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Science fictions: Comment on Godfrey-Smith

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Abstract This is a comment on Peter Godfrey-Smith's, "Models and Fictions in Science". The comments explore problems he raises if we treat model systems as fictions in a naturalized and deflationary framework.

Keywords Hans Vaihinger · Models · Fictions · Peter Godfrey-Smith

Hans Vaihinger's *Philosophy of 'As If'* (Vaihinger 1911) was an influential account of the use of models in science and one of the earliest sustained treatments of those models and their elements as fictions. Vaihinger was inclined to see fictions of one sort or another in almost all scientific theorizing. That is not the attitude of Peter Godfrey-Smith, who sees fictions as important in a certain scientific style that he refers to as "model-based science", but not ubiquitous in scientific practice more generally. Nevertheless, where fictions are used, like Vaihinger, Godfrey-Smith finds that the practice deserves philosophical attention. In particular he wants to examine what model-based science achieves in relation to the world at large and what kind of knowledge it makes possible. He appears to be worried about whether a modeler's account of the practices with model systems (the fictions) is defensible from a metaphysical point of view. He takes this as a question about the folk ontology of the practitioners, a question, he suggests, we need to examine from a perspective external to the scientific practice. He frames this as the need to reconcile the ontology as it appears from the inside, the modeler's ontology, with an "all-things-considered ontology". That ontology and the things-to-be-considered are constrained by and contribute to general metaphysics and epistemology, where these are conceived of in a deflationary, naturalistic spirit. This is the spirit one associates with Quine and, before him, with the positivism of Vienna and Berlin.

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It was also the spirit of Vaihinger's "as if", a philosophical stance allied to the best pragmatic elements of logical positivism. Indeed the label "logical positivism" was first used by Vaihinger himself. (See Fine 1993 for more details about Vaihinger's connections to logical positivism.)

With the phrase "logical positivism" (or sometimes "idealistic positivism") Vaihinger was pointing to the imaginative or constructive elements in modeling (the "logical" elements), and also pointing to the naturalistic elements (the "positive" ones) connecting models to observation and experiment. Using fictions, he suggests, is like constructing a scaffolding that we cast off when its purpose has been fulfilled. Like John Dewey and the American pragmatists, Vaihinger invokes the image of a mental expedient, an instrument of thought used to help navigate the world at large. Vaihinger cautions us against any tendency to take fictions as real, in his words, the "as if becomes if". Godfrey-Smith seems to be issuing a similar caution with his distinction between the folk ontology of modeling and an all-things-considered ontology. The difference is that Vaihinger does not think that heeding the caution requires a general, philosophical account of the semantics of modeling. For Vaihinger modeling practice is part of how people use their imagination. The only general account he anticipates is a psychological one that would study the constructive work of imagination empirically. Godfrey-Smith points us in a quite different direction.

Godfrey-Smith imagines a "total picture" of science that connects a general account of scientific practice with a philosophical account of what the practice achieves. Thus his discussion of model-based science and fictions begins with an overview of significant kinds of modeling practices and then proceeds to raise a problem for our philosophical understanding of what is going on there. Primarily, the problem is not, as one might have thought, about the particular fictions that populate the models (the point masses or whatever) but rather about the model system itself, which Godfrey-Smith treats as a fictional thing. He often regards the model system as an imagined simpler analogue of an actual target system, as something that would be concrete (physical objects causally connected and arranged in space and time) if it were real. The difference, say in the philosophy of mathematics, would be over worrying about the ontological status of numbers as opposed to worrying about the status of the several number systems. So what is the worry over model systems? That, it turns out, is hard to say, or at least to say clearly.

One expression of the worry seems connected to Platonism or modal realism, where the concern is about treating a model system, with respect to its target, as a "shadowy additional graspable thing." Another expression of the worry comes from trying to understand comparisons between the model system and its target where, philosophically speaking, introducing "extra fictional 'objects'"to explain what is going on "is surely a problem." Finally Godfrey-Smith also describes the worry as a new problem of modality; namely, how apparent knowledge of "very dubious modal facts" concerning (fictional) model systems gets us to actual knowledge of real world systems.

What can we find in common here and how might that be seen as constituting a philosophical problem? Model systems are considered fictions. They are treated as unreal creatures of our imagination. They are shadowy (like ghosts) and yet

graspable (like a hammer). They seem to be objects referred to in explanations of real world happenings, even though they do not really exist (hence "extra" objects and surely not objects at all by Quine's lights). What we claim about them in such explanations is presumably a "modal fact" but a "very dubious" one. (Is it the modal or the factual that is so dubious, or the combination?) I am not sure how much overlap there is among these various elements, but perhaps there is at least this. Model systems are imaginary objects (in this sense, fictions) that we refer to and make claims about to help understand how some non-imaginary things behave. What then is the philosophical problem?

To bring it out Godfrey-Smith directs us to problems about mathematics and about literature. With respect to mathematics he points to the issue of Platonism, which he takes as the folk ontology of mathematicians but clearly not acceptable from the perspective of an all-things-considered ontology. He also emphasizes the problem of "unreasonable effectiveness" to highlight the gap between Platonism and the real-world uses of mathematics. The literary case brings out the perhaps puzzling use of statements that bridge the real world and the fictional one ("Sherlock Holmes is more famous than any real detective."). But with respect to literature Godfrey-Smith thinks we do not have much of an effectiveness problem because, he thinks, we do not learn that much about the world. He presumes that understanding how narratives facilitate the imagination will get us quite a long way to resolving any philosophical issues of how literature achieves its ends. This treatment of literature, however, significantly underestimates how much real knowledge it can provide. Sherlock Holmes teaches us about hypothetical reasoning and its limits. Thick descriptions in the novels of Jane Austin are lessons in complex human relations. More generally, literature is an important and effective part of our moral education. Still, if Godfrey-Smith is correct in suggesting that the function of narratives in the coordination and extension of human imagining provides the key to understanding the effectiveness of literature, then I think we have to make sure that our all-things-considered ontology includes all the elements that go into the creation of literature. Perhaps that is what Godfrey-Smith had in mind.

Consider Godfrey-Smith's description of Kendall Walton's "prop-oriented make-believe" (Walton 1993) as constructing literature by engaging in games of make-believe. If this is not subject to the problem of the gap then we will need to include narratives and also games of make-believe (or imaginings) when we build up our all-things-considered ontology. While model systems themselves may not be narratives, certainly our descriptions of them are ("Imagine two point particles moving frictionlessly with instantaneous velocities …"), and the way we construct them and use them is surely a game of make-believe. It certainly seems that the gap between our model system considered as tools and the world of their targets is not so big. Nevertheless it still appears to be too big for Godfrey-Smith's comfort, as we can see by his criticisms of attempts to bridge the gap. He looks at three approaches. The first approach is the analogue of Platonism in mathematics. It treats model systems as abstract objects but also as real. The problem, then, is the analogue of the unreasonable effectiveness problem. We need mappings that relate abstract structures to "real" real-world ones and these seem elusive. Of course, this is just another expression of the gap. That is, even in a metaphysical picture

(a metaphysical model!) in which the model systems are treated as real, a problem remains over how they relate to other elements in the ontology (what we might call the really real ones!). Thus enlarging the domain of objects in our ontology will not solve the problem. We also need ontologically transparent relations among the elements of that domain, and also between the fictive elements of our models and the real elements of their targets. Nothing in our account of what model systems achieve can lie outside our all-things-considered ontology.

This issue about relations between the denizens of our model system (rather than about the model system itself) and elements of its target comes out in discussing Roman Frigg's approach (Frigg, forthcoming). According to Frigg, when we compare model with target we only need to consider the properties of the objects in our model system and not the objects themselves. (Barberousse & Ludwig (2009) offer a similar approach.) Our comparisons, however, will need to make use of uninstantiated properties, which to Godfrey-Smith do not seem very different from non-existent objects. The contrast between the uninstantiated properties in our model and the instantiated ones in our target once again raises an issue of a gap. This is also true of the last approach that Godfrey-Smith looks at. This approach by-passes any reference to the objects or the properties used in a model and concentrates instead on the function of model systems, which it regards as centered on conditional assertions with respect to the target system. An example of this might be the treatment proposed recently by Mauricio Suaréz (Suaréz 2009). Suaréz takes the hallmark of a model, considered as a scientific fiction, to be expediency in facilitating inferences about the target, where some of the inferred conclusions are empirically testable. This conception fits right in with Suaréz' account of representation, which also features inferences to a target (Suaréz 2004). A pragmatic approach of this sort is seriously deflationary, which Godfrey-Smith would surely applaud. But still he finds it wanting. He notes that the conditionals (or the inferences) would presumably have antecedents concerning such and so configurations of objects, where those configurations are generally known not to exist. Given these counterfactuals he concludes, "the problem of explaining the empirical usefulness … reappears." The problem in each of these three approaches is how can something that relates to the model system (something "real" but abstract, something uninstantiated or non-existing) enables us to achieve success with respect to the world of the target.

The shape of the problem that concerns Godfrey-Smith is clear enough: there is a gap between model and target and we need to explain how the gap is bridged when we use the model to gather useful information about the target. But "bridging the gap" is a metaphor for a claim and the claim is that something here calls for an explanation, and not any-old explanation. It calls for an explanation that goes beyond the ones that scientists, who actually use the model to get information about the target, are able to offer. We are told explicitly that the explanatory task needs to be carried out from a perspective external to the practice. And we are told that general metaphysics and epistemology enter in as well. So the answer is not the scientist's explanation for why (or how) the model works and achieves as it does. We require a distinctively philosophical explanation.

Surely most of us understand that not everything we can encounter "needs" or "requires" explanation (at least so we come to believe, roughly, after the age of five). Most things in our lives just are. Thus to claim that something requires (or "calls for") explanation is to adopt the burden of saying why that is so. In science that burden is generally carried by theories and standard practices in the field. They provide background for what is understood or taken for granted (in given contexts) and highlight what stands out as proper targets for scientific explanation. They also offer guidelines for how to do it. (Recall Wilfred Sellars (1961) "unexplained explainers", "explained explainers", and "explained nonexplainers.) Thus Newtonian physics, whose explanatory task concerns the motions and interactions of bodies, takes inertial motion as "natural", not in need of explanation. Accelerated motion calls for explanation and Newton's laws are the means for carrying it out. Likewise, the ordinary flow of everyday life provides a background against which some events stand out, frequently as surprising or unexpected, and so call for explanation. In this way scientific and everyday norms legitimate requests for explanation (again in context). Of course, as Thomas Kuhn (1970) and Stephen Toulmin (1961) taught us, those explanatory paradigms (Toulmin's "ideals of natural order") can be called into question and may shift over time.

Philosophers sometimes forget that demands for explanation have a texture, and proceed as though, in a given context, everything were a suitable explanatory target. Even when they do not commit that fallacy, philosophers often fail to make clear the background and norms in terms of which a particular request for explanation may or may not need to be honored. This seems to me the situation with the problem of the gap. In the context of a working model system, scientist's will have an account of how the model succeeds in conveying relevant information about the target. What is the background, then, against which there is a need for a different, external, philosophical explanation of that very fact? I find several ways of thinking about this in Godfrey-Smith's approach.

One way emerges from his articulation of a general goal for philosophy of science, which is to give us a "total picture of science." This picture would contain an account of scientific practice together with a justification for what science achieves (he writes of telling an "epistemological story"), including what kind of knowledge it makes possible. The justification amounts to an explanation for the successes of the practice, couched in an acceptable philosophical framework (e.g., using an all-things-considered ontology). Of course this goal amounts to a norm according to which literally everything done in science requires explanatory, philosophical reconstruction and justification. In the context of the realism debates a demand to explain the success of science is familiar. By now one would have thought it also suspicious, since the endless run-around pursued in those debates makes it clear that, in this context, attending to the demand for explanation leads nowhere philosophically useful. In view of that it is hard to see why a global demand for philosophical explanations, a demand with respect to *all* of scientific practice, should even be a player in coming to terms with our philosophical work. Talk of goals here seems a thinly disguised way of imposing a philosophical agenda without providing any plausible rationalization for why it would be reasonable or fruitful to pursue.

There is a second background that might set the stage for an explanatory request. One of Godfrey-Smith's basic moves is to assimilate the issues over model systems to the problem of unreasonable effectiveness That can seem like an genuine problem with respect to mathematics if one adopts some form of Platonism and thinks that mathematics deals with abstracta which are causally (and "hence", one concludes, epistemologically!) disconnected from the phenomenal world. If model systems were such disconnected abstracta, then we might well appear to have a gap problem that calls for an explanation of how we achieve useful information from them. But why should we think that our working models are detached epistemologically? What reason is there to suppose that model systems are, as it were, born in epistemological sin? The rhetorical device of referring to model systems as "fictions" or as "shadowy" things may well create that impression, but where is the argument for thinking that such systems, whatever we call them, carry that sinful burden? One case that Godfrey-Smith examines in some detail concerns the use of "impossibilities", what Vaihinger called "genuine" or "full" fictions. Surprisingly, Godfrey-Smith is unsure of what to say about them, showing some sympathy with the side (e.g., Vaihinger's) that takes impossibilities to be used normally and fruitfully in science. But he is also open to reconstructing the role of impossibilities in some more or less eliminative way. If they are not eliminable, however, he thinks their use would raise issues for a realist construal of model systems, demanding a deflationary treatment. It is precisely this demand that we hoped to understand and perhaps to challenge. Instead the discussion of impossibilities simply takes it as an assumption that genuine fictions are problematic and that their use requires an external, philosophical explanation.

Finally, there may seem to be a third background for the need to supplement scientists' explanations with philosophical ones. It would come from semantics. Scientists' accounts of how their models work may appear to refer to structures or objects that either do not exist at all or that are not members of our all-things-considered ontology. In that case we may feel the need to finesse those references somehow in order to avoid commitment to the undesired referents. Think of Bertrand Russell and "the present king of France" (Russell 1905). But does the successful use of otiose-referring expressions actually commit one to believing in otiose referents? Surely after more than one hundred years of semantic theory we know better. Not all language use that appears to be referential is referential, not all referential use is extensional, and even extensional reference need not be combinatorial or executed in terms of simple denotation ("frictionless plane" denotes a plane that is frictionless). Surface grammar is not a reliable guide to meaning or reference. We do many things with words and only detailed examination can show what commitments, if any, are plausible in a given case. So where a scientist is clear about using fictions, why assume that some alternative account is needed to clean up what is said? Why imagine that understanding modeling requires a general mechanism for ontological de-commitment or cleansing? Nothing in our understanding of how we use language supports the demand for an add-on philosophical treatment.

So we come round full circle. The backgrounds against which fruitful uses of model systems could be seen to "require" philosophical explanation turn out

themselves to be little more than expressions of that very same requirement. At least in Godfrey-Smith's eyes the idea that model systems should be fruitful at the empirical level seems to run counter to certain ideals concerning the natural philosophical order. It is hard to dispute paradigms and particularly metaphysical ones, which are especially resistant to ordinary inductive evidence (here the evidence that demanding such explanations in other cases has contributed little to our understanding of science). As Kuhn emphasized, however, in science what appears as an unresolved problem in the light of one paradigm will sometimes be become an iconic solution under a paradigm shift. That was certainly the case with the concept of inertia. Aristotle required that all local motion (change of place) in the sublunary realm demands explanation and indeed demands to be explained by means of an in-contact mover. No gaps. That was his version of deflation; real honest touching: no spirits, actions-at-distance, or the like. But Galileo (1630, The Second Day) responds to Aristotle's challenge concerning the rotation of the earth with his parable of the tower. When we drop a heavy rock from a tower it lands at the base (not way behind) because the rock simply keeps pace with the tower, even unattached, moving horizontally with the same speed as before. (Actually Galileo thinks it moves in a circle; but never mind.) There is no in-contact mover. Nothing bridges the gap between tower and rock. For Galileo and the Newtonian tradition there is no question of a "mover" or a bridge at all. Inertial motion is natural, it does not require explanation; explanations are reserved for accelerations.

Given his survey of the most plausible candidates for explaining the utility of model systems and given his conclusion that none pass tests set by his ontology, maybe Godfrey-Smith needs to consider a different kind of response. Maybe he should be open to a change of ideal in the manner of Galileo's response to Aristotle's problem of the gap. We might try to take what happens typically, in fruitful applications of model systems, not as the problem but as indicating the solution. We could try to see the use of model systems as setting a paradigm for how imagination is useful in the investigation of nature. If we wonder, say, at how impossibilities such as infinite populations in population biology, can be useful we might study carefully just how models of this sort are applied and use that knowledge to develop standards for good or improved use. There are philosophical treatments of modeling that have already made such a shift in understanding. Godfrey-Smith looks at one kind, which is exemplified by the inferential picture offered by Suaréz (2004). The idea is to take seriously that we make inferences from a model system to the world and to treat those inferences as connected to the way a model represents its target. In Suaréz' account that means not to treat the system or its elements in terms of reference or denotation at all. Thus there is no question to be raised about the "reality" of the system or its elements. Typically, models contain adjustable parameters that enable us to play informative "what-if" games. As Godfrey-Smith points out, these have a counterfactual character. Nevertheless we use them to make real-world inferences. Instead of regarding those inferences as encapsulating a puzzle of the gap, one might just say, "That is what using a model system is." Alissa Bokulich (2008) also offers an account of modeling that corresponds to the shift in ideals that I am suggesting. She develops an account of what she calls "model explanations" that makes patterns of counterfactual

dependency central. When those patterns are suitably mirrored by an appropriate target, she counts those counterfactual dependencies as yielding genuine understanding; that is, as explanatory of real-world dependencies in the target. Both Suaréz and Bokulich use orbits in Bohr's model of the atom to illustrate their respective accounts and both make it plain that they regard those orbits (and the model) as scientific fictions. The orbits are impossible, given our current understanding of the microworld, and the model system as a whole has no real-world instantiation.

One can imagine that the followers of Aristotle cried out in astonishment, "It's impossible! How can the rock keep pace with the tower when there is no actual contact between them?" I imagine those sympathetic to Godfrey-Smith's philosophical ideals reacting similarly, "It is impossible. How can one draw conclusions about a target across the gap?" In neither case can one respond to the plea in the manner set by the old ideal. We have to find a different way of thinking about the matter. History suggests that as we live with a new way of thinking, it becomes familiar and begins to feel natural. Without answering to its demands, the old itch just stops – at least until new anomalies become apparent. Writing just before Vaihinger, Dewey put it like this, "But in fact intellectual progress usually occurs through sheer abandonment of questions together with the alternatives they assume …. We do not solve them: we get over them" (Dewey 1909, p.188).

One thing I want to make clear. The strategy I am suggesting is not what Martin Thomson-Jones (forthcoming) calls a strategy of deferral. I am not suggesting that the gap corresponds to metaphysical or semantic or epistemological problems that we philosophers of science can leave to those specialists while we get on with other tasks. 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.

The problem set by Godfrey-Smith is how to treat a model system and its contents as "objects" we can "grasp" without compromising a certain conception about how the things we humans do and create *must* fit together. That conception goes along with a wish for an explanatory, philosophical theory of everything. Letting go of that grandiose wish and its related ontological demands resets the problem as one of understanding how model-based science works. Surely it works by means of pictures, replicas, narratives, the exercise of human faculties of imagination and judgment, ordinary empirical methods and standard formal techniques. These are intelligible resources (no witches here), available to scientists and philosophers alike, and once the task of understanding is correctly set it is hard to see why these resources should not suffice for understanding modeling. Probably that is what Vaihinger's "as if" was trying to teach us.

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References

Barberousse, A., & Ludwig, P. (2009). Models as fictions. In M. Suaréz (Ed.), *Fictions in science: Philosophical essays on modeling and idealization* (pp. 56–76). London: Routledge.

Bokulich, A. (2008). Can classical structures explain quantum phenomena? *British Journal for the Philosophy of Science, 59*(2), 217–235.

Dewey, J. (1909). The influence of Darwinism on philosophy. In J. M. Capps & D. Capps (Eds.), *James and Dewey on belief and experience* (pp. 179–188). Urbana: University of Illinois Press. 2005.

Fine, A. (1993). Fictionalism. *Midwest Studies in Philosophy* 18, 1–18. (Reprinted in M. Suarez (Ed.), *Fictions in science: Philosophical essays on modeling and idealization*) (pp. 19–35). London: Routledge.

Frigg, R. Models and fiction. To appear in *Synthese* (forthcoming).

Galilei, G. (1630). *Dialogue concerning Two Chief World Systems*. S. Drake (trans.) New York: Modern Library, 2001.

Godfrey-Smith, P. Models and fictions in science. *Philosophical Studies*. doi:10.1007/s11098-008-9313-2.

Kuhn, T. S. (1970). *The structure of scientific revolutions*. Chicago: University of Chicago Press.

Sellars, W. (1961). The language of theories. In H. Feigl & G. Maxwell (Eds.), *Current issues in the philosophy science*. New York: Henry Holt, Rhinehart and Winston.

Suaréz, M. (2004). An inferential conception of scientific representation. *Philosophy of Science, 71*, 767–779.

Suaréz, M. (Ed.). (2009). Scientific fictions as rules of inference. In *Fictions in science: Philosophical essays on modeling and idealization* (pp. 158–178). London: Routledge.

Thomson-Jones, M. Missing systems and the face value practice. To appear in *Synthese* (forthcoming).

Toulmin, S. (1961). *Foresight and understanding*. Bloomington: Indiana University Press.

Vaihinger, H. (1911). *Die Philosophie des Als Ob*. Berlin: Verlag von Reuther & Reichard. Translated as *The Philosophy of 'As If'* by C. K. Ogden. London: Kegan Paul, 1924.

Walton, K. (1993). Metaphor and prop oriented make-believe. *European Journal of Philosophy, 1*, 39–57.