Modeling without Models

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1. Introduction

In 1952 Alan Turing published a paper entitled "The Chemical Basis of Morphogenesis". It begins thus:

> "In this section a mathematical model of the growing embryo will be described. This model will be a simplification and an idealization, and consequently a falsification. It is to be hoped that the features retained for discussion are those of greatest importance in the present state of knowledge.
> The model takes two slightly different forms. In one of them the cell theory is recognized but the cells are idealized into geometrical points. In the other the matter of the organism is imagined as continuously distributed."

Turing goes on to describe, and then solve, a pair of differential equations, the first instance of what is nowadays called a Reaction-Diffusion model. In this model, the interaction between two diffusing chemicals – form-producing factors he labeled "morphogens" – generates distinct spatial and temporal patterns resembling those found in developing organisms, such as stripes, waves and polar zones. This paper broke important theoretical ground in developmental biology. But that is not why I quote from it. I do so because it is a characteristic and wonderfully self-conscious example of a ubiquitous scientific practice: *modeling*, i.e. the construction and analysis of models.

Much contemporary scientific knowledge arises from modeling and its importance is, if anything, growing. But while undoubtedly common and important, modeling raises significant philosophical questions. The aim of this paper is to assess some existing accounts of the practice, and then to suggest an alternative. While some accounts regard models as abstract and mathematical, I will argue that we are better off viewing modeling as concrete in character. And while some treat modeling as a form of indirect representation and analysis, in a sense to be discussed below, I suggest a view on which models are directly about the world.

As a preliminary, and in order to better understand why an account of modeling is needed and what its main desiderata are, let us zoom in on several specific features made vivid by the quotation from Turing. First and most important– one might almost regard it as the defining feature of modeling – is idealization. Models are typically simplified versions of real-world systems. Turing's model, for instance, idealizes away cells, depicts the embryo as ring-shaped and, as noted, treats its chemical makeup as consisting of two freely diffusing morphogens. Although the rationale for idealization is clear enough, it raises puzzles. When Turing, say, speaks of "a model…in which the cells are idealized into geometrical points" he is evidently not speaking of any concrete, actual, real-world thing. What then is he talking about? Some other thing (or object or system) in some other realm? A Turing-style embryo can exhibit a distinct pattern of stationary waves. This is a true statement – but what are its truth-makers? More generally, model discourse has many of the features of ordinary discourse surrounding real-world objects. But there are no visible, tangible, audible things corresponding to the statements and beliefs of modelers. What gives these statements and beliefs content? What makes some of them right and others wrong?

Two further (albeit related) features of modeling are also present in Turing's text. Firstly, modelers often speak of their work in terms of the imagination, as the construction of "stylized scenarios", sometimes even in terms of fiction (Turing says that in one version of the model, "the matter of the organism is *imagined* as continuously distributed", emphasis added). This terminology is associated with idealization: it is, in part, a way of saying that the model is idealized, rather than an accurate portrayal of reality. But it also indicates that the imagination has a special cognitive role in modeling. As we will see, a number of philosophers have emphasized this feature. Secondly, modeling operates at a certain "distance" from empirical reality. That's to say: commonly, the development of a model is partly or wholly decoupled from its application to real-life phenomena. The practice can be seen as involving two independent, or semi-independent operations – the positing and analyzing of a model; and an assessment of its bearing on the world. In Turing's case, this two-part structure is well-marked in the text. In much of it, he develops the model: describing the idealized embryo, formulating a mathematical representation and then solving the resulting equations. Only in the final few pages of the paper, in a section entitled "restatement and biological interpretation of the results" does Turing ask whether his analysis has a biologically interesting import. In general, though, the two operations need not be separate in actual practice, nor do they need to be undertaken sequentially. What matters is that they are logically distinct.

I will take these features – idealization, independence and the role of the imagination – as explananda for a philosophical account of modeling. I place most emphasis on idealization. Of course, any account of modeling must also allow for (and preferably explain) the epistemic utility of models: how can idealized models tell us about the world? This too will play an important role in my discussion.

The paper is organized as follows. In the next section (§2) I discuss whether models are abstract or concrete. I argue that the most plausible interpretation of the practice is that models have a concrete character. In section 3 I look at one recent set of ideas, according to which models are akin to fictions. Such accounts have attractions, in that they make sense of model discourse and of the appeal to the imagination. But I argue that they have a hard time with the model-target relationship and therefore with model-based knowledge. Then, in an extended section 4, I offer my own account, which can be construed as fictionalist, but doesn't appeal to anything like fictional entities. I elaborate on this account, the model-world relationship in particular, via a notion of partial truth, and then tackle some apparent problems.

2. Are models concrete or abstract?

A good place to begin an account of modeling is with a distinction most clearly articulated by Michael Weisberg (2007). In "ordinary" theorizing the theorist attempts to produce a description of some bit of the world – a description that may omit many details, but one that is seen as straightforwardly about some portion of the empirical world. Modeling, in contrast, often appears to proceed in an *indirect* manner: the modeler posits a certain scenario, a model system, and uses it as a stand-in, a surrogate, for the bit of the world she is after, the target system. Once the model is sufficiently well understood, the modeler assesses whether the findings apply to the target, by way of comparing the two systems. The point of this two-stage process is that it's easier to study the model system than to deal with the full complexity of the target. In this way of looking at modeling, the feature I described above as independence is given priority. Models are seen as objects of sorts, and so there is a clear sense in which work on the model proceeds independently from work on the target: the model *literally has* properties that can be analyzed and understood in their own right. Thus, on the indirect picture, the results of modeling apply in the first instance to the model; it is a separate (albeit important) question if and how they bear on the target.

It is intuitively appealing to view modeling in terms of indirectness. But a question immediately arises: what is the model system? Is it a real, bone fide object and if so what kind of object? My ultimate answer to this question will be that models need not be seen as genuine objects. But I will make the case for this in two steps: in this section I will operate under the presumption that models are objects and I will ask what *kind* of objects they might be. Specifically, I will discuss whether models are concrete or abstract – options that have been advanced in recent literature. In later sections, I drop the presumption of object-hood and offer a deflationary treatment of models.

A number of authors have proposed that models are abstract, some more explicitly than others. Ronald Giere (1988) maintains that models are abstract entities which satisfy – in the logician's sense – the descriptions appearing in scientific texts. In his recent book Michael Weisberg (2013) has greatly developed this view, arguing for further distinctions, especially between mathematical and computational models, and relaxing somewhat the relationship between model descriptions and the abstract entities they pick out. Weisberg regards models as interpreted mathematical structures: sets of abstract objects with relations ranging over them. On both these views model-based knowledge depends on similarity relations with target systems. Modelers compare the model to the target and, roughly, the more similar the two the more informative (vis-à-vis the target) is the model. Weisberg has developed a detailed account of model-target comparisons, specifying constraints on the relevant notion of similarity and describing the criteria according to which model-target similarity is evaluated.

A second option is that models are concrete hypothetical objects. Peter Godfrey-Smith has suggested such a view as has Roman Frigg. In this view, models can be ascribed ordinary physical properties such as size, weight and color. Being concrete, models can be described in ordinary English, or via graphical means, but they can also (and very often are, on the concrete hypotheticals view) specified and analyzed mathematically, as we might do with actual concrete objects.

Both the abstracta and the concrata views comport well with the independence of models, since both regard modeling as indirect: If modeling involves the positing of standalone model systems, then it is clear in what sense model description and analysis is independent of a description of the world. This holds irrespective of whether the model is abstract or concrete. Relatedly, both views can make sense of idealization and the discourse surrounding it. If models are abstract objects then, by virtue of their abstractness, they cannot be seen, touched or heard. Yet they do have definite properties, so that one can speak about them in a factual, right-and-wrong supporting manner. Analogously, if models are concrete hypothetical objects, then by virtue of their non-actuality, they are not the kinds of things we can observe and come into contact with. And yet they have definite features that one may get right or wrong.

But in other respects, the two ways of thinking about models are not on a par. I will discuss three such issues, in order of increasing importance. First, the abstracta view does not do as well with the role of the imagination in modeling. Second, model-target comparisons are harder to accommodate on the abstracta view. Lastly and most importantly – in my view a sufficient reason to reject the abstracta view – the abstracta view doesn't make adequate room for non-mathematical modeling.

Imagining typically involves having a visual or other sensory-like mental state – a "seeing in the mind's eye".[1] And so it involves the entertaining of a concrete scenario, with

[1] The notion of imagining is sometimes used in an experiential way, to denote "offline" sensory experiences. Other times it is understood as a propositional attitude. I am not here endorsing the idea that the imagination must be understood as constituted by "offline" sensations. Some imaginings may have a propositional character. But the experience-like aspects of the imagination appear to be common and cognitively important.

concrete attributes. Turing, let us suppose, was aided in his development of the model by the experience of "seeing" a ring-shaped embryo with continuously distributed matter. This role for the imagination, in all its concrete glory, is easy to understand if models are concrete objects – for then the imagination is a tool for engaging with the model itself. On the abstracta account, however, there cannot be as tight a link between modeling and the imagination, since abstract objects do not have properties such as bulk, shape and color and so cannot be readily visualized. Advocates of the abstracta view can deal with this difficulty in two ways. First, they may deny that the imagination plays a central role in modeling. I believe that the role of imaginative cognition is evident in many episodes of modeling (Turing's words attest to this, but there are many other examples) and so I do not regard this as an adequate move. Second, the abstracta view may assign a real but secondary role to the imagination. Weisberg, for instance, argues that the imagination can help the modeler translate intuitions and assumptions about the world into mathematical language, tease out the properties of the model, especially when the mathematics are complex, and assist in coordinating between models couched in different formalisms (2013, Chapter 4). This is not an entirely implausible view, but I think it is somewhat less natural and immediate. In some mathematical contexts – geometry, for instance – the role of the imagination can be readily appreciated, even irrespective of modeling . But this isn't always the case. So it seems that on the concrata view the imagination's role is more naturally understood.

A second concern about the abstracta view has to do with model-world relations. On an indirect conception, models bear on the world via relations of resemblance. A simple notion of resemblance, as the sharing of properties in common, will not do here, since abstracta and concrata cannot share properties (Thomson-jones, 2010). A popular alternative has been to view model-target relations in set theoretic terms, specifically as involving a mapping, such as an isomorphism (or a partial isomorphism), between models and targets (Llyod 1994;

French & Ladyman 1999). Model-target mappings have been widely discussed. They make good formal sense, but they suffer from significant problems in terms of their match with the practice: in some cases it is highly doubtful that the evaluation of model-target similarity is made (or is even amenable to) formal terms. Furthermore, as Suarez (2003, 2010) argues, isomorphism and related mappings do not suffice for model-target representation, because any target will instantiate many formal structures and hence the relevant isomorphism will be underdetermined. Isomorphism is also unnecessary, since many targets will share a formal structure with a model without being represented by it. It is also unclear whether a mapping-based view can make sense of misrepresentation, either deliberate as in idealization (Pincock, 2005) or inadvertent (Suarez, 2003). Now, I do not want to suggest that these problems are insoluble; the mapping account of representation can be corrected and/or supplemented. It need not rely on formal mappings alone, and may appeal to, e.g., the intentions of modelers to fix targets (Callender & Cohen, 2006; Weisberg, 2007). But I think that the problems with a straightforward application of the mapping idea mark a drawback of the abstracta view.

A final, and to my mind the most serious problem for the abstracta view of models, is that there are important aspects of the practice which it seems unable to accommodate. True, much modeling involves mathematical analysis – Turing's paper, as mentioned in the introduction, is a case in point – and the abstracta view is well-suited to account for them. But modeling isn't wholly mathematical and cannot be assimilated, in the main, to mathematics (Downes, 192; Thomson-Jones, 2010). This is the case even for models such as Turing's. For even in these cases it is common to find that the model is ascribed concrete properties (a chemical makeup, a shape and size etc.). Presumably, mathematical entities, and abstracta more generally, do not have properties like these. So such ascriptions must either be seen as not fully serious, or as expressions of the interpretation of the model. The former option isn't appealing, as the ascription of concrete properties to models is common and central in many cases. The latter option seems to jeopardize the two-step structure of modeling, as it ties the model's description and analysis fairly closely to its bearing on the target.

But even if cases in which models are treated in both mathematical and concrete terms are set aside, a more important point is that some models are *wholly non-mathematical*. Cellular and molecular biology contains many important examples. Explanations of key cellular processes, such as DNA replication or hormonal signaling, are almost always couched in qualitative, non-mathematical terms. They consist of a simplified mechanistic description, which shows how the process looks "under the hood": What are the structural components, how do they interact and change, what material inputs and outputs are required and so on. Instances from other parts of biology, and the special sciences more generally, abound. In such cases work usually proceeds in graphical or verbal terms, consisting of informal descriptions of seemingly concrete systems with concrete properties. These are typically referred to as 'models' by scientists, involve idealization and often call for imaginative engagement. Thus, they appear to be models in the relevant sense, yet they are not specified in formal terms nor is their analysis reliant on deductive or quantitative reasoning. It would be possible, of course, to re-describe this type of scientific work in mathematical terms, for example as an automaton with states (and transitions among them) corresponding to the different steps in the cell's molecular machinery. Such a move is possible, but it seems to jar with scientific practice. It is not a coincidence that mechanistic models in molecular biology are rarely couched in mathematical terms. Such work does not aim to make use of formal deduction or quantitative prediction. It does not require the kind of precision and certitude that mathematics enables. Rather, it is devoted to qualitative causal reasoning, which is greatly facilitated by concrete, informal description. In other words, re-describing concrete models, such as those found in cellular and molecular biology, in mathematical language involves substantial shoehorning and results in a view that, to my mind, does a real injustice to the relevant scientific practice.

Another way the advocate of the abstracta view might handle non-mathematical models is by treating them as descriptions of concrete models. Weisberg entertains this possibility. He considers the case of textbook depictions of a cell – which tend to consist of a picture of the cell with simplified depictions of organelles and other structure within it – and suggests it can be treated as "a concrete model, albeit one that probably has never been built." (2013, p. 19). On this proposal, non-mathematical models are seen as unrealized instructions, so to speak, for the construction of actual concrete objects: potential three dimensional counterparts to the pictures and verbal descriptions of the biologists. This is an ingenious solution, but, to my mind, it is even less appealing than an attempt to re-describe qualitative verbal models in mathematical language. For one thing, it clearly involves substantial shoehorning, as such pictorial and verbal depictions are rarely if ever treated this way by the relevant scientists. Secondly, this creates an unnecessary complication: if our account already involves an unactualized concrete cell, why not identify it with the model, obviating the need for an abstract mathematical go-between? Finally, if the pictorial and/or textual description is indeed an un-built concrete model, then one would expect that using it to build an actual model would prove scientifically useful. This seems highly doubtful; indeed the opposite seems more likely. In many cases the relevant models would be less perspicuous if transformed into actual concrete objects. The cell is a case in point, as it would be harder to comprehend the internal structure and operation of a fully material model of the cell, and many of the advantages of the simplified depictions one finds in textbooks would be lost.

Thus, the abstracta view seems hard pressed to make room for these sorts of qualitative, informal models. It is worth emphasizing that the situation isn't symmetrical – the concrata view can handle mathematical modeling. In both the abstracta and the concrata views, equations and other mathematical formulae are vehicles of description, whose role is to specify the model system and facilitate its analysis. But mathematical language can be used to describe concrete objects, whereas non-mathematical language (in all but fairly simple cases) cannot be used to describe mathematical objects. Thus, I think the existence of non-mathematical models raises a serious concern for the abstracta view, but there is no parallel problem for the concrata view.

On the whole, then, I think the view that models are abstracta has difficulties in accommodating some central features of modeling– namely the role of the imagination, the possibility of comparison, and the existence of non-mathematical models. For these reasons I favor the concrete approach. But, as I will argue in the next two sections, the conclusion we ought to draw from these considerations isn't that models are bona fide concrete objects. Modeling has a concrete character, but we can accommodate that without appealing to anything beyond the targets they are directed at.

3. Models as fictional concrata

So far, I have argued for the idea that models have a concrete character. In the remainder of the paper I want to "close in" on a particular version of this view, according to which modelling is directly about real-world targets, albeit under special, idealized, descriptions. I will arrive at this view by first assessing the idea that modeling is concrete and indirect – more specifically, that it involves *fictional* concrata. Such an account has been put forward by both Peter Godfrey-Smith and Roman Frigg, in ways that are largely complimentary.

In a paper that endorses and expands on the indirect view of modeling, Godfrey-Smith suggests that:

> “Roughly, we might say that model systems are often treated as "imagined concrete things" – things that are imaginary or hypothetical, but which would be concrete if they were real… Although these imagined entities are puzzling, I suggest that at least much of the time they might be treated as similar to something that we are all familiar with, the imagined objects of literary fiction. Here I have in mind entities like Sherlock Holmes' London, and Tolkein's Middle Earth. These are imaginary things that we can, somehow, talk about in a fairly constrained and often communal way. On the view I am developing, the model systems of science often work similarly to these familiar fictions. The model systems of science will often be described in mathematical terms (we could do the same to Middle Earth), but they are not just mathematical objects.” (2006 p. 735)

Godfrey-Smith motivates this view in large part by emphasizing that modelers often think of models as concrete, ascribe concrete properties to them and, importantly, make substantial use of the imagination in modeling: “It is a striking feature of our psychological capacities that we can engage in [a] process of schematic imagining. The skill is put to one kind of use in recreational and literary fiction, and to another kind of use in science.” (Ibid p. 736) In making these suggestions, Godfrey-Smith remains non-committal on how to understand fictional entities. He voices a preference for a naturalist, deflationary account, but does not develop one. Frigg’s view fills exactly this gap, drawing on Kendall Walton’s pretense theory of fiction (Walton 1990). Here is a brief recap.

Many philosophical discussions of fiction have made the common-sense assumption that fictional texts are descriptions and have sought to give an account of the referents of the descriptions, the things that fictional texts are about (Thomasson 2009). Such accounts have faced serious problems and part of Walton's motivation is to avoid these problems. His key move is to regard fictional texts not as descriptions but as *prescriptions*, i.e. instructions for the imagination. Thus Walton treats fictions as akin to games of make-believe: interpersonal, rule-governed exercises of imagination. In standard games of make-believe there is often a prop – an object, such a hobbyhorse or a costume – that guides the imagination, and constrains what players of the game are supposed to imagine. Extending the notion of a prop, Walton regards texts and paintings as props too: they are objects whose properties (the inscriptions on the page, the splotches of paint on the canvas) guide the imagination of readers and viewers.

Frigg's view is an application of Walton's theory to modeling: Models are Waltonian games of make-believe. A set of equations or a mechanism sketch is a prop that, together with the rules relevant for the scientific context, determines what those engaging with the model – the game's participants – ought to imagine. Turing's text and equations aren't, in this view, a description of an imaginary entity but a prescription to imagine a ring-shaped embryo with the specified chemical makeup. Thus, there is no object, The Turing Embryo, to which Turing's equations somehow correspond. There are only inscriptions on a page which function as instructions for the imaginations of modelers.

While this account treats models as mere make-believe, it is intended to preserve the basic structure of an indirect take on modeling: there is a genuine, right-and-wrong supporting sense in which one can speak about the model, irrespective of the target. And so there is a sense in which model specification and analysis can be done independently of assessing its bearing on the target. Accordingly, Frigg takes seriously the idea that model-based knowledge requires a comparison between the model and the target. The Turing embryo is informative about real embryos to the extent that it resembles them in certain respects and to a sufficient degree. Thus, Frigg's account has two attractive features: it assigns a central role to the imagination; and it portrays models as concrete and independent of the target, without introducing ontological extravagancies such as fictional entities. However, these two features are also associated with some significant bugs.

Frigg aims to preserve indirectness and the concomitant appeal to model-target resemblance and comparative judgments. Resemblance, whatever exactly it comes down to, is a relation between objects (or events, or at any rate, bona fide things). But on the make-believe view, the model is not *a thing* in any robust sense – it is only a set of prescriptions for the imaginations of scientists. How then are models to be compared with targets (or, for that matter, with one another)? Statements comparing fictional entities to non-fictions – so-called "transfictional" statements – occur in connection with literary fiction and they have been discussed in that context, mainly because they appear to pose problems for deflationary treatments such as Walton's. Indeed Walton discusses various ways of handling such statements. For the most part, these are attempts to explain away the phenomenon: Walton suggests paraphrasing comparative statements such that they are seen as either pretend-comparisons or statements that are in fact about the text and/or the rules of generation, rather than statements about the apparent target of comparison. Neither option will do in the context of modeling, understood indirectly, since comparisons are meant to generate knowledge (rather than pretend-knowledge) about targets (and not about the rules of the game).Perhaps with these concerns in the background, Frigg suggests handling the issue differently:

> "The transfictional statements that are relevant in connection with model systems are of a particular kind: they compare features of the model systems with features of the target system. For this reason, transfictional statements about models should be read as prefixed with a clause stating what the relevant respects of the comparison are, and this allows us to rephrase comparative

> sentences as comparisons between properties rather than objects, which makes the original puzzle go away." (2010, p. 263).[2]

But this way of making the puzzle go away carries a significant price: such comparisons presumably invoke uninstantiated properties. The model, being merely imaginary, cannot instantiate properties. But this diminishes both the perspicuity of the account, and its metaphysical attractiveness, to the point where it calls into doubt the appeal to pretense. As Godfrey-Smith has commented: "It is not clear that giving an explanation of modeling in terms of uninstantiated properties is more down-to-earth than giving one in terms of non-existent objects." (2009, p. 114). Thus, I think that while Frigg's indirect make-believe account is an improvement over the abstracta view, it also carries unappealing commitments. The source of the trouble, it seems, is its indirect character, which underlies the need to appeal to model-target comparisons.

Thus, we have good reasons to treat models as concrete and to assign a central role to the imagination in model development and analysis. However, identifying models with *imaginary entities*, even if in the end they are nothing but mere make-believe, is problematic because it is hard to make sense of model-target comparisons on such a view. The next section suggests an account that retains a fictionalist spirit, but in a way that avoids the problems just discussed.

[2] This, too, can be seen as application of a strategy offered by Walton. In (1990, §10.5.) Walton considers the statement: "Sherlock Holmes is more famous than any living detective". It can be paraphrased, he says, as: "There is a degree of fame such that no real detective is famous to that degree, and to pretend in a certain manner [in the manner in which one who says "Sherlock Holmes is famous to that degree" normally would be pretending] in a game authorized for the Sherlock Holmes stories is fictionally to speak truly." (1990, p. 414). The complexity of this paraphrase notwithstanding, it seems that Walton is arguing that the comparison between detectives can be understood as a claim about a property sharing relation – in the example, the property of being famous to such-and-such a degree. At least in the case of Holmes this would seem to involve an uninstantiated property.

4. A direct view of modeling

The account I will suggest relinquishes the notion that modeling is indirect. Instead it conceives of models as immediately about targets in the world. A similar view, also invoking Waltonian ideas, has recently been suggested by Adam Toon (2012), in a book length treatment which I am largely in agreement with. Toon does not elaborate an account of the model-target relation, focusing instead on accounting for the content of the model in and of itself (but see ibid, pp. 66-67, for some suggestive comments). The ideas I develop below are therefore largely complimentary to Toon's; as far as I can tell, he should be broadly sympathetic to them.

4.1. Models as prop oriented make-believe. The account I shall offer rests on a very simple idea, and most of the necessary work consists in explaining how, despite its simplicity, it can fulfill the requirements of an account of modeling. To introduce it, consider the following comments by Kendall Walton:

> "Where in Italy is the town of Crotone? I ask. You explain that it is on the arch of the Italian boot. 'See that thundercloud over there – the big, angry face near the horizon,' you say; 'it is headed this way.'...We speak of the saddle of a mountain and the shoulder of a highway... But it is not for the sake of games of make-believe that we [do so. Rather, the make-believe] is useful for articulating, remembering, and communicating facts about the props – about the geography of Italy, or the identity of the storm cloud...or mountain topography" (1993, p. 40).

When we speak of Italy as a boot or of thunderclouds as faces – we are doing so in an as if mode; it is a make-believe of sorts. Ordinarily, we think of make-believe as a playful, recreational activity and, in some sense, as providing us with access to an "imaginary world". But games of the Italy-as-a-boot sort are different in two respects. First, they involve imagining *of certain real-world objects* that they are different from how they actually are. Second, *the motivation is epistemic*: we play as a means of describing and reasoning about a real-world object. Games with these features are what Walton calls prop oriented make-believe – they are games we play for the sake of learning and thinking about the objects on which they center. My suggestion is that we treat models as games of prop oriented make-believe – where the props, as it were, are the real-world target phenomena. To put the idea more plainly: models are special descriptions, which portray a target as simpler (or just different) than it actually is. The goal of this special mode of description is to facilitate reasoning about the target. In this picture, modeling doesn't involve an appeal to an imaginary concrete entity, over and above the target. All we have are targets, imaginatively described.

It is probably evident that this idea breaks with the notion that models operate indirectly. Instead of appealing to model systems, the account treats modeling as directly about real-world targets. Such an account retains the idea that modeling has a concrete character, and makes sense of idealization as, quite straightforwardly, the provision of simplified descriptions of targets. (I will comment about independence below). So an account like this dispenses with problems associated with the ontology of modeling, without ignoring the observations that led to the idea of indirectness. Furthermore, the direct account seems to fit quite nicely with some of the language modelers use. Turing, for instance, speaks of "the" developing embryo, suggesting that his subject matter is none other than regular, real-world embryos.

The direct account is simple and ontologically economical. But it raises at least two important questions. On the indirect account, model-based knowledge is grounded in comparisons: the model's properties are compared to the target, and the extent of their similarity permits conclusions about the model to be applied to the target. But on a direct account, there is no model system, not even an imaginary one. What, then, grounds model-based knowledge? Second, at least some modeling appears not to be directed at a specific target. Can a direct account handle such cases? Let us attend to each of these issues in turn.

4.2. Partial truth. The bearing of models on the world, I will suggest, can be accounted for in terms of partial truth. The idea, to put it tersely, is that while model descriptions are typically idealized, hence not true of their targets *simpliciter*, they are nevertheless *partly* true, at least when successful. Partial truth (of a sentence, or a collection of sentences) is then understood as truth of a part (of the sentence). The idea of partial truth, variously labeled, has been put forward in the philosophy of mathematics in order to explain the application of mathematical discourse to the world without invoking abstract entities such as numbers and sets (Balaguer 2011). Recently, Stephen Yablo (2014) has developed and extended the notion of partial truth, suggesting that it may figure in a variety of contexts in which "ordinary" whole truths are hard to access or express. In this framework, the notion of partial truth depends on a more general notion of subject matter. Subject matters, and a number of derivative notions, are construed in terms of possible worlds. My discussion is deliberately phrased so as to make minimal reference to possible worlds. I will remark on the ontological issues associated with possible worlds below.

According to Yablo's account, the subject matter of a given sentence S is, roughly speaking, the set of possible circumstances PC such that there cannot be a difference in PC without a difference in S's truth-value.[3] In other words, a sentence's subject matter is the possible circumstances on which its truth conditions supervene.[4] The intuitive idea here is that the subject matter of a sentence is a partition of the space of possibilities – an indication of what makes the sentence true or false, when it is. With this notion of subject matter in hand, Yablo goes on to define a notion of content part. A is part of the content of S iff S implies A and A's subject matter is part of S's subject matter, i.e. if the possible circumstances on which S supervenes include the possible circumstances on which A supervenes. The notion of partial truth is defined, straightforwardly, in terms of this notion of content-parthood: a statement is partly true if it has a content-part that is (wholly) true. In other words, a statement is partly true if it is true when evaluated only relative to a subset of the circumstances that make up its subject matter – the subset corresponding to the relevant content-part. "The number of planets in the solar system is nine" equates the number of planets with the number nine. Its truth or falsity supervenes in part on facts about numbers, and in part on the composition of the solar system. Even if we assume that there are no numbers, it would still seem that this sentence says something true *about the solar system*. On the partial truth account, this is because the sentence has a true part, namely the part that pertains to objects of the solar system. It should be emphasized that partial truth is not approximate truth: it is not that "the number of planets in the solar system is nine" is more or less true. Rather it has a distinct part that is true, i.e. the part concerning the solar system and a distinct part that is false, i.e. the part concerning numbers (at least if we suppose that there are no numbers).

[3] This formulation can be extended to other propositional representations, but I will not do so here.
[4] 'Possible circumstances' is my term. Yablo further refines the notion to distinguish between those possible circumstances that account for why the sentence holds, where it holds, and those that account for why it doesn't, where it doesn't (the latter he calls the "subject anti-matter"). I will not dwell on these details here.

Yablo briefly suggests that the notion of partial truth can be applied to models (2014, §5.6). I will elaborate somewhat, although a full discussion will not be possible in this paper. As I've explained, the motivation for introducing partial truth in the mathematical context stems from a concern about reference: the nominalist wants to retain mathematical discourse, while making sense of the apparent reference to mathematical entities. In the scientific case the problem is not quite the same: idealized elements of a model simplify (i.e. misdescribe) but, typically, they do not fail to refer. However, in some cases the situation can be treated analogously because what we learn from a model may be seen as "bracketing" the idealized aspects. Consider, for instance, the case of the ideal gas model. It explains Boyle's law, namely: a gas's volume is inversely proportional to pressure. As the name suggests, the ideal gas model makes a number of idealizations – for instance, it assumes that gas particles do not collide with each other. But these assumptions can be shown not to affect the relationship between pressure and volume. That is to say: the question of inter-particle collisions turns out to be irrelevant to explaining Boyle's law. Now, on a similarity view, we should understand such a case roughly as follows: an ideal gas (the model system) is similar in some respects to real gases, e.g. in that both are composed of particles which obey a certain energy distribution. In other respects, an ideal gas is dissimilar to real gases, .e.g. in that real gas particles collide all the time, a fact that is responsible for their specific trajectories, velocities (and for much else about their behavior). Ideal gas particles, in contrast, never collide with each other and so collisions play no role in determining their behavior. Given these similarities and dissimilarities, and a judgment about their relative importance, we can reach a judgment about the explanatory merits of the model. In contrast, on the partial truth view, we do not proceed via an assessment of resemblance. Rather, we evaluate the truth of parts of what the ideal gas model says, when interpreted as about real gases. When we do so, we find that it is partly true, i.e. with respect to the role of the motion of particles; and partly untrue, i.e. with respect to the role of collisions among particles. We know that the untrue part does not affect the explanandum (Boyle's law), so we regard the partial truth contained in the ideal gas model as an explanation of Boyle's law.

Now, you might think that there is a simpler way of describing the situation with the ideal gas model– namely, that some of what it asks us to imagine is *true simpliciter* of real gases (e.g. that their molecular energies have a certain distribution) while other elements of what it asks us to imagine are *false simpliciter* of real gases (e.g. that their molecules do not collide). [5] This is correct but incomplete. For the model is taken to be an *explanation of the Boylian behavior of real-world gases*, i.e. the fact that they behave in accordance with Boyle's law.[6] We might think of the situation as follows. The model is an instruction to regard (imagine) a real world gas as if it had various features (including non-colliding molecules). When it is used to explain real-world Boylean behavior, we are in effect told that because of the specified features, the gas behaves in a Boylian way. This, we know full well, cannot be true as stated, because the gas simply doesn't have all the specified features (in particular, its molecules collide all the time). Here we bring in partial truth: the model (or, more precisely, the derivation of its Boylean behavior) is partially true and partially untrue: true with respect to the role of energy distribution, but false with respect to the role of collisions.[7]

[5] I thank an anonymous referee for spurring me to clarify this portion.

[6] Note that if the model were merely predictive this problem would not arise. We could then simply say that some of what the model instructs us to imagine is true, some false, and the result of following these instructions allows us to arrive at an accurate (let's suppose) prediction. In a (merely) predictive model, no claim is being made about the dependency relations underlying the target (of prediction).

[7] We could go a step further here and argue that what the model (understood in context) says is that whether molecules collide or not is *irrelevant* to the Boylean behavior, or that collisions *do not make a difference*. (Strevens, 2008, chapter 8). This, as I say, represents a further step. It is compatible with understanding the model's content as partially true with respect to the role of collisions that, given the context, it conveys another proposition that is wholly true, namely that even if their role were portrayed accurately, the gas would behave the same way. (This latter proposition, of course, requires additional justification, as it does not strictly follow from the ideal gas model).

The ideal gas model represents a simple example, because the role of the different factors can be separated. Another example will further clarify the account, I hope, and illustrate that it applies also, perhaps even especially, in cases where the role of some factors cannot be neatly "cordoned off" as representing irrelevant partial (un)truths. Consider, for instance, a simple physical model of a solid body moving down a frictionless inclined plane. If we ask what factors affect the body's velocity, we cannot simply regard the issue of friction as an irrelevance. Velocity is affected by friction (as well as gravity). However, we can say that the model truly depicts the relationship between the body's mass and its sliding velocity – how the latter depends on the former. This may be construed as a claim about underlying component forces or capacities (Cartwright 1983), but I am not presupposing this here (we might, for instance, understand this as a claim about dependency relations within a specific causal context). Either way, we can regard the claim about the lack of frictional force as a content-part of the model – a false part of its content – and the claim about the effect of gravity as another content part – a true part. Here, unlike in the case of the ideal gas model, the partial truth doesn't concern a sufficient causal effect. We cannot say that the body would slide at the same velocity irrespective of friction. But we can say that the relation between gravity and velocity – a part of the content of the model – has been described truthfully.

To be sure, there are other sorts of cases, which may be harder to capture in terms of partial truth. Sometimes, a model treats some factor in a system in an idealized fashion, and this factor modulates the relationship between two other factors (where the latter, let's suppose, are depicted accurately). In this kind of case – a kind of failure of causal modularity (Woodward 2003) – it is often not obvious what the model says about the relationship between the non-idealized factors, and whether that can be treated as a partial truth. However, for analogous reasons, it is not obvious how such case might be handled by a similarity view. For such views regard similarity as a pertaining to features of the objects (this is required to avoid an overly permissive notion of overall similarity) and analogous concerns about isolating the pertinent aspects and making adequate similarity judgments may well arise.

Issues such as these require, I think, a detailed comparative discussion of the similarity and partial truth perspectives, which I am not able to offer here. Moreover, I have not looked at questions that are indirectly or derivatively related to the empirical content of a model – such as justifying one's idealizing assumptions, and the not uncommon case in which a model that is developed with one target in mind is reapplied to a different target. Let me be clear that I do not rule out an appeal to similarity in these contexts. My aim has not been to show that similarity has no place in an account of modeling. Rather, the goal is to outline a way in which, even if one views models as concrete in character, it is possible to move beyond the idea that model-based knowledge pertains to model-target similarities. I leave a fuller discussion, and a more considered judgment on these issues, for another day.

4.3. Ontological commitments. Before discussing models that do not have targets, let me comment on the ontological commitments of the appeal to partial truth. As I've noted, the notion of partial truth is analyzed by Yablo in terms of possible worlds. These are anathema to many philosophers, including some who have written on modeling. I am not sure, myself, what to make of possible worlds. However, I want to highlight a few points that are relevant in the present context. First and most simply: modal discourse is meaningful and important, both in science-related contexts and elsewhere. We need some account of it. I presume that such an account, if it isn't couched in terms of possible worlds, will do as well in the context of partial truth. Indeed, this appears to be Yablo's attitude too – his appeal to possible worlds is part of a semantic analysis, but he does not accept the associated ontology (Yablo, 1997). Second, it should be emphasized that other accounts of modeling, including in particular the indirect make-believe view, also presuppose modal notions, and to that extent are equally “guilty” of a commitment to possible worlds. This point is often neglected in discussions of the make-believe approach, so it is worth highlighting. Walton’s framework is designed to make sense of fiction without appealing to *sui generis* fictional entities. But it is not nominalistic or metaphysically austere across the board. Walton is no foe of abstracta. In fact his strategy for deflating fictional entities presupposes entities such as propositions and properties.[8] The notion of a proposition, as is well known, is often understood in terms of sets of possible worlds – and indeed Walton understands it this way (1990, p. 145). Perhaps Frigg and others who have recruited make-believe to make sense of modeling hold that propositions and properties are to be understood without reference to possible worlds (or to modal notions in general). If so they have not developed this idea, nor even made it explicit. If they were to do so, it seems likely that the same sort of story can be used in an account of partial truth.

4.4. Taregtless models? The account of models I have proposed ties modeling fairly closely to targets – it views models as directly about them. But it might be objected that some models seem not to be directed at any specific bit of the world. Can my direct account accommodate such targetless models? I think it can, and I will argue for this by distinguishing different cases of (real or apparent) targetlessness.

Sometimes, a model is proposed in a non-committal spirit: it is meant to apply to a target, but the specific range and features it captures are not known for sure. Such cases do not present a serious difficulty for a direct account of the sort I have offered, since the

[8] “My resistance to fictional objects is not part of a suspicion of abstract entities in general. It does not derive from broad empiricist or nominalist tendencies that would equally apply to properties, numbers, propositions, and meanings... Indeed I have shamelessly helped myself to properties and propositions in the previous chapters, and I will use them now in explaining away fictional entities” (Walton 1990, p. 390).

appearance of tragetlessness is an artifact: it only seems this way because we have taken a snapshot of the modeling process. When a model is proposed it might not be clear at first what target it is tied to, and there might be a period in which the right target is sought. But later, assuming the model is retained, this issue is usually clarified. At that point the direct account can be applied.

A second kind of case is described by Godfrey-Smith:

> "The role of some [models] might be illustrated with a "hub-and-spoke" analogy. In these cases, what scientists do is give an exact description of one case of the target phenomenon, which acts as a "hub" that anchors a large number of other cases. The "other" cases include all the actual-world ones; the hub is a fiction…This organization of theory is one in which idealized models do not go away once knowledge becomes highly developed. They retain an explanatory role as a consequence of their generality." (2009, pp. 106-7)

The kind of case Godfrey-Smith is describing isn't a case of non-committal. As he makes clear, the "hub-and-spoke" structure is a permanent one. However, as he also remarks, the reason the hub is retained is its generality. This is a key point, and allows us to regard hub-and-spokes cases as direct representations of *generalized* targets. Godfrey-Smith mentions cases from population biology, in which the "hub" is simple yet understood in an exact way, whereas specific models use the basic template provided by the hub, together with approximation tools from demography, ecology and related areas. I think we can regard the hub, in such cases, as a highly idealized treatment of, say, population growth in general. The population biologist understands the general case in an exact way, but she also understands that the general model, as such, is true only in a very partial way. When confronted with a specific case, she therefore limits the generality of the account, but obtains a truer model for the context at hand. Thus, both the hub and the spoke are anchored to the world albeit at different levels of generality, and with different degrees of partial truth. Turing's model is a case in point: originally, the model was aimed at a generalized target ("the developing embryo"). In the ensuing years, it has spawned a large family of so-called Reaction-Diffusion models, some of which are tailored to specific developmental systems (Kondo & Muira 2010).

A third case of targetless models consist of what we may call pure targetlessness – models that are never applied to any target, not even in a generalized or roundabout way. Michael Weisberg discusses a number of cases including the so-called Game of Life, and the broader class of cellular automata. Weisberg observes that even such purely targetless constructions can be relevant to the real world. They may "sensitize our imagination, so that we learn how to notice things we might have missed otherwise" and "can inspire a more general modeling framework that can be used for target directed modeling." (2013, pp. 130-131). I accept Weisberg's observations, but I do not think it natural to regard Game of Life and similar cases as instances of modeling. It is much more plausible to see them as bits of mathematics. Sometimes such bits of mathematics may be adapted so as to serve as models, and in these instances we can expect that they will have targets (specific or general). When they are not so adapted they need not be seen as models. In this context it is interesting to note that Game of Life was first presented as "recreational mathematics" (Gardner 1970), and is often regarded as a piece of mathematics even today (after its bearing on the world has been debated at length). Thus, I accept that pure targetlessness can be of relevance in science, but I do not think we need to tailor our account of modeling to accommodate it.

5. Conclusion

I have discussed various accounts of modeling, raising objections to the abstracta view and to the indirect version of the concrata view. Against this background I have described an account that aims to be philosophically modest while at the same time illuminating with respect to the practice. It portrays modeling as the direct representation and analysis of target phenomena, but also as intimately tied to the imagination. Models are imaginative descriptions of real-world phenomena. An upshot of this view is that there is no room for model-target comparisons, but I have argued that an account of model-based knowledge can be grounded in the notion of partial truth.

In his recent book, Weisberg complains that "a consequence of [a view such as the present one] seems to be that there really is no difference between the practice of modeling and the practice of abstract direct representation. Far from explaining the special uses to which models can be put, [it] says that there aren't any models at all. (2013, p. 64)" There is some truth to this remark, but isn't as damning as Weisberg seems to think. It is true that on the account I have offered the line that divides modeling from non-modeling is not very sharp. This is because it is not a matter of there being some entity, the model system, through which the model bears on the world. On the other hand, the account I have offered explains the features outlined in the introductory section – idealization, independence and the role of the imagination – and these, I take it, are the features that make modeling distinctive. I believe that this sort of account displays a good balance between illuminating the practice and general philosophical concerns. That is what recommends it over the alternatives.

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