Models, Fictions, and Conditionals

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> The logical simplicity characteristic of the relations dealt with in a science is never attained by nature alone without any admixture of fiction.
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> Frank Ramsey, 1926.

1. Introduction

A philosophical outlook associated with Quine (1960) champions science and is distrustful of modality – of possibility and necessity, essences, counterfactuals, and so on. This combination shows some tension, however; scientific language and practice seem alarmingly imbued with the modal. In response to Quine himself, this challenge was made by pointing at science's use of causal modalities (Føllesdal 1986). Another aspect of everyday modal thinking is treating the actual as surrounded by a cloud of possibilities – other ways things could be, or could have gone. In science, the cloud acquires more structure; it can become a state space. One of the ways nature has been mathematicized is by means of organized spaces of possibilities.[1] Once the properties of things are seen as the values of variables, it's natural to recognize other values, including those never instantiated. Laws, and the like, usually relate variables with respect to all those values. This scientific organization of possibilities is akin to the philosophical idea of an array of possible worlds, with comparative nearness relations between the worlds. In the

[1] See Williamson (forthcoming a, b) for discussions of modality and state space models.

philosophical project of Lewis (1986), the two roles for modality I've mentioned so far are linked: the basis for the causal push, to the extent that such a thing exists, is nearness relations between worlds.

The most *obvious* role for something beyond the actual in science, though, is the kind of model-building in which the merely possible is overtly on the table – the science of infinite populations, frictionless planes, and wholly rational economic agents.[2] In this kind of model-based science, we see attempts to understand complex actual systems by analyzing simplified analogues of those systems, analogues that do not, as far as anyone knows, actually exist. Many useful scientific tools can only be applied, or can only yield comprehensible results, if you apply them to a modified analogue of the system you are trying to understand.

The push of causation, the organization of clouds of possibilities, and overt detours through fiction can all be understood as *some* sort of scientific employment of the merely possible. Science takes possibility seriously. That is not to say that science takes the non-actual to be in some way *real*, but it treats possibility in an apparently weight-bearing way.

In contemporary metaphysics, questions about what might be going on in talk about the merely possible are embraced, but in philosophy of science there's sometimes a reluctance to attempt a general story about it. The metaphysical road is seen as one to avoid. Arthur Fine (2009), commenting on recent discussions of the role of fictions in scientific modeling, says that this work is looking for a theory where one is not needed: "I am suggesting that there is no genuine problem for anyone to look into" (p. 124).[3] Science makes use of fictions, and this sometimes helps us to understand things, and that's the end of the story.

I think this is a mistake, and the attempt to give a general account of how fictions work in modeling is appropriate. The problem is analogous to a well-known family of problems about mathematics, especially what Wigner (1967) called the "unreasonable

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[2] Lewis made a brief comment about this in his 1986 book about modal realism – he saw this as another application of his framework.

[3] "I am suggesting that there is no genuine problem for anyone to look into. There is no need for the philosophical equivalent of an ''impetus theory'' to explain how a gap is bridged. Properly understood, nothing about a gap calls for philosophical explanation." (Fine 2009, p. 124)

effectiveness" of mathematics in science. Fictions are not as unreasonably effective as mathematics, but their effectiveness does raise questions. Various papers in this collection also link questions about scientific modeling to questions about the psychological faculty of imagination. In this paper I'll offer – eventually – a way of thinking about the role of fictions in model-building. I'll get there by a road that visits several other proposals that have been offered on this topic, and also embeds scientific modeling within a fragmentary but general treatment of fiction, possibility, and the imaginary.

2. The Imaginary

A set of central cases pose the problems clearly. In these cases, scientists want to understand a particular complex system, and they make what seems to be a detour, and investigate a simplified analogue of their target. An example used by Weisberg (2007, 2013) is Vito Volterra's attempt to understand fish populations in the Mediterranean. The work starts with a real system (a sea), populated by unproblematic entities (fish), but the system is intractably complicated, and that motivates the detour. There's an apparent similarity between cases like this and those where the "model" is a physical object – a scale-model such as the San Francisco Bay model (also used by Weisberg). In one case the simplified analogue is built; in the other the model is merely imagined.

This practice is related to several others. Salis and Frigg, in this collection, discuss Galileo's remarkably effective use of *thought-experiments*. Thought-experiment need not involve a path through the deliberately fictional; it may be concerned merely to bracket questions of actuality – if what is imaged turns out to be real, that would be fine. Imagination can function in the service of "direct" representation; a scientist freely imagines, but the goal is literal truth.[4] The situation is different when what is imagined is known to be non-actual, but remains scientifically useful despite this. Salis and Frigg use the example of Maxwell, who made use of an "imaginary" fluid in his study of lines of force. Maxwell's discussion makes explicit the fact that he does not see his construct as a

[4] "Direct" here does not mean entirely unmediated, or infallible. It just means: not mediated by a deliberate introduction of fiction.

"hypothetical" fluid, but as "merely a collection of imaginary properties" – Maxwell distinguishes the hypothetical (but perhaps actual) from the avowedly fictional.

Maxwell also says this side of his work is merely "heuristic." But some constructs of this kind are not merely heuristic, in any ordinary sense of that term. Consider large-scale climate models, and the like. Here, idealized models appear to be a central element in theorizing, a large part of our means for theoretical understanding.[5]

As I have set things up, the detour is a choice, something optional. It is possible to reach a related view by saying that *all* scientific work has, and must have, some of this character. That is the view expressed in the Ramsey quote I used as my epigraph – "The logical simplicity characteristic of the relations dealt with in a science is never attained by nature alone without any admixture of fiction." One tradition holds that manageable scientific ideas *must* have certain features, including simplicity and unity in forms the world may be reluctant to furnish. (That is a theme in Kuhn 1962, though it can be seen as going back to Kant). If the world is that way, and the scientist recognizes that it is, then an attitude like Ramsey's is motivated. But Ramsey's "never" is something I deny, and I'll assume in this paper that the description of fictional analogues of real systems is an optional matter, one scientific strategy among several.

The style of work I invoked above – "Imagine a population..." – is one category of cases within model-based science, but there are others. There is also a style of work in which mathematical structure itself is central. The first move is not imagining a physical system but specifying a mathematical structure – a space, parameters, functions – and then giving an interpretation of this structure that relates it to a real system. The "semantic view of theories," in some forms, was an account of science that made this style of work central, and tried to understand other kinds of theorizing in those terms (Suppes 1960). Instead, I think this kind of work is real and somewhat different from modeling that makes use of imagined economies, populations, and planets. Work that moves directly to consideration of a mathematical structure need not have significant dealings with fictions. The philosophical problems it poses are instead related to Platonism and the status of mathematical objects.

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[5] For the case of climate models, see Parker (2014) and Katzav and Parker (2015).

That suggests a view in which there are two distinct phenomena – the scientific use of imaginary systems, and the scientific use of apparently free-standing mathematical objects – with distinct metaphysical questions arising in each case. I think the situation is more complicated, as each practice shades into the other. The "imagined" systems that matter in science are usually not single systems but classes of them, related by their mathematical properties. Modeling involves a lot of *semi-schematic* imagining, moving between more abstract and more quasi-concrete structures. In this paper I am concerned mostly with the style of work in which imaginary systems are evident features of the practice.

One option that has been explored is the view that a model is a representation – not just a structure that might be used as one, but one that has, as an inherent feature, a key to its interpretation. This is the view that guides Yablo's paper in this collection. Yablo acknowledges Frigg's view and treats it as similar – models "not only represent their target; they do so in a clearly specifiable and unambiguous way" (Frigg quoted by Yablo, this collection).[6] This can be distinguished from a situation in which a model system is a resource with which one can do as one chooses, and has no natural or "unambiguous" interpretation. I think model systems do not carry with them their interpretations; they *are* "ambiguous" in that sense. This issue may be partly terminological – "model" might refer to a model system plus something that determines its interpretation. Either way, I think progress has been made by distinguishing between model systems and the commentaries that bring them to bear on empirical systems – what Giere (1988) called "theoretical hypotheses" and I called "construals" (2006). One kind of work is exploring the model system itself – this is true of physical scale models, imaginary systems, and purely mathematical structures – and a separate activity is working out what the structure might tell us about empirical goings-on. Models are often

[6] I think that Frigg's view is different on this point, though the passage Yablo quotes does support what he attributes to Frigg. I am not sure how some parts of his view fit together. Frigg says "Taking model-systems to be intrinsically representational is a fundamental mistake. Model-systems, first and foremost are objects of sorts, which can, and de facto often are, used as representations of a target-system" (2010c, p. 99). He also says scientific "[m]odels not only represent their target; they do so in a clearly specifiable and unambiguous way" (2010b, p. 275). I think that if models are not inherently representational, they are not "unambiguous."

developed in one domain and used as a resource in another. Spin-glass models were taken from physics into biology. Game theory went from economics into biology – and then went back to economics again, after it had been evolutionized by biologists. Places like the Santa Fe Institute specialize in encouraging work of this kind – formal but multi-discplinary, with a continual eye to the exchange of models between fields.

As Weisberg discusses (in this collection), computer models raise special problems of interpretation: which aspects of the computational processing are to be treated in a representational way, and which are to be regarded just as how a set of procedures are being physically implemented?

These questions about interpretation have connections to the more vexed ontological questions about modeling. Some views oppose the idea of model systems as *objects* – they oppose this even as the right way to see things *prima facie*, at face-value. Modelers, on this view, do not talk *about* model systems that are distinct from real-world targets, but merely engage in make-believe about real-world systems. I think views of that kind have an element of truth, and I'll discuss them below, but I think they have to massage the phenomena quite a lot. It is partly the *actual* reification of model systems by scientists – who write as if constructing a model is one thing, and determining its application is another – that contributes to the putative reifiability of model systems in a philosophical context.

Suppose we do take seriously the apparent treatment of model systems as objects, as topics of discussion. What sort of thing are those systems *taken* to be? There are two standard options, one of which acts mostly as a foil in discussion. That one is the view that model systems might be non-actual but real and concrete entities, in the style of Lewis (1986). The other option is that they are abstract objects like numbers and other mathematical entities. Those views need not exhaust the options – "abstract artifacts" of the sort described by Thomasson do not have the familiar features of abstract objects like numbers (such as necessary existence). I will discuss Thomasson's view shortly. First I want to add something to the list of options. I think that *the imaginary* is a folk-ontological category, somewhat different from the categories philosophers usually work with. If something is imaginary, then it is not actual and concrete, but the imaginary is not merely the possible; it has a made-up or constructed side to it. Everyday thinking has it that imaginary things are contingent products of human activity (they have to be imagined to get their imaginary status), but they are not abstract in the way numbers and sets are. They are unsuccessful candidates for concreteness, and this makes them different from things that were never countenanced at all, and also different from abstract objects. Imaginary things are candidates for occupying space, though they don't actually do so, and candidates for entering into causal relations with real objects. So they are not like numbers, which could not occupy space, and not like the occupants of Lewisian possible worlds, which don't have to be thought up. Once imagined, they can be talked about by many different people. (Many of the quotations from scientific modelers used by Salis and Frigg in their chapter in this collection strongly suggest a conception of the imaginary like this.)

You might say at this point that "the imaginary" in this sense is a dubious ontological category, one that falls between the good ones: if things are real but not in space, then they are abstract and should be like numbers. In that case, they are not contingent products of human action. I agree that imaginary things are dubious in this sense. I am trying to capture a *folk*-ontological category, not a professional-ontological one. The idea is indeed a bit of a mess. But I think the way people think and talk involves a mild commitment to this category despite its instability. Metaphysicians spend their time coming up with ontological categories that are better-behaved.

I think that the objects described in literary fictions are imaginary also. This claim, again, does not determine how those fictions will be treated in a fully-considered, non-folk ontology. Noting this role for the imaginary is just a first step.

Some years ago Ron Giere entitled a paper "Why Scientific Models Should Not be Regarded as Works of Fiction" (2008). The paper was a response to the literature linking scientific models with fiction. Several people, including me, who had been influenced by Giere's 1988 book thought that, although Giere did not put things in these terms, the only way to make sense of what he said was to see Giere's "model systems" as fictional objects (see Thompson-Jones 2010 and Thomasson's paper in this collection for these arguments). Giere said, in the title of his 2009 paper, that scientific models should *not* be regarded as works of fiction. In the paper itself, though, he said this:

> It is widely assumed that a work of fiction is a creation of human imagination. I think the same is true of scientific models. So, ontologically, scientific models and works of fiction are on a par. They are both imaginary constructs. (p. 249).

Giere goes on to say: "In spite of sharing an ontology as imagined objects, scientific models and works of fiction function in different cultural worlds" (p. 251). This talk of "cultural worlds" is a picturesque way of saying that they have different roles in our lives. Of course they have different roles; no one had suggested otherwise. Literary fictions function in recreation, in artistic endeavour, and allegorical exposition; scientific models are part of attempt to understand the workings of natural systems. The question being grappled with in this literature is about the "ontological status" of model systems. On this point, Giere in his 2008 paper endorses the same view as some of the authors he said he was opposing. Giere said he does not want models-as-fictions talk to help anti-scientific movements, such as creationism and postmodernism. That is understandable, and helps explain the tension in Giere's discussion. But it is true, and Giere accepts it as true, that modeling makes use of imaginary constructs, and somehow this does help us understand the actual world.

3. Make-Believe, Abstract Artifacts, and Easy Ontology

I'll look now at two families of ideas developed in response to these questions, and will offer some argments against both. The arguments are not intended to be decisive, but they help motivate my positive view. I'll be looking mostly at proposals due to Thomasson and Levy, with input also from Frigg and Toon. All make use of a notion of *make-believe*, influential within theories of fiction elsewhere in philosophy, and they augment this idea in different ways.

The idea of make-believe is central to Kendall Walton's account of fiction (1990). Make-believe is a psychological attitude that (in some forms) is part of a social practice guided by "props" that writers and other artists produce. Acts of make-belief are constrained by "principles of generation" associated with the practice. This analysis aims to avoid the wrong kind of reification of fictional objects; the text of *Hamlet* should not be taken to describe a "nonexistent or abstract" prince. Instead, it should be seen as "enjoining us to make-believe" that there was such a prince (Thomasson, this volume, p. tk*). The same view can be applied to modeling in science; we pretend that there is a system with such-and-such features. And then: "for a statement *p* about what is the case within a model system to be true, is for the model-description together with the relevant principles of generation to prescribe *p* as to be imagined" (Frigg 2010a, p. 262).

This is, so far, a partial account that does not address several topics, such as comparisons scientists make between model systems and real-world targets. I'll look at two views that augment this first move, developed by Thomasson and Levy.[7]

Thomasson holds that make-believe gives a good analysis of talk that is "internal" to model-building, but works less well for talk that compares models with real systems. This, she says, parallels the situation with literary fictions. She uses the term "pure pretense" for views that use a psychological state of make-believe to analyze both talk within a fiction *and* "external" talk about fictions, such as talk about fiction/world relations. She thinks that make-believe does not a good job of making sense of external discourse in this sense, and "well-known problems with pure pretense views of fiction carry over to the parallel views of models" (p. tk*).

Responding to the case of literary fictions, Thomasson developed a view that treats fictional objects as *abstract artifacts*. Make-believe is the first step in fictionalizing, but once socially organized make-believe has been undertaken, it can be seen as giving rise to an abstract artifact that can be talked about as an object. The idea of an abstract artifact she sees as independently motivated; laws (not scientific laws, but positive, societal ones) and symphonies are also abstract artifacts.[8] Recognition of abstract artifacts enables a "far more straightforward account of external historical, theoretical, and critical discourse" – about literary fictions and scientific models alike.

Thomasson thinks that it is only general ontological qualms that might make us reluctant to say that model systems are real in this way. She argues that abstract artifacts are not ontologically costly, either in scientific or literary cases: "What more should one

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[7] I use the terms "pretence" and "make-believe" interchangeably.

[8] Thomasson: "[A]bstract artifacts are extremely commonplace. Entities such as theories, stories, laws of state, symphonies, etc. all seem best understood as abstract artifacts. If you are prepared to accept that we refer to any of these, then there should be no barrier to accepting fictional characters and model systems—considered as abstract artifacts."

think it would take for a model-system to be created, than for scientists to engage in certain kinds of modeling activities and to provide certain model descriptions?" (this volume, p. tk*).

My reservations about this view don't primarily involve ontological costs. They have to do with what role abstract artifacts can play in making sense of modeling, with how much they might explain. I'll start with a general idea. I think there are at least two roles that talk of objects (object-talk) can play in a description of a practice of this kind, a minimal role and a richer one. The minimal role is one where objects function as little more than points of intersection points in a certain kind in pattern of talk. Participants in a practice (perhaps novelists and readers, mathematicians, scientists) use nouns in a certain way, either casually or on the basis of some kind of real commitment, and we as commentators follow them, but do so in a deliberately low-key spirit. We don't think (or necessarily deny) that if we were constructing a pattern of first-order talk in this area from scratch, we would use nouns in the same way they do. Once playwrights and actors talk of Hamlet or the ideal pendulum as a *thing*, we follow them. (I think this is close to what Thomasson in other work has called the "covering" use of object-talk: 2009.)

The richer kind of object-talk that commentators might engage in is one in which we are commited to objects as a certain kind of source of *constraint* on what is said within the practice. We take them to be targets of the talk, that can somehow constrain what is said. This talk of "constraint" is not merely a relabeling of constraints that are internal to the practice. To talk of Hamlet as an object is not to see that object as exerting any constraint over what is said about him; more exactly, such an object exerts no constraint over and above those inherent in the pattern of talk itself. Ordinary physical objects, on the other hand, are sources of constraint in their own right. They affect what we say about them in ways that outrun decisions we might make internally to the practice. Mathematical objects are also supposed to be a bit like this – at least, many would say so, and Platonism is an expression of that attitude. Mathematical objects, such as the real numbers, seem to be a source of constraint. This may be an illusion, or something that invites a revisionary analysis (see below, section 4), but the *prima facie* case is there.

What about imaginary systems of the kind that figure in modeling? The problem might be expressed by saying they seem a bit like Hamlet, but also a bit like numbers, and the category of abstract artifact only captures their Hamlet-like role, their role as mere points of intersection in practices of talking and imagining.

My reservations about what abstract artifacts contribute can be illustrated by looking more closely at one kind of "external" discourse about models, the kind that involves comparisons of model systems to empirical targets. In his 2010a paper, Frigg moves some distance away from a "pure" pretence view (in Thomasson's sense) in order to make sense of these comparisons. Frigg says that talk of model/world matches involves comparison of properties. Building a model puts on the table certain properties, and a good model puts on the table properties similar to those actually possessed by the target. To talk about the empirical value of a model (over and above its predictive role) is to talk about this relation between properties.

I objected (2009) to Frigg that the realism about nowhere-instantiated properties this approach requires is not so much different from realism about non-actual objects. Thomasson, on behalf of Frigg, responds by defending "easy ontology" for nowhere-instantiated properties. She does not say that these properties are abstract artifacts, or simular; she thinks such properties are real for different reasons. They have a direct licensing (by way of negative predications) that is not applicable in the case of non-actual objects. I am not saying this move fails – perhaps it is fine. But it does show how little is being done by the abstract artifacts themselves. If the most important "external" questions about models are those that involve world-target relations, then abstract artifacts are not playing much of a role. The move Frigg made did not require introduction of abstract artifacts, and Thomasson's development of Frigg's view does not employ them either.

Thomasson says that abstract artifacts are not very demanding as posits. I reply that this may be fine, but the most important problems with "external" discourse about models do not seem to be helped much by them. We don't pay much to introduce them, and don't seem to gain much either. When I say this, I assume that the most important "external" talk about scientific models is talk about model/world relations. In the case of literary fictions, those kinds of comparisons are not usually seen as especially important, certainly no more important than talk about the history of a literary work and its relations to other works. As Arnon Levy noted in comments on a draft of this chapter, abstract artifacts do seem to help us handle those other kinds of external talk, in scientific as well as literary cases.

A different approach to all these questions is to be firmer on the worldly side ("hard" as opposed to "easy" ontology) and then be prepared for a complicated story about how the practice relates to the world. That is the approach I'll take in the next section.

A different view that uses make-believe in company with extra arguments has been defended by Levy (2015). He thinks a wrong move was made early on, when people accepted the face-value appearance that model systems are distinct from their targets. Instead, models are "directly about the world."

> 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. (2015, p. 791).

The make-believe a modeler engages in is always directed on real-world objects, and the fictional side of modeling is imagining modifications to those objects. There are no extra objects we need to make sense of; there are only unusual things being said about ordinary target systems.

As Levy notes, Adam Toon (2012) has developed a similar view. This approach is in a sense of the opposite of Thomasson's. She thinks it is important to recognize that model systems are additional objects that people can talk about, and argues that this is not as ontologically costly as people have thought. Levy and Toon think there is no need to recognize such objects at all, whether they are cheap or expensive.

Let's move immediately to the hard questions about the utility of modeling. For Levy, models are not merely predictive instruments. They have a descriptive goal that includes truth. You can compare what you are prescribed to *imagine* about a target with what you may consider *believing* about the same thing. Model descriptions, because of idealization, are not in the simplest sense true of their targets, but they can be "partly true" of them: "Partial truth (of a sentence, or a collection of sentences) is... understood as truth of a part (of the sentence)" (p. 792). Levy's position draws on Yablo (2014), who develops the view further in his paper in this collection. The case of numbers provides a model:

> "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*. (Levy 2015, pp. 792-93)

To find the partial truth in a description, we subtract some of what it says. Similarly, what we learn from a model can be seen by setting aside its idealizing features. The ideal gas model, for example, can be seen as saying some *true things about real gases*, as well as saying some false things (because of its simplifying assumptions).

This is not an account in terms of *approximate* truth, and Levy explicitly distances himself from that path.[9] Below, I'll give a treatment that does make approximation central.

For Levy, you engage in make-believe about a target system, and you are enabled by this exercise to say some things that are partly true. Levy's is an attempt at a very parsimonious view. Its downside is that it insists that all cases fit a certain mold even when they seem not to. There seems to be modeling that is not about actual systems. And many modeling practices seem to be more *indirect*; a model system is explored first, in its own right, and comparisons are made to targets later. I think that Levy might want to cut those cases loose, as ultimately not entirely coherent, and say that his account captures the part of the practice that works. I will develop my own view next and then compare it further with Levy's.

[9] Levy: "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)" (2015, p. 793).

4. Models, Conditionals, and Truth

This section will outline a positive view of scientific modeling. I'll approach it by way of a general picture of the imagination and modal thinking, and will also return to the case of mathematics at the end.

I begin by taking a practical and psychological approach to the idea of possibility. Plausibly, the idea of possibility has a primitive association with action: the world at large determines how things *are*; we determine what to *do*, and in these episodes we take ourselves to choose from possibilities. From there, a sense of possibility projects backwards and sideways. We see other events, including past events, as embedded in a cloud of ways-things-might-have-been.[10] (As Alison Gopnik says, regret is the evolutionary price paid for planning.)

Action gives us the idea of possibility, and also an accompanying idea of dependence: *if I do this, things will go like that*. The forward-models used in planning can also be applied to testing (if I do this, I expect things to look like that – unless I am wrong...). The sense of possibility thus gains an epistemic role.

On top of this deep-rooted family of skills of useful modalizing, additions are overlain, dependent on language and the social organization of science. In a central class of scientific cases, you specify a set-up and try to see what follows. In work of this kind, computers are now a powerful aid to the scientific imagination, as they enable complex dependence relationships to be broken down into many smaller ones. Large numbers of small and well-understood procedures can be combined to enable us to imaginatively explore a larger system.

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[10] This modal orientation linked to action might be neurobiologically deep and seen outside of humans. Neurobiological work on internal "spatial maps" in rats has progressed so far that it is possible to read some things off their neural activity, and one study reports that as rats make a spatial decision, they activate a collection of neural paths that sweep ahead of the animal's representation of its current position, running "first down one path and then the other," apparently representing future possibilities (Johnson and Redish 2007).

It's interesting to think in this context about the neglect of ordinary action in Quine, foe of modality.

In a wide range of cases, the output of a piece of scientific modeling can be expressed as a *conditional*, a statement of the form: if *A*, then *C* (or a collection of these conditionals).[11] The usphot of modeling need not always be stated in this form, but I think it is generally pretty close to the surface. Philosophy recognizes several kinds of conditionals. *Material* conditionals are simple: "if *A* then *C*" is treated as equivalent to "either *C* or not *A*." The others, including how many others there are, are more controversial. An example is the *indicative* conditional: "if it rains, the show will be called off." Is that conditional truth-functional, and if so, is it a material conditional (true in any case where it does not rain, as well as any case where the show is called off)? More overtly problematic are *subjunctive* conditionals:

> If it were to rain, the show would be called off.
> If Jones had taken arsenic, he'd show these symptoms.

When a subjunctive conditional has an antecedent that is known or assumed false, the result is a *counterfactual* conditional.

> If it had rained, the show would have been called off.
> If Oswald had not killed Kennedy, someone else would have.

Unlike the Kennedy and Nixon cases often used in discussions of counterfactuals, the conditionals generated by model-building often have generalities in antecedent and consequent: if there was a set-up like this, it would do that. This might be expressed instead as: *Any* setup like this would do that. Now it looks like a statement of a law (or a "law in situ": Millikan 1984). You might then simply say that modeling yields laws, except that natural laws are not supposed to have false (never satisfied) antecedents. A law with a false antecedent could be said to apply in the actual world if it was a material conditional, but then it would not tell you want to expect on the consequent side.[12]

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[11] A first sketch of this view is in Godfrey-Smith (2014).

[12] This is a way of expressing some of Cartwright's (1983) insights.

Conditionals are both philosophically controversial and practically indispensible, especially in planning and determining responsibility. As I said, the products of modeling can often be seen as conditionals. Re-expressing some familiar cases:

> *If* a pair of populations had features *F*, *then* it would have/do *G* (Lotka-Volterra behaviors).
> *If* an object had the features of an ideal pendulum, *then* it would do this...

These look like counterfactual conditionals. They are subjunctive with respect to the kind of link asserted between antecedent and consequent, and their antecedents specify arrangements that are assumed to be never realized in the actual world. Not all cases need fit this pattern – you might model without knowing whether the antecedent is satisfied. The use to which these products are applied also vary. You might want to make predictions for use in guiding behavior (if you are confident in the model), or for testing (see what happens and give the model credit or blame). I'll focus here on cases where the modeler's aim is working out what a system will do.

In describing the scientific role of these conditionals, there are two features to consider. One is the status of the conditional itself – whether it is true, for example. The other is the conditional's relation to actual events, something that depends on both the status of the conditional and on whether the conditions specified in the antecedent actually obtain. I'll come to the status of the conditionals in a moment. Assume for the moment that some of them are true. In the case of antecedents, I'll talk of *satisfaction* as the truth-like feature that some of them have (this is supposed to be more general than truth, as antecedents need not be propositions). Let's assume that the antecedent of some particular conditional is never satisfied. How could such a conditional help us with the actual world? If conditionals of this kind are the exported products of modeling, how can they bear on real-world targets? The key is approximation. Suppose a model is built and a conditional is extracted at the end. The antecedent of the conditional specifies all the assumptions that went into the model, and this is a fictional set-up. The condition in the antecedent is fictional, but you might think it is *close* to situations that do actually obtain. Then we can tell a possible story about utility, by way of the idea of approximation.

I'll work through what I take to be a typical sort of case. By modeling you learn: *if A then C*. ("*A"* below is antecedent, "*C"* is consequent.)[13] You also know, of a target system: *approximately A*. That is, there is approximate satisfaction of the antecedent. In this paper I won't say more about what that amounts to. Suppose you know *if A then C* and *approximately A*. Can you say: *if approximately A, then C?* No, as there are many cases where only *A* suffices (consider a credit card number). How about: *if approximately A, then approximately C?* This is still not OK in general, even if "approximately *C*" makes sense. But sometimes you do have reason to move from "If *A* then *C*" to the version with "approximately" on both sides. Being able to do this in the right cases is an important skill. *If* this object approximates a simple pendulum, then its swinging will have an approximately constant frequency (as long as the amplitude is small). Sometimes slight variation in *A* leads to slight variation in *C*. Then you can use the conditional as a guide to behavior. You can't derive the exact behavior of the real, very complex set-up, but you can work out the behavior of a simpler analogue, and that can be a guide.

How much can we say about when it is OK to infer from "If *A* then *C*" to "If approximately *A*, then approximately *C*?" Particularities and case-specific skills probably rule to a large extent, but they are not the whole story. "Robustness analysis," often discussed in connection with model-based science, has some of its utility explained here.[14] If you can show that *many* models that make different idealizing assumptions all lead to the same outcome, exactly or approximately, that can be very valuable. If an outcome is robust over many different antecedents, all of which simplify actuality differently, then the outcome might be found (at least approximately) in the actual world as well. (We learn that *if A then C*, and *if A* then C*, and *if A** then C*... where *C* is coarse-grained, not too detailed). The "spread" achieved over a range of antecedents may be such as to suggest that the actual world is included in the set of worlds that leads to *C*.

In other cases you might not need robustness in this sense. You might have some other basis for going from the subjunctive to something directly useable. Either way, a characteristic sequence of steps may run like this:

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[13] I think (2006) that formal analysis of a model system can only lead us to a conditional of this kind in the context of a *construal*. In this discussion I assume that a construal has either explicitly or implicitly been introduced.

[14] See Levins (1966), Weisberg and Reisman (2008).

1. *If A then C* (subjunctive conditional with an unsatisfied antecendent, hence a counterfactual, determined by modeling).
2. *If approximately A, then approximately C* (also a subjunctive, inferred invalidly but perhaps reasonably from 1).
3. *Either approximately A does not hold, or approximately C* (a material conditional, or perhaps something a bit more complicated – Edgington 2014 – but something aimed squarely at the actual, and as dependent as possible on truth-functional features. From 2).
4. *Approximately A* (via other information).
5. *Approximately C* (a conclusion about the actual world, from 3 and 4).

I think this sequence is often a useful one to walk down, even though some of its steps are not deductively valid.[15]

What is the status of the counterfactual conditionals at the top? Are they *true*, when all goes well? I don't think that is a required part of the account. It should be possible to have a view along lines I am describing which includes a nonfactualist treatment of counterfactuals. I won't try to resolve these questions here, but will briefly sketch some ideas.

A great deal of ingenious work has tried to find rules that assign truth-conditions to familiar kinds of counterfactuals, and does so such a way that the most uncontroversial counterfactuals come out as true. This has proven a difficult task. For example, Hájek (forthcoming) argues that familiar counterfactual conditionals are almost all false. "If *A* then *C*" can always be defeated by "If *A,* it *might* not be that *C*." The role of probability in physics generally tells us that the "might" claim is true, so the counterfactual is false, strictly speaking. Hájek thinks that false counterfactuals can still be useful, and in many cases they can also be carefully re-expressed (perhaps probabilistically) to yield a truth. I think considerations like these suggest that nonfactualism about counterfactuals may be appropriate: there are good and bad counterfactuals, but truth might be the wrong thing to ask for. There is reason to reconsider approaches like the one developed by John Mackie

[15] Note also that this sort of chain can be expressed with "approximately *C*" or just a weak and coarse-grained "*C*."

(1974), an approach regrettably sidelined by the elegant and more technical work of Lewis and Stalnaker around the same time. Mackie thought that in counterfactual thinking we stipulate set-ups, mentally and/or verbally, with varying degrees of detail, and try to run the scenarios forward, in a way guided by what we take to be laws and other true generalizations. The results of this exercise are often useful, and some counterfactual claims are better than others, but they are not determinately true or false. Our sketches of the initial conditions are never complete enough for a particular outcome to be guaranteed. Hájek's argument makes this thought more precise.

You might object that this view only applies to informal counterfactual thinking, and the use of mathematics and computers ensures that the conditionals established by modelers have a tighter connection between *A* and *C*. I think this thought is partly right but not in a way that makes a difference to the essential features of the situation. Though in modeling the aim is (sometimes) to push conditional claims as far as possible towards a situation where the relation between antecedent and consequent is mathematically guaranteed, in the kind of modeling we're talking about here – a kind where the product is a conditional about a physical system – it's not (ever? generally?) possible to get all the way there. Consider a very simple case. "7 + 5 = 12" is mathematically necessary, but "if you put seven marbles on a table and add five there will be twelve marbles on the table" is not mathematically necessary (and the problem is not fixable by means of a consequent that is probabilistic). Whether you will find yourself with 12 marbles depends on the physical characteristics of marbles and tables, and initial conditions. The same applies to conditionals about what will happen in an ecological system where a certain kind of predator eats a certain kind of prey. Mathematics may be where the work is done, but the conditionals that result are not merely mathematical statements. They are statements about physical systems, dependent on physical regularities and the details of initial conditions.

These ideas about counterfactuals are not essential to the view of modeling defended here. The story would easier to develop if the goodness of a counterfactual was a matter of simple truth.

How does my position relate to Levy's view, discussed in the previous section? One difference is my use of approximation rather than partial truth – truth with respect to some of what is said. There might be some hidden equivalence between the two approaches, but I think approximation is an important and somewhat neglected element in this area. The role of approximation is also important in consideration of scientific progress. Many claims made on the basis of old theories and models are better seen as approximately true (close to the truth) than as saying literally true things about *part* of what they describe (McMullen 1984, and see below). The role given to approximation also seems to better capture the role of assessments of model/world similarity within the face-value practice of modeling. The view I've outlined also does not make much of the difference between modeling-talk that is aimed at a particular target and modeling-talk that is not. A person might say, "If the Adriatic Sea contained just two species of fish...." That is a Levy-Toon-style antecedent. But they could also just say: "If there was a sea like this..." and build a similar model. Target systems as objects of make-believe need not be in the picture at the model-building stage.

I'll mention a few other connections. Suárez (2009) has developed a view in which models are tools for inference, and the specific role for fictional assumptions in models is to furnish us with conditional statements. He does not make use of approximation in the way I did above, but this might be added to his view. Bokulich (2011) has argued that the way models explain is by being a basis for counterfactuals: models and targets must have isomorphic counterfactual structure. This seems a strong requirement, perhaps appropriately loosened with an appeal to approximation. Bokulich also discusses McMullin, and says that her view draws on his. McMullin was indeed on the right track; Bokulich gives this quote:

> The fact that the Bohr model worked out so remarkably indicates that the structure it postulated for the H atom had some sort of approximate basis in the real. . . . Later QM would modify this simple model in all sorts of fundamental ways. But a careful consideration of the *history* of the model... strongly suggests that the guidance it gave to theoretical research in quantum mechanics for an immensely fruitful fifteen years must ultimately have derived from a "fit" of some sort, however complex and however loose it may have been, between the model and the structure of the real it so successfully explained. (McMullin 1968, p. 396)

My view is intended to be not far from this.

I'll also say more about the analogy to mathematics, especially the problems of ontology and efficacy seen in that older and more famous case. In mathematics, a Platonist attitude is often expressed by practitioners – I accept that as a sociological fact. The puzzles this attitude raises are made more acute by the "unreasonable effectiveness" of mathematics as a tool for dealing with the world. I see the analogy with imagined-system modeling – more than an analogy – as follows.

In both cases there is a constrained inference practice, prompted by real-world questions (questions about magnitudes and counts in the case of mathematics, questions about the tendencies of complex systems in the case of modeling). The inferential practice gives rise to claims whose fit to their targets is not straightforward in some respects (abstraction, idealization). The practice also comes to include reifying moves – there comes to be talk of *objects* of a special kind, distinct from concrete actual objects (numbers, imaginary systems). There is then an expansion of the inferential practice in this light of this reification (an expansion much more elaborate in the mathematical case).

As an outsider, one might then try to explain what is going on. You might argue that the Platonic mathematical objects or imaginary systems have a kind of reality that fits the ways these things are talked about within the practice, or you might give a more revisionary treatment. That revisionary treatment may be one that takes the story back to concrete actual-world objects, where it all started.

A sample theory of this kind in the case of mathematics is *nominalist structuralism*. The philosophy of mathematics is an intricate field, and I am no expert. I introduce nominalist structuralism here mostly for the way it illustrates a strategy of analysis.

Nominalist structuralism denies that there are any special mathematical objects. Mathematical claims are claims about concrete, actual-world structures. When you assert mathematical propositions, you say things of this form: any system with property F has property G as well. Following the sketch given by Horsten (2016): If p is a sentence within real analysis, and RA are the principles of real analysis, the content of p is (roughly) given by "Every concrete system S that makes RA true, also makes p true."

All that is recognized in this interpretation are concrete systems (and the problems with this view partly concern whether concrete systems of the right kind really exist).

There are no mathematical objects, even though mathematicians routinely talk as if there were, saying "there is an infinite number of primes," and so on. The face-value practice is object-based and Platonist, but the outside observer can look at mathematical claims differently. Mathematicians are Platonists, but *we* don't have to be Platonists, when we describe what they do and what they achieve.

Similarly, modelers are a bit like modal realists – or modal realists *and* Platonists, given what I said about more purely mathematical modeling in section 2 of this paper. Modelers think that imaginary systems can be topics of discussion, and assessed for similarity to non-imaginary systems.[16] But the outside observer can give an interpretation of what is going on, and what is achieved, that is different. An example is the interpretation I gave above using subjunctive conditionals and approximation.

The two cases have interesting similarities and differences. Both have a role for conditionals. That role is somewhat hidden in the mathematics case (assuming nominalist structuralism is true), and only barely hidden in the modeling case. In the modeling case there is a role for idealization and approximation, and some of the conditionals might lack truth values. In the mathematics case, the antecedents of the crucial conditions are supposed to be satisfied, and there is not supposed (at least at this first stage) to be a role for approximation, though an account of how mathematics usefully applies to actual systems might include this. In both cases, the practice is admitted to include *reifying moves*, moves that commit to special objects, and there is a structuralist explanation of why the practice works despite the nonexistence of the objects thereby introduced.

In a sketch given by John Burgess (2005) of how nominalist structuralism fits into a larger picture, real numbers are initially seen in a physically grounded way – Newton said that he saw them as "abstracted ratios" of magnitudes such as lengths, and Burgess says this view was common. As mathematical practice matures, it comes to include reification – there is *the* ordered field of real numbers, **R**. But what is this thing and how could it help us? Thus the nominalist structuralist interpretation: there is no such thing, and we should re-express mathematical claims conditionally. The case of model-based science is less elaborate and the reifying moves are less integrated. Similarly, though, the

[16] When I have engaged in modeling of the relevant kind, mostly in collaboration with Ben Kerr and Manolo Martínez, I have certainly thought that way.

story starts with reasoning about tendencies in complex systems with the aid of imagined simplifications, and then there comes to be reification: I am studying *the* two-locus system with inbreeding, or the ideal pendulum. We can then look on, from the outside, and tell a story in terms of conditionals and approximation, a story in which imaginary systems do not figure.

I can also make a connection here to my earlier discussion of objects, in response to Thomasson. I distinguished between object-talk of a minimal kind and a richer kind. In the richer kind, objects are seen as sources of constraint. I am denying the *second* role for model systems here, just as the nominalist structuralist does for mathematical objects. If someone wants to hold onto talk of model systems or numbers as objects in the more minimal way, then I don't think this has to be resisted.

5. Conclusion

The imagination is a psychological faculty with several roles – a role seen in planning, a recreational role, and a set of epistemic roles that include a place in science. The scientific role for the imagination, in turn, includes at least three aspects: (i) consideration of epistemic possibilities – ways things might be, (ii) modal organization of the actual with the possible (state spaces, etc.), and (iii) consideration of tractable analogues of complex real systems in model-based science.

In modeling of the kind discussed here, we imagine scenarios – construct the *if* side of the conditional – and have to work out what would ensue, how the system would behave. A central difference between this scientific activity and recreational cases is that in science, there has to be a highly constrained way of working out what would follow from the set-up imaged. The aim is to set up the *if* side in a way that lends itself to the determination of what follows using methods of known reliability. Otherwise the model is just a game. If your *if* is an empirically irrelevant *if*, then the model also has reduced value as science... but we should not be too quick there, as what looks like an empirically unimportant *if* may become important later.

In scientific modeling, the face-value practice is also invested in imaginary systems as subjects of discussion. The view I have defended is one whose first step is to acknowledge the role played by *the imaginary* in the practice of modeling, but this view then gives an account of modeling that concedes no role to imaginary objects. Instead, the account involves a route from subjunctive conditionals, through approximation, to indicative conditionals that can be used to draw conclusions about empirical systems. Regarding the general shape of the explanation that results, I am encouraged by the mathematical case and the analogy with nominalist structuralism. In both cases, in mathematics and model-based science, these problems are ones to address with positive accounts, rather than matters to pass over quietly.

References

Bokulich, A (2011). How scientific models can explain. *Synthese* 180: 33–45.

Burgess, JP (2005). Review of Charles S. Chihara. *A Structural Account of Mathematics. Philosophia Mathematica* (III) 13: 78–113

Cartwright, N (1983). *How the Laws of Physics Lie.* Oxford: Oxford University Press.

Edgington D (2014). Indicative conditionals. *The Stanford Encyclopedia of Philosophy* (Winter 2014 Edition), Edward N. Zalta (ed.), URL = <https://plato.stanford.edu/archives/win2014/entries/conditionals/>.

Fine, A (2009). Science fictions: Comment on Godfrey-Smith. *Philosophical Studies* 143: 117–125.

Føllesdal, D (1986). "Essentialism and Reference," In L Hahn, PA Schilpp (eds.), *The Philosophy of W. V. Quine, Volume 18 (Library of Living Philosophers)*. Chicago: Open Court, pp. 97-113.

Frigg, R (2010a). Models and fiction. *Synthese* 172: 251–268.

Frigg, R (2010b). Fiction in science. In J Woods (ed.): *Fictions and Models: New Essays*. Munich: Philosophia Verlag, pp. 247-287.

Frigg, R (2010c). "Fiction and Scientific Representation," in *Beyond Mimesis and Nominalism: Representation in Art and Science* (R. Frigg and M. Hunter, eds.). Boston Studies in the Philosophy of Science. Berlin and New York: Springer, 2010, 97-138.

Giere, R (1988). *Explaining Science: A Cognitive Approach*. Chicago: Chicago University

Press.

Giere, R (2008). Why scientific models should not be regarded as works of fiction. In M. Suárez (ed.), *Fictions in Science: Philosophical Essays on Modeling and Idealization*. London: Routledge, pp. 248-258

Godfrey-Smith, P (2006). The strategy of model-based science. *Biology and Philosophy* 21: 725–740.

Godfrey-Smith, P (2009). Models and fictions in science. *Philosophical Studies* 143: 101-116.

Godfrey-Smith, P (2014). *Philosophy of Biology.* Princeton: Princeton University Press.

Harman, P (2001). *The Natural Philosophy of James Clerk Maxwell*. Cambridge: Cambridge University Press.

Hájek, A (forthcoming). Most counterfactuals are false.

Horsten, L. (2016). Philosophy of mathematics. *The Stanford Encyclopedia of Philosophy* (Summer 2016 Edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/sum2016/entries/philosophy-mathematics/>.

Johnson A and Redish AD (2007). Neural ensembles in CA3 transiently encode paths forward of the animal at a decision point. *Journal of Neuroscience* 27: 12176-89.

Katzav, J and Parker WS (21015). The future of climate modeling. *Climatic Change* 132: 475–487

Kuhn, TS (1962). *The Structure of Scientific Revolutions.* Chicago: University of Chicago Press.

Levins, R (1966). The strategy of model-building in population biology. *American Scientist* 54: 421–31

Levy, A (2013). Anchoring fictional models: A review of *Models as Make-Believe* by Adam Toon. *Biology and Philosophy* 28:693-701.

Levy, A (2015). Modeling without models. *Philosophical Studies* 172 (3): 781-798.

Lewis, D (1986). *On the Plurality of Worlds*. Oxford: Blackwell.

Mackie, J (1974). *The Cement of the Universe: A Study of Causation.* Oxford: Clarendon Press.

McMullin, E (1968).What do physical models tell us? In B. van Rootselaar & J. Staal

(Eds.), *Proceedings of the third international congress for logic, methodology and philosophy of science*, Amsterdam, 1967. Amsterdam: North Holland Publishing Co, pp. 385–396.

McMullen, E (1984). A case for scientific realism. In J. Leplin (ed.) *Scientific Realism*. Berkeley: University of California Press.

Millikan, RM (1984). *Language, Thought, and Other Biological Categories*. Cambridge MA: MIT Press.

Parker WS (2014). Simulation and understanding in the study of weather and climate. *Perspectives on Science* 22: 336-357.

Quine, WV (1960) *Word and Object.* Cambridge MA: MIT Press.

Ramsey, F (1926). Truth and probability. In Ramsey *The Foundations of Mathematics and other Logical Essays*, Ch. VII, 1931, edited by R.B. Braithwaite, London: Kegan, Paul, Trench, Trubner & Co, pp.156-198.

Salis, F and Frigg R (this volume). Capturing the scientific imagination.

Suárez, M. (2009), Scientific fictions as rules of inference. In M. Suárez (ed.), *Fictions in Science: Philosophical Essays on Modeling and Idealization*. London: Routledge, pp. 158-178.

Suppes, P (1960) A comparison of the meaning and uses of models in mathematics and the empirical sciences. *Synthese* 12:287–301.

Thompson-Jones, M (2010). Missing systems and the face-value practice. *Synthese* 172: 283.

Thomasson, A (2009). Answerable and unanswerable questions. In D Chalmers, D Manley & R Wasserman (eds.), *Metametaphysics: New Essays on the Foundations of Ontology.* Oxford: Oxford University Press.

Thomasson, A (this volume). If models were fictions, then what would they be?

Toon, A (2012). *Models as Make-Believe*. London: Palgrave-Macmillan.

Walton, K (1990). *Mimesis as Make-Believe*: *On the Foundations of the Representational Arts*. Cambridge MA: Harvard University Press.

Wigner, E (1967). The unreasonable effectiveness of mathematics in the natural sciences. In *Symmetries and Reflections: Scientific Essays.* Bloomington: Indiana University Press, pp. 222–237.

Williamson (forthcoming a). Modal science. To appear in the *Canadian Journal of Philosophy*.

Williamson, T (forthcoming b). Objective possibilities.

Weisberg, M (2007). Who is a modeler? *British Journal for the Philosophy of Science* 58: 207-233.

Weisberg, M (2013). *Simulation and Similarity Using Models to Understand the World* Oxford: Oxford University Press.

Weisberg M, Reisman K (2008). The robust Volterra principle. *Philosophy of Science* 75: 106–131.

Yablo, S (2014). *Aboutness*. Princeton, NJ: Princeton University Press.