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Modelling and representing: An artefactual approach to model-based representation

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ABSTRACT

The recent discussion on scientific representation has focused on models and their relationship to the real world. It has been assumed that models give us knowledge because they represent their supposed real target systems. However, here agreement among philosophers of science has tended to end as they have presented widely different views on how representation should be understood. I will argue that the traditional representational approach is too limiting as regards the epistemic value of modelling given the focus on the relationship between a single model and its supposed target system, and the neglect of the actual representational means with which scientists construct models. I therefore suggest an alternative account of models as *epistemic tools.* This amounts to regarding them as concrete artefacts that are built by specific representational means and are constrained by their design in such a way that they facilitate the study of certain scientific questions, and learning from them by means of construction and manipulation.

1. Introduction

The question of representation arose in the philosophy of science only relatively recently although the idea of representing the world accurately has been central to our common conception of science and to the philosophical discussion on realism. Yet it was not until the beginning of the 2000s that representation as a specific topic began to interest philosophers of science more generally. Once started, the philosophical discussion focused almost exclusively on modelling. This may seem odd given that scientific endeavour employs manifold representations that are not readily called models. Such representations include visual and graphic displays on paper and on screen, such as pictures, photographs and audiographic and 3D images, as well as chart recordings, numerical representations, tables, textual accounts and symbolic renderings of diverse entities such as chemical formulas. Why, then, should the discussion on representation have arisen precisely in the context of modelling?

Part of the answer lies in the history of the philosophy of science. Once the semantic approach had detached itself from the linguistic paradigm of the received view and began to conceive of theories as extra-linguistic entities, as families of (theoretical) models, the question turned to how these entities were linked to the world. Unlike propositions and sentences, terms such as "true" and "false" did not seem suited to dealing with the relationship between models and their target systems. "Representation" seemed to be more appropriate—and flexible. As Woodward noted: "The notion of [adequate] representation is a more general idea than the notion of a statement's being 'true', with representation having to do with a qualitative notion of 'fit' between a model and world—a notion that admits of degrees" (2002, p. 380). Of course, the conviction that models are representations is of far more distant origin than the semantic approach to theories in its various guises. One of the criticisms levelled at the syntactic account was precisely that it neglected the representational role of models (see Portides, this issue).

However, with regard to models, the notion of representation has also proved to be problematic because the notion of "degrees of fit" does not, after all, suit the representationalist paradigm, which makes more or less accurate correspondence between the model and its target object the criterion of fit. Models contain idealizations, simplifications, approximations, fictional entities and so

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on, which seem to make them hopelessly inaccurate and distorted representations of the world. Indeed, very soon in the discussion on scientific representation the strong accounts, which attempted to explain representation in terms of the respective properties of a model and its target system, gave way to the deflationary pragmatist view that any such substantive accounts were doomed to fail. I will trace this development in what follows. In line with the pragmatists I doubt the existence of a substantive philosophical analysis of scientific representation that could account for, on a general level, how and in virtue of what models give us knowledge. Yet it seems to me that a lot more can be said about model-based scientific representation if only it is not approached solely from the representational perspective. This certainly seems like a paradoxical claim. However, I will argue that the relative unfruitfulness of the representational approach to model-based representation is due to its narrow scope. The characteristic unit of analysis of the representational approach, the relationship of a single model and its supposed real target system[1], is too limiting in that it pays no attention to the models themselves as unfolding, constructed entities, or to the model-based theoretical practice that typically proceeds on the basis of many related, and also complementary, models (see Peschard, 2009). Moreover, with its focus on the formal, or general, features of a representational relationship between a model and a target system it also neglects the actual representational means with which scientists go on representing—a rather strange consequence of a *representational* approach to models.

I therefore propose that rather than approaching models from the representational perspective, we could better capture many of their epistemic qualities if we considered them from a productive point of view, as purposefully constructed artefacts—that is, as *epistemic tools*. As epistemic tools models are constructed in the light of certain scientific questions and they make insightful use of available representational means and their characteristic affordances. From this perspective models function as external tools for thinking, the *construction* and *manipulation* of which are crucial to their epistemic functioning. Moreover, the notion of models as epistemic tools also provides a new solution to the misrepresentation problem. The point is that the highly idealized and simplified construction of models need not only be seen as a shortcoming, something that needs to be made good by referring to their other virtues or to their future correction by de-idealization. On the contrary, it is often part of a consistent epistemic strategy making cognitive use of the constraints built into a concrete artefact, a model.

In the following I discuss this suggestion in detail, with reference to the discussion on models and representation in order to justify my impression that the representational approach to models does not succeed in accounting for the epistemic value of modelling as a specific theoretical practice. First, however, I will briefly discuss the concepts of representation and representationalism (sections 2 and 3). The point I wish to make is that my intention is neither to contest the notion of representation *per se*, nor to contradict the fact that many scientific models successfully represent some external target systems. My critique is rather directed against those representational accounts that assume the existence of some kind of *privileged* relationship between a model and its supposed target and thus set the representational relationship quite apart from agents and the intended uses of models. In this I join the pragmatists of scientific representation. However, I also wish to step outside the traditional representational problematics and question whether an alternative account exists, and if so how extensive it is.

2. Representation and representationalism

The word representation comes from the Latin *repraesentatio*, derived from repraesentare, which had dual semantics referring to both the rhetorical impact of "vividly recalling" and the economic impact of "immediately paying" (Ker, 2007, see also Crane, 1995a).[2] Many other meanings were subsequently attached. In her etymological study of the concept of representation Pitkin (1967) describes the meanings of the word in the following way:

> It can mean to make them [inanimate objects] literally present, bring them into someone's presence, accordingly it also comes to mean appearing in court in answer to summons, literally making oneself present. It can also mean the making present of an abstraction through or in an object, as when a virtue seems embodied in the image of a certain face. And it can mean the substitution of one object for another (Pitkin, 1967, p. 240).

The modern usage of representation as "standing for" developed from the latter meaning in the above quotation. There have, of course, been many other conceptions, but representation as "standing for" seems to be the most common and prevalent (see e.g., Crimmins, 1991, p. 791; Palmer, 1978, p. 262). Thus Prendergast (2000), for instance, discriminates between two basic meanings of the term: the older "re-presentation" and the more recent "standing for". Instead of striving to produce the illusion of presence—to re-*present*—the representative relation of standing for is that of substitution, of substituting something absent with something present. The substitution can take the form of *simulacrum*, but it is a form of representation as making present (in the older sense) only if it produces an illusion of presence by virtue of being an accurate replica of the real thing.

In contrast to "re-presentation", representation as "standing for" is not to be confused with the thing itself. It is typically approached through the metaphors of portrait, map or mirror: what they have in common is that they are all renderings of an "original" in a different medium. Thus the function of representation as "standing for" is to bring knowledge: it "consists of the presence of something from which we can draw accurate conclusions about the represented, gather information about the represented because it is in relevant ways like the represented" (Pitkin, 1967, p. 81). This idea comes close to the idea of models providing surrogates for reasoning (Suárez, 2004; Swoyer, 1991).

In terms of knowledge, the idea of "standing for" is productively ambiguous: Pitkin notes how representation as "standing for" seems to require a certain distance or difference as well as resemblance and correspondence (1967, p. 68). Indeed, it is exactly the difference between the representation and its target that makes it possible to reflect the target in the first place. Yet it also creates the epistemological problem of how one thing can truthfully depict quite another thing (Crane, 1995b). If human cognition is mediated by representation, then there appears to be a need for an account of how representations can depict or refer to their objects. This is the aim behind the various accounts of representation in the field of the philosophy of the mind and language, and recently in the philosophy of science.

Representation as "standing for" is embedded in *representationalism.* The term was originally coined in the philosophical discussion on perception to refer to a position according to which immediately experienced sense-data, combined with the further beliefs that are ultimately based on them, constitute *representations of the independently existing* external objects such that we are justified in believing to be true (BonJour, 2007). The implication

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[1] Elsewhere I have called this unit of analysis the *model-target dyad* (Knuuttila, 2009b). I adopted the idea of unit of analysis from Paul Humphreys (2004). He has applied in an insightful way this notion, which plays an important role in the methodology of the social and behavioral sciences, to the analysis of the computational science.

[2] The *Oxford Latin Dictionary* gives one additional meaning to representation: an image or a representation in art.

is that the sensing and knowing mind cannot have direct acquaintance with its objects. It can approach them only through internal representations, which are assumed to depict them accurately. In its present usage the term representationalism has loosened its ties with perception and also covers (philosophical and other) theories that conceive of knowledge in terms of representations that reproduce accurately, i.e. stand truthfully for, mind-independent real entities. Such representations standing for reality can be ideas, observations, beliefs, concepts, propositions, neural states, or scientific models (see Wright, 1993). The crucial difficulty with the representationalist theory of mind is that internal representations are supposed to stand for something else, but there is no access to this something else except via another representation. More generally, if knowledge is bestowed in (internal or external) representations, what links these representations to the world?

The philosophical answer to this representationalist problem has been to build knowledge on what is ''given'' in sensory experience, or to look for privileged representations[3] that would ground knowledge: immediate ideas, concepts, logical forms, or Husserlian ''essences''. Representations of these sorts are attributed, as Brandom has put it, ''a *natural* or *intrinsic* epistemic privilege, so that their mere occurrence entails that we know or understand something. They are self-intimating representing: having them counts as knowing something'' (2009, p. 6). Not surprisingly, this quest for privileged representations has been typical of the cognitive sciences as well, in which hypothetical entities such as concepts, symbolic structures, mental models, prototypes and schemes are ascribed to our minds in order to explain our cognitive capabilities.

What seems evident is that the numerous contemporary critics of representation are ultimately not criticizing the possibility of successful representation, which seems an anachronistic reaction in view of our increasingly technologically mediated life-style. They are rather questioning the representationalist view of knowledge and its reliance on privileged, or accurate, representations (e.g., Pickering, 1995; Rorty, 1980; Rouse, 1986). Interestingly, it is possible to see the recent discussion on scientific representation as one variant of the representationalist predicament in that it has centred on whether or not *structural relations* should be given a privileged status in the analysis of scientific representation. In the next section I briefly review the current discussion on models and representation. In anticipation of my further argument I will show how a certain impasse has been reached in this discussion. Whereas the strong representationalist accounts fail to present an adequate notion of representation and impose too strict success criteria, deflationist accounts remain too minimalist to assess the epistemic value of models. I will argue that this situation could be avoided if we did not choose the representational relationship between a single model and a certain target system as the basic unit of analysis, the inspection of which is expected to provide some insights into how we gain knowledge by modelling, and how this knowledge is justified. In contrast, I suggest broadening the unit of analysis, which entails also a closer look at the models themselves, thereby making it possible to differentiate between various kinds of scientific representations and to single out what is specific to model-based representation.

3. Models and representation

Scientific models come in a variety of guises (e.g. mathematical models, scale models, computer models, model organisms) and have multiple representational roles due to their multiple roles in science (see Frigg and Hartman, 2009).[4] Firstly, they may represent theories either by interpreting the axioms of the theory or specifying the general laws as they are applied to particular systems. Secondly, given the use of sophisticated statistical techniques in preparing data for testing or confirming theories the results of these procedures have been called models of data (Suppes, 1962). Thirdly, models may represent some selected aspects or parts of the world. The recent discussion on scientific representation has concentrated on this representational role, motivated by the general agreement among philosophers of science that models give us knowledge *because* they represent some real-world ''target systems'' (e.g., Bailer-Jones, 2003; Contessa, 2007; da Costa & French, 2000; French & Ladyman, 1999; Frigg, 2002; Giere, 2004; Mäki, 2009; Morrison & Morgan, 1999; Suárez, 1999). As a result several accounts of scientific representation have been proposed.

Although these accounts vary considerably, they share the same basic unit of analysis: the relationship between a single model and its supposed real target system (i.e. model-target dyad, Knuuttila, 2009b). The term ''target system'', or simply ''target'', has been used to refer to what is represented, such as a physical object, a process, a population or a phenomenon. Some philosophers, including Bailer-Jones (e.g., 2009) and van Fraassen (e.g., 2008), propose that models represent phenomena, whereas others such as Suárez (e.g., 2010) prefer to remain uncommitted.[5] As far as the representational relationship is concerned, two sorts of questions have been raised. On the one hand philosophers have been interested in specifying the kind of relation that holds between a model and a target, and on the other hand they have asked what makes this relationship successful. Depending on the analysis, these questions are answered either simultaneously or separately as two different aspects of representation. With regard to the nature of the representational relationship, philosophical analyses of scientific representation fall roughly into two classes depending on whether representation is analyzed in terms of a two-place or (at least) a three-place relation, which also takes into account its users (and, depending on the analysis, also various other factors). I refer to these multi-place accounts as three-place accounts because the crucial difference between the two classes depends on what role is given to use or users in representation, and it is from this decision that other possible factors such as audiences or purposes follow. Although these three-place accounts actually extend the model-target dyad, a point to which I return below, their point of departure is the same as that of the two-place accounts—the relationship of a single model with its putative real target system.

In terms of representational success the intuitions of philosophers differ as to whether the analysis of representation should also account for what counts as its success, i.e. whether representation should be regarded as a success term. Whereas two-place accounts deliver an account of success as a by-product of their analysis of representation, three-place accounts separate these two questions with their various views on whether success should be added to the analysis, and if so, according to what criteria. (The question of accuracy of representation is related to the question of success. A host of pragmatically inclined philosophers have variously argued that representation should be distinguished from accuracy, maintaining that an inaccurate or misrepresenting model M of a target system T can still be a representation of T (e.g., Contessa, 2007; Frigg, 2006; Suárez, 2003)[6]. I prefer to speak about success rather than accuracy as there may be other criteria for

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[3] The term privileged representation comes from Rorty (1980).

[4] The semantic account of scientific theories had a clear definition of models (see below), but there is no longer such consensus. Most philosophers settle for the fact that there is a class of entities in scientific practice that are referred to as models.

[5] The notion of a ''target'' comes from Cummins (1996).

[6] Cf. ''[T]he puzzles regarding the notion of representation are prior to and independent of the issue of accuracy'' (Suárez, 2010, p. 93).

success, such as reliability, empirical adequacy, explanatory power and truth.)

The terminology concerning scientific representation is in flux, perhaps reflecting the relative novelty of this topic in the philosophy of science. Every author tries to impose his or her own distinctions. Although the terms are not shared, for the most part they divide the existing accounts of scientific representation in a like fashion. Thus what I call a strong or two-place account is variously also called a structuralist or semantic account (Frigg, 2006; Knuuttila, 2005), which points to its origin in the semantic view of theories. The recent literature also refers to "substantive reductive theories of representation" (Suárez, 2010) and "informational theories" (Chakravartty, 2010). Three-place accounts in turn have variously been called "pragmatic" or "deflationary" (Giere, 2004; Knuuttila, 2005; Suárez, 2010), or "functional" (Chakravartty, 2010). As shown below, each of these terms serves to highlight different characteristics of the various accounts.

3.1. Strong accounts

Two-place analyses of the nature of scientific representation are based on the intuition that there is something that a model and a target system share that grounds the representational relationship. Consequently, they revert solely to the properties of the model and its supposed target system, and analyze the representational relationship between the two in terms of a morphism of some kind. In mathematics morphism is an abstraction derived from structure-preserving mappings between two mathematical structures. The suggested morphisms in the discussion on scientific representation include isomorphism, partial isomorphism and homomorphism[7]. The structuralist conception of scientific representation is usually cast in terms of isomorphism: a given structure represents its target system if both are structurally isomorphic to each other (da Costa & French, 2000; French, 2003; French & Ladyman, 1999; Suppe, 1974; van Fraassen, 1980). Isomorphism refers to a kind of mapping that can be established between two structures and preserves the relations among the elements. Consequently, the representational power of a structure derives from its being isomorphic with respect to some real system or a data model derived from it; Supporters of the structuralist view take different stands on whether the structures represented should be taken to refer to the underlying structures of real systems, as the so-called structural realists contend, or rather more cautiously to structures of "appearances", as van Fraassen (1980) claims. (Note, however, that in his recent writings van Fraassen advocates a pragmatic approach to representation, e.g. van Fraassen, 2008).

The structuralist account is strong, reductive and substantive. It is strong in the sense that it simultaneously gives an analysis of scientific representation and the condition on which its success rests. It is reductive and substantive in that it attempts to reduce the relationship of representation to the properties of models and their targets. Finally, it is also representationalist in that the attempt to reduce the relationship of representation to isomorphism extracts from actual models used in science a privileged layer, the structure, in virtue of which accurate representation is possible. Moreover, the fact that isomorphism affords a rigorous mathematical formulation adds to its attractiveness in the analysis of scientific representation in that it "makes the intuitive idea of the same structure precise", as Patrick Suppes put it (1967, p. 59). However, the account is riddled with problems. Many of these problems are directly related to the fact that scientific representation is a relation between a representational vehicle (e.g., a model) and a real target, and thus a mere mathematical relation between two structures fails to capture some of its inherent features.

Firstly, isomorphism does not have the right formal properties to capture the nature of the representational relationship: it is a symmetric, transitive and reflexive relationship whereas representation is not. [8] Secondly, it does not leave room for misrepresentation. The idea that representation is either an accurate depiction of its object or not a representation at all does not fit actual representational practice. Thus, if a model represents a target it follows that it is also successful. (This may be one reason why van Fraassen (2008) firmly distinguishes the relationship of representation from its success. Although nowadays he approaches representation in a pragmatic fashion, he stays faithful to his earlier views in grounding the success of models in structural relations). Thirdly, structure sharing is not necessary for representation. Scientific practice is full of examples of inaccurate models, which are difficult to render as isomorphic with their targets. Fourthly and perhaps most importantly, isomorphism does not capture the directionality of representation. Take, for example, the symbols that an extinct civilization has left us: unless we know how to interpret them they lack what Suarez has called representational force (e.g., Suárez, 2004). In science representational force is created and maintained through scientific practices in particular contexts of inquiry.

Structuralists have tried to counter these criticisms in two ways, either amending the structural account in adding to it directionality (e.g., Bartels, 2006), or trying to weaken the conditions that isomorphism imposes on representation by suggesting different morphisms such as homomorphism (Ambrosio, 2007; Bartels, 2006; Lloyd, 1988) or partial isomorphism (Bueno, 1997; da Costa & French, 2003; French & Ladyman, 1999). Both of these alternative morphisms attempt to do away with the problems of misrepresentation and non-necessity. It is worthy of note that in defending homomorphism as an alternative to isomorphism Bartels (2006) suggests that it has to be complemented with a representational mechanism connecting the representational vehicle to its target—even though homomorphism, unlike isomorphism and partial isomorphism, is not a symmetric notion. Thus, whereas Bartels makes an effort to give a fully-fledged analysis of representation, it is indeed questionable whether other structuralists have even attempted to present any necessary and sufficient conditions of scientific representation. Yet it seems that in their conviction that "it involves isomorphism" (French, 2003) they have usually left the rest unexamined. Structural relations provide the privileged foundation on which our knowledge rests.

3.2. Deflationary approaches

Pragmatic approaches make representation less a feature of models and their target systems than an accomplishment of its users (Bailer-Jones, 2003; Frigg, 2006; Giere, 2004; Mäki, 2009; Suárez, 2004; van Fraassen, 2008). These studies criticize the assumption that representation could be regarded as a two-place relationship of correspondence between a representative vehicle and its target, adding users to the representational relationship (and possibly other factors such as purposes and audiences). Two-place analyses attempt, as Suárez (2004) put it, "to reduce the essentially intentional judgments of representation-users to facts about the source and target objects or systems and their properties" (p. 768). According to pragmatic approaches on the other hand, no thing is a representation of something else in and of itself:

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[7] Since nothing in my argument hangs on the exact mathematical formulations of the different morphisms suggested, I do not present them here.

[8] These points derive from Nelson Goodman's famous critique of similarity (Goodman, 1968). For reasons of space I cannot deal with them in detail, and readers are referred to Suárez (1999, 2003) and Frigg (2002, 2006). Suárez has also directed this line of critique towards the similarity account, but it seems to me that the philosophers of science currently favouring a looser (i.e. not mathematical) notion of similarity all take into account users and use (e.g., Giere, 2004, 2010).

it always has to be used by scientists to represent some other thing (Giere, 2004; Teller, 2001). This is a more fundamental point than it may seem at first glance. Apart from their focus on the activity of representing, pragmatic approaches are also deflationary in that they claim that the representational relationship is *irreducibly triadic* in taking into account intended use. Consequently, I do not think that the structuralist and pragmatic accounts could be reconciled as Chakravartty (2010) suggests. He claims that "[a]t the heart of [this] dichotomy ... [is] a conflation of means and ends. It is a conflation of thinking about what scientific representations *are* ... and thinking about what we *do* with them" (Chakravartty, 2010, p. 28). For pragmatists, however, what representations are depends on how we *use* them. Of course, structuralists would not deny that scientific representation as a specific activity involves human agents. Yet their aim of grounding the representational relationship on the factual relations between vehicles and targets is evident in their preference to construe directionality as a mind-independent feature of representations, thereby removing the users from the equation (see Bartels, 2006; French, 2003).

Pragmatic approaches to representation solve the problems with the strong accounts mentioned above: intended uses (or users' intentions) both create the directionality needed to establish a representative relationship and introduce the necessary indeterminateness (given that human beings as representers are fallible). (See Knuuttila, 2005). However, this comes at a price. When representation is grounded *primarily* on the specific goals and representing activity of humans instead of some specific properties of the representative vehicle and the target object, it is deprived of much of its explanatory content: if one opts for a pragmatist deflationary strategy, not much is gained in claiming that models give us knowledge *because* they represent their target objects.

This may lie behind the gesture of adding to the basically pragmatist analysis of representation a further stipulation concerning its success. Rather unsurprisingly, then, what has earlier been presented as an analysis of the representational relationship, i.e., structural relations (van Fraassen, 2008) or similarity (Giere, 2010), is now suggested as a success criterion. As for the various morphisms, they pose too stringent conditions on the success of representation in the light of scientific practice. The case of similarity is trickier. On the one hand, it does not really supply any user-independent success criterion in that it is the users who identify the "relevant respects and sufficient degrees" of similarity. Giere (2010) admits this, arguing that the agent-based approach "legitimates using similarity as the basic relationship between models and the world" (p. 269). On the other hand, similarity is on the verge of becoming an epistemically trivial notion if one goes on to argue, as Chakravartty (2010) does, that the success of models would be incomprehensible "[...] were it not for the similarity between the representation and the thing it represents" (p. 201). This relatively common belief is grounded on the way human (and animal) cognition works by identifying and matching patterns, which makes the idea of similarity so natural and appealing. Yet, as far as science is concerned, any similarity between a model and some real-world target is not simply available but is usually just tentatively inferred. What is available, are the model *results*, but it is quite another thing to declare a fit between them and the available data than to claim that the structure of the model is similar to the underlying structure of some real system. (This is, of course, the reason why empiricists such as van Fraassen do not take this step).

Another possibility is to go deflationary all the way, as Suárez (2004, 2010) has done, and resist saying anything substantive about the representational relationship or its success, in other words whether they rest on isomorphism, similarity or denotation[9], for instance. According to Suárez, substantive accounts of representation err in trying to seek for some deeper constituent relation between the source and the target which could then, as a by-product, explain why the source is capable of leading a competent user to consideration of a target, and why scientific representation is able to sustain "surrogate reasoning". Hence he explicitly denies any privileged relationship between a representational vehicle and its target. Instead, Suárez builds his analysis directly on the aforementioned by-products. His inferential account of scientific representation is two-sided consisting of *representational force* and the *inferential capacities* of the representational vehicle. Representational force results from the practice of using a particular representational vehicle as a representation, determining its intended target. In addition to that the vehicle must have inferential capacities that enable the informed and competent user to draw valid inferences regarding the target. In other words, it has to have an internal structure such that its parts and relations can be interpreted in terms of the target's parts and relations. The success of representation also implies that there are some norms of inference in place distinguishing correctly drawn inferences from those that are not (Suárez, 2010).

The way in which deflationary approaches to scientific representation also point beyond the representational model-target dyad is most clearly exhibited in Suárez's proposal with its stress on the representational activity and the norms of inference governing it. Indeed, quite apart from the discussion on scientific representation, a body of work has arisen that focuses on the practices of model building and use. This, I suggest, has significant implications in terms of how model-based representation should be conceived of, although it simultaneously tends to liberate models from representing definite target systems and considers them independent objects. It also gives good reason to ask whether model-based representation presents us with a specific kind of representational practice that differs in important respects from many other varieties of scientific representation.

4. Models as epistemic tools

The idea of models as independent entities has arisen in various ways in the recent discussion. Whereas for some authors the independence of models means independence from real-world target systems (Godfrey-Smith, 2006; Knuuttila, 2005; Weisberg, 2007a), others stress independence in relation to the theory-data framework (Boumans, 1999; Morrison & Morgan, 1999). What is common to these approaches is an interest in *modelling*, in the construction and use of models.

Michael Weisberg (2007a) and Peter Godfrey-Smith (2006) recently suggested that there should be different kinds of theoretical representational strategies. What distinguishes modelling as a specific theoretical practice of its own are the procedures of *indirect representation* and analysis that modellers use to study real-world phenomena. Indirect representation here refers to the way in which modellers construct simple, ideal model systems to which only a few properties are attributed instead of striving to represent some real target systems directly. This deliberate detour through the hypothetical systems is contrasted with *abstract direct representation,* which proceeds from the abstractions of data in an attempt to identify the key factors accounting for certain behaviour.

The idea of modelling as indirect representation takes a different perspective on the epistemic value of modelling from the representational approach. Namely, Weisberg and Godfrey-Smith argue that in modelling, models are constructed and analysed *before* the relationship between the model and any target system is

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[9] Hughes (1997) presents an analysis of scientific representation based on denotation.

considered, “if such an assessment is necessary” (Weisberg, 2007a, p. 209). Thus modellers may go on studying model systems without giving too much explicit attention to their relationship with the world, which makes models independent of any real target systems. Although Weisberg and Godfrey-Smith play down this suggestion, eventually reverting to representation in general, and similarity in particular, in trying to account for how we gain knowledge from models, I think that they are on the right track. The question that they do not directly pose, but which arises from their suggestion, is this: Why do modellers proceed in the way they do? What cognitive gains does it involve? Presumably it has something to do with how they learn from the construction and manipulation of models quite apart from any determinate representational ties to specific real-world systems they might have.

The importance of this interactive aspect of modelling is stressed by Morrison and Morgan (1999) whose account of models as autonomus investigative instruments focuses on how we learn from models by constructing and manipulating them. However, it seems to me that they leave this crucial insight somewhat underdeveloped. If our aim is to understand how models enable us to learn from the processes of constructing and manipulating them, it is not sufficient that they are considered autonomous in terms of theory and data: they should also be concrete in the sense that they must have a tangible dimension that can be worked on. Consequently, I suggest that models could be seen as *epistemic tools*, concrete artefacts, which are built by various representational means, and are constrained by their design in such a way that they enable the study of certain scientific questions and learning through constructing and manipulating them. The notion of models as epistemic tools highlights the following interlinked characteristics of models that contribute to their cognitive functioning: (i) the *constrained design* of models, (ii) *non-transparency* of the *representational means* by which they are constructed, (iii) their *results-orientedness*, (iv) their *concrete manipulability* and (v) the way their *justification* is *distributed* so as to cover both the construction and the use of models.

4.1. Constrained design

Treating models as artefacts draws attention to the fact that in many fields they constitute objects of knowledge in themselves. This is apparent, for instance, in artificial intelligence and in the development of computational methods. Obviously, building and using models enables us to learn about them, but how does it enable us to learn about the real world (given that the sphere of man-made artefacts and various mathematical and computational methods provides an equally worthy object of study as the natural and social worlds)? Many philosophers have distinguished the study of models themselves from the study of models as a way of learning about reality. Mäki (2009), for instance, makes a distinction between models as surrogate systems that are studied in order to obtain knowledge about the external world, and as substitute systems that are “freely floating subject[s] of inquiry, unconstrained by any concern as to how [they] might be connected to the real-world facts” (Mäki, 2009, p. 36). It seems to me that such a clear-cut distinction is not characteristic of modelling practice. Models are not freely floating objects in need of being linked to the real world: they are already linked to our knowledge of the real world by way of the scientific questions that motivate their construction. Scientific models are typically constrained by their construction in such a way that they make certain scientific problems more accessible and manageable, helping scientists to tackle them in a systematic manner (Knuuttila & Voutilainen, 2003). This is one of the main roles of the idealizations, simplifications and approximations made in modelling.

Idealizations, simplifications and approximations have traditionally been considered distortions or shortcomings of models, made for the purpose of tractability, that should then be corrected and eliminated as the research progresses (see e.g., Bailer-Jones, 2009, pp. 188–189). Although de-idealization is without doubt one important goal of modelling, there is, from the cognitive point of view, something wrong in the idea that scientific representation should aim for as accurate a representation as possible. This ideal makes highly idealized models seem defective at the outset in assuming that the more realistic or truthful in detail the model is, the better it is.

What might appear to be misrepresentation could also be part of a purposeful representational strategy. Indeed, there is another idea of idealization, minimalist idealization[10], which stresses the *selectivity* of modelling. It is based on the idea that one could isolate some causal factors of interest from the workings of other factors and focus on their interrelationships (see e.g., Cartwright, 1989; Mäki, 1992). An analogy to experimental practice, which is also based on manipulating certain elements in controlled environments, supports this. Whereas in experiments other intervening elements are sealed off by experimental controls, models use assumptions to neutralize the effect of other things (Mäki, 2005, p. 308).

Although minimalist idealization captures part of what I mean by the constrained design of models, it also tends to sidestep the actual problems of model-based representation. It takes for granted that we already knew what the relevant causal factors were and could then, as a result of suitable idealizations, study their behaviour “in isolation” (Knuuttila, 2009a). Yet, the problem is often that model assumptions do not merely neutralize the effect of other causal factors. They do much more: they construct the modelled situation in such a way that it can be conveniently mathematically represented, often making the results of a single model dependent on the model as a whole (Cartwright, 1999b). Moreover, it may be difficult to know which of the model assumptions are responsible for the result derived, and neither is it always possible to relax assumptions that are made mainly for the purpose of tractability or in order to derive a certain result (Alexandrova, 2006; Hindriks, 2006). Morrison (2008) refers to this feature of mathematical abstractions when she claims that, as they are often necessary in order to arrive at certain results, “there is no question of relaxing or correcting the assumptions in the way we de-idealize cases like frictionless planes and so on; the abstractions are what make the model work” (p. 110; see also Gelfert, this issue). Thus part of the constrained nature of models is due to the representational media used and their characteristic constraints.[11] This also means that triangulating models with different constraints can be a viable epistemic strategy. For instance in the field of synthetic biology researchers combine mathematical modelling, synthetic modelling and working with model organisms for this very reason (see Knuuttila & Loettgers, in press; Loettgers, 2007).

4.2. Representational non-transparency

These above-mentioned features of mathematical representation show how on the one hand the *representational means* impose their own constraints on the model design, yet on the other hand they facilitate the results derived from it (on the results-orientedness of modelling, see below). More generally, it seems

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[10] On the notion of minimalist idealization, see Weisberg, 2007b.

[11] Gelfert (this issue) discusses at length the constraining role of mathematical formalisms. See especially the sections 4.2 and 4.4 of his article.

that the wide variety of representational means modelers make use of i.e. diagrams, pictures, scale models, symbols, natural language, mathematical notations, 3D images on screen) all *afford* and *limit* scientific reasoning in their characteristic ways. For instance, pictures or graphs both "afford" different kinds of reasoning than linguistic expressions or mathematical equations, which is commonplace in media studies (see e.g., Kress & van Leeuwen, 2001).

The way in which the means of representation are often ignored by philosophers suggests that scientists have, or at least could have, the right representational means at hand, malleable as such and suitable for describing the very aspects of reality that happen to interest us. In other words, more often than not philosophers of science at least implicitly assume that representational means could be transparent in such a way they would neither prevent us from capturing the real structures and processes underlying the phenomena, nor add some features of their own to our theoretical accounts. Or, that we could at least clearly tell apart those features of our scientific representations that are attributable to the phenomena described from the conventions used to describe them.[12] The intricacies of mathematical modelling show that this is clearly not the case.

The reason why philosophers often ignore the effects and consequences of the representational means used in modelling is that they are inclined to think that models are either abstract objects (e.g., mathematical models), or if not (e.g., scale models) that it is the underlying structure that matters. Yet, although it is tempting to see scale models as proportionate to their targets this is often not the case. Scaling is selective and distortive, trading both with the purpose at hand and the medium used. A good example is Galileo's observation that large ships taken out of the water are in danger of breaking down under their own weight (e.g., van Fraassen, 2008, pp. 50–51). When we make scale models, as Max Black points out, our purpose is to reproduce in a relatively manipulable or accessible embodiment, selected features of the "original". Yet there is simultaneously "something self-defeating in this aim, since ... we are forced to replace a living tissue by some inadequate substitute, and a sheer change of size may upset the balance of factors in the original" (1962, p. 221).

Perhaps the principal rationale for not paying attention to the actual representational means of modelling is the idea that models are ultimately abstract entities. As an abstract entity a model can be described using different representational means and is thus independent of its descriptions. However, this approach neglects the way humans, as cognitive agents, are able to use different kinds of representational means. Vorms (in press, this issue) uses the examples of classical mechanics and Feynman diagrams in claiming that different kinds of representational means do not facilitate the same kind of inferences, even if they were equivalent in empirical and formal terms. This point is corroborated in various studies in the cognitive sciences. Zhang (1997), for instance, shows in an experimental setting that different representations of the same abstract structures have different affordances as to how humans are able to understand them. It seems that analogical and visual representations are easier for humans to grasp than those in digital and numerical form. This fits well with the various pronouncements of scientists concerning the virtues of visualization and their importance for scientific understanding (e.g., de Regt & Dieks, 2005).

4.3. Results-orientedness

Whereas the affording and limiting nature of representational means applies to scientific representation in general, Paul Humphreys (2002, 2004) points out a characteristic of mathematical representation that seems to differentiate mathematical and computational models in particular from other scientific representations. He notes the "enormous importance of a relatively small number of computational templates" (2004, p. 68), which are used across the sciences. Such computational templates include functions, sets of equations, and computational methods, for example. What makes them popular is their tractability and solvability, which reflects the results-orientedness of modelling: the starting point is often the output and effects that models are supposed to produce. Instead of directly trying to represent some selected aspects of a given target system—as is conventionally assumed—modellers often proceed in a roundabout way, seeking to build hypothetical systems in the light of the anticipated results or of certain general features of the phenomena they are supposed to exhibit (see Gelfert, this issue).[13] The overall usability of computational templates is based on their generality and the observed similarities between different phenomena. Thus there is an element of opportunism in modelling: the template that has proven successful in producing certain features of some phenomenon will be applied to other phenomena, often studied within a totally different discipline. If a model succeeds in producing the expected results or in replicating some features of the phenomenon it provides an interesting starting point for further model building, whose typical aim is to correct and adjust the template to better suit the domain it is applied to.

Models are also supposed to produce other, preferably unexpected, results apart from the expected behaviour.[14] What is more, the results-orientedness of modelling is indicative of what goes wrong in the current discussion on models and scientific representation. It is usually assumed that models are prototypical scientific representations, whereas it appears to me that Weisberg and Godfrey-Smith are right in claiming that this is not the case. That models are specific kinds of entities built in the light of their results can be attributed to their holistic *systemic* nature, which distinguishes them from many other scientific representations that often fragment and analyze an object or specimen in minute detail (Knuuttila, 2008; Lynch, 1990).

4.4. Concrete manipulability

What is the cognitive point of constructing artificial hypothetical systems? How are they supposed to give us knowledge if not by means of representing more or less accurately some real target system? The suggestion already implicit in the results-orientedness and systemicity of models is that their cognitive value is largely based on *manipulation.* A theoretical model could be seen as a system of interdependencies, whose various features can be studied by manipulating it in the light of its results. That this way of proceeding should give us knowledge is dependent on the theoretical

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[12] One goal of *robustness analysis* is exactly to separate the conclusions that depend on the assumed common core of a model from those that result from the simplifications, distortions and omissions introduced to facilitate the analysis (e.g. Levins, 1993).

[13] With "result" I refer loosely to the predictions, theorems, proofs, or demonstrations achieved by using and manipulating a mathematical model. But, more generally, the results of data analysis may qualify as well, as pointed out by Gelfert (this issue). He discusses the "outcome-orientedness" of mathematical techniques and points out the continuity between the techniques of deriving results and the methods of data-analysis. Gelfert (2009) targets the importance of model results and constraining features of models, too. He argues that rigorous results derived from models provide a genuinely model-specific way of assessing them being also important for understanding the connections between different models.

[14] The results-orientedness of modelling is obviously displayed also by those styles of modelling, whose primary aim is to maximize the precision and accuracy of model's results irrespective of its explanatory value.

information built into the model and the way it facilitates the study of various hypothetical possibilities. This points to the modal nature of modelling: Modellers are interested in studying also different non-actualized and inexistent systems in an effort to thus understand some basic relationships and interactions that might explain the phenomena we encounter (Weisberg, 2007b). Understanding of the possible is the way to understand why the actual emerged and how it functions. Thus modelers can be conceived of as instrumental realists in the sense of Woodward (2003). They are studying "what would happen if various contrafactual possibilities were to be realized" (Woodward, 2003, p. 115).

Although various authors, notably Morrison and Morgan (1999), recognise the importance of the manipulability of models it has gone largely without notice how this feature is related to their concrete material dimension (see however Klein, 2003; Knuuttila, 2005). In other words, any actual manipulation assumes materialisation in some representational medium. In this respect it is important to keep in mind the difference between representational modes and representational media (which together form what I refer to above as representational means). Different representational "languages" such as the pictorial, the symbolic and the diagrammatic constitute different *representational modes*, with which various meanings or contents can be expressed. This is the abstract side of representational means. The *representational media* are, in turn, the *material* means with which representations are produced and in which they are embodied (such as ink in paper, a digital computer, biological substrata and so forth). For instance, natural language is a representational mode that can be realized by different media, for example as speech or as writing.[15] Materiality plays different epistemic roles depending on the models in question and the media in which they are realized. The crucial role of the representational media for how we are supposed to gain knowledge from models is clear in the cases of model organisms and synthetic models, as well as with scale models and computer simulations, which all allow different ways of manipulation. For instance graphics in 3D have created a new mode of interacting with numerical data, which allows users to gain understanding of the modeled objects through intervening kinaesthetically in the simulation (Griesemer, 2004; Myers, 2006). But even in the case of symbols and diagrams, the fact that they are materially embodied as written signs on a paper accounts partly for their manipulability. Klein (2003) describes this aspect of Berzelian formulas in chemistry in the following way: "Their compositional syntax and semantics, their graphic suggestiveness, and their simple manereuvability made it simple to play through various models [...] All this reshuffling of letters and numbers could be done without any syntactic restriction besides additivity" (p. 244).[16] Klein calls Berzelian formulas "paper tools" and her analysis of them shows nicely how their epistemic productivity was due to the way in which the features of their specific representational mode intersected with their material realizations.

This epistemic importance of the concrete manipulability of models can be related to those accounts in the cognitive science and philosophy of mind that stress the role external artefacts play in our cognition. In these studies the idea of a cognitive agent working on the basis of having as complete as possible representations of his environment in his head has been questioned by an alternative conception that can be characterized as "cognition in the wild" (Hutchins, 1995). This economical and embodied cognition uses external scaffolding, environmental clues and cheap tricks in its cognitive tasks instead of creating complete, internal representations of the world. The argument is that the human brain evolved originally to coordinate the body, which made cognition action-oriented rather than reflective. Instead of one single central processor controlling all the cognitive activities, evolution preferred a solution with many, more specialized processors (see e.g. Clark, 1997, 2003; Varela, Thompson, & Rosch, 1991). On this basis it is possible to claim that our cognition is distributed between individuals and artefacts (Hutchins, 1995; Sterelny, 2004) and that it is also largely skill-based and tool-using. From this perspective external representation functions as external *scaffolding* which both narrows the space of information search by localizing the most important features of the object in a perceptually salient and manipulable form, and enables further inferences by making the previously obscure or scattered information available in a systematic fashion (see e.g., Larkin and Simon 1987; Clark, 1997). To this Sterelny (2004) has added that agents who live in unstable and challenging environments need a rich array of decoupled representations that can support a variety of plans. Scientific models are superior examples of such things.

4.5. Distributed justification

Although the construction and manipulation of models may yield knowledge on some potentially relevant interrelationships as regards certain phenomenon of interest, the question of the justification of this knowledge remains still unanswered. If I have managed without the notion of accurate representation so far, surely at this point it starts to get more difficult? Yet it seems to me that despite its intuitive appeal the representational approach does not do much philosophical work in accounting for the justification of models either. Quite apart from the more general problems of structural similarity in its various guises (see above), one could well ask whether and in which ways any structural similarities between models and their targets are available. The model is typically assumed to represent the structure and the behaviour of the system that is supposed to produce the phenomenon of interest. This, in turn, seems to require resources for assessing the representational relationship beyond the comparison of experimental data and model predictions. However, reverting to the assumed characteristics of the representational relationship between a target system and a single model, its purposes notwithstanding, does not seem to provide us with such resources. Furthermore, in scientific practice the fitting of experimental data with models is often a bi-directional process in which the model and the data are tailored to fit each other (Cartwright, 1999a; Koponen, 2007). From this perspective any fit between data and a model is a scientific achievement rather than something that grounds our knowledge claims. Once again, it seems to me that the answer lies in going beyond the representational model-target relationship, in extending the unit of analysis to cover the construction of models as well as the multilayered triangulation process in which different model architectures and model results are being compared with each other and with experimental and observational data. It is important to note that models often carry with them initial justification that is due to the various ingredients they comprise, such as various renderings of empirical data and knowledge, theoretical assumptions, mathematical formalisms, and established modelling methods (Boumans, 1999; Humphreys, 2004; Knuuttila & Loettgers, submitted for publication). Part of the justification also comes from the relevant scientific context: the established empirical findings as well as existing models and results. To seek justification from the supposed relationship models have with some target systems, various pragmatic constraints notwithstanding, does not take enough account of how they are justified in actual

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[15] For the distinction between mode and media, see Kress & van Leeuwen (2001).

[16] In this sense I cannot see why "abstract models" and "material models" should be placed in two distinct classes, the difference between them seems rather to be one of degree and due to the epistemic role their material dimension plays.

model-based practice. In purely philosophical terms, the idea of grounding the knowledge we gain from models in accurate representation may seem impeccable, but then what other evidence could there be for the correctness of a model than what has been built into it, the results it gives, and the inferential links to other bodies of knowledge it establishes? To require, then, that we have knowledge first when some definite representational relationship between the model and some real target system has been forged leaves much scientific work unrecognized and its epistemic value unexplained.

5. Conclusion

Certain notions first attract explicit interest only when they become problematic. This has certainly been the case with representation in the philosophy of science, in which intensive discussion on scientific representation has emerged in the last decade. Models have occupied centre stage in this debate. Although they have traditionally been considered representational, their representational status has nevertheless remained problematic in that they typically contain idealizations, simplifications, approximations and fictional elements. The question has been, as aptly put by Callender and Cohen: "How can [models] represent, if they, well, misrepresent?" (2005, p. 5).

I doubt that any philosophical analysis of representation will, in itself, solve that problem, or tell us on a general level *in virtue of what* do models give us knowledge (which is usually the basic motivation behind the interest in representation). Either such accounts are too strong in their claims about actual scientific models, or else they are too deflationary. Although I tend to support the deflationary account of scientific representation, it seems to me that there is more to be said about how we learn from models if we give up trying to account for their epistemic value in representational terms. Instead of accepting the apparent misrepresentation as a defect—which results from taking accurate representation as a criterion of knowledge—one might consider the motivations and possible cognitive gains of the purposeful misrepresentation that is characteristic of modelling. In line with this idea I have given a productive account of models as epistemic tools, which pays heed to cognitive agents as limited beings and to the limited representational means at their disposal.

Although this account provides an alternative to the representational view of models, it is not directed against representation *per se*. I do not doubt that in many cases we have good reasons to believe that our scientific representations succeed in adequately depicting some real-world targets. My aim was rather to see how else we might approach model-based practice other than assuming in a representationalist fashion that in order for us to learn something from models they have to accurately represent some (selected aspects of) some target systems. The answer to this question lies in the concretely constrained and manipulable nature of models: if they were merely abstract structures it would be difficult to understand how they could give us knowledge except by representing their targets more or less accurately. On the other hand, if they are recognized as materially embodied manipulable objects into which a lot of scientific knowledge is already built, then it is evident that they provide something tangible for us to study and experiment with.

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