Chapter 2
Scientific Representation and the Uses of Scientific Models

2.1 Models and Their Functions

The great variety of models in scientific practice, which is reflected by the somewhat sprawling taxonomies of model-types we encountered in Chap. 1, has led some philosophers to propose quietism as the only viable attitude towards the ontological question of *what a model is*. As Steven French puts it, 'whereas positing the reality of quarks or genes may contribute to the explanation of certain features of the physical world, adopting a similar approach towards theories and models—i.e., reifying them as entities for which a single unificatory account can be given—does nothing to explain the features of scientific practice' [1, p. 245]. (Quietism is usually a position of last resort in philosophy and, perhaps not surprisingly, French's professed quietism has not stopped him and others from developing an elaborate, if controversial, unified framework for thinking about models and theories.) In the present chapter, we will instead follow the alternative suggestion to treat models as 'functional entities' [2, p. 120]; on this view, the various functions of models in scientific inquiry are our best—and perhaps only—guide when it comes to finding answers to any of the more fundamental questions about scientific models, including those about their ontology, epistemic status, confirmation, and so forth.

In exploring the different functions of scientific models, it will be useful to keep in mind some of the distinctions drawn in the previous chapter (Sect. 1.1), notably between *instantial* and *representational* views of models, and among the latter, between *informational* and *pragmatic* versions of the view. The instantial view regards models as instantiations of the axioms of a theory and considers the relationship between models and theories to be the primary locus of philosophical significance. By contrast, the representational view takes models to be a way of accessing the world, thereby shifting attention to the way models represent the world. Various locutions are typically employed in order to characterize what is involved in treating a model as a representation: thus, a model may be said to 'stand

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in for' its target system, or its relation to the target system may be described as analogous to that between a map and the territory it shows. In its most general form, the representational view thus considers models to be 'tools for *representing the world*' [3, p. 44]. '[T]he crux of the problem of representation', as Margaret Morrison puts it, then becomes the following question: 'in virtue of *what* do models represent and how do we identify what constitutes a correct representation?' [4, p. 70] Informational views take representation to be an objective relation between the model and its target, which imbues the former with information about the latter, irrespective of a model user's beliefs or intentions, and regardless of the cognitive uses to which he or she might put the model. This contrasts with more pragmatic versions of the representational view, according to which one cannot 'reduce the essentially intentional judgments of representation-users to facts about the source and target object or systems and their properties' [5, p. 768]. Similarly, in his defence of similarity as the basis of model-based representation, Ronald Giere, although committed to the idea that 'what is said to be similar to what, in what ways, and to what degrees' can always be specified, insists that there are 'many possible specifications depending on the particular interests of those doing the modeling' [3, p. 46]. Model-based representation, thus understood, is essentially a three-place relation between the model, its target, and the model user. The value and function of models then derives from their role in inquiry, i.e. from the way they enable users to draw inferences on their basis about the target system, make predictions, or facilitate other courses of action. In its widest sense, the term 'model' may then be understood, as Günter Abel puts it, 'as a reconstruction of central features of a concrete object, process, or system, which itself becomes a matter of further investigation' [6, p. 33].

Before discussing two specific proposals of how model-based representation comes about—R.I.G. Hughes's DDI account (Sect. 2.3) and Mauricio Suárez's inferential account (Sect. 2.4)—it will be instructive to consider, in the next section (Sect. 2.2), whether there is anything distinctive about *scientific* representation in particular, or whether representation in science is of a piece with representation more generally. The final section of this chapter (Sect. 2.5) will comment on the issue of realism and anti-realism in the context of model-based representation and will broaden the perspective further to also consider non-representational uses of models in science.

2.2 Scientific Representation

Human beings, through biological and cultural evolution, have developed elaborate ways of representing the world around them: via the mental representations that feature in cognition, through language and its ever-expanding vocabulary, and through the deliberate creation of artefacts in art, technology, and science. 'Representation', conceived of as an umbrella term for these various activities and relations, may seem like a nebulous philosophical concept, which might explain why many philosophers of representation restrict their theories to rather more specific domains. Thus, Bas van Fraassen states rather categorically that he 'will have no truck with mental representation, in any sense', claiming that the way philosophers of mind have historically discussed the notion 'has nothing to contribute to our understanding of scientific representation' [7, p. 2]. With respect to scientific representation, van Fraassen's own position has changed considerably over the years. Whereas in *The Scientific Image* [8] van Fraassen argued that '[t]o present a theory is to specify a family of structures, its *models*', and that a theory 'is empirically adequate if it has some model such that all appearances [as described in experimental and measurement reports] are isomorphic to empirical substructures of that model' [8, p. 64], almost thirty years later his position has moved to a much more pragmatic account of scientific representation: '*There is no representation except in the sense that some things are used, made, or taken, to represent some things as thus or so.*' [7, p. 23; italics original] Hence, the notion of 'use' and pragmatic considerations are to be 'give[n] pride of place in the understanding of scientific representation' [7, p. 25], even if a unified 'theory of representation' may turn out to be elusive. In a similar spirit, Morrison argues that even '[w]ithout articulating a specific theory of representation we can nevertheless appeal to some general ideas about what it means for models to represent phenomena or systems' [9, p. 125]. This deflationary approach seems exactly right to me: even in the absence of a widely accepted *general* theory of representation, it seems perfectly possible to characterize *scientific* representation in productive and insightful ways —so long as one's account does not hinge on overly controversial assumptions regarding the general problem.

In his seminal book *Languages of Art* [10], Nelson Goodman aimed to bring together previously separate debates about the nature of representation in language, art, and science, as indicated by the subtitle of the book which promises 'an approach to a theory of symbols'. Traditionally, philosophers have distinguished between *natural* and *non-natural* signs. Whereas non-natural signs acquire their meaning by way of—e.g., linguistic—convention, natural signs stand in certain non-arbitrary (typically: causal) relations that connect them to their target. Thus, the word 'fire' refers non-naturally, simply because as competent speakers of English we take it to be a symbol of fire, whereas smoke is a natural sign of fire since the latter is usually the cause of the former. One might ask where along the natural/non-natural spectrum we should locate representation. Surely it would be too strong to demand that a representation must be causally dependent on its target; indeed, in the example just given, one might find it more appropriate to consider smoke *evidence* of fire (rather than a *representation* of it). Goodman introduces the notion of 'denotation' in order to capture an important ingredient in any representational relationship, namely the fact that a representation 'stands in for' its target (where such a target, in fact, exists).[1] Denotation, thus understood, frees the

[1]Whereas in *Languages of Art* (1968/1976), Goodman offers no definition of the term 'denotation', in *Of Mind and Other Matters* (1984), he writes: 'This common relationship of applying to or

representational relationship from the constraints of causality or resemblance, in that it may be entirely stipulative: 'almost anything may stand for almost anything else.' [10, p. 5]

From this it does not follow that any attempted act of denotation will automatically succeed at representing a target. For one, in order for there to be an instance of denotation, what is being denoted must exist; if the purported target does not exist—for example, because we are dealing with non-existent entities such as unicorns or the ether—denotation must necessarily fail. Goodman hints at workarounds for this problem, by distinguishing between a) what a representation *denotes*, and b) what *kind* of representation it is—so that, say, a certain picture could be a unicorn-representation (i.e. would belong to the class of unicorn-images) without thereby denoting a unicorn (since unicorns do not exist): 'A picture that represents a man denotes him; a picture that represents a fictional man is a man-picture; and a picture that represents a man as a man is a man-picture denoting him' [10, pp. 27–28]. Furthermore, successful representation of an existing target requires more than mere denotation. While Goodman, in various places, gives the impression that anything may represent anything else, he also acknowledges that *successful* representation is subject to de facto constraints. Even before issues of faithfulness, accuracy, and truth arise, there is the question of whether a given representation makes relevant information salient and whether it can draw on entrenched denotative practices: 'Representation […] is apt, effective, illuminating, subtle, intriguing, to the extent that' its originator 'grasps fresh and significant relationships and devises means for making them manifest' [10, pp. 32–33].

Much of Goodman's philosophy is an attempt to negotiate the tension between, on the one hand, the arbitrariness that seems to follow from the radical contingency of thought and action and, on the other hand, the apparent stability of our conceptual frameworks and practices. Hence, while 'there are countless alternative systems of representation and description', these are themselves 'the products of stipulation and habituation in varying proportions' [10, p. 40]. To be sure, we often find that we inherit established notational systems and representational formalisms, which—though in principle arbitrary—for this very reason are no longer 'up to us'. Although we have some degree of choice among such systems, given a particular system, 'the question whether a newly encountered object is a desk or a unicorn-picture […] is a question of the propriety, under that system, of projecting the predicate "desk" or the predicate "unicorn-picture" […], and the decision both is guided by and guides usage for that system' [10, pp. 40–41]. Yet critics of Goodman have found this appeal to entrenchment and past usage a little too casual: if the only resistance we face when introducing new concepts and predicates is in terms of past usage that needs to be overcome, this would seem to leave little room

(Footnote 1 continued)

standing for, I call *denotation*—not to preclude but rather to introduce examination of various types of denotation in different symbol systems and also the relationships between denotation and other types of reference.' [11, p. 80].

for any independent contribution from the world 'out there'. As Joseph Margolis remarks, somewhat pointedly, Goodman has 'no theory of the actual behavior of scientific thinking', which makes his account of entrenchment, along with the goal of '*ultimate* acceptability' (which Goodman considers a legitimate substitute for truth, [11, p. 38]), ultimately untenable: 'under historicized and radically relativized circumstances, the governing notion of "*ultimate* acceptability" and its regularized bearing on these other distinctions are rendered completely meaningless or inoperable' [12, p. 121]. Whatever the merits of these more specific criticisms of Goodman's project, what matters for our purposes is the general realization that representation arises from the interplay of denotation (which affords considerable, though not unlimited latitude) and the various factors that determine whether one thing can successfully 'stand in for' another (i.e., make relevant relationships in the target system manifest to its user).

Similarity, Goodman argues, fails as a criterion of successful representation: though it may play an auxiliary role in certain contexts, it contributes nothing essential to the representational relationship.[2] Considering different versions of this—as he puts it: 'most naive'—view of representation ('*A* represents *B* if and only if *A* appreciably resembles *B*', or '*A* represents *B* to the extent that *A* resembles *B*'), Goodman claims, with some hyperbole, that 'more error could hardly be compressed into so short a formula' [10, pp. 3–4]. Why does Goodman think resemblance fails as a basis of representation? For one, resemblance is a reflexive and symmetric relation, whereas representation is neither. Nothing resembles a portrait of the Duke of Wellington more than the painting itself, but that does not mean that the portrait represents itself. Furthermore, the painting resembles the Duke of Wellington to exactly the same degree as the Duke resembles the painting, but it does not follow that the Duke represents the painting. And in any case, the painting arguably is more similar to other two-dimensional paintings than it is to the three-dimensional, flesh-and-blood Duke or his identical twin brother—yet, this neither prevents the painting from representing the Duke, nor renders the twins representations of one another. We must therefore already have resolved to treat one thing as a representation of the other (and *not* vice versa!) before questions of faithfulness or accuracy can be raised: this is precisely the function of denotation. Importantly, any account of representation should also make room for *misrepresentation*. As van Fraassen reminds us: '*Misrepresentation is a species of representation*' [7, p. 14]. Thus, a caricature may, as a political statement, purposely represent Tony Blair as George W. Bush's lapdog and, in doing so, may misrepresent him as considerably smaller in size than his American friend, yet it remains no less a representation of the two men (as opposed to, say, a hypothetical owner/dog pair).

Having sketched some of the complexities and constraints of a general theory of representation, let us focus more narrowly on scientific representation. As we shall see, the very idea that 'scientific representation' merits special treatment has been

[2] For a defence of resemblance as the basis of representation, at least for the case of depiction, see [26].

the subject of much contestation—not least since it has proved notoriously difficult to arrive at any general demarcation criterion that would allow us to tell science from non-science. For the moment, let us assume that we have a good enough grasp of what constitutes a scientific context to be able to recognize certain representational devices—e.g. scientific theories, models, hypotheses, data etc.—as instances of *scientific representation*. Focusing on scientific models as one class of scientific representations, we may then return to Morrison's earlier question: 'in virtue of *what* do models represent and how do we identify what constitutes a correct representation?' [4, p. 70]. As Craig Callender and Jonathan Cohen have noted, this question really addresses two distinct problems: the first part of the question concerns the problem of what *constitutes* the representational relation between a model and the world, whereas the second relates to 'the normative issue of what it is for a representation to be correct' [13, p. 69]. Let us call the first problem the *constitutive question* and the second the *evaluative question*. Callender and Cohen claim that Morrison and other contemporary philosophers of science, in their focus on scientific practice, have tended to run both questions together when, in fact, the two should be contrasted sharply. We will return to Callender's and Cohen's criticism shortly; before doing so, it will be instructive to draw a few more useful distinctions.

In the previous section, we already encountered a broad distinction, within the representational view of models, between *informational* and *pragmatic* approaches. An even more basic distinction derives from opposing stances concerning the prospects of analyzing representation in terms of more basic relations, such as similarity or isomorphism. If one holds that representation—whether in science or in general—can be fully analyzed in terms of such more fundamental relations, one would properly be called a *reductionist* about representation. By contrast, if one believes such a reduction to be impossible and instead holds that representation is a basic relation *sui generis*, one should be deemed a *non-reductionist* about representation.[3] A further distinction may be drawn between *substantive* and *deflationary* accounts of representation, with the latter settling for a broad characterization of the functional point of representation—e.g., the fact that it allows users of representational devices to gain new information about the target—and the former aiming for a deeper, more 'robust' explanation of the functional utility of a representation in terms of an underlying constituent relation between a representation and its target (see [14, p. 94].) Accounts that equate representation with similarity relations between a representational device and its target are a good example of reductionism, as are structuralist accounts that analyze representation purely in terms of relations of (partial) isomorphism between models and their targets. Both types of accounts have been the target of criticism, as exemplified by Goodman's attack on similarity-based accounts and as discussed in the previous chapter in connection

[3]Similar to 'non-reductionism' about the representational relation, Suárez describes as 'primitivism' any position that 'claims that the representational relation, if there is any, may not be further analysed' [14, p. 94].

with structuralist accounts (see Sect. 1.6). In Sects. 2.3 and 2.4 below, we will encounter two examples of non-reductionist accounts which, however, will differ in regard to their place along the substantive/deflationary spectrum.

On the issue of terminology, while it has become customary to refer to that which is being represented as the 'target' (or 'target system'), there is less agreement on what, in general, to call that which does the representing. When dealing exclusively with one type of representation—portraits, say, or mathematical models—this difficulty can be easily avoided: what represents the Duke of Wellington is simply the portrait that depicts him. Speaking in more general terms, it is certainly possible to refer to 'a representation' of a target—as indeed I already have on various occasions. However, this usage runs the risk of eliding the distinction between the general representation relation and specific realizations of it. Some authors prefer to speak of 'sources' and their targets; other locutions include 'representational device' or 'representational vehicle'. It is obvious that the constitutive question pertains to the nature of the representation relation in general, whereas the evaluative question—what it takes for a given realization to be a correct, faithful, or accurate representation of its target—will depend on the specifics of the case at hand. Suárez makes a useful distinction between the *constituents* of representation and its *means*, with the former being implicitly defined by whatever it takes to establish representation for *any* source–target pair and the latter referring to the variable, context-dependent resources a model user draws on when reasoning about a target by way of engaging a model of it [14, p. 93]. It is clear that the means of representation employed by a given representational vehicle will heavily determine its overall effectiveness and 'representational power' [15, p. 294]. As we shall see in Chap. 5, an important function of models is that they allow us to move back and forth between the representational means and aspects of the target system—sometimes effortlessly, but often in a way that requires explicit attention to the format and medium of representation.

What about those who criticize as incoherent the very idea that there is something which sets scientific representation apart from representation-at-large? On this view, there simply is no special problem of 'scientific representation', since representation in science is no different in character from representation in other domains. Such critics, to be sure, can point to various bits of evidence in support of their denial of the coherence of the notion of 'scientific representation'. For one, there is the absence, already mentioned, of a clear-cut (e.g. logical) demarcation criterion between science and non-science. Within science, too, there is considerable disagreement between different disciplines about what constitutes a viable representational target; this is reflected in a mind-boggling diversity and disunity concerning the representational vehicles employed across the various sciences. Last but not least, scientists often differ in their axiological commitments regarding the standards and criteria for what makes something a good representation. Apart from these intra-scientific considerations, there is also the further observation that 'scientists use entities other than models—language, pictures, mental states, and so on—to represent the very same targets that models represent'. This, Callender and Cohen argue, points to model-based (scientific) representation being derivative of representation outside science since ‘it would be surprising that scientific, linguistic, pictorial, mental, and other sorts of representations should coincide in their representational targets were they not at all related’ [13, p. 71]. More specifically, they propose that all representations, including ‘the varied representational vehicles used in scientific settings (models, equations, toothpick constructions, drawings, etc.) represent their targets (the behavior of ideal gases, quantum state evolutions, bridges) by virtue of the mental states of their makers/users’ [13, p. 75]. Thus, when a theoretical biologist writes down the Lotka-Volterra equation and stipulates that it should represent the population dynamics of a predator–prey system, he intends that his audience recognize his intention to activate *in them* the belief that the equations should be taken as a stand-in for the real-world system. The representational vehicle —in this case, the set of equations—is merely a useful prop for facilitating conversation about predator–prey systems and for expressing the modeler’s beliefs about them.

By linking representation-at-large—including scientific representation—to the expression of intentions on the part of the modeler, Callender and Cohen emphasize the stipulative element in our representational practices. However, they are keen to point out that representation by stipulative fiat alone is not the norm, in science or elsewhere; as already noted by Goodman, our representational devices often depend on entrenched symbolic systems and the utility of our representational vehicles depends on them. Questions of utility, however, are simply ‘questions about the pragmatics of things that are representational vehicles, not questions about their representational status *per se*’ [13, p. 75], or so Callender and Cohen argue. In other words, ‘virtually anything can be stipulated to be a representational vehicle for the representation of virtually anything’ [13, p. 74], leaving the evaluative question of the suitability of a given representational vehicle entirely a matter of contingent, context-dependent factors. The basic idea that scientific representation is continuous with representation-at-large and is fully derivative of actions and intentions on the part of the modeler, is not new. Marx Wartofsky, in a paper first published in 1966, makes essentially the same point the other way around, by equating all representation with model-based representation of one sort or another:

> We begin by modelling, therefore, with our first mimetic acts, and with our first use of language. And we continue modelling by way of what, on various grounds, have been distinguished as analogies, models, metaphors, hypotheses and theories. [16, p. 10]

Models, Wartofsky argues, are ‘used to communicate an intended factually true description’ (ibid.); that is, they serve communicative purposes and depend on us as modelers: ‘Our own cognitive activity enters here, to take one as representing the other’, subject only to pragmatic ‘constraints on what may or may not be made into a model’ [16, p. 4]. Regarding the specific constraints that various substantive accounts impose as conditions of scientific representation, Callender and Cohen argue that these, too, are of merely pragmatic significance:

> Likewise, we suggest that, while resemblance, isomorphism, partial isomorphism, and the like are unnecessary for scientific representation, they have important pragmatic roles to

play; namely, they can (but need not) serve as pragmatic aids to communication about one's choice of representational vehicle [13, p. 76].

This attempted dissolution of the problem of scientific representation by reducing it to a matter of stipulation—subject only to pragmatic constraints, in order to facilitate the communication of the modeler's intention to represent a given target—has been criticized for assigning the model user rather too central a role in bringing about successful representation. As Morrison objects: 'There may be no representation without users, but that doesn't mean that users determine what's required for something to represent something else' [9, p. 128].

Callender's and Cohen's approach lends itself to an even more fundamental criticism. While models, along with other scientific representations, often serve the purpose of enabling communal inquiry, by functioning as means for the communication of one party's intentions and mental states to another, their role goes far beyond that of being a mere 'facilitator' of inquiry. Morrison hints at this when she argues that, often, 'scientific representation is about conceptualising something in a way that makes it amenable to a theoretical or mathematical formulation' [9, p. 129], and Callender and Cohen seem to acknowledge as much when they note that sometimes a modeler may include himself in the audience at which the model is aimed [13, p. 77]. What, one might ask, would be the point of directing a model *at oneself*, if a model is nothing but a mere prop for communicating one's beliefs and intentions? The answer, it seems to me, must be that the role of models in inquiry is not exhausted by their functioning as mere props for communicating mental states in the way suggested by Callender and Cohen. Models can surprise us, open up unforeseen lines of inquiry, and lead to novel insights about their targets, all of which suggests that they enjoy considerable autonomy. Mathematical models, in particular, are imbued with a considerable internal structure and dynamics, which renders them partially independent from the intentions of their users. None of this is easily captured by Callender's and Cohen's account, which accords them only an auxiliary role as vehicles of pre-existing intentions and beliefs on the part of their users. Rather than thinking of models as mere *facilitators*, I want to suggest—in a phrase that I will unpack in detail in Chap. 5—that we should think of them as *contributors* to inquiry.

2.3 The DDI Account of Model-Based Representation

R.I.G. Hughes [17] has proposed an account of scientific representation, according to which the representational capacity of theoretical models is due to the interplay between three components: denotation, demonstration, and interpretation. *Denotation*, following Goodman, is conceived of as the basic relation whereby certain elements of a model 'stand for', or are 'a symbol of', elements in the physical world; as such, it accounts not only for the fact that theoretical elements of a model purport to refer to elements in the physical world, but also for the asymmetry that exists between a representational device and its target system. The possibility of *demonstration*—either within a theoretical model, through the application of mathematical derivation techniques, or via experimental intervention in the case of material models—attests to the fact that models possess an internal dynamic and can lead to new results and insights. *Interpretation*, finally, relates what has been demonstrated back to the physical world. Though Hughes is careful to distance himself from the reductionist claim 'that denotation, demonstration, and interpretation constitute a set of acts individually necessary and jointly sufficient for an act of theoretical representation to take place' [18, p. 155], he considers all three components to be involved in scientific representation and takes the interplay between them to be distinctive of the way models represent reality—which is why he takes his *DDI account* to be 'a very general account of theoretical representation' [18, p. 153]. Thus, in the terminology introduced in the previous section, Hughes's account may be deemed a *substantive*, *non-reductionist* account of scientific representation.

Although Hughes's DDI account follows Goodman's advice that 'we must examine the characteristics of representation as a special kind of denotation' [10, p. 5], it does not simply equate denotation and representation; instead, it demands that denotation be put to the test by successful demonstration and interpretation. Consider the case of a mathematical model of a physical phenomenon, e.g. a set of partial differential equations intended to represent the flow of heat in a solid. The theoretical activity of modeling heat flow using the calculus of partial differential equations involves the interplay between what Chris Pincock has called the *physical attitude*—'which insists that throughout we are talking about physical systems and physical magnitudes'—and the *mathematical attitude*, which considers such steps as taking the 'unphysical' limit $\Delta x \to 0$ (e.g. in order to mathematically define the temperature 'at a given point'—even though temperature, in the physical sense, only applies to spatially extended ensembles of particles) as 'involving only mathematical objects' [19, p. 88]. When we resolve to treat certain variables as denoting physical quantities, we clearly do so by taking a physical attitude towards the model, and we again need to adopt this stance when interpreting results—e.g. concerning the final distribution of temperature—as predictions the model makes about the target system. In between, however—that is, during the phase of *derivation*—we can rely on a host of tried and tested mathematical derivation techniques. While such techniques may have their own practical and conceptual problems, they do not directly touch upon the question of how theoretical models represent a reality external to themselves. Mathematical demonstration, thus, may take place entirely from within the mathematical attitude, yet it is no less essential to the process of modeling as a whole, both insofar as it allows for the derivation of results and predictions from a model, and because it makes salient that a mathematical model, by virtue of its being a mathematical object, has an internal dynamic. For Hughes, this insight is fundamental to the use of models in physics in general:

> To be predictive, a science must provide representations that have a dynamic of this kind built into them. That is one reason why mathematical models are the norm in physics. Their internal dynamic is supplied, at least in part, by the deductive resources of the mathematics they employ [17, p. 332].

Once again, this points to models as being more than a vehicle for a user's intentions or beliefs: by tapping into the rich resources of mathematics, mathematical models are imbued with considerable deductive resources and an internal dynamic that may go well beyond what an individual user may intend or be able to survey.

Interpretation, like the other two components of the DDI account, is an important ingredient in the way theoretical models represent. Without it, demonstrated results would remain merely formal results within a deductive mathematical structure, lacking empirical meaning. What is needed is 'a function that takes us from what we have demonstrated […] back into the world of things' [17, p. S333], and interpretation plays this role. Whereas denotation picks out features in the world, which are then referred to by elements within the model, interpretation projects internally-derived results back onto the world, where they must be assessed in terms of their empirical adequacy. This may require considerable ingenuity and imagination. In the case of a mathematical model, even when a result has been successfully derived within the formalism of the model equations, its empirical interpretation may not always be self-evident. As an example, consider the case of mathematical divergences: if one or more of a model's variables diverge for certain parameter values, the user may be faced with the choice of either dismissing it as an 'unphysical' result—for example because the corresponding physical magnitude is recognised as necessarily finite for any finite physical system under consideration—or interpreting it as an indicator of a real feature in the world (e.g., a phase transition), which the model may simply be unable to capture in its entirety. Neither denotation nor interpretation comes with a guarantee of success, but *when* they succeed—that is, when a model picks out the right features in the world, and interpretation assigns empirically adequate meanings to demonstrated results—denotation and interpretation may indeed be said to be the inverse of each other, and the model as a whole may be deemed a successful representation.

2.4 Representation and Surrogate Reasoning: Suárez's Inferential Account

By relying on denotation for the requisite representational asymmetry between model and target, the DDI account of scientific representation is open to the earlier criticism that, for denotation to be successful, its intended target must exist. Yet scientific models sometimes deals with systems that do not—or perhaps could not—exist, such as higher-dimensional or infinitely extended systems. While there may be workarounds for the problems arising from the non-existence of intended targets, some modifications of the DDI account appear to be necessary. The second key ingredient of the DDI account—*demonstration*—may also be less straightforward than appears at first sight. As Mauricio Suárez has noted, 'for Hughes, representation involves demonstration essentially, and hence requires the actual carrying out of inferences about the target on the part of some agent' [5, p. 770]: that is, it requires the actual performance of *demonstrating* a result, whether mathematically or, in the case of material models, via physical manipulation and reasoning on its basis. Even more fundamentally, although Hughes distances himself from reductionism (see previous section), his account remains committed to giving a *substantive* account of scientific representation, in that scientific representation is thought to be characterized by the tight integration of the three theoretical ingredients of denotation, demonstration, and interpretation—that is, by more than just its functional role in inquiry.

Partially in response to these shortcomings, Suárez has proposed an alternative, *inferential account* of scientific representation—one that is unabashedly 'deflationary' in character, in that it seeks 'no deeper features to representation other than its surface features' [5, p. 771]. Giving up on the possibility of a substantive account, however, should not be misunderstood as a lack of ambition; instead, it reflects the need to refocus on the core question of what makes certain types of representation instances of *scientific* representation. In this regard, Suárez's inferential account explicitly commits itself to a demarcation between scientific and non-scientific forms of representation. While both types share certain general features of the representation relation—its asymmetry, non-reflexivity, and non-transitivity—what distinguishes scientific representations is their 'characteristic form of objectivity', which renders them of 'cognitive value because they aim to provide us with specific information regarding their targets' [5, pp. 771–772]. More specifically, on the inferential account, a representational vehicle *A* and its target *B* are related in such a way that

> *A* represents *B* only if (i) the representational force of *A* points towards *B*, and (ii) *A* allows competent and informed agents to draw specific inferences regarding *B*. [5, p. 773]

The expression 'representational force' here refers to what, on the DDI account and following Goodman, is achieved by denotation, namely the stipulated asymmetry whereby *A* is to be treated as a representation of *B* (but not vice versa). Though denotation may often be involved in generating representational force, the inferential account allows for the possibility of other sources of representational force, thereby sidestepping the problems associated with non-existent targets of representation.

It is worth comparing the second part of the above formulation of the inferential account with its correlative element in the DDI account: demonstration. Recall that one criticism of the DDI account was that it requires the actual carrying out of steps amounting to a demonstration of results, either by mathematical derivation or by physical manipulation. The inferential account's demands, by contrast, are substantially weaker, in that it merely requires that *A* have 'the internal structure that allows informed agents to correctly draw inferences about the *B*, but […] does not require that there be any agents who actually do so' [5, pp. 774–775]. Potential suitability for the purpose of enabling inferences about the target system may thus take the place of actual derivation of results. By separating the issue of representational force from the question of whether a given model supports the drawing of inferences about its target, the inferential account is able to account for various ways in which our use of models can go awry: either because a model misses its target, or because an agent lacks the requisite competence to draw valid inferences on the basis of the model. Furthermore, the account recognizes the importance of choosing formats and media of representation that enable the drawing of inferences, for example by making relevant information in the model salient to its user. As Suárez puts it, models must be 'inferentially suited to their targets' [5, p. 778]. Whether a given model is suited to its target in such an inference-enabling way is thought to be an objective fact and not reducible to the intentions or mental states on the part of a specific user.

There exists an unresolved tension within the inferential account, which, though falling far short of inconsistency, calls for further refinement. On the one hand, the account recognizes that, in spelling out the necessary conditions for scientific representation, 'the reference to the presence of agents and the purposes of inquiry is essential' [5, p. 773]. On the other hand, the account insists that we need not attribute any properties to those agents—not even, as we saw in the previous paragraph, their actual existence. This raises the question of how we are to think about such hypothetical agents, especially given that the absence of competent users need not invalidate a model's status as a scientific representation. If all that matters is that *some* hypothetical agent with sufficient inferential prowess and access to relevant information could, in principle, use the model to correctly draw inferences from it about the target system, one may wonder just how much in terms of inferential and epistemic ability it is reasonable to demand. Presumably, an omniscient (or nigh-omniscient) agent would be able to achieve a great deal more by way of inference than we ever could, and she would be able to do so on the basis of models that are far too complex for us mere mortals to comprehend. Yet we would rightly hesitate to speak of 'scientific representation' in such a case. At the very least, then, the degree of competence and inferential prowess required for a model to serve as a representation must be within human reach.

Whereas Suárez is explicit about his deflationism concerning scientific representation, others have built on his inferential account in an attempt to reinstate a full-fledged substantive account of scientific representation. Thus, Gabriele Contessa has argued for what he calls an 'interpretational conception' of scientific representation, according to which 'a vehicle is an epistemic representation of a certain target (for a certain user) if and only if the user adopts an interpretation of the vehicle in terms of the target' [20, p. 57]. On this view, an agent employing a model of the atom—say, Thomson's plum pudding model, which conceives of the electrons as embedded in an evenly distributed positive charge the size of the atom, like plums in a pudding—resolves to treat (i) the representational vehicle as a whole as standing for the target system (the atom), (ii) some elements of the vehicle as standing for some component parts of the target system, and (iii) some of the properties and relations that obtain in the vehicle as corresponding to properties and relations holding between component parts of the target system [20, p. 59]. This interpretation of the representational vehicle *in terms of* the target system, Contessa argues, allows us to explain 'why, if a vehicle is an epistemic representation of a certain target, users are able to perform valid surrogative inferences from the vehicle to the target and allows us to tell which inferences from a vehicle to a target are valid' [20, p. 61]. As he sees it, this renders the (substantive) interpretational account superior to the (deflationary) inferential account proposed by Suárez, since the latter simply posits the agent's ability to perform valid inferences from a vehicle to a target as a 'brute fact' [ibid.]. However, it seems to me that Contessa is moving too quickly here. For, as mentioned earlier in this section, Suárez is well aware of the fact that representational vehicles—in virtue of the different formats and media they employ—have different constraining and enabling effects on their users: whether a vehicle is inferentially suited to its target depends not only on factors intrinsic to the vehicle itself, but also on how we conceive of the epistemic capacities of the prospective model users—including their inferential prowess and interpretative abilities. Recognizing that the interaction between the model user and the representational vehicle is mediated by a variety of representational means renders the ability to perform valid inferences from a vehicle to a target far less mysterious than it might seem at first sight.

2.5 Realism, Instrumentalism, and the Varied Uses of Models

One of the core debates in the philosophy of science concerns the issue of *scientific realism*. Even setting aside sceptical worries about the existence of the external world or regarding the possibility of knowledge in general, one might harbour doubts about the status of scientific knowledge. Is the world really as science describes it? Are scientific claims to be taken 'at face value', and are they (by and large) true, or at least approximately true? And is science as a collective enterprise getting ever closer to a true and complete account of the world? These are some of the staple questions in the debate about scientific realism, and while they were traditionally directed at scientific theories, it is easy to see why they may also be raised—perhaps with even more urgency—in relation to scientific models. Anti-realists who are doubtful either about the historical thesis that science is moving closer to the truth or about the existence of unobservable entities posited by scientific theories, may be aghast at the casualness with which scientists readily help themselves to inconsistent models and employ idealizations and false assumptions in their model-building practices. Scientific realists, in turn, need to explain how science as a whole can be 'on the right track', when so much of it relies on models, many of which are false 'by design', as it were.

In the past, it was not uncommon to assume a stance of instrumentalism towards models. As discussed in the previous chapter (Sect. 1.3), during the time the syntactic view of theories held sway, philosophers of science tended to accord models at best a marginal role in scientific inquiry. Models were largely seen as limiting cases or approximations, or as mere heuristic tools to be used in the derivation of explanations or predictions from fundamental theories, which in turn were regarded as the proper object of realist evaluation. Models were at best thought 'to serve an auxiliary function in leading theories to the test' [21, p. 31] by generating testable predictions. As Wartofsky puts it, on this view

> the burden of commitment is passed on to the theory of which some […] model may be constructed. The postulates of the theory may make existential claims, therefore, but the model serves merely to channel these to some confrontation with experimentally testable consequences [21, p. 31].

While the view reported (but not endorsed) by Wartofsky may simply reflect an overly narrow understanding of the role of models in scientific inquiry, a more thoroughgoing instrumentalism would extend similar considerations to the underlying theory itself, with the latter

> being itself no more than an instrument for coherent organization and testing, and the question remains—*of what*? The reference beyond such theory-model 'instruments' remains forever delayed; or it is defined in terms of practical purposes, decisions concerning which lie outside the theory, but are vaguely defined as 'successful prediction' or 'control of the environment'. [ibid.]

Though instrumentalism at first sight may appear to be more modest, in that it foregoes a commitment to the truth, or approximate truth, of models and theories, this may be seen as simply pushing the crucial question one step further back, since the instrumentalist must now lay out criteria for what constitutes an instance of successful prediction or control.

Given that the focus in philosophical discussions of scientific models, as in the present chapter, is often on their representational function, one might expect the issue of scientific realism in connection with models to be largely decided by the question of how faithfully scientific models represent their targets. Against this expectation, William Wimsatt, in a paper with the programmatic title 'False Models as Means to Truer Theories', has argued that philosophers should not ignore 'the role that false models can have in improving our descriptions and explanations of the world' [22, p. 94]. Taking his lead from evolutionary biology, Wimsatt considers the case of so-called *neutral models*, i.e. models of species and populations which do not include selection pressures. Absence of selection does not entail that there is no change in the form of speciation or extinction events; rather, it might mean that such events, when they occur, are simply random. For many—perhaps most—evolutionary processes that biologists are interested in, including all those that are the result of adaptation to environmental pressures, a neutral model would be false; yet even in those cases neutral models may be essential, insofar as they provide a 'baseline' for further inquiry, 'for the explicit purpose of evaluating the efficacy of variables that are not included in the model' [22, p. 100].[4] Other epistemically valuable uses of false models include situations where an incomplete model may be used 'as a template, which captures larger or otherwise more obvious effects that can then be "factored out" to detect phenomena that would otherwise be masked or be too small to be seen', or the consideration of two or more false models which 'may be used to define the extremes of a continuum of cases in which the real case is presumed to lie' [22, p. 100]. All in all, Wimsatt considers twelve distinct ways in which false models may facilitate, or even be essential to, the search for better theories and scientific inquiry more generally, and it seems plausible to assume that any such list is likely to be incomplete.[5]

Wimsatt's observation that models, even when false—*and sometimes deliberately false*—may make a positive contribution to our overall epistemic situation is significant, in that it productively blurs a number of distinctions, whether between realist and instrumentalist stances towards models or between representational and non-representational uses of models. Non-representational uses of models, in particular, have been treated only cursorily in philosophical discussions—as hinted at in the earlier quote by Wartofsky who notes that notions such as 'control of the environment' or 'success' are often left undefined. Yet non-representational uses of models abound, in pure science as well as in more applied contexts such as engineering. Of course, not every non-representational use is of interest: someone might find a three-dimensional material model beautiful and use it as a decorative sculpture in his living room, but this would be of little relevance to us. What matters for the present argument are non-representational uses that nonetheless facilitate learning about the world—where, following Till Grüne-Yanoff's analysis of such cases, model-based learning can be defined as occurring when a model justifies 'changing one's confidence in some hypothesis about the world' [23, p. 852]. As an example, Grüne-Yanoff discusses Thomas Schelling's *checkerboard model*, which consists of two types of tokens distributed randomly across a checkerboard, with tokens being moved in each iteration according to a fixed rule, until no further movements occur. The rule is simple: if more than half of the neighbouring fields are occupied by tokens of the opposite type, a given token will move to any vacant field where this is not the case. In other words, tokens of a given type may be interpreted as having a preference for being in a neighbourhood where they are not in a minority. Over time, this reliably gives rise to patterns in which the two types of tokens are spatially segregated. Schelling did not claim that the rule reflected actual behavioural patterns or that the geometry of the checkerboard, the initial distribution of tokens, or their relative proportion represented aspects of the actual world. Yet, as Grüne-Yanoff rightly notes, we learn from Schelling's model: what

[4]Similarly, Uskali Mäki [28, pp. 12–13] notes that, in many cases, apparent falsehoods included in models are best interpreted as (true) claims about the neglibility of certain empirical factors.

[5]For example, Alisa Bokulich has argued that models that are false in virtue of being 'fictionalized'—because they involve 'fictional entities or processes that are not related to the true ones in the world by what might be thought of as a distortion or series of successive cases' [27]—can nonetheless offer genuine scientific explanations.

the model shows is that spatial segregation, of the sort found in racialized urban geographies of American cities, can occur simply due to individuals not wanting to be in a minority (rather than due to overtly racist preferences): ‘The model result thus justified changing one’s confidence in hypotheses about racist preferences being a necessary cause of segregation.’ [23, p. 856] Another non-representational aspect of models is their *performative* use. A case in point is the discipline of economics which, as Michel Callon [24] has argued, not only *studies*, but at the same time *performs* the economy. If this sounds too abstract, consider the example of the Black-Scholes equation in finance, which purports to be a model of the efficient pricing of stock options. As Donald MacKenzie [25] has shown, the model gained empirical adequacy largely because traders adopted it as a method for identifying, say, overvalued stocks and based their selling decisions on it, thereby effectively bringing stocks in line with what the model ‘demanded’—at least until the next stock market crash.[6]

Finally, it is worth noting the case of *exploratory models*, discussed in detail in Chap. 4. Much exploratory modeling aims only indirectly at the representation of actual target systems or empirical phenomena, and instead concerns itself more immediately with models that lack specific intended targets. For example, mathematical physicists might study certain model equations in higher ($d > 3$) spatial dimensions, so as to get a qualitative understanding of certain limiting cases or of the range of behaviours their model may be expected to display. Given that such models do not purport to represent, however imperfectly, any real target system, it does not seem quite right to consider them ‘false’ in the way that idealized representations of actual system may be deemed false. Often, the exploratory use of models aims at greater mastery and understanding of the repertoire of modeling techniques as a whole. All else being equal—with some obvious caveats, to be discussed in Chap. 4—the exploratory use of models is entirely legitimate, yet it, too, requires moving beyond the traditional narrow focus on the representational functions and uses of scientific models.

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[6]Against this conclusion, Mäki has argued that one should resist such talk of ‘performativity’: ‘If it happens that certain practices and arrangements and patterns in real world finance are in line with the Black-Scholes-Merton formula, this naturally does not mean that the theoretical formula or its uttering by […] academic scholars—or by practitioners in the world of finance—“performs” those practices’, because as he sees it, ‘there is no constitutive relationship here between the theoretical model and some empirical practices and patterns’ [29, p. 448].

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