Users, Structures, and Representation

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Abstract

This paper defends a pragmatic and structuralist account of scientific representation of the kind recently proposed by Bas van Fraassen. As I show, the account appears to have the unacceptable consequence that the domain of a theory is restricted to phenomena for which we actually have constructed a model—a worry arising from the account's pragmatism, which is exacerbated by its structuralism. Yet the account has the resources at least partially to address the worry. What remains as implication is a strong anti-foundationalism.

1. Introduction

My aim in this paper is to offer a partial defense of a pragmatic and structuralist theory of scientific representations. The central tenet of the view I will explore is the claim that representation is an essentially pragmatic notion, dependent on a context of use. This claim has recently been forcefully defended by Bas van Fraassen (van Fraassen 2008) and in what follows I will take his account as my main point of departure. I will try to make various aspects of van Fraassen's pragmatic account of representation plausible, but my aim here is not to offer a comprehensive defense of this view. Rather I am interested in some of the consequences of the account for philosophical views of scientific theorizing, for the question of scientific foundationalism and for the issue of inconsistent theories.

Van Fraassen's account of representation amounts to a critique of the view that a physical theory's representational content—what the theory says about the world—is given simply by a statement of the theory's basic equations or by a set-theoretic predicate defining the theory's representational structures. The latter view, which Nancy Cartwright has derisively dubbed "the vending machine view" of scientific theories (Cartwright 1999) and which van Fraassen himself appears to have once held (see, e.g., van Fraassen 1980), is inadequate precisely because it ignores the central role played by users of representations. Furthermore, as I will argue, a pragmatic account of representation has radically anti-foundationalist implications of a kind which van Fraassen himself may be reluctant to accept.

In the next section I will motivate and partly defend the claim that both the target and the content of a scientific representation depend on a context of use of the representation. In section three I will argue that just as selective resemblance plays an important role in successful scientific representation, distortions and selective non-resemblance play an important role as well. There is, as Paul Teller has argued, no 'perfect model'. Defending this claim will lead to a worry for a pragmatic account of representation, however: the reason why there can be no perfect model, it seems, is that our theories only represent those phenomena for which we have *actually* constructed a representation. This worry gets exacerbated, if we take scientific representation, at least within physics, to be structural representation. For then, as I will argue in section four, we have to confront Putnam's model-theoretic argument and responding to the argument requires a further emphasis of the role of the user in the link between a representation and its target. But, as I will argue in section five, the worry concerning the overly restricted domains of our theories can be met, if we distinguish between 'horizontal' and 'vertical' extensions of the domain of a theory. A context of use can be extended horizontally from phenomena modeled to others of the same kind that have not been actually modeled. But we cannot similarly extend the domains of our theories vertically, as foundationalism claims, or so I will argue.

2. 'No representation without representer'

What is it for a scientific representation to represent a phenomenon? Van Fraassen argues, to my mind convincingly, that representation is an essentially pragmatic, user-dependent notion that we cannot "define" or "reduce […] to something else" (2008, 7). To call a thing a representation is to say something about its use. Thus, the "*Hauptsatz*" of van Fraassen's account of representation is that "*There is no representation except in the sense that some things are used, made, or taken, to represent things as thus and so*." (2008, 23, italics in original) This implies that there can be no 'natural representations'—no naturally produced objects or phenomena that represent other phenomena without being used by someone to represent. Independent of its actually being used as a representation a pictures has no representational content: "*To call an object a picture at all is to relate it to its use*." (2008, 25, italics in original)[1]

I do not here want to defend the view that we cannot give a more substantive account of representation than what is expressed in van Fraassen's *Hauptsatz*, but I do wish to claim that the *Hauptsatz* will have to be part of any satisfactory account of scientific representation in particular. For example, one cannot define representation independently of use exclusively in terms of likeness or resemblance, for several obvious reasons. As has often been pointed out, at least since Nelson Goodman's seminal work (Goodman 1976), resemblance is a symmetric relation but representation is not. Moreover, while perfect resemblance would be much too strong a requirement for representation, partial resemblance is much too weak: arguably for every two objects there will be some respect in which the two objects resemble each other. Thus, partial resemblance cannot be sufficient for representation, for otherwise we would be forced to the conclusion that everything represents everything else. But partial resemblance as necessary condition, without additional constraints, would be an empty requirement.

It might seem that by insisting on the pragmatic character of representation, I am thereby disagreeing with recent structuralist accounts of representation, such as the partial structure account proposed by Steven French and Otavio Bueno (see, e.g. Bueno and French 2011), or the homomorphism account defended by Andreas Bartels (Bartels 2005; 2006). But French and Bueno also acknowledge an important role for pragmatic and context-dependent considerations. According to them, "partial isomorphism [that is, the structural relationship that they see at the core of successful representation] is not sufficient and that other factors must be appealed to." (2011, 29) Indeed, they emphasize that just as a certain structural relationship is not enough to fix a representation's target, one also cannot rigidly build a particular intention into the representational mechanism that would permanently fix the representation's target. Rather we must allow for the flexibility of "pragmatic or

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[1] A similar non-reductive account of representation is defended in (Suárez 2004) and (Suárez 2010).

broadly contextual factors to play a role in selecting which [representational] relationships to focus on." (Bueno and French 2011, 31).

Initial appearances to the contrary, Bartels also agrees with the claim that structural relationships are not sufficient for representation. Bartels argues that the common symmetry-objection to structural accounts of representation is unsuccessful and can be disarmed by taking the appropriate structural relationship between a representation and its target to be a non-symmetric homomorphism instead of a symmetric isomorphism. Bartels then proposes a homomorphism condition either as a sufficient condition (Bartels 2005) or as a necessary condition(Bartels 2006) for representation. While this might be taken to suggest a purely structural account of representation, he ultimately distinguishes the notion of *potential representation* from that of *actual representation* and maintains that only the former notion satisfies the homomorphism condition. Whether a potential representation is also an actual representation is determined by pragmatic and context-dependent factors.

The difference, then, between a formal, structural account, such as French and Bueno's or Bartels's, and van Fraassen's account of representation (which, after all, also takes representation in physics to be structural representation) strikes me as one of emphasis: whereas French and Bueno stress the structural relationships that have to exist between a successful scientific representation and its target, van Fraassen focuses on the ineliminable pragmatic aspects of the representation relation and the insufficiency of *purely* structural relations in establishing a representation relation between a structure and its target.

Thus, we can concede that resemblance plays an important role in representation, but where it does play a role it does so as a function of the representation's use. For example, in certain contexts we identify a representation's target with the help of selective resemblances between representation and target. Yet which aspects are important in assessing the likeness between representation and target is given by the context in which the representation is used.

Scientific representation is representation *as*: a representation has a certain target and represents its target *as* being thus and so. The content of a representation is use-dependent as well and cannot be simply read off the representational structure. As we will see in somewhat more detail below, the use of a given representation determines for each of the models properties under which of a number of different categories a given property or feature of the model falls: some features are part of the model's representational content with the aim of representing the model's target approximately correctly; other features are part of the model's representational content but purposefully misrepresent its target; and there are features of a model that do not play any representational role (in a given context). The classical point-charge model for an electric charge can illustrate all three types of feature: We take it that the $1/r^2$-dependence of the electric field posited in the model accurately represents the actual field dependence. The model represents charges as point particles, which is usually taken to be an idealization, but arguably the model does not represent electric charges *as* having infinite mass and self-energy (for otherwise it would represent charges as objects that cannot be moved by any finite external force). The infinities of the model are mathematical inconveniences that arguably play no representation role in how the model is used.[2]

The success of a scientific representation depends on a selective likeness between the representation and its target: what a representation *r* represents its target *t* as, needs to be appropriately similar to the target. And again which aspects of the representation are relevant to judging its likeness to the target and what counts as sufficiently similar for success depends on the context in which the representation is used and can change from context to context. Thus, one and the same scientific model can provide an adequate or successful representation of an object in some contexts but not in others.

Representation, then, is best thought of not as a two-place relation but rather as a multi-place relation, which includes a place for the user of the representation and for its context, aim, or purpose. If we take aims and purposes to be implicit in the context, we can construe representation as a four-place relation: *a* is a representation of *b*, exactly if there is some context *c* in which a user *u* uses *a* to represent *b*.[3]

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[2] A partial structure approach is tailor-made to capture the various roles that properties and relations of a model can play. My claim here is that which partial structure adequately captures the representational content of a given scientific representation is determined by pragmatic and context-dependent factors.

[3] Similarly, Ronald Giere has proposed to understand scientific representation in terms of the following four-place relation: '*S* uses *X* to represent *W* for purposes *P*' (Giere 2006, 60).

3. No perfect model

Van Fraassen argues that just as partial resemblance can figure in successful representation *distortion*, or *selective non-resemblance* plays an important role as well. As a scientific example he discusses the fact that classical physics represents objects as having sharp boundaries. Already the very idea of the true and exact shape and the true and precise boundaries of a macroscopic physical object might strike one as suspect. More troubling, sharp boundaries in a mathematical model often result in discontinuities or singularities, where the physics used to represent a system's behavior sufficiently far away from the boundaries breaks down. Thus, representing objects as having sharp boundaries could not possibly be completely accurate and non-distorting, at least if they involve singularities, nevertheless such representations fulfill an important role in our scientific image of the world and may in certain contexts provide us with the only means to construct useful representation of certain phenomena.

Sometimes there are techniques to patch over what happens at such boundaries, but often not in ways that allow for a single unified representation of the system. Mark Wilson's book (Wilson 2008) is replete with fascinating examples of how our representational practices in classical physics have to distort to be successful at all, representing phenomena by partially overlapping yet in some sense incompatible 'theory façades', breaking down at the fault lines between the façades. The situation here is in certain respects analogous to multi-perspectival paintings by artists like Picasso, which bring together on a single canvas different perspectives on different parts of the human body, without, however, being able to combine these multiple perspectives into yet another unifying perspective (see also van Fraassen 2008, 38).

However, I want to focus here on a different cluster of reasons for why distortions play a central role in scientific representations. Paul Teller (Teller 2001) has argued forcefully against what he calls "the perfect model model"—that is, the view that our best scientific theories provide use with complete and perfectly accurate models of physical phenomena, or at least that physics is progressing toward and aiming at developing ever

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I take Giere's proposal to be implied by my perhaps slightly broader suggestion: purposes can be understood to be given by contexts.

more complete and accurate and non-distorting models of the world. Teller argues that this view of physics is mistaken and emphasizes in its stead the importance of highly idealized models. Idealized models distort in that they represent only some aspects of the physical system modeled while leaving out other aspects and may purposefully misrepresent some of the aspects represented. According to some of Teller's arguments, which I want to amplify here, distorting models at the very least play an important explanatory role and would not be rendered explanatorily superfluous by complete and perfect models. But there are even stronger arguments that suggest that the idea that physics even in principle presents us with perfect models of the phenomena is a myth and that all scientific representations are distorting.

Teller illustrates the importance of distorting models by pointing to different ways in which physicists model different aspects of the behavior of water. Continuum models can correctly represent the wave behavior of water, whereas particle models are used to represent diffusive behavior. While both kinds of model represent certain aspects of the behavior of water sufficiently accurately, neither type constitutes a perfect model of water that can successfully represent all of its properties. And both types of model manage to capture aspects of the behavior of water by distorting, by representing water either as a continuum or as a collection of classical particles. The example, thus, supports the claim that given the way our world is, for many physical systems, at least, there is no single model of the system that allows us successfully to represent the system in any kind of circumstance and we need to employ different, and even in some sense incompatible models to represent successfully different aspects of the system's behavior.

Now, one might reply to Teller's example by arguing that in addition to these two types of models there exists a third type—quantum mechanical models—that do provide us with perfect, non-distorting representations of water and that can unify the two classical types of model by showing how both can be approximately derived from the correct and complete micro-theory by taking appropriate limits. This reply can be read as arguing either against the claim that the idealized models are *explanatorily ineliminable* or against the claim that *all models* in physics are distorting. I want to focus on the issue of explanatoriness first.

Let us provisionally grant for now that present-day quantum mechanics provides us in principle with an immensely complex yet accurate—indeed, perfect—model of water. Nevertheless, as Teller argues convincingly, even if we imagined that we were given the solution to the Schrödinger Equation for a system of 10^{25} variables, this would not provide us with an explanation of macroscopic waves in a body of water, since being able to grasp what this solution says about the behavior of the body of water goes far beyond what is cognitively possible for us humans. Explanation and understanding are inherently pragmatic notions—explanation is always *explanation for us*—and *we* absolutely require the more idealized classical models to be able to understand the water's behavior.

(Frisch 1998) makes what is essentially the same point by contrasting classical Newtonian explanations of the collection of balls know as "Newton's cradle" with a putative quantum mechanical account of the system of five balls. Frisch argues that there are additional reasons not to take a putative quantum mechanical account as providing a satisfactory explanation. Scientific explanations answer "what-if-things-had-been-different-questions" (see also Woodward 2003); that is, a scientific explanation embeds the explanandum phenomenon into a pattern of counterfactual dependencies. In the physical sciences explanations provide mathematical models of a phenomenon that embed it into a pattern of functional dependencies, which informs us how features of the explanandum vary with the values of the variables used to represent the phenomenon. In a sense, this account puts rather weak constraints on what counts as an explanation: functions are easy to come by. What distinguishes putative explanations from one another is not so much the question whether a given account is an explanation or not—even Moliere's dormative virtue is weakly explanatory, one could claim—but rather the relative strength of an explanation. The goodness of an explanation depends on its accuracy, strength, and simplicity. An explanation is stronger, if it includes more factors that make a difference to the occurrence of the explanandum. The consideration of simplicity pulls in the opposite direction: An explanation is simpler if it does not add irrelevant details. Both these conditions, as well as what counts as sufficient accuracy, are context-dependent. Including certain variables in a model of a phenomenon may be explanatory important in one context, while the same variables may be adding irrelevant details in another. The best explanation is one that scores best on some weighted average of these criteria, where again there is no context-independent algorithm for computing this average.

A consequence of this account is that an explanation that is more accurate than all of its rivals need not be the best explanation of a phenomenon. In fact, in most contexts it will be case that a microscopic quantum-mechanical account of the state of all the molecules composing the water will be taken to offer much too much unnecessary details for successfully explaining the water's wave-like behavior. Thus, in most explanatory contexts a full microscopic quantum mechanical model would be explanatorily inferior to the classical model *even if per impossible* we were able to cognitively grasp the details of the former model. What is crucial for an understanding of the behavior of water waves (in most contexts) is an understanding of the general patterns that transcend the details of the given case—patterns that arguably would be lost in the minute details of a quantum mechanical model with on the order of 10^{25} variables and that transcend the details of a putatively quantum mechanical micro-model. Thus, even if we were to grant that quantum mechanics provides us, at least in principle, with a perfect model of the behavior of water, this would not render the distorting models superfluous: in many contexts the distorting models still provide us with the best explanation of the behavior of water and their explanatory success depends precisely on the fact that these models are highly idealized and distorting.

But in fact we have granted much too much to the objection, for we do not actually possess a perfectly accurate and complete quantum mechanical model of wave or diffusion phenomena. This brings us to the second issue—the foundationalist assumption that there exist perfectly accurate, fundamental models, constructed with the help of our most fundamental theories, underlying the idealized higher-level models we use in practice. I now want to challenge this assumption. First, quantum mechanics has its own limits in scope and accuracy—current quantum mechanics, too, distorts and is not the final and correct theory of the world. Second and more importantly, even if we were to belief that present day quantum theory was exactly correct wherever it can be applied, it still would not provide us with models of the macro-phenomena at issue here. We have so far imagined that we were somehow given a quantum-mechanical model of macroscopic bodies of water, but that is of course only an impossible fiction. To actually construct any such model, we would have to solve the Schrödinger equation for on the order of 10^{25} variables—something that is impossible to do and far beyond our computational capacities.

At this point one common reply is to insist that even if it is impossible actually to solve the Schrödinger equation for macroscopic systems, the theory nevertheless provides us with models of arbitrary complexity. The equation defines a class of models, many of which we of course never construct explicitly. Indeed, any physical theory has many, many more models than the ones scientists have actually constructed and actually used. Quantum mechanics contains a model of the hydrogen atom, of Bose-Einstein condensates, but also, one might argue, of any arbitrary system. If a solution of the Schrödinger equation exists for certain arbitrarily complex initial conditions for systems consisting of 10^{25} particles, then simply in virtue of being in possession of the equation we thereby also are given a model of such systems, even if we do not know how to—or are practically unable to—explicitly construct this solution.

But if van Fraassen's account of scientific representation is correct, then the mere fact that an equation has solutions in addition to the ones actually constructed does not imply that whenever we possess a theory we thereby also are in possession of a large range of models of arbitrarily complex systems governed by that theory. That quantum mechanics provides us with a model even of macroscopic bodies of water is supposed to follow from the claim that among the set of solutions to the Schrödinger equation are ones that are structurally similar to bodies of water. Yet, as we have seen, according to van Fraassen's account no structural relationship between a model and a phenomenon can on its own suffice to make the model a representation of that phenomena (see also van Fraassen 2008, 250). Rather something is a representation only if it is *used* to represent a thing. But since we do not even actually *have* the quantum mechanical initial state of the system of water let alone a solution of the Schrödinger Equation for that system—since there is no way for us to pick out the appropriate solutions from the class of solutions defined by the Schrödinger equation—we cannot *use* the putatively existing solution to represent anything.

But is it not correct that the Schrödinger Equation has many more models than the ones actually constructed by us? And if we allow that the Schrödinger equation has models for arbitrarily complex initial conditions, does it not follow that among the equation's models there will be some with initial conditions representing the state of the body of water? This reply, however, trades on an ambiguity in the term 'model'. There are two quite different senses of model between which we have to distinguish carefully. On the one hand, there is the notion of *model as representation*, according to which a structure is a model of a thing just in case it is used to represent that thing. That is, something *is a model of some object or system* in virtue of representing the object or system. And if van Fraassen is right, then nothing is a model in *this sense* without actually and as a matter of fact being used as a representation—the existence of a certain structural relation between the model and the target system is not enough for it to be a representation of the system. On the other hand, a structure is a *model of a set of sentences*, in the *logical* or *model-theoretic sense* of 'model', just in case it satisfies that set of sentences or the set of sentences are true in that structure. A linguistic description of a theory serves to specify the theory's model-theoretic models in the sense in which a set of equations specifies the set of its own solutions. This second notion of model is not an intentional notion. All that is required for a structure to be a model in this sense, is that a mapping from the structure to the set of sentences exists such that all the sentences in the set come out true; it need not be the case that there is a user who takes the set of sentences to be true in that particular structure.

If we accept van Fraassen's account of representation and "there is no such thing as representation apart from or independent of our practice" (2008, 258), then it does not follow from the fact that a set of equations has solutions or models in the non-intentional, model-theoretic sense that exist all along even without us using or being able to construct these solutions, that these solutions are also models in the first, representational and intentional sense. Recall van Fraassen's "*Hauptsatz*": "*There is no representation except in the sense that some things are used, made, or taken, to represent things as thus and so*." Thus solutions to equations that we have not found or constructed cannot be used to represent anything, simply because we cannot use anything that we do not have or do not even know exists. Even if we assume that the Schrödinger Equation has a solution for a system of 10^{25} variables of the kind that we might possibly use to represent a small macroscopic body of water if we were to possess this solution, the *mere existence* of the solution does not imply that there is a quantum mechanical representational model of the waves in a body of water or of diffusion phenomena in water. The idea, then, that our most fundamental microtheories provide us with representational models covering all physical phenomena is a foundationalist myth.

Surprisingly, van Fraassen himself does not draw this conclusion or at least is ambiguous in his views regarding this issue. He considers the question in what sense a theory or equation can be a theory of phenomena not actually encountered in practice—that is, of phenomena that we have not actually described and for which have not actually constructed a model with the help of the theory or equation. Van Fraassen would like to agree with what I described as the common view and "would like to say that if the equation does have [an appropriate] solution—equivalently, if the theory has such a model—then that (equation, theory) does correctly represent that phenomenon." (2008, 250) Later in the book he says:

> The sense in which a theory offers or presents us with a family of models is just the sense in which a set of equations presents us with the set of its own solutions. In many cases, no solutions to a given equation are historically found or constructed for a very long time … though *mathematically speaking*, they exist all along. When the equations formulate a scientific theory, their solutions are the models of the theory. (van Fraassen 2008, 310, italics in original).

The latter passage occurs within the context of a discussion of Nancy Cartwright's view that there are models in science that have an existence that is in some sense independent of the theories with the help of which they are constructed. Thus, van Fraassen here appears to be guilty of not carefully differentiating between the two notions of model that I distinguished above: the solutions of the equations are *model-theoretic models* but not thereby automatically also *representational models*. Even though Cartwright is clearly interested in models as representational structures, van Fraassen invokes the model-theoretic notion of model in his response to her. Yet—putting the point without using the term 'model'—the fact that we can formally define a class of structures that satisfy a set of sentence says nothing about the representational use to which we might put those members of the class that we have in fact explicitly constructed. And van Fraassen himself elsewhere in the book appears to stress this very point:

> There is no such thing as representation apart from or independent of our practice. So how can we say something like "this theory accurately represents that bacterial growth phenomenon" although the relevant model was never constructed and the bacterial colony was certainly not encountered in human practice? The structural relationship between the model in question and the phenomenon, that we just described, does not suffice to make the model a representation of the phenomena. (2008, 249)

And:

> Undoubtedly in many contexts something is called a model only if it is a representation, and the sense in which any solution of an equation is a model of the theory expressed by that equation certainly does not have that meaning. (2008, 250)

Thus, van Fraassen apparently wants to be committed to two ideas that seem to be in tension with each other: on the one hand, the idea that "we would like to say" that if a theory formally has an appropriate solution then it does represent the phenomenon in question even if the solution has not been explicitly constructed; and on the other hand, the idea that there is no representation independent of its being used as such and that "there is nothing useful to be found in 2-place structure-phenomenon relations alone when we try to understand representation." (2008, 258)

His resolution of the apparent tension is to understand the notion of the empirical adequacy of a theory in terms of counterfactual representations:

> If the theory is offered, that amounts to the offer of a range of structures—the structures we call models of the theory—as candidates for the representation of the phenomena in its domain. If this range contains a candidate that would satisfy the structural constraint—if the phenomenon is embeddable in it […]—then the theory is empirically adequate. (2008, 250)

That is, offering a theory amounts to offering a range of model-theoretic models—of mathematical structures that we could use to represent phenomena. And a theory represents a particular phenomenon within its domain adequately, just in case there is a structure among the range defined by the theory such that *were we to use* this structures as representation for that phenomenon, then the phenomenon *could* be embedded into the model.

There is a certain irony in the fact that van Fraassen appears to be driven to appeal to counterfactuals here, given his well-known view that counterfactuals are inherently and irreducibly context-dependent. What are the truth-conditions of claims of the form 'if we were to use a structure *s* to represent phenomenon *p*, then *p* would be embeddable in *s*'? The problem is that it is not clear, independent of our actual use of a structure to represent a phenomenon, that there is a unique answer as to how the structure would be used to represent the phenomenon and what the appropriate embedding relation would be were we to use the structure as representation. And again it is van Fraassen himself who has convincingly shown, for reasons having to do with Putnam's model-theoretic argument, why there is no unique embedding relation independently of our use of a given structure. In that context, van Fraassen stresses precisely the point I wanted to emphasize here: That we have to be careful about an "illegitimate slippage from 'there exists' to 'we have'" (van Fraassen 2008, 233). While there may exist solutions to the Schrödinger equation for systems of 10^{25} variables, simply writing down the general form of the equation does not imply that we thereby have all of its solutions of arbitrary complexity.

In the next subsection I will summarize some of the considerations leading up to Putnam's argument that are relevant to our discussion here and which will serve to further amplify the claim that the notion of representation is essentially tied to a representations' use.

4. Structures and Use

Physical theories provide us with mathematical representations of phenomena—that is, with abstract structural models. Successful theories, it seems, provide us with models that in some sense resemble the phenomena they represent. One issue in this context is the one that divides scientific realists and empiricists and concerns the question whether we can have good reasons to believe that successful models resemble the physical systems they represent in their entirety or only with respect to their observable substructures. This is not an issue I will be pursuing here. A conceptually prior question is what kind of resemblance can be possible at all between abstract mathematical models and concrete goings on in the world. The most plausible answer to this questions is: structural resemblance. That is, successful theories provide us with models that are structurally similar to the phenomena they represent. This view has a long tradition in the philosophy of science, dating back at least to the *Bildtheorie* of Heinrich Hertz and Boltzmann and is expressed, for example in Hertz's famous dictum: "We form for ourselves inner apparent images or symbols of external objects, and we do this in such a manner that the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the objects pictured."[4]

In a recent paper, Roman Frigg (Frigg 2010) has presented an argument against the structuralist view that scientific representation, at least in the physical sciences, is structural representation and for the view that the model systems at the heart of a physical theory ought to be thought of as hypothetical or imagined concrete physical systems. Frigg points out that in order to make sense of a structural resemblance between a model and its target one has to assume that the target also exemplifies a certain structure but, Frigg argues, "this cannot be had without bringing non-structural features into play." (Frigg 2010, 254) Frigg's argument for this claim proceeds in two steps. First, he argues that since structures are abstract, structural claims about a physical system cannot be true unless some non-structural claims are true as well. Second, he argues that the "descriptions we use to ground structural claims are almost never in fact true descriptions of the intended target system" (*Ibid.*) From which he concludes that "the descriptions that ground structural claims (almost always) fail to be descriptions of the intended target system. Instead, they describe a hypothetical system distinct from the target system." (*Ibid.*) Thus, Frigg wants to conclude, theoretical models cannot merely be mathematical structures but are concrete, albeit merely imagined or hypothetical physical systems.

Frigg's second step begins by echoing a point also made by van Fraassen and to which we will return below: that structural resemblance is possible only between two structures and hence that the subject of the representation also has to be depicted by us as structured in a certain way. Frigg then argues that such a depiction cannot be *merely* structural but has to be concretely 'fitted out.' (For example, that three iron rods can be

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[4] For an investigation of H. A. Lorentz's adoption of Hertz's *Bildtheorie* see (Frisch 2005).

taken to exhibit a certain (abstract) ordering relation is true only in virtue, say, of the rods having different lengths.) So far so good. But what Frigg wants to show is not that a structured depiction of the phenomena is accompanied by a more concrete description, but that the *theoretical* models we employ are hypothetical concrete physical systems. And the fact that any structure attributed to the phenomena needs to be embedded into a concrete description of the phenomena does not imply that theoretical models, too, need to be concretely fitted out. The missing step in the argument is meant to be provided by the observation that the concrete descriptions of the phenomena are almost never true descriptions, which is supposed to make the introduction of hypothetical systems necessary.

I have two worries about this step in the argument, however. First, it is not clear why the fact that the descriptions are false implies that they "fail to be descriptions of the target system." More plausibly, it seems to me, one might hold that even a false description of a system is a descriptions *of that system*—it just may be a descriptions that we do not take to be completely true but that nevertheless plays a useful role in our understanding of the system. Consider as an analogy a caricature that depicts Barack Obama as having huge ears. It does not follow from the fact that the caricature misrepresents Obama that there is some hypothetical person, which the caricature represents completely truthfully. Similarly, it does not follow from the fact, say, that a prepared description represents a certain wooden beam in an idealized manner as perfectly rigid, that the description describes a hypothetical rigid object. Rather the description represents the *actual* wooden beam *as* perfectly rigid. That is, what the description "literally describes," as Frigg puts it, is the actual wooden beam, even though what the description says about the beam is strictly speaking false. Frigg worries that this proposal leaves the notion of idealized description unanalyzed and that any plausible attempt to analyze it would need to introduce the notion of a hypothetical system after all. But one plausible way to cash out the idea that a description is idealized is by reference to some other description or representation of that very system. For example, we might say that a description *D* of a system *S* is idealized relative to some other description *D** of *S* just in case *D* ignores certain features attributed to *S* by *D** or simplifies certain features attributed by *D**. Both description literally describe the target system, but *D* is idealized relative to *D**.

My second worry is that even if we were to grant that an idealized description of a target system required that we introduce a hypothetical or fictional system that the idealized description truthfully describes, it does not follow that we *also* need to think of *theoretical* models as hypothetical concrete physical systems. Frigg proposes that we simply identify the hypothetical systems which satisfy the idealized prepared description of a physical system with the "model systems" of our theories. But this presupposes that our theories imply or at least are logically strictly compatible with the prepared descriptions of the phenomena, for otherwise our theories could not be true of the putative hypothetical system. By contrast, if rather more plausibly we require only that our theoretical models approximately resemble our representations of the target system, then the hypothetical physical systems that would concretely realize the structures of a class of theoretical models cannot be identical to the hypothetical systems that satisfy an idealized depiction of the phenomena. But if we need to distinguish between a prepared description of the phenomena and the theoretical models that are meant to approximately resemble the former, we still need an argument for why we need to think of *both* kinds of models and not merely of the prepared descriptions as hypothetical concrete physical systems.

Frigg does offer a second argument in support of the claim that model systems cannot be purely structural appealing to the fact that "scientists often talk about model systems as if they were physical things" (2010, 253). This is surely right, but it is unclear what lesson we should draw from this observation. One option might be to argue that this is merely points to a surface feature of scientific practice and ought not to be understood literally. Another possibility is that what scientists understand by a model system or a theoretical representation differs widely from discipline to discipline, from context to context and even from scientist to scientist. In some contexts, especially in the more fundamental theories of physics, theoretical models might consist of purely structural, mathematical representations of the phenomena, in other contexts the models might be concrete yet imagined natural systems. One might even grant that the structural models in physics may sometimes be concretely 'fitted out' for didactic reasons or because physicists might find it fruitful to think of a concrete analogy of the system represented, even though their commitment is only to a structural resemblance between the theoretical models and the systems modeled. Perhaps the planetary model of the atom is an example of this kind, which may be best thought of as involving two kinds of model—a purely mathematical model that is taken to structurally resemble actual atoms, and a hypothetical physical system that is proposed as a useful concrete analogy of the system modeled. The way in which atoms are proposed to be like planetary systems is exhausted, however, by the structural resemblance postulated in the mathematical equations governing the Bohr atom.

Thus, contrary to the view that all scientific modeling involves hypothetical concrete physical systems it seems to be more plausible, as Godfrey-Smith has argued (Godfrey-Smith 2009, 104), that scientific modeling involves a "gradient of abstraction". On the one end of the spectrum we find approximate or idealized concrete descriptions *of a target system*, without the introduction of fictional entities representing the target, as I suggested above. On the other end of the spectrum are the mathematical models of theoretical physics, which sometimes are investigated as purely mathematical structures in their own right, to some extent independently of possible empirical applications. Somewhere in the middle are fictional concreta—hypothetical physical systems—which can play a variety of different roles, including that of direct representations of the phenomena, as Frigg argues, or that of offering concrete structural analogies to systems represented mathematically.

Pace Frigg, then, I agree with van Fraassen that scientific representations, at least in the mathematical sciences, are in the first instance abstract mathematical structures that are intended to structurally resemble their target. There is, however, a well-known problem for the view that scientific representation is purely structural representation: structural resemblance, it seems, is much too easily to be had. This point is expressed, for example, in Putnam's model-theoretic argument, which argues the following: as long as the physical system that we want to model is composed of—or is taken to be composed of—a sufficiently large number of parts, there will always be a mapping from the variables of the model onto parts of the system such that the system and the model exhibit similar structures. Van Fraassen illustrates this point with the example of a blank sheet of paper that can be taken to provide an accurate map of the streets of Paris. As long as the sheet of paper has enough distinguishable elements (due to small unevennesses of the surface of the sheet), then there will be some function that maps the landmarks and streets of Paris onto the sheet of paper such that the sheet of paper thus structured is isomorphic to the structure given by the network of Parisian streets The problem is how to single out from the myriad possible mappings from a model onto a phenomenon one particular mapping as the intended one, with the possibility that our model turns out not to resemble the phenomenon under the intended mapping, as we would like to conclude in the case of the sheet of paper. If there is no additional constraint that allows us to distinguish permissible from impermissible mappings, then the claim that there exists an appropriate structural resemblance between a model and some physical system turns out to be nothing more than a claim about the number of elements of the model and the system.

David Lewis replied to Putnam's argument by arguing that there is an additional constraint on the mappings given by preferred or natural divisions and relations among objects in the world. A representation is successful, according to Lewis' proposal, not merely if the physical system represented can be structured in some way that is isomorphic to the representation, but only if the representation is approximately isomorphic to the structure given by the natural kinds out of which the system is composed. That is, according to Lewis, nature itself has a preferred or natural relational structure and a theory's models are intended to represent this structure as accurately as possible. But aside from worries about the metaphysical commitments implied by Lewis's reply, it is not clear how general his strategy can be applied, since the kinds invoked by all but our most fundamental scientific theories are not good candidates for being natural kinds.

Putnam's own solution to his puzzle is to advocate a deflationary view of reference combined with the view that our use of our representations fixes their meaning (see also Frisch 1999). Van Fraassen (2008) agrees at least with the second part of this and further amplifies it as follows. Rejecting Lewis's anti-nominalism appears to leave us with a problem: how can an abstract mathematical structure resemble something in nature that is not abstract? We said that the appropriate resemblance relation between the mathematical models of our theories and the phenomena they are intended to represent is one of structural resemblance. But if we reject the idea that the world itself exhibits a preferred relational structure given by the natural kinds, then what are the structures in nature that our scientific models are intended to resemble? Van Fraassen's answer to this problem once more emphasizes the role of the user in representation. Theoretical models, he maintains, are intended to resemble *data models of the phenomena*, which are constructed through the "selective relevant depiction of the phenomena by *the user of the* *theory* required for the possibility of representation of the phenomenon." (2008, 253). That is, the phenomena, which our theoretical models are meant to represent are structured by us relative to our interests: "the phenomenon, what it is like taken by itself, does not determine which structures are data models for it—that depends on our selective attention to the phenomenon." (2008, 254)

The overall picture, then, that emerges consists of two stages. We represent a phenomenon by what van Fraassen calls a 'data model' of the phenomenon and which provides a structured depiction of the phenomenon; a successful theory provides us with theoretical models into which data models of the phenomena within the theory's domain can be approximately embedded, where it is our use of the theoretical models that singles out the intended embedding. Thus, the user enters the account of scientific representation at two places: first, in the depiction of a phenomenon as structured in a certain way; and second, in taking a model to represent the phenomenon, depicted as thus structured.

One might object that in requiring of our theories' models only that data models can be approximately embedded into them we are committing ourselves to van Fraassen's empiricism. Yet the account of representation is independent of constructive empiricism. Someone who takes theories not merely to represent observational substructures of a phenomenon could replace van Fraassen's notion of a data model with a notion of phenomenological model that includes in its depiction of a phenomenon also an unobservable substructure not represented in the data model. One might also worry with Cartwright that the first step in depicting a physical system is not yet a mathematically precise data model, which often already involves a significant amount of theoretical analysis, but rather a still somewhat informal prepared description. The resulting picture is somewhat more complicated than the one suggested by van Fraassen and includes a prepared description, data models, phenomenological models, and theoretical models. We test our theories by examining whether a data model can be approximately embedded into a theoretical model; if the answer is 'yes', then this provides us with reasons to believe that the theoretical model structurally resembles the phenomenon as formally depicted in the phenomenological model, and that our initial prepared description has proven itself.

Another worry is that on this account it may seem that we lose the ultimate link of our theories to reality, since we never appear to get beyond the phenomena as described by us (either in a data model or in a phenomenological model). Van Fraassen reply to this objection is to argue that the worry disappears once we appreciate that *for us* there is no difference between the question whether a theory fits a phenomenon and the question whether a theory fits that phenomenon *as represented by us*. That the two questions are equivalent for us, he maintains, is a pragmatic tautology. Thus, the gap that the objection tries to exploit cannot be coherently expressed by us. Of course we can ask if *someone else's* structured depiction of a phenomenon is adequate or appropriate. But we cannot do this by contrasting the depiction by the phenomenon with the phenomenon itself, but only by comparing the depiction with the phenomenon as represented *by us*.

5. Anti-fundamentalism

For our present purposes the crucial point is that considerations from Putnam's model-theoretic argument further amplify the pragmatic dimension in the notion of representation and provide additional support for van Fraassen's *Hauptsatz* according to which a structure is a representation only if it is used as a representation. But if all this is correct, then it seems to follow that the threat of Putnam's argument can only be avoided for structures that we are *actually* using as models and this conclusion seems highly counterintuitive. In accepting Newtonian physics, say, are we not committed to the claim that the theory successfully applies to planetary systems yet to be discovered and systems of billiard balls never explicitly modeled? Any theory's domain, it seems, extends well beyond the class of phenomena for which we have actually constructed models. How, then, can we combine this seemingly obvious point with the lessons of Putnam's argument?

It seems to me that we need to distinguish carefully between the kind of example van Fraassen himself mentions when he discusses this issue—that of a colony of bacteria located somewhere in Antarctica long before the first humans appeared on Earth—and the example we discussed above: a putative quantum mechanical micro-model for the macro-behavior of water.[5] Van Fraassen asks whether we can say that a theory of exponential

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[5] Van Fraassen himself (2008, 25-6) discusses the putative worry as to how our models might be able "to represent something that has not yet entered our acquaintance". I agree with van Fraassen that this worry does not genuinely arise. My worry here is, as it were, the mirror-image of this: How can we represent anything with models that have not yet entered our acquaintance?

growth adequately represents the evolution of this colony, even though by hypothesis no model for this particular phenomenon was ever offered. His reply, as we have seen above, is to appeal to a counterfactual account of empirical adequacy: the theory is adequate if among the solutions to its equations is one defining a structure that would satisfy the relevant constraints on adequacy if it were used to represent the colony's evolution. The worry raised by Putnam's argument is whether this counterfactual has reasonably well-defined truth conditions. I want to suggest that the answer is 'yes' in the present case, since scientists actually and as a matter of fact use models of bacterial colonies to represent their growth and arguably this practice sufficiently constrains how we would represent the Antarctic colony were we to do so. That is, scientists actually depict bacterial colonies through data models that are appropriate for a representation of the colonies' evolution in terms of exponential growth models; and scientists actually use the latter models to represent bacterial colonies. Arguably, this practice sufficiently constrains what it would be to provide a data model of the Antarctic colony—that is, what it would be for us to selectively structure the phenomenon in a way that is relevant to exponential growth theory. As van Fraassen correctly emphasizes, however, the notion of relevance here is relative to a user and a specific context of use:

> *There is nothing in an abstract structure* ***itself*** *that can determine that it is the* ***relevant*** *data model, to be matched by the theory*. A particular data model is relevant because it was constructed on the basis of results gathered in a certain way, selected by specific criteria of relevance, on certain occasions, in a practical experimental or observational setting, designed for that purpose. (2008, 253, italics and emphases in original)

There is a well-defined answer to what it would be to depict the Antarctic colony and embed its data model into a model of exponential growth only because the situation so closely resembles phenomena we have actually modeled and which can therefore provide appropriate criteria of relevance.

The situation is dramatically different in the case of the putative micro-model of water—for the solution to the Schrödinger equation for 10^{25} variables which we have posited exists, but which do not actually possess and cannot as a matter of fact use to represent macroscopic bodies of water. We do not actually have the relevant prepared descriptions for macroscopic bodies of water to be matched by a microscopic quantum mechanical model and we do not have actual examples of how a data model might be embedded into a putative quantum-mechanical micro-model. Thus, there is neither a well-defined answer what the relevant data model would look like, completely independently of any actual modeling practices, nor a well-defined answer what the appropriate embedding relation would be. It is utterly unclear, then, what it would be for the range of structures defined by the Schrödinger equation to satisfy van Fraassen's condition of empirical adequacy—that is, for the range to "contain a candidate that would satisfy the structural constraint" that the phenomenon would be embeddable in it (2008, 250). The lesson of Putnam's argument is that there will be some mapping from the 10^{25} variables onto bits of the body of water such that the theory comes out true, no matter what its details are. And in this case there is no additional constraint—no practice of actually modeling the wave or diffusion behavior of macroscopic bodies of water microscopically—that can serve to single out an intended mapping.

In response one might try to appeal to our actual practices of modeling simple microscopic systems quantum mechanically as providing the relevant constraints, but implicit in such an appeal would be a commitment to natural kinds—a commitment that independently of our actual practices of modeling macroscopic bodies of water in certain practical and experimental settings there is a preferred and privileged microscopic relational structure that would be the correct phenomenological model for an application of quantum mechanics to the system. Contrary to the nominalism defended by van Fraassen one would have to be committed to the idea that for each phenomenon taken by itself there is a determinate answer which structures are appropriate data- or phenomenological models for it, independently of "our decisions in attending to certain aspects, to represent them in certain ways and to a certain extent". (2008, 254).

6. Conclusion

If the pragmatic and structuralist account of representation outlined and defended here is correct, then this has far-reaching implications for how we think about scientific theorizing. First, the view undermines what Cartwright has called "the vending machine view" of theories, according to which the representational content of a theory is given simply by stating a set of sentences or by defining a model-theoretic class of models, independently of a theory's users. According to van Fraassen's account presented here theories do not have any representational content independently of our actually using them to construct models of the phenomena.

Second, the view has radically anti-reductionist and anti-foundationalist implications of a kind van Fraassen himself appears to be reluctant to accept. At first sight it might seem that the view severely and dramatically restricts the domain of a theory to those phenomena for which we *actually* have constructed a model. This, I take it, would amount to a *reductio* of the account of representation. The consequence can be avoided by distinguishing between 'horizontal' and 'vertical' counterfactual extensions of a theory's domain of validity. Our actual modeling practices sufficiently constrain what it would be to construct adequate models of other phenomena of the same kind as the models we have actually constructed. But our practices do not license a vertical extension of our theories of the kind posited by physical foundationalism.

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