Scientific Representation and the Semantic View of Theories[1]

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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 briefly explain why other accounts of scientific modelling do not fit the bill either and conclude by pointing out in what direction a tenable account of scientific representation has to be sought.

1. Introduction

Models are of central importance in many scientific contexts. The roles the bag model of quark confinement, the hard ball model of a gas, the Bohr model of the atom, the Gaussian-chain model of a polymer, the Lorenz model of the atmosphere, or the double helix model of DNA play in their respective domains are cases in point.

The importance of models is based on the fact that 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.[2] So if we want to understand how we learn from models, we have to come to terms with the question of how they represent.

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

Although many philosophers,[3] realists and antirealists alike, agree with a characterisation of science as an activity aiming at representing parts of the world, the issue of scientific representation has not attracted much attention in recent analytical philosophy of science. So the first step towards a satisfactory account of scientific representation is to get clear on the questions that such a theory is supposed to deal with and on what would count as a satisfactory answer. I address this issue in the next section and argue that there are three basic conundrums that such an account has to come to terms with. In the remainder of the paper I discuss currently available accounts of scientific theories and models and argue that, whatever their merits on other counts, they do not provide us with a satisfactory answer to any of the problems a theory of representation has to solve.

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? Or to render the question more precisely, what fills the blank in '*M* is a scientific representation of *T* iff ___', where '*M*' stands for 'model' and '*T*' for 'target system'? To appreciate the thrust of the question it is helpful to consider the analogue 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 battle horse, a nude, or some anecdote, essentially is a plane 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 a model represents its target as the '*enigma of representation*' ('enigma', for short).

The third conundrum is what one might call the '*problem of style*', which comes in a factual and a normative variant. Not all representations are of the same kind; there are different ways in which models can represent reality. 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, and yet they do so 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. Weizsäcker's liquid drop model represents the nucleus of an atom in a manner that is very different from the one of the shell model. A scale model of the wing of a plane represents the wing in a way that is different from how a mathematical model of its cross section does. Or Bill Phillips' famous hydraulic machine and Hicks' mathematical models both represent a Keynesian economy but they use very different devices to do so. As in painting, there seems to be a variety

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[3] Cartwright (1999, esp. Ch. 8), Giere (1988; 1999; 2002), Hughes (1997), Kitcher (1993), Morgan and Morrison (1999), Morgan (1999a), Morrison (1999), Psillos (1999), Redhead (2001), Suppe (1989), Suppes (1967; 2003), van Fraassen (1980; 1997; 2002), to mention just a few.

of devices the scientist can use. But it is not at all obvious what these devices are. This leaves us with the question: in what ways can and do scientific models represent? For want of better terms, I call the way in which a model represents its ‘style’, ‘mode of representation’ or ‘representational strategy’. 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 we can distinguish between scientifically acceptable and unacceptable modes of representation. 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’. So are there any constraints on the choice of modes of representation?

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 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 center stage in the discussion.[4]

Many answers to the above 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.

Learning from Models. Different representations serve different purposes. Some are devised to please the eye, others serve the purpose of communication, and again others are used as objects of religious devotion or means of ideological identification. In contrast to these, scientific representations function cognitively. Models do not merely stand for something beyond themselves; they represent things in a way that allows us to acquire knowledge about them. Knowledge about a part or aspect of the world is often gained by investigating the model that represents this part or aspect, because models are the units on which significant parts of scientific investigation are carried out rather than on reality itself. 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.[5]

The possibility of misrepresentation. A second general constraint on a theory of scientific representation is that it has to be able to explain how misrepresentation is possible.[6] 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. We often construct idealised or simplified models or build assumptions into our models of which we know that they are false. Despite this, these models are representations. Any 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 and 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

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[4] To frame the problem in this way is not to say that these three questions concern separate and unrelated issues. This division is analytical, not factual and it does not imply that an answer to one of these questions can be dissociated from what stance one takes on the other issues.

[5] 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).

[6] 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.

regard to other issues, it does not serve as a theory of scientific representation. And neither do other current accounts of scientific modelling. After having discussed the problems that attach to the semantic view in detail, I briefly revisit other views on modelling and point out that although some of them seem to offer promising outlooks, no full-grown answers to the above questions emerge from them.

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 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 O (i.e. an ordered list) 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, for the sake of simplicity, omit operations 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.[7]

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.[8] Or to put it another way, all we need is dummies or placeholders.

A similar 'deflationary' move is needed in the case of relations. 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.

This leaves us with a notion of structure that deals with dummy-objects between which purely extensionally defined relations hold.[9]

The crucial move now 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

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[7] See Boolos and Jeffrey (1989, 98-99) and Shapiro (1991, 63). Basically the point is that an operation taking *n* arguments is equivalent to a *n*+1 place relation.

[8] This is very clearly stated in Russell (1919, 60).

[9] 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.

models is irrelevant; rather it is the structural representation [...] which is important' (1999, 109).[10]

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.[11] 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*.[12]

I refer to this as the *structuralist view of models*.[13] This view comes in grades of refinement and sophistication. What I have presented so far is its simplest form. The leading idea behind all ramifications is to weaken the isomorphism requirement and 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 mistaken too. 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 the dealing with in this section, aim to show that

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[10] 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 the 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)

[11] Recently, Bas van Fraassen (2004) and Steven French (2002) have paid some attention to the issue of representation within the framework of the semantic view of theories. However, no systematic account of representation emerges from their discussions.

[12] This view is extrapolated from van Fraassen (1980, Ch. 2; 1989, Ch. 9; 1997), French and Ladyman (1999), French and Da Costa (1990), French (2000), and Bueno (1997 and 1999), among others. Van Fraassen, however, adds pragmatic requirements – I shall come to these below.

[13] This coincides with the terminology used by its advocates. While the term 'structuralism' has been used by authors in the Balzer-Moulines-Sneed tradition all along, it is now also used by other proponents of the semantic view (van Fraassen, DaCosta, French, Ladyman, and Bueno, see references above).

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.

4.1 Some straightforward objections

The first and simple reason why representation cannot be cashed out in terms of isomorphism is that the latter has the wrong formal properties: isomorphism is symmetric and reflexive while representation is not.[14]

Second, structural isomorphism is too inclusive a concept to account for representation. 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. Hence, isomorphism not sufficient for representation.[15]

Third, (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. Linear functions, for instance, are widely used in physics, economics, biology, and the mathematised parts of psychology. The $1/r^2$ law of Newtonian gravity is also the ‘mathematical skeleton’ of Coulomb’s law of electrostatic attraction and the weakening of sound or light as a function of the distance to the source. Harmonic oscillations are equally important in the context of classical mechanics and classical electrodynamics; and so on.

To see how this clashes with the representational power of models, we need to bear the following feature of representations in mind: models are representations of some particular target system. The target can be a token (as in the case of cosmological models) or a type (as in the case of models of the atom), but models are always representations of some specific physical phenomenon like an electric circuit, a falling object, magnetism in a solid, or an exploding star. This implies that the extension of a representation must be fixed correctly; a model of the hydrogen atom, say, has to represents hydrogen atoms and nothing else.

It is at this point that (SM) fails because it is unable to correctly fix the extension of a representation in cases in which the same structure can be instantiated in different systems. If a model is isomorphic to more than one kind of system instantiating the same structure, which one does it represent? We seem to be forced to conclude that it represents all of them, but this clashes with the fact that models are representation of a particular target.

4.2 (SM) and intentionality

Throughout these arguments against (SM) there was a temptation to counter that an appeal to observers would make the problems vanish. This suggests that (SM) is overly ‘purist’ in stipulating that representation has to be accounted for *uniquely* in terms of isomorphism and that what we really need for representation is intention. Remedy then seems easy to get: concede that

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[14] 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).

[15] One might counter that this critique is spurious since in the given set-up this problem cannot crop up. The models under consideration are structures and the target systems are objects in the world. Counterexamples of the aforementioned type can then be ruled out simply by introducing the ontological restriction that a model *must* be a structure and a target *must* be a concrete object (or process) in the world. Unfortunately this does not solve the problem. Though models often do represent things in the world, this is not necessarily so. Just as a picture can represent another picture, a model can represent another model rather than anything in the world.

representations are intentionally created and hold that structures only become models when someone uses them as such:[16]

> (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*.

At first glance, 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 an essential part of an account of representation, but merely tacking on the condition that someone intends to use *S* as a model of *T* is not enough.

First, it is question begging. Of course it is scientists who make representations. The question is how something that would not otherwise be a representation is turned into one by a user. What exactly does a scientist do when she uses *S* to represent *T*? If we are then told that she intends *S* to represent *T*, this is a paraphrase of the problem rather than a solution. To make the thrust of this criticism clear, consider an analogue problem in the philosophy of language: by virtue of what does a word refer? We do not solve this problem by merely saying that speakers intend the word to refer to certain things. Of course they do, but this by itself does not answer the question. What we want to know is how the speaker achieves reference and coming to terms with this puzzle is what philosophers of language try to do in theories of reference. The situation in the philosophy of science exactly parallels the one in the philosophy of language: 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 intention.

Second, when we look at how (SM’) solves the above-mentioned problems and accounts for why *S* represents *T* we realise that it is the appeal to intention that does all the work and that isomorphism has become irrelevant.

One may counter that something must be wrong with this because isomorphism certainly is doing some work in the above account. Agreed, it does, but not the work the counter expects it to do. Isomorphism regulates the way in which the model has to relate to its target. Such a regulation is needed because an account of representation solely based on intention is too liberal. On such an account, nothing prevents us from stipulating, for instance, that the dot I have just put on a piece paper is a representation of a carbon atom. But this is not enough to make a model. It is absurdities of that sort that are effectively undercut when isomorphism is added as a further requirement.

Now the problem becomes apparent: isomorphism is not put forward as a response to the enigma, but as one to the problem of style. The function isomorphism performs within (SM’) is to impose constraints on what kinds of representations are admissible but it does not contribute to explaining where the model’s representational poser comes from.

The bottom line is this: isomorphism is irrelevant to understanding how a model comes to represent something. Whether it 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: Structural Claims Are Abstract and Rest on More Concrete Descriptions

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

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 certain 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. *Game* is more abstract than *chess* or *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? Nancy 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 the concept *structure S* 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 is that it consists of a set of individuals and that these enter into a certain relational pattern. Trivially, this implies that the concept *individual* applies to some parts of the system and *relation of type x* to others (where 'type x' is a placeholder for a formal characterisation of the relation specifying, for instance, that it is transitive or symmetric). The crucial thing to realise at this point is, I maintain, that *individual* and *relation of type x* are abstract concepts on the model of *game*, or *travel*. To call something an individual or a relation is an abstract assertion relative to more concrete claims; and if it is true then it is true relative to more concrete truisms.

Consider *relation of type x*. Within the structuralist framework, relations are defined purely extensionally and have no properties other than those that derive from this extensional characterisation (i.e. transitivity, reflexivity, symmetry, etc.). Relations of this kind are abstract in the above sense. Take *transitive relation*, for instance. There are many transitive relations: *taller than*, *older than*, *hotter than*, *heavier than*, *stronger than*, *more expensive than*, *more recent than*, etc. (and their respective converses: *smaller than*, *younger than*, etc.). By itself, there is nothing worrying about that. However, what we have to realise is that *transitive relation* is true of a relation only if either *greater than*, or *older than*, or … is true of it as well. Something cannot be a transitive relation without also being one of the relations listed above. Being taller than, say, is what being a transitive relation consists in on a particular occasion. There simply is no such thing in the physical world as a relation that is nothing but transitive.

And similarly in the case of *individual*, whose applicability also depends on whether other concepts apply as well. What these concepts are depends on contextual factors 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 things. A minimal condition for such a thing to be an individual is that it occupies a certain space-time region. For this to be that 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; for instance having mass, or charge. 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.

From this I conclude that *structure S* 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 the T is isomorphic 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 might seem that the use of abstract concepts is somewhat far-fetched and that the same point could be made much easier by employing 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 – but only as far as this. In the next section I argue that structures are not unique in the sense that the same object can exhibit different non-isomorophic 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 Engima 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 bearing labels saying ‘this is an individual’ or ‘this is a relation ’. What we recognise as individuals and what relations hold between these depends, in part at least, on what scheme we employ for ‘cutting up’ the system. But different schemes may result in different structures. So there is no such thing as the one and only structure of a target system and a system has a determinate structure only relative to a certain description. Needless to say, there are ways of ‘cutting up’ a system that seem simple and ‘natural’, while others may be rather 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.[17, 18]

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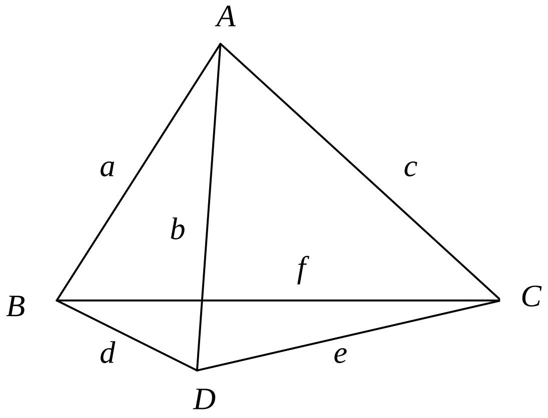
[17] This position is compatible with, but does neither presuppose nor imply any form of metaphysical antirealism or internal realism. I am only arguing for the much weaker claim that things do not have one, and only one, structure.

[18] 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 what I have in mind.

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 a 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 at stake (e.g. their shape). For this reason, it is easy to carry over the strategies used to other cases as well. 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 and a carbon located in its 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 a tetrahedron?



To apply our notion of structure we need a set of basic objects and relations on it.[19] A natural choice seems to regard the corners (vertices) as the objects and the lines that connect the vertices (the edges) as the 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* (*Lx*y = '*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 lines as the objects and the vertices as the relations? There is nothing in the nature of vertices that makes them more 'object-like' than lines. Following this idea we obtain the structure T_S 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*)}.

The upshot of this is that we need to conceptualise certain parts of the tetrahedron as objects and others as relations before we can tell what its structure is. The tetrahedron exemplifies a certain structure only with respect to a certain description, namely one that specifies that the vertices are the individuals in the domain of the structure and the lines the relations, or vice versa. And with some ingenuity one might find yet other descriptions that give rise to structures other than T_S and T_V.

This example shows that there is no such thing as *the* structure of a tetrahedron. And this is by no means a peculiarity of this example. The argument only relies on general geometrical features of the shape of a molecule and therefore can easily be carried over to any kind of object consisting of lines (not even necessarily straight) that intersect at certain points.

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

Another straightforward example illustrating my claim is the solar system, which only has the structure that we usually attribute to it[20] 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 gives rise to a different structure.

As a final example an ecological model. One of the earliest, and by now famous, models has it 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. From a structuralist point of view one 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, which involves many substantial modelling assumptions. As Hofbauer und 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 large and intractable sets of equations. The 'smart' choice is to take generations rather then individual insects as objects. We furthermore have to assume that the generations are non-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 a different description (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.[21] 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 Jim Bogen and Jim Woodward (1988) have pointed out: it is not true that models represent data; they represent phenomena.[22]

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).

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

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

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

How does isomorphism fare as response to the problem of style? My answer to this question is sober as well. While isomorphism can be understood as one possible answer to the factual aspect of the problem, it is unacceptable as a normative stance.

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, I think, 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.

The claim that isomorphism is necessary for representation is mistaken. First, it is well-known that this claim is descriptively inadequate. 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 the second condition of adequacy, namely that misrepresentation must be possible. The leading idea behind the claim that a model must be isomorphic to its target is that only accurate representations count as scientific representations and that isomorphism provides us with a criterion for what counts as accurate. As a consequence, we have to rule out cases of misrepresentation 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 systems. This claim, however, falls outside the scope of a theory representation for it is just convergent realism in structuralist guise and questions concerning realism or antirealism should be kept apart from the issue of scientific representation. Of course, convergent realism is a time-honoured position one can hold, but as a view on representation it is beside 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. So making convergent realism a part of a theory of representation seems 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 requirement and use a less restrictive mapping instead. Some prominent suggestions include embedding (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 evades any of the difficulties that attach to the isomorphism version when it comes to issues in connection with representation. 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 so the arguments put forward against isomorphism in sections 5 and 6 equally apply. And also as regards the problems mentioned in section 4, less restrictive 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; and all these mappings fail to fix the extension of a model correctly for exactly the same reasons as isomorphism. Hence the second and the third of the above objections go unscathed. 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 reflexive). But this improvement is not sufficient to compensate for all other difficulties and so I conclude 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 fair 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). Accordingly we obtain: model *M* represents target system *T* iff *M* is similar to *T*.

This view imposes fewer restrictions on what is acceptable 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 the same as 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. And Giere indeed adopts a different ontology than the structuralists: he takes models to be fictional entities (1988, Ch. 3).

However, despite these advantages it does not offer satisfactory answers to the s questions introduced in section 2 either. When understood as a response to the enigma similarity does not fare better than isomorphism; it is empty when put forward as a response to the problem of style; and it needs to be qualified when taken as an ontological stance.

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, it is not necessary for representation, and it is not able to fix the extension of a model correctly. 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 that everything resembles everything else in any number of ways. 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’. But this hypothesis is a linguistic item.

Similarity *per se* does not provide us with a satisfactory answer to the problem of style either. To say that model and target are similar may amount to whatever; it is only after we have specified relevant respects and degrees that this claim has a determinate content. So it is these specifications that identify the relation into which the two enter. 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 our disposal, we have not satisfactorily solved the problem of style in either its normative or its descriptive variant.

What about the ontological claim that models are fictional entities? This is an interesting suggestion, but one that is in need of qualification. Fictional entities have a bad track record and in the wake of Quine’s criticisms most analytical philosophers have adopted deflationary views. Can fictional entities rendered benign, and if so how exactly are they used in science? This is an interesting and important problem but one, as Fine (1993) has pointed out in a programmatic essay, which 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. As far as I can see, the questions of where the representational power of models comes from and of what kind of objects models are have not been recognised, much less seriously discussed in this literature.

However, various discussions have been going on for a while which can be understood as at least partially addressing the problem of style. What I have in mind are debates about the nature of idealisation, the functioning of analogies, and the like. Although these issues have not been discussed within the context of a theory of scientific representation, it seems natural to understand idealisation, for instance, as one answer to the question 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 under one theoretical umbrella. 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 as well as a comparison between them. Needless to say, this is a research project in its own right that I cannot pursue here.

9. Outlook

In sections 5 and 6 I have argued that structural claims (and hence isomorphism 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 of representation. 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 came with the semantic view of theories questions concerning the use of language in science have been discredited as misguided and it was assumed that this confusion was resolved by replacing a linguistic understanding of theories – the so-called syntactic view advocated by the logical positivists – by a non-linguistic one, the semantic view. This is throwing out the baby with the bath water. 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 exactly this mixture is like and how the different part 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.

This 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. The question of what a scientist has to do in order to turn a structure into a representation of something beyond itself now becomes the question of what sort of description a scientist has to use to connect the structure and reality.

A sceptic might still reply that although there is nothing wrong with my claim that we need descriptions, there is not much of an issue here. What we are ultimately interested in, so the objection goes, is the isomorphism claim and that such a claim is made against the background of

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[23] A conclusion very similar to this has recently been reached by Anjan Chakravarrty (2001), but based on a different argument.

some description may be interesting to know, but is without any further significance. I disagree. Phrases like '*S* is isomorphic to *T* with respect to description *D*', '*S* is isomorphic to *T* relative to description *D*', or 'isomorphism claims operate against the background of description *D*' point in the right direction; but they are deceptive in that they might 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. More needs to be said about how structures, targets and descriptions integrate into a consistent theory of representation.

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