Success and Scientific Realism: Considerations from the Philosophy of Simulation

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Introduction

It is perhaps not immediately obvious why a volume on scientific realism should contain an entry on computer simulation. Not all computer simulations are even directed at the natural world, and few if any aim to offer table-thumpingly true descriptions of it. They are usually tools of opportunity, not instruments of epistemic rigor. This is no exciting news for the realist or the anti-realist. When simulations are directed at the natural world, especially the physical world, they usually depend on physical theory to begin with. Computer simulations of the earth's climate, of astrophysical phenomena, of solid-state systems, etc. depend on the epistemic credentials of the basic equations that drive them. Those in turn come from theories: theories of fluids, of intermolecular forces, or even of the dynamics internal to molecules--quantum mechanics. And so the epistemic attitude we have towards simulations will for the most part bottom out in the antecedent attitudes we have towards those very theories. Realism, or not, about theories comes first. Finally, it seems unlikely we can straightforwardly settle what attitude to have towards our best theories by looking at how they are deployed in some of their least principled applications.

Not surprisingly, then, this is not an essay which aims to proselytize for realism or anti-realism. Rather, what we aim to investigate is whether considerations arising from a close look at the practice of computer simulation in the physical sciences have lessons to offer us about the opportunistic ways in which those sciences sometimes proceed—lessons that we can take back to those much more basic debates about scientific realism itself.

We think that they can. Or at least, they can to a limited degree. That is, we aim to argue that such considerations can shed light on the strength of a particular line of argument that is often mustered in favor of scientific realism: the so-called "no miracles" argument. We argue, in particular, that the success of certain techniques in computer simulation, those that employ what we sometimes call "fictions" might offer reasons for doubting the strength of the "no miracles" argument for securing scientific realism against the pessimistic meta-induction and kindred pessimistic arguments. They highlight the fact that "success" can be highly purpose dependent. We believe this is a general feature of scientific theories and models---there is no theory or model that is successful for every imaginable purpose. Physicists and other mathematical modelers are opportunistic in the ways in which they generate their successes. In what follows, we present the structure of our argument and defend it from recent criticisms. We go on to explore various consequences of our argument for the proper epistemic attitude to have regarding various features of our best scientific representations.

The No Miracles Argument and the Success-to-Truth Rule

Proponents of the "no miracles" argument, such as Kitcher (2002), have sought to secure the validity of the argument by making use of the "success-to-truth" rule. An initial, simplistic formulation that nonetheless captures the intent of the rule can be given as follows[1]:

If X plays a role in making successful predictions and interventions, then X is true.

As it stands, however, this rule has a number of known counterexamples. Phlogiston theory, caloric theory, and the humoral theory of disease all played a role in making successful predictions and interventions yet none was true. In response, realists have claimed that such theories fail as counter-examples because they did not allow for sufficiently specific and fine-grained predictions and interventions; they were not a part of mature, sophisticated science. The rule, then, has to be modified such that only those X's that play a role in sufficiently specific and fine-grained predictions and interventions ought to be considered true.

Another, structural realist, response to such historical counter-examples has conceded that there are specific and fine-grained counter examples (including, for instance, the wave-ether theory of light) and has sought to bifurcate such historical theories into two parts: (A) the part that plays no role in the successful predictions and interventions (the part now considered to have been false) and (B) the part that did play a genuine role in the successful predictions and interventions and is still considered to be true. Structural realists admit that these theories are at least partly false (due to part A) but maintain that there is a core structure (part B) in the theory that explains whatever predictive and interventional success the theory has. To account for this, the "success-to-truth" rule has to be qualified to only apply if X, in its entirety, plays a genuinely central role in the successful predictions and interventions.

Additionally, it is clear that many scientific theories, for example Newtonian mechanics or punctuated equilibrium, omit details and are not perfect representations of their target phenomena, but still have a history of successful predictions and interventions. Moreover, these theories can be expected to have projectable, future success, and they are not simply successful due to repeated ad hoc qualifications. Consequently, the success-to-truth rule must be further qualified such that (1) its consequent does not speak of X's truth tout court but only of "approximate truth" or truth in "some qualified sense", and (2) that the success of X in predictions and interventions be systematic and not merely ad hoc.

Lastly, the rule must also be limited so as to only apply to those X's which have representational content. As (Winsberg, 2010) points out, "[n]No one would deny that a calculator, a triple-beam balance, and even a high-energy particle accelerator can all play genuinely central roles in making specific and fine-grained interventions" (pg. 126). These entities, however, are not the sort of things that can be true or false, or to represent accurately or inaccurately, and, so, are not the targets of the "success-to-truth" rule. The rule must only apply to the right sort of X.

The considerations made so far lead us, following Kitcher, to a more plausible rule:

If . . .

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[1] In earlier work, (Winsberg 2006a, 2010) one of us has argued that considerations arising from a close look at the practice of computer simulation in the physical sciences provide counter-examples to the "no miracles" argument for scientific realism. This section mostly follows the presentation of the issue in those texts.

(the right sort of) X (in its entirety) plays a (genuinely central) role in making (systematic) successful (specific and fine-grained) predictions and interventions.

Then . . .

X is true (in some suitably qualified sense).

The commonly cited historical examples of false-but-successful theories and models now fail as counter. If any X is to be a counter example to the more sophisticated rule, it must meet the following conditions:

i. X must play a genuinely central role in making predictions and interventions
ii. The successful predictions and interventions X plays a role in must be (a) specific and fine-grained, (b) systematic, and (c) projectable.
iii. X must not be separable into a part that is false and a part that does the relevant success-fuelling work.
iv. X must be a relevant sort of representational entity.
v. X cannot plausibly be described as true even in some suitably qualified sense (e.g. as "approximately true").

It is clear that (i-v) can not be satisfied by any of the usual suspects drawn from the history of science. More promising candidates, meeting all the requisite conditions, can be found from computational fluid dynamics, as well as some multi-scale modeling in nano-mechanics. Artificial viscosity, vorticity confinement, and silogen atoms are considered in Winsberg (2006a, 2006b). Here, we review two of these examples (artificial viscosity and vorticity confinement) and present two new ones: vortex particles and synthetic thermostats. We discuss present these new examples in part to show how ubiquitous these sorts of methods are, but also to highlight that they exist both in continuum modeling (artificial viscosity, vorticity confinement) and particle-based modeling (silogens and synthetic thermostats), and also that they sometimes involve fictitious forces, and sometimes fictitious particles.[2]

A Nice Derangement of Examples

Artificial viscosity

Artificial viscosity was initially a product of the Manhattan project. As John von Neumann's team used simulations to study the dynamics of shockwaves, it was apparent that shock waves—which are highly compressed regions of fluid undergoing rapid yet still continuous pressure change (rather than instantaneous pressure change)—could not be modeled as such. The simulations simply are not fine-grained enough to allow direct modeling at the molecular level at which the shock front occurs. If the study of shock waves was to be made computationally tractable, it would require a more coarse-grained model. The problem, however, is that a more coarse-grained model results in oscillations around the shockwave that, given the evolution of the shock wave over time, results in an erroneous model of shock wave dynamics

[2] For a discussion of silogen atoms see Winsberg (2010 or 2006b). We do not discuss them in detail here in part because the methods they are involved in do not seem to have become as successful as they looked to do some years ago.

and leads to unreliable predictions. If these oscillations could somehow be damped, the reliability of the model would remain intact. To do this, von Neumann's team inserted a viscosity-like variable that is a function of the square of the divergence in the velocity field (which is of significant value only close to the shockwave) to damp the unwanted oscillations. This variable, called "artificial viscosity," had a value far too high to correspond to any real-world feature, yet it became indispensable for making a wide range of fluid dynamic computations; the predictive and interventional power found in many fluid dynamic models is parasitic on the use of artificial viscosity. It is unlikely, for instance, that von Neumann's team could have succeeded in reliably modeling shock waves if it wasn't for the employment of artificial viscosity.

Artificial viscosity, then, clearly meets condition (i) of the "success-to-truth" rule, it plays a genuinely central role in making predictions and interventions. Condition (ii) is also met: (a) the predictions and interventions that artificial viscosity allowed for in the Manhattan project, amongst many others, are specific and fine-grained; (b) the success of artificial viscosity is not ad hoc and stems from its use in novel circumstances; and (c) its history of success in specific, systematic, and novel circumstances makes its success projectable.

Moreover, artificial viscosity cannot be separated into real and non-real parts; the whole notion of such a high-valued viscosity-like term is a fiction in its entirety designed to set simulations aright. Thus, it satisfies condition (iii). Regarding condition (iv), artificial viscosity requires one to assume a structure for the shock-wave (or other fluid dynamic phenomena). And, given that such an assumption is the sort of thing that could be true or false, artificial viscosity is the right sort of entity to meet condition (iv). Moreover, the structure assumed is one that the real-world systems simply do not have; meaning that a well-known falsehood has to be assumed in the construction of the model if it is to have predictive and interventional success. Thus, condition (v) is met as well. The upshot is that artificial viscosity speaks against the success-to-truth rule, and against realism relying on this rule.

Vorticity confinement and vortex particles

Artificial viscosity is not the only non-physical “effect” that can be put to use in reliable simulations. Another example from computational fluid dynamics is what is known as Vorticity Confinement (Steinhoff and Underhill 1994). Vorticity confinement is similar to artificial viscosity in that it is an artificial construct used to overcome a fundamental limitation of discretizing the flow of a fluid. The problem to be overcome in this case arises because fluid flows often contain significant amounts of rotational and turbulent structure that is invariable, occurring below the grid size of any reasonable computational scheme. And when that structure manifests itself below the grid scales, significant flow features can get damped out in an unrealistic manner. This undesirable effect of the grid size is called “numerical dissipation” and it often needs to be mitigated. Vorticity confinement is a method that consists in finding the locations where significant vorticity has been damped out and adding it back using an artificial “paddle wheel” force. Much as in the case of artificial viscosity, this is achieved with a function that maps values from the flow field onto values for the artificial force. A naïve reader, coming across this term for the first time, could be forgiven for thinking they had come across something not entirely unlike the Maxwell-Faraday equation. But of course, no such effect actually exists in this case.

Vortex particles are an alternative approach to addressing the same issues, but one that is employed when it is particularly important to create phenomena that have the appearance of real fluid flow (Selle, Rasmussen, and Fedkiw 2005). Thus, vortex particles are employed primarily in applications to computer graphics. We mention them here in part because they are an interesting example of a fictitious particle (and one that lives in a hybrid continuum model), but also because they highlight the fact that "success" can be highly purpose dependent.

Synthetic Thermostats

The use of fictions for the sake of more reliable models is not an idiosyncratic feature of computational fluid dynamics. Synthetic thermostats, also called "artificial thermostats," are used in the study of macromolecules, defects in crystals, friction between surfaces, and porous media, in addition to fluid dynamics (See Rondoni, Lamberto, and Monasterio, 2007). These fictional thermostats have been employed in modelling for well over two decades. Denis Evans and Mark Gillan used them to account for the lack of a temperature gradient in simulations as early as 1982, while they were used for flow calculations as early as 1980 (Hoover et.al 1980; Evans, 1982; Evans, Morriss 2007). The use of these fields as techniques thermoregulating simulation proliferated and was pivotal in the development of non-equilibrium molecular dynamical simulation, where their use has allowed for the calculation of the exact thermal transport coefficients (See Williams, Searles, and Evans 2004).

Transport coefficients describe a rate of diffusion that is the response of a system to some perturbation (for example, the shear viscosity coefficient describes the response of the system to shearing forces). *Mechanical* transport coefficients can be calculated by applying the relevant mechanical perturbation and using the constitutive relations to determine the response of the system. Note, what makes these transports "mechanical" is that they are descriptions of responses to mechanical fields such as an electric or magnetic field. Thermal transport coefficients, on the other hand, describe the behavior of the system that is driven by thermal boundary conditions. Simulating these boundary conditions, however, makes calculation of the thermal transport coefficients complicated because particles gathering near the walls lead to artifacts of the computation scheme (See Evans, Morriss 2007).

The solution, one involving synthetic fields, is to invent a fictional mechanical field to account for the missing thermal dynamics:

> We invent a fictitious external field which interacts with the system in such a way as to precisely mimic the linear thermal transport process. [. . .] These methods are called 'synthetic' because the invented mechanical perturbation does not exist in nature. It is our invention and its purpose is to produce a precise mechanical analogue of a thermal transport process. (Evans, Morriss, 2007; pg. 119)

The fictitious external field is used to create the same sort of transport processes that would be present if the system had the relevant thermal dynamics; with the transport processes adequately mimicked, the thermal transport coefficients can be calculated as the system responds to the artificial perturbation. This synthetic field accounts for the missing thermal dynamics without forcing one to model the more complicated thermal dynamics itself. In fact, Rondoni, Lamberto, and Monasterio (2007) consider the real world system's degrees of freedom as "practically impossible" to include in the simulation models. Accordingly, these fictional fields are called synthetic or "artificial" thermostats because they play the requisite thermoregulating role in the absence of a model of real world thermal processes. Now, the use of synthetic thermostats has evolved over the past few decades and it has become clear that their success is highly reliant on the conditions of the target system; in some cases, the specifics of the thermostat become irrelevant and, in others, these thermostats have led to erroneous or non-physical dynamics requiring the construction of other methods.

The use of synthetic thermostats clearly will not do for every application. It is also clear, however, that these synthetic thermostats have been pivotal in allowing for the increase in predictive and interventional capabilities resulting from non-equilibrium molecular dynamics simulations. More so than anything else, it is their significant prevalence in a wide variety of simulations over the last two decades that makes it difficult to see how such an increase in predictive and interventional capabilities would have been possible without them (See Daivis, Dalton, and Morishita 2012). Like artificial viscosity and vorticity confinement, synthetic thermostats are used to make simulations successful, yet they do not exist in the real world.

Some Objections

There are two objections to the above line of argument worth considering. The first objection has to do with approximate truth, and where, exactly, we ought to be looking for it.[3] It is, of course, part and parcel of the success of techniques like artificial viscosity, synthetic thermostats, and the like, that they can be used to build local, representative models that depict their target phenomena to a great deal of accuracy. In other words, if one is building a fluid flow model in astrophysics, say of an intergalactic gas jet, and one uses the von Neumann-Ricthmeyer method of artificial viscosity, one can use that method to create a highly realistic model of the gas jet. Shouldn't it be pointed out, therefore, that the models in which artificial viscosity appear, virtually by definition of them being successful, are themselves approximately true—or at least exhibit some close enough cousin of approximate truth to please the scientific realist?

Here we think it is worth drawing a distinction between *local* models of phenomena, like a model that depicts the inner convective structure of a star, or the inner flow dynamics of a gas jet, and the *broader* model-building principles that inform, motivate, or govern those local models. We certainly agree that it is the local models of phenomena—models that are put together using a variety of model-building tools: including not only well-confirmed theory but also bits of physical intuition and lots of calculational tools, including falsifications—that are the instances of local success. But we still insist that some of these falsifications, including those we have described above, have their own, much less local, track records of success. And as model-building tools that can be applied across domains of whatever breadth, it is these bits of representational structure that should be compared apples-to-apples to scientific theories—the domain of concern of the scientific realist. The fact that false model-building principles play a genuinely central role in making specific, fine-grained, systematic, and projectable predictions and interventions then puts pressure, we believe, on the intuition that only true theories could possibly do this.

[3] This objection and its reply is discussed in Winsberg (2006a)

More recently, we have also frequently encountered the following sort of objection: We already know that artificial viscosity is artificial! We already know that the world does not contain fluids whose viscosity is proportional to the square of the divergence of the velocity field! We already know that there is no “confinement term” in fluid dynamics, and so on. Etc. So, these cases are nothing like the cases of phlogiston or bodily humors, which were part of widely accepted theories about how combustion and disease, respectively, worked.

Jesper Jerkert has highlighted this worry rather clearly in response to a review of Winsberg (2010):

> But are we really forced by notions such as artificial viscosity and silogen atoms in scientists' models and simulations to reconsider the role of truth and realism in science? After all, are we not all aware that artificial viscosity is a fictitious entity? And don't we all know that silogen atoms do not exist? In both cases, we should be extremely surprised if we found out that artificial viscosity and silogen atoms do exist. Since the springboard of Winsberg's argument is the demonstration that there are counterexamples (such as artificial viscosity) to the success-to-truth rule, it should be important to him that the version of the success-to-truth rule he is using is the only reasonable one when discussing scientific realism. But as far as I can see, this is not so. We could simply add another extra parenthesis to the rule, stating the quite obvious clause that we do not believe something to be true in the face of strong arguments against it:
> If (the right sort of) X (in its entirety) plays a (genuinely central) role in making (systematic) successful (specific and fine-grained) predictions and interventions (and if we have no other strong reasons for believing X not to be true), then X is (with some qualification) true. (Jerkert 2012; pg. 174)

There are a few different things to say in response to this line of argument. Recall that the point of success-to-truth rule is that it was supposed to legitimize a certain kind of inference ticket. You give me some kind of representation of the world, I check to see whether it has certain qualities (specific and fine-grained success, centrality, etc.), and if it does, I get to infer that the representation is true, or accurate, or approximately true, or whatever. I get to do this, so says a defender of the rule, no matter what other misgivings I might have had about the representation, because the possession of those other qualities by the representation would be a miracle if it were not (approximately) true. The question then, the thing that might be contested, the very thing that the examples from simulation are meant to shed light on, concerns the degree to which that inference ticket is defeasible. Given that, it is illegitimate in the present dialectic to include among the list of qualities that go on the input side of the inference ticket, a proviso to the effect that the inference ticket is not known to be defeated. The very question was: if I'm worried about the veridicality of some representation, should its possession of those qualities necessarily put my worries to rest.

By way of analogy, suppose, for example, that you are a diamond dealer, and you would like to know whether a certain check list of properties can assure you that a purported diamond is not fake. Maybe you think something like the following: if a stone is brilliant, can cut glass, and cannot be smashed with a hammer, then it's a diamond. Now you want to know whether this rule is indefeasible, so you ask a friend to bring you some fake diamonds to make sure that none of them pass the test. Your friend brings over ten fake diamonds, and to your dismay, four of them pass the test. "Don't worry," your friend consoles you, "all we need to do is to amend your test by adding one more criterion. As long as the stone is brilliant, can cut glass, can't be smashed with a hammer, and *you don't have any reason to believe it's not a diamond*, then you can be sure it's a diamond. None of the stones I brought are counter-examples to the reliability of that test." This suggestion seems illegitimate given that the very reason you were trying to devise a test was to allay your suspicions about fake diamonds. Note, furthermore, that this test is logically insusceptible to counter-examples, since any stone you knew to be a non-diamond wouldn't count.

The anti-realist should feel the very same way about Jerkert's suggestion. The anti-realist is suspicious of assurances that all scientific representations (with various credentials) are genuinely veridical. The point of the success-to-truth rule was to convince her that she is being paranoid, that her suspicions are far-fetched. It would be a miracle, we are meant to be assuring her, if representations could do what they do without being veridical. But now, we are meant to think, the success-to-truth rule only applies in the absence of doubt to begin with.

Relatedly, given that the whole point of the success-to-truth rule is that it is supposed to ground a no miracles argument, the success to truth inference ticket needs to be virtually indefeasible to make any sense. It is supposed to be so indefeasible that finding a counter example to it would be a miracle. The very intuition behind the rule, and the no miracles argument, is that the only thing that could possibly play a (genuinely central) role in making (systematic) successful (specific and fine-grained) predictions and interventions is something true or at least approximately true. If there are any counter examples to this, they should undermine that intuition, regardless of the provenance of the counter-examples. So even if we try to adapt Jerkert strategy and try to add a clause like "and the representation was not specifically concocted for such and such purpose," the anti-realist should not find any comfort in this.

Thus, the move that Jerkert and others seem to want to make here misunderstands the dialectical situation. The examples from simulation like artificial viscosity are not meant to underwrite scientific anti-realism. The argument does not go: artificial viscosity is not real, so therefore neither are electrons. The argument goes like this: for whatever antecedent reasons, the anti-realist wants to place limits on the epistemic scope of science. She doubts that all mature scientific representations should be trusted to the degree that the realist claims. Her interlocutor is trying to convince her that there is a useful and reliable inference ticket that will allay her suspicions. He proposes the rule. Her job then becomes to produce counter-examples to his purported rule, and his job is then to make plausible modifications to the rule to rule out her counter examples. Having understood the dialectic this way, it should be clear that the modifications to the rule cannot include conditions on the antecedent degree of belief we have in the representation in question since this is exactly what is at issue between the anti-realist and realist.

In short, the point of the examples was not to provide motivation to be an anti-realist. The point of the examples was to rebut an argument against the anti-realist. It was to provide , as the title of previous work ("Models of success vs the success of models") suggests, an alternative possible model of predictive and interventional success that the anti-realist could hang her hat on—one according to which there are other possible sources of systematic fine-grained and specific predictive and interventional successes other than truth. Viewed in this proper context, Jerkert's proposed modification does nothing to defuse that point.

Optimism saved? – A nod to Wimsatt

The considerations above lead to two conclusions: (1) that the target of our earlier argument is an argument against anti-realism based on the success-to-truth rule, rather than an argument for anti-realism, and (2) that defending realism from its critics by modifying the success-to-truth rule faces significant obstacles unlikely to be overcome. Indeed, insisting on defending realism via such a rule might prevent one from recognizing the myriad ways in which scientific work uses false principles in order to set the knowledge, predictions, and interventions embodied in our best models aright.

One philosopher of science who has done a great deal to advance the thesis that false model-building principles can lead to reliable local models of phenomena: William Wimsatt. For Wimsatt, the use of a false model is an aid to achieving truer representations of the world. In what follows, we argue that Wimsatt's approach can be modified and extended (there is one significant source of disagreement to be discussed, however) to account for the use of artificial viscosity and our other examples. The result is a more promising form of epistemic optimism about science than the one defended by the proponents of the no miracles argument. It is one that makes sense of fictitious model-building principles being reliable for building local models in which we can be epistemically confident.

Wimsatt (1987) list seven ways in which a model might be said to be false:

> "1) A model may be of only very local applicability. This is a way of being false only if it is more broadly applied.
>
> 2) A model may be an idealization whose conditions of applicability are never found in nature, (e.g., point masses, the uses of continuous variables for population sizes, etc.), but which has a range of cases to which it may be more or less accurately applied as an approximation.
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> 3) A model may be incomplete--leaving out 1 or more causally relevant variables.
>
> 4) The incompleteness of the model may lead to a misdescription of the interactions of the variables which are included, producing apparent interactions where there are none ("spurious" correlations), or apparent independence where there are interactions--as in the spurious "context independence" produced by biases in reductionist research strategies.
>
> 5) A model may give a totally wrong-headed picture of nature. Not only are the interactions wrong, but also a significant number of the entities and/or their properties do not exist.

> 6) A closely related case is that in which a model is purely "phenomenological." That is, it is derived solely to give descriptions and/or predictions of phenomena without making any claims as to whether the variables in the model exist. Examples of this include: the virial equation of state (a Taylor series expansion of the ideal gas law in terms of T or V.); the automata theory (Turing machines) as a description of neural processing; and linear models as curve fitting predictors for extrapolating trends.
>
> 7) A model may simply fail to describe or predict the data correctly. This involves just the basic recognition that it is false, and is consistent with any of the preceding states of affairs. But sometimes this may be all that is known."
>
> (1987, pg. 28-29)

He adds, however, that the "the productive uses of false models would seem to be limited to cases of types 1 thru 4 and 6." He continues, "[i]t would seem that the only context in which case 5 could be useful is where case 6 also applies, and often models that are regarded as seriously incorrect are kept as heuristic curve fitting devices."

How do our examples relate to these ways in which a model can be false? Note, first, that artificial viscosity is not false because of its local applicability (1); in fact, it has broad applicability, but it is simply not a correct representation of the real world systems in broad or local applications. Nor is it an idealization (2): while the value given to artificial viscosity is high around the shock front, there is nothing in the real world that even approaches it. It is neither incomplete (3), nor does it lead to a misdescription (4) between variables (it actually results in the correct descriptions of variables though it is not so in and of itself).[4] Artificial viscosity, instead, gives an entirely wrong-headed description of reality. Indeed, artificial viscosity, and the other examples discussed above, seem to be nice examples of Wimsatt's type 5 false model. Wimsatt, however, was suspicious of the idea that models that are false in way (5) could lead to reliable representations:

> Cases 5 and 7 above represent models with little useful purchase... The most productive kinds of falsity for a model are cases 2 or 3 above, though cases of types 1 and 4 should sometimes produce useful insights.

We agree about case 7. Our examples , however, present an alternative picture of the usefulness of false models (or really, what we would call model-building principles—see above) of type 5. Artificial viscosity, vorticity confinement, and, we think, synthetic thermostats show that a model that is false in way 5 may yet have tremendous predictive potential. It might seem intuitive that entirely wrong-headed models could not result in the fine-grained and specific predictions central to scientific work (indeed this is the very intuition behind the success-to-truth rule!), but such intuitions are shown, by the case studies, to be as wrong-headed as the models themselves.

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[4] The models in which "artificial viscosity" is likely to be used in are, of course, commonly incomplete and, so, are an instance of (3). However, "artificial viscosity" deserves to be treated as a model in and of itself due its history of success that is independent of any such individual model. Treated as a model in its own right, with its own qualities and properties, artificial viscosity is not incomplete but, rather, entirely erroneous.

The disagreement here with Wimsatt is clear, but subtle. Wimsatt shows that false models are of assistance to scientific work by allowing them to set aright our knowledge, interests, and predictions (See Wimsatt 1987). But he hesitates in allowing entirely wrong-headed models to play a central role in scientific predictions. Even Wimsatt felt the pull, it would seem, of the intuition behind the success-to-truth rule. The case studies here, however, require us to broaden what sorts of false models we take as conducive of useful scientific predictions and interventions. The last vestige of the success-to-truth rule must be given up: the truth or falsity of a model-building principle does not determine its predictive or interventional success. One cannot infer the degree of usefulness of a model from the nature of its falsity (at least not independent of the context of application).

This is not to say, of course, that nothing explains why some models are useful and other are not. Wimsatt (1987) says, for instance:

> Will any false model provide a road to the truth? Here the answer is just as obviously an emphatic "no!" Some models are so wrong, or their flaws so difficult to analyze that we are better off looking elsewhere.

But whether a false model is of use has more to do with its context of application then it does with the seriousness of its falsity simpliciter. Artificial viscosity is an entirely wrong-headed representation of the area around shock fronts, but its context of application explains in toto why it works. There is no miracle here: because simulations in fluid dynamics cannot be fine-grained enough to allow modeling at the molecular level (leading to a false description) another false model (artificial viscosity) is inserted to counter-act the erroneous results of the initial coarse-grained model (it is as if the wrong-headedness of each model cancels out that of the other). In the case of synthetic thermostats, a fictional force is used to imitate the dynamics that would occur if thermal dynamics could be efficiently inserted into the simulations.

Following Wimsatt, one might subsume such a use of false models under a higher level optimism about science: whatever false model-building principles are used in science, they are used in order to provide a better picture of reality on a higher level of analysis. So, while the surrounding area around the shock wave may not have the structure assumed by artificial viscosity, assuming such a false structure allows one to predict and describe what happens in fluid dynamics with more reliability and precision; a false model-building principle, artificial viscosity, is used to produce a more reliable (perhaps one could say, if one wanted: a truer) model of shock wave dynamics.

Conclusion

The success-to-truth rule, in its various guises, is underpinned by the intuition that truth and success (properly construed) are coextensive. The above considerations from the philosophy of simulation provide us with the opportunity to test the viability of this intuition. The result, we think, is that this broadly realist intuition behind the success-to-truth rule should be abandoned. Even though models like artificial viscosity, vorticity confinement and particles, as well as synthetic thermostats are outright fictions, their "falsity" does not exclude them from not only being part of successful models but from having their own characteristic success in so far as predictions and interventions are concerned.

Once success and truth part ways, there is nothing to drive the no miracles argument and any realist account supported by the argument loses force. Modifications to the rule have traditionally been the preferred route. But they all, for the considerations provided above, fail to successfully account for our examples. Moreover, it is hard to see, once one accepts that "fictions" play a role in computational modeling, what the rule has to recommend it.

As stressed above, this need not mean that one abandon a higher level optimism about science. Rather, the foundation for such an optimism should not stem from a conflation between truth and success. Indeed, we think that the ability of simulators to set their models aright *in spite of* the impossibility of a direct model of the real world systems is a reason for a broad epistemic optimism about such scientific work. The fact that not only true models but also fictions can be successfully used in scientific modeling both enhances our understanding of the predictive and interventional power of scientific work and broadens our view of the toolkit that scientists, especially simulators, have at their disposal.

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