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TWILIGHT OF THE PERFECT MODEL MODEL

1. THE PERFECT MODEL MODEL

Contemporary thinking about science remains in the grip of the long standing ideal of exact natural laws. This ideal fits with a second dogma. In the 19th century scientists became acutely aware that humans introduce error in making observations. Not just random error. Also systematic error. For example, pairs of trained astronomers of impeccable credentials were sometimes found to differ systematically in their observations, despite their best efforts. The solution to this distressing situation seemed to be to get people out of the observation-making business as much as possible. Efforts were made to mechanize observation. One can imagine what a stir the discovery of photography must have caused![1]

The photograph provides a good icon: The ambition has been to produce a perfect likeness of nature, a perfect model. The natural law ideal can be seen as the theoretical side of this more general enterprise, complemented by efforts to get untainted observations. Of course characterizing our efforts to describe nature as aimed at producing a perfect model is itself a model of the human knowledge-gathering enterprise. Hence we may call it the Perfect Model Model.

2. AN ALTERNATIVE TO THE PERFECT MODEL MODEL

The metaphor of the Perfect Model Model is, to be sure, an exaggerated representation, intended to call to mind an attitude realized by various combinations of more specific slogans such as ‘(exact, exceptionless) natural laws’, ‘comprehensive theories’, ‘reduction’, and ‘convergent realism’. Today the many careful advocates of such views acknowledge the need for refinement. But a few of us take three reasons for caution to indicate that the Perfect Model Model should be scrapped entirely. First, nature may comprise no ultimate refinement of structure, encodable in finitely statable natural laws. Second, even supposing wonderfully simple and universal laws, including a complete description of forces and dynamics, such laws

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would not by themselves provide a theory we could much use, for the initial conditions are far too messy. While most recognize this fact, few have really thought through the repercussions. Think in terms of a Newtonian world as envisioned by Laplace. Matter is scattered around the universe far too irregularly to permit us exactly to apply the simple laws of a Newtonian universe. Laplace already clearly recognized this limitation:

> Such perfection as the human mind has been able to give to astronomy affords but a feeble sketch of [an imagined intelligence capable of exact application of Newton's Laws to the whole world] .... All our efforts in our search for truth tend, without respite, to approximate the intelligence we have imagined, *but our efforts will always fall infinitely short of this mark*. (1812, pp. 2–3, emphasis added)

A third reservation, which refines the second, is that the messiness of initial conditions, theoretical virtues such as simplicity, and other such constraints on theorizing are really matters which are relative to our intellectual capacities. Relatively speaking, we clearly have too little candle power. To have theories which we can actually apply in describing and understanding the world we have no choice but to work with nature to do what it does not sufficiently do by itself: We must simplify further.

Indeed, simplifying is just what physics and most other sciences systematically do, a fact about science until recently largely neglected as the following personal anecdote illustrates. In 1974 I read through all of Feynman's *Lectures on Physics* (1963). I was flabbergasted. Almost nowhere could I find deductions from first principles. Most of the work involved ingenious tailoring of both the laws and the facts to produce accounts which, at the same time, fit the world well enough but were also sufficiently simple for us to manage. Trained to think of physics in terms of the natural law stereotype as adumbrated by the positivist tradition, I could make nothing of the method and relegated my observations to the anomaly file.

In the last two decades a few have begun to sketch a framework for understanding what baffled me in 1974. The relevant prehistory resides in the Beth-Suppes-Suppe-van Fraassen so-called 'semantic' view of theories.[2] These authors held that characterization of a theory in terms of language obscured rather than illuminated the content of the theory because what clearly counts as 'the same theory' admits of various, even inequivalent, linguistic formulations.[3] The solution was to characterize theories as sets of models, the same set whatever the language used to specify them. Models were generally taken to be abstract mathematical structures, in the sense developed in formal logic and set theory. Theories, embodied as models, were taken to apply to the world by the relation of isomorphism between models and (parts of) the world.[4]

None of this addressed the phenomenon of approximations and idealizations which so bothered me in 1974. The new turn began, so far as I know, with Giere's 1979 *Understanding Scientific Reasoning*.[5] Chapter 5 stipulated that 'A scientific theory is a definition of a kind of natural system.' For example, one might define a 'Newtonian particle system' as '... a system of objects satisfying Newton's three laws of motion and the law of universal gravitation.' Such theories-cum-definitions themselves say nothing about the real world (p. 69). Empirical content is provided by 'theoretical hypotheses' which have the form: 'Such and such real system is a system of the type defined by the theory' (p. 70).

In 1980 and 1981, John Beatty published two papers (Beatty 1980, 1981), explicitly citing Giere's 1979, which used population biology to illustrate Giere approach. In the next few years a number of other philosophers of biology also applied this kind of approach to understanding theory in population biology.[6]

In 1983 Cartwright's *How the Laws of Physics Lie*, not written in the tradition of the semantic view, took a different approach to illuminating the problems of the misdescription of physics by the philosophy of science. This book focused on problems of approximation and idealization, and addressed them with reference to 'prepared descriptions' (e.g., pp. 15, 133 ff., 147), 'Physics as Theater' (pp. 139–42), models as 'caricatures' (p. 150), 'works of fiction' (p. 153), 'simulacra' (pp. 17, 143 ff.) and lies. Physics works not by deduction from first principles but by carefully prepared descriptions or models which purchase one or another limited kind of generality, but always at the price of sacrificing accuracy.

In my own thinking, the issues were brought into much better focus by the third chapter of Giere's *Explaining Science* (Giere 1988a, also well presented in his 1985). This work made clear what was not clearly specified in his (1979), that models correspond to the world not by a relation of isomorphism but by a looser relation of similarity. An outline of Giere's 1988 formulation will provide a kind of 'canonical' presentation of what I will refer to as the 'model view'. In the first instance science produces models, which are sometimes concrete physical objects, but which in most cases of interest are abstract objects. Models are connected to the world by theoretical hypotheses, sometimes more, sometimes less explicitly stated. Crucially, Giere takes each theoretical hypothesis to characterize some specific system, or some kind of system, as similar to a model, but only in limited respects and even in those respects only to a limited degree of accuracy. Laws function as central, but not exclusive, means of defining models, so that laws are true by definition of the models they serve to characterize.[7] Laws connect with the world only indirectly, through the theoretical hypotheses, in which all the relevant empirical content resides. Theories are loose collections of models, organized inexactly in terms of the laws, methods, and other aspects which one uses to characterize models. One can also appeal here to the analogy of thinking of theories as 'model-building tool kits'.[8]

I want here to make a terminological specification to mark a crucial distinction. Most of those who have endorsed what they call the *semantic view* of theories have taken theories to be collections of models which are supposed to represent the world by some kind of isomorphism. Whatever others may have meant by 'semantic view', by the term *model view* as I will use it, I will mean a view which takes models to represent by a relation, not of exact isomorphism, but of similarity, always in limited respects and degrees.[9] By a *model view* I will also mean one which takes theories to be imprecisely characterized collections of models or of model building guidelines. One theory will include whatever one naturally builds with a given 'model building tool kit', and theories can overlap as when we compose 'semi-classical approximations.'

The foregoing constitutes no more than a brief summary of what itself must be acknowledged to be but the barest initial sketch of a very general program. Many have thought that this program is a non-starter because it founders on (at least) three general problems. (1) It looks to be very hard to say what is going to count as a model. (2) The prospects for saying what is going to count as 'similarity' appear to be quite hopeless and (3) The model account promises to be false to science's dramatic successes in uncovering and unifying the hidden wellsprings of manifest phenomena. Having sketched the basic ideas and their motivation, I want now to address these three worries.

3. WHAT ARE MODELS?

The worry is not so pressing with physical, and especially, scale models. One understands, at least in outline, how science makes good use of engineers' aircraft models in wind tunnels, analog computing devices, and the like, all of which do good service in both prediction and explanation, especially in technological applications. When we specify 'scale models', the problem of similarity also dissolves: To specify the scale is just to specify the operative similarity. But in the theoretical sciences, for the most part, one does not have physical models in mind.

As mentioned, Cartwright talks in passing of such things as idealizations, prepared descriptions, physics as theater, caricature, works of fiction, and simulacra, but gives no uniform account.[10] Giere takes most models of interest to be non-linguistic abstract objects (e.g., 1988, p. 78). He illustrates with the example of the harmonic oscillator model as presented in any introductory physics text but does little to further clarify.

I take the stand that, in principle, anything can be a model, and that what makes a thing a model is the fact that it is regarded or used as a representation of something by the model users. Thus in saying what a model is the weight is shifted to the problem of understanding the nature of representation.

I do not begin to have a workable account of representation, so what is accomplished by this move? The point is that when people demand a general account of models, an account which will tell us when something is a model, their demand can be heard as a demand for those intrinsic features of an object which make it a model. But there are no such features. WE make something into a model by determining to use it to represent. Once this is fully appreciated it becomes clear that we can get on with the project on the strength of a good supply of clear cases of things which are used to represent. These will adequately support study the variety of such uses, the way they function in the scientific enterprise, their interrelations, and so on.

If one is still in doubt about this attitude, consider the following analogy. The received view of theories takes theories to be sets of (meaningful) sentences. Suppose I were to raise a fuss that this account is unintelligible until I am given a general account of what counts as a sentence. I am not satisfied by being given examples, or even examples of languages which specify rules for what will count as a sentence in that language, just as the model critic was not satisfied by examples such as a Newtonian model of the solar system or a very general class of models of n-particle systems modeled with a 6n dimensional phase space. In the analogy the problem is likewise that a wide range of things can be used as sentences. What makes something into a sentence are the rules we compose and accept, and what makes such sentences meaningful is our determination to press them into certain sorts of representational use. We have no general account of languages or how they come to represent. But we understand central cases quite clearly enough, thank you, to get on with the project. It's the same with models.

What then is the difference between models and sentences? I need not be committed to any absolutely sharp distinction – indeed, certain accounts of propositions pose a possibility of a substantial middle ground of linguistic and modeling approaches. And having said that *anything* can be a model when pressed into representational service I am not now going to turn around and exclude sentences! But I will follow Giere (and I think what is also Cartwright's tacit position) that in a great many cases models are abstract objects which have been pressed into the service of representations which work by various kinds of similarity, such as similarity of structure, possessed properties, or functional role. I propose to take a lead from recent work in cognitive psychology which suggests that much of human representation works, in the first instance, by agreement in form; and that linguistic representation works by attachment of arbitrary symbols to the primary morphic (as I will call them) representations (e.g., Cummins 1995; Churchland 1995; and Churchland and Sejnowski 1992). An abundance of examples already cited by Cartwright, Giere, and many others show that in a great many instances representation in science makes essential use of morphic representations which function as an essential intermediary between linguistic description and the objects of representation.[11] The next section will illustrate with a concrete example.

In fact it would be a mistake for a general account of the use of models in science to specify more narrowly what can function as a model. Science uses many things as models, such as ordinary functions, phase spaces, vector spaces, fiber bundles, groups, structures (in the sense of formal logic) and much other abstracta, as well as physical models. It would misdescribe science to rule any of these out, and it would be foolish a priorism to rule out in advance possible novel applications.

Having included sentences, statements, and propositions among the things which can function as models, one might ask what the point is of broadening the domain of what we should include as models. In addition to the points suggested in the last two paragraphs, this is a good occasion to emphasize that what I am calling the model view includes two components. First, that we should take science to be in the business of providing not just descriptive sentences, statements or propositions, but a much wider range of abstracta which represent by agreement in form. Second, that such representation, whether by description or by form, is rarely, if ever, exact. My interest in the present paper is actually much more the second than the first component. The interest in emphasizing extra-linguistic models is that, in addition to what I believe is accuracy in describing what science actually does, the emphasis will enable us to see the second point much more clearly. Once we have cleared away the worries about similarity in Section 4 we will see in Section 5 that the conclusions easily transcribe to remove widespread worries about approximate truth, thereby removing the obstacle to seeing the point about inexact representation for the special case of representation by linguistic entities. Once these worries are assuaged, I am hopeful that the extensive literature by Cartwright, Giere and others will immediately apply to show that the model view provides the best available account of the intellectual product of science as actually practiced. The last section will additionally indicate why I believe that model view also provides our best account of the aims of science.

4. QUESTIONS ABOUT SIMILARITY

Three problems have been posed for modelers about their appeal to similarity. Two concern purported problems of application, and a third demands a general characterization of similarity before any appeal is made to it.

When models are taken to be abstract, there looks to be a particularly intractable problem with similarity: In what way can an abstract object be similar to a concrete object? Take this triangular shaped paper cutout on my desk, and the, or some, triangle, understood as an abstract object. One is spatio-temporally located, the other is not. So there is no 'putting one up next to the other' for a similarity comparison.[12] Or, for a somewhat different example, the pendulum or other device in a harmonic oscillator model is said to move with a specific period and amplitude, and to obey an equation of motion. But abstract objects, not being located in physical space, can't move, much less have motions governed by an equation of motion.[13] Critics reject as unintelligible any talk of similarity between concrete physical systems and models understood as abstract objects.

We can sidestep these apparent problems by considering the properties which a specific concrete system in fact instantiates. 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 ones 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. Nominalists (who won't have the problem to begin with) can play along by applying their translation schemes to the forgoing formula. For example they could refer to whatever brain structures might be thought to be doing the work which Platonists assign to abstract objects.

Still, why bother? Savage (to appear) argues that, since most comparisons require the use of language, any appeal to non- linguistic models would be an idle detour. How will one compare a triangular paper cut-out with a model constituted by some abstract triangle? To specify the model one must, says Savage, use the definition of a triangle, so that the model, by definition, has three vertices. To compare the cutout with the model, say for number of vertices, requires counting the vertices on the cutout, formulating the result in a statement, and then comparing this statement with the definition characterizing the model. Linguistic entities are doing all the work. This conclusion in turn suggests that what one is ultimately comparing are linguistic entities, in this case the description of the paper cutout and the definition of a triangle.

To see what has gone wrong in this line of thinking, consider a slightly more robust example. Consider a physical system, such as the pendulum in Giere's grandfather clock, and the varying values of some physical quantity of interest, such as the pendulum's angle of deflection. Call this sequence of values over time the 'actual course of values'. One models the system using an abstract object specified, to be sure, linguistically, using a formula to specify a function, say $x(t) = A\sin(\omega t)$. On interpreting (values of) t as (corresponding to values of) time and (values of) x as (corresponding to values of) the physical quantity in question, one can make comparisons of similarity between the function, so interpreted, which is part of the model, and the actual course of values.

Now, what role, in this example, does language play? Language functions to pick out the relevant function (as well as to fix interpretation of the variables). But it is the function – the course of values in the model, *not* the formula picking out the function – which gets compared with the actual course of values of the physical system. In particular, the fact that language is used to indicate which function (construed, say, as a set of ordered pairs of values) constitutes (part of) the model does not show that linguistic entities are the objects of comparisons. Such a suggestion would make the mistake of confusing language with what language is used to describe.

One might think that I must have missed Savage's point because the point was supposed to be epistemic, while I claim to have uncovered a use-mention confusion. But this is just right. In the triangle example Savage claimed that comparison requires counting the vertices of the triangle, formulating the results of our observations in a statement, 'The paper cutout has three vertices', and then comparing this statement with the definition of the model. Savage concluded that linguistic entities are doing all the work. Agreed that in this example, and many others, linguistic entities function to do the work. But let's not confuse the work done – the process – with the work accomplished – the product. If I use a hammer to build a house, there is a perfectly good sense in which the hammer is 'doing all the work'. But let's not confuse the hammer with the house! In our present case, linguistic entities are 'doing all the work' to accomplish a comparison between a model and a represented object. That we use linguistic objects to accomplish this comparison does not eliminate either term of the comparison. That we use language to compare the paper cutout with something does not show that what we compare it with is the *definition* of a triangle.

In fact, in the foregoing and many other cases, language in a strictly verbal sense is inessential to the comparison which can be made instead with a morphic graph of both the function and the actual course of values.

Let me turn to the third and overarching purported problem about similarity. According to modelers, science produces models that are, always, no better than similar to modeled systems, with the (relevant) similarity always limited both in respects and in degree. Critics demand to be told what similarity is. Presumably the problem is that any two objects will be similar in countless ways. What respects are relevant? And given a respect, what degree is called for? Since the prospects for a general account answering such questions seems plainly hopeless, the model approach is summarily rejected.

This problem dissolves as soon as we notice that the demand for a general account of similarity can't be met because what is going to count as a relevant similarity depends on the details of the case at hand. No general account is needed precisely because it is the specifics of any case at hand which provide the basis for saying what counts as relevant similarity. In other words, the very facts which make this demand impossible to meet also show that the demand was misguided to begin with.

To see how this works, let's glance at an example. Suppose that one is interested in explaining the flow of water and wave propagation; or, alternatively, in explaining diffusion, say, of a drop of ink in a glass of water. Then one will model water, respectively, as a continuous incompressible medium or as a collection of discrete particles in thermal motion. Each model is similar to water in radically different respects. If one is interested only in a qualitative explanation of flow or of diffusion, inexact similarities will do. Furthermore, if the aim is prediction or explanation of quantitative detail one will need to specify the interests of the model users in more detail. Is one concerned only with water waves of length on the order of a meter or more? Then (an analog for) surface tension forces may be left out of the model. Not so if ripples on the order of a millimeter in length are at issue. For diffusion models, at what temperatures and pressures are numbers required? And how accurate do those numbers need to be? Answers to these questions about interests and intended applications must be provided before one can specify how accurately one needs to include representations of intermolecular forces in a diffusion model.

When it comes specifically to prediction one will also need to specify the respects in which accuracy is required. Accuracy of prediction can fail in a variety of ways, in ways with different repercussions in different problem situations; and one needs to specify, at least qualitatively, a 'cost function' which indicates how much different errors will hurt. In particular, when errors can compound, error bounds, which are reliable for models individually, may fail to be reliable when models are amalgamated. Whether or not one can live with the resulting weaker error estimates will depend on the needs of the case. One can determine both what new error bounds apply and whether one can tolerate these new bounds only from a specification of the case at hand, including specification of intended use of the model.[14]

In short, once the relevant context has been specified, for example by saying what is to be explained or predicted and how much damage will result from what kinds of error, the needs of the case will provide the required basis for determining what kind of similarity is correctly demanded for the case at hand. More specifically, similarity involves both agreement and difference of properties, and only the needs of the case at hand will determine whether the agreement is sufficient and the differences tolerable in view of those needs. There can be no general account of similarity, but there is also no need for a general account because the details of any case will provide the information which will establish just what should count as relevant similarity in that case. There is no general problem of similarity, just many specific problems, and no general reason why any of the specific problems need be intractable.[15]

5. APPROXIMATE TRUTH

Once we have understood the context dependent functioning of similarity in the model account, the relevant considerations apply immediately to clarify a widespread misapprehension about the nature of approximate truth. Indeed, the issues are largely the same, dressed in slightly different verbal presentations. Here the my approach largely recapitulates Peter Smith's (1999), but I hope that coming to these views via consideration of similarity will provide a useful alternative presentation.

Many have held that there is no coherent notion of approximate truth, and to show the plausibility of this skepticism, let me review how it manifested itself in one chapter of the realist-antirealist debate. We turn back the clocks two or three decades, to a time when everyone presupposed that science provides theories, in turn composed of sentences. In this context a simple approach to saying what one meant by scientific realism was to appeal to truth: Science aims to provide literally true descriptions of the world. In particular to say that the posits of scientific theories are real is just to say that the existential statements appearing in scientific theories are true. To be a realist about atoms was simply to take the 'atoms exist' of atomic theory to be, literally, true.

Antirealists objected that, historically, theories have generally turned out to be false, and that no one believes that any of our important theories today will stand exactly as stated. Well, responded the realists, we'll make do with a relation of approximate truth. The antirealists then demanded an account of this relation. While realists were at a loss to say what would work generally, they argued that at least for quantitative statements one knows where to look. A false statement is approximately true if it can be transformed into a literally true statement by putting in error bounds. The pendulum law is approximately true because $T = 2\pi\sqrt{(l/g)} \pm \epsilon$ is exactly true, for suitably chosen ϵ.[16]

This stratagem was then said to succumb to the conjunction problem (e.g., Fine 1986, pp. 120–1; van Fraassen 1980, pp. 83–7). Given two statements which are approximately true in the error bounds sense, their conjunction need not be approximately true, so that approximate truth cannot serve as a surrogate for truth. In particular, if we have reason to believe that two sentences are approximately true, we will not, in general, have reason to believe that their conjunction is approximately true. The conjunction problem is in fact just a special case of a much broader problem: What error bounds will count as making a statement approximately true? How do we proceed in the many cases in which there is not even a vague notion of 'error bounds' on which to fall back? Many concluded that trying to give a general account of approximate truth is plainly a hopeless enterprise.[17]

Let us see how this issue looks when we bear in mind the lessons learned from considerations about similarity, especially bearing in mind that language is often used to describe both models and real systems and that these, not bits of language, are ultimately the things which are up for comparison. The antirealists' challenge was: From among all the false statements, which of these describe situations which are 'close enough' to the situation described by a true statement to warrant the title of 'approximately true'? This is just to ask, which described non-actual situations count as relevantly similar to the actual situation? The problem is one of identifying relevant similarities between what we describe with statements, not, at least not in the first instance, a problem of identifying a relation between statements. The problem is to identify the relevant similarity between situations, on the one hand the actual situation, and on the other some non-actual idealized simplification of the way the world really is, what is being called a model.

We are called upon to compare actual and idealized situations. Of course our access to these situations is often linguistic. We then make the use-mention slip which generated the second worry about similarity and substitute descriptions of what we are trying to compare for the objects of descriptions themselves, the situations up for comparison. The conviction that theories are collections of sentences or statements steers us straight into this pitfall. Thinking instead of theories as collections of models helps us see that talk about approximate truth comes down to the same issues as those covered by talk of the similarity between models and their objects of representation. And when the common issue is put in terms of similarity, we are quickly led to appreciate that the matter is dependent on interests and many other aspects of the immediate context. The demand for a general and context independent account of approximate truth was a bit of wormy bait which realists swallowed, misled, I suggest, because the issue was put in terms of sentences or statements instead of in terms of what these linguistic entities serve to describe.[18]

Smith's (1999) takes a similar approach to approximate truth. While there are many points of difference in detail, let me provide a brief summary emphasizing the points of agreement. Smith considers what he calls 'geometric modeling theories', each of which has two components (pp. 258–9): A geometrical structure, construed very broadly, and the claim that this abstract structure approximately replicates the geometric structure to be found in some real-world phenomena.[19] Note that in calling for a comparison of geometrical structures – the one in an idealized model, the other exhibited by a real system – Smith is doing something very similar to the above call for making similarity comparisons by comparing the properties or quantities which a real system HAS with those which are PART of the model. Since in Smith's treatment the comparison is between geometrical structures, many of the candidates for the relation of 'approximate replication' can be given exact characterizations. Finally, '... elucidating the claim [that a certain geometrical modeling theory is approximately true] will just require spelling out what it is for an appropriate geometrical 'closeness' relation to hold between structures of the relevant kinds' (p. 259). Clearly Smith intends that WHICH relation is appropriate – which counts as approximate replication – is to be determined by contextual fixed interests: '... a theory is approximately true if the world exhibits a relevant structure sufficiently similar to the abstract structures specified by the theory (though which similarities to weight will, no doubt, be interest relative) (p. 264).[20]

Smith also applies his analysis to the so called 'Miller problem' for approximate truth. Miller (1974) shows that for a wide range of extant formal accounts of verisimilitude the accounts give inconsistent results when applied to different theory formulations. Suppose we have sentences, A and B, in a first language L, and A′ and B′ in a second language L', where L' is obtained from L by a simple change of primitives or variables. A is logically equivalent to A′ and B to B′. But there are cases in which proposed measures of verisimilitude make A closer to the truth than B, but B′ closer than A′.

Smith observes that the problem dissolves if we '[r]ecall … that what makes for approximate truth is interest-relative.' (p. 271). In reworking some prior examples, Smith shows that the sensible thing to say is that A (aka A′) is closer to the truth than B (aka B′) in certain respects, while B (B′) is closer in others, where the language sensitive measure gives different responses precisely because the variations in language correspond exactly to the variations in relevant respects in which statements can be 'close to the truth'. There is no closeness to the truth simpliciter.

Smith considers quantitative examples. The point can likewise be made with a very simple example which readers of Miller will recognize as mimicking his infamous weather example. Suppose we use tokens in Coke vending machines. The tokens can be (a) 1 or 2 oz., (b) square or round, and (c) red or green. These properties are taken to be exclusive and exhaustive characteristics of the tokens.

Consider the following competing descriptions, we will call them 'theories', of some specific token:

> Theory 1: The token is 1 oz., round, and green. Equivalently we can say that the token is 1 oz, that it is 2 oz. iff square, and that it is 2 oz. iff red.[21]
>
> Theory 2: 1 oz., square, and red; also given by 1 oz. and not (2 oz. iff square) and not (2 oz. iff red).
>
> Theory 3 (The true theory): 2 oz., square, and red; also given by 2 oz. and 2 oz. iff square and 2 oz. iff red.

Now suppose that in California the vending machines are set so that a token will get me a Coke just in case the token is square and red. In New York the machines are set so that I will get a Coke just in case the token is 2 oz. iff square and 2 oz. iff red. In sum:

> California Token: square and red (1 or 2 oz.)
>
> New York Token: 2 oz. iff square and 2 oz. iff red (1 or 2 oz.)

So in fact my token, unbeknownst to me correctly described by theory 3, will get me a Coke in both California and in New York. If I hold theory 2 as the correct description of my token I will be led to believe that my token will work in California but not in New York. If I hold theory 1 I will believe that my token will work in New York but not in California. I submit that we have here a clear and natural sense in which theory 1 is closer to the truth than theory 2 when our interests are getting a Coke in New York, while theory 2 is closer to the truth than theory 1 when our interests are getting a Coke in California. Another way of putting this: Consider three tokens, #1 described by theory 1, #2 by theory 2 and #3 by theory 3. Tokens #1 and #2 are similar to token #3, but in different ways. #1 is similar to #3 with respect to getting a Coke in New York, and #2 is similar to #3 with respect to getting a Coke in California. Now, if we MISdescribe token #3 as token #1 or #2, which description is closer to a 'correct' description? Well, one in one way, the other in the other, just as with the similarity of the three tokens.

Theory 1, as a description of my token, gets right that part of the 'truth' about my token which pertains to getting a Coke in New York. Theory 2 gets right that part of the 'truth' about my token which pertains to getting a Coke in California. And all of that is perfectly objective. Miller is right, as his examples show, that there is no such thing as relative closeness to the truth simpliciter. But the foregoing example illustrates how there is a perfectly clear, and objective, notion of closeness to the truth relative to various aspects of a situation.

6. THE PTOLEMY PROBLEM AND THE PROBLEM OF UNIFICATION

Smith restricts his analysis of approximate truth to the special case in which comparisons are between geometrically specifiable structures. This appears to lead to a problem, for if we restrict attention to comparisons among geometrically specifiable structures, a Newtonian model of the solar system with some slightly inaccurate parameters may do worse than a Ptolemaic model as theories of observed planetary motion. Yet surely we want to say that the Newtonian model, even with the somewhat incorrect parameters is 'closer to the truth' than the Ptolemaic model.

Smith's very brief comment on this issue (p. 274) is to note that if one's interests are, say, purely navigational then the Ptolemaic theory will count as the closer to the truth. But as scientists we ordinarily also have interests in explanation, and if we take an approach to explanation in terms of unification, the Newtonian model will come out far ahead in spite of the faulty parameters because of its close connection with a host of other successful Newtonian models.

I would like to expand, both on Smith's statement of the problem and on how modelers can sensibly deal with it.

Stated more generally, how will modelers distinguish between fatally flawed models such as that of the Ptolemaic, phlogiston, and caloric theories, and models, such as the Newtonian model of the solar system and current atomic theory, which we regard as basically sound even if they may not get all the details exactly right? There are really two, interconnected problems here. Some appear to think that a model is to be evaluated only with respect to its accuracy in modeling the phenomena and that, consequently, the model view appears to be stuck with treating on a par all factitious and correct aspects which go beyond the phenomena. Modelers are claimed to have neither basis for nor even any reason to attempt to sort out the idealizations and convenient fictions from those realistic aspects of models which go beyond the phenomena. Second, the model view appears to be committed to settling for structures that only represent the phenomena rather than searching for underlying mechanisms and unifying accounts.

These impressions of the model view have perhaps been encouraged by some things some modelers have said. Van Fraassen rejects belief in those parts of a model which go beyond the empirical phenomena or empirical substructure. Cartwright insists that in many situations there are no facts of the matter to be more faithfully reflected by our models, so that striving for greater details in such cases will be doomed to failure. In particular, she has argued that in complex dynamical situations there is no reason to think that there are forces to be described which will give a full account of the motion of something such as a thousand dollar bill flapping about in the wind in St. Stephen's Square (1994, pp. 283–5; 1995, p. 360) and that there is not, in general, any fact of the matter about how forces compose (1983, pp. 59 ff.).

I reject any fixed and context independent distinction between (observable) phenomena and facts or characterizations of a hypothetical 'unobservable' theoretical domain. All our access is indirect, so that there are only differences in degree, but not in principle, between our epistemic access to apples and to atoms. Those who agree with me on this point will not see the model program as subject to the first, Ptolemy problem. Since there is no (context independent) distinction to be made between observable and theoretical phenomena, any aspect of a model can be assessed for its accuracy. In particular, modelers can make perfectly good sense of looking for 'hidden mechanisms', characterizing these mechanisms with models, and then evaluating the models with respect to their accuracy of characterization of various aspects, access to which is sometimes more and sometimes less direct. This last point then also applies in addressing the second problem. There is nothing in the modeling program which prohibits or discourages efforts to find and model 'hidden mechanisms' or other ways in which disparate phenomena that are relativley close to immediate observation fit together or are manifestations of something more basic or general. It's no accident that the 'standard model' of the structure of matter is so called.

However, this approach to the issue of unification raises another potential difficulty: If the modeling program encompasses the search for basic models which unify as wide a range of phenomena as possible, are we now slipping back to the Perfect Model Model of the aims of science? I think that some attention to a sensible understanding of explanation will show that we can make good sense of the search for general and unifying models while rejecting the Perfect Model Model.

Many readers will have responded to the earlier water/water example by saying: Of course an incompressible continuous medium model works well to explain fluid flow and wave motion, while a thermal particle model works well to explain diffusion. But both these models are stop-gap idealization. A real understanding of all these phenomena comes only from quantum mechanics which, along the way, explains why the two more superficial models work as well as they do.

Quantum mechanics itself is, of course, just one more family of models, each of which is limited in scope and accuracy. But even if quantum mechanical models were exactly correct wherever they applied, the foregoing statement is misleading. The quantum mechanical explanations of water flow, waves, and diffusion are not reductive accounts; that is, they are not deductions of flow, wave, and diffusion phenomena from quantum mechanical principles. Instead, general considerations from quantum mechanics allow us to see how the inaccuracies of the more superficial models do not undermine the accurate description of certain limited phenomena. In so doing these arguments allow us to see how the admittedly inaccurate models still correctly get at certain aspects of the way things work. But such quantum mechanical arguments do not, generally, themselves count as explanations of the sort of facts about water flow, waves, and diffusion that we get from the continuous medium and thermal particle models.

To get a description of water flow, waves, or diffusion directly out of quantum mechanics we would have to solve a Schroedinger equation with on the order of 10^{25} variables. Even if we got a computer to solve such an equation for us, the results would give no humanly accessible understanding of such phenomena. And if explanations are required to provide humanly accessible understanding, such calculations, even if they were possible, would not count as explanations. Generally, we can take more basic models to explain (the accurate function of) more superficial models, which in turn explain more readily accessible phenomena. But the relation of explanation is not generally transitive, and the example with the 10^{25} variables suggests that transitivity will often fail when it comes to applying models of microstructure to models of the macroscopic.

The point here presupposes that the world is much more complicated than what the human mind can encompass at one go, but otherwise the point has nothing to do with the 'real structure of the world'. The point concerns only human cognitive limitations, limitations which are certainly not fixed, but which may plausibly be expected always to fall 'infinitely short of [the] mark' of the Laplacian superintelligence's comprehensive cognition. Science has produced models of astonishing accuracy and generality, such as quantum mechanics, the standard model, and general relativity. These 'more basic' models unquestionably function to help us understand why more superficial and more highly idealized models work as well as they do. But the more basic models do not thereby do the explanatory work of the more idealized ones.

In Putnam's old example (1975, pp. 295–8) if you want to understand why the round peg won't go through the square hole, solving a Schroedinger equation will never help. Instead you need to appeal to the approximate rigidity of the peg and the board and some basic facts of physical geometry. Quantum mechanics will help you, in a very rough and qualitative way, in understanding why those materials are approximately rigid. But it would be at least misleading to say that this explanation itself is part of the explanation of why the round peg won't go through the square hole. The explanation of an assumption of an explanation isn't thereby automatically part of the original explanation. One might argue about that, with danger that the dispute could turn terminological. What matters here is the confluence of two conflicting constraints. On the one hand, in cases like Putnam's, the water/water case, and countless others, human understanding absolutely requires the more highly idealized models. But on the other hand, one can't literally put together – unify – the underlying 'unifying' model with the higher level 'unified' models. They don't combine into one coherent picture. Water is not, literally, both quantum mechanical and fluid mechanical, and no one consistent model can describe it as such. Human understanding requires the more idealized models, but often it is not possible to amalgamate unified and unifying models into a (more nearly) perfect model.

These paragraphs do no more than suggest an alternative approach to characterizing unification. But I trust that I have said enough to show that there are ways of making sense of unification and our interest in it which provide promising alternatives to analysis in the spirit of the Perfect Model Model.

7. TWILIGHT OF THE PERFECT MODEL MODEL

By the end of the 20th century we have learned that even photographs require interpretation. The only PERFECT model of the world, perfect in every little detail, is, of course, the world itself. While one may intelligibly seek to characterize ideals of perfection in model building which fall short of getting every speck of dust in the right place, I am moved by acute awareness of our cognitive limitations to conclude that such programs are on the wrong track. Most will agree that in many respects the model view accurately describes science as it is actually practiced. A sober appraisal of human limitations suggests that it is also a plausible framework for models of the intellectual aims of science.

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NOTES

[1] See Daston and Galison 1992, e.g., p. 120, and passim.

[2] Suppe (1989) contains extensive references.

[3] For example, see Suppes (1967), pp. 57–8; Suppe (1989) pp. 82, also 269 ff. and 420 ff.; and van Fraassen (1987), pp. 108–9; (1989), pp. 188, 221; and (1991), pp. 4–7.

[4] See Suppe (1989) pp. 5–20 for some history. Suppes (1961, p. 165) cites Tarski's notion of a model, understood set theoretically. Van Fraassen construed models more broadly, in terms of state spaces, characterized with the language of the science in question (1980, pp. 64–9; 1987, p. 109). Suppes advocated comparison with the world indirectly through isomorphism with models of data (Suppes 1962).

[5] Of course, the history of science has seen many discussions of idealizations and approximations. Some credit Galileo with making crucial contributions to what remain our current attitudes. See Koertge (1975), McMullin (1985), and Cavelin (1974) among many

references on Galileo's influence. Duhem (1954) also discusses idealization. Scrivin (1961) and Shapere (1969) are two more recent references. I am here attempting to sketch the development of a coherent contemporary line of thought which brings the idealizing function of science to center stage. It does so in ways crucially different from the recent study of 'approximate truth' or 'verisimilitude'. Relations to that literature will be take up below.

[6] Richardson (1986a, 1986b) and Wimsatt (1987). See also references to Thompson and to Lloyd listed in Suppe (1989).

[7] Thus laws, properly construed, are not directly about the world, and hence are neither true nor false of it. Cartwright agrees that fundamental laws are true not of objects in the world, but of objects in models (1983, e.g. pp. 4, 129; 1995, p. 358). In her (1983, e.g., p. 4) she held that laws are, literally, false about the world. But in her (1997, p. S293) she qualifies that claim:

I began my career by arguing that the laws of physics lie (Cartwright 1983). That was on the assumption that what we call *laws* in physics really are laws in the sense I grew up with: claims about what necessarily or reliably happens. Ever since then I have been looking for an alternative philosophical account of laws closer to the way law claims are expressed and more responsive to the way they are used, an account that would give them a more reputable status.

I should add that, on the full view, laws may function to define a model, but they may also enter into models in others ways, for example when mathematical approximations apply in solving equations.

[8] Giere (1995, pp. 133–4) proposes to reconstrue laws in terms of 'principles' understood as model building guidelines. See also his (1988b) and Cartwright et. al. (1995).

[9] Many writing in the 'semantic view' tradition have been vague about whether or not strict isomorphism was intended, and if so, how 'isomorphism' should be understood. Some discuss approximation and idealization along the way. But only Richardson, and especially Cartwright and Giere (in very different ways) have been clear and explicit in advocating that we should take the relation between models and represented systems *not* as isomorphism but as similarity or inexact fit of some kind.

[10] Cartwright, more than anyone, has examined what is involved in such activities as idealization, making "prepared descriptions" (1983), abstraction and concretization (1989). She also uses the word 'model'. In so doing she examines important aspects of the ways in which models (inexact representations) are used. But, as far as I can see, she, no more than others, addresses the question that many address to modelers, of what models are supposed to be.

[11] Arenson et. al. (1994) also takes models to represent by form, but differs from the position described in Section 4 below in taking the relevant similarity relation to be objective and fixed.

[12] The example, and objection are taken from Savage (to appear).

[13] This objection is urged by Jones (to appear).

[14] This example, and indeed the whole account I am sketching, raises pressing questions about realism and objectivity which must be addressed in a separate project. For the present I take there to be much prima facie plausibility to the claim that such models succeed in providing objective information about the nature of what they describe in spite of the fact that the models are known to be highly inaccurate or idealized in important respects.

[15] Giere's (1985, pp. 80–1; 1988a, p. 81) could not be more specific in characterizing a view on which models are taken to be similar to systems only in respects and degrees. But

(1988a, p. 81) appears to suggest that there may be a context independent and objective basis for the relevant similarity relations. On the other hand, (1988a, pp. 106–8) contains many of the elements of the analysis of this section. More specifically, van Fraassen (1985, p. 290), commenting on Giere (1985) sees in Giere's formulation the same kind of analysis I am here suggesting:

. . . [Giere's] formulation and my own both have the virtue that they do not suggest (as various realists have suggested) that we need a theory of approximate truth or verisimilitude or approximate fit as a subject in semantics on a level with the theory of truth. [Giere's] formulation, moreover, makes very clear that assertions of approximate correctness are context-dependent, requiring for completeness a separate specification of respects, degrees, and criteria of similarity. In practice, I think, the specifications in mind are largely a function of interest, which affects not only the degree to which, but also the respect in which, the models must be similar to constitute "good" approximations.

Cartwright's (1983) is specific in places about the ways in which different models are intended to cover different facets of complex situations: "No single model serves all purposes best." (p. 11, see also, e.g., pp. 104, 152). Wimsatt (1987, p. 24) also briefly makes the present point.

[16] There is a huge literature attempting to make out a relation of approximate truth, "truthlikeness", or "verisimilitude". In many respects, these efforts can be seen as attempts to make precise, refine, and generalize the basic idea of putting in "error bounds". See Niimiluoto (1999) for an excellent literature review.

[17] Leplin (1984) presents representative attitudes in the early 80's on both sides of this debate.

[18] Some of the verisimilitude literature recognizes some context relativity of the relevant measures of comparison. For a summary see Niiniluto 1999, pp. 14 ff. I would urge that the context relativity described in this literature, being limited to relativity to a question asked, or a language chosen is not nearly broad enough.

[19] Smith restricts attention to geometric modeling theories. I take the present exposition to provide the obvious generalization of this special case.

[20] Smith take himself to differ on this point with Giere: "Giere sets his sights higher . . . he seems to be aiming for a rather general treatment of the content of theoretical hypotheses. Since theoretical hypotheses are all approximative, by his lights, that in effect means saying something general about approximate truth. But I doubt, given the discussion earlier, that we can say anything both general and substantive" (p. 273). At points Giere does write things which support Smith's reading (see footnote 15) However, I have from the beginning found it natural to read his exposition in terms of similarity determined by contextually determined interests – see footnote 15 for van Fraassene's same reading of Giere's (1985).

[21] Following Miller we could introduce new primitives for the compound properties, 2 oz. iff square, and 2 oz. iff red. So doing encourages the illusion that there is a problem and makes the details harder to follow.

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