aim to avoid in our attempts to develop an adequate account of models and modelling, of scientific representation, of idealization, and of theory structure, explanation, prediction, experimental methodology, and the rest.

One suggestion concerning the interpretation of descriptions of missing systems which I will consider in detail later is worth special mention here. 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.[26] 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 ask just one set of questions along these lines here: 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? Are descriptions of missing systems, in that sense, little fictions?[27] 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? At least the first two of these questions are relatively new. In sections 3.4-3.6 I will look at some ways of elaborating on the ideas they put forth, and draw some conclusions about whether taking such an approach to the passages in question will help to make the sorts of claims about models, representation, and idealization we have considered seem defensible.[28]

First, though, in sections 3.1-3.3, three other options. And I will explore all the options with primary reference to the example of passages we are inclined to call "descriptions of the simple pendulum." Such passages serve as good stand-ins for descriptions of missing systems more generally, I think, though we will keep an eye out for those points at which differences might make a difference.

3.1 *Straightforward descriptions of abstract objects*

The simplest approach is to take it that such a passage really does describe an entity which, of course, we can call the simple pendulum. That is, we might take it that, corresponding to a passage of the sort we have in mind, there is in fact an entity fitting the description given. And we might take it that the entity in question 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.[29] On this approach, then, 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. And the utterances making up a description of the simple pendulum are just true assertions about that entity. If this is right, the face value practice 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.

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 passages count as descriptions of missing systems. So how could this be a tenable way of reading them?

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.
>
> (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 simple pendulum actually has the properties denoted by the predicates appearing in the description).[30]

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 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).[31]

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 several variations on that theme in what follows.[32] But we should pause to note that replacing the straightforward reading with any such alternative will significantly reduce the initial appeal of Giere's account of representation. 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 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*).[33] 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.[34] Closely related, and only slightly less straightforward, is the sort of similarity which consists in the having of "nearby" determinates of the same determinable. The heightwise similarity of the two Buddhas on my bookshelf, for example, consists in their having different but nearby heights.[35] 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:
>
> > 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.

> Here the respects are "position" and "velocity," while the degree is claimed to be "very close." (*ibid*., p. 81)[36]

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

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

(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).[37] 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.[38]

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.[39] And 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 part mean non-spatiotemporal, then calling the object abstract does not address the problem.[40]

Finally, note that as the straightforward reading of the passages in question leads to internal inconsistencies, presumably neither Achinstein nor Suppe would want to elaborate their views by appealing to it. Nonetheless, Achinstein does talk just as though unconstructed analogue models actually have the properties mentioned in their descriptions: as analogue models they are supposed to represent other systems by *having* certain properties which correspond, as analogues, to the properties of the target system. Thus, as he offers us no special account of what he means by saying that such models have the properties in question, that part of Achinstein's account faces just the sorts of difficulties we have encountered here for Giere's view. If there is an object which actually has the properties mentioned in Maxwell's description of a system of tubes containing an incompressible fluid, that object has to be spatiotemporal. So where is it?

3.2 *Indirect descriptions of abstract objects: mathematical structures*

Given that we have ruled out the option of interpreting the descriptions of missing systems as straightforward descriptions of abstract objects, then, we might next consider the possibility that they offer us more *indirect* descriptions of abstract objects. One way of developing this idea is to suppose that although there is an object which in one sense fits the description of the simple pendulum (say), the object is a mathematical structure – a phase space with a trajectory defined on it, perhaps – and it fits the description not by having the properties denoted by the predicates appearing in the description, but by having various corresponding *mathematical* properties. The requisite correspondence, between various properties of mathematical objects and the physical properties denoted (or usually denoted) by the predicates appearing in the description of the simple pendulum, might be something we set up, either by prior stipulation or by some other means. Instead of having the property of moving sinusoidally, say, the mathematical structure 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) then has

a mathematical property which corresponds to the property of perfect rigidity, a property which is (let us suppose) the sort of property that an abstract object can have without any trouble. Once all this is in place, furthermore, the mathematical object in question can play the representational role Giere wants "the simple pendulum" to play via the very same correspondence relation. If we employ theoretical hypotheses of the kind Giere discusses, for example, then 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 "The 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 to which *corresponds to* one of the mathematical properties possessed by the appropriate mathematical structure.[41]

Now this view, that descriptions of missing systems pick out mathematical structures, would certainly seem to make sense of the face value practice – after all, on this picture there *are* entities which fit the descriptions given in the sorts of textbook passage we have in mind, provided only that description-fitting is understood in the right, somewhat roundabout way. And that there are such entities is important for those who, like Giere and Achinstein, want to say that we use entities corresponding to such passages to represent, and that the passages in question describe models, and for those who, like Giere, want to say that those models are components of theories. We should note three consequences of combining such claims with the view that the entities in question are mathematical structures, however, and one reason for doubting the general plausibility of that view.

First, note that if we take the entities which (in the indirect way we have characterised) fit the descriptions in question to be models, for then the resulting notion of model is simply that of a mathematical structure used to represent systems from the domain of inquiry.[42] In the terms of my taxonomy these entities are then just

mathematical models, and, as I argue elsewhere, this is the notion of model Suppes and van Fraassen had in mind when they presented their formulations of the semantic view – or at least, 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.[43] So there is no new notion of model here, and no need to make room for one in the taxonomy I presented in section 2.1.

Secondly, and as a corollary, Giere's version of the semantic view, though it may be novel in other respects (for example, in its explicit emphasis on the need for what Giere calls theoretical hypotheses), would on this approach not be novel in employing a new notion of model.

Third, 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 now that we are considering a particular alternative. Rather than good old ordinary similarity, understood as property-sharing or as the 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. But now we must ask: How are these relations of correspondence put in place? And this is just the kind of question Giere had hoped to lay aside. It is, after all, the question the logical positivists were attempting to answer with their theory 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 answers we might pursue, of course. Perhaps the relations of correspondence derive from some particular mapping which renders the mathematical structure isomorphic to the modelled system, or partially so, for example, so that *structural* similarity of some sort is the crucial relation. But this answer is not

unproblematic, either, as much recent debate in the philosophy of science 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.

In any case, it is far from clear that the idea that descriptions of missing systems might pick out mathematical structures yields a workable general picture of the semantics of such passages. Consider a passage characterising, in idealized terms, the nuclear model of the cell, say, or a passage characterising, in largely qualitative ways, the billiard ball model of gases.[44] In both cases we have a description of a missing system which proceeds entirely, or at least in large part, in non-mathematical terms. As a result, we would have to engage in some fairly unseemly contortions in order to see them as picking out some corresponding mathematical structures.[45] Yet such passages are not particularly atypical in being non-mathematical, and, what is more, they play important roles in their respective scientific disciplines.

In light of this problem, a variant on the approach being considered here suggests itself: Drop the requirement that the abstract entities picked out by descriptions of missing systems be mathematical structures, allowing instead that some of them might be abstract structures of other sorts. The idea would still be that the abstract entity which fits the description of a given missing system does so indirectly, by having properties which correspond in some way to the properties normally denoted by the predicates appearing in the description; but we would no longer require that the properties in question be mathematical.

The problem with this suggestion is simply that is too vague and incomplete as it stands to afford any genuine understanding. Perhaps it can be developed further in some useful way; in the meantime, however, we will go on to consider other options.

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

As we saw in section 3.1, there is trouble to come from supposing that there is an object corresponding to the textbook description of the simple pendulum which has the properties normally denoted by the predicates appearing in that description. We have just considered the option of avoiding such trouble by, so to speak, substituting other properties. An alternative, however, might be to hold on to the properties normally denoted by the predicates in question, but to propose that the abstract object corresponding to the description stands in some relation other than instantiation to those properties.[46]

Paul Teller recommends a resolution of just this latter sort in "Twilight of the Perfect Model Model" (2001).[47] At the point at which he makes his proposal, Teller is in part seeking to defend Giere's account against my argument for thinking that it is internally inconsistent as it stands.[48] Here is Teller's own presentation of the 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 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)

In other words, begin with the assumption that properties (including both the properties which actual concrete pendula have, and properties like perfect rigidity, which no actual concrete pendulum has) are themselves abstract objects. Then perhaps there is an object which corresponds to the description of the simple pendulum in the following way: the properties denoted by the predicates appearing in the description are properties which are *parts of the object in question*, but which the object does not *have*.

If there is such an object – call it a *property-containing object* – 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, again in a somewhat loose or indirect sense of the phrase "fits the description." 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 employ it as a representation of various real pendula, understanding such representation to rely on relations of similarity in a slightly extended sense: 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 which Giere, Achinstein, and Suppe have in mind for the objects they seem to be positing when they each engage in the face value practice.

I confess to not finding this proposal – call it the *property-containing view* – very appealing intuitively. Perhaps that is partly because it involves positing a kind of object, the property-containing kind, which we have seen no independent reason for believing in. In that respect, at least, the view seems somewhat *ad hoc*. This is in contrast with the proposal that we think of the simple pendulum as a mathematical structure, as there is a range of other reasons for believing in mathematical objects (which is not to say, of course, that we should believe in mathematical objects at the end of all reflection). On the other hand, perhaps it can be argued that positing a realm of property-containing objects provides us with the raw materials for accounts of modelling, representation, and theory structure which have greater flexibility and scope than we can achieve if our basic building blocks are mathematical structures, given that, as we have noted, not all descriptions of missing systems are mathematical in nature. However these considerations should balance out, we should take into account the fact that there are two substantial metaphysical commitments which are inescapable if we follow this route

– inescapable because logically prior to the ontological commitment to property-containing objects – and more than a threat of a third.

First, we must be willing to say that there are such things as properties: nominalism about properties is clearly precluded by the property-containing view as Teller formulates it.[49] If there are objects which contain properties as parts, then there must be properties. And note that there is no obvious reason for thinking that Giere, Achinstein, Suppe, and others who rely on the face value practice in doing their philosophical work are automatically committed to the existence of properties; rather, this is a cost imposed by adopting the specific understanding of that practice implied by the property-containing view.

Second, we must agree that properties are abstract objects. This can mean more than one thing, but note that we must be willing to accept that there exist uninstantiated properties – no actual pendulum, lest we forget, instantiates the property of perfect rigidity. This means that we cannot take properties to be *in re* universals (a view which in any case would sit uneasily, at best, alongside the assumption that the posited property-containing objects are nonspatiotemporal), and nudges us in the direction of a full-fledged Platonism.[50]

A third metaphysical commitment we might well need to take on to make the property-containing view tenable is a rejection of the view that particulars are property-bundles. A lot is going to depend on the details here, but if the notion of a bundle is such that a given bundle of properties contains the properties of which it is a bundle as parts (as is surely true on at least one natural way of elaborating the view), then given the thesis that particulars just are property-bundles – or more, carefully, given the thesis that particular X is just a bundle of all and only those properties which (as we would normally say) X has – it will turn out that a particular has property ϕ iff it contains ϕ as a part. It is thus natural (and usual, I think) to see it as part of bundle theory that, for particulars at least, having a property *just is* containing it as a part. And if that is so, then

it will clearly not work to say that the simple pendulum avoids concreteness, and avoids being "part of the world around us" by containing properties such as having non-zero length and moving in such-and-such a way as parts without having them.[51]

There may be escape routes available here. Perhaps, for example, one could accept, as part of one's bundle theory, the claim that a *concrete* particular has property ϕ iff it contains ϕ as a part but resist the *identification* of having a property with containing it as a part. One could then say that although a concrete pendulum is a bundle of properties which both contains the property of moving in such-and-such a way as a part and has that property, the simple pendulum is a different sort of whole which contains properties as parts without having them. But this is again to build in some nontrivial (and perhaps not entirely appealing) metaphysics to get the view to work.

Now none of the three metaphysical commitments I have just identified is untenable, and so I am not claiming that we have here any decisive objection to the property-containing view. I do take those commitments to be weighty enough, however, to propel us onwards in the search for other ways of interpreting descriptions of missing systems, in the hopes that we will discover a way of understanding that practice which costs us less, or (to put it another way) which is available to those who would not want to take on the commitments in question.

3.4 *Interlude: descriptions of missing systems as little fictions*

Let us now turn consider the two questions about fiction I mentioned earlier: 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?[52] Are they, in that sense, little fictions? And, relatedly, are the missing 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?

The immediate problem with turning to this suggestion for help in thinking about how to understand descriptions of missing systems – call it the *little fictions approach* – is that there are a number of different views about the semantics and pragmatics of ordinary fiction, and about the ontological status of fictional characters. I will not be able to consider all the contenders in this discussion, and my conclusions will need to be qualified accordingly; but we can at least make a start.

Let's begin with the former issue – the semantics and pragmatics of ordinary fiction. Call the utterances which make up a fiction *fictional utterances*. (An imperfect label, of course: fictional utterances in this sense are events as real as any.) Given that the sentences uttered in giving a description of a missing system are predominantly declarative, we will focus just on fictional utterances of sentences of that grammatical type. Then here are the central elements of three views about the semantics and pragmatics of such utterances.[53]

(i) In uttering a declarative sentence, we often perform the act of asserting the proposition expressed by the sentence.[54] In producing a declarative fictional utterance, however, the author of the fiction is (merely) pretending to perform such an act – she is *pretending to assert* the proposition expressed by the sentence used.[55]

(ii) Rather than pretending to perform the act of asserting various propositions in producing a string of declarative fictional utterances, the author is (actually) performing a different sort of act altogether: she is presenting a string of propositions with the intention that her audience make believe that they are true. Moreover, the propositions in question must be at most accidentally true (and will typically be false).[56]

(iii) A fictional utterance is one which contributes to the construction of an object which then functions as a prop in games of make-believe. There need not be an author – the "utterances" might be events of sentence-shaped cracks appearing in a rock – and thus, if there is one, her intentions play no defining role, and she need not be pretending to assert the propositions expressed by her sentences.[57]

Now there are some important differences between the accounts of fictional discourse just outlined,[58] but they have this in common: on none of the three accounts does the fact that someone has successfully produced a descriptive passage made up of a series of fictional utterances give us any reason to posit an object fitting the description given. That someone has successfully pretended to assert a series of propositions, or has successfully presented a series of propositions with the intention of our making believe those propositions to be true gives us, in itself, no reason to think that there is an object fitting the description which the propositions in question (or the sentences which were used to express them) make up; nor does the fact that that series of propositions is expressed by a string of sentences which are employed as a prop in games of make-believe. Indeed, it seems reasonable to hold that if an account of the semantics and pragmatics of fictional utterances *did* have the consequence that the appearance of a descriptive passage in a work of fiction by itself gives us some reason to believe that there is an object fitting that description, then that would give us at least some *prima facie* reason to reject the account. Accordingly, if our starting point is the thought that descriptions of missing systems might work in the same way as descriptive passages of ordinary fiction, but our elaboration of that thought begins and ends with the idea that the utterances made in producing a description of the simple pendulum, say, are semantically and pragmatically on a par with fictional utterances, then at least on the accounts of fictional utterance just outlined, we have no reason to think that there is an object fitting the description of the simple pendulum. Without objects of that sort, however, it is hard to see how the sorts of views of models, theory structure, representation, and idealization which we encountered in section 2 could be true.[59] So, for example, the representation of real pendula cannot rely on the holding of relations of similarity between the real pendula and an object which fits the description of the simple pendulum if there is no such object. Similarly, the simple pendulum model cannot be an object which fits the description given in such a passage if there isn't one,

and classical mechanics cannot be made up of such objects, even in part, if there are none. And if we have only the description itself, and no nonlinguistic object corresponding to it, we face the prospect of having to conclude that scientific representation is, after all, centrally a linguistic matter, that the simple pendulum model is a linguistic entity (such as a set of propositions), and so on.

In sum, then, the problem with the little fictions approach as developed thus far is that it does not provide us with the sorts of objects we seem to need to make sense of the face value practice, or to underwrite the sorts of philosophical accounts we have been examining. Call this the *shortfall problem* for the little fictions approach. We will clearly encounter the shortfall problem in trying to combine any philosophical account which relies heavily on the face value practice with the little fictions approach to descriptions of missing systems, if there is no more to that approach than we have outlined so far.[60]

Fortunately, however, there is more we can say by way of elaborating the little fictions approach, and by saying more we can hope to find a way of bringing description-fitting objects into the picture (one which accommodates the fact that there are no straightforwardly description-fitting objects in the world around us corresponding to the passages in question). The crucial thing to notice with regard to ordinary fiction is that, in addition to the fictional utterances which make up a telling of fiction F, there are *meta-fictional utterances*: utterances which are *about* F in one way or another. We can thus extend the little fictions approach to include the idea that it might it help to think of the discourse surrounding descriptions of missing systems in the sciences as semantically and pragmatically on a par with the discourse surrounding descriptive passages in the sorts of things we ordinarily class as works of fiction. Accordingly, in sections 3.5 and 3.6 I will consider some further strategies for making sense of the face value practice which are suggested by thinking about two different kinds of meta-fictional utterance.

A second worry one might have about the little fictions approach deserves mention at this point. Surely, one might object, it is *true* that the simple pendulum moves sinusoidally (or approximately so), and experiences no friction at the point of suspension, and moves through a uniform gravitational field; so then how could the textbook description of the simple pendulum be a little fiction?

Note that a clear conflict with the claim that it is true that the simple pendulum moves sinusoidally would be a more severe problem for the little fictions approach than the shortfall problem. There is certainly room to hope that the shortfall problem might be addressed by further elaboration of the little fictions approach, and even if not, it might still be that the little fictions approach is on the right track, and that we should desist from engaging in the face value practice, and reject philosophical accounts which rely on it too heavily. At least at first sight, it would seem much more difficult to convince ourselves that we should reject the idea that it is true that the simple pendulum moves sinusoidally in order to hold on to the little fictions approach.

This objection is not very clean as it stands, however, because the accounts of fictional utterances described above, as characterised, do not rule out the possibility that a proposition expressed by a fictional utterance might be true – indeed, at least one of them (the second) explicitly allows for the truth of such propositions. On those accounts of fictional utterances, then, there is no contradiction in claiming both that an utterance of the sentence 'The simple pendulum moves sinusoidally,' appearing as part of a description of a missing system, is a fictional utterance, and that the proposition it expresses is true. Nonetheless, there is some tension to be resolved here. Perhaps we can sharpen the difficulty this way: Surely we come to *know* that the simple pendulum moves sinusoidally by studying our textbooks. If the textbook description of the simple pendulum is made up of fictional utterances, however, then even if some of those utterances happen to express true propositions, we would not be able to come to know them simply by studying the description – there is not the right sort of link between the

fact that the sentences appear in the textbook and the fact that they express true propositions for us to gain knowledge in that way.[61] Call this the *truth and knowledge problem* for the little fictions approach.

There are no compelling grounds for rejecting the core idea of the little fictions approach here, however, as there is no clear disanalogy in this respect between descriptions of missing systems and descriptive passages in works of ordinary fiction: we are also inclined to say that we come to know that Emma Bovary was unhappy in her marriage by reading *Madame Bovary* (and, accordingly, that it is true that Emma Bovary was unhappy in her marriage).[62] If the accounts of fictional utterance outlined above are incompatible with such claims, then that might simply be reason to reject those accounts, rather than the core idea of the little fictions approach. But it is not clear that there is such an incompatibility in any case: the claims about truth and knowledge we are inclined to make can plausibly be accommodated, whether we are considering ordinary fiction or descriptions of missing systems, by insisting that the propositions which are true, and which we come to know by studying the texts, are certain distinct propositions expressed by meta-fictional utterances (utterances which can, unfortunately, share their syntax with fictional utterances). For those who are not familiar with the philosophical literature on fiction, this will become clearer over the course of the next two sections.[63]

3.5 *Meta-fictional utterances and property-holding*

In "Fiction and Metaphysics," Peter van Inwagen argues that we are ontologically committed to the existence of fictional characters by the variety of meta-fictional discourse he calls "critical" discourse, in which we treat fictions as "things that can be broken down and taken apart *in intellectu*," explicitly employing such notions as character, action, and plot (1983, p. 72). So, although on van Inwagen's view Flaubert did not commit himself to the existence of Emma Bovary by writing *Madame Bovary*, we do

so commit ourselves when we say "Emma Bovary is one of the most finely drawn female characters in European literature" (*ibid*., pp. 72-3). (In saying such a thing one is making a claim which is at least in part *about* the fiction *Madame Bovary* – that it contains one of the most finely drawn female characters in European literature. The claim in question can thus be classified as a meta-fictional claim. Indeed, we might say that the claim concerns what is true *of* the fiction, rather than what is true *in* it; we will turn to consider claims of the latter sort in the next section). Perhaps, then, if the description of the simple pendulum is a little fiction, we should likewise say that in telling the story of the simple pendulum we do not commit ourselves to the existence of such a thing as the simple pendulum, but that we so commit ourselves when we say things like "The simple pendulum is one of the most elegant models in classical mechanics."

This extension of the little fictions approach gets us an entity corresponding to a description of the simple pendulum, but of course it presents us with a familiar problem at the same time: What kind of a thing *is* the simple pendulum? And if there is such a thing, is it not true to say that it has a non-zero length, moves in certain ways, and so on? Then why do we not find it in the world around us?

Here is van Inwagen's solution to the parallel problem for an ontology of fictional characters. (Mrs. Gamp is a character in Dickens's *Martin Chuzzlewit*, one who is said, in the novel, to be fond of gin.)

> "But, look. There must be some sense in which it is true to say that Mrs. Gamp was fond of gin. If you say that it is false that she was fond of gin – presumably because theoretical entities of criticism cannot be said to drink at all – how will you distinguish between the sense in which it is false that she was fond of gin and the sense in which it is false that she was a teetotaler? If it is false that she was fond of gin, there must be a sense in which it is even more false that she was a teetotaler."
>
> This point is right, of course. I am afraid I shall meet it simply by stipulation. I shall simply introduce the word "hold" as a term of art and say that, while Mrs. Gamp does not *have* the property of being fond of gin, she does *hold* it. Being a teetotaler, on the other hand, is a property she neither has *nor* holds. (*ibid*., p. 75)

So perhaps this is how we should understand the description of the simple pendulum: It is a little fiction, and the critical discourse surrounding it commits us to the existence of an object corresponding to it. Moreover, even though the object in question does not have the properties denoted by the predicates appearing in the description, it fits that description in the sense that it "holds" the relevant properties.

This approach – call it the *property-holding view* – does provide a solution of some sort to the familiar problem in question. First, it follows that it indeed is true to say that the simple pendulum has a non-zero length, and moves in certain ways, provided only that in saying such things we manage to make claims about the properties the simple pendulum holds, and not those it has (despite our use of the verb 'has' – see *ibid*., pp. 75-76, for van Inwagen's remarks about what he takes to be the dual use of 'is'). Second, given that holding does not entail having, there is no conflict between saying that the simple pendulum holds the properties of moving sinusoidally and of non-zero length, on the one hand, and recognising that it is not to be found in the spatio-temporal world around us – the only objects which are a part of the spatio-temporal world are those which *have* spatio-temporal properties.[64]

It is equally clear that this property-holding view contains a solution to the truth and knowledge problem for the little fictions approach: Once we have granted that the sentence 'The simple pendulum moves sinusoidally', when uttered outside the context of the initial description of the simple pendulum, should typically be understood to express the claim that the simple pendulum *holds* the property of moving sinusoidally, there is no additional strain in maintaining that it can say something which we come to know by studying our textbooks (and, of course, something true), despite the assumption that the description of the simple pendulum is itself a little fiction.

Finally, the property-holding view contains a solution to the shortfall problem: corresponding to a description of a missing system, on this view, there exists an object which, we can say, fits the description. Admittedly, the object "fits the description" in a

somewhat extended sense, by holding the properties picked out by the predicates appearing in the description, rather than by having them; but that sort of description-fitting may be all that is needed to underwrite the sorts of philosophical accounts we are considering. So, for example, when Giere says that the simple pendulum represents a particular real pendulum by standing in certain relations of similarity to it, we can take standing in a relation of similarity in this sort of case to be a matter of the representing object's holding a property which the represented object has (or one object holding a determinate of a determinable of which the other object has a nearby determinate), and nod our heads.[65] And, accordingly, when philosophers engage in the face value practice, we can take them to be acting entirely appropriately: there are indeed objects which fit the descriptions given in the passages in question, albeit in a somewhat special way, and people who engage in the face value practice are simply talking and thinking about those objects.[66]

There is an analogy to both the mathematical structures approach and the property-containing view here, in that all three ways of thinking about descriptions of missing systems and the face value practice employ the strategy of understanding the relation of description-fitting unstraightforwardly. The comparison with the property-containing view is particularly close: in both cases, we propose that although the object corresponding to a description of a missing system stands in some direct relation to the properties denoted by the predicates appearing in the description, we take that relation to be something other than instantiation, and we do so precisely because we think that nothing *has* all those properties. (If something did have those properties, if would have to be part of the world around us, given the sorts of properties which are involved, but there is nothing in the world around us which has those properties, and so nothing at all has them.) The difference is just that the property-containing view substitutes the part-whole relation for instantiation, whereas the property-holding view introduces a new and *sui generis* relationship.

The property-holding view is not easy to evaluate. The difficulty in evaluating it is a consequence of the fact that it is hard to dispel the sense that there is something mysterious about talk of an object's "holding" a property. We know what holding is it not – it is not instantiation – and we know that it supposed to be the relation which fictional characters, and now missing systems, stand in to the properties mentioned in the passages we call descriptions of them; but we know little else. So, for example, it is hard to know whether the property-holding view commits us to the existence of properties – can talk of property-holding successfully be given a nominalistic paraphrase? There are a few things we can say, however.

First, *if* the property-holding view is committed to the existence of properties then, like the property-containing view, this will probably be a new ontological commitment, one not obviously entailed by anything else in the philosophical views we are considering, such as those belonging to Giere and Achinstein. Second, as properties are held by nonphysical things, and as some of the properties held by some missing systems (such as perfect rigidity) are uninstantiated properties, then if the property-holding view is committed to properties, it seems to be committed to something a lot like Platonism. If it is committed to properties at all, then, the view seems to be on equal footing with the property-holding view in those two respects.

The property-holding view does seem clearly to avoid one of the commitments the property-containing view faces, on the other hand: there is no tension with bundle theory. Assuming that for an object to hold a property does not involve its having the property as a part, we will face no special difficulty in distinguishing missing systems as an ontological category from concrete particulars understood as bundles of properties. But it is hard to regard this is as a great achievement of the property-holding view, deriving as it does from the sheer nebulousness of our understanding of the holding relation. And there are at least two clear advantages of the property-holding view: we know quite a bit about the part-whole relation, and we have a good deal of independent

reason for positing it in the first place, or for employing the concept of such a relation in our theorising. (Better, perhaps, to say that it is unclear how we could do without the concept of the part-whole relation.) By contrast, the holding relation is a mysterious creature, and positing it to solve a problem with a theory of the ontology of a certain kind of discourse surrounding fiction, and surrounding descriptions of missing systems, cannot but seem relatively *ad hoc*.[67]

There is room for further debate on this front, of course. Perhaps positing the holding relation is the only or the best solution to a problem created by positing fictional characters and missing systems, and perhaps our best overall theory of the world carries with it an ontological commitment to such objects. (Van Inwagen argues for a belief in the existence of fictional characters along just such Quinean lines (*ibid*., pp. 67-73), and the sort of reasoning he presents would extend quite naturally to the case of missing systems.) And one might try to argue that the mere fact that we have now identified two arenas in which the holding relation might do useful work – in making sense both of some of our discourse about fiction and some of our scientific and philosophical discourse – means that it is not *ad hoc* to appeal to it in either. Still, things are at best too unclear here for us to feel convinced that we have arrived at a satisfactory account of the workings of descriptions of missing systems, and of the face value practice. I will thus move on to consider one last approach, corresponding to a different kind of meta-fictional utterance.

3.6 *Meta-fictional utterances and counterfactuals*

Suppose I say to you now "Emma Bovary was a deeply dissatisfied woman." The sentence I utter in saying what I say might itself appear in the relevant work of fiction (or a translation of that work), but if it does, it is used there to make a fictional utterance – to tell the story. In uttering the sentence in question to you now, I am uttering it outside the fiction – not as part of the telling of the story, but after the fact, so to speak.

Yet I am not engaging in critical discourse in van Inwagen's sense, as I am not employing the notion of character, or narrative structure, or subtext, or any of the other "technical" notions we use to discuss the features of works of fiction.[68] We thus have here a distinct variety of extra-fictional discourse.

Our next observation is that in engaging in this sort of extra-fictional discourse by uttering the words 'Emma Bovary was a deeply dissatisfied woman' to you now, I would seem to be saying something true. There is then a puzzle about how that can be so, if we assume that the utterances making up the fiction are (at least by and large) not making true claims,[69] and if we want to leave open the possibility that there is some straightforward sense in which Emma Bovary does not exist. The standard solution to this puzzle is to propose that there is a tacit "fiction operator" present as a prefix to such extra-fictional utterances: what I am really saying when I utter the sentence in question outside the fiction (at least, in many cases) is that *according to the novel "Madame Bovary"*, or *in the fiction "Madame Bovary"*, Emma Bovary was a deeply dissatisfied woman.[70] And that is surely true.

(Note also that on this proposal when I utter the words 'Emma Bovary was a deeply dissatisfied woman' outside the fiction, I am making a claim *about* the fiction – namely, that according to it, Emma Bovary was a deeply dissatisfied woman. My utterance can thus be counted as not only extra-fictional, but meta-fictional, too.)

This suggests a new solution to the truth and knowledge problem for the little fictions approach, one which does not require us to posit property-holding objects – call it the *fiction operator approach* to extra-textual utterances of sentences such as 'The simple pendulum moves sinusoidally.' The idea is that although the description of the simple pendulum given in the textbook is a little fiction, and we can suppose that most or all of the utterances made in giving that description express propositions which are either false or truth-valueless, we can nonetheless say something true by uttering the sentence 'The simple pendulum moves sinusoidally' outside the fiction, because we can use that

sentence to express the proposition that *according to the simple pendulum story*, the simple pendulum moves sinusoidally. Moreover, this latter proposition is clearly something we can come to know to be true simply by studying our textbooks.

So far, so good; but we do not yet have a solution to the shortfall problem. That is, we have not yet seen a reason to posit a class of description-fitting entities corresponding to descriptions of missing systems, entities which can help us to make sense of the face value practice and underwrite the accounts of representation, models, theory structure, and idealization we identified in section 2. There is a way of solving that problem by extending the fiction operator approach, however, and we can move towards it by considering the following question: When is a given proposition, P, true in (or according to) a given fiction F? That is, when is a claim of the form "According to F, P" true?

A little reflection makes it clear that this question is not a trivial one. We cannot, for example, say simply that P is true in F iff some sentence which expresses P is employed in the telling of F: it is surely true that Emma Bovary had two nostrils, even though (if memory serves) Flaubert nowhere says as much.[71] Unsurprisingly, a number of theories of when P is true in F have been put forward, but we will look at only one: David Lewis's, as presented in the paper "Truth in Fiction" (1978). This account does have the advantage of being relatively well-known, but I focus on it here because it suggests a way of extending the version of the little fictions approach we have been developing in this section (that is, the version which incorporates the fiction operator approach) to yield a solution to the shortfall problem.[72]

By the conclusion of his paper Lewis has settled on a pair of alternative analyses, corresponding in his view to two distinct usages, but we need consider only one of them, as the points I wish to make carry over to the other quite straightforwardly. Here, then, is a concise statement of Lewis's "Analysis 1":

> 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.
>
> (1978, p. 42)

Here 'P is true in F' is analysed by the counterfactual 'If F had been told as known fact, then P would have been true.'[73] (It is important to bear in mind that the phrase 'told as known fact' is intended here in such a way that F's being told as known fact entails both F's being true and F's being known by the teller; the story-teller's telling F and regarding it as known fact is not enough.) Applying this analysis to the case of a standard extra-textual utterance of the sentence 'The simple pendulum moves sinusoidally', which we are now reading as making an "According to F…" claim, we get the result that I say something true in producing such an utterance if, and only if, if the simple pendulum story had been told as known fact, then it would have been true that the simple pendulum moved sinusoidally – that is, it would have been true that there was such a thing as the simple pendulum, and it would have moved sinusoidally. And at this point it might occur to us to take the simple pendulum to be a possible object: something which does not actually exist, but might have (and would have if the textbook description of the simple pendulum had been presented as known fact).[74]

Now if this talk of possible objects is paraphrased away, so that we do not regard it committing us to an ontology of possibilia, we will of course have said nothing to address the shortfall problem. But what if we do not paraphrase such talk away? What if, instead, we fill out our story so far by insisting that there are such things as possible objects? In that case, we could quite naturally take descriptions of missing systems to characterise such possible objects; and when philosophers engage in the face value practice, we could take them to be making claims about objects in that category. And the supposition that the simple pendulum, though an existing object, is a merely possible one would certainly explain the fact that we do not find an object fitting the description

of the simple pendulum in the world around us – the world around us is, after all, the actual world.

Making sense of such talk plunges us into the dark heart of the metaphysics of modality, of course. But here is a quick canvassing of three ways of developing the notion of a possible object (understanding the term in such a way as to exclude actual objects, so that we can drop the qualifier 'merely'):[75] (1) A possible object is a linguistic object, such as a consistent set of predicates which nothing actual jointly satisfies, but which something might have (Lewis 1986, pp. 148-149).[76] On this approach, the simple pendulum would be a set of predicates which includes such members as 'is subject to no frictional forces' and 'has non-zero length.' (2) Consider first the idea of an abstract object which represents some actual concrete thing, such as the Eiffel tower, by having all and only the properties the Eiffel tower has, and by having parts which correspond to the parts of the tower, such that a part of the abstract representation of the tower has all and only the properties of the part of the tower to which it corresponds, and two or more parts of the abstract thing stand in all and only the relations which the corresponding parts of the tower stand in to one another. (A part of the abstract object corresponds to a part of the concrete tower, I take it, precisely in that the two have all the same properties and stand in all the same relations.) Then take a possible object to be a thing of the same sort as the abstract object which represents the Eiffel tower, but one which does not represent any actual concrete object in that way. (Cf. *ibid*., pp. 165-167.) The simple pendulum would thus be an abstract object which has all and only the properties denoted by the predicates appearing in the textbook description. (3) Assume first that the actual world sits amongst a plenitude of worlds, many of them (at least) containing objects which are spatiotemporally related to one another, but each of which is spatiotemporally and causally unrelated to all the others. Suppose further that there is a one-to-one correspondence between such worlds and ways things might have been. That is, assume Lewisian modal realism. Then take a possible object to be an inhabitant

of any world other than ours. The simple pendulum would then be a concrete, spatiotemporal object which has a length, and is perfectly rigid, and moves through an unvarying gravitational field, and experiences no frictional forces, and so on, but which lives in a world spatiotemporally and causally unrelated to ours.[77] Will developing the notion of a possible object in any of these three ways provide us with the sorts of objects we need to solve the shortfall problem?

It clearly will not do for Giere's purposes to take the simple pendulum to be a set of predicates, or a linguistic object of any other sort. The simple pendulum and other objects like it are explicitly required to be nonlinguistic objects so that the resulting views of theories and of representation can hope to avoid a slew of familiar problems which philosophers of science have burdened themselves with in the past by taking a language-based approach. The same goes for Suppe and his physical systems; and it seems clear enough that Achinstein has something nonlinguistic in mind when he writes about unconstructed analogue models and imaginary models. So I will leave aside the idea of taking it that the simple pendulum and its ilk are consistent sets of predicates. The second option can be rejected straightforwardly, too, at this point in the discussion, for it delivers us back into the hands of Giere's proposal that we take the simple pendulum to be an abstract object having properties such as non-zero length and the property of moving sinusoidally, and as we have seen, that proposal cannot succeed as an account of the nature of *missing* systems.[78]

The remaining option is take the simple pendulum to be a concrete, spatiotemporal inhabitant of a world causally and spatiotemporally unrelated to ours, one which has, in just the ordinary way of having, all the properties denoted by the predicates employed in the standard textbook passages – that is, to take it to be the sort of thing which counts as (merely) possible object in Lewis's modal realism. This approach – call it the *concrete possibilia view* – is more promising in a number of

respects.[79] And interestingly, the idea is one 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)[80]

If the simple pendulum is a thing of that sort, then it fits the description given in the standard textbook passages in the most straightforward way there is; and if there are things of that sort, then there are objects corresponding (in that straightforward, ordinary description-fitting sort of way) to the passages which are at the focus of our attention, so that we might seem to be in a position to conclude that the face value practice, the practice of talking as though there are such objects, is unproblematic. Furthermore, such concrete possibilia would seem to have the features needed to underwrite the philosophical accounts outlined in section 2. For example, such concrete possibilia will stand in relations of similarity, in various respects and to various degrees, 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 required for Giere's theory of representation thus seem to be at our disposal on the concrete possibilia view. Paraphrasing Lewis, then, we might be tempted to claim that solving the shortfall problem for the little fictions approach is among the theoretical benefits to be found in the paradise of Lewisian possibilia.

Of course, entry into this paradise is not inexpensive. Giere, Achinstein, Suppe, and other philosophers of science who engage in the face value practice in the course of offering up accounts of representation, theory structure, the nature of models, and idealization, can adopt this approach to making sense of that practice only at the cost of

embracing the ontological commitments it requires – to a plenitude of worlds and their inhabitants. Alongside that fundamental ontological commitment sit certain associated commitments regarding such matters as reference and knowledge; most obviously, one cannot combine an unrestricted causal theory of knowledge with the approach in question and still make sense of the apparent fact that we know things about the simple pendulum.[81] Relatedly, note that by elaborating on the fiction operator approach to extra-textual utterances of sentences such as 'The simple pendulum moves sinusoidally' by adopting a Lewisian counterfactual analysis of claims prefixed by the fiction operator, and then interpreting counterfactuals as claims about concrete possibilia (and the worlds which contain them), a pair of moves designed to solve the shortfall problem, we have introduced certain difficulties for our solution to the truth and knowledge problem. The initial idea was that when a sentence such as 'The simple pendulum moves sinusoidally,' uttered extra-textually, makes a claim which we can come to know simply by studying our textbooks, and so makes a true claim, that is because it is being used to express the claim that according to the simple pendulum story, the simple pendulum moves sinusoidally; and it seemed, at that point, clear enough that we could come to know such a claim simply by studying our textbooks, and that we could manage to say something true in that way even though the description of the simple pendulum is a little fiction. Once the claim in question – that according to the simple pendulum story, the simple pendulum moves sinusoidally – is analysed as a counterfactual, however, and we have gone on to embrace a modal realist understanding of such counterfactuals, then to know the claim to be true is a matter of knowing something about causally and spatiotemporally unrelated to ours, and our managing to express the claim at all involves our being able to make claims about those worlds. So now we have to address some problems about knowledge and about language. The sorts of epistemological and semantical commitments which would seem to be required if we are to address those problems successfully are perhaps less

controversial than the primary commitment to an ontology of other worlds, but they are commitments nonetheless.

Perhaps the commitments mentioned so far are a fair price to pay, and perhaps they can be independently motivated – certainly Lewis argues at great length that they can in *On the Plurality of Worlds*, even though he has not succeeded in persuading many to take on the fundamental ontological commitment to other worlds of the sort he recommends. There are two problems which are specific to the project at hand, however, and which should cause us to hesitate before concluding that we have a defensible version of the little fictions approach which makes sense of the face value practice and can underwrite the philosophical views described in section 2.

First, there is a particular difficulty we will run into if we combine this approach with Giere's proposal that theories are collections of models, together with theoretical hypotheses relating the models to actual systems in the domain of inquiry (see section 2.2). It already sounds a little odd to think of a theory such as classical mechanics as an entity which is made up, at least in large part, of spatiotemporal objects, whether from this world or a collection of others, but an accusation of odd-soundingness has too little suasive force. A separate, and more acute problem, however is this: given the assumption that an object actually exists if and only if it is part of the actual world, the vast majority of the models which are the central constituents of, say, classical mechanics do not *actually* exist on this approach (even though they do exist). And if a large number of the constituents of classical mechanics – that is, all or almost all the models which go to make it up – do not actually exist, then the natural conclusion to draw would seem to be that the theory itself, classical mechanics, does not actually exist.[82] To put it another way, we seem forced into saying that there is no actual theory called "classical mechanics" – surely an unfortunate result for an account of structure of scientific theories.

Admittedly, there is a way around this problem. In fact, Lewis has already devised one, for certain parts of his metaphysics would at first sight seem to commit him to similar, and similarly unpalatable conclusions. According to Lewis, propositions are sets of possible worlds, properties are sets of actual and possible objects, and events are sets of actual and possible regions of spacetime; indeed, he regards it as one of the selling points for his ontology of worlds that it provides him with the entities he needs to advance these theories of propositions, properties, and events (1986, pp. 50-54, and 1986b, pp. 243-245). We might worry, however, that it is a consequence of these theories that very few propositions, properties, and events actually exist, or occur. Lewis notices this problem, and adopts the following solution to it:

> Suppose there are things that are not our world, and not parts of our world, and not sets built up entirely from things that are parts of our world – but that I might nevertheless wish to quantify over even when my quantification is otherwise restricted to this-worldly things. If so, no harm done if I sometimes call them 'actual' by courtesy. (1986, p. 95)

The idea is that when we use the word 'actually' to restrict our quantifiers, or use context to impose the same restrictions, we sometimes do so in a way which leaves lots of sets of possibilia within range, even though many of their members are excluded by the restriction. We do this simply because it is useful for us to do so: we want to be able to talk about the events, properties, and propositions which (on Lewis's account) are sets of just that sort even when our attention is focussed primarily on the world we inhabit.

No reason, then, that we could not incorporate the same manoeuvre – let me call it, tendentiously, the "actuality dodge" – into the view of theories we are now considering. On Giere's view, classical mechanics is a collection of models and theoretical hypotheses relating those models to systems in the domain of inquiry; on the elaboration of that view we are currently toying with, the models are spatiotemporal systems inhabiting (all or most of them) other worlds spatiotemporally and causally unconnected from ours, and so not actual; but classical mechanics is an actual theory

nonetheless simply because it is amongst the things we choose to quantify over when limiting our attention to those things we call "actual," even though many of its constituents are not.

The actuality dodge does solve the problem in question. The problem with it is simply that it seems strikingly *ad hoc* in the present context, in which it is introduced solely to save a view of the structure of scientific theories from having the consequence that there are actually no theories. It can perhaps be made to seem less *ad hoc* if in addition we adopt Lewis's views about events, propositions, and properties, as then it is solving a number of distinct problems at once; but of course that response to the problem of *ad hoc*-ness would require us to take on board a number of highly nontrivial philosophical commitments.

A second problem specific to the present context with taking descriptions of missing systems to be straightforwardly accurate descriptions of objects in other worlds arises from the observation that, arguably, some descriptions of missing systems are logically inconsistent. One candidate can be found in textbook treatments of systems undergoing synchrotron radiation in classical electrodynamics (see Frisch 2005, pp. 33 and 51-53).[83] If we take it that a statement of the law of conservation of energy is implicitly present as part of the standard description, then Frisch's discussion makes it clear that such a description is inconsistent. One could argue that there is an inconsistency even without the law of conservation of energy, however, in the following way: The standard textbook description applies the Lorentz force law in combination with Newton's second law to derive a circular trajectory for the particles, given that they are subject to an external magnetic field at right angles to their initial velocity. This derivation assumes that the external magnetic field is the only electromagnetic field acting on the particles. Next, given the circular trajectory, Maxwell's equations are used to derive the result that the particles will radiate energy (the "synchrotron radiation"). But from this it follows that the particles are a source of fields themselves, and that those

fields (the "self-fields" of the particles) will act on the particles. Thus it follows that the external magnetic field is not the only electromagnetic field acting on the particles, and we have a contradiction.[84]

In any case, if there are logically inconsistent descriptions of missing systems, then the only hope for understanding such passages in a way which straightforwardly parallels the proposed way of treating consistent passages would be to embrace the existence of logically impossible objects. If we are not willing to take that step, then on the assumption that there is as much reason to say that such inconsistent passages present models as consistent passages, and as much reason to think that we need a philosophical understanding of the representational roles of inconsistent passages, and of the nature and function of idealization in those cases, we would need to find some more complex way of holding on to the concrete possibilia view as providing a general account of descriptions of missing systems. One would be to treat inconsistent passages as picking out two or more other-worldly objects, one of which has and the other of which lacks a certain property, say. Whether this can be done in a sufficiently natural and convincing way would be a matter for further investigation, on a case-by-case basis; but note that even if it can, we would not be able to say that an inconsistent passage characterises a single model, if models are just other-worldly objects. Another possible manoeuvre would be to develop bluntly disjunctive theories of models, representation, and idealization. A disjunctive theory of representation, for example, will say that a consistent description of a missing system is a straightforward description of a nonlinguistic object in another world which represents systems in the domain of inquiry by standing in various relations of similarity to them, but that inconsistent passages of that sort are involved in the representation of systems in the domain of inquiry in some other, quite different way. I have no knockdown objections to either approach here, but the awkwardness and lack of coherence inherent in either of the resulting pictures is

surely a strike against the idea of taking even consistent descriptions of missing systems to pick out Lewisian other-worldly objects in the first place.[85]

One final point: We have been considering the idea of understanding the face value practice by taking descriptions of missing systems to have possible objects corresponding to them. We arrived at that idea via the thought that such descriptions might usefully be regarded as semantically and pragmatically on a par with descriptive passages in works of ordinary fiction, and the further thought that certain kinds of meta-fictional utterance might then be read as implicitly counterfactual. But we might have arrived at the concrete possibilia view by another, more direct route, quite independently of the idea that the passages in question are little fictions: we might have simply wondered whether descriptions of missing systems should themselves be read as implicitly counterfactual. ("If there were a pendulum which experienced no friction at its point of suspension, and which moved through a perfectly uniform gravitational field, and…then the amplitude of its motion would be unvarying, and….") The search for objects corresponding to such passages which could help us to make sense of the face value practice, and underwrite the sorts of philosophical views we described in section 2, would then have taken us to possible objects in fewer steps. At that point, however, we would again have needed to ask the question, what kind of thing is a possible object? From there on, our investigation would have merged with the last part of the discussion in this section, and the outcome would have been the same.

What if we were to adopt this reading of descriptions of missing systems, on which such passages should be taken to be implicitly counterfactual, but reject all talk of possibilia? Of course, it is not a trivial question whether one can provide an adequate semantics for counterfactuals without invoking possibilia, but let us lay those general worries aside. Suppose that some good general account of counterfactuals which did not posit possible objects were available (as, of course, some people believe there is). Where would such an interpretation of descriptions of missing systems leave us?

Well, then we would not have a realm of description-fitting objects corresponding to descriptions of missing systems, and we could not take the utterances people produce when they are engaging in the face value practice at face value. This would leave accounts like Giere's with serious problems. If there are no description-fitting objects, then it cannot be the case that we use description-fitting objects to represent real systems, models (if there are any) cannot be description-fitting objects, and theories (if there are any) cannot be composed in large part of description-fitting systems. Things would clearly have gone awry in Achinstein's taxonomy of models, too, for if there are no unconstructed analogue models, and no imaginary models,[86] then there is no need to make room for them in the taxonomy.[87]

Suppe, on the other hand, might be able to adopt this interpretation of descriptions of missing systems. His physical systems are neither components of nor identified with anything else which plays a central role in his epistemological and methodological accounts; and he is not counting the class of physical systems as a taxon in a taxonomy of anything. It is true that on Suppe's picture we use physical systems to represent real systems, and talk of physical systems does play a central role in his account of idealization, prediction, experimentation, and theory testing. But Suppe also says that physical systems are "characterizations of how the [real systems] *would have* behaved *had* the idealized conditions been met" (1989, p. 65). Perhaps, then, it is not too revisionary to read Suppe's talk of physical systems as itself implicitly counterfactual. In saying that we make predictions about real systems by considering the appropriate corresponding physical system, he is perhaps saying just that we make predictions by considering how the real system would have behaved under other, idealized conditions – and so on. What is more, Suppe says that he believes that an account of the modal talk at the heart of his picture can be given which "does not involve ontological commitments beyond the constituents of the actual world" (*ibid*., p. 294, n. 3). There is

thus room to hope that Suppe's engagement in the face value practice is relatively unproblematic.

The general point here is as one would expect. It would be surprising, after all, if *any* engagement in the face value practice whilst pursuing investigations in the philosophy of science were automatically damaging. Nonetheless, we should note that the reading of Suppe's account on which its reliance on the face value practice is harmless does change the shape of the picture which emerges from it in certain ways. Most significantly, perhaps, with physical systems replaced by sets of counterfactuals, it is now linguistic representation which seems to be doing the heavy lifting when we make predictions and so on. This is not to say that theories have become linguistic entities, a development which would clearly be at odds with one of Suppe's most central theses. Still, the increased emphasis on linguistic representation in the testing of theories which results from this way of avoiding the trouble that description-fitting objects bring may not be a shift that Suppe himself would welcome.[88]

4. NOT INTERPRETING THE PRACTICE: THE DEFERRAL STRATEGY

I turn now to consider the possibility that the work we have done thus far has been unnecessary. That is, I turn to consider the proposal that for the purposes of the philosophy of science, we can defer these difficult questions about the existence and nature of objects corresponding to descriptions of missing systems (and so, for practical reasons, should defer them). The justification I have in mind for adopting this strategy rests on the claim that any adequate account of the discourse surrounding such passages will legitimate the practice of talking as though there are objects fitting the descriptions in question – the face value practice. In consequence, the idea goes, we philosophers of science can simply get on with constructing accounts of scientific representation,

modelling, theory structure, and the rest, and engage in the face value practice when it is useful to do so, safe in the assumption that when philosophers of language and metaphysicians (and perhaps philosophers of mathematics) settle on a satisfactory account of such talk, we will simply be able to add that account as an appendix to our own work.[89]

This sort of manoeuvre is familiar throughout philosophy, and that is one reason for considering it here. But one can also find recommendations along these lines with respect to specific issues bearing directly on the task of understanding the face value practice. So, for example, Bas van Fraassen advocates the adoption of such a strategy by philosophers of science with regard to worries about the existence and nature of mathematical objects, and he offers exactly the sort of justification I described, insisting that "*for any philosophy of mathematics to be acceptable it must imply that the ordinary use of mathematics is fine*" (2005, p. 97, italics in original). We can talk about functions and vector spaces and set-theoretical *n*-tuples in offering accounts of theory structure, representation, and the like, and leave it to philosophers of mathematics to decide the answers to a range of further questions about the interpretation of such talk.[90] Similarly, Peter Godfrey-Smith, who has independently taken up the idea that we might think of models as "imaginary objects" on a par with fictional characters, seems happy to work with our ordinary ways of talking about fictional characters whilst developing an account of scientific modelling, and defer the task of settling on an acceptable interpretation of that talk until later, when we may wish to take it on for "general philosophical reasons" (2006, pp. 734-736; quoted phrases from p. 735).[91]

There may well be areas in which employing this sort of strategy is unproblematic, but I want to argue that, for two independent reasons, this is not one of them. I will focus on the deferral strategy as applied to talk of missing systems such as the simple pendulum in philosophical accounts of scientific representation, theory structure, models, and idealization, but much of what I say carries over, I think, to its

application to explicit talk of either mathematical or fictional entities in those same contexts.

(1) Both of the objections I will raise against the deferral strategy lie with its justification. We begin by noting that that justification rests on the assumption that any adequate account of the discourse surrounding descriptions of missing systems will legitimate the practice of talking as though there are objects fitting the descriptions in question. It follows that we should feel comfortable in employing the deferral strategy only if we have some reason to believe that assumption – call it '(L)'.

So what reason do we have to believe (L)? There are two obvious ways of providing a comforting answer to this question. One is to do some thinking about possible accounts of the discourse in question, enough to make it seem plausible that…well, that every adequate such account will legitimate the practice of talking as though there are objects fitting descriptions of missing systems – that is, enough to make (L) seem plausible. But that would just be to give up on the deferral strategy. (True, we might be able to make (L) seem plausible in this way without *settling* the matter of whether there are objects corresponding to descriptions of missing systems in a description-fitting way, and if so, what sorts of things they are, and how they fit the descriptions; but doing so will nonetheless involve extensive investigation of possible answers to those questions, and that is still, at least in large degree, to give up on the strategy of deferral.) The other option is to insist that (L) is trivially true, on the grounds that it is part of what we mean by 'adequate' that an adequate account of the discourse surrounding descriptions of missing systems will legitimate the practice of talking as though there are objects fitting the descriptions in question. But granting this can only get us so far, as it is not trivially true that *there is* an adequate account of the discourse in question, in the sense of 'adequate' on which (L) is trivially true – that is, it is not trivially true that there is an account of the discourse surrounding descriptions of missing systems which legitimates the face value practice (and which is acceptable in

various other obvious respects), let alone that the right account will do so. It seems at least epistemically possible, prior to any further inquiry, that the face value practice is illegitimate, and that the right account of the ordinary discourse which surrounds descriptions of missing systems will (therefore) make that clear. One way of trying to convince ourselves there is at least one account of that discourse which is adequate in the relevant sense would of course be to do some thinking about possible accounts and their adequacy, but that would again be to give up on the deferral strategy.

This first objection to the deferral strategy concerns a difficulty we face in attempting to convince ourselves that for the purposes of developing accounts of scientific representation, modelling, theory structure, and so on, we really do not need to worry about the answers to questions concerning the existence and nature of missing systems. One might instead recommend deferral as, strategically speaking, an admittedly risky best bet, whilst allowing that the deferred investigation into the right way of understanding talk about missing systems might ultimately yield the result that some of the ways of talking we have employed in the meantime are illegitimate. If that is the spirit of the strategic recommendation, however, we will want to be much more vigilant than otherwise about keeping an eye on the questions we are deferring, and we will want to get back to them as soon as we can. This will be deferral of a more partial and temporary sort, for we will still have reason to worry.

(2) The second problem with the deferral strategy also centres on (L), and on the justification of the strategy; but we will now put aside the question of the justification of (L) itself. In fact, let us suppose that although we have not gone to the trouble of discovering good reasons to opt for any one particular account of the face value practice, we know, somehow, that (L) is true: we know that any adequate account of the discourse surrounding descriptions of missing systems will legitimate the face value practice. Suppose we know, moreover, that there are in fact one or more adequate accounts, so that we know that the face value practice is legitimate. The idea we are

examining is then that if we find ourselves in such an epistemic situation, it follows that (i) we can engage in the face value practice in constructing accounts of representation, the nature of models, theory structure, and the rest, and furthermore, (ii) in doing so, we need not adopt any particular account of the semantics or ontology of that practice. I will argue, however, that neither thing follows.

My arguments will rest, first, on the observation that there is more than one thing the verb 'legitimate' might mean, and second, on the assumption that in constructing accounts of representation, theory structure, and so on, we are aiming to know the truth about those things, and to understand them. (This assumption about the aims of our philosophical work may not meet with universal agreement, but I will not attempt to defend it here.) The problem is that if one or more of the adequate accounts of the face value practice which we know there to be legitimates the face value practice in the wrong sense, (i) may still be false, for without knowing more we cannot be sure that engaging in the face value practice will not be at odds with aiming to know the truth; and even if all adequate accounts legitimate the practice in the right sense, so that engaging in the face value practice is perfectly compatible with our interest in knowing the truth, (ii) will be false nonetheless, as engaging in the face value practice without further insight into the semantics and ontology of that practice will make it impossible to achieve the aim of understanding scientific representation, theory structure, etc.

The crucial distinction amongst senses of the verb 'legitimate' here is between those which rule out knowable truth and those which do not. So, for example, in saying that an account of the discourse surrounding descriptions of missing systems "legitimates" the face value practice, we might mean that the account makes it clear that, although speaking as if there are description-fitting objects is *ipso facto* a matter of uttering falsehoods, say, or claims without a truth-value, it is reasonable nonetheless to treat some such utterances as true in the right context. (Not all such utterances, of course – given that the simple pendulum has a period proportional to the square root of its

length, no account should make it seem reasonable in any normal context to treat as true an utterance of 'The period of the simple pendulum is proportional to its length,' as some student somewhere no doubt has.) An account which legitimates the face value practice in this sense might even come equipped with a story about why we are so easily gulled into thinking that some of the utterances in question are genuinely true – an error theory. But legitimation in this sense is in any case incompatible with the supposition that some of the utterances which make up the face value practice are true, and known to be so.

The same goes for an account which legitimates by insisting that the utterances in question make claims which, though truth-valued, are unknowable, but then goes on to explain how the claims in question can be "acceptable" in something like the sense in which some scientific claims about the unobservable world are acceptable according to the constructive empiricist – a sense of 'acceptable' in which P's acceptability does not entail either its truth, or its falsehood, or its believability.[92] Again, legitimation in this sense is clearly not a matter of showing that when we produce the sorts of utterances which make up the face value practice, the things we say are true and known to be; indeed, legitimation of this sort is incompatible with such a result.[93]

Alternatively, in speaking of legitimation we might mean precisely the activity of making it clear that at least a good range of the utterances produced when we engage in face value practice make claims which are both true and knowable. There are thus at least two senses of 'legitimate' in which the knowable truth of the claims we express when engaging in the face value practice is ruled out, and one in which it is entailed.

Now, if an account of the discourse surrounding descriptions of missing systems legitimates the face value practice in a way which involves ruling out the knowable truth of the claims we make we engaging in that practice, then given that account part (i) of the assumption we are examining will be false: we cannot engage in the face value practice in constructing accounts of representation, the nature of models, theory

structure, and the rest, given that we are aiming to know the truth in constructing such accounts, for our accounts will necessarily contain either false claims or unknowable ones. If, for example, we offer an account of scientific representation which us commits to uttering sentences like 'Scientists use the simple pendulum to represent real pendula by specifying the respects and degrees of similarity between the two,' but utterances which involve speaking as though there are description-fitting objects corresponding to descriptions of missing systems are all false, or unknowable, then at least some important part of our account of scientific representation is either false or unknowable.[94] Consequently, if, for all we know, one or more of the adequate accounts which we somehow know there to be legitimates the face value practice in a way which precludes the knowable truth of the claims we make when we engage in that practice, and we know no more, then for all we know the *correct* account of the face value practice is one which rules out knowable truth. And this means that, for all we know, engaging in the face value practice in developing accounts of representation, theory structure, and the rest is entirely at odds with aiming to know the truth about those things. We will thus not be justified in continuing to engage in the practice in such an epistemic situation.

Suppose instead, then, that we do know, somehow, that all adequate accounts of the discourse surrounding descriptions of missing systems legitimate the face value practice by reassuring us that a good range of the utterances we produce when engaging in that practice make claims which are both true and knowable. Such accounts will give us no reason to eschew the face value practice in our philosophical work; but that is not to say that they will justify the deferral strategy. Countenancing a practice is not the same thing as showing that we can achieve our immediate philosophical aims without an articulated understanding of it. And I want to argue that if we engage in the face value practice when developing accounts of representation, theory structure, and the rest, then those accounts will yield an understanding of their targets only when supplemented by an understanding of the face value practice. Deferral is thus at odds

with the immediate aim of understanding representation, and theory structure, and the rest.[95]

To see this, consider the two questions at the centre of our inquiry into the semantics and ontology of the face value practice: Are there objects which correspond to descriptions of missing systems in some description-fitting way or other? And if so, what sorts of things are they (mathematical structures, concrete possibilia, property-containing objects…)? Call these *the existence question* and *the nature question*, respectively. The issue at hand is whether we can arrive at an understanding of scientific representation, theory structure, and so on by speaking as though there are such objects, knowing (somehow) that we are speaking truly in doing so, but putting off the task of answering those two questions.

Take the existence question first. To show that this question cannot be deferred consistently with our immediate aims, I will consider the familiar example of Giere's account of representation; the argument carries over to any account of representation which centrally relies on the face value practice, and to accounts of idealization, theory structure, the nature of models, explanation, confirmation, etc., which do so.

Giere's account of representation commits us to engaging in the face value practice by uttering sentences like this one:

Scientists use the simple pendulum to represent real pendula by specifying the respects and degrees of similarity between the two. (S)

(And note that although neither of them is in the business of providing an explicit account of scientific representation, both Achinstein and Suppe say things of just the same sort.) Now the mere fact that the account involves us in the face value practice does not preclude our knowing it to be true, given the assumption we are now making that a good range of the utterances which make up the face value practice make claims

which are knowably true. Of course, the account might be false nonetheless (or, perhaps, true but unknowable), as the assumption in question does not guarantee the knowable truth of the claims made by any particular set of utterances; but in that case appealing to it would be at odds with the aim of knowing the truth about representation. So let us suppose then, that Giere's account is knowably true; suppose, in fact, that we know it to be true. The remaining problem is simply that without an answer to the existence question we cannot claim to know what Giere's account *says*, and so we cannot claim to have arrived an understanding of scientific representation by considering it.

To see this, consider the following. Either there are description-fitting objects such as the simple pendulum, or there are not. If there are, then presumably sentence (S) should be taken literally, along with the rest of the account of which it is a part. If there are not, on the other hand, then scientific representation does not involve such objects; and as the sentence in question says otherwise on its surface, then (given our assumption that the sentence is making a true claim) the sentence is not to be taken literally, and nor, by extension, is the account of which it is a part. So if we do not know whether there are description-fitting objects – if we do not know the answer to the existence question – we cannot know whether Giere's account of representation is to be taken literally. We also do not know how the account should be taken if it is not to be taken literally. If there is no simple pendulum, but (S) is true nonetheless, then it is not obvious what (S) means; so what does it mean? We surely cannot claim to have arrived at an understanding of scientific representation by referring to Giere's account in the midst of such fundamental uncertainty about how the account should be taken. Yet removing that uncertainty will at least involve answering the existence question about description-fitting objects, and so will require us to reject deferral strategy.[96]

What about the nature question? Suppose we somehow managed to convince ourselves that there are description-fitting objects corresponding to descriptions of missing systems, thus settling the existence question; could we at least then defer the

question of what sorts of things those description-fitting objects are? Again, the answer is no – not if we hope to come to understand scientific representation, theory structure, the nature of models, and the like by relying on accounts of them which engage in the face value practice.

Consider, for example, Giere's accounts of representation and theory structure. One lesson of the preceding sections of this paper is that if there are description-fitting objects corresponding to descriptions of missing systems, then inserting different stories about what sorts of thing they are into Giere's accounts will yield very different final pictures. If, *per impossibile*, the simple pendulum were an abstract object which had all and only the properties ascribed to it in the textbook description, then Giere's account of representation would lead most naturally to a picture in which we are drawing on property-sharing relations of similarity between the simple pendulum and my grandfather clock when we use the first to represent the second; and his account of theory structure would lead to a picture in which theories are composed in part of many such abstracta. If instead the simple pendulum is a concrete inhabitant of some Lewisian possible world other than ours, the resulting picture of representation will again rest on property-sharing relations of similarity, but the picture of theory structure will be quite different, as theories will now have collections of concrete possibilia drawn from other possible worlds as central constituents. And if the simple pendulum is a mathematical structure, the picture of theory structure will be more familiar, but representation will no longer be grounded in property-sharing relations of similarity, relying instead on some variety of structural similarity, perhaps, or on relations of denotation between mathematical properties and properties of the systems in the domain of inquiry. And so on for the other conceptions of missing systems we have considered.

It is clear, then, that the answer to the nature question can make a substantial difference to the picture of representation or theory structure which emerges from a given account when that account is enmeshed in the face value practice. We thus cannot

claim to have achieved the aim of understanding representation, or theory structure, by appealing to accounts which make use of the face value practice until we have answered the nature question (unless, of course, we have already answered the existence question negatively); the same goes, presumably, for accounts of models, idealization, explanation, and other phenomena. Once again, then, we see that deferral is at odds with our immediate philosophical aims in constructing such accounts.

I have considered two ways in which an account of the discourse surrounding descriptions of missing systems might legitimate the face value practice: a legitimating account might either preclude or entail the knowable truth of the claims we make when engaging in the face value practice. But there is a third possibility, of course, at least logically speaking: an account might legitimate the relevant discourse, in some recognisable sense of 'legitimate,' whilst leaving open the knowable truth of the claims in question. At this point, however, it is easy to see that legitimation of this third variety would also make it impossible to justify the deferral strategy. Suppose we know that there are adequate accounts of the discourse surrounding descriptions of missing systems, and that all the adequate accounts legitimate the face value practice in one sense or another. Still, if one or more of the legitimating accounts leaves open the knowable truth of the claims we make when engaging in the face value practice, and we defer further investigation, then for all we know, everything we say when we engage in the face value practice is false, or unknowable. And as long as we are ignorant on that score, we cannot come to know the truth about representation, or theory structure, or models, or anything else by engaging in the face value practice.

5. CONCLUSION

We have explored a number of ways of making sense of the face value practice, several of which were suggested by following up on the idea that descriptions of missing systems are, as I put it, little fictions. And we have drawn a number of specific conclusions about particular options along the way. As a result, it has become clear that the face value practice cannot be treated as unproblematic, and 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. We should thus think carefully about whether we are going to continue to engage in it when offering up accounts of such central topics as scientific representation, the nature of models, the structure of theories, and the workings of idealization.

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.[97] There is a case for that conclusion to be found in the fact that we have encountered so many difficulties in the attempt to spell out an explicit understanding of the practice on which there are description-fitting entities of the requisite sort. Believing in entities corresponding to descriptions of missing systems, as we have seen, repeatedly lands us in trouble one way or another. And if we throw out description-fitting objects, certain views in the epistemology and methodology of the sciences may have to go with them.

Of course, there is room for further exploration here. It would be particularly valuable to explore alternative ways of developing the little fictions approach, drawing on other accounts of fiction and fictional characters than the ones I have examined here.[98] Despite my suspicions to the contrary, we may discover that doing so will lead us to an attractive way of accommodating the idea that there are description-fitting objects

capable of doing the theoretical work that accounts like Giere's and Achinstein's require. But we might instead conclude that some version of the little fictions approach which eschews description-fitting objects gets the interpretation of descriptions of missing systems right. There is, furthermore, at least one other entirely distinct idea worth examining: perhaps descriptions of missing systems are not descriptions at all, but definitions which serve to define non-denoting terms.[99] And if in the end we must do without a realm of description-fitting objects, then the important task which remains will be to say how we should think about representation, models, theory structure, and idealization once we take seriously the thought that missing systems are, indeed, not there.[100]

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Weisberg, M. (2007), "Who is a Modeler?", *The British Journal for the Philosophy of Science 58*: 207-233.

[1] See section 2.1 for further discussion of the example from Poincaré, and see Jones (2005) for my own attempt to say something about the significance of talk about idealization.
[2] See Giere (1988), pp. 70-71, and the textbooks cited in n. 2 on p. 64.
[3] At least, these are likely to be the terms in which an intermediate-level textbook in classical mechanics will describe the simple pendulum. In his critique of Giere's book, Norton Wise (2005, pp. 273-4) emphasises the fact that whereas such intermediate-level texts put Newton's laws at centre stage, and with them the notion of force, advanced textbooks tend to give primacy to the Hamiltonian approach, and so shift the focus to the notion of energy. Wise writes: "The entire conceptual apparatus is different, including the causal structure. And it includes large areas of experience to which Newton's laws do not apply" (2005, p. 273). This means that the range of missing systems described in a pair of texts drawn from the two levels might be significantly different; it also means that when a given missing system makes an appearance in both, the two descriptions might be quite different. (See, for example, the discussion of the simple pendulum in Goldstein's classic advanced text (Goldstein, 1980 [1950], p. 459).) Note, though, that although these are important observations in some contexts, they do not bear directly on the points I am making here: it is enough for my purposes that textbooks at both levels are replete with descriptions of missing systems. (In his response, incidentally, Giere concedes Wise's point, but argues that it does not affect the plausibility of his claims about theory structure or representation (2005, pp. 287-288).)
[4] There are other ways, too, in which real pendula fail to match the textbook description of the simple pendulum, but the point is made.
[5] So although both scientists and philosophers of science readily engage in the face value practice, I wish to limit attention to its employment in philosophical contexts for the purposes of this paper. Accordingly, the phrase 'the face value practice' should hereafter be taken to refer to the practice as engaged in by philosophers.
[6] I rephrase the question this way in order to distinguish it from the parallel question I raise about philosophers of science at the end of this subsection. Note also that this is the question Frigg calls "the *ontological puzzle*" (2006, p. 50, Frigg's italics).

[7] See also the preceding paragraph, in which Giere prefaces his introduction of this use of the term 'model' by saying that "[i]t is a useful methodological tactic in constructing a theory of science to employ, insofar as possible, terms that are current in sciences themselves—and with roughly the meaning they have in these sciences" (1988, p. 78).

[8] Actually Achinstein divides representational models into *four* categories, dividing those which represent by reproduction into three subgroups (*ibid*., pp. 209-210). Those details are irrelevant to our purposes, however.

[9] The first example is one of Achinstein's own (*ibid*., pp. 209-210). The second is taken from Spector (1965, pp. 126-128), and is a minor variant of a more sketchily-drawn example Achinstein gives (*ibid*., p. 210, and see his n. 12).

[10] As for what Poincaré did mean to be doing, on Achinstein's view: to classify something as an imaginary model is also, in part, to ascribe to it a certain function, that of establishing that if the "object or system [described]...were to satisfy certain conditions initially specified...then it would be at least logically possible to suppose the object or system is as described in the model" (*ibid*., p. 220). The idea is in part that by showing that some object or system might be (or might have been) a certain way, an imaginary model serves to establish that a certain sort of system is possible – a world with a certain sort of non-Euclidean geometry, for example, or an ultimately mechanical electromagnetic field (to take the other example Achinstein gives, which is Maxwell's mechanical model of the electromagnetic field as a system of vortices separated by layers of rolling particles). This difference in function is perhaps the feature of imaginary models (when we are talking about the "objects described") which distinguishes them from unconstructed analogue models, as the purpose of the latter is to represent actual systems as actually having a certain structure or actually behaving in certain ways. See *ibid*., pp. 220-221, for Achinstein on the function of imaginary models, and Poincaré (1952), pp. 64-68.

[11] One might wonder whether, by "the object described," Achinstein means simply the actual universe (in Poincaré's case), or the actual electromagnetic field (in Maxwell's), for even knowing misdescriptions can properly be called descriptions. But apart from the obvious fact that this would make it odd to call the objects described "imaginary," the actual electromagnetic field (say) cannot count as an imaginary model simply because, not being mechanical, it cannot serve the purpose of establishing the possibility of a mechanical electromagnetic field. Furthermore, it seems clear that Achinstein would want to say that a different radically inaccurate description of the electromagnetic field would pick out a distinct imaginary model (in the "object described" sense), but that is not the result we get if we take the actual field to be the object described in such cases.

[12] Most prominently, many proponents of the semantic view of theory structure have been explicit about meaning to employ a logician's notion of model in presenting their accounts (though not always the same one – see Thomson-Jones (in preparation)). Suppes (1960, esp. p. 289) has gone further and claimed, with some qualifications, that we can usefully treat scientists and logicians as referring to the same sorts of things (or even employing the same notion) in their respective uses of the term 'model,' but this is clearly distinct from the more widespread idea that the logical notion of model is a valuable analytical tool when we face the philosophical task of constructing a fruitful account of scientific theory structure.

[13] See Thomson-Jones (2006) and (in preparation) for my own answers to both questions, and a critique of the semantic view in light of those answers.

[14] Thomson-Jones (in preparation) contains an extensive presentation and discussion of the taxonomy. Note, however, that I am not claiming complete comprehensiveness for the taxonomy. The most significant outstanding question, I think, is whether computer simulations should be regarded as models (as opposed to being merely ways of presenting, say, propositional or mathematical models), and if so, whether they can be seen as falling into one of the five categories I have specified.

[15] I have in mind here the ways of filling out their talk of models considered in sections 3.2 and 3.6. The proposals considered in sections 3.3 and 3.5, on the other hand, yield notions of model which do not fit neatly into my taxonomy. See Thomson-Jones (in preparation) for further discussion.

[16] See, for example, Cartwright, Shomar, and Suárez (1995), 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.

[17] See, for example, Suppes (1957, ch. 12), (1960), (1967), and (1974), and van Fraassen (1970), (1972), (1980, ch. 3), (1985), (1987), and (1989, ch. 9). See also Thomson-Jones (2006) for more on the semantic view as formulated by these authors.

[18] Likewise Giere: "This view of the relationship between linguistic entities, statements or equations, and models, which, even though abstract, are not themselves linguistic, is very similar to that advocated by van Fraassen" (1988, p. 79).

[19] As I argue in Thomson-Jones (2006).

[20] Another author whose work is usually included in the list of primary elaborators of the semantic view is Frederick Suppe; his (1989) is typically cited. Although Suppe certainly defends a view of theories according to which they are nonlinguistic entities, I think there are significant differences again between his view and those of Suppes, van Fraassen, and Giere, the most obvious of which is that he does not seem to think of

theories as collections of models – indeed, he does not even employ the term 'model' very often, as a quick inspection of the index of his (1989) will confirm – or as collections of any other sort of thing. I will turn to consider some of Suppe's views about the epistemology and methodology of scientific theorising in section 2.4 and at various points throughout the rest of the discussion.

[21] See Callendar and Cohen (2006) for a contrarian answer to the latter question. In fact, I am inclined to agree with them that scientific representation is in some sense not fundamentally different from other kinds, or that what constitutes representation is science is no different from what constitutes representation in the pictorial arts, or cookbooks, or any number of other areas. It is compatible with this, however, that representation in science often *proceeds* in certain ways which are peculiar to the sciences – or perhaps in ways which are peculiar to the sciences and certain other realms of discourse, but which are not universal to representation. Suppose, for example (and just as an example), that we decide that all representation is fundamentally a matter of the use, by a Gricean intender, of some item to represent, or convey something about, some other item or class or items; but suppose we also agree that representation in science typically involves the use of a certain kind of item which is not much used for representation outside the sciences. Then it may still turn out that focussing on the ways of proceeding representationally – the representational techniques, as we might call them – which are relatively peculiar to the sciences (namely, the use of items of the specific sort in question) will yield some insight into the workings of scientific explanation, or confirmation, or into the question of scientific realism, say. Callendar and Cohen are, I suspect, sceptical of the idea that there might be such a systematic difference of even this non-constitutive kind between scientific representation and representation in at least some other areas; but none of this needs to be decided for our present purposes.

[22] Giere develops and modifies this account in various ways in later writings, but not in ways which make an important difference here. In his (2004), for example, Giere replaces the just-quoted claim that "models are *the* means by which scientists represent the world" (1988, p. 80, emphasis added) with the weaker claim that that models are "the primary (though by no means the only) representational tools in the sciences" (2004, p. 747). He also insists that we play a crucial role in bringing it about that a given model represents a given system (e.g., 2004, p. 743.) To paraphrase: it is not that when model *m* represents system *s* it does so independently of us, and just in virtue of the holding of some relation of similarity which constitutes representation, or (more weakly) on which the relation of representation supervenes; rather, when *m* represents *s* it does so because we use it as a representation of *s*, in part by focussing on certain similarities between the two. Giere also claims that all our scientific representing is done with some end in mind, and that the ends of our representational activity can vary. Thus representation is, on this view, a pragmatic matter in two senses: to understand representation we must take into account the relationship between the representing object and ourselves as representers; and representation as an activity is indexed to a goal. I take these features of the view laid out in Giere's (2004) to be elaborations or extensions of the basic picture presented in his (1988).

[23] Peter Godfrey-Smith endorses the basic picture in a recent paper (2006, sections 1 and 4), for example, although it should be noted that he regards Giere's picture as characterising just one kind of scientific representation, and opts for a development of that basic picture which goes beyond Giere's writings (and is perhaps at odds with it in certain respects – see pp. 734-735 and 736-737).

[24] In this, Giere is in a number of important respects echoing the picture advanced in Nancy Cartwright's *How the Laws of Physics Lie* (1983); see especially essays 7 and 8.

[25] See, e.g., *ibid*., p. 83: "[I]dealized conditions are imposed [on physical systems] which actual phenomena cannot ever meet."

[26] For example, in his introduction to *Fictionalism in Metaphysics* (2005), Mark Kalderon sees van Fraassen's constructive empiricism as embodying a fictionalist approach to scientific theories more or less on a par with the approaches that moral and modal fictionalists take to moral utterances and utterances about possible worlds, respectively (Kalderon, 2005a), as does Gideon Rosen in his contribution to that volume (Rosen 2005); and see http://fs-morente.filos.ucm.es/actividades/2006/congreso/inicio.htm for abstracts of the papers given at a recent conference in Madrid entitled "Scientific Representation: Idealisations, Fictions, Reasons."

[27] Note that this is not the fictionalism of Vaihinger (1924) (see also Fine (1993)), nor those discussed by Scheffler (1963), Scheffler (1970), or Nagel (1961), p. 134.

[28] Frigg (2006) and Godfrey-Smith (2006) independently raise similar questions. Frigg asks whether Giere's models might be taken to be "fictional entities" (2006, p. 61), but does not attempt an answer. Godfrey-Smith advocates an approach to understanding modelling as a representational strategy which would seem to involve answering 'yes' to at least the last two of the questions in my list, and perhaps to all four. He does not address the question of exactly how we should understand the semantics or the pragmatics of works of fiction, however, or the question of the ontological status of fictional characters, arguing instead (as I read him) that an answer to those questions can be deferred, and that the constraints on any acceptable answer to them are such that we can make progress as philosophers of science simply by drawing the analogy, and without yet having any full-fledged account of the semantics or the ontology in either case. It is in this comfort with deferral that Godfrey-Smith and I part ways. See section 4 for discussion of that issue.

I have also recently learned that David Davies has a forthcoming paper in which he asks some similar questions about thought-experiments, and one might suppose that a description of a thought experiment is just a particular sort of description of a missing system. In any case, it would certainly be worth exploring the connections between the two sets of issues.

[29] 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.

[30] See also Frigg (2006), p. 61.

[31] There is a small logical gap in my reasoning here. One could argue that the fact Giere's proposal needs to respect is just that there are (to the best of our knowledge) no simple pendula in *our* spacetime. So perhaps we might address the initial difficulty facing the thought that there is a such a thing as the simple pendulum, and that descriptions of the simple pendulum are straightforward descriptions of it, by proposing that the simple pendulum is an inhabitant of another space-time, one just as real as ours but unconnected to it. In that case, there would be no reason the simple pendulum could not have all the properties ascribed to it in the descriptions we find in textbooks. That is, Giere can avoid inconsistency by using 'abstract entity' to mean 'entity inhabiting another spacetime.'

This would be a very odd use of the word 'abstract,' and, for that reason and others, I take it that the corresponding reading of Giere's text is very implausible, but we might nonetheless consider it (at least for a moment) as an alternative to the proposal I take it Giere intends to be putting forward. In fact, this idea will cross our path again, for other reasons, in section 3.6, and I will discuss its merits and demerits then. For the remainder of this section, however, I will lay it aside.

[32] More specifically, we will consider the options of supposing that the simple pendulum (a) instantiates certain corresponding mathematical properties (section 3.2), (b) contains the properties mentioned in the textbook descriptions as parts (section 3.3), and (c) "holds" those properties (section 3.5).

[33] Note that I am not attributing to Giere the view that representation just is similarity, or that similarity is necessary and sufficient for representation, even in some restricted domain. See Giere (2004), pp. 747-748, for an explicit rejection of any such thesis, including his endorsement (p. 747, n. 8) of Suárez's (2003) arguments for such a rejection. See also my n. 22, above.

[34] Cf. Suárez (2003, p. 227), who calls the thesis that "A and B are similar if and only if they share a subset of their properties" the "*identity-based theory* of similarity" (Suárez's emphasis).

[35] To be more precise: object x is similar to object y in respect of ψ in this second way iff x has property ϕ, y has property ϕ', and ϕ and ϕ' are distinct determinates of the determinable ψ which are close to one another as judged by some relevant metric on the determinates of ψ, and some relevant standard of closeness. (In the Buddha case, the obvious metric would set the "distance" between two heights, h_1 and h_2, at $| h_1 - h_2 |$, and it is easy to imagine that in ordinary contexts we might employ a standard of closeness according to which two heights which are only 1 cm apart, say, count as nearby.) Both the metric and the standards of closeness might vary with context, and both might be vague, but this fits nicely with the fact that judgements of similarity in a respect can themselves be both context-dependent and vague. Note also that by allowing the metric to be vague – in that, for example, that there might be no determinate answer to the question of whether ϕ is closer to ϕ_1 or ϕ_2, or equally close to ϕ_1 and ϕ_2, for some ϕ, ϕ_1, ϕ_2 – I am using the term 'metric' loosely here. (One reason for employing a notion of metric which is loose in this way is that not all determinables are quantitative in the way in which, say, height, position, and velocity are. Consider, for example, the case of colour. It seems plausible to suppose that ordinary judgements of similarity in respect of colour rest implicitly on a sort of metric on the colours – these two colours are very close to one another, these are quite far apart – but even if particular colours are ultimately quantitative properties, individuated in ways which involve wavelengths, frequencies, spectral reflectances, or the like, it is clear that the metric we are relying on in making *ordinary* judgements is not set up with reference to any such quantities.)

Note that first kind of similarity – property-sharing – can often be regarded as a special case of the second, in which $\phi = \phi'$ (that is, x and y instantiate the same determinate of some determinable). In such a case, presumably, the distance between ϕ and ϕ' will be 0 on all metrics, and so ϕ and ϕ' will count as close on all standards of closeness. This will seem artificial, however, when the property which x and y share is not a determinate of any very natural determinable.

Alternatively, one might try to collapse the second sort of similarity into the first, by claiming that whenever two objects instantiate nearby determinates ϕ and ϕ' of some determinable ψ, there is a single property they both instantiate – χ, say – such that they are similar in virtue of their common instantiation of χ. This will seem natural enough in some cases: if book 1 instantiates scarletness, and book 2 instantiates crimsonness, then instead of saying that their similarity consists in their possession of nearby determinates of the determinable colour, we might say that their similarity consists in their common possession of the (distinct) property of redness. In other cases, however, there will again be some artificiality in the manoeuvre. What is the property which both the Buddhas on my bookshelf have, and in virtue of their common possession of which they are heightwise similar? The property of having either height h_1 or h_2? The property of having a height in some range into which both h_1 and h_2 fall? (Which such range?)

On balance, then, I will treat these two kinds of similarity – property-sharing and the possession of nearby determinates – as just that, and resist the temptation to reduce one to the other.

Given the fact that judgements of similarity of the nearby-determinates variety are entirely quotidian, incidentally, and given the *prima facie* awkwardness involved in reducing similarity of that sort to property-sharing similarity, it seems that if Suárez is right in saying that it is "typically assumed" that "A and B are similar if and only if they share a subset of their properties" (2003, p. 227), then that is a problem. At the very least, further argument would be needed to make such an assumption convincing. In the meantime, we have reason to resist one of Suárez's arguments against the thesis that "A represents B if and only if A is similar to B," which he calls "[sim]" (*ibid*.). Suárez argues (*ibid*., pp. 234-235) that [sim] cannot accommodate the fact that inaccurate representation is ubiquitous. The thought seems to be that a Newtonian model of the solar system, for example, might share no properties with the solar system itself, because in the absence of the necessary relativistic corrections, all the numbers will be a little off, so to speak; and yet it is surely false to say that the Newtonian model does not represent the solar system (albeit somewhat inaccurately). But this argument relies crucially on the assumption that similarity must involve property-sharing. Clearly if similarity can instead be a matter of the possession of nearby determinates, then the inaccuracies of the Newtonian model present no obstacle to its being similar to the solar system, and we have no difficulty for [sim] here. If, on the other hand, nearby-determinates similarity can be collapsed into property-sharing similarity, then if the Newtonian model is similar to the solar system in the nearby-determinates way, it will follow that the two are similar in the property-sharing way, and we again have counterexample to [sim]. (This is not to say that I ultimately want to defend [sim]. In addition to many of the other problems Suárez discusses in his (2003), [sim] faces the difficulty that, as I am arguing in the present section, the Newtonian model cannot have the sorts of properties it would need to have to be similar to the solar system in *either* of the two ways we have been considering.)

[36] 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 markings as, well, quotation marks. See also other passages sprinkled throughout this part of the book, such as one on p. 80, where Giere speaks of the "length of the string" in the "pendulum oscillator" (which object, context makes it clear, is supposed to be a model, and thus an abstract entity.)

[37] 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 $\cos\theta = 1$) – see the transition from Giere's (3.5) to his (3.7) on pp. 70-71.

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. It is worth noting, however, another problem for Giere's account which lurks here: Giere says that models (such as the simple pendulum) satisfy the equations used to characterise them *by definition*, and this claim plays a role in his argument for the conclusion that his account of representation and of theory structure enables us to avoid the sorts of worries about the correspondence theory of truth which have plagued scientific realism in the past (*ibid*., pp. 79 and 82). But given the fact that the equation of motion used to characterise the simple pendulum relies on an approximation, the simple pendulum does not in fact satisfy it, and so does not satisfy it by definition. Nor is this a unique feature of the simple pendulum. (The alternative here would be to insist that the simple pendulum does in fact satisfy the equation in question by definition, even though the equation was arrived at by reasoning which involved making an approximation at a certain point; but this would leave us in the uncomfortable position of saying that the simple pendulum does not satisfy Newton's laws.)

[38] 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.

[39] Modulo the qualification made in n. 31, above.

[40] As noted above (n. 23), Godfrey-Smith (2006) endorses a number of the essential elements of Giere's picture of representation (although it is a central point of Godfrey-Smith's paper that he regards the picture as capturing just one sort of scientific 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. Weisberg, on the other hand, does not seem to face these difficulties, despite the fact that he and Godfrey-Smith are arguing for a similar distinction amongst representational strategies; for Weisberg, the intermediate entities employed in modelling are "abstract, mathematical models" (2007, p. 217), and he claims only that the relations between such models and real systems "can…*loosely* be described as relations of similarity" (*ibid*., p. 221).

[41] 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 R. I. G. Hughes's (1997) "DDI" account, applied to mathematical structures. It seems plausible to assume that Hughes himself intends his account to apply to representation by mathematical structures, given his initial discussion of Galileo's use of geometrical structures to represent motions (*ibid*., section 2). There is some room for doubt, however: perhaps Hughes is here thinking of Galileo's diagrams as the representations, rather than the mathematical structures towards which those diagrams point. And although Hughes speaks explicitly of representation by "mathematical models" (e.g., on p. S332), I am not entirely sure whether he means to be picking out mathematical structures of the sort I have in mind with that term, or, for example, sets of equations (or the corresponding sets of propositions). (Relatedly, see *ibid*., p. S326, ¶1.)

[42] In the case of Achinstein's imaginary models, the point is not to represent the way systems from the domain of inquiry actually are, but to represent ways they could have been. See n. 10, above, and Thomson-Jones (in preparation) for further discussion.

[43] See Thomson-Jones (2006).

[44] See, for example, various passages in chapter 5 of Gould and Keeton (1996) for the nuclear model of the cell.

[45] See Thomson-Jones (in preparation) for an argument in support of this claim.

[46] 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 mean to use it simply to pick out the relation, whatever exactly its nature, and whatever the nature of the relata, which holds between, say, my car (which is lime green) and the property of being lime green (supposing there to be such a thing as the property of being lime green). I will thus speak interchangeably of objects having properties and instantiating them.

[47] See also section 3.5 for another proposal in this category.

[48] See Teller (2001), p. 399, n. 13 and the corresponding text. Teller is here 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. He also claims that his proposal defuses a distinct objection due to Wade Savage.

[49] 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 (*ibid*., p. 399). This is true in one sense, of course: if there are no abstract objects, then there is no question of understanding how abstract objects can stand in those sorts of relations of similarity to concrete objects – or relations of similarity of any other sort, or any other sort of relation, to anything. But this does not mean that the nominalist can simply point at Giere's account, say "I believe that, nominalistically translated," claim thereby to avoid the problem I have raised, and then insist that she has now given a satisfactory account of representation, models, theory structure, or idealization. As the nominalist denies that there are really any abstract objects, she will (if she has embraced Giere's account, or Teller's proposal, albeit in her nominalistic way) be committed to denying that there is really any such thing as the simple pendulum. Accordingly, the picture of how representation and idealization works, and of how we should understand talk of models and theories, which we end up with once the nominalistic translation has been applied is bound to look rather different than the picture we get from Giere's account read in a more literal-minded way (even if the nominalist translation strategy yields some true sentence from "There's this abstract object, the simple pendulum, which we use to represent actual pendula via relations of similarity it stands in to them, and which is a component of classical mechanics"). See section 4 for a more general discussion of this sort of issue.

[50] Cf. Armstrong (1989), ch. 5, and esp. pp. 75-77. Note, incidentally, that given a commitment to the existence of properties understood as abstracta, one might argue that we do have independent reason for believing in property-containing objects after all, given the right development of the idea: If a property-containing object is taken to be the mereological sum of the properties it contains, for example, then it will follow that there is such an object from the existence of the abstract properties themselves and mereological universalism (the view that, given any F's, there exists a thing which is the mereological sum of the F's). Alternatively, if the property-containing object is taken to be the set of all and only those properties it contains (as members, now), we might see the existence of the object as following from the existence of the abstract properties themselves and some sort of analogous universalism about the existence of sets; the details will have to be handled more carefully in this case if we want to avoid Russell's paradox, of course. This line of argument would rest on the claim that there is good independent reason for accepting one of these varieties of universalism.

[51] Note that there is no general problem for bundle theory in making room for nonspatiotemporal particulars here: a nonspatiotemporal particular can simply be a bundle which contains no spatiotemporal properties. The special problem for the property-containing view is that a great many of the objects which correspond to descriptions of missing systems will have to contain spatiotemporal properties as parts in

order to fit those descriptions in the way Teller suggests, and in order to be similar to the systems we find in the world around us in the somewhat indirect way he has in mind.

Note also that there are some who claim that particulars (and so concrete particulars) contain their properties as parts quite apart from any commitment to bundle theory. If people with that view also hold that for a particular, having a property just is containing it as a part, then they will have trouble with the property-containing view, too.

[52] The simplest case to have in mind here is that of descriptive passages in novels.

[53] The formulations which follow presuppose the existence of propositions. That presupposition will remain in place throughout the ensuing discussion, but we could dispense with it easily enough given the right sort of reformulation. Nor do I mean to imply that all the authors cited in connection with these views themselves employ a theoretical framework involving propositions.

[54] Although not, for example, when we are joking, or when we are being sarcastic, and perhaps not when we are speaking metaphorically.

[55] See John Searle's "The Logical Status of Fictional Discourse," reprinted as essay 3 of his (1979), and Lewis (1978, p. 40) for two prominent expressions of this view, which Currie (1990, p. 12) calls the "pretense theory"; and see Currie (1990), p. 13, n. 14 for references to a number of other appearances of it in the literature. See also *ibid*., section 1.4 ff., and Walton (1990), pp. 81-83, for criticisms of the pretense theory and Searle's arguments for it.

[56] This is an outline of the theory Gregory Currie develops in *The Nature of Fiction* (1990). See sections 1.5-1.11 for a more detailed exposition. Statements of the core components of the view, as presented here, can be found on pp. 30-34 (section 8) and p. 46 (section 1.10).

[57] This is a slight extension of Kendall Walton's account, as presented in his *Mimesis as Make-Believe* (1990). Walton tends to focus on sentences when discussing narrative fiction, rather than on the utterances which produce them, perhaps precisely because he wants to allow that appropriately-patterned cracks in a rock might make up a work of fiction. For the sake of parallelism, I have presented the view as a view about fictional utterances nonetheless, with a corresponding broadening of the notion of an utterance.

[58] Despite a common emphasis on the importance of the notion of make-believe in Currie's and Walton's accounts – Currie credits Walton's work with having drawn his attention to this idea (*ibid*., p. 18, n. 22) – there are central points of disagreement between them. See Currie, *ibid*., pp. 18-19 and 35-56, and Walton (1990), pp. 85-89.

[59] Though see the end of section 3.6 for a qualification.

[60] One might think that the right view of propositions will solve this problem for us. The line of thought would go as follows: An author can only pretend to assert, or present for use in games of make-believe, propositions which exist. When a proposition is expressed by a sentence containing a singular term, the proposition contains the denotatum of that term as a constituent. Thus if there are fictional utterances of the sentence 'The simple pendulum moves (approximately) sinusoidally,' there is an object denoted by the term 'the simple pendulum.'

(It is tempting to baulk at the claim that an author can only pretend to assert propositions which exist – after all, I can surely pretend to climb into a submarine without there being a submarine into which I am pretending to climb. But any such baulking would be misplaced. Given that propositions are supposed to play the semantic role of providing, or being, the meanings of sentences, then given that a fictional utterance is meaningful, unpacking the pretence theory by appeal to propositions will clearly involve treating pretending-to-assert as a two-place relation between a speaker and the proposition expressed by the sentence she utters fictionally, and will thus require the existence of the relevant proposition.)

There are at least two reasons that such a line of thought is unpersuasive, however. First, of course, is the fact that a singular term like 'the simple pendulum' would seem like a prime candidate for a descriptivist treatment, so that the corresponding constituent of a proposition expressed by a sentence in which it appears will be some concept or property (or perhaps a combination of concepts or properties), rather than a denotatum. Secondly, even if we let such a line of thought carry us to its end, that is not far enough. At most, we get the result that there is some object picked out by the term 'the simple pendulum.' But the fact that someone has successfully produced a fictional utterance of 'The simple pendulum moves (approximately) sinusoidally' gives us no reason, on the views described above, to think that the proposition expressed by that sentence is *true*. Consequently, we have no reason for saying that the denotatum of 'the simple pendulum' fits the description given. Yet Giere's view of representation and Achinstein's accounts of unconstructed analogue models and imaginary models both seem to require description-fitting objects corresponding to descriptions of missing systems (as, on the surface, does Suppe's talk of physical systems – but see section 3.6).

It is also worth pointing out that solving the problem in this way involves committing quite seriously to the talk of propositions, too. To make an analogous manoeuvre without them would require appeal to a discredited general referential theory of meaning. Appeal to a Millian theory of reference for proper names would not do the trick for the simple reason that it is implausible to suppose that proper names are a very common component of descriptions of missing systems (even leaving aside the fact that

defenders of Millian theories have developed ways of avoiding automatic ontological commitment to a referent whenever a proper name is used precisely in order to deal with the problem of fictional names).

[61] Matters are a little more clear-cut if we focus on the second account of fictional utterances mentioned, due to Currie. Not because he claims that fictional utterances are typically false: for one thing, one might try to argue that the fictional utterances which make up descriptions of missing systems are all atypical, and for another it is not clear that he intends this to be a conceptual or otherwise necessary truth, so that one might dispute it without challenging the core of his account. The point is rather that Currie builds it into his account that fictional utterances must be accidentally true at best; given this, it seems clear that we could not come to know any truths found in a work of fiction simply by encountering them in the work, assuming that we recognise it to be a work of fiction.

[62] Perhaps there is a greater sense of hesitation surrounding the claim that we come to know that Emma was unhappy by reading Flaubert's novel than there is around the claim that we come to know that the simple pendulum moves sinusoidally by studying a classical mechanics text. But as things stand, any such sense seems too intangible to ground a rejection of the little fictions approach, especially given that there is room to wonder whether our reactions, as philosophers, might be tainted by philosophical views we already hold about fiction and existence.

[63] Another option, of course, would be to attempt a dissolution of the truth and knowledge problem by insisting that although an extra-textual utterance of the sentence 'The simple pendulum moves sinusoidally (or approximately so)' expresses just the proposition it seems on its face to express, that proposition is false, and is merely one which we choose to treat as true in various contexts. Accordingly, one would then presumably deny that we come to know the proposition in question by reading a textbook, and maintain instead that it is merely a proposition which we treat as known.

[64] It is tempting to say that Mrs. Gamp and the simple pendulum must be abstract objects on this view. Although van Inwagen says that Mrs. Gamp is not "an inhabitant of the physical world" (or at least, he says it is not "strictly true" that she is, presumably meaning to leave room for the thought that she does *hold* the property of being an inhabitant of the physical world), he does not use the term 'abstract.' One might take abstractness to rule out temporal properties, and think that both Mrs. Gamp and the simple pendulum have, for example, the temporal properties of having been created at such-and-such a time; in any case, it seems clear enough that van Inwagen is not thinking of Mrs. Gamp as being part of the spatiotemporal world we inhabit.

[65] We might even try to ground the asymmetry of the representation relation in the asymmetry of this special sort of similarity for such cases, although there is presumably no prospect of finding a general account of the asymmetry of representation here – the simple case of a still life makes it clear the representing object does not hold the properties which it represents the target as having in *all* cases of representation, or even in all cases of representation in which we might take similarity to be playing a constitutive role.

[66] Is there a problem with the claim that we are ontologically committed to a description-fitting object corresponding to *every* description of a missing system? I think not. On the view we are considering, we are ontologically committed to such an object wherever we have engaged in some critical discourse about a description of a missing system (as opposed to merely constructing the description, which we are here taking to be a little fiction). And, given that descriptions of missing systems are only introduced in order to be used in various ways, it seems quite plausible that there is indeed a bit of scientific critical discourse which carries the relevant ontological commitment surrounding every description of a missing system: "We can tackle this problem by using simple pendulum model to model the system," "The ideally rational agent is an extremely useful construct," "This model is highly idealized," etc.

[67] Perhaps one interesting question for theorists of fiction, then, is whether the property-containing view might be usefully adapted to provide a theory of fictional characters.

[68] It is also noticeable that the sentence I used as an example of a piece of critical discourse, 'Emma Bovary is one of the most finely drawn female characters in European literature', would not appear in the work itself. That is at most a clue as to whether a given utterance counts as piece of criticism or not, however, as (for example) novels can contain sentences which comment on the intricacy of their own plots.

[69] It is often noted that a story-telling can involve utterances which are plausibly regarded as straightforwardly making true claims. For example, this excerpt from Ian McEwan's *Enduring Love* (1997, pp. 6-7), whose narrator is describing his partner's research: "…all the biographies [of Keats] agree that Keats was in remission from tuberculosis when he wrote [his last known] letter, and remained so for a further ten days. He visited the Villa Borghese and strolled down the Corso. He listened with pleasure to Severn playing Haydn, he mischievously tipped his dinner out the window in protest at the quality of the cooking, and he even thought about starting a poem." It is entirely natural to suppose that these utterances of McEwan's express true claims, or at least that if they do not, then it is for reasons entirely unrelated to the fact that they occur in a work of fiction.

[70] I might also utter the sentence in question extra-textually and in fact express the bald claim that Emma Bovary was a deeply dissatisfied woman, perhaps because I am out to deceive, or because I am confused, or because I mean to insist on a particular philosophical theory of fiction. On the present account, we are free to

maintain that I would be saying something either false or truth-valueless in those cases. Cf. Lewis (1978), p. 38.

[71] The example is adapted from Lewis (1978), p. 41, although Lewis is there objecting to a somewhat different proposal about when P is true in F.

[72] I do not mean to give the impression that I take Lewis's account to be either canonical or unproblematic. Byrne (1993) presents and discusses some serious objections to Lewis's account, and to the account Currie offered in his (1990), and then goes on to develop an alternative view (one which Currie tells me he now shares, in essence). As I am about to argue that interpreting the face value practice by appealing to Lewis's account of truth in fiction will not help Giere, Achinstein, or Suppe much, I can simply suppose for the sake of argument that Lewis's account can somehow be defended against all objections.

It might be interesting to consider where some alternative account of truth in fiction might lead the little fictions approach, but it is clear at least that Byrne's account will not yield a solution to the shortfall problem (1993, p. 33). Not that this is a strike against it, of course – it is only a problem for those whose views commit them to the existence of description-fitting objects.

[73] Given Lewis's account of the truth-conditions of counterfactuals, we then get the (supposedly) desirable result that it is true in *Madame Bovary* that Emma has two nostrils, as amongst worlds in which the story is told as known fact, worlds in which the flesh and blood Emma has two nostrils will be more like the actual world than worlds in which she has three, say, or (Danniella Westbrook notwithstanding) one.

[74] Or perhaps a class of such possibilia: perhaps 'The simple pendulum experiences no friction' is semantically analogous to 'The combustion engine is not perfectly efficient' (as uttered in, say, a textbook, and not in reference to some particular combustion engine), and perhaps sentences of that sort, when uttered in that way, are being used to make claims about the members of a certain class of things. In fact, I take it that something like this must be right, but for the sake of simplicity I will leave the issue aside and continue to speak as though the phrase 'the simple pendulum' picks out a single possible object.

[75] And noting that, for present purposes, we can perfectly well employ modal notions in developing the notion of a possible object, should it prove convenient to do so.

[76] On Lewis's version of what he calls "linguistic ersatzism," a possible object is a *maximal* consistent set of predicates – or, equivalently, a maximal consistent set of open sentences (1986, pp. 148-149). I have dropped the requirement of maximality to allow the simple pendulum to have (for example) a non-zero length without having any particular length. Alternatively, we could retain the maximality requirement and take claims about "the" simple pendulum to be claims about all the maximal consistent extensions of the set of predicates employed in the textbook description. (See also n. 74.)

[77] Or take claims about "the" simple pendulum to be claims about all those objects living in other worlds which have all the properties denoted by the predicates appearing in the textbook description of the simple pendulum. This "set of otherworldly objects" manoeuvre avoids the result that the simple pendulum has some specific length and mass, is made from some particular materials, and so on. This is clearly not a problem on the first approach to elaborating the notion of a possible object if we do not include the requirement of maximality (see n. 76), and neither would it seem necessary on the second. (See also n. 74.)

Note, incidentally, that Lewis avoids use of the term 'concrete.' See 1986, pp. 81-86, for his reservations.

[78] Indeed, my argument against Giere's proposal that we take descriptions of missing systems to be straightforward descriptions of abstract objects has much in common with the third part of Lewis's argument against "pictorial ersatzism" (1986, pp. 171-174).

[79] Again, note that it might be a smarter theoretical move to take the simple pendulum to be a set of such objects instead – see nn. 74 and 77. And again, I will ignore this complication for the sake of brevity.

[80] Lewis is clearly, if somewhat idiosyncratically, using the term 'idealisation' to refer to objects such as the simple pendulum, the frictionless plane, and so on, rather than using it refer to assumptions we make, or aspects of models, or anything else.

[81] It is worth noting, too, that these various commitments would presumably be in tension with the sort of naturalism to which Giere is attracted. See Giere's (1999), ch. 4, for more on his naturalism.

[82] I am granting, for the sake of argument, that the theoretical hypotheses which relate models to actual systems in the domain of inquiry are actual.

[83] Does the standard textbook description of systems undergoing synchrotron radiation count as a description of a missing system? Not simply because it is logically inconsistent, if it is. Although logically inconsistency guarantees the absence of actual, concrete systems fitting the description from the world around us, it does not guarantee that that absence will be recognised from the outset by competent practitioners of the scientific discipline in question – an inconsistency might be deeply buried, and reveal itself only on close foundational analysis. Inconsistent or not, however, the standard textbook description of synchrotron radiation is indeed a description of a missing system, as, competent practitioners know that no real particle will be subject to a perfectly uniform external magnetic field, for example, and that no particle in a real synchrotron is free of gravitational influences.

Note also that if Vaihinger was right, then logically inconsistent descriptions of systems are commonplace in the sciences, especially the mathematical sciences. (See Fine (1993), esp. pp. 5-7.)

Presumably, however, the majority of these would not count as descriptions of missing systems in my sense, as in many of the cases Vaihinger has in mind competent practitioners do not take the descriptions in question to be inconsistent, and do take it that there are real, concrete systems in the world around us which fit them.

[84] Frisch points out (personal communication) that this argument for inconsistency could be undercut by resisting the inference to the claim that the self-fields will act on the particles. However, although there are some tricky interpretive issues lurking here, it does seem that this manoeuvre would amount to a rejection of what one could plausibly argue is the standard interpretation of the Lorentz force law, on which that law takes into account all the electromagnetic fields which have a non-zero value at the particle's location in determining the force on the particle. (A case that this *is* the standard interpretation would perhaps best be made by appealing to the details of theoretical practice in various places.) And as Frisch notes, there would be some cost in terms of physical plausibility, too, as a given particle would somehow have to be able to recognise its own fields, so to speak – an idea which is also at odds with the standard way of thinking about electromagnetism. We should note, then, that it is enough for my purpose here if the textbook description is logically inconsistent *as it is standardly interpreted*, for it is that standard interpretation which plays a role in (standard) scientific practice. (Thanks to Mathias Frisch for helpful correspondence on these issues.)

Incidentally, another example is the standard textbook description of the simple pendulum. Given the usual boundary conditions for such a system, if the description is read as asserting both that the system obeys Newton's laws of motion and that it moves in accordance with the equation cited in section 3.1, then the description is inconsistent, as it follows from Newton's laws together with the boundary conditions that the equation of motion governing the horizontal displacement should be

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

where 'θ' is the angular displacement of the pendulum from the equilibrium position. This example might be dismissed, however, on the grounds that we should read the description as asserting only that the *approximate* motion of the system is given by the standard equation (the relevant approximation being that $\cos\theta \approx 1$ for small θ). This example is also discussed in Frisch's book (2005, p. 52), although note that he focusses on the equation of motion governing the angular displacement, and is making a slightly different philosophical point.

[85] Note that the linguistic conception of possibilia as sets of predicates seems better able to take inconsistent descriptions of missing systems in its stride: we could simply drop the requirement that a possible object be a *consistent* set of predicates. Of course, we have given other reasons for thinking that the linguistic approach will not do the job Giere, Suppe, and others would like to get done.

Note also that the possibility that there are logically inconsistent descriptions of missing systems presents a further problem for the idea (discussed in section 3.1) that such passages might function as straightforward descriptions of abstract objects.

[86] Except in so far as the term 'imaginary model' refers to a set of assumptions. See section 2.1.

[87] Giere and Achinstein have the option of insisting that *when properly interpreted* their accounts are quite compatible with the nonexistence of description-fitting objects; but then until we understand how to properly interpret their accounts so as to see this, we will not really know what their accounts are. See section 4 for more on this point.

[88] Of course, the feasibility of taking description-fitting objects out of Suppe's account in this precise way does depend on the success of the project in the semantics of counterfactuals. But we should be careful in separating distinct worries on that front. If the project in the semantics of counterfactuals is not viable, that *may* mean that Suppe is stuck with description-fitting objects, in which case I would hold that his views are vulnerable to the arguments I am presenting in this paper. But he may instead be able to take on a different sort of account of modal talk than the one he envisages, one which allows him to avoid embracing description-fitting objects in some other way. In any case, the mere fact that Suppe engages in some modal talk in presenting his account, and so is committed to whatever modal talk commits us to, is not in my view a strike against it.

[89] Perhaps 'bracketing' might have been a better term than 'deferral,' as the latter suggests a recommendation that we return to the issues in question later, whereas the idea as I understand it is that *qua* philosophers of science, we can put them aside altogether. But the recommendation is merely one of deferral for philosophy as a whole, and in any case 'deferral strategy' sounds nicer.

[90] Anjan Chakravartty has also suggested to me that we should take this sort of approach to mathematical discourse in philosophical accounts of scientific representation (personal communication).

[91] Godfrey-Smith is not explicit about taking deferral to be justified by the assumption that any acceptable account of our talk about fictional entities will legitimate the sorts of things we ordinarily say, but it is hard to see how the approach he recommends could be a reasonable one without that assumption. No such legitimation assumption would be required, of course, if Godfrey-Smith were aiming only to characterise a particular way of thinking and talking that scientists engage in; but it seems clear enough that he is also

aiming at an understanding of what is happening when scientists think and talk in those ways, and that he is recommending an account which itself engages in talk of "imagined concrete things" (2006, pp. 734-735).

Incidentally, Godfrey-Smith does say that "[a]t the end of the day…some general account must be given of the imagined objects of both ordinary fiction and scientific modeling" (2006, p. 735), but given that he mentions ordinary fiction and scientific modeling in the same breath here, and given that he is only requiring that a "general" account be given "[a]t the end of the day," I still read him as endorsing the idea that we can address certain questions of immediate concern to the philosophy of science without such an account, even if (to put the point in my terms) we engage in the face value practice in the process. One of Godfrey-Smith's immediate philosophical concerns is to offer an account of one representational strategy we find employed in the sciences – that is, to offer at least a partial account of how representation works in science. Another, closely related to the first, is to say something about the nature of models. He is thus concerned with at least two of the issues I have been focussing on in this paper.

[92] See, for example, van Fraassen (1980), pp. 12-13. Note that van Fraassen speaks there only of acceptance, rather than of acceptability; by calling the claim P "acceptable" I mean just that acceptance in van Fraassen's sense is a reasonable attitude towards P. And I say "something like" the constructive empiricist's sense of 'acceptable' in order to leave open the question of whether the notion of empirical adequacy is directly applicable in the present context, or whether some other kind of adequacy might here play the role that empirical adequacy plays when it is claims about the unobservable which are up for assessment.

[93] This notion of legitimation is suggested not only by van Fraassen's constructive empiricist account of scientific discourse about unobservable objects and events, but also by the view that the best way to make sense of mathematical discourse might be to see it as aimed at something other than "true story-telling," a view van Fraassen describes himself as "sympathetic to" in the *Harvard Review of Philosophy* interview cited above (2005, pp. 96-97). The notion of legitimation just set out clearly goes beyond the latter sort of view, however: saying that the aim of a kind of discourse is something other than true story-telling is compatible with saying that the utterances which make it up uniformly express false or truth-valueless claims, or fail to express claims at all (and indeed, it is even compatible with saying that they express knowable truths).

[94] Note that in this case it would be misleading to say that simply that we do not have a justification for the deferral strategy, as though an appropriate response would be to continue to look for a justification, or to drop the deferral strategy in favour of investigating the semantics and ontology of the face value practice right away. Rather, in this case we ought simply to drop the face value practice itself, for the purposes of the philosophical tasks at hand. (This is not to say that we would have reason to eschew the face value practice in every context – perhaps it would be fine to go on engaging in it at the right moments in our scientific work, for example.)

As I noted earlier, some may reject the metaphilosophical assumption that we are aiming to know the truth in constructing philosophical accounts of the nature of models, or theory structure, or idealization. (Rosen (1994, esp. pp. 151-152) lays out a reading of van Fraassen on which he is not aiming at the truth in presenting constructive empiricism; see van Fraassen (1994) for his reaction.) I am not sure I know how to defend that assumption. One might appeal to the distinct idea that we are aiming to understand scientific representation, say, by constructing an account of it, and then try to argue that an account cannot yield understanding if it rests on claims which are false, or unknowable, so that aiming to know the truth is part of aiming at understanding. But this is to take on contentious issues, too. Van Fraassen, for example, clearly thinks that unknowable claims can explain, as he thinks that claims about the unobservable can explain, but are unknowable (1980, ch. 5); it would not be surprising, then, if he were to insist that unknowable claims can produce understanding. And Frisch (1998) argues that the best explanations are sometimes false. (Of course, Cartwright has argued that explanation often involves false law statements, too (e.g., 1983, essay 2), but as I read her, she has gone on to reframe her view. As she conceives it in her later work, the point is just that if the law statements we appeal to in many of our explanations are read in the traditional way, as purportedly exceptionless generalizations about what happens, then they are false. We should, however, read them differently, as telling us about the capacities that various properties carry with them, and so read, law statements face no special obstacle to truth.)

[95] That is, although given this sort of legitimation there is no special problem with part (i) of the idea we are examining (that we can engage in the face value practice in constructing accounts of representation, the nature of models, theory structure, and so on), I want to argue that there is still a problem with part (ii) (that in doing so, we need not adopt any particular account of the semantics or ontology of that practice).

[96] The point can be put a little more rigorously if we are willing to grant a certain assumption about meaning and linguistic understanding, as follows: If there is such a thing as the simple pendulum – an object which fits the description of the simple pendulum – then it surely features in the truth conditions of (S), which will have to do with that object's standing in certain relations to scientists and real pendula. If there is no such thing as the simple pendulum, on the other hand, then clearly no such thing features in the truth conditions of the sentence. Thus if we do not know whether there is such a thing as the simple pendulum, we cannot know whether the truth conditions of sentence (S) involve such a thing; and so we cannot know the truth conditions of the sentence. Given the right assumptions about meaning and linguistic understanding, it follows from this that we do not really know what (S) means, and that we do not understand it.

Consequently we do not understand the account of representation we have been offered, and so cannot come to understand representation itself by considering that account. Thus deferral of the existence question is not an option.

[97] 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.

[98] I have in mind particularly the work of Kripke (unpublished), Currie (1990), ch. 4, Salmon (1998), and Thomasson (1999). Thanks to Gabriele Contessa for my drawing my attention to the work of Kripke and Salmon.

[99] This idea should not be conflated with Giere's proposal that (to put it in my terms) descriptions of missing systems stand in a relation of "characterization, or even definition" to models, and so are true of them by definition (1988, p. 79). The clear difference is that there is a description-fitting object on Giere's proposal.

[100] As I mentioned in n. 48, 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 (but under the old title) at two conferences. I would like to thank 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 Jim Brown, Anjan Chakravartty, Gabriele Contessa, Greg Currie, Mathias Frisch, Greg Radick, Kyle Stanford, William Taschek, Kate Thomson-Jones, and my departmental colleagues at Oberlin (to whom the older version was presented at a meeting of our Faculty Research Seminar) for very helpful discussion and correspondence.