Scientific Representation and the Semantic View of Theories

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ABSTRACT: It is now part and parcel of the official philosophical wisdom that models are essential to the acquisition and organisation of scientific knowledge. It is also generally accepted that most models represent their target systems in one way or another. But what does it mean for a model to represent its target system? I begin by introducing three conundrums that a theory of scientific representation has to come to terms with and then address the question of whether the semantic view of theories, which is the currently most widely accepted account of theories and models, provides us with adequate answers to these questions. After having argued in some detail that it does not, I conclude by pointing out in what direction a tenable account of scientific representation might be sought.

Keywords: Scientific representation, models, semantic view of theories, isomorphism, similarity.

1. Introduction

Models are of central importance in many scientific contexts, where they play an essential role in the acquisition and organisation of scientific knowledge. We often study a model to discover features of the thing it stands for. For instance, we study the nature of the hydrogen atom, the dynamics of populations, or the behaviour of polymers by studying their respective models. But for this to be possible models must be representational. A model can instruct us about the nature of reality only if we assume that it represents the selected part or aspect of the world that we investigate.[1] So if we want to understand how we learn from models, we have to come to terms with the question of how they represent.

Although many philosophers, realists and antirealists alike, agree with a characterisation of science as an activity aiming at representing parts of the world,[2] the issue of scientific representation has not attracted much attention in analytical philosophy of science until recently. So the first step towards a satisfactory account of scientific representation is to be clear on the questions that such an account is supposed to deal with and on what would count as satisfactory answers. I address this issue in the next section. In the remainder of the paper I discuss currently available accounts of theories and models and argue that, whatever their merits on other counts, they do not provide us with satisfactory answers to the problems a theory of representation has to solve.

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[1] This is not to say that models are 'mirror images' or 'transcripts' of nature. Representing need not (and usually does not) amount to copying.

[2] Cartwright (1999, esp. Ch. 8), Giere (1988; 1999; 2004), Hughes (1997), Kitcher (1993), Morgan and Morrison (1999), Psillos (1999), Redhead (2001), Suppe (1989), van Fraassen (1980; 1997; 2002), to mention just a few.

2. The Three Conundrums of Scientific Representation

A theory of scientific representation has to come to terms with (at least) three conundrums. The first one is the ontology of models: what kinds of objects are models? Are they structures in the sense of set theory, fictional entities, concrete objects, descriptions, equations or yet something else? I refer to this issue as the '*ontological puzzle*'.

The second and the third conundrum are concerned with the semantics of models. Models are representations of a selected part or aspect of the world (henceforth 'target system'). But in virtue of what is a model a representation of something else?[3] To appreciate the thrust of the question it is helpful to consider the analogous problem with pictorial representation, which Flint Schier eloquently dubbed the 'enigma of depiction' (1986, 1). When seeing, say, Pissarro's *Boulevard des Italiens* we immediately realise that it depicts one of the glamorous streets of *fin de siècle* Paris. Why is this? The symbolist painter Maurice Denis famously took wicked pleasure in reminding his fellow artists that a painting, before being a nude or a landscape, essentially is a flat surface covered with paint, a welter of lines, dots, curves, shapes, and colours. The puzzle then is this: how do lines and dots represent something outside the picture frame? Slightly altering Schier's congenial phrase, I refer to the problem of how models represent their targets as the '*enigma of representation*' ('enigma', for short).

The third conundrum is what I call the '*problem of style*', which comes in a factual and a normative variant. Not all representations are of the same kind. In painting this is so obvious that it hardly deserves mention. An ink drawing, a wood cut, a pointillist painting, or a geometrical abstraction can represent the same scene in very different ways. This pluralism is not a prerogative of the fine arts. The representations used in the sciences are not all of the same kind either. Bill Phillips' hydraulic machine and Hicks' mathematical models both represent a Keynesian economy but they use very different devices to do so; and Weizsäcker's liquid drop model represents the nucleus of an atom in a manner that is very different from the one in the shell model. As in painting, there seems to be a variety of representational styles in science. But what are these styles (or 'modes of representation')? A theory of representation has to come up with a taxonomy of different styles and provide us with a characterisation of each of them. This is the factual aspect of the problem of style.

A further aspect of the problem of style is the normative question of whether there is a distinction between scientifically acceptable and unacceptable styles. One might be willing to grant that there are different representational strategies but still hold that only some of them truly deserve the label 'scientific'. Are there any constraints on the choice of styles of representation in science?

In sum, a theory of representation has to come to terms with three conundrums, two semantic, and one ontological. I do not claim that this list is exhaustive; but I

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[3] One could render this question more precise by asking 'what fills the blank in "*M* is a scientific representation of *T* iff ___", where "*M*" stands for "model" and "*T*" for "target system"?'. However, it is not obvious that there are necessary and sufficient conditions to be had here and it does not seem appropriate to regard an account of representation as successful only if it provides such conditions.

think that whatever list of questions one might put on the agenda of a theory of scientific representation, these three will be among them and they will occupy centre stage in the discussion.

Many answers to these questions are in principle possible and it is far from clear what would count as an acceptable theory of scientific representation. But there are (at least) two requirements that any such theory should satisfy.

First, learning from models. Scientific models represent things in a way that allows us to acquire knowledge about them. We study a model and thereby discover features of the thing it stands for. Every acceptable theory of scientific representation has to account for this interplay between knowing and representing.[4]

Second, the possibility of misrepresentation. A tenable theory of scientific representation has to be able to explain how misrepresentation is possible.[5] Misrepresentation is common in science. Some cases of misrepresentation are, for all we know, plain mistakes (e.g. ether models). But not all misrepresentations involve error. Many models are based on idealising assumptions of which we know that they are false. Nevertheless these models are representations. A theory that makes the phenomenon of misrepresentation mysterious or impossible must be inadequate.

Where do we stand on these issues? Over the last four decades the semantic view of theories has become the orthodox view on models and theories. Although it has not explicitly been put forward as an account of scientific representation, representation-talk is ubiquitous in the literature on the semantic view and its central contentions clearly bear on the issue. So it seems to be a natural starting point to ask whether the semantic view provides us with adequate answers to the above questions. I argue that it does not. Whatever the semantic view may have to offer with regards to other issues, it does not serve as a theory of scientific representation.

3. The Structuralist Conception of Models

There are two versions of the semantic view of theories, one based on the notion of structural isomorphism and one based on similarity. I will now focus on the former and return to the latter in section 8.

At the core of the first version of the semantic view lies the notion that models are structures. A structure $S = <U, O, R>$ is a composite entity consisting of (i) a non-empty set U of individuals called the domain (or universe) of the structure S, (ii) an indexed set (i.e. an ordered list) O of operations on U (which may be empty), and (iii) a non-empty indexed set R of relations on U. In what follows I will omit opera-

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[4] This is in line with Morgan and Morrison who regard models as 'investigative tools' (1999, 11) and Swoyer who argues that they have to allow for what he calls 'surrogative reasoning' (1991, 449).

[5] This condition is adapted from Stich and Warfield (1994, 6-7), who suggest that a theory of mental representation should be able to account for misrepresentation.

tions and take structures to be a domain endowed with certain relations. This is can be done without loss of generality because operations reduce to relations.[6]

For what follows it is important to be clear on what we mean by 'individual' and 'relation' in this context. To define the domain of a structure it does not matter what the individuals are —they may be whatever. The only thing that matters from a structural point of view is that there are so and so many of them.[7] Or to put it another way, all we need is dummies or placeholders.

Relations are understood in a similarly 'deflationary' way. It is not important what the relation 'in itself' is; all that matters is between which objects it holds. For this reason, a relation is specified purely extensionally, that is, as class of ordered n-tuples and the relation is assumed to be nothing over and above this class of ordered tuples. Thus understood, relations have no properties other than those that derive from this extensional characterisation, such as transitivity, reflexivity, symmetry, etc.

This leaves us with a notion of structure containing dummy-objects between which purely extensionally defined relations hold.[8]

The crucial move is to postulate that scientific models are structures in exactly this sense. In this vein Suppes declares that 'the meaning of the concept of model is the same in mathematics and the empirical sciences' (1960a, 12). Van Fraassen posits that a 'scientific theory gives us a family of models to represent the phenomena', that '[t]hese models are mathematical entities, so all they have is structure [...]' (1997, 528-99) and that therefore '[s]cience is [...] interpreted as saying that the entities stand in relations which are transitive, reflexive, etc. but as giving no further clue as to what those relations are' (1997, 516). Redhead claims that 'it is this abstract structure associated with physical reality that science aims, and to some extent succeeds, to uncover [...]' (2001, 75). And French and Ladyman affirm that 'the specific material of the models is irrelevant; rather it is the structural representation [...] which is important' (1999, 109).[9]

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[6] See Boolos and Jeffrey (1989, 98-99). Basically the point is that an operation taking n arguments is equivalent to a $n + 1$ place relation.

[7] This point is clearly stated in Russell (1919, 60).

[8] There is a controversy over whether these structures are Platonic entities, equivalence classes, or modal constructs. For what follows it does not matter what stance one takes on this issue. See Dummett (1991, 295ff.), Hellman (1989), Redhead (2001), Resnik (1997), and Shapiro (2000, Ch. 10) for different views on this issue.

[9] Further explicit statements of this view include: Da Costa and French (1990, 249), Suppes (1960b, 24; 1970, Ch. 2 pp. 6, 9, 13, 29), and van Fraassen (1980, 43, 64; 1991, 483; 1995, 6; 1997, 516, 522; 2001, 32-3). This is not to deny that there are differences between different versions of the semantic view. The precise formulation of what these models are varies from author to author. A survey of the different positions can be found in Suppe (1989, 3-37). How these accounts differ from one another is an interesting issue, but for present purposes nothing hinges on it. As Da Costa and French (2000, 119) —correctly, I think— remark, '[i]t is important to recall that at the heart of this approach [i.e. the semantic approach as advocated by van Fraassen, Giere, Hughes, Lloyd, Thompson, and Suppe] lies the fundamental point that theories [construed as families of models] are to be regarded as *structures*' (original emphasis).

In keeping faithful to the spirit of this take on models, proponents of the semantic view posit that the relation between a model and its target system is isomorphism. As I mentioned at the beginning, the semantic view has not explicitly been put forward as a theory of representation.[10] But given the general outlook of this approach, one might plausibly attribute to it the following account of representation:

> (SM)   A scientific model S is a structure and it represents the target system T iff T is structurally isomorphic to S.[11]

I refer to this as the *structuralist view of models*. This view comes in grades of refinement and sophistication. What I have presented so far is its simplest form. The leading idea behind its ramifications is to replace isomorphisms by less restrictive mappings such as embeddings, partial isomorphisms, or homomorphisms. This undoubtedly has many technical advantages, but it does not lessen any of the serious difficulties that attach to (SM). For this reason, I consider the structuralist view in its simplest form throughout and confine my discussion of these ramifications to section 8, where I spell out how the various shortcomings of (SM) surface in the different ramified versions.

The question we have to address is whether (SM) provides us with a satisfactory answer to the three conundrums of scientific representation. The bulk of my discussion will be concerned with the enigma (sections 4 to 6) and I argue that (SM) is inadequate as a response to this problem. In section 7 I discuss the problem of style and conclude that (SM) fares only marginally better when understood as an answer to this problem. And what about the ontological claim? Are models structures? As I point out in section 9, it is a by-product of the discussion in sections 4-6 that this is not tenable either. Models involve, but are not reducible to structures.

4. *Structuralism and the Enigma I: Isomorphism Is Not Representation*

The arguments against (SM) as an answer to the enigma fall into two groups. Criticisms belonging to the first group, which I will be dealing with in this section, aim to show that scientific representation cannot be explained in terms of isomorphism. Arguments belonging to the second group regard the very notion of there being an isomorphism between model and target as problematic and conclude that in order to make sense of isomorphism claims structuralists have to tack on elements to their account of representation that they did not hitherto allow for. I discuss these objections in sections 5 and 6.

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[10] Recently, van Fraassen (2004) and French (2003) have paid some attention to the issue of representation from the perspective of semantic view of theories. However, no systematic account of representation emerges from their discussions.

[11] This view is extrapolated from van Fraassen (1980, Ch. 3; 1989, Ch. 9; 1997), French and Ladyman (1999), Da Costa and French (1990), French (2000), and Bueno (1997 and 1999), among others. Van Fraassen, however, adds pragmatic requirements —I shall come to these below.

The first and simple reason why representation cannot be explained in terms of isomorphism is that the latter has the wrong formal properties: isomorphism is symmetric and reflexive while representation is not.[12]

Furthermore, structural isomorphism is not sufficient for representation because in many cases neither one of a pair of isomorphic objects represents the other. Two copies of the same photograph, for instance, are isomorphic to one another but neither is a representation of the other.[13] A corollary of this is that (SM) is unable to correctly fix the extension of a representation. It is a matter of fact that the same structure can be instantiated in different systems. For instance, a pendulum and certain kinds of electric circuits instantiate the same structure (Kroes 1989). In cases like this the model of the pendulum is isomorphic both to the pendulum and to the circuit. But it only represents the pendulum and not the circuit. Hence, isomorphism is too inclusive a concept to account for representation.

These criticisms suggest that (SM) is overly 'purist' in stipulating that representation has to be accounted for *solely* in terms of isomorphism, as all these problems vanish when we include intentional users in the definition of representation:[14]

> (SM')   The structure S represents the target system T iff T is structurally isomorphic to S and S is intended by a user to represent T.

This appears to be a successful move since (SM') is not vulnerable to the above objections. However, the move is so straightforward that it should make us suspicious. I agree that users are play an essential role in scientific representation; but merely tacking on intentions as a further condition is question begging. To say S is turned into a representation because a scientist intends S to represent T is a paraphrase of the problem rather than a solution. Consider an analogous problem in the philosophy of language: by virtue of what do some words refer? Merely saying that speakers intend words to refer to this or that is not an answer. Of course they do. What we really want to know is what is involved in a speaker establishing reference and a good deal of philosophy of language is an attempt to come to terms with this question. So what we have to understand is how a scientist comes to use S as a representation of T and to this end much more is needed than a blunt appeal to intentions.

Moreover, when we look at how (SM') solves the above-mentioned problems we realise that isomorphism has become irrelevant in explaining why S represents T as it is the appeal to intention that does all the work. Rather, isomorphism regulates the way in which the model has to relate to its target. Such regimentation is needed because an account of representation solely based on intention allows that everything

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[12] This argument has been levelled against the similarity theory of pictorial representation by Goodman (1968, 4-5) and has recently been put forward against the isomorphism view by Suárez (2003).

[13] This problem cannot be solved by requiring that models are structures and that targets are objects in the world because some models represent other models just as some pictures represent other pictures.

[14] This is explicitly held by van Fraassen (1994, 170; 1997, 523 and 525).

can represent just about everything else by a mere act of *fiat*, which cannot be right as on such account we cannot explain how we learn from a model about the target.

However, when used in this way, isomorphism is put forward as an answer to the problem of style rather than the enigma: it imposes constraints on what kinds of representations are admissible but it does not contribute to explaining where a model's representational power comes from. Whether isomorphism is a sensible constraint to impose on the way in which a model represents will be discussed in section 7.

5. Structuralism and the Engima II: The Abstractness of Structural Claims

Isomorphism is a relation that holds between two structures and not between a structure and a piece of the real world *per se*. Hence, if we are to make sense of the claim that model and target are isomorphic we have to assume that the target exhibits a structure. What is involved in this assumption? Using a particular notion of abstraction I argue that structural claims do not 'stand on their own' in that a structure S can represent a system T only with respect to a certain description. As a consequence, descriptions cannot be omitted from an analysis of scientific representation and one has to recognise that scientific representation cannot be explained solely in terms of structures and isomorphism.

Some concepts are more abstract than others. *Playing a game* is more abstract than *playing chess* or *playing soccer* and *travelling* is more abstract than *sitting in the train* or *riding a bicycle*. What is it for one concept to be more abstract than another? Cartwright (1999, 39) provides us with two conditions:

> First, a concept that is abstract relative to another more concrete set of descriptions never applies unless one of the more concrete descriptions also applies. These are the descriptions that can be used to "fit out" the abstract description on any given occasion. Second, satisfying the associated concrete description that applies on a particular occasion is what satisfying the abstract description consists in on that occasion.

Consider the example of *travelling*. The first condition says that unless I either sit in the train, drive a car, or pursue some other activity that brings me from one place to another I am not travelling. The second condition says that my sitting in a train right now *is* what my travelling consists in.

I now argue that *possessing a structure* is abstract in exactly this sense and it therefore does not apply without some more concrete concepts applying as well.

What is needed for something to have a certain structure S is that it consists of a set of individuals and that these enter into certain relations. Trivially, this implies that for it to be the case that *possessing a structure* applies to a system, *being an individual* must apply to some of its parts and *standing in a relation* to some of these. The crucial thing to realise at this point is that *being an individual* and *being in a relation* are abstract on the model of *playing a game*.

The applicability of *being an individual* depends on whether other concepts apply as well. What these concepts are depends on the context and the kinds of things we are dealing with (physical objects, persons, social units, etc). This does not matter; the salient point is that whatever the circumstances, there are *some* notions that have to apply

in order for something to be an individual. As an example consider ordinary medium-size physical objects. A minimal condition for such a thing to be an individual is that it occupies a certain space-time region. For this to be the case it must have a surface with a shape that sets it off from its environment. This surface in turn is defined by properties such as impenetrability, visibility, having a certain texture, etc. If we change scale, other properties may become relevant; but in principle nothing changes: we need certain more concrete properties to obtain in order for something to be an individual. If something is neither visible nor possesses shape, mass, or charge, then it cannot be treated as an individual.

And similarly with *being in a relation*. For it to be the case that two objects enter into a relation it also has to be the case that, say, one is hotter than, greater than or more beautiful than the other. In other words, *being in a relation* only applies if either *being hotter than*, *being greater than*, or *being more beautiful than* applies as well. *Being hotter than* is what being in a relation consists in on a particular occasion. Therefore, *being in a relation* is abstract in the above sense.

From this I conclude that *possessing a structure* does not apply unless some more concrete description of the target system applies as well. Naturally, this dependence on more concrete descriptions carries over to isomorphism claims. If we claim that T is isomorphic to S then, trivially, we assume that T has a structure S_T, which enters into the isomorphism with S. This assumption, however, presupposes that there is a more concrete description that is true of the system.

Let me end this section with a remark about supervenience. It may seem that the use of abstract concepts is somewhat far-fetched and that the same point could be made in a more elegant way by appeal to supervenience: structures supervene on certain non-structural base properties and hence one cannot have structures without also having these base properties. Details aside, I think that this is a valid point as far as the argument of this section goes. However, in the next section I argue that structures are not unique in the sense that the same object can exhibit different non-isomorphic structures. This is incompatible with supervenience because supervenience requires that any change in the structural properties be accompanied by a suitable change in the base properties. Abstraction does not require such a tight connection between structures and the concrete properties on which they rest.

6. Structuralism and the Enigma III: The Chimera of the One and Only Structure of Reality

The main contention of this section is that a target system does not have a unique structure; depending on how we describe the system it exhibits different, non-isomorphic structures. If a system is to have a structure it has to be made up of individuals and relations. But the physical world does not come sliced up with the pieces having labels on their sleeves saying 'this is an individual' or 'this is a relation'.[15] What we recognise as individuals and what relations hold between these depends, in part at
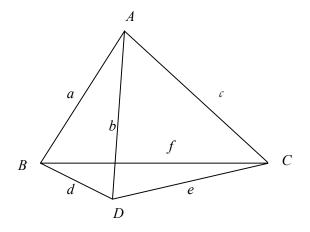
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[15] And even if there is something like an 'ultimate' structure of reality, it is not this structure that most scientific models aim to represent.

least, on how we conceptualise the system. Because different conceptualisations may result in different structures there is no such thing as the one and only structure of a system. Needless to say, there are ways of 'cutting up' a system that seem simple and 'natural', while others may seem contrived. But what seems contrived from one angle may seem simple from another one and from the viewpoint of a theory of scientific representation any is as good as any other.[16, 17]

My argument in support of this claim is inductive, as it were. In what follows I discuss examples from different contexts and show how the structure of the system depends on the description we choose. These examples are chosen such that the imposition of different structures only relies on very general features of the systems (e.g. their shape). For this reason, it is easy to carry over the strategies used to other cases. From this I conclude that there is at least a vast class of systems for which my claims bear out, and that is all I need.

The methane molecule (CH_4) consists of four hydrogen atoms forming a regular tetrahedron (see the figure below) and a carbon atom located in the middle. In many scientific contexts (e.g. collisions or the behaviour of a molecule *vis a vis* a semipermeable membrane) only the shape of the molecule is relevant. What is the structure of the shape of methane?

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[16] This position is compatible with, but does neither presuppose nor imply any form of metaphysical anti-realism or internal realism. I am only arguing for the much weaker claim that things do not have a unique structure.

[17] This point, though pulling in the same direction, is not equivalent to Newman's theorem, which, roughly, states that any set can be structured in any way one likes subject to cardinality constraints (Newman 1928, 144). This theorem is a formal result turning on the fact that relations are understood extensionally in set theory and that therefore a domain can be structured by putting objects into ordered *n*-tuples as one likes. What I argue is that a system can exhibit different *physically relevant* structures, i.e. structures that are not merely formal constructs but reflect the salient features of the system. I am aware of the fact that this is a somewhat vague characterisation and I rely on the subsequent examples to clarify the point.

To apply our notion of structure we need a set of individuals and relations.[18] It seems a natural choice to regard the vertices as objects and take the edges to define relations. As a result we obtain the structure T_V which consists of a four-object domain $\{A, B, C, D\}$ and the relation L (Lxy = 'x is connected to y by a line'), which has the extension $\{(A, B), (A, C), (A, D), (B, C), (B, D), (C, D)\}$.

However, this is neither the only possible nor the only natural choice. Why not consider the edges as the objects and the vertices as defining the relations? There is nothing in the nature of vertices that makes them more 'object-like' than edges. Following this idea we obtain the structure T_E with a domain consisting of the six edges $\{a, b, c, d, e, f\}$ and the relation I (Ixy = 'x and y intersect'), which has the extension $\{(a, b), (a, c), (a, d), (a, f), (b, c), (b, d), (b, e), (c, e), (c, f), (d, f), (d, e), (e, f)\}$.

The upshot of this is that methane exemplifies a certain structure only with respect to a certain description and that there is no such thing as *the* structure of methane. And this is by no means a peculiarity of this example. The argument only relies on general geometrical features of the shape of methane and can easily be carried over to other objects.

Another straightforward example illustrating my claim is the solar system, which only has the structure that we usually attribute to it[19] when we describe it as an entity consisting of ten perfectly spherical spinning tops with a spherical mass distribution. No doubt, this is a natural and in many respects useful way to describe this system, but it is by no means the only one. Why not consider the individual atoms in the system as basic entities? Or why not adopt a 'Polish' stance and also take the mereological sums of some planets as objects? There are many possibilities and each of these leads to a different structure.

The problem becomes even more pressing when we also take idealisations into account. As an example consider one of the earliest, and by now famous, ecological models. This model postulates that the growth of a population is given by the so-called logistic map: $x' = Rx(1 - x)$, where x is the population density in one generation and x' in the next; R is the growth rate. For this to be a representation, the structuralist has to claim that the structure S_L, which is defined by the logistic map, is isomorphic to the structure of the population under investigation. But this is only true when we describe this population in particular way. As Hofbauer and Sigmund (1998, 3) point out, in many ecosystems thousands of species interact in complex patterns depending on the effects of seasonal variations, age structure, spatial distribution and the like. Nothing of this is visible in the model. It is just the net effect of all interactions that is accounted for in the last term of the equation ($-Rx^2$). And a similarly radical move is needed when it comes to defining the objects of the structure. An obvious choice would be individual animals. But one readily realises that this would lead to intractable sets of equations. The 'smart' choice is to take generations rather than individual insects as objects. We furthermore have to assume that the generations are non-

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[18] This example is discussed in Rickart (1995, 23, 45).

[19] For a detailed discussion of this structure see Balzer *et al.* (1987, 29-34, 103-8, 180-91).

overlapping, reproduce at a constant average rate (reflected in the magnitude of *R*) and in equidistant discrete time steps. Hence we have to describe the system in this particular way for it to exhibit the structure we are dealing with; and if we choose different descriptions involving different modelling assumptions we obtain different structures.

To end the discussion of the enigma, let me briefly mention a possible objection: all I have said so far is wrongheaded from beginning to end because it misconstrues the nature of the target system. I have assumed that what a model represents is an object (or event) of some sort. But, so the objection goes, this is mistaken. What a model ultimately represents is a not an object, but a data model.[20] Space constraints prevent me from discussing this objection in detail, so let me just state that I think that this objection is wrong for the reason which Bogen and Woodward (1988) have pointed out: models represent phenomena, not data.[21]

7. Structuralism and the Problem of Style

So far I have argued that (SM) is untenable as a response to the enigma. Before drawing some general conclusions from this, I want to address the question of whether (SM) fares better as a response to the problem of style (this section) and argue that amended versions face, *mutatis mutandis*, the same difficulties (section 8).

The problem of style in its factual variant is concerned with modes of representation: what different ways of representing a target are there? For sure, isomorphism is one possible answer to this question; one way of representing a system is to come up with a model that is structurally isomorphic to it. This is an uncontroversial claim, but also not a very strong one.

The emphasis many structuralists place on isomorphism suggests that they do not regard it merely as one way among others to represent something. What they seem to have in mind is the stronger, normative contention that a representation *must* be of that sort.

This contention is mistaken. First, it is a common place that many representations are inaccurate in one way or another and as a consequence their structure is not isomorphic to the structure of their respective target systems. Second, it runs counter to the second condition of adequacy, namely that misrepresentation must be possible. To require that a model must be isomorphic to its target amounts to saying that only accurate representations count as representations and to ruling out cases of misrepresentation (i.e. cases in which isomorphism fails) as non-representational, which is unacceptable.

Structuralists may counter that this reading of the claim that representation involves isomorphism is too strong and argue that it is only something like a regulative ideal: as science progresses, its models have to become isomorphic to their target sys-

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[20] See van Fraassen (1981, 667; 1985, 271; 1989, 229; 1997, 524; 2001, 31; 2002, 164, 252) and French (1999, 191-92).

[21] See also Woodward (1989), McAllister (1997) and Teller (2001).

tems. This claim, however, falls outside the scope of a theory representation for it is just convergent realism in structuralist guise. But questions concerning realism or anti-realism should be kept apart from the issue of scientific representation. Convergent realism is a position one can hold, but as a view on representation it is besides the point. Representations can be realistic, but they do not have to be. Scientific modelling does not always amount to pointing a mirror towards things and making convergent realism a part of a theory of representation is neither necessary nor desirable.

8. Why Other Accounts Do Not Fare Better

The leading idea of amended versions of (SM) is to relax the isomorphism require-ment and use a less restrictive mapping. Some prominent suggestions include embed-ding (Redhead 2001), homomorphism (Mundy 1986), and partial structures (French and Ladyman 1999).

Whatever advantages these mappings enjoy over isomorphism in other contexts, it is not difficult to see that none of them resolves any of the above-mentioned difficul-ties. In order to set up any of these mappings between the model and the target we have to assume that the target exemplifies a certain structure and therefore these views are also subject to the criticisms levelled against (SM) in sections 5 and 6. And with regards to the problems mentioned in section 4 amended versions fare only marginally better. None of these mappings is necessary for representation as there can be many objects that are, say, homomorphic to one another without one being a representation of the other. It is only with respect to the first objection —that isomorphism has the wrong formal properties— that other mappings fare better because they can evade some of isomorphism's difficulties (e.g. embeddings need not be symmetric). But this improvement is not sufficient to compensate for all other difficulties and so I con-clude that they do not provide us with a satisfactory answer to the enigma. And the same goes for the problem of style. As isomorophism, they can be a good answer to the factual variant of the problem but it does not seem to be the case that all scientific representations conform to one of these patterns.

According to an alternative version of the semantic view, the relation between a model and its target is similarity rather than isomorphism (Giere 1988, Ch. 3; 1999; 2004). Accordingly we obtain: model M represents target system T iff M is similar to T.

This view imposes fewer restrictions on what counts as a scientific representation than the structuralist conception. First, it enjoys the advantage over the isomorphism view that it allows for models that are only approximately like their targets. Second, the similarity view is not committed to a particular ontology of models. Unlike the isomorphism view, it enjoys complete freedom in choosing its models to be whatever it wants them to be.

However, these advantages notwithstanding, the similarity view does not offer sat-isfactory answers to the above questions.

The problems similarity faces when understood as a response to the enigma by and large parallel those of isomorphism. It also has the wrong logical properties and it is

not necessary for representation. As isomorphism claims, similarity claims rest on descriptions, but for a different reason. In saying that M resembles T one gives very little away. It is a commonplace observation that everything resembles everything else in any number of ways (see Goodman 1972). The claim that M is similar to T remains empty until relevant respects and degrees of similarity have been specified, which we do with what Giere (1988, 81) calls a 'theoretical hypothesis', a linguistic item.

Similarity *per se* does not provide us with a satisfactory answer to the problem of style either. An unqualified similarity claim is empty; relevant respects and degrees need to be specified to make a similarity claim meaningful. So what we need is an account of scientifically relevant kinds of similarity, the contexts in which they are used, and the cognitive claims they support. Before we have specifications of that sort at hand, we have not satisfactorily solved the problem of style in either its normative or its descriptive variant.

What about Giere's ontological claim that models are abstract entities (1988, 81)? It is not entirely clear what Giere means by 'abstract entities', but his discussion of mechanical models suggests that he uses the term to designate fictional entities. To regard models as fictional entities is an interesting suggestion, but one that is in need of qualification. Fictional entities are beset with difficulties and in the wake of Quine's criticisms most analytical philosophers have adopted deflationary views. Can fictional entities be rendered benign, and if so how exactly are they used in science? This is an interesting and important problem but, as Fine (1993) has pointed out, one that has not received the attention it deserves.

Let me conclude this section with some remarks on accounts of modelling other than the ones suggested within the framework of the semantic view of theories. During the last two decades a considerable body of literature on scientific modelling has grown and one might wonder whether this literature bears answers the question that I have been raising in this paper. In the case of the enigma and the ontological puzzle this does not seem to be case. The questions of where the representational power of models comes from and what kind of objects models are have not received much attention.[22] With regards to the problem of style the situation is somewhat different. Debates over the nature of idealisation and the functioning of analogies have been prevalent for many years, and these can be understood as at least partially addressing the problem of style. The problem with the issue of style is a lack of systematisation rather than a lack of attention. Icons, idealisations and analogies are not normally discussed within one theoretical framework. As a consequence, we lack comparative categories that could tell us what features they share and in what respects they differ. What we are in need of is a systematic enquiry, which provides us with both a characterisation of individual styles and a comparison between them.

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[22] Noteworthy exceptions are Bailer-Jones (2003) and Suárez (2004); but no full-fledged account of representation has emerged yet from these discussions.

9. Outlook

In sections 5 and 6 I have argued that structural claims rest on more concrete descriptions of the target system. For this reason, descriptions are an integral part of any workable conception of scientific representation and we cannot omit them from our analysis. This is more than a friendly, but slightly pedantic and ultimately insignificant amendment to the structuralist programme; it casts doubt on a central dogma of the semantic view of theories, namely that models are non-linguistic entities. Models involve both non-linguistic and linguistic elements.[23]

If I am right on this, the face of discussions about scientific representation will have to change. In the wake of the anti-linguistic turn that replaced the syntactic view with the semantic view of theories questions concerning the use of language in science have been discredited as misguided and obsolete. This was too hasty a move. There is no doubt that the positivist analysis of theories is beset with serious problems and that certain non-linguistic elements such as structures do play an important role in scientific representation; but from this it does not follow that language *per se* is irrelevant to an analysis of scientific theories or models. Scientific representation involves an intricate mixture of linguistic and non-linguistic elements and what we have to come to understand is what this mixture is like and how the different parts integrate. What kinds of descriptions are employed in scientific representation and what role exactly do they play? What kinds of terms are used in these descriptions? These are but some of the questions that we need to address within the context of a theory of scientific representation. And this also seems to tie-in nicely with the conclusion of section 4, because the intentionality required for scientific representation seems to enter the scene via the descriptions scientists use to connect structures to reality.

A sceptic might reply that although there is nothing wrong with my claim that we need descriptions, there is not much of an issue here because what we are ultimately interested in is the isomorphism claim itself and that such a claim is made against the background of some description may be interesting to know but is ultimately insignificant. I disagree. Neat phrases like '*S* is isomorphic to *T* with respect to description *D*' are deceptive in that they make us believe that we understand how the interplay between structures, descriptions and the world works. This is wrong. These expressions are too vague to take us anywhere near something like an analysis of scientific representation and more needs to be said about how structures, targets and descriptions integrate into a consistent theory of representation.

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[23] Chakravartty (2001) has come to a very similar verdict, although based on a different argument.

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