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THE SEMANTIC OR MODEL-THEORETIC VIEW OF THEORIES AND SCIENTIFIC REALISM

ABSTRACT. The semantic view of theories is one according to which theories are construed as models of their linguistic formulations. The implications of this view for scientific realism have been little discussed. Contrary to the suggestion of various champions of the semantic view, it is argued that this approach does not make support for a plausible scientific realism any less problematic than it might otherwise be. Though a degree of independence of theory from language may ensure safety from pitfalls associated with logical empiricism, realism cannot be entertained unless models or (abstracted and/or idealized) aspects thereof are spelled out in terms of linguistic formulations (such as mathematical equations), which can be interpreted in terms of correspondence with the world. The putative advantage of the semantic approach – its linguistic independence – is thus of no help to the realist. I consider recent treatments of the model-theoretic view (Suppe, Giere, Smith), and find that although some of these accounts harbour the promise of realism, this promise is deceptive.

1. THE SEMANTIC OR MODEL-THEORETIC VIEW

What, precisely, is a scientific theory? One might well be sceptical about the possibility of any unified account, given the sheer diversity of disciplines contained under the rubric of 'the sciences'. A view generally associated with logical empiricism is that a theory is an axiomatic system, closed under deduction, expressible in a formal language whose elements are characterized by a syntactical structure. A theory, on this account, is identified with a particular linguistic formulation. The emphasis given here to the syntax of linguistic formulations led critics to dub this the *syntactic view*. (A more perspicuous label might be the *sentential* view). The implausibility of this thesis as a general account of theories has been stressed in connection with criticisms which have resulted in the rejection of logical empiricism this century.

Many critiques of the syntactic view have coalesced around an alternative account of theories: the so-called *semantic view*. The term 'semantic' here is used in the sense of formal semantics or model theory in mathematical logic. That is, the semantic view 'construes theories as what their formulations refer to when the formulations are given a (formal) *semantic* interpretation' (Suppe 1989, 4). Theories on this view are not

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linguistic, but rather abstract, set-theoretic entities – models of their linguistic formulations.[1] A theory is a family of models: systems that satisfy the theoretical laws we commonly associate with scientific theories. The position is thus sometimes referred to as the *model-theoretic* view.

The nature of the models involved, however, is a matter of some disagreement among semanticists. For some a model, as well as being a 'structure' that satisfies certain axioms, also includes a mapping from elements of a linguistic formulation to elements of that structure. Imagine, for example, a function that assigns subsets of objects to one-place predicates, two-place relations, and so on.[2] Others hold that one should not think of a model as *including* any such interpretation of sentences. The relation between a linguistic formulation and its models is one of definition. The models at issue are by definition simply those that satisfy, for example, the mathematical equations of a quantitative theory, such equations being linguistic devices; but theories themselves are models in the sense of "pure" structure: abstract entities and relations among them, excluding the linguistic formulations with which they may be linked.[3]

This difference of opinion among supporters of the semantic view essentially concerns the issue of how best to achieve an appropriate level of independence on behalf of theories from language. Independence here saves us from the worry that one and the same theory given different linguistic formulations – that is, expressed in different languages – must be viewed as different theories.[4] If independence from language is the desideratum, however, why bother with models? Won't propositions suffice? The answer no doubt depends on what one takes to be the proper understanding of the nature of propositions. More specifically, it depends on the extent to which one sees propositions as abstract entities, and free from the shackles of syntax. There are accounts, it seems, according to which one might say that theories are sets of propositions, thus abstract entities capable of being given various linguistic formulations, in such a way as to assuage the worries of those motivated to adopt the semantic approach.[5] Indeed, evidenced by the paucity of latter twentieth century logical empiricists, some such idea is presumably basic to the understanding of most philosophers and scientists. These people might welcome the news that the semantic view offers a departure from a previous theory of theories with the same sort of amazement that might greet the news that one has been speaking one's native language for some time now.[6]

It is not my present intention to resolve the finer points of the semantic view. Rather, what I hope to do is to discuss how this approach bears on the issue of scientific realism. If theories are repositories for scientific knowledge, one might well suspect that our notion of what a theory is may tell us something about the knowledge contained therein. On the semantic view, the realist project becomes one of determining which aspects of models may be thought to represent the natural world, and how. In what follows, I will show that contrary to the suggestion of some advocates of the semantic view, this approach does not help to facilitate any plausible scientific realism. In some cases, an emphasis on models seems tailor-made to promote varieties of instrumentalism. Realism on the semantic view is by no means impossible, but faced with precisely those familiar, perennial difficulties of reference and correspondence that some semanticists think their approach does without.

One of the primary motivations for the model-theoretic approach has been to escape worries about how linguistic entities link up with the world. It may seem, *prima facie*, that if there are problems inherent in the attempt to forge links between language and world, these will dissolve when the issue is rather one of forging links between non-linguistic entities. Surely models are better suited to representing the phenomena than collections of sentences. As we shall see, however, this speculation does not help the realist. For ultimately, realist commitment requires assertions as to the nature of correspondence relations between substantive descriptions of models and reality. The use of linguistic devices in making such relations explicit seems to run afoul of the semanticist aspiration for linguistic independence.

2. SCIENTIFIC REALISM, ABSTRACTION, AND IDEALIZATION

On the semantic view, theories are families of models. The very notion of a model invites comparisons with that which is modelled. Models are generally caricatures of the natural world. Consider, for example, the simple pendulum: the theory of its motion describes a mass attached to a frictionless pivot by means of a massless string, swinging in a uniform gravitational field and encountering no resistance. The fact that models are often caricatures is of course widely appreciated. What is perhaps less well appreciated is that there are, in fact, two quite different ways of constructing caricatures. Following Suppe's (1989) account, I will call the first of these practices 'abstraction'; for the second, I will use the term 'idealization'.[7] The distinction between abstraction and idealization will prove an important tool in our discussion of the import of the semantic view for scientific realism.

Abstraction is a process whereby only some of the potentially many relevant factors or parameters present in reality are built-in to a model concerned with a particular class of phenomena. Here we simply ignore other parameters that are potentially relevant to the phenomena at issue. This is done for two reasons. First, the number of factors that are potentially relevant to a given class of phenomenon is often exceedingly high, making the construction of an equally refined model impractical. Secondly, the influence of many if not most potentially relevant factors is generally negligible within the scope of that class of phenomena defined by our interests in any particular theoretical or practical investigation. The class of phenomena of interest and the level of predictive accuracy required within a particular scientific context together determine which and how many potentially relevant factors we choose to incorporate in our models. Crucial to abstraction, however, is the idea that those parameters which *are* chosen to comprise model elements in this way may be thought to have counterparts in reality. The fact that other parameters are ignored does nothing to impugn the reality of those factors which are, in fact, chosen.

Idealization is another matter altogether. Here too pragmatic concerns enter into model construction, but with very different consequences. For the hallmark of idealization is that model elements are constructed in such a way as to differ from the systems we take to be their subject matter, not merely by excluding certain parameters, but by employing assumptions that could *never* obtain. The assumption in classical mechanics of masses concentrated at extensionless points is an idealization. We do not actually think that masses of bodies are concentrated at extensionless points. Contrast this situation with one of the aspects of our model of the simple pendulum. Masses swinging in terrestrial pendulums usually encounter air resistance. In ignoring this parameter, our model of the simple pendulum is an abstraction. Abstraction involves choosing some parameters and ignoring others; idealization involves simplifying the natures of particular parameters chosen. Models are generally, at best, abstracted and idealized versions of reality.

The distinction between abstraction and idealization helps us to see how realism may be adopted in connection with some models, but not others, and more importantly how some aspects of one and the same model may be viewed realistically while others are viewed instrumentally. If all we claim of a scientific theory is that it makes accurate predictions, we value the theory for its instrumental virtues. If, however, we have opinions about the entities and/or relations composing theoretical models, realism becomes a possibility. A model employing abstraction only may well be thought of realistically. The entities which it invokes and the relations between them may be thought to have counterparts in the world. This is true even though there are other entities and further relations that may be relevant to the phenomena under investigation. For example, an entity realism which asserts the reality of those particulars abstracted in constructing a model seems perfectly at home here. So too does a structural realism which asserts the reality of those relations constituting a model, often given in the form of mathematical equations. Such relations will generally hold only in the limit as certain potentially relevant factors are ignored (absent), but this is well within the remit of a structural realism.

Idealization, on the other hand, cannot be adopted so straightforwardly by a realist analysis. Model assumptions here *contradict* what we take to be true of reality. Realism in this context will be carefully qualified at best. If some element of a model constitutes an idealization, as opposed to a more severe sort of fiction, we have warrant at least for believing in the existence of those aspects of reality which have been idealized. When idealizations concern abstracted but nonetheless genuine parameters, as in the example of masses in classical mechanics, the realist can admit the *existence* of such parameters. Is much more in the way of a realist commitment possible here, or are such theories better thought of as promising precursors to more realistically construed theories? Ultimately, the question of how to make sense of idealization may pose the greatest challenge to the realist, but this topic must await another occasion. For now, let us return to the question of how the semantic or model-theoretic view fares in providing for the possibility of scientific realism.

3. THE SEMANTIC VIEW AND REALISM: WHAT MODELS TELL US

The semantic view may have attractive features, but a shortcut to scientific realism is not one of them. This assertion runs contrary to the intuitions of some advocates of the model-theoretic approach. In what follows, I will press the point of mistaken intuitions by way of a dilemma for the semanticist: so far as realism is concerned, one must either face up to traditional challenges, or abandon any substantive realist commitment; one cannot both be a realist *and* dodge these problems – not by adopting the semantic view, at any rate. Before we continue, however, let us be plain about the extent to which this dilemma understates its case. While the present interest is in scientific realism, our dilemma in fact applies to realist commitment very broadly construed. Anyone who is a realist about *any* portion of theory, even the instrumentalist, who traditionally believes only in what she takes to be observable, will find no special facilitation of her commitment in the semantic view.

This is an important point, and merits clarification. Proponents of the semantic view generally do *not* contend that this position favours any one particular epistemology of science (realism, empiricism, instrumentalism, etc.).[8] What they do claim is that the model-theoretic approach provides an account of theories which, owing to its emphasis on models as opposed to language, permits a less problematic treatment of such issues. Though many of the arguments to follow may be interpreted in such a way as to contest this general thesis, I will focus primarily on the case of realism. The moment the model theorist opts for *any* sort of commitment, be it instrumentalist or realist, she opens the door to the very difficulties the development of the semantic approach was in part intended to leave behind: namely, issues of correspondence between language and world. As soon as we give not merely a prediction, but a description of ontological commitments associated with that prediction – concerning which elements of our model are meant to correspond to reality and which are not – the traditional challenge to the realist of giving a satisfactory account of such correspondence returns.

Here the semanticist might claim an advantage. Surely it's a simpler matter to compare theories with phenomena if theories are models, as opposed to sets of sentences, for here we compare like with like. Surely it's easier to compare two non-linguistic entities than it is to compare one linguistic and one non-linguistic entity. This, however, is misleading. One might think that a model system is more easily compared to a worldly system because both, as kinds of objects, can be visualized, thus facilitating comparison. But this is to appeal to a purely metaphorical sense of 'visualization', both in the case of those portions of the world that are beyond the grasp of our visual sensory apparatus, and in the case of all models, which in the present context, recall, are *abstract* objects. It is unlikely that "comparison" here is any less complex a task than that required in the case of linguistic descriptions of models and the world; perhaps it is no different at all. In any case, on a realist account, even if models weren't abstract, this wouldn't help the situation. Theories are not merely objects which replicate or imitate the phenomena; even if they are such things, they are meant, in addition, to *tell us something substantive* about the nature of the world. The obvious question, then, is how is this achieved?

A model can tell us about the nature of reality only if we are willing to assert that some aspect(s) of the model has a counterpart in reality. That is, if one wishes to be a realist, some sort of explicit statement asserting a correspondence between a description of some aspect of a model and the world is inescapable. This requires the deployment of linguistic formulations, and interpreting these formulations in such a way as to understand what models are telling us about the world is the unavoidable cost of realism. Scientific realism cannot be entertained unless we are willing to associate models with linguistic expressions (such as mathematical formulae) and interpret such expressions in terms of correspondence with the world. In the absence of this kind of assertion, there is no realism. Theories can't tell us anything substantive about the world unless they employ a language.

'You misunderstand', replies the semanticist: 'I do not deny that theories can be given linguistic formulations. I deny that theories should be identified with such formulations'. Fair enough. But here the issue of independence of theory from language becomes quite pressing. One of the primary motivations for adopting the semantic approach is to escape the perceived difficulty of having to deal in the currency of sentences. I have argued that even if we take theories to be models as opposed to axiomatic sentential systems, being a realist requires that we interpret sentences which spell out the ways in which descriptions of models correspond to entities and/or relations in the world. Now if theories are separated strictly from their linguistic formulations, and some linguistic constructs are required to entertain the possibility of scientific realism, it would appear that scientific theories *themselves* are, in principle, incapable of being true, false, or approximately true (whatever this might mean; see Section 6) in the sense of the realist. To be perfectly accurate, we would have to say that *theories* aren't true, etc., but that linguistic descriptions of them are. Realism would here require the introduction of "extra-theoretical" elements: descriptions. An unforgiving critic might object to this consequence. After all, does this not reduce theories themselves to mere metaphors or analogies for natural systems – that is, at most good or bad, but never true or false? Realism, of course, is a doctrine about substantive truths, and most would argue that *whatever* a theory is, it should be amenable to various construals, including realist ones.

These considerations, however, do not preclude realism on the semantic view. Rather what is indicated is the requirement that we be flexible in phrasing our epistemic commitments. Models may not be true or false *per se*, but certainly descriptions of them have this capacity in application to the world, and this provides sufficiently for the possibility of realism. But this point is telling, for what has been gained in the shuffle? From the point of view of the realist: nothing, since knowledge of the world is here once again dependent on evaluating the correspondence truth of sentences. Thus, the emphasis in the semantic view on models does nothing to eliminate the currency of sentences so far as the project of determining how aspects of theories might be cashed out as literal representations of the world is concerned. This project is the realist's principal interest. The semantic view is in no position to remove problems of linguistic interpretation (reference and correspondence) thought to attach to realism. It is this insight that stands behind my central dilemma for the model theorist.

In the remainder of this paper, I consider some recent treatments of the semantic approach so as to explore what these accounts offer in relation to the possibility of scientific realism. Though some of these analyses hold the promise of a more easily facilitated realism, such promise, I argue, goes unfulfilled. We shall move in the direction of increasing ambition with respect to the issue of realism (Suppe, then Giere), and finally consider a semanticist proposal regarding the notion of approximate truth (Smith).

4. SUPPE: 'QUASI-REALISM'

On Suppe's version of the semantic view, a model is a 'physical system': 'a relational system consisting of a domain of states and a sequence defined over that domain; the sequence is the behaviour of the physical system'; any particular physical system 'may be construed as the restriction of the theory to a single sequence' (1989, 90). The state of a physical system at a time is defined as the set of simultaneous values of its parameters. The behaviour of a system is its change in state over time as governed by laws: 'relations which determine possible sequences of state occurrences over time that a system within the law's intended scope may assume' (*ibid.*, 155).

Suppe recognizes the importance of links between theory and world, however they are forged, for the concept of theory. Thus he claims that 'abstract structures ... do not become scientific theories until they are provided with physical interpretations (mapping relations between theory structure and phenomena). Further, it is clear that these physical interpretations are not explicitly stated ... but are implicitly or intensionally specified and are liable to alteration, modification, or expansion as a science progresses' (*ibid.*, 422–423). Describing links between models and world as 'implicit' is an interesting move. The advocate of such a position may claim that different epistemic attitudes toward a given theory are possible depending on what one takes its intensional content to be. It is doubtful, however, that this is a helpful thing to say. Intension is a concept that we most commonly apply to elements of language, and though this is not to say that non-linguistic objects have no intensional content, the idea of such content becomes even more nebulous once we stray from the context of language. It isn't clear that 'the intension of a model', where 'model' refers to an abstract entity, refers to anything in particular.

In any case, it is Suppe's own prescription for how theories (models) should be thought to represent the world that is of interest here. 'Quasi-realism' is a position according to which some scientific theories are in fact true, but not in any straightforward correspondence sense. Rather, 'truth' is given a *counterfactual* sense. A true theory characterizes the phenomena counterfactually: it describes how the phenomena would be if the phenomena were the way the model describes them. As a condition on what theories count as true, the counterfactual criterion is clearly insufficient. For if the phenomena were, in fact, the way a theory says they are, there would be no question of the theory describing the phenomena incorrectly. On this condition alone, *any* theory would count as true, no matter how outlandish or divorced from empirical reality. Suppe does not, of course, hold that all theories are true. What, then, does the job that counterfactual truth cannot do – what separates theories that are true from ones that aren't? Suppe's account relies heavily on a distinction between logically and causally possible models (*ibid.*, 67). Any theory will define a class of logically possible systems, but if a theory is true, it defines a class of causally possible systems. The set of causally possible physical systems is that subset of logically possible systems whose members do, or could, in fact, correctly describe actual systems in situations where parameters not incorporated into the models concerned exert negligible influence.[9]

Thus, in connection with a true theory incorporating model S, we have the following more nuanced rendering of the counterfactual account of truth: 'If P were an isolated phenomenal system in which all other parameters exerted a negligible influence, then the physical quantities characteristic of those parameters abstracted from P would be identical with those values characteristic of the state at t of the physical system S corresponding to P' (*ibid.*, 95). Note, however, that this characterization of a model, S, is a description of what we have referred to as abstraction. In cases of abstraction *simpliciter*, the counterfactual criterion is indeed compatible with a realist interpretation. For in cases of abstraction, parameters appearing in models may also be parameters occurring in nature, featuring the same interactions and relations as those constitutive of the models in question. The fact that there are other factors in nature potentially relevant to the phenomena at issue rules out neither the possibility of realism concerning those we have included, nor the possibility of augmenting theories by adding further parameters.

But what about cases of idealization? In the presence of idealization it would be wrong to claim that a theory is straightforwardly true in the sense of the scientific realist, even though it may well be true in the sense of the quasi-realist. For in such cases, models are constructed in such a way as to incorporate assumptions which contradict our beliefs about aspects of reality. Here there is no situation in which the relevant phenomenal system is just like that described by the theory, and while adding extra parameters may help to facilitate greater accuracy of prediction, it does nothing to make true, in the realist sense, representations of parameters that are idealizations. Thus, truth for the quasi-realist is something which is compatible with realism, but also with cases in which realism is ruled out in principle.[10] Quasi-realism isn't realism at all, its condition on truth with respect to theories being too permissive. The key to this observation is the fact that idealized systems cannot describe, strictly speaking, causally possible systems, for idealized systems are constructed to be unlike, for the purpose of simplification, all actual situations. Thus, in addition to the fact that in cases of idealization, a theory can be true for the quasi-realist but not strictly true for the scientific realist, Suppe's account harbours internal difficulties: true theories are supposed to define causally possible systems, but counterfactual truth embraces idealized theories which are by their very nature causally *im*possible.[11]

5. GIERE: 'CONSTRUCTIVE REALISM'

While aspiring to linguistic independence for models, Giere inhabits the more liberal end of the semanticist spectrum in allowing linguistic entities a constitutive role in theories. '[W]e understand 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, 85). Giere defines an hypothesis as a linguistic entity used to assert a relationship between a model and some aspect of the world.[12] Hypotheses are true or false, depending on whether or not the relationships they assert obtain. The relationship asserted by an hypothesis is one of similarity of phenomenal system to model, specified in terms of relevant respects and degrees. Consider the following 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. The earth and moon form, to a high degree of approximation, a two-particle Newtonian gravitational system' (*ibid.*, 81).

Giere holds that his version of the semantic view supports what he calls a 'constructive realism'. The position, however, exemplifies perfectly our previous dilemma: either it abandons any serious realist commitment, or it is confronted with those very same issues of correspondence and language that some hope the semantic view leaves behind. Let us consider the issue of linguistic independence. According to Giere, there is no need to posit a *direct* relationship between language and the world. The relation is rather indirect, via the intermediary of theoretical models.

But this, surely, is a case of wanting to have one's cake and eat it. No matter how many models one stacks between linguistic formulations and reality, language unavoidably enters the picture in the form of hypotheses once ambitions for determining what theories tell us about the world are entertained. This is something the realist must do. The fact that models stand between those linguistic formulations defining them and the world is of little consequence, given that hypotheses stand between models and reality. There can be no linguistic independence if it is our intention to evaluate claims of correspondence.

Constructive realism has a response to this objection. Theoretical hypotheses expressing similarity relationships 'are indeed linguistic entities . . . [b]ut for these a "redundancy theory" of truth is all that is required' (*ibid.*, 82). Since, ultimately, it is concern over the notion of correspondence truth that fuels debates about realism, we're on safe ground. Garden variety realism errs because it attempts to 'forge a direct semantic link' between statements defining models and the world, but the model theoriest avoids this error, since models represent the world in virtue of similarity relationships, not relationships between linguistic entities and reality.

This attempt, however, to substitute 'similarity' for correspondence will not suffice. Consider again the example of the earth and moon above. If all 'similarity' means in this context is that classical mechanics generates values for positions and velocities of bodies that match (within some agreeable error tolerance) that which we observe, perhaps a redundancy theory of truth is all that we require. But if this is all we mean, we are endorsing nothing more interesting than a kind of phenomenalism, for the only work an assertion of similarity is doing is to affirm that the model reliably generates predictions that make good enough sense of our phenomenal experiences. The realist, clearly, needs more than this. The realist wants to assert something as to whether objects, properties, and/or relations comprising models have counterparts in an external reality – whether, for example, inverse square relations actually exist in nature. No mere redundancy theory will accommodate this demand. Let us be very clear about this, for many realists would claim to subscribe to a redundancy (or perhaps a 'minimalist') theory of truth. These people, I take it, reject the idea that 'truth' refers to a property that all true things must have. But this idea is not what I have in mind when I say that correspondence is important to the realist. What I mean is that, for the realist, what determines whether or not something is true are things in the world. Whatever commitments one may associate with correspondence theories of truth, the realist requires, minimally, a correspondence theory of *truth makers*. It is in this sense that the issue of correspondence is unavoidably invoked once the claim is made that the world is or is not like some substantive aspect of a theoretical model; that is, once the claim is made that a description of some such aspect applies, not just to a model, but also, literally, to the world. These are the sorts of claims that realists make.

Let me put the point another way. There are different sorts of "correspondence" at issue when models are taken to represent reality. First, there is a correspondence between linguistic formulations and the models they define. This relation is unproblematic, being as it is a matter of simple definition. Secondly, we hold that models "correspond" to certain classes of phenomena; again, no cause for controversy here, given that we construct particular models with specific actual world systems in mind. A third sort of correspondence arises in the context of Giere's theoretical hypotheses: there is a putative correspondence relation between any claim regarding a similarity relation and an actual similarity relation. In contrast with the previous two situations, this is a case in which correspondence might fail. There is correspondence here if and only if the claimed similarity relation is in fact actual – that is, if and only if the theoretical hypothesis is true. Finally, a fourth correspondence relation may or may not hold between linguistic descriptions and the world. Correspondence obtains in this case if and only if such descriptions are true of reality.

Here we come to the crux of the matter. The constructive realist maintains that we may combine the unproblematic first case of correspondence (that between models and their axioms) with the third case (a claim of similarity between model and world), to yield substantive knowledge of reality. A redundancy theory of truth is all that is required where claims of similarity are concerned, for to say that it is true that a relation of similarity obtains is merely to say that a relation of similarity obtains. This, however, is not sufficient for the realist. A claim of similarity, even when cashed out in terms of 'respects' and 'degrees', is only disambiguated by a claim to the effect that some one thing is true of both parties to the similarity relation. That is, in order to understand unambiguously what a claim of similarity is telling us about the world, we must interpret the claim that some description is true, both of a model, and of reality. It is the second conjunct that interests the realist; this is our fourth case of correspondence. Here the application of a linguistic description to the world must be interpreted, and the detour via models does nothing to prevent us coming full circle, back to square one, and engaging with issues of reference and correspondence in connection with language.

The example of classical mechanics is a good one. It is generally held that Newtonian gravitational theory, though terrifically useful, is not true, strictly speaking. If there are elements of truth to the theory, these must be extracted in the ways according to which previous theories are often subsumed into those that succeed them. For example, one might contend that some of the relations posited by the theory hold, not generally, but in certain limiting case scenarios. Such subtleties, however, cannot be accommodated on a scheme of asserted similarities unless and until vagueness is removed with clarificatory qualifications. The constructive realist says: 'The *interpretation* of terms used to define the models does not appear in the picture; neither do the defining linguistic entities, such as equations' (*ibid.*, 86). But leaving out interpretations and equations creates a climate unsuitable for realism, for these tools are indispensable to the project of spelling out precisely what similarities obtain and how. The relation of similarity cannot, as Giere hopes, bypass issues such as correspondence truth and the reference of theory terms, for 'similarity' is by itself hopelessly vague. Leaving it vague sacrifices any pretence of a theory to represent the world in a clear or substantive way. Spelling it out produces linguistic renderings of aspects of models which require interpretation in application to the phenomena.

Similarity left vague professes virtues which are exposed as wishful thinking when claims of similarity are refined. Consider, for example, worries about the possibility of a "pessimistic induction" on past scientific theories: most past theories are held to be false, strictly speaking, from the perspective of the present day; it is thus likely that present day theories are also false. One of the key moves in the pessimistic inductivist's argument focuses on the failure of reference of theory terms in past theories.[13] But, claims the constructive realist, on her version of the semantic view, such worries are a thing of the past. Since models need only be similar to natural systems, the non-existence of referents of past theory terms is not a problem. 'Whether the ether exists or not, there are many respects in which electromagnetic radiation is like a disturbance in an ether' (*ibid.*, 107). This attitude, however, serves only the instrumentalist at best, and our phenomenalist at worst. It is only if the existence or non-existence of things like objects, properties, and relations is unimportant, and similarity is cashed out in terms of sufficiently accurate prediction, that this line of argument sees off the pessimistic induction.

If 'similarity' is understood in such a way as to facilitate no more than instrumentalism, the pessimistic induction loses all force. But of course the pessimistic induction was never intended to threaten the instrumentalist in the first place. The correspondence truth or falsity of assertions regarding "unobservable" aspects of a theory is irrelevant to its status as an instrument for the generation of "observable" predictions. Here we have another case of wanting to have one's cake and eat it: Giere goes on to say that one good reason for rejecting ether models is the fact that there is no ether in the world, and that this constitutes an important respect in which similarity between such models and reality fails. But if we go this route, refining similarity in such a way as to consider whether aspects of models (such as the ether) have counterparts in reality as the realist requires, the above strategy of employing similarity to dissolve the pessimistic induction along instrumentalist lines falls apart.

Similar dilemmas confront the similarity schema on various fronts of traditional worry about scientific realism. What sort of an account of theory choice does one give from the realist perspective? On the semantic view, this is a matter of providing defensible criteria with which to decide which families of models best fit the world. Construe 'fit' in terms of similarity, and we are faced with a now familiar choice. Either spell out explicitly which and how aspects of models are meant to represent the phenomena, or renounce claims to any realism worthy of the name. How does one give a satisfactory account of approximate truth, in light of which successions of theories might be shown to incorporate increasingly truthful renderings of the world? Giere advocates trading in notions of approximate truth for notions of greater or lesser similarity of relevant respects and degrees. But this is of no help to the realist. The idea of greater or lesser similarity is simple enough if by this we mean nothing more than greater or lesser accuracy of prediction. Giving an account of comparative similarity that facilitates realism, however, is no less daunting a task than giving an account of approximate truth, and may represent nothing more than a change in terminology.

6. SMITH: 'APPROXIMATE TRUTH'

Focusing on dynamical theories, Smith (1998a) offers what might be described as a geometrized version of the semantic view. By 'dynamical' he intends theories that specify how certain parameters evolve over time, without concern for why such evolution occurs (thus excluding, for example, details as to causal mechanisms). A model is here an abstract structure which has as a defining feature a precise geometrical structure. If a theory is successful, this geometry approximates to a geometrical structure which can be associated with a phenomenal system. The idea of a geometrized semantic view is perhaps best illustrated with an example (*ibid.*, 259–260):

> [C]onsider the familiar account of the dynamics of a freely swinging pendulum. One standard way of looking at this account is to regard it as first characterizing a pure abstraction, the

ideal frictionless pendulum moving in a plane according to Newton's laws. The governing equations determine the allowable patterns for the time-evolution of the ideal pendulum's angular displacement and velocity as a function of the pendulum's fixed length, etc. If we conceive of plotting a three-dimensional graph of time against displacement against velocity, then a certain bundle of three-dimensional curves will trace the allowable behaviours of a pendulum of given length subject to a given force. If we conceive, yet more abstractly, of these three-dimensional bundles being 'plotted' against pendulum length and applied force, we will get a more complex five-dimensional structure that in addition encodes the way that the possible behaviours of the pendulum depend on the length and force.

Here, mathematical relations between parameters in both models and phenomenal systems are analyzed in such a way as to generate geometrical structures, which may then be compared.

It is the use to which Smith puts his version of the model-theoretic approach in developing an account of approximate truth that is of interest here. "Approximate truth" has long been a problematic plank supporting the realist's epistemology. Smith (*ibid.*, 275, fn. 30) is careful to skirt the issue of whether his account may be applied in aid of scientific realism, but holds nonetheless that the proposal should be congenial to realists and non-realists alike. Congenial it may be, but this should strike us as suspicious; we should be wary of an account of approximate truth that is acceptable to all comers, in just the same way that we would be wary of an account of truth claiming the same virtue. (Consider, for example: "unobservable" theoretical elements must correspond to things in the world if a theory is to count as true for the realist, but this is not the case for the instrumentalist. These people give different accounts of what it means for a theory to be true.)

If having a notion of approximate truth is of *particular* importance to the realist, this might suggest that she requires something more from an account of this notion than that delivered by one that is acceptable to her interlocutor. Indeed, it turns out that if Smith's account of approximate truth gives the impression of universal acceptability, it is only because, like Giere's notion of similarity, it is highly non-committal. In its given form, it satisfies the instrumentalist, but is insufficient for realism. Realism requires that Smith's account be supplemented with additional criteria. This, as we shall see, has the effect of again making unclear how best to think about approximate truth, and brings us back to familiar worries about contravening the spirit of the semantic view.

While the realist accepts that most theories are false, strictly speaking, she likes to think that they are getting better, and not merely predictively. Hence the motivation for an account of approximate truth with which to "measure" improvements in theoretical knowledge. Traditional accounts such as Popper's attempts to explicate 'verisimilitude' are fraught with difficulties. Smith proposes an alternative. A ‘wide class’ of dynamical theories, he claims, can be thought of as containing two parts: one specifying an abstract geometrical structure, the other giving empirical application to that structure via the claim that it approximates a geometrical structure associated with some dynamical phenomenon. Approximating truth is thus simply a matter of approximating geometrical structures.

But what is the informational content of an assertion to the effect that the geometric structure of a model approximates that of a system in the world? If the geometric structures are sufficiently close, this tells us that the values of parameters whose functions are graphically represented to produce the structures in the first place are sufficiently close. And what does this mean? Just that a model makes sufficiently accurate predictions regarding our measurements of those parameters. But this by itself makes no commitment with respect to ontology, and in particular, with respect to the “unobservable”. Realists contend that truth and approximate truth have something to do with correct mappings of things and/or relations present in reality, but what if ‘geometrical closeness’ is evident in cases where little such mapping occurs? We shall illustrate this worry with an example, momentarily.

If our interest is confined to making predictions within specified margins of error, our ambition does not exceed the use of models as tools for this purpose. We can *call* a model which meets a stricter error tolerance ‘more approximately true’ if we like, but this does not go so far as to exceed an instrumentalist sense of approximate truth. Being approximately true in this sense does not deliver the substantive sorts of knowledge the realist has in mind. To put the point another way, consider the difficulty for the realist of the potential underdetermination of theory by data. For the realist, no more than one of a set of empirically equivalent but otherwise contradictory theories can be true; though such candidates are predictively equivalent, they make different commitments elsewhere. So too for approximate truth. Rival theories that score equally well on Smith’s test of geometrical closeness do *not* thereby have the same degree of approximate truth so far as the realist is concerned, for such theories, if they are indeed rivals, make different ontological commitments.

Perhaps it is a failure to respect Smith’s focus on ‘dynamical’ theories that gives rise to this seeming discrepancy. By restricting our attention to theories that have nothing to say about underlying (e.g., causal) mechanisms, perhaps we exclude cases in which geometrical closeness and a significant failure to map the world might coincide. This, however, is unlikely. It is only by excluding theories (models) whose elements are taken to correspond to unobservable things that we could make certain of avoiding this problem. If a model is constructed out of nothing more than variables that correspond to "observable" measurements of actual systems, geometrical closeness and successful mapping may well amount to the same thing. But in the case of models that incorporate elements whose putative counterparts in the world are "unobservable", geometrical closeness of model parameters and worldly measurements does not entail that such counterparts exist. If they do not, such theories will not count as approximately true for the realist.

From a realist perspective, talk of geometrical structures is at best a shorthand for finer grained theoretical claims which are either true or false, concerning those parameters whose values map out geometrical structures to begin with. Assertions as to the existence or non-existence of specific entities and/or relations are what counts here. To this end, clarity regarding whether aspects of models are abstractions or idealizations is crucial. Note that on Smith's account, approximate truth looks indifferently on these very different methods of theory construction. A theory can be more or less approximately true regardless of whether it embodies abstraction or idealization – all that matters is sufficient approximation of geometrical structure. As we noted earlier, however, realist commitment discriminates between abstraction and idealization: the former is straightforwardly compatible with true, substantive beliefs about the world; the latter less so. Once we take stock of the kinds of assertions required to formulate realist commitments, we see that the criterion of geometrical closeness is not sufficient for approximate truth in the realist sense. Furthermore, the question of what *would* serve as an account of approximate truth here is left unanswered. Geometrical closeness, while not itself sufficing as an account, may well constitute an important criterion to be weighed with others (concerning, for example, whether the entities and/or relations posited by a theory exist, and to what extent the theory employs idealization) in determining whether or not a theory is approximately true in the sense of the realist.

Smith partially anticipates some of these criticisms in an example (*ibid.*, 274–275). Imagine that T_1 and T_2 are Ptolemaic and Newtonian dynamical theories of planetary motion, respectively. The parameters of T_1 and T_2 can be chosen in such a way that T_1 is more approximately true than T_2. That is, T_1 is better at predicting motions of planets than T_2; T_1 is superior in approximating actual geometries mapped out by planetary motion. But there is something uncomfortable about this; we would like to think that the Newtonian theory is more approximately true than the Ptolemaic. Smith suggests two ways out of this conundrum, but neither, I believe, is acceptable to the realist. The first is to claim that judgements of approximate truth are interest relative. If our interests are purely navigational, we should accept that T_1 is more approximately true in this respect. This, however, is at best an instrumentalist deployment of 'approximately true'. T_1 is a better tool than T_2 so far as the task of navigation is concerned. Conversely, the realist countenances the validity of human interests in myriad aspects of scientific theorizing, but *not* in the determination of what counts as true, false, or degrees of either.

The second proposal holds greater promise. What if it turns out that T_2 can be unified with theories which are both greater in number and more approximately true (in the strict, non-interest relative sense of geometric approximation) than those with which T_1 might be unified? Smith claims that despite the fact that T_2 does not approximate to geometrical structures associated with planetary motion as well as T_1, we have good reason to conclude that T_2 is more approximately true than T_1. I will not here consider the merits of unification as an indicator of truth. Many have thought that it is, and this may well be plausible, even in the idiosyncratic context of the present account. But even if there is virtue in unification, this escape will not work. For once you make an exception, everyone expects the same treatment. If T_2 can be more approximately true than T_1 despite the fact that it fares less well on Smith's criterion of greater geometrical closeness, then this criterion cannot be used to determine which other theories – those with which T_2 might be unified – are more or less approximately true, without taking into account the approximate truth of theories with which *they* might be unified, and this is plainly circular.

The kinds of assertions required to get truth talk going in the realist camp, having to do with descriptions of (aspects of) models and their relation to phenomena, are in excess of those employed by Smith's account of approximate truth. There would have to be more to such an account before realists would speak of truth. This brings us back once again to the issues that prompted our consideration of the implications of the semantic view for scientific realism. The assertions of the realist mentioned here in connection with approximate truth are those very same assertions with which the realist differentiates herself from those satisfied with lesser epistemic commitments. In formulating these assertions, she opens the door to concerns traditionally associated with realism – problems of interpretation, reference and correspondence – that are part and parcel of the use of linguistic devices. The semantic view seeks to separate theories from language, but scientific realism cannot be entertained on too strict a separation. Some model theorists have missed this point, for they do not take seriously the question of what realism requires.

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NOTES

[1] The precise formulation of these extra-linguistic 'structures' varies according to which advocate of the semantic view one consults. Some prefer the device of set-theoretic predicates (Suppes, Sneed), others prefer state spaces (Beth, van Fraassen), and yet others relational systems (Suppe). For a summary of these differences, see the Prologue to Suppe (1989). Leaving the details aside, I will use the generic term 'model' in connection with the semantic view.

[2] See Giere (1988, 47–8).

[3] See French and Ladyman (1999, 114–8).

[4] 'Different languages' here might refer to different natural languages, or technical ones, such as in the case of classical particle mechanics which can be given a Lagrangian or a Hamiltonian formulation, but is, presumably, the same theory in either case.

[5] Consider, for example, the description of Carnap's work on 'Q-predicates' given by Niiniluoto (1998). For a recent treatment of propositions that incorporate syntactical structure, see King (1995). This distinction between syntax-free and syntax-bearing propositions was suggested to me by Paul Teller.

[6] Given that syntactically different linguistic formulations can specify the same models, one might wonder about the motivation for the semantic view. Why focus on syntactic *in*equivalence when we can insist on equivalence of non-syntactic theoretical content?

[7] See Suppe (1989, 82–3, 94–9). McMullin (1985) makes a distinction between 'causal idealization' and 'construct idealization' that I take to mirror the one presented here between abstraction and idealization. Cartwright's (1983, 1989, ch. 5) characterization of abstraction and idealization is similar, but serves a different purpose, and thus differs somewhat from the present account. For a critique of Cartwright's position, see French and Ladyman (1997, 1998).

[8] Consider, for example, van Fraassen's (1985, 289) remarks to this effect.

[9] The modality introduced by claiming that causally possible physical systems 'could' describe the world seems to imply some sort of nomic or natural necessity where phenomenal systems are concerned. In other words, logically possible physical systems correspond to logically possible phenomena, causally possible physical systems to nomically possible phenomena.

[10] Suppe would not, of course, contest the fact that quasi-realism makes commitments that are not co-extensive with those of various forms of scientific realism. His account is

intended as an alternative kind of realism. The point is rather that nothing resembling any traditional form of scientific realism is facilitated by Suppe's account.

[11] For the details leading to this problematic consequence, see Suppe (1989, 99–100, 154–5).

[12] Recalling my earlier comments about propositions, it is worth noting that on Giere's view hypotheses are 'statements', by which he refers to propositions, construed as abstract (in the sense that different sentences, say in different languages, might express the same proposition), but nonetheless linguistic entities. See Giere (1988, 285–6, fn. 5). My criticism of Giere's account does not turn on the issue of whether or not statements and propositions are the same metaphysical item.

[13] See Laudan (1981).

REFERENCES

Cartwright, N.: 1983, *How the Laws of Physics Lie*, Oxford University Press, Oxford.

Cartwright, N.: 1989, *Nature's Capacities and Their Measurement*, Oxford University Press, Oxford.

French, F. and J. Ladyman: 1997, 'Superconductivity and Structures: Revisiting the London Account', *Studies in History and Philosophy of Science* **28**, 363–393.

French, F. and J. Ladyman: 1998, 'Semantic Perspective on Idealization in Quantum Mechanics', *Poznan Studies in the Philosophy of Science and the Humanities* **63**, 51–73.

French, F. and J. Ladyman: 1999, 'Reinflating the Semantic Approach', *International Studies in the Philosophy of Science* **13**, 103–121.

Giere, R. N.: 1984, 'Toward a Unified Theory of Science', in J. T. Cushing, C. F. Delaney and G. M. Gutting (eds), *Science and Reality: Recent Work in the Philosophy of Science – Essays in Honour of Ernan McMullin*, University of Notre Dame Press, Notre Dame, pp. 5–31.

Giere, R. N.: 1988, *Explaining Science – A Cognitive Approach*, University of Chicago Press, London.

Giere, R. N.: 1996, 'Visual Models and Scientific Judgement', in B. Baigrie (ed.), *Picturing Knowledge: Historical and Philosophical Problems Concerning the Use of Art in Science*, University of Toronto Press, London, pp. 269–302.

King, J. C.: 1995, 'Structured Propositions and Complex Predicates', *Noûs* **29**, 516–535.

Laudan, L.: 1981, 'A Confutation of Convergent Realism', *Philosophy of Science* **48**, 19–48.

McMullin, E.: 1985, 'Galilean Idealization', *Studies in History and Philosophy of Science* **16**, 247–273.

Niiniluoto, I.: 1998, 'Verisimilitude: The Third Period', *British Journal for the Philosophy of Science* **49**, 1–29.

Smith, P.: 1998a, 'Approximate Truth and Dynamical Theories', *British Journal for the Philosophy of Science* **49**, 253–277.

Smith, P.: 1998b, *Explaining Chaos*, Cambridge University Press, Cambridge.

Suppe, F.: 1989, *The Semantic Conception of Theories and Scientific Realism*, University of Illinois Press, Chicago.

van Frassen, B. C.: 1985, 'Empiricism in the Philosophy of Science', in P. M. Churchland and C. A. Hooker (eds), *Images of Science: Essays on Realism and Empiricism, with a Reply from Bas C. van Fraassen*, University of Chicago Press, London, pp. 245–308.

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