Modeling and Realism: Strange Bedfellows?

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1. Introduction

Most characterizations of scientific realism involve one or both of the following elements. First, they include a claim about *the aims of science* – sometimes called an axiological claim – roughly, that science aims to discover truths about the world, including truths about unobservables (van Fraassen 1980; Lyons 2005). Second, they advance a claim about *the achievements of science* – sometimes referred to as an epistemic claim – roughly, that science often discovers said truths, providing us with knowledge of them (Boyd 1983; Psillos 1999; Chakravarty 2007). [1] However, and as recent philosophy of science has emphasized (Frigg & Hartmann 2013), when one pays attention to the actual practice of science one finds many cases in which scientists put forward models containing assumptions they know full well to fall short of the truth, sometimes strikingly so. Physicists study point masses sliding down frictionless planes, despite knowing that such things do not (indeed cannot, physically) exist. Similarly, population biologists routinely concern themselves with infinitely large populations whose generations do not overlap, while economists analyze the behavior of fully rational agents, unhindered by cognitive biases and other limitations. How can such practices be squared with scientific realism?

Specifically, the ubiquity of modeling and associated practices such as idealization and abstraction raises two sorts of questions. First, a question about aims: how can science be aiming at truth while at the same time putting forward knowingly false and partial models? Does this show science has a different aim, or have we subtly mis-described the practice of modeling? Second, a question about achievements: if our best science consists of models, and if we know these models to be rife with idealizations and abstractions, then perhaps science cannot or at any rate does not deliver truths, its aims notwithstanding. Moreover, models often play a key role in prediction and explanation. This facts threatens to clash with an important argument in favor of realism – the No Miracles Argument (NMA). The NMA rests on the connection between predictive and explanatory success and truth – more on it below, in section 4 – a connection that seems to be contravened by predictions and explanations that knowingly employ falsehoods. Is the NMA to be dispensed with or revised? Or is it, rather, our thinking about models, and their role in explanation and prediction, that ought to be reconsidered?

Before delving into these issues we must first characterize some basic notions, especially modeling and idealization. This is done in the next section. In section 3 I look at whether modeling poses challenges to realist claims about the aims of science, and in section 4 at potential challenges to science's success in meeting those aims. In closing I discuss possible implications for the status of inference to the best explanation.

2. Models and Modeling

There are various senses of 'model' in science and its philosophy. The focus of this chapter will be on issues connected with the *practice of modeling*, where this is understood as constituting a particular (albeit very common) strategy of representing and studying phenomena (Godfrey-Smith 2006; Weisberg 2007a).[2] Modelers offer local and limited representations, often containing intentional deviations from completeness and accuracy and tailored to specific needs and contexts. Of particular significance in the present context is idealization: the introduction of assumptions which are known not to reflect the realities of the phenomenon being modeled. Idealization is very common – examples can be multiplied beyond those given above (infinite populations, point particles, fully rational agents). They involve *a deliberate mismatch* between the model and the world – not error or oversight. It is useful to contrast is idealization with abstraction. The latter involves lack of detail, i.e. incompleteness, but no inherent distortion. A description that omits fine detail, settles for a range of possible values rather than a particular number, averages over a population etc. is thereby abstract. A description that misrepresents – typically, depicting the world as simpler than it is known to be – is thereby idealized.[3]

Further distinctions within the practice of modeling will be discussed below. It will be helpful, however, to note a difference between two types of models: some are theory-based and some aren't. In cases of the former sort, a general, often fundamental theory exists and modeling comes in when the general theory is applied to a particular context. Such is the case, for instance, in statistical physics and quantum theory. In these areas there are well-understood fundamental laws, but applying these laws to particular phenomena requires various idealizing assumptions (Cartwright 1983). This is sometimes referred to by saying that models serve as “mediators” between theory and the world (Morgan & Morrison 1999). I will refer to such cases below as *mediating models*. In other contexts, however, there is no general theory, so that modeling of the relevant phenomena does not function in application or mediation, but as the “highest” theoretical understanding available. Such is the case, for instance, in studies of animal behavior, for instance in the common framework of evolutionary game theory. Although theoretical work is organized around a family of related models and associated analytic techniques, these models are not derived from a more basic set of laws or axioms. When the contrast with mediating models matters, I will refer to models in this second category as *standalone models*.

3. Do modelers aim at truth?

Recall the first central element of scientific realism described above: the claim that science aims at truth. In this section we will inquire whether the practice of modeling casts doubt on this claim.

Now, to say that the aim of science is truth or accuracy is crude at best. The idea that a multi-faceted social institution like science has an aim and that, if so, it can be summarized succinctly, may appear puzzling or unrealistic. Moreover, it is not obvious that truth – rather than some more refined notion of truth-likeness (Oddie 2014) – is the right choice here. Nor is it obvious *which* truths science seeks? Is it *only* truths that matter (matter for *what*)? (Kitcher 2001; Lyons 2005). However, these finer points recede into the background when one takes note of the blatant and radical nature of many idealizations found in science. Recall the examples we began with: point particles, infinite populations, rational agents. However one understands truth, or some suitable variant thereof, it is clear that such assumptions will not count as true or approximately true or near the truth in any reasonable sense of 'near'. These assumptions are wide off the mark. And however exactly one understand the idea that science *aims at* truth, it should be at the very least puzzling how assumptions of this sort are consistent with such an aim.

To be sure, there are good reasons for studying models of point particles and rational agents. They are cognitively accessible, computationally tractable and often facilitate simple and compelling explanations. But, prima facie, fidelity to the real-world does not seem to be among these reasons. To the contrary, it may seem that such models are best seen as an expression of instrumentalism: a modeler will make whatever assumptions she finds productive for saving the phenomena, irrespective of their truth or accuracy.

How should a committed realist respond to this situation? One option is to deny that modeling is the right arena in which to debate realism in the first place. Instead, the realist might say, the debate should center on laws of nature and, more generally, on the fundamental theories from which models are derived. Models, in this view, are merely tools for applying theories to the world, and as such their truth is not a matter of concern for the realist. But this response is hardly adequate. For one thing, as noted, in many areas we find standalone models – models that are not derived from fundamental theory. For another thing, there are substantial doubts over whether laws of nature themselves are to be read as literal statements of fact. Moreover, it has been argued that laws depend on models for their predictive and explanatory power (Cartwright 1983; Giere 1999; Lange 1993). So the appeal to laws may turn out to be a detour, eventually leading us back to the consideration of models.

Instead of appealing to laws, the realist may try to explain how idealized models, despite their apparent falsity, can still be seen as subservient to the goal of truth: appearances to the contrary, modeling is part and parcel of the attempt to accurately depict nature. Here, two strategies can be discerned. The first views modeling as involving literal yet *indirect* representation of targets in the worlds. The second, in contrast, views models as being *directly* about the world. Let us look at these approaches in turn.

3.1 Modeling as indirect representation

The indirect approach, as the name suggests, treats models as relating to the world indirectly – via a simpler (idealized) model system. It is perhaps easiest to understand this idea by thinking first of concrete physical models, such as Watson and Crick's famous scale model of DNA. Watson and Crick constructed a large DNA-like structure made of stiff wire and metal sheets. They took this to be a representation of DNA: the wire stood for the molecule's backbone, the sheets for the bases bound to it. They tinkered and manipulated, changing the shape and number of wires, the placement of the metal sheets etc., regarding these manipulations as informative about features of real DNA. The indirect approach takes something like this investigative structure as characteristic of modeling in general, including mathematical and mechanistic models. As the indirect approach has it, the modeler constructs an object (the "model system"), one that is simplified and easier to handle relative to the phenomenon under study (the "target system"). She then uses this object as a representation of the target system. It functions as a surrogate for the target. In a case like Watson and Crick's model, this involves physical construction and actual causal manipulation. In mathematical or other non-material models, the construction phase consists of writing down equations, a text and/or drawing a figure. These serve to pick out, or specify (Giere 1989) a non-material object, which serves as a stand-in for the target. Once specified, the modeler analyzes the model system, attempting to learn about its properties and behavior. Then she compares the model to the target and, depending on how the comparison pans out, reaches conclusions about the target system.

Recall that the puzzle we are dealing with stems from the apparent tension between the idea that science *aims at truth* and the seeming falsity of idealized models. How can a population biologist be aiming at truth if she portrays the population she studies as infinitely large? The indirect approach resolves this puzzle by treating the population biologist's claims about infinite populations as being about the model system, and only indirectly, via resemblance or mapping, about the world. If the modeler decides to specify an infinitely large population she is not making a false claim about the world. Indeed she is not making a claim at all. Rather, she is picking out a system she wishes to study. This system can then be compared to a real-world system, potentially yielding true (or near-true) claims about the relationship between the model and the world. Thus, the appearance of falsity is removed and a distinct locus of truth is identified: models are *things*, to be compared with the world; statements about the model-world relationship are where truth or accuracy evaluations come in.

However, the indirect approach raises puzzles and problems. First, there is the issue of what model systems are. Some model systems, like Watson and Crick's model, are actual concrete objects. But most aren't: mathematical, computational and mechanistic models are "missing systems", as Thomson-Jones (2010) puts it – they cannot be seen, heard, or otherwise causally interacted with. What are they, then? I will briefly discuss three recent proposals.

One natural suggestion is that model systems are abstract objects – mathematical objects, in particular. Suggestions along these lines have been made by several authors (e.g.

Giere 1988; Teller 2001). This account makes sense of why model systems cannot be heard, seen or interacted with, as well as the fact that mathematics is often central to model specification. But several questions arise, too. First, there are general concerns about how knowledge of mathematical abstracta is possible (Benacerraf 1973; Field 1989). Second, there are concerns about whether abstract models can be suitably related to concrete targets – specifically whether a similarity-based account, preferred by several advocates of the indirect approach of model-world relations, is coherent (Giere 1989; Weisberg 2013; Thomson-Jones, 2010). Third, many models do not have a mathematical character and treating them as abstracta seems like tenuous, at best (Thomson-Jones 2012; Levy 2015)[4].

In response, some have suggested that models are best viewed as concrete hypothetical objects. In particular, several authors argue that models are akin to fictions like Sherlock Holmes' London and Tolkien's Middle Earth (Godfrey-Smith 2006; Frigg 2010). Advocates argue that such a fictionalist view of model systems, besides accommodating non-mathematical models, fits well with the practice of modeling and with the often prominent role of the imagination in modeling. Such an approach, however, brings with it metaphysical anxieties not dissimilar to those connected with the models-as-abstracta view. What are fictional objects? How do they fit into the natural world? How can we know about them? Etc. (Kroon & Voltolini 2013). Additional issues pertain specifically to the models-as-fictions approach. For one thing, it is not clear what the identity conditions are for fictional systems, and so it may be unclear whether two scientists are engaging with the same model (Weisberg 2013, §4.4.1; See also Friend forthcoming). For another thing, some models may seem hard to picture or otherwise imagine (Weisberg 2013 §4.4.2) and it is unclear how they fit into a fictionalist view.

Thus, both versions of the indirect approach – the abstracta view and the concrete-hypotheticals view, have attractions but also problems. They allow one to make sense of the realist aim underlying modeling, but they generate ontological commitments and semantic puzzles. Perhaps an intermediate view, combining elements of both accounts, could overcome these issues (Contessa 2010; Thomasson forthcoming). Or perhaps the indirect approach ought to give way to a direct conception of modeling – to which I now turn.

3.2 Modeling as direct representation

While the indirect approach treats modeling as the specification and study of model systems, the direct approach "skips over" the model system and treats models as being directly about the world, sans intermediaries. Let us look at two ways of developing this idea.

Sorensen (2012) suggests that modeling may be understood as a form of suppositional reasoning, akin to conditional proofs and reductio ad absurdum. Sorensen explicitly motivates his view as a solution to the problem of how idealizations are compatible with the realist aim of truth. He suggests that the suppositional approach relies on well-understood concepts from classical logic and that it accommodates the creativity of modeling, while explaining its epistemic utility. These are certainly advantages. But it is hard to see how, on Sorensen's view, one learns from modeling *about the actual natural world*. That is, it is readily apparent how one learns hypothetical facts – what the world would be like under such-and-such a supposition. And it is fairly clear how one can learn negative modal facts – such-and-such cannot be the case. But most model-based knowledge pertains to contingent, actual facts, and Sorensen says very little about how such knowledge is generated on the suppositional approach.

Another way of developing a direct approach is by tying it to a particular, pretense-based view of fiction due to Walton (1990). Walton's view takes it cue from pretend play and games of make-believe. It treats fictional statements not as descriptions (of a fictional entity) but as prescriptions to the imagination. Applied to modeling, this leads to viewing models as games of *make-believe about real-world targets*: a pretense according to which systems in the world are different – simpler, more tractable, etc. – then they actually are. Toon (2012) argues for such a view, following closely in Walton's footsteps

Toon's account is mainly aimed at understanding the content of models, and the status of the associated speech-acts and flights of the imagination. Levy (2015) also relies on Walton's theory to develop a direct account, placing more emphasis on the model-world relationship. He suggests that it may be understood via Yablo's (2014) notion of partial truth. Yablo's framework, the specifics of which will not be discussed here, focuses on the notion of a statement's subject matter and allows us to make sense of the idea that a statement can be true specifically with regards to a given subject matter[5], even if it is false in other respects. So suppose we have a model of a predator-prey system that assumes an infinite population. Yablo's framework would allow us to say that while idealized, and hence partly false, such a model is true as regards the subject matter of how predator abundance affects prey abundance.[6]

The fiction-based version of the direct account accommodates salient aspects of the practice, such as its reliance on imagination, without incurring heavy metaphysical costs. However, some argue that it is subject to worries similar to those afflicting indirect fictionalist accounts. There are also questions about whether a direct fictionalist account – or perhaps any fictionalist account – can accommodate models that appear to have no target (Weisberg 2013, Ch. 4; Friend forthcoming; Thomasson forthcoming).

My own sympathies lie with a direct approach to modeling, as it seems to accommodate the practice fairly well, while being ontologically modest and semantically straightforward. However, the key message of this section is less committal: there are ways of reconciling the ubiquity of idealization and the practice of modeling more generally, with a realist understanding of the aims of science. Such reconciliations face some questions and difficulties, but on the whole they seem to permit a view of model-based science that does not slide into instrumentalism.

4. Does modeling challenge the realist's achievement's claim?

We have so far dealt with the question of whether modeling undercuts the idea that science *aims* at truth, reflecting a kind of instrumentalism. But the ubiquity of modeling might lead one to suspect that even if science aims at truth, it often *fails to fulfil* this aim. The challenge here is somewhat indirect, and it is best seen as threatening the No Miracles Argument (NMA) – perhaps the best-known and most important argument for scientific realism (See Wray, CHAPTER X, this volume). In a nutshell, the NMA states that the immense predictive success of science would be inexplicable, a "miracle", if the theories from which predictions were derived were not true. But many models: (1) serve in prediction and explanation. And (2) contain (known) falsehoods. So how can the idea that predictive and explanatory success is a mark of truth be maintained? And what becomes of the NMA without this idea?

It is important to see how this challenge differs from traditional objections to realism, and to the NMA in particular. For one thing, it is distinct from standard empiricist concerns, according to which science cannot afford us knowledge of unobservables. Idealization has no special connection to unobservables. The challenge from modeling also differs from (although it shares some features with) history-based concerns about realism such as the pessimistic induction (Laudan 1981; Stanford 2006). Roughly speaking, these are challenges that appeal to past scientific failures, arguing that our current theoretical beliefs are no more justified than those of our (refuted) scientific ancestors. So while history-based challenges emphasize a legacy of scientific *error*, the challenge from modeling pertains to abstraction and idealization, i.e. *deliberate* omissions and misrepresentations. The challenge is whether we can form true beliefs on the basis of models, consistent with our knowledge that they are incomplete and idealized.

I will describe three responses to this challenge. The first focuses on ways of de-idealizing, i.e. relaxing idealizations, thereby obtaining more realistic models. The second concerns strategies that may be put into use when de-idealization is impossible – especially so-called ‘robustness analysis’. Meanwhile, the third response consists of a move towards perspectivism, an attenuated form of realism.

4.1 Correctable idealizations

In some cases an idealized model can be *de-idealized*: its false assumptions can be corrected for. Consider the ideal pendulum. It is a model according to which a point-mass bob is suspended from a massless rod, oscillating in two dimensions only and experiencing no air resistance. This highly unrealistic model can be de-idealized, introducing corrections and refinements so that it does take account of the bob’s mass distribution, the effects of surrounding air and so on. When this is done, the model's predictions with respect to the motion of actual pendula become progressively better. Let us call this *the Galilean Strategy* (McMullin 1985; Weisberg 2007).

When the Galilean strategy works, it supplies a ready and direct answer to the challenge from modeling. For while the idealized model may be simple and computationally tractable, it is also dispensable in favor of more accurate models. Even if this can only be done progressively, over time, the realist can still maintain that idealized models are merely temporary, pragmatic devices, to be replaced if necessary and/or in due course. They need not pose a fundamental worry to the realist. Moreover, if as one persists with the Galilean strategy one also obtains better and better predictions, then it would appear that rather than undermining the connection between predictive success and truth, the practice of modeling can buttress it (McMullin, 1985; Laymon, 1989). Thus, if successfully pursued, the Galilean strategy may turn the tables on the challenge from modeling, using it as an argument *for* realism.

But this response is not problem free. One concern is that, oftentimes, scientists do not de-idealize; instead they move on to other idealizations (Hartmann 1999). A more serious problem is that de-idealization is not always possible: predictive and explanatory success may depend on the presence of idealizations. Robert Batterman, for instance, has argued that this kind of situation obtains in explanations of universal phenomena such as phase transitions. The phenomenon can be deduced only by taking certain limits and any attempt to relax the idealization and look at behavior below the limit will not yield valuable predictions or understanding (Batterman 2002; 2005). Similar claims have been made with respect to other areas and phenomena, such as hadron physics (Hartmann 1999), quantum "dots" (Bokoulich

2008), models of oscillatory behavior (Wayne 2011) and climate science (Parker 2006). The next subsection looks at how the realist might handle such cases of non-correctable idealization.

4.2 Non-correctable idealization

The existence of non-correctible idealizations in science might seem to pose a grave difficulty for the realist, insofar as she appeals to the NMA. If the best available theoretical representations contain assumptions that are known to misrepresent the world and, moreover, there is good reason to believe that these misrepresentations cannot be corrected for, then how can one hold that predictive and explanatory success is an indication of underlying truth?

In fact, the cases of predictive and explanatory success may pose different challenges (Batterman's arguments, alluded to above, place most emphasis on explanatory uses of uncorrectable models). But I will not be able to cover both cases, and so I will primarily look at predictive uses of such models, and the challenges they pose for the NMA. A shorter discussion of explanation-related issues is contained in the next section, apropos IBE.

A number of authors have discussed suggestions that are relevant to this concern. Their details vary but they all contain a common underlying idea: To the extent that one can show that the predictive success of the model is not dependent on the particular idealizations it contains, one can have faith in the model's capturing a genuine truth about reality. Note that the suggestion isn't that one demonstrate that the model can be freed from idealization. That would be a case of correctable idealization. Rather, the idea is that for any particular idealizing assumption, it can be shown that the model's success does not depend on it. [7]

There are at least two ways of fleshing out this suggestion. The first is via a direct argument – or, if possible, a proof – that the idealizations in question are not relevant. Michael

Strevens (2008, Ch. 8) discusses this possibility, in connection with his view of scientific explanation.[8] He takes as a case study the explanation of Boyle's Law by the ideal gas model. Boyle's Law states that for a dilute gas under fixed temperature, pressure is inversely proportional to volume. The model commonly used to explain this law makes substantial idealizations, including an assumption that gas molecules do not collide with each other. Strevens quotes the following lines from a physical chemistry textbook: "We . . . assumed that the molecules of the gas do not collide with each other . . . But if the gas is in equilibrium, on the average, any collision that deflects the path of a molecule from [the path assumed in the derivation] will be balanced by a collision that replaces the molecule (McQuarrie and Simon 1997, 1015)." He goes on to comment: "The no-collision assumption is justified, then, by an argument, or rather an assertion, that collisions or not, the demonstration of Boyle's law goes through." (2008, 316). Let us not worry about the details of this case, as it is the general idea that matters here: if a model's result can be shown not to depend on specific idealizing assumptions then one can regard the model as capturing genuine features of its target-in-the-world, as the realist hopes.

A related way of carrying out this realist strategy employs what is often labeled *robustness analysis*. In this type of analysis, the model's idealizations are varied and the modeler then checks whether its results are retained over a range of different idealizations. This way one can show that the idealizations are, in a sense, irrelevant to the model's success. William Wimsatt has commented:

> "[A]ll the variants and uses of robustness have a common theme in the distinguishing of the real from the illusory; the reliable from the unreliable; the objective from the subjective; the object of focus from artifacts of perspective; and, in general, that which is

regarded as ontologically and epistemologically trustworthy and valuable from that which is unreliable, ungeneralizable, worthless, and fleeting." (1981, 128).

Thus, the point of varying the idealizing assumptions is to distill the trustworthy "core" of the model, separating the assumptions upon which the model's success depends.[9] On the supposition that these core assumptions are true, the result is a justification for believing that the model is "getting at" something true, the idealizations notwithstanding.

Let me note that the response from robustness is, as stated here, only a sketch. The realist must still provide an indication of what counts as the invariant "core" of a model – how are core elements to be identified? What distinguishes them from other elements? Can these discriminations be done in a forward-looking and reliable way? Absent quite detailed responses here it is unclear whether the robustness response is adequate.

Now, in some ways the first strategy – direct proof of the irrelevance of the idealizations – is better than robustness analysis. For one thing, robustness analysis probes only a certain range of variation in the idealizing assumptions, since it is virtually never possible to cover all possible idealizations. One's confidence that the model's success is a function of the "core" alone is therefore limited by the extent to which one can model the space of possible alternatives to one's idealized assumptions.[10] Another benefit of the direct proof strategy is that it typically helps one see why the idealizations do not matter. In the ideal gas example discussed by Strevens, for instance, the justification of the no-collision assumption he cites not only shows *that* the assumption is not a difference maker for Boyle's Law. It also shows *why* this is so: because of the statistical profile of the trajectories of the gas molecules any collision will be balanced by a counter-collision, annulling their overall effect. However, it is often difficult to provide a genuine proof that idealizations are irrelevant. So one may still need to rely on robustness analysis.

4.3 Perspectivism

A third and final response to the ubiquity of modeling consists in a more dramatic shift – towards a different view within the realism debate, namely perspectivism. This view may be seen as an attenuated form of realism or as a middle ground between full-blown realism and a kind of relativism. In a nutshell, the perspectivist suggests that scientific knowledge is inextricably bound to a (limited) perspective, i.e. it depends, essentially, on the information gathering tools and analytical techniques of scientists.[11] Perspectivism can be seen as a kind of constructivism, with illustrious predecessors such as Kant and Kuhn. But the viewpoint I want to discuss here takes a more practice-oriented approach, arguing that concrete features of scientific practice, especially those associated with modeling, give a special impetus and flavor to their view.

Central advocates of perspectivism in recent philosophy of science include Paul Teller and Ronald Giere. The latter, in particular, has given the view a book length treatment (2006) and has defended it from subsequent criticism (2009). Morrison (2011) and van Fraassen (2008) endorse parts of the perspectivist outlook, although I will not attempt to describe each author's specific commitments.

Giere defines perspectival knowledge as knowledge the scope and character of which is a product of the knower's epistemic wherewithal: her other beliefs and interests, her knowledge gathering capacities, her tools and her methods of analysis and inference. Giere points out that the choices scientists make regarding how to idealize, what to abstract from and other issues associated with modeling stem from the kinds of phenomena they care about, the sort of knowledge they already have, and perhaps most crucially – the tools and techniques they possess. In other words, models embody a perspective; they reflect modelers' knowledge and concepts, tools and pragmatic interests. So they lead to perspectival knowledge. Moreover, as Giere notes, and as others like Morrison (2011) discuss at greater length, oftentimes in science a phenomenon is addressed via multiple, mutually incompatible models. Such incompatible models pose an especially serious challenge to the realist, from the perspectivist's point of view, since it seems to prevent a "piecing together" strategy, where the results of different models are combined to provide an overall, non-perspective-bound picture of the phenomenon.

However, several writers have provided plausible responses to the model-based perspectivist challenge (Chakravartty 2010 and Reuger 2005 respond to perspectivism directly. Pincock (2011) addresses similar issues under a different heading). These responders agree that observations and models provide perspectival information. But they argue that this information is perfectly objective information, hence "kosher" from a realist point of view. As Rueger puts it: "These models describe the system relationally: from this perspective, the system looks as if it has intrinsic property x, from that perspective it looks like it has property y." Chakravartty (2010) develops the idea further by arguing that models can be read as informing us about dispositional properties of target systems, where these dispositions are revealed by the relational properties we detect in experiments and observations. The dispositions themselves are intrinsic and perspective-independent, argues Chakravartty, but they lead to different manifest behaviors depending on the conditions: "salt sometimes dissolves in water, and other times does not, depending on the circumstances. The ability to dissolve is a property of salt that is manifested in some circumstances and not in others" (Chakravartty, 2010, 410).

Is this response adequate? Two issues might be raised, although their significance is not fully clear. First, it is seems likely that we have knowledge of non-dispositional properties – such as the number of protons in an iron atom's nucleus, or the three-dimensional structure of DNA. If knowledge of these properties is also contained in idealized and abstract models, might the perspectivist have it right about such properties? This would seem like a substantial concession on the part of the realist, to the extent that such non-dispositional properties are common and important. But perhaps we *can* gain model-based knowledge of such non-dispositional facts; responders to perspectivism have not shown how, so far; but they may yet do so. A second, related worry is articulated by Massimi (2012): The realist can assert that science affords access to non-perspectival facts. But how can the realist *justify* this assertion. According to Massimi, any justification of a concrete scientific claim is embedded in an epistemic network, consisting of information about observation devices, measurement techniques and background theoretical assumptions. This information, in turn, is perspectival. And so any claim for non-perspectival knowledge must "bootstrap" its way out of existing perspectives, a task which Massimi views as incoherent.[12]

These issues remain under discussion. Although it is related to older philosophical views, the modern, model-related guise Perspectivism is a relative newcomer in philosophy of science. It remains to be seen whether it can be articulated or refuted in a fully compelling fashion.

5. Troubles for IBE?

In closing, I want to explicitly address a methodological issue that has been largely in the background so far: the status of inference to the best explanation (IBE). Roughly speaking, IBE is a non-deductive inference rule that instructs us to believe the explanans of our best explanations. Versions of IBE vary, as do opinions about its epistemic credentials – Douven (2011) provides an up-to-date survey – but more than a few philosophers of science and epistemologists take IBE to be an important, even indispensable epistemic method (Lipton 2004; Weintraub, 2011). This issue has general and widespread significance in philosophy of science, but it poses special concerns for those philosophers – and they appear to be in the majority among realists – that put stock in the No Miracles Argument (NMA). For, as noted above, the NMA is typically seen as an instance of IBE, i.e. as the argument that successful scientific theories are likely to be true, since that is the best explanation of their success. However, one might worry that the ubiquity of models, especially idealized models, casts doubt on the very method of IBE. If many of our explanations make false assumptions about the world, then it would seem that we cannot use our best explanations as indicators of the truth.

But it seems that discarding IBE, at least on account of the apparent tension with modeling, would be somewhat hasty. Strictly speaking, IBE has an "if true" proviso: the IBE inference rule states that we ought to believe in the proposition that, if true, would best explain the data. This proviso is sometimes stated explicitly (e.g. Douven, 2011, §1), but not always. Clearly, the "if true" proviso entails, at the very least, that the candidate explanations that enter into an IBE should be ones that we do not *know* to be false, or else they would not even be admissible as candidate explanations. But this is patently not the situation with idealizing explanations. In such cases, we knowingly introduce falsehoods into our explanation. Thus, the existence of idealized explanatory models does not invalidate IBE; such explanations ought not to serve as inputs to IBEs to begin with.

That said, awareness of the prevalence and special features of modeling should give us pause, and engender caution, when relying on IBE. The discussion of non-correctable idealizations above attests to the difficulty of extracting the kernel of truth underlying an idealized model. They therefore serve to remind us that one cannot simply read off, from our most predictively successful theories and models, truths about what the world is like. Often enough such inferences are possible, but they are far from straightforward.

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[1] Not everyone accepts both claims and authors differ regarding their formulation. I am neither assuming that both elements are essential to realism, nor am I assuming some very specific understanding of the content of the two claims, or the relations between them. These matters do not affect the discussion below, as far as I can tell.
[2] In contrast, the so-called semantic view of theories (Suppes 1960; Suppe 1977; van Fraassen 1980; French and da Costa 2003) appeals to the logician's notion of model and uses it to give a general account of the nature of theories. Issues relating to the semantic approach will be set aside in this chapter.
[3] For more on this distinction, see (Thomson-Jones 2005; Godfrey-Smith 2009; Levy forthcoming).
[4] See Weisberg (2013) for responses to some of these problems.
[5] Yablo provides a precise technical gloss to the notion of a subject matter. But for present purposes an everyday informal understanding suffices.
[6] Yablo (forthcoming) develops some aspects of this view with an eye, specifically, to modeling.
[7] In this respect, the response to the non-correctable idealization challenge is similar to responses realists have sought to offer to the pessimistic induction (Saatsi, forthcoming).
[8] Strevens is not directly concerned with realism, but with how idealized models manage to explain. But since he regards truth as a necessary condition on explanation the problems are closely related.
[9] There is a debate over whether robustness analysis can serve as a surrogate, or perhaps even a type of, empirical confirmation (Orzack and Sober, 1993; Levins, 1993). For our purposes, what matters is the manner in which robustness analysis allows one to distinguish "the real from the illusory", regardless of whether this is taken to be a form of confirmation or not.
[10] Odenbaugh (2011) raises a closely related worry, arguing that only if one can replace the idealizations in the model by true assumptions (and provided the model's predictive success is not undermined by this) can one be sure that the "core" assumptions in the model do not depend on the idealizations. An assumption that seems to underlie Odenbaugh's argument is that the there is no in-principle limit to the idealizations one can put forward as a replacement. This assumption seems correct, at least in a sizeable range of cases. With it, Odenbaugh's argument is sound. However, it does not follow that by varying the idealized assumptions enough, so as to cover a wide range of possibilities, one cannot have *quite high confidence* that it is the core assumptions that matter for the model's success.
[11] Note that this is an epistemic, not a metaphysical, claim. Some perspectivists also hold that *reality itself* is, in some sense, perspectival. I doubt that this idea is coherent, but I will not discuss the matter here.
[12] This is a version of the bootstrapping criticism levelled at epistemic reliabilism (Vogel 2008).