There Is No Special Problem About Scientific Representation[1]

Craig CALLENDER and Jonathan COHEN

BIBLID [0495-4548 (2006) 21: 55; pp. 67-85]

ABSTRACT: We propose that scientific representation is a special case of a more general notion of representation, and that the relatively well worked-out and plausible theories of the latter are directly applicable to the scientific special case. Construing scientific representation in this way makes the so-called "problem of scientific representation" look much less interesting than it has seemed to many, and suggests that some of the (hotly contested) debates in the literature are concerned with non-issues.

Key words: scientific representation, mental representation, models.

> ... important philosophical problems concerning language have been misconstrued as relating to the content of science and the nature of the world.
>
> van Fraassen 1980, 196.

1. Introduction

The harmonic oscillator, Ising model, and logistic map are typical representative structures used in science. In recent years the question of how such models can be about parts of the world has led to a burgeoning literature. Philosophers find it particularly puzzling how models, which commit sins of omission and commission by lacking and having features the world does and does not have, respectively, can nevertheless be about bits of the world. There are now a variety of different accounts of how scientific models represent, and of course, the usual philosophical squabbling over which one is right. It seems that a new philosophical problem has been discovered and philosophers of science have dutifully risen to the call.

Perhaps, however, they shouldn't have. For it is not clear that there is a special problem about scientific representation, as opposed to artistic, linguistic, and culinary representation. While philosophers have been quick to provide answers, few have spent time discussing the nature of the problem.

We'll undertake such an examination in this paper. We'll propose a more general framework in which to think about scientific representation that solves or dissolves the so-called "problem of scientific representation" while shedding light on many other questions surrounding scientific models. While the view we'll be advocating does not make all of the work on scientific representation insignificant, it does suggest that some of the debates in the literature are concerned with non-issues. Our framework re-orients much of this work, so that some of it survives if understood as answering a different question than one about the nature of scientific representation *per se*.

---

[1] This work is fully collaborative; the authors are listed alphabetically.

2. The Alleged Problem of Scientific Representation

Current work on scientific representation is best appreciated against the backdrop of developments in philosophical conceptions of scientific theories beginning in the 1960s. In this work, Patrick Suppes and others developed the so-called semantic view of scientific theories, according to which the whole class of semantic or meta-mathematical models of the theory provides its semantic content (Suppes 1967, 1969; van Fraassen 1980; Giere 1988). Whatever its virtues and vices, the semantic view made newly salient the problem of explaining the relationship between models and the world.[2]

Whether one understands models as abstract or concrete, abstractions from theory or not, many philosophers have worried that they are not the sorts of things that are truth-apt, or even approximate-truth-apt. Just as there seems to be something wrong with claiming that a toy model airplane is true or false, there seems something wrong with claiming that an Ising model, Bloch model, or logistic map is true or false. Yet, even if models (unlike propositions, sentences, etc.) are not, or are not always, truth-apt, they are about the world in some sense. Surely it is correct to say that models can represent the world. This situation invites us to ask a question that has become one of the strands in the alleged problem of scientific representation, and that we shall call 'the constitution question': what constitutes the representational relation between a model and the world?

Various answers have been proposed to the constitution question. For example, Giere seems to be offering one in saying that there is a relation of "fit" or "similarity" to some degree and in some respects between a model and the world (Giere 1988, 81), where the respects and degree are picked out by scientists' intentions in designing and using the model (Giere 1992, 122-123; but see note 7). Others instead think the relationship between model and the real world is one of isomorphism, partial isomorphism (French 2002), inference generation (Suárez 2003), and more.

In recent years, these issues have been woven closely together with related but distinct problems. For example, consider the "DDI" theory of representation of (Hughes 1997). Looking at Galileo's use of a geometric figure in solving a problem in kinematics, Hughes argues that scientific representation typically has elements of "denotation" (elements of the model, e.g., lines, denote phenomena), "demonstration" (one uses the model to get a result) and "interpretation" (the result is then interpreted physically). DDI is not meant to provide necessary and sufficient conditions for when a representation takes place; rather Hughes is "making the more modest suggestion that, if we examine a theoretical model with these three activities in mind, we shall achieve some insight into the kind of representation that it provides" (Hughes 1997, S329; see also S335).

Here it seems that Hughes is interested in distinguishing scientific from other sorts of representation —i.e., he is attempting to solve a kind of demarcation problem for

[2] This problem can also be raised for those "mediating models" theorists who hold that scientific models are to some extent independent of theory.

scientific representation. He claims that DDI will inform us (typically) about what kind of representation occurs; for example, it will distinguish (i.e., demarcate) Galileo's scientific scribbles from Vermeer's masterly strokes. Hughes also criticizes Giere's "similarity" theory by pointing out the seeming truism from Goodman (1976) that every pair of entities is similar in some respects and dissimilar in others. Since the 'D' of denotation is set against Giere's similarity account, it is tempting to conclude that at least one 'D' is supposed to be a solution to Giere's problem (see especially pp. 6-8; see also Hughes 1999, 126). As we read him, then, Hughes offers his DDI proposal as an answer to both the constitution question and the demarcation problem about representation.

The issues are complicated further by another problem that comes out in Morrison's characterization of "the heart of the problem of representation" as the question "in virtue of *what* do models represent and how do we identify what constitutes a correct representation?" (Morrison 2006). There are at least two distinct questions here. The 'in virtue of what' question clearly sounds like the constitution question we took Giere to be addressing. But the second half of the quotation introduces a distinct problem: the normative issue of what it is for a representation to be correct. Many writers in the "models as mediators" school have focused on the normative question of what makes some models explanatory (cf. Morrison and Morgan 1999). Morrison (1999) claims that the representational and explanatory capacities of a model are interconnected (40). Inasmuch as 'interconnected' means that the explanatory/normative questions presuppose answers to the constitutive ones, we agree. But we do not believe the two questions are any more deeply connected.

Our feeling is that many authors writing on models don't contrast these questions as sharply as they should. For example, Bailer-Jones (2003) demands an answer to the constitution problem, and criticizes DDI for failing to provide such an answer. But it is not clear that DDI is intended as an answer. The inference generation theory of Suárez (2003) is explicitly directed at the constitution question, but it is not clear that the views Suárez criticizes (e.g., that of van Fraassen) are directed at the same question. Other work —for instance, much of that featured in Morrison and Morgan (1999)— seems focused on the normative problem. Still others, e.g., Hughes, also want to tackle the demarcation problem. In our view, running these issues together is conducive to confusion.

We would be remiss if we didn't mention that much of the writing about models concentrates on the fact that models misrepresent in some respects. How can they represent if they, well, mis-represent? For instance, the Lorentz model of convection in the atmosphere misses out on a variety of features of the Earth's real convection patterns; the model ignores scores of parameters relevant to the atmosphere and makes a number of false assumptions. It only captures a very small piece of the dynamical behavior of air (see Smith 1998, 9-13). As another example, consider that a Hardy-Weinberg model of a rabbit population will assume there are an infinite number of rabbits (to rule out the possibility of genetic drift). That's a lot of rabbits. As a limiting case, many have worried that some models, like Bohr's of the atom, seem in some sense inconsistent (French 2002).

These kinds of problems have led philosophers to what we consider some pretty desperate measures. For example, Stephen French, a would-be defender of isomorphism, has retreated to the weaker claim that models must be partially isomorphic to the real world if they are to represent (French 2002). Likewise, the idealization and abstraction of models leads Bailer-Jones to the proposal that models entail certain propositions in some non-logical (and, as far as we can tell, magical) sense. An architect's plans for a bridge, just lying there on a desk, "entails" various propositions, according to her theory. Though her positive proposal is opaque to us, she is initially concerned with the ratio of true propositions to false propositions "entailed" by such objects as a way of saving the representational capacities of idealized models.

To those familiar with other theories of representation (and even those not), many of the concerns that seem to be driving such philosophical proposals may seem strange. To see why, consider a quotidian example of representation from outside science and notice how the questions analogous to those philosophers ask about scientific representation fail to get much of a grip. For example, consider the lowly stop sign. Are stop signs at intersections isomorphic or partially isomorphic to the imperative 'stop!' that they represent? Do they non-logically entail more true propositions than false ones? Taking another example, do the marks 'cat' in any way resemble real cats? Are philosophers of language worried that the marks 'cat' aren't furry or that cats lack constituents that are part of an alphabet? These questions about non-scientific representations strike us as bad ones, and we hope they strike you that way too. This suggests to us that there may well be something wrong with the questions being asked about scientific representation. Therefore, before further answers are given, we think it is high time to think a bit about what the questions are supposed to be.

3. Scientific Representation, Meet Philosophy of Mind

How are philosophers to understand scientific representation? Three *prima facie* plausible observations can guide us.

The first is that, in general, it is economical and natural to explain some types of representation in terms of other, more basic types of representation. We'll call this idea 'General Griceanism', as it amounts to a generalization of Grice's important views on representation. The General Gricean holds that, among the many sorts of representational entities (cars, cakes, equations, etc.), the representational status of most of them is derivative from the representational status of a privileged core of representations. The advertised benefit of this General Gricean approach to representation is that we won't need separate theories to account for artistic, linguistic, representation, and culinary representation; instead, the General Gricean proposes that all these types of representation can be explained (in a unified way) as deriving from some more fundamental sorts of representations, which are typically taken to be mental states. (Of course, this view requires an independently constituted theory of representation for the fundamental entities.)

The second observation is that, so long as we are in the General Gricean business of describing dependency relations among various sorts of representations, there is good reason to think that we should extend this treatment to scientific representations —i.e., that we should take the latter to be located somewhere in the web of dependency relations with other types of representations. After all, scientists routinely use entities other than models —language, pictures, mental states, and so on— to represent the very same targets that models represent. This coincidence of representational targets is explicable if (i) scientific representations get their representational status from linguistic (etc.) representations, or (ii) vice versa, or (iii) scientific representations and linguistic (etc.) representations get their representational status from some third sort of representation. But it would be surprising that scientific, linguistic, pictorial, mental, and other sorts of representations should coincide in their representational targets were they not at all related in the way that the General Gricean treatment indicates that they should be.

The third and final observation is that, if we distinguish derivative from fundamental representations, and are attempting to include scientific representations in the mix, it is reasonable to think that they belong among the derivative representations rather than the fundamental ones. For one thing, the distinction between science and non-science is famously elusive. Does Freud's model of the unconscious represent one way if Freud's theory is scientific and another (derivative) way or not at all if not? That seems unsatisfactory. Whether and how the model is about something shouldn't hang on this classification.

Now, General Griceanism is so general, as stated, that discussion of it is much more easily carried out by reference to Grice's specific version of the position that sometimes goes under the label 'intention-based semantics', and that we'll call 'Specific Griceanism' in order to contrast with General Griceanism. We'll advert to Specific Griceanism at times in what follows partly just to facilitate discussion, and partly as a way of showing that, since there are proposals about how to fill in the details, General Griceanism is not a mere promissory note. Despite this policy, however, we don't want to be committed too much to the (by comparison, controversial) details of Specific Griceanism, and so present the latter only as an example.

3.1. Explaining Representation

As noted, the General Gricean proposes to distinguish between fundamental and non-fundamental representation, and to explain the latter in terms of the former. The Specific Gricean version of this distinction is made between so-called natural representation and non-natural representation. Natural representations are those whose representational powers are constituted independently of the mental states of their users/makers; these would include the number of rings on a tree (representing the age of the tree), the presence of smoke (representing the concomitant presence of fire), and so on. Non-natural representations, by contrast, are produced by human beings for the purpose of communicating something to an audience; this class would include linguistic tokens, some artworks, pre-arranged signals, and the like. To a zeroth approximation, the Specific Gricean program attempts to explain representation by giving a reductive account of non-natural representation in terms of natural representation. The next step (about which Grice himself had relatively little to say) is to combine the latter reduction with a naturalistic, reductive account of natural representation, thereby providing a full, naturalistically acceptable, reductive account of representation.

At the risk of obscuring the generality of General Griceanism, it may help to consider the Specific Gricean explanation of linguistic representation. Grice clearly thinks linguistic tokens are non-natural representations, so he proposes to use the general strategy outlined above to explain what he calls 'speaker meaning' —i.e., what it is for a speaker *S* to mean something by uttering *U* in terms of his acting with the intention of producing a belief or action in a hearer *H*. That is, he hoped to give a theory of roughly this form:

> In uttering *U*, S means that *p* iff, for some *H*, *S* utters *U* intending in way ... to activate in *H* the belief that *p*.

Of course, the details of this Specific Gricean theory schema for speaker meaning are not without controversy (see Schiffer 1987, chapter 9). But the hope is that the theory will reduce the notion of speaker meaning for linguistic tokens to specific mental states of producers/hearers of these tokens —namely, the states of *S*'s intending to do something, and *H*'s believing that something else.

But the Specific Gricean's job is not finished until she provides an account of the representational contents of mental states. This question about the metaphysics of representation for the fundamental units of representation is currently the subject of intense philosophical controversy. However, there is a range of popular answers to the question that are available for use at this stage of the Gricean explanation.[3]

There are several points about the Specific Gricean explanation of the representational powers of linguistic tokens that bear emphasis, and that provide lessons for General Griceanism, once we abstract away from the Specific Gricean details. We pause to belabor them.

First, notice that the account divides naturally into two stages. The first stage of Specific Griceanism consists in explaining the representational powers of linguistic tokens in terms of the representational powers of something more fundamental —namely, mental states. In the second stage, the Specific Gricean needs some other story to explain representation for the fundamental bearers of content, mental states.

---

[3] Some of the most popular accounts of representation for mental states are functional role theories, informational theories, and teleological theories. A useful anthology is Stich and Warfield 1994; see also Cohen 2004 for a critical overview of much of this literature.

There is another (currently less popular) family of views of representation for mental states that should be mentioned —views according to which a mental state represents by virtue of being similar to its target in the sense that it occupies a similar position in an abstract phase space (cf., Churchland 1986; for criticism see Fodor and Lepore 1992, ch. 6). If something like this were correct, this would require some qualifications to some of our claims about the impotence of similarity in the constitution of scientific representation. We'll return to this in note 9.

Likewise, the General Gricean view consists of two stages. First, it explains the representational powers of derivative representations in terms of those of fundamental representations; second, it offers some other story to explain representation for the fundamental bearers of content. Still, General Griceanism doesn't insist on the Specific Gricean way of drawing the line between its two stages.

Second, it is worth noting that, of these stages in either Specific or General Griceanism, most of the philosophical action lies at the second. The first stage amounts to a relatively trivial trade of one problem for another: you thought you had a problem of representation for linguistic tokens (or whatever you take to be derivative representations)? exchange it for a problem of representation for mental states (of whatever you take to be fundamental representations). This trade, in effect, just pushes back the problem of representation by a single step. The second stage, in contrast, amounts to a fairly deep metaphysical mystery. What is needed to solve it is a fundamental, non-derivative account of the metaphysics of representation; in particular, here it won't do to push the problem back a step. Accordingly, here there is sharp controversy surrounding matters large and small.

The third point is that the explanatory pattern at work here is extremely general. In particular, if you are sympathetic to this account of representation for linguistic tokens, you can use the same apparatus to generate accounts of representation for all sorts of other non-natural representations. For example, the very same apparatus answers this deep question about representation: how did the placement of a pair of lanterns in Boston's North Church belfry arch represent to Paul Revere that the British were coming by sea rather than land?[4] Presumably Revere and the friend who sent him the signal, Joseph Warren, met beforehand and brought into being (by stipulation) their famous code: one if by land, two if by sea. Consequently, when Warren later determined that the British were indeed traveling by sea rather than land, he could reasonably intend that his hanging the pair of lanterns in the belfry would activate in his audience (Revere) the belief that the British would take the sea route. In this case, too, the initial question about representation (how does a pair of lanterns hanging in a belfry represent) is reduced, by a relatively trivial move, to a more fundamental question about how mental states represent. Having this one explanatory strategy, then, means having an account of representation that works for all sorts of representational objects (other than mental states, for which some other story about representation is needed).[5]

Fourth, as a reflex of its generality, the explanatory strategy we are now considering places almost no substantive constraints on the sorts of things that can be representational relata. Can the salt shaker on the dinner table represent Madagascar? Of

---

[4] Our historical scholarship regarding this case was exhausted by consulting Longfellow's poem, "Paul Revere's Ride".

[5] In yet another application, Fodor (1993) extends the same explanatory framework to the problem of the representational power of artworks, and uses this account to distinguish artworks from both rhetorical devices (say, the *Mona Lisa* from a shampoo advertisement) and mere things (say, Warhol's *Brillo Boxes* from Brillo boxes).

course it can, so long as you stipulate that the former represents the latter. Then, when your dinner partner asks you what is your favorite geographical land mass, you can make the salt shaker salient with the reasonable intention that your doing so will activate in your audience the belief that Madagascar is your favorite geographical land mass (obviously, this works better if your audience is aware of your initial stipulation; otherwise your intentions with respect to your audience are likely to go unfulfilled). Can your left hand represent the Platonic form of beauty? Of course, so long as you stipulate that the former represents the latter. Then, when your dinner partner asks you what you are thinking about, you can direct attention to your left hand with the reasonable intention that your doing so will activate in your audience the belief that you were thinking about the Platonic form of beauty. On the story we are telling, then, virtually anything can be stipulated to be a representational vehicle for the representation of virtually anything (including itself, in the odd circumstance where that is desired); the representational powers of mental states are so wide-ranging that they can bring about other representational relations between arbitrary relata by dint of mere stipulation. The upshot is that, once one has paid the admittedly hefty one-time fee of supplying a metaphysics of representation for mental states, further instances of representation become extremely cheap.

Fifth, the Gricean story we are telling allows for two distinct but related sorts of representation, examples of both of which have already come up in our discussion. On the one hand, there is representation of things (/properties/events/processes/etc.); thus, for example, a left hand can represent the family cat. On the other hand, there is representation of facts (/propositions/states of affairs/etc.); thus, for example, a left hand can represent that the family cat is on the mat. These two sorts of representation fit neatly into the same General Gricean explanation; in each case, the story is that the left hand represents what it does (a cat, a fact about a cat) by virtue of (i) an analogous representational relation that obtains between a mental state and its object (alternatively, a cat or a fact about a cat), together with (ii) a stipulation that confers upon the left hand the representational properties of that mental state. Indeed, the easy adaptability of the Gricean story to these different sorts of representation is a mere corollary of its indifference to the kinds of things that serve as representational relata. As noted, because our story puts almost no substantive constraints on the representational relata, it is neutral between representation of (or by) concreta and abstracta, the large and the small, and the near and the distant. The present point is just that the account is similarly neutral between representation of objects and facts.

Sixth, despite what was just said about the absence of constraints on the representational relata, there are plausibly pragmatic constraints on which representational vehicles and targets are used in particular cases. For example, the intentions underpinning the representational powers of salt shakers, left hands, and the like, are likely to go unfulfilled in the absence of certain kinds of communication. We take this consideration not to show that salt shakers and left hands are incapable of serving as full-blooded representational vehicles in principle. Rather, it shows that these objects, while capable of serving as full-blooded representational vehicles in principle, may not do so in practice because they fail to serve the purposes at hand, given pragmatic constraints in force.

3.2. Explaining Scientific Representation

Our proposal, which will come as no surprise, is that scientific representation is just one more special case of derivative representation, and as such can be explained by the General Gricean account sketched above.

In particular, we propose that the varied representational vehicles used in scientific settings (models, equations, toothpick constructions, drawings, etc.) represent their targets (the behavior of ideal gases, quantum state evolutions, bridges) by virtue of the mental states of their makers/users. For example, the drawing represents the bridge because the maker of the drawing stipulates that it does, and intends to activate in his audience (consumers of the representational vehicle, including possibly himself) the belief that it does.

One might reasonably ask at this point why scientific representation could possibly be as useful and interesting as it undoubtedly is, were our analysis correct. Why bother to construct the drawing if its representational relation to the bridge is a product of mere stipulative fiat? Moreover, if fiat would as easily connect the bridge with anything at all, why not use cheaper (more readily available, more easily constructed) materials? In our view, the answers to these questions about scientific representations are no different from the answers to analogous questions about non-scientific representations. Just as the salt shaker (or, for that matter, the linguistic token 'Madagascar') is worth having for facilitating conversation about Madagascar in the absence of Madagascar, the drawing might be useful for facilitating conversation about the bridge in the absence of the bridge. Just as an upturned right hand is worth having because the geometrical structure it shares with the state of Michigan supports inferences about the geography of that state, the drawing of the bridge might (by virtue of preserving certain structural relationships among the represented parts) support inferences about the structure of the bridge.

But note that, just as in the case of similar questions about non-scientific representations, the questions about the utility of these representational vehicles are questions about the pragmatics of things that are representational vehicles, not questions about their representational status *per se*. Thus, if the drawing or the upturned right hand should happen not to rank highly along the dimensions of value considered so far, this would, on our view, make them non-useful vehicles that do represent, rather than debar them from serving as representational vehicles altogether.[6]

[6] The idea that virtually anything can serve as a vehicle for scientific representation has met with some resistance, even scorn, in the literature (despite having been occasionally endorsed by some, e.g., Teller (2001, 397)). French writes "Not anything can serve as a scientific model of a physical system; if the appropriate relationships are not in place between the relevant properties then the 'model' will not be deemed scientific" (French 2002, 6). Bailer-Jones, in criticizing Hughes, points out that on Hughes's account representation is stipulative, "as if 'what represents what' *could* be entirely arbitrary and merely set per decree. This could in some instances preclude that a model is about the empirical world in any meaningful and informative way" (Bailer-Jones 2003, 72).

Presumably scientific contexts come with their own set of pragmatic constraints, and these may drive the choice among possible scientific representations in ways that are idiosyncratic to science. For example, pathological cases like Weierstrass's example of a continuous but nowhere differentiable function ($f(x)=\sum_{k=1}^{\infty}[(\sin(\pi k^{a}x))/(\pi k^{a})]$) will not typically be used in science, nor would scientists use the picture of people climbing up a growth chart from the Microsoft clip-art that comes with every PC, or live jellyfish. And we can make conjectures at (and, in principle, even investigate) the reasons for these constraints. Weierstrass's pathological function typically won't be the first choice for scientific representation because scientists usually want to use the functions they choose, and that usually means differentiating them. The silly pre-drawn Microsoft graph that comes with most PCs, by contrast, won't be used for sociological reasons: it would simply be too embarrassing to have a graph from a Microsoft picture gallery in an academic economics journal (on the other hand, it might be used to represent in a Powerpoint display in a business' human resources department). Finally, live jellyfish won't be used because they can sting.

That said, it should be clear that the constraints ruling out these choices of would-be representational vehicles are pragmatic in character: they are driven by the needs of the representation users, rather than by essential features of the artifacts themselves.

Likewise, we suggest that, while resemblance, isomorphism, partial isomorphism, and the like are unnecessary for scientific representation, they have important pragmatic roles to play; namely, they can (but need not) serve as pragmatic aids to communication about one's choice of representational vehicle.

To see this, consider again the problem first raised for the salt shaker —that of making one's representational stipulations clear to one's audience. One alternative to announcing the stipulated representational relationship is to make one's intentions obvious by choosing a representational vehicle (from among indefinitely many candidates) that resembles its representational target in salient respects. For example, the geometric similarity between the upturned human right hands and the geography of Michigan make the former a particularly useful way of representing relative locations in Michigan, and it normally would be foolish (but not impossible!) to use an upturned left hand for this purpose since a more easily interpreted representational vehicle is typically available. Similarly, the behavior of billiard balls may prove a useful choice of model for the behavior of elastic particle interactions in a gas because there is a salient similarity/isomorphism between the dynamics of the vehicle's objects (billiard balls) and the target's objects (gas particles). This is not to say that the very same target could not be represented by an upturned left hand, or anything else for that matter,

---

To our eyes, these sentiments seem motivated more by intuition than argument; we suspect they come from running together the constitution question (what constitutes representation?) with the normative question (what makes a representation a good one?). We propose that intuitions to the effect that such and such cannot serve as a model are best understood as reflecting the unlikelihood of anyone's using such and such as a model, given certain assumptions about pragmatic purposes. If so, then our view accommodates them.

but only that similarity/isomorphism can make one of these choices more convenient than the other (given the scientific purposes at hand).

Our proposal, then, is that scientific representation is just another species of derivative representation to which the General Gricean account is straightforwardly applicable. This means that, while there may be outstanding issues about *representation*, there is no special problem about *scientific* representation.

4. Surrounding Problems Dissolved/Reframed

Once our view of scientific representation is in place, the surrounding landscape of problems —problems that have inspired much of the philosophical interest in models— changes dramatically. This can be viewed in two ways. The more dramatic assessment would be to say that these problems have been dissolved. The less dramatic (but probably more accurate) assessment would be to say that our view allows for the fruitful reframing of these problems as pragmatic issues about which among alternative (and equally viable) representations best meet scientific needs.

4.1. What Does it Take For x *to Represent* y*?*

We've seen that a cottage industry has arisen in recent years around what we called (in §1) the constitution problem about scientific representation: what does it take for *x* to constitute a scientific representation of *y*? Some (French) hold that *x* and *y* must stand in some sort of isomorphism (or partial isomorphism), while others (Giere, Teller) insist that what is crucial to representation is that *x* is similar to *y*.[7] Still others (Suárez) have argued that it is essential to representation that *x* allows its users to generate inferences about *y*. Suffice to say that the debates between proponents of these different accounts have not resulted in consensus. As far as we can see, all of the proposals are either vacuous or too demanding. Since there is always, trivially, some or other isomorphism of structure, similarity, or generated inference that relates an arbitrary *x* to an arbitrary *y*, the accounts in question will be vacuous if they are not supplemented with a robust account of what sort of isomorphism, what respect of similarity, or what sorts of inference generation, are required. On the other hand, it has proven exceedingly difficult to specify the needed sense of isomorphism, similarity, or inference gen-

---

[7] However, it is possible to read Teller (2001) and Giere (1999) as appealing to similarity in a more deflationary way, and indeed in a way that ends up anticipating the position we are defending. For, while they claim that *x* represents *y* in virtue of a similarity between *x* and *y*, they also insist that there is no substantive sense of similarity that unites all vehicle, target pairs and that can be specified in advance. Rather, on their view, the relevant similarity relation is stipulated by users of the representations, according to their own purposes, on a case by case basis.

If this is right, then our disagreement with Teller and Giere is largely terminological. Our reason for preferring our own terminology is only that, insofar as the sense of 'similarity' is entirely given by stipulation on a case by case basis, it seems that representation is only nominally constituted by similarity. What does the real representational work, it turns out, is stipulation. Better, then, we think, to drop the empty talk of similarity in favor of an up-front admission that representation is constituted in terms of stipulation (plus an underlying account of representation for the mental states subserving stipulation), as per the General Gricean view we are defending.

eration in any detail: invariably, such specifications have been insufficiently general to cover the wide variety of instances of scientific representation.

From the perspective of the General Gricean story we've been telling, these difficulties are unsurprising. For if, as we've been urging, scientific (and other non-natural) representation is constituted in terms of a stipulation, together with an underlying theory of representation for mental states, isomorphism, similarity, and inference generation are all idle wheels in the representational machinery —none of them (on any understanding) amounts to a necessary condition on scientific, or any other non-natural, representation.

Is there, then, nothing at all to the traditional disputes over the role of isomorphism (etc.) in scientific representation? That seems to us not quite right either. We are not denying that isomorphism, similarity, and inference generation may relate representational vehicle and representational target in many cases of scientific (and other non-natural) representation. We claim that these conditions do not constitute the representational relation, and hence are not necessary features of representation. However, we allow that there are important roles for these conditions —viz., they may serve as pragmatic aids to the recognition of a representational relation that is constituted by other means.[8] Moreover, since the expectations representation users have about how audiences will interpret form an important part of the story we're telling, such pragmatic aids can constrain our choices about which representational relations to use.

If this is right, then there will remain a role for considerations about isomorphism, similarity, and inference generation after all. Namely, these considerations (and possibly others) may contribute to an anthropology of the use of scientific representations by providing a taxonomy of the sorts of pragmatically guided heuristics scientists bring to bear on their choices between representational vehicles.[9] But if so, then there is no longer any reason to think that there is a conflict between, say, Giere's similarity and Suárez's inference generation, and so no reason that there should be a dispute between proponents of such accounts: these are simply independent pragmatic constraints that may work together or separately to guide choices between scientific representations. This point, we think, should serve to undercut that growing proportion of the literature on scientific representation devoted to arguing in favor of one of these

[8] In saying that the constraints on representational vehicles are pragmatic in character, we certainly don't mean to deny that they have epistemic force or rationale. On the contrary, it is plausible that the pragmatic constraints on scientific representation typically will center around epistemic demands insofar as scientists *qua* scientists are in the business of acquiring knowledge about the world.

[9] Recall that, on some views, the fundamental level of representation appealed to by the General Gricean is itself constituted in terms of similarity. If some such similarity-based view were correct, this would mean that similarity has a role to play in the explanation of scientific representation that goes beyond the role we've allowed for it in the main text. On the other hand, even on the envisioned scenario, the relata related by similarity would be (not scientific models and worldly targets, but) mental states and worldly targets. Consequently, even this outcome would fail to give the defenders of similarity *qua* explanation of scientific representation what they most seem to want.

accounts and against the others; if, as we contend, these accounts are not in competition, this should spare the needless consumption of much ink and many trees.

4.2. How Do Models Represent Despite Idealization?

If our General Gricean story is correct, the question of how models can represent despite their use of idealization, abstraction, etc., can't really be a question of how they manage to represent.

It is important to be clear that one can succeed along the dimension of representation but fail along the dimension of truth: something can be a representation although it represents falsely or comes up short on various pragmatic measures. For example, suppose the instruction is "one if by land, two if by sea," and the British come by sea but Warren hangs only one lantern. Then Warren would have successfully induced in the mind of Revere the belief that the British are coming by land. The representation would have induced in Revere a false belief. It misrepresents (i.e., represents falsely) the situation to Revere; moreover, given that the point of Warren and Revere's coordination was to produce a true belief in Revere's head, the representation meets its goals badly. But, for all that, it is still a representation.

Clearly, as Morrison (2006) emphasizes, looking at the details of the model in isolation will not answer the question of whether it represents truly, falsely, or approximately truly. Truth, falsity, and approximate truth are features that putatively apply to things that are representations; as such, the question of whether *x* represents *y* is independent of (indeed, prior to) the question of whether *x* is a true, false, or approximately true representation of *y*. Contrary to what many seem to have thought, then, there is no reason for fearing that the merely approximate status of a model impugns its capacity to represent.

5. Objections and Replies

5.1. Whither Realism?

Our view is extremely permissive about representation —it requires only an act of stipulation to connect representational vehicle with representational target (once the underlying metaphysics of representation for mental states is in place). It is so permissive, in fact, that it might suggest that we have begged the question in favor of irrealism about the posits of science. After all, if all that is required is mere stipulation, there is nothing to distinguish a stipulation connecting a vehicle to electrons, on the one hand, from a stipulation connecting a vehicle to phlogiston, on the other. But, a realist would say, this is a distinction we really want to make between representations, insofar as the former model tells us something about what really exists in the world (electrons) and the latter model tells us something about what really does not (phlogiston).

We would indeed be bothered if our view of scientific representation precluded realism about the posits of science. For one thing, we are rather fond of electrons. Moreover, we would strongly prefer not to have our commitments about the realism-antirealism debate decided by our theory of representation.

Luckily, there is no clash between realism and our view of scientific representation. For, while our approach to the question of what constitutes representation marks no distinction between models of existent entities and models of non-existent entities, it leaves plenty of room for further distinctions between such models, including those that the realist needs to get her position off the ground. In particular, the relevant distinction between a model of electrons and a model of phlogiston is not that one counts as a model and the other doesn't, but that one may be a better model in some normative respects than the other. Realists have famously offered a number of criteria —predictive and explanatory success, coverage of a wide range of data , etc. —that they use to measure the merit of models. Since, as we have emphasized, the constitution question about models is not identical to the various normative questions about the merits of things that are models, our proposal about the former question leaves room for many answers— including realist answers —to the latter questions.[10]

5.2. Whither Irrealism?

In responding to worries from realists (§5.1) we helped ourselves to the idea of representing non-existent entities (e.g., phlogiston). But how, one might ask, is representation of the non-existent possible on our view? After all, we have insisted that scientific representation is a relation between a representational vehicle and a representational target. And, insofar as relations cannot hold in the absence of relata, this commitment might seem to rule out the possibility that there is no genuine worldly entity that scientific models succeed in representing. For consider: if kicking is a relation, then you can't kick *x* unless *x* exists; if kissing is a relation, then you can't kiss *x* unless *x* exists (cf. Quine 1956). Likewise, if representation is a relation, then a model can't represent DNA unless DNA exists. But, just as we are loath to rule out realism about the posits of science, we would be equally embarrassed if our view about scientific representation ruled out irrealism about the posits of science.

This worry is too general to be a particular problem for us. First, the worry arises for anyone who thinks of scientific representation as any kind of relation at all. This crowd is broad indeed, and certainly includes all of the defenders of answers to the constitution problem that are competitors to our view: similarity, isomorphism, and related notions are proposed as ways of understanding representation as a relation too, so defenders of these views also owe a story about how we manage to represent the non-existent. Second, the worry arises for all species of representation —not just scientific representation— and there is no reason to suspect that whatever ultimately ex-

[10] There are multiple normative dimensions along which models can be measured. Our claim is that models of electrons are good and models of phlogiston are bad along dimensions that realists have stressed as ways of distinguishing between the posits we should accept and the posits we shouldn't accept. But one might also be interested in independent dimensions of evaluation: e.g., how well the model communicates what one wants to communicate about the representational target, how well the model functions in science (e.g., does it help explain the phenomena?), etc.

plains representation of unicorns and golden mountains won't work for representation of phlogiston and the ether.[11]

5.3. Is the Cure More Controversial than the Sickness?

In the foregoing we have made blithe use of the Gricean framework for explaining representation. On the other hand, there are outstanding difficulties in the details of that framework (for examples of disputes about these difficulties, see Grice 1989, ch6, ch14; Schiffer 1972; and Loar 1981). But if the details of the Gricean framework are not understood either, why think that appealing to them will shed any light on problems in the philosophy of science? Why hope to explain one mystery by appeal to another?

We have two main reasons for not being bothered by such outstanding controversies. First, everyone needs an account of mental representation. We should think philosophers would be delighted to learn that the price we are all already committed to paying in the philosophy of mind also buys a solution to the constitution problem about representation in the philosophy of science (even if no one has yet raised the funds). Second, as we pointed out when we introduced the Gricean apparatus in §3, our General Gricean proposal for understanding scientific representation is largely independent of the details of Specific Griceanism. Though we have appealed to Specific Griceanism in attempting to show that flesh can be put on the skeletal framework of General Griceanism, we do not mean to commit to Specific Gricean particulars. But, with one significant exception, the controversies about Grice's program are largely confined to the level of Specific Griceanism. Indeed, there seems to be a fairly solid consensus in favor of General Griceanism among the relevant experts in philosophy of mind and language, arguably for good reasons.

The significant exception to the idea that General Griceanism is insulated from controversy concerns the understanding of representation at the fundamental, non-derivative level. General Griceanism is, of course, committed to telling some story about the metaphysics of fundamental (typically mental) representation. However, while, as noted in §3.1, there is a notable absence of consensus even about the broad shape that a fundamental metaphysics of mental representation should take, nothing we have said chooses sides in these debates; consequently, General Griceanism is in trouble only if it turns out that mental representation is unreal.

In fact, even this commitment is dispensable; for, although in presenting General Griceanism we have welcomed the idea that the fundamental sort of representation at the bottom of the intentional stack is one that applies to mental states, it is easy to

[11] Thus having shifted the dialectical burden, we hasten to add that there are actual strategies for responding to the general worry, and that they seem applicable to scientific cases. First, one might bite the bullet and hold that, in cases where x doesn't exist, agents don't succeed in representing x but merely believe that they are representing x. Alternatively, one might appeal to a Humean strategy that (i) draws a distinction between atomic and compound representations, (ii) explains representation for the atoms by a relational theory, and (iii) explains compound representations as recursive structures built from other representations (cf. Hume 1777/1975, §II).

imagine a variant of General Griceanism that does without this idea. For purposes of illustration, suppose it turns out that, as urged by Dennett (1987), maybe Quine (1960; e.g., 221), Stich (1983), and others, mental representation is unreal. Then, so long as there is some genuine representation in the world by something not a scientific model —say, linguistic representation— we can still get our story off the ground by running a story analogous to Grice's that construes scientific representation as derivative from this other sort of representation. So, really, the only way we can lose is if either (i) there is no representation anywhere, or (ii) scientific representation is fundamental. Both of these possibilities seem pretty far beyond the pale to us, so we aren't particularly worried about them.

6. Conclusion: Where We Are Now

It is somewhat surprising that current disputes over scientific representation have often been carried out in isolation from more general work on representation. After all, this is justifiable only if scientific representation is constituted in a fundamentally different way from non-scientific representation, and that would seem to make a mystery of the possibility of expressing the content of scientific models by other means. Moreover, as we have shown, there are relatively well-worked out views about representation that seem to apply straightforwardly to scientific representation and substantially clarify the parochial disputes that have grown up around representation in philosophy of science.

Though we have deflated the constitution problem for models, there are still related questions that survive. We conclude by describing three of these questions and commenting on their relative interest.

First, we have claimed that anything can represent anything in science when the appropriate conditions are met. But what are these conditions and when are they typically met? Why, for example, did the Minkowski diagram triumph over the Loedel, Breheme and complex rotation diagrams as the standard vehicle of representing the spacetime of special relativity? How and why did the Feynman diagram come to dominate post-war physics (Kaiser 2000)? These anthropological questions, identified in §3, remain interesting questions in sociology of science.

Another question concerns the confirmatory and explanatory relationships between models, theories, and data. Though we would caution against overstating the "independence" of scientific models from overarching theories, one of the valuable lessons of the modeling literature is its study of the idea that models can sometimes take on a "life of their own" in science: the model can itself become the subject of scientific endeavor. We can think of no more prominent example than the Ising-Lenz model in statistical mechanics. When Onsager in 1944 ingeniously showed that the $d = 2$ Ising model displayed singular behavior despite having a non-vanishing partition function, he precipitated a real revolution in the study of phase transitions (see Domb 1996; Hughes 1999). For more than 50 years, large groups of physicists and mathematicians have devoted their time solely to solving various Ising models that represent an increasingly large number of systems.

Serious philosophical questions attend such changes. Suppose one asks how a physical system can exhibit multiple phases (solid-liquid-gas). Statistical mechanics answers by showing that the so-called Gibbs measure for the system is non-unique. But it demonstrates this non-uniqueness only for an infinite volume lattice with nearest-neighbor interactions, a simple interaction energy, and a host of other unrealistic assumptions. Physicists will want to say such a model represents the co-existence of phases in real (and hence finite volume non-lattice) macroscopic systems with complicated interactions. That such an idealized infinite system might represent a real macroscopic system is no problem. But physicists clearly also think that such a demonstration has important explanatory and confirmatory powers. Philosophers, however, might ask: Are the coexistence of phases in real systems explained by this model? Does this model confirm the basic tenets of statistical mechanics? Or of thermodynamics? And what relationship does this model have to the experimentally measured values of thermodynamic parameters of various gases? The question is similar to that asked recently about the explanatory/confirmatory status of computer-generated simulations. These simulations, like solutions to the Ising model, are often treated as having the status of genuine experiments. What are we to make of this in either case? The traditional questions of philosophy of science regarding explanation and confirmation arise again in the context of models. Much of the modeling literature has admirably examined the evidentiary/explanatory relationships between models that have a life of their own and theory and data, especially in various case studies. They have worked on what we called the normative problem in §2. By showing that there is no special puzzle about scientific representation, we hope to free these studies to focus on the confirmatory and explanatory role of models unencumbered by the perceived need to talk about the representation relation.

Finally, §2 described Hughes as seemingly interested in a kind of demarcation problem —that of saying what separates Galileo's geometric figures from Vermeer's masterpieces. Plausibly, scientific representation is just representation that takes place when the agents are scientists and their audiences are either fellow scientists or the world at large. But that means that to solve the demarcation problem in scientific representation one must first solve the prior question, THE demarcation problem famously discussed by Popper, Lakatos, Grünbaum, and Laudan. We are not optimistic about solving this problem. And we think it a virtue of our account that it allows one to see clearly that the demarcation problem for representation just is an instance of the general demarcation problem concerning the difference between science and non-science.

Demarcation worries aside, we've seen that there remain a number of interesting questions about representation in philosophy of science. We submit that 'what constitutes scientific representation?' is not one of them.[12]

[12] We are grateful to Nancy Cartwright, Paul Churchland, Andrew Hamilton, Sam Rickless, Mauricio Suárez, and Paul Teller, UCSD's Philosophy of Science Reading Group, and an audience at the London School of Economics for helpful discussions of these matters.

REFERENCES

Bailer-Jones, D. M. (2003). "When Scientific Models Represent," *International Studies in the Philosophy of Science* 17, 59-74.

Churchland, P. M. (1986). "Some Reductive Strategies in Cognitive Neurobiology," *Mind* 95: 379, 279-309. Reprinted in Churchland (1989), 77-110.

——— (1989). *A Neurocomputational Perspective: The Nature of Mind and the Structure of Science*, MIT Press, Cambridge, Massachusetts.

Cohen, J. (2004). "Information and Content," in L. Floridi (ed.), *Blackwell Guide to the Philosophy of Information and Computing*. New York: Basil Blackwell, 215-227.

Dennett, D. C. (1987). *The Intentional Stance*. Cambridge, Mass.: MIT Press.

Domb, C. (1996). *The Critical Point: A Historical Introduction to the Modern Theory of Critical Phenomena*. Bristol, PA: Taylor & Francis, Ltd.

Fodor, J. A. (1993). "Deja vu All Over Again: How Danto's Aesthetics Recapitulates the Philosophy of Mind," in M. Rollins (ed.), *Arthur Danto and His Critics*. Oxford: Blackwell, 41-54.

———, and E. Lepore (1992). *Holism: A Shopper's Guide*, Cambridge, Mass.: Blackwell.

French, S. (2003). "A Model-Theoretic Account of Representation (Or, I Don't Know Much About Art... But I Know It Involves Isomorphism)," *Philosophy of Science Supplement* 70, 1472-1483.

Giere, R. (1988). *Explaining Science: A Cognitive Approach*. Chicago: Chicago University Press.

——— (1992). "Introduction: Cognitive Models of Science," in R. Giere (ed.), *Cognitive Models of Science*. Minneapolis: University of Minnesota Press, Vol. 15, xv-xxvii.

——— (1999). "Using Models to Represent Reality," in L. Magnani, N. Nersessian, and P. Thagard (eds.), *Model-Based Reasoning and Scientific Discovery*. New York: Kluwer/Plenum, 41-57.

Goodman, N. (1976). *Languages of Art: An Approach to a Theory of Symbols*. Indianapolis: Hackett.

Grice, H. P. (ed.) (1989). *Studies in the Way of Words*. Cambridge, Mass.: Harvard University Press.

Hughes, R. (1997). "Models and Representation," *Philosophy of Science* 64, S325-S336.

——— (1999). "The Ising Model, Computer Simulation, and Universal Physics," in M. Morgan and M. Morrison (eds.), *Models as Mediators*. Cambridge: Cambridge University Press, 97-145.

Hume, D. (1777/1975). *An Enquiry Concerning Human Understanding*. L. A. Selby-Bigge and P. H. Nidditch (eds.). Oxford: Oxford University Press.

Kaiser, D. (2000). "Stick-Figure Realism: Conventions, Reification, and the Persistence of Feynman Diagrams, 1948-1964," *Representations* 70, 49-86.

Loar, B. (1981). *Mind and Meaning*. Cambridge: Cambridge University Press.

Morrison, M. (1999). "Models as Autonomous Agents," in M. Morgan and M. Morrison (eds.), *Models as Mediators*. Cambridge: Cambridge University Press, 38-65.

——— (2006). "Models as Representative Structures," in S. Hartmann and L. Bovens (eds.), *Nancy Cartwright's Philosophy of Science*. Routledge. Forthcoming; available at http://www.uni-konstanz.de/ppm/workshop2/Morrison_final.pdf.

———, and M. Morgan (1999). "Models as Mediating Instruments," in M. Morgan and M. Morrison (eds.), *Models as Mediators*. Cambridge: Cambridge University Press, 10-37.

Quine, W. V. O. (1956). "Quantifiers and Propositional Attitudes," *The Journal of Philosophy* 53, 177-187.

——— (1960). *Word and Object*. Cambridge, Mass.: MIT Press.

Schiffer, S. (1972). *Meaning*. Oxford: Clarendon Press.

Schiffer, S. (1987). *Remnants of Meaning*. Cambridge, Mass.: MIT Press.

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

Stich, S. (1983). *Form Folk Psychology to Cognitive Science: The Case Against Belief*. Cambridge, Mass.: MIT Press.

———, and T. A. Warfield (1994). *Mental Representation: A Reader*. Oxford: Basil Blackwell.

Suárez, M. (2004). "An Inferential Conception of Scientific Representation," *Proceedings of the Philosophy of Science Association* 71: 5, 767-779.

Suppes, P. (1960). "A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences," *Synthese* 24, 287-300.

——— (1967). "What is a scientific theory?" in S. Morgenbesser (ed.), *Philosophy of Science Today*. New York: Basic Books, 55-67.

Teller, P. (2001). "Twilight of the Perfect Model Model," *Erkenntnis* 55, 393-415.
van Fraassen, B. (1980). *The Scientific Image*. Oxford: Clarendon Press.

Craig CALLENDER is a professor of philosophy at the University of California, San Diego. He has published widely on philosophy of science, philosophy of physics, and metaphysics, and especially on questions about spacetime, quantum mechanics, and the foundations of statistical mechanics.

ADDRESS: Department of Philosophy. University of California, San Diego. 9500 Gilman Drive. La Jolla, CA 92093-0119. E-mail: ccallender@ucsd.edu.

Jonathan COHEN is an associate professor at the University of California, San Diego. He works largely on topics in philosophy of mind, language, and perception, particularly as these are informed by the cognitive sciences. Much of his work in recent years has concerned color and color vision.

ADDRESS: Department of Philosophy. University of California, San Diego. 9500 Gilman Drive. La Jolla, CA 92093-0119. E-mail: joncohen@aardvark.ucsd.edu.