Synthese (2021) 198 (Suppl 21):S5061–S5076
https://doi.org/10.1007/s11229-017-1665-8

S.I.: MODELING AND REPRESENTATION

Models, models, models: a deflationary view

Jay Odenbaugh[1]

Received: 15 February 2017 / Accepted: 21 December 2017 / Published online: 3 January 2018

Abstract In this essay, I first consider a popular view of models and modeling, the *similarity view*. Second, I contend that arguments for it fail and it suffers from what I call "Hughes' worry." Third, I offer a deflationary approach to models and modeling that avoids Hughes' worry and shows how scientific representations are of apiece with other types of representations. Finally, I consider an objection that the similarity view can deal with approximations better than the deflationary view and show that this is not so.

Keywords Models · Modeling · Idealization · Abstraction · Representation · Deflation · Approximation · Truth

"Few terms are used in popular and scientific discourse more promiscuously than 'model"' (Goodman 1976, 171)

1 Introduction

In this essay, I first discuss a popular position about models developed by Hesse (1966), Giere (1988, 1999), and Weisberg (2012). Secondly, I consider an objection to the view due to Hughes (1997) which shows that its notion of similarity between

I thank Steve Downes, Catherine Elgin, Melissa Vergara Fernández, Jim Griesemer, Andoni Ibarra, Iñaki San Pedro, and Chris Pinnock for their help with this essay. Additionally, I thank two anonymous referees for the very helpful feedback.

✉ Jay Odenbaugh
jay@lclark.edu

1 Department of Philosophy, Lewis and Clark College, Portland, OR, USA

model and world is deeply problematic. Thirdly, I sketch what I call a "deflationary" view of models and modeling (c.f. Callender and Cohen 2006; Downes 1992; Suárez 2010, 2015). It avoids the above problem and thus is prima facie more plausible than the alternative. It also has the added beneficial consequence that models and modeling derive from familiar types of representation and can be studied by cognitive science.

2 The similarity view

A very common approach to understanding what models are and how they work is the *similarity view.* Stated most simply, the similarity view says that models are more or less similar to the world and it is in virtue of their similarity that they successfully represent the world. I will first trace this important theory through three figures: Mary Hesse, Ronald Giere, and Michael Weisberg. Their motivations for developing their accounts differ, but they have all articulated likeminded views. Tracing this history is important because the objections I raise later in the essay are not specific to any one version of the position. Rather, the objections I raise are a problem for all of them since they form a family.

2.1 Mary Hesse

Hesse (1966) provided an analysis of models as analogies. Consider molecules in a gas, which we are trying to understand. We might "model" the molecules using some other group of objects—in this case, billiard balls. On her view, we have a positive, negative, and neutral analogy. That is, there are properties shared between both systems which is the positive analogy; there are the properties not shared which is the negative analogy; there is the neutral analogy which are those properties which we simply do not whether they are positive or negative. A model_1 is "the imperfect copy (the billiard balls *minus the negative analogy*..." (Hesse 1966, 9). She also writes,

> Since I shall also want to talk about the second object or copy that includes the negative analogy, let us agree as a shorthand expression to call this 'model_2' (Hesse 1966, 10)

Hesse's models use analogy and hence similarity. Similarity is here understood as the sharing of properties. Things are similar insofar as they share properties and dissimilar insofar as they do not.[1] We explain or understand something unfamiliar by virtue of its

[1] Notoriously, Nelson Goodman argued that this proposal was "useless" (Goodman 1972). For any two objects, there is at least one set of which they are both members. Hence, the claim that similarity can be understood in terms of shared properties is universal and thus useless. The same is true if we say that a and b are more similar than c and d if, and only if, the former have more properties in common than the latter. For any two things, they will have exactly the same number properties in common. If the number of objects is n, then the number of shared properties is 2^{n-2} and if the number of objects is infinite, the number of properties shared is infinite. But, Goodman assumes properties simply are sets. Many would argue this is false because there are sets for which there is no property (or as Lewis puts it, no "natural" property for every set) (Lewis 1983).

similarity to something previously understood. Thus, for Hesse, models as analogies are required for interpreting theory and explaining phenomena. Suppose we are trying to understand the force exerted by a molecule x on another y at a time. We recognize that this force is equal in magnitude and opposite in direction to that exerted by y on x at that time. Thus, we can use a model (e.g. billiard balls) to understand the behavior of molecules in a gas. We "make sense" of the former is in terms of the latter. Hesse's approach is particularly illuminating when we consider material or scale models, but can include some more theoretical models (e.g. water and sound waves).

2.2 Ronald Giere

A second source of the similarity view is Ronald Giere (1988). He articulated his view of models, and of theories, in response to the received view of theories. On this view, theories are axiomatic systems (Hempel 1966). However, after criticisms from a variety of philosophers, the semantic view of theories was born. Models on the semantic view are those structures which satisfy the sentences of a theory (van Fraassen 1980; Suppe 1989). Giere argued that even this semantic view was too distant from scientific practice and thus developed his own naturalized account (Giere 1988, Chap. 3).

On Giere's view, a model is an idealized, abstract structure and the relationship between model and world is that of similarity. He writes,

> My preferred suggestion, then, is that we understand a theory as comprising two elements: (1) a population of models, and (2) various hypotheses linking those models with systems in the real world. (Giere 1988, 85)

Thus, there is a theoretical definition and a theoretical hypothesis. The theoretical definition describes a structure often thought of as a phase portrait in a state space which satisfy some differential equations. For example, we might interpret the Lotka–Volterra predator–prey equations

$$\frac{dV}{dt} = rV - aVP$$
$$\frac{dP}{dt} = baVP - qP$$

such that they are made true by the phrase portrait in the state space visually depicted by this graph.

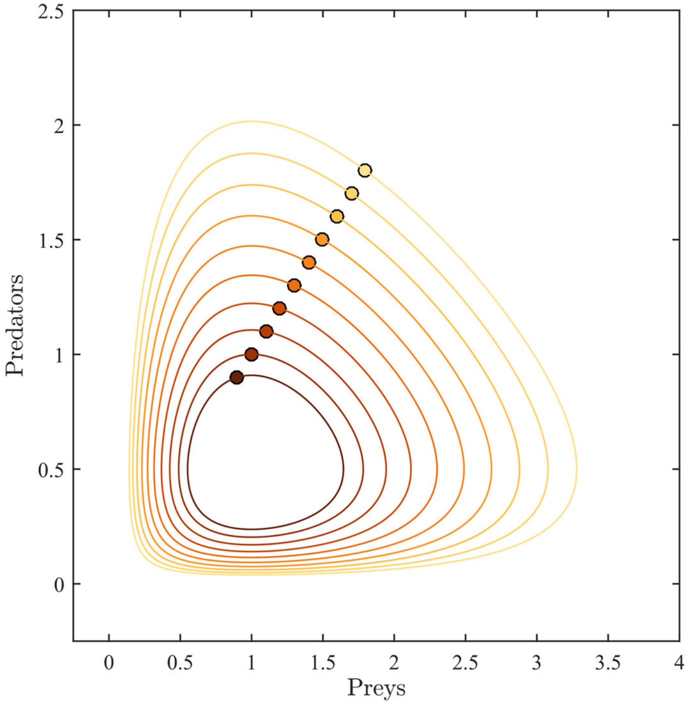

Fig. 1 Graphical depiction of the Lotka–Volterra state space

The theoretical hypothesis is one of similarity. A model is similar to some system in some respects and to certain degrees. Giere writes,

> The positions and velocities of the Earth and moon in the Earth-moon system are very close to those of a two-particle Newtonian model with an inverse square central force. (Giere 1988, 81)

Thus, the abstract structure, in this case a phase portrait in a state space, makes the equations true. The target system, some predator–prey populations are then more or less similar with regard to certain properties of this abstract structure.

2.3 Michael Weisberg

Recently Weisberg (2012) has offered his own rich account of modeling and models. On his view, models are interpreted structures, which can be mathematical, computational, or material. For example, the Lotka–Volterra model is mathematical, the Schelling model of segregation is computational because it consists in simulations, and the physical model of the San Francisco Bay is, well, material. An interpretation involves a construal which includes an assignment, an intended scope, and fidelity criteria. Returning to our Lotka–Volterra model as an example, we assign parameters like a, b, r, q and variables like V and P to properties of the system such as 'r' denotes the property *intrinsic rate of increase of prey* and 'V' denotes the prey population.

An intended scope specifies over what range of the respective parameter and variable values the model applies. Finally, the fidelity criteria are those criteria by which the model is accurate. It is important to note that for Weisberg, model descriptions and models are distinct. He writes,

> When we talk about models, write about them, or show a picture or diagram, we are employing a model description. These descriptions must be distinguished from the models themselves. (Weisberg 2012, 33)

> Equations or other kinds of statements specify mathematical objects and these objects satisfy their descriptions. However, unlike in the case of concrete models, mathematical models can be studied and manipulated only via their descriptions. While the Lotka–Volterra model itself is not a set of equations, it can be studied only through proxies such as these equations. This is probably the main reason that scientists often informally refer to equations as models; their attention is focused on these equations. (Weisberg 2012, 37)

What the Lotka–Volterra equation describes is not predator–prey populations, but a mathematical structure that is more or less like predator–prey populations. This distinguishes theorizing which is direct versus theorizing which is indirect. Modeling, according to Weisberg (and Giere) is indirect.

Weisberg also offers a weighted-feature account of similarity. The similarity between model and target $S(m, t)$ is,

$$\frac{|M_a \cap T_a| + |M_m \cap T_m|}{|M_a \cap T_a| + |M_m \cap T_m| + |M_a - T_a| + |M_m - T_m| + |T_a - M_a| + |T_m - M_m|}$$

where M_a are model properties, T_a are target properties, M_m are model mechanisms, and T_m are target mechanisms. Terms of the form, $|M_i \cap T_i|$ denote the the intersections of properties between model m and target t, terms of the form $|M_i - T_i|$ and $|T_i - M_i|$ denote properties had by m and not t and t and not m respectively.[2] If no attributes or mechanisms are shared, then his measure $S(m, t) = 0$ since the numerator $|M_a \cap T_a| + |M_m \cap T_m| = 0$ and the relevant terms in the denominator $|M_a - T_a| + |T_a - T_m| + |T_m - T_a| \neq 0$. It is extremely interesting to note that Weisberg's approach is continuous with Giere's but also with Hesse's.[3]

2.4 Arguments for the similarity view

A variety of arguments have been offered for the similarity view. I will just consider three very prominent ones. Let's say that the traditional view of scientific theorizing

[2] Weisberg includes weights given to terms in the equation, but I ignore those for simplicity; i.e. including them would not make a difference to the arguments presented below.

[3] I am not making a historical claim of influence (though I think there is such a chain of influence). Rather, the type of view articulated by Hesse, Giere, and Weisberg are all developing similar thoughts on the matter. By emphasizing similarity, we are locating the model-world relation as one of analogy with positive (the intersections), negative (the differences), and neutral analogies. Additionally, scientists select the respects in which a model and target are thought to be similar.

supposes theories are sentential. They consist in the conjunction of a set of sentences closed under entailment. Some have argued scientific theorizing goes beyond this since theories consist in a small set of laws. However, in many sciences, there are no laws. Hence, theorizing in those sciences cannot consist in theories as small sets of laws. However, the similarity view does not presuppose there are laws and hence is superior (c.f. Beatty 1980, 1982; Giere 1999; Hausman 1992; Rosenberg 1994; Thompson 1989). However, there are two responses. First, with regard to biology where laws are often denied to exist, there may be no laws for species, but there are laws for *kinds* of species such as host and parasitoid, predator and prey, r-selected and K-selected species, etc. Second, if models are actually and possibly similar to empirical systems, how can there not be laws? That is, similarity between the two across possibilities presumably covers what systems would do in non-actual circumstances. This just takes us back to laws.

A second argument offered for the similarity view is this. Modelers devise models independently of application (Weisberg 2012). The similarity view can distinguish theoretical models from theoretical hypotheses, but the traditional view cannot since theories must already must have their application encoded. Modeling is indirect contrary to the traditional view which claims it is direct. There is a response here too. Traditional theories do provide empirical interpretations but they need not provide empirical applications. They tell us, according to the theory, what would happen if the relevant set of conditions is met. However, they are silent whether in fact those conditions are met. Thus, the traditional view is indirect if the similiarity view is.

Third argument for the similarity view (or more specifically for the semantic view), is given by Bas van Fraassen. He writes,

> Perhaps the worst consequence of the syntactic approach was the way it focused attention on philosophically irrelevant technical questions. It is hard not to conclude that those discussions of axiomatizability in restricted vocabularies, 'theoretical terms', Craig's theorem, 'reduction sentences', 'empirical languages', Ramsey and Carnap sentences, were one and all off the mark – solutions to purely self-generated problems, and philosophically irrelevant. (van Fraassen 1980, 56)

I agree that the received view was extremely distant from scientific practice and this introduce irrelevant pseudo-problems. Additionally, I think the similarity view is an improvement. But, this argument hinges on their being no better account of models which fits scientific practice. This assumption I challenge below with my defense of General Griceanism.

3 Hughes' worry

There are many objections to understanding representation or representational accuracy in terms of similarity (c.f. Goodman 1972; Cummins 1989). In this paper, I raise and focus on what I believe is the most serious objection to the similarity view which comes from thinking about scientific models. Consider a model of an ideal pendulum,

$$m\left(\frac{d^2x}{dt^2}\right) = -(mg/l)x$$

For a pendulum with very little friction, with very small angles of swing, and very short time intervals, this model can accurately represent a pendulum's oscillations.

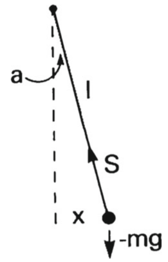

Fig. 2 Idealized pendulum

Here is a worry raised by R. I. G. Hughes, He writes,

> ...[W]e may model an actual pendulum, a weight hanging by a cord, as an ideal pendulum. We may even be tempted to say that in both cases the relation between the pendulum's length and periodic time is approximately the same, and that they are in that respect similar to each other. But the ideal pendulum has no length, and there is no time in which it completes an oscillation. It is an abstract object, similar to material pendulums in no obvious sense. (Hughes 1997, 330)[4]

We can state the argument explicitly in this way. An object can have the properties *periodic time* and *length* only if it is spatiotemporal. Mathematical objects are not spatiotemporal. Hence, they cannot have the properties *periodic time* and *length*. Two objects are similar only if they share properties. Mathematical objects and pendulums cannot share the properties *periodic time* and *length* since the former can't have them. Therefore, they cannot be similar with respect to *periodic time* and *length*. There can be no similarities between mathematical models and world for Giere and Weisberg since they do not share the relevant spatiotemporal properties. Weisberg's weighted-feature measure of similarity will be equal to zero since the intersection terms in the numerator will be zero and the complement terms in the denominator will be non-zero. Hesse's approach avoids Hughes' worry only if all models are material and not mathematical. There is no problem with molecules and billiard balls sharing spatiotemporal properties. But, there is no good reason to think that *all* models are like this.[5]

There are three responses to Hughes' worry as far as I can tell. First, one might argue mathematical objects are concrete and not abstract; i.e. the truthmakers of mathematical claims are concrete (c.f. Kitcher 1984; Field 2016). But, there are not enough

[4] Chakravartty (2001) provides similar worries to the ones Hughes provided.

[5] As we will see, my own view is that concrete objects like inscriptions and utterances do represent the world. However, they don't do so, or least don't generally do so, by being similar to it. My name represents me but doesn't do so by being similar to me.

concrete truthmakers to make mathematical claims true (e.g. the set of real numbers is larger than the set of natural numbers).[6] Second, one might deny that mathematical objects like state spaces exist. But, if two things are similar, then both exist. Existing and non-existing objects do not share properties. One might propose an alternative account of similarity that does not involve sharing properties but it is not clear what this would be (c.f. Goodman 1972; Quine 1969). Third, one might claim mathematics (Balaguer 2001), and models that use it (Frigg 2010; Toon 2012), involves "make-believe." Maybe we should be fictionalists, but Giere (2009) and Weisberg (2012, Chap. 4) are not fictionalists about models. And, I worry that fictionalists about models have an "exportation" problem in any event. How can we learn about how the world works from what occurs in a fiction? We cannot infer simply from Sir Arthur Conan Doyle's novels truths about London. Why would scientific models construed fictionally be any different?

Weisberg (2015) has responded to Hughes' worry as I have presented it (Odenbaugh 2015). I want to consider his thoughtful responses. First, Weisberg is largely concerned to understand the "epistemic level" of theorizing and not to determine the ultimate ontology of those practices he analyzes. Moreover, he takes his view to be consistent with whatever ontology of mathematics is correct. This is of course perfectly reasonable only if his account is consistent with a wide swath of these ontologies. I have argued that claiming there are relevant similarities between mathematical structures and concrete objects requires that they share properties.[7] This requires both exist and thus his account is committed to a mathematical realism. However, I have also argued that they cannot share the relevant properties of interest like *periodic time* and *intrinsic rate of growth*. Thus, his mathematical realism embroils him in a debate over the ontology of mathematics and thus means he cannot stick to the "epistemic level." His views force him to take sides in the philosophy of mathematics. Second, he writes,

> Mathematical objects as understood by scientists don't have properties that would make them similar to real-world targets, and they have many properties that no physical system can have. This is an important objection when directed at those who see mathematical models as strictly mathematical objects, such as some structural realists and traditional defenders of the semantic view of theories. But I think that mathematical models are *interpreted* mathematical objects. A harmonic oscillator model can be said to have a period because modelers interpret part of its mathematical structure as denoting a period. These relations of denotation are such that it makes sense to say that the model, but not the mathematical structure itself, has properties like a period.

[6] This is true even if we restrict our mathematics to that utilized in scientific theories insofar as they employ the real numbers. Hartry Field proposes a very large number of spacetime points and their relations as truthmakers for Newtonian classical mechanics. But these seem as recondite as pure mathematical objects. Likewise, Kitcher understands mathematical claims as made true by idealized constructors who group and permute. These constructors are also as recondite as the objects they replace.

[7] Of course, if mathematical realism is correct, one might correctly claim there are "Cambridge properties" they share; I am thinking about π and a beer right now. Both share the property *being thought of by Jay*. But those are not relevant to our purposes.

Here I remain unpersuaded. Consider the following mathematical claim, $T = 2\pi\sqrt{\frac{l}{g}}$, where l and g are positive real numbers. This is as stated is just mathematics. However, suppose I interpret the positive real numbers l and g as length and gravitational constant respectively. Likewise, I interpret the real number T as periodic time. I then claim that there are similarities between this interpreted structure and an actual pendulum. My denoting parts of a mathematical structure with terms associated with physical magnitudes does not mean the mathematical structures share properties like *periodic time* with the actual pendulum. Interpreting a mathematical structure leaves us with another problem. How can an abstract object come to represent a concrete one? I am reminded of the question supposedly asked of Abraham Lincoln, "How many legs does a dog have if you call his tail a leg? Four. Saying that a tail is a leg doesn't make it a leg." And, if we use a term associated with spatiotemporal things and name a mathematical structure with it, we do not thereby make the thing so named spatiotemporal or similar to things which are. From here, I will assume Hughes' worry provides a good reason to reject the similarity view.[8]

4 A deflationary view

In this section, I want to sketch a deflationary approach to models and modeling (c.f. Callender and Cohen 2006; Downes 1992; Suárez 2010, 2015). Many philosophers of science writing on models have provided an account of representation specifically for models. The thought is that scientific models are a really special *sui generis* form of representation. For example, after examining Galileo's proof of a kinematical claim using geometry, Hughes (1997) provides a DDI account of how models represent the world. The DDI account supposes we *denote* physical magnitudes with mathematical ones, we *demonstrate* physical claims through mathematical analysis, and *interpret* those claims in terms of physical objects and events. Giere (1988) offers his own account of scientific representation as well. On Giere's account an *interpretation* is the assignment of general terms to sets of abstract objects, and *identification* is the assignment of terms or names to specific abstract objects. It is in virtue of the activity of interpretation and identification that the model comes to represent the world. It is a separate issue as to whether it is an *accurate* representation; that is settled by intended similarities between model and world. Likewise, van Fraassen (2010) offers his own account. He argues that there are several features that make a scientific representation

[8] Incidentally, I am inclined to think that Weisberg could reformulate his view to avoid Hughes' worry. First, Weisberg's models and target systems can be construed as relational structures. Second, we can formulate whatever morphism we like between an abstract object and a concrete one construed as relational structures. For example, sets A and B have the same cardinality, if there is a bijection from A to B. Two sets having the same cardinality is a property they can share regardless of the ontology of their members of the respective sets. This presumes that the model and target system have both been construed as mathematical objects. But now we have a problem of how a mathematical object can denote a concrete one. Bas van Fraassen (2010) has argued that we model phenomena of the world with data models or what he calls appearances. We then determine the fit of our theory to the appearances. That is, we evaluate how one model fits another another model. But you ask, how does something abstract like a mathematical structure represent something concrete? van Fraassen suggests we ignore this question since it engages in metaphysics. As a naturalist, I do not think we can reasonably avoid this question.

a representation. First, the fact that x represents y must be established intentionally. Some agent must intend that x represents y. Second, there must be a coding convention in place for the consumer. Imagine a novice picks up a textbook on epidemiology and examines the SIR model of of the spread of disease,

$$\frac{dS}{dt} = -\frac{\beta IS}{N}$$
$$\frac{dI}{dt} = \frac{\beta IS}{N} - \gamma I$$
$$\frac{dR}{dt} = \gamma I$$

The text utilizes all sorts of mathematical apparatus for representing the world but the novice does not understand the model. For the vehicle to represent some phenomena, there must be a convention for how the represented is "coded" in the representation and the novice must be capable of understanding the convention. For example, after reading the text, they learn that S represents the susceptible population, I represents the infected population, R represents the recovered or immune population, β is the rate of contact, γ is the rate of recovery, and $N = S + I + R$. Third, some aspects of the represented must be selected for representation. When a map represents it does so in certain respects but not in others. Maps do not usually represent the color of an area but do often represent relative distances. Fourth, there is a "fitting" or accuracy relation between the representation and the represented, which is contextually determined. Usually, it concerns only those aspects that are selected to be represented or some subset of them. On van Fraassen's account, all representations are intentionally established, have a coding convention, have selected aspects, and have an accuracy relation.

As such, there is nothing wrong with these sorts of approaches. The worry is that they are "reinventing the wheel." Following Callender and Cohen (2006), I am a proponent of *General Griceanism* (Grice 1991). There are fundamental and derived representations and the latter are explained in terms of the former. This is distinct from *Specific Griceanism* which claims that sentence or utterance meaning is to understood in terms of speaker meaning, speaker meaning is understood in terms of communicative intentions, and this is explicated in terms of a naturalistic account of mental content. General Griceans think that we do not need a special account of scientific representation for models. We simply deploy those general accounts of representations that we find in cognitive science, cognitive psychology, linguistics, and other related fields (Cummins 1989; Sterelny 1990). This does not imply that there are not different types of representation—there most assuredly are. For example, pictures and words represent in different ways (Goodman 1968). But these different ways are orthogonal to a science/non-science distinction. The similarity view departs from General Griceanism in two ways. First, it supposes that models are special *sui generis* form of indirect representation. Second, the representations are not true of or satisfied by objects but are aspectually similar to the represented. Here I deny we need to make either supposition. The arguments for the similarity view are unconvincing and Hughes' worry shows there are extra costs to accepting this view.

Why accept General Griceanism? First, scientific representations are built from ordinary representational tools like language, diagrams, etc. That is, scientific representations are derived from those other representational resources. It would be extremely odd to think that scientific representations are *sui generis* given they are constructed from these elements. Second, the things that make scientific representation seem distinct from other forms of representation are actually found in them as well. Models are representations that involve *abstraction* and *idealization* (c.f. McMullin 1985; Cartwright 1994; Morrison 2015). A representation abstracts when there are properties of the system which it does not represent. A representation idealizes properties it represents by distorting them. But these so-called special features are found in other types of representation. For example, a black and white drawing of a friend does not represent their colors. In language, we presuppose sharp boundaries where there are none between things (e.g. "x is bald"). Third, Hughes, Giere, and van Fraassen already employ a Gricean framework for thinking about representation (e.g. they cite the importance of intentions and interpretation). However, they don't engage the cognitive sciences and their treatment of representation.[9]

We can distinguish between representational vehicles and representational contents (Dretske 1997). Representational vehicles are the objects, events, or properties that do the representing. Representational contents are how the vehicles represent objects as being so-and-so. Scientists use various vehicles to represent the world including concrete objects, equations, graphs, pictures, etc (Perini 2005a, b). These representational vehicles and the content they express *are* the models. We might say models are nothing over and above their mode of presentation. When we "write down a model" we are doing just that. The similarity view assumes that models are the objects which are represented by the vehicles with their content and those objects represent empirical systems. For example, the Lotka–Volterra predator–prey equations represent an state space which represents via similarity an actual predator–prey population. However, I reject this view—the models are the vehicles with their representational content and not the objects so represented (if any object is so represented). On my view, the Lotka–Volterra predator–prey equations are the model and they represent predator–prey populations. This avoids Hughes' worry since models are just model descriptions. We can also make good sense of how models relate to the world—they do so just like other representations. They are true or false of it. This avoids confusing properties of vehicles and contents. Weisberg wrote that in modeling we interpret part of a mathematical structure as denoting periodic time. Incidentally, I largely agree *if* we mean by that we use part of a mathematical equation, a part of our language, to denote a property like *period length*. Suppose I utter the following in a population biology class,

> Let 'N' stand for population abundance, 'r' stand for the rate of increase independent of other species, and 'K' represent the number or organisms the environment permits. The growth of the population is $\frac{dN}{dt} = rN\left(1 - \frac{N}{K}\right)$.

[9] Giere (1999, Chap. 6) utilizes psychological work on categorization involving prototypes and exemplars to understand how models form families with great insight. However, I would argue that the similarity view per se is not driven by findings in cognitive science.

I have used mathematical language to denote biological species and their properties. And, I can apply various tools devised in calculus to study those groups of organisms.[10]

5 Approximate truth: an objection

Suppose you often think of models as representational vehicles with some content in a natural language and not as some mathematical object. The similarity view has a easy time explaining how models approximate targets. There are similar in certain respects and to certain degrees. But, the General Gricean view has to deal with that old problem of "approximate truth." This seems hopeless and I myself have worried that it was indeed so (Odenbaugh 2011). In this section, I want to show how this deflationary approach can use resources of cognitive science to deal with approximation independent of the similarity view. That is, the General Gricean can use resources from philosophy and the sciences to make headway on the problem just as they would recommend.

Here is a statement of the problem of approximate truth from Ronald Giere.

> Yet the failure of philosophers to explicate a viable notion of approximate truth must not be taken as grounds for concluding that approximation is not central to the practice of science. Perhaps the source of the difficulty is the philosophers' insistence on understanding approximation in terms of a notion of approximate truth... My suggestion, of course, is that the notion of similarity between models and real systems provides a much needed resource for understanding approximation in science. For one thing, it eliminates the need for a bastard semantical relationship – approximate truth. For another, it immediately reveals – what talk about approximate truth conceals - that approximation has at least two dimensions: approximation in respects, and approximation in degrees. Armed with just these distinctions, we can begin to attack other recent objections to realism. (Giere 1988, 107)

In response, I will first sketch a deflationary approach to approximate truth articulated by Peter Smith (1998). Second, I will employ the work of linguist Peter Lasersohn (1999) on "pragmatic halos" to fill in details at to how approximation works in ordinary contexts. Third, I will argue that pragmatic halos, or something very much like it, are provided by statistics in the sciences. This also has the implication that we need not invent measures of similarity out of whole cloth since we already have them on the books.

[10] To be fair, one might ask what my own view of mathematics is. If pressed, I am inclined to adopt a structuralist philosophy of mathematics (Resnik 1997; Shapiro 1997). Mathematics describes patterns with positions. For example, the natural number system is the pattern shared by any system of objects that has a specific initial object and a successor relation that satisfies the induction principle. However, I am inclined to accept *in rem* rather than *ante rem* structuralism. Ante rem structuralists claim that mathematical structures exist independently of their exemplifications whereas the in re structuralist thinks that the structures exist in virtue of their exemplifications. A different way of putting the view is that there are no mathematical objects but only mathematical properties.

Peter Smith has provided a deflationary or minimalist approach to approximate truth that I sketch here (Smith 1998). According to Smith, this approach starts with two claims,

[A] "*P*" is approximately true if and only if approximately *P*.
[Exp] The order of explanation goes from right to left across the biconditional **A**.

Thus, approximate truth reduces to understanding how the modifier 'approximately' is applied to various propositions. Let's take one such example, "The human population grows logistically." This is of course strictly speaking false. But, even when we utter such a claim, we really mean that it is "true enough" (c.f. Elgin 2004, 2017; Teller 2012). So, what we really mean is, "The human population grows logistically" is approximately true. Conjoined with **[A]**, it follows that, "Approximately, the human population grows logistically." So, how do we understand the meaning of this claim? Here we turn to ordinary contexts studied by linguists.

Consider the following question and answer.

"What time did Amy arrive?"
"She arrived at 3pm."

It is extremely unlikely that Amy arrived *exactly* at 3pm because both she probably arrived before or after 3pm and arrivals do not occur at instants. That is, "Amy arrived at 3pm" is approximately true because approximately Amy arrived at 3pm. Consider the following question and answer.

"How tall is Jay?"
"He is 5′8″."

Again, this is extremely unlikely because our height varies over time. "He is 5′8″" is approximately true because approximately he is 5′8″. In both cases, the answers are true because of pragmatic features associated with the respective utterances even if false in terms of their semantic content alone. The approximation is conveyed pragmatically. Linguist Lasersohn (1999) has termed these pragmatic features *pragmatic halos*. Here is one of his examples.

15. This ball is perfectly spherical.
17. The surface was etched with perfectly spherical grooves.

He writes,

> I suggest that when we describe an object as spherical even though its shape deviates slightly from that of a true sphere, we are employing pragmatic slack - saying something which is literally false, but close enough to the truth for practical purposes. The purpose of perfectly, in examples like 15 and 17, is to take up some of this slack, to reduce the acceptable level of deviance from the truth allowed by the pragmatics. (1999, 524)

Lasersohn defines a pragmatic halo as follows.

> Given an expression α denoting some object x, I like to think of the set the context associates with x as around x in a sort of circular cluster, so I will call this set, together with its ordering relation, the PRAGMATIC HALO of x, or, extending the terminology, as the pragmatic halo of α. (Lasersohn 1999, 527)

Thus, a pragmatic halo of an expression α denoting some object x, is the set of values the context associates with x as arrayed around x. I am suggesting that approximate truth is to understood in terms of approximation and this is convey pragmatically by pragmatic halos or something very much like it.

In modeling, pragmatic halos are provided for target systems described by models enriched by statistics. For example, to keep it very simple, our logistic equation might be approximately true of the human population provided we estimate appropriate confidence intervals on our parameters r and K and given estimates of population size are inexact.

$$\frac{dN}{dt} = (r \pm x_1)N\left(1 - \frac{N}{K \pm x_2}\right)$$

To bring Lasersohn's suggestion and modeling closer together suppose we define a model as set-theoretical predicate following Suppes (1957). As an example, consider our logistic model; here is the predicate.

x is a logistic population if, and only if,

1. $x =< T, N, r, K >$,
2. T is an interval of times,
3. N is a population abundance,
4. r and K are the intrinsic rate of growth and carrying capacity respectively, and
5. For all $t \in T$, then $dN/dt = rN\left(1 - \frac{N}{K}\right)$.

We can using biostatistics and empirical data to define a pragmatic halo around this predicate. Thus, strictly speaking, "The human population grows logistically" is false, it is approximately true with a pragmatic halo. This is because approximately the human population grows logistically. This can be made explicit of course or conveyed pragmatically with pragmatic slack. Though we may wonder whether any population is a logistic population, a pragmatic halo around this predicate extends its extension.[11] One way my deflationary approach departs from Hesse, Giere, and Weisberg is that I claim we already have the relevant measures of similarity available. We find them in statistics and model fitting; they provide our pragmatic halos. We don't need a measure of similarity *de novo* because statisticians have already developed them. Thus, we are concerned, insofar as we are, with models being *imprecisely and approximately true*.[12]

Smith's (1998) approach can be used by the similarity view; specifically with what he calls a geometric modeling theory—a GM-theory for short. A GM-theory has

[11] The logistic model assumes a constant carrying capacity, linear density-dependence, no time lags, no migration, no genetic variation, or age structure in the population.

[12] My view is not that all successful models are approximately true. Rather, it is that models which are accurate representations are approximately true. Models can be successful and not approximately true provided that satisfy other scientifically relevant aims (Odenbaugh 2005).

two components: an abstract geometrical structure described by equations M, and an application of the geometric structure which is described by sentence A which is a claim that the model replicates the geometric structure that is "read-off" some real-world system. This is in essence Giere's distinction between theoretical models and theoretical hypotheses. On Smith's account, by **[A]** we have, "$M\&A$" is approximately true if and only if approximately ($M\&A$). Since M is a theoretical definition, it is trivially true. Thus, by **[Exp]**, we have M and approximately A. On the similarity view, A is proposition that some data model is similar to some concrete phenomena. For example, the theoretical and data model maintain some minimal distance from one another. However, this reproduces the problem of how a mathematical object can represent some concrete phenomena. On the General Gricean view, this problem becomes how a vehicle, even when mathematical, can represent some concrete content. And, we have sketched a solution.

6 Conclusion

First, I have sketched how we arrived a popular view of models and modeling, the similarity view. Second, the arguments for the view fail and the ontological costs it bears are heavy. Hughes' worry is a genuine worry. Third, I offered sketch of an deflationary alternative. Models are representations like any others. We don't need some special measure of similarity since we already have statistics. Last, we don't need some fancy theory of approximate truth—we have what we need in the cognitive sciences and statistics. These sciences gives us tools for understanding how models are "true enough" which is about as much as I can claim for my own deflationary view.

References

Balaguer, M. (2001). *Platonism and anti-platonism in mathematics*. Oxford: Oxford University Press on Demand.

Beatty, J. (1980). Optimal-design models and the strategy of model building in evolutionary biology. *Philosophy of Science*, *47*(4), 532–561.

Beatty, J. (1982). What's wrong with the received view of evolutionary theory? In *PSA: Proceedings of the biennial meeting of the philosophy of science association* (Vol. 1982, pp. 397–426). Philosophy of Science Association.

Callender, C., & Cohen, J. (2006). There is no special problem about scientific representation. *Theoria. Revista de Teoría, Historia y Fundamentos de la Ciencia*, *21*(1), 67–85.

Cartwright, N. (1994). *Nature's capacities and their measurement*. Oxford: Oxford University Press.

Chakravartty, A. (2001). The semantic or model-theoretic view of theories and scientific realism. *Synthese*, *127*(3), 325–345.

Cummins, R. (1989). *Meaning and mental representation*. Cambridge: MIT Press.

Downes, S. M. (1992). The importance of models in theorizing: A deflationary semantic view. In *PSA: Proceedings of the biennial meeting of the philosophy of science association* (Vol. 1992, pp. 142–153). Philosophy of Science Association.

Dretske, F. (1997). *Naturalizing the mind*. Cambridge: MIT Press.

Elgin, C. Z. (2004). True enough. *Philosophical Issues*, *14*(1), 113–131.

Elgin, C. Z. (2017). *True enough*. Cambridge: MIT Press.

Field, H. (2016). *Science without numbers*. Oxford: Oxford University Press.

Frigg, R. (2010). Fiction and scientific representation. In R. Frigg & M. Hunter (Eds.), *Beyond mimesis and convention. Boston studies in the philosophy of science* (Vol. 262). Dordrecht: Springer.

Giere, R. N. (1988). *Explaining science: A cognitive approach*. Chicago: University of Chicago Press.
Giere, R. N. (1999). *Science without laws*. Chicago: University of Chicago Press.
Giere, R. N. (2009). Why scientific models should not be regarded as works of fiction. In M. Suárez (Ed.), *Fictions in science: Philosophical essays on modelling and idealisation* (pp. 248–258). London: Routledge.
Goodman, N. (1968). *Languages of art: An approach to a theory of symbols*. Indianapolis: Hackett Publishing.
Goodman, N. (1972). Seven strictures on similarity. In N. Goodman (Ed.), *Problems and projects* (pp. 23–32). Indianapolis: Bobbs-Merrill.
Goodman, N. (1976). *Languages of art: An approach to a theory of symbols*. Indianapolis: Hackett Publishing.
Grice, H. P. (1991). *Studies in the way of words*. Cambridge: Harvard University Press.
Hausman, D. M. (1992). *The inexact and separate science of economics*. Cambridge: Cambridge University Press.
Hempel, C. (1966). *Philosophy of natural science*. Englewood Cliffs: Prentice-Hall.
Hesse, M. (1966). *Models and analogies in science*. Notre Dame: University of Notre Dame Press.
Hughes, R. I. (1997). Models and representation. *Philosophy of Science*, *64*, S325–S336.
Kitcher, P. (1984). *The nature of mathematical knowledge*. New York: Oxford University Press.
Lasersohn, P. (1999). Pragmatic halos. *Language, 75*(3), 522–551.
Lewis, D. (1983). New work for a theory of universals. *Australasian Journal of Philosophy*, *61*(4), 343–377.
McMullin, E. (1985). Galilean idealization. *Studies in History and Philosophy of Science Part A*, *16*(3), 247–273.
Morrison, M. (2015). *Reconstructing reality: Models, mathematics, and simulations*. Oxford: Oxford Studies in Philosophy.
Odenbaugh, J. (2005). Idealized, inaccurate but successful: A pragmatic approach to evaluating models in theoretical ecology. *Biology and Philosophy*, *20*(2–3), 231–255.
Odenbaugh, J. (2011). True lies: Realism, robustness, and models. *Philosophy of Science*, *78*(5), 1177–1188.
Odenbaugh, J. (2015). Semblance or similarity? Reflections on simulation and similarity. *Biology & Philosophy*, *30*(2), 277–291.
Perini, L. (2005a). The truth in pictures. *Philosophy of Science*, *72*(1), 262–285.
Perini, L. (2005b). Visual representations and confirmation. *Philosophy of Science*, *72*(5), 913–926.
Quine, W. V. O. (1969). Natural kinds. In W. V. O. Quine (Ed.), *Ontological relativity and other essays* (pp. 114–38). New York: Columbia University Press.
Resnik, M. D. (1997). *Mathematics as a science of patterns*. Oxford University Press.
Rosenberg, A. (1994). *Instrumental biology, or the disunity of science*. Chicago: University of Chicago Press.
Shapiro, S. (1997). *Philosophy of mathematics: Structure and ontology*. Oxford University Press.
Smith, P. (1998). *Explaining chaos*. Cambridge: Cambridge University Press.
Sterelny, K. (1990). *The representational theory of mind: An introduction*. Oxford: Basil Blackwell.
Suárez, M. (2010). Scientific representation. *Philosophy Compass*, *5*(1), 91–101.
Suárez, M. (2015). Deflationary representation, inference, and practice. *Studies in History and Philosophy of Science Part A*, *49*, 36–47.
Suppe, F. (1989). *The semantic conception of theories and scientific realism*. Champaign: University of Illinois Press.
Suppes, P. (1957). *Introduction to logic*. New York: Van Nostran Reinhold Company.
Teller, P. (2012). Modeling, truth, and philosophy. *Metaphilosophy*, *43*(3), 257–274.
Thompson, P. (1989). *The structure of biological theories*. Albany: Suny Press.
Toon, A. (2012). *Models as make-believe: Imagination, fiction and scientific representation*. Berlin: Springer.
van Fraassen, B. C. (1980). *The scientific image*. Oxford: Oxford University Press.
van Fraassen, B. C. (2010). *Scientific representation: Paradoxes of perspective*. Oxford: Oxford University Press.
Weisberg, M. (2012). *Simulation and similarity: Using models to understand the world*. Oxford: Oxford University Press.
Weisberg, M. (2015). Biology and philosophy symposium on simulation and similarity: Using models to understand the world. *Biology and Philosophy*, *30*(2), 299.