Modeling Reality

Christopher Pincock, Purdue University (pincock@purdue.edu)[1]

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Abstract: My aim in this paper is to articulate an account of scientific modeling that reconciles pluralism about modeling with a modest form of scientific realism. The central claim of this approach is that the models of a given physical phenomenon can present different aspects of the phenomenon. This allows us, in certain special circumstances, to be confident that we are capturing genuine features of the world, even when our modeling occurs in the absence of a fundamental theory. This framework is illustrated using models from contemporary meteorology.

I.

Traditional scientific realism is the view that science aims at truth and that we have some reason to believe that our most successful scientific theories are true or approximately true. Realists typically appeal to the predictive success of these theories when challenged to say exactly why we should place such confidence in these theories. These realist arguments can take the crude form of asking why you would get on an airplane if you did not believe the theories underlying its construction. But, more often, realists develop sophisticated explanatory arguments for their position. For example, Psillos argues that the best explanation of the success of these theories is that they are true or approximately true, and that we have a reason, in this case at least, to believe that the best explanation of this phenomena is true (Psillos 1999).

Explanatory arguments for scientific realism have been challenged by two quite different groups. On the one hand, there are anti-realists who rest at the level of generality of their realist opponents, and who consequently argue that we never have any reason to believe that our most successful scientific theories are true or approximately

[1] This essay is a draft. Comments are welcome, but please do not quote from this paper.

true. Global anti-realists offer alternative and supposedly more tractable goals for science, as with van Fraassen's empirical adequacy (van Fraassen 1980). But, on the other hand, there are anti-realists who descend to the messy details of scientific practice and use this local perspective to undermine the arguments for global realism. It is of course here that we find most of the work on models and simulations, from Cartwright's *How the Laws of Physics Lie* (Cartwright 1983) through Giere's recent *Scientific Perspectivism* (Giere 2006). While not all of these authors draw the same conclusions about the resulting conception of scientific knowledge, the challenge to global scientific realism is relatively consistent. It takes the form of what I will call "the argument from modeling". Perhaps nobody has presented the argument from modeling in the exact way that I will here, but hopefully it is close enough to the concerns about realism that modeling practice raises.

The argument from modeling emphasizes the limitations that our successful scientific theories face in motivating the details of the scientific models that are used in deriving conclusions about physical systems. These limitations are manifest in the widespread idealization or otherwise seemingly ad hoc techniques of model construction that invariably appear whenever we turn to the details of some scientific practice. If these moves are not motivated by the theory in question, the argument continues, then a crucial part of the success of the theory in prediction and testing is exposed as unrelated to the truth of the theory. In particular, it becomes less plausible to claim that the truth of the theory would explain the success of the theory because so many steps in the successful application of the theory depend on non-theoretical assumptions.

What results from this argument is a kind of limited anti-realism which concludes that the presence of non-theoretical assumptions in modeling practice should undermine our confidence that the theory yields true claims about the situation modeled. This limited anti-realism takes several forms. In Cartwright, we are told that we should doubt the scope of the regularities observed in our successful modeling contexts (Cartwright 1999).[2] Still, for a group that takes scientific practice so seriously, there is a strange disconnect between these pessimistic conclusions and the optimism found in the works of the scientists themselves. Scientists are in many, though by no means all, cases confident that, despite whatever idealizations or ad hoc adjustments they may have made in their modeling practice, they have justified a claim about a genuine physical phenomenon whose scope vastly exceeds the limits that someone like Cartwright would impose. The challenge, then, is to uncover the reasons that scientists have for making these limited realist claims, and to see to what extent these reasons can be grounded in reality.

II.

Here I want to lay out a template for arguments for the conclusion that we know some aspect A of some system (or type of system) S. The premises of instances of this template, when well justified, will give a scientist a good reason to believe the instance of the conclusion. In line with the limited realist conclusions that I wish to draw here, I will be happy to grant that these conditions are not always met in scientific practice. But they are met in many cases where models exceed the scope of a scientific theory. The basic line of attack that I will develop is to draw a distinction between the different parts of a scientific model and what these parts represent. For a model with parts P_1 through P_n, we can have a good reason to think that P_1 accurately represents some aspect A_1 of the

[2] See also Morrison 2005.

system even when we lack a reason to think that the remaining parts accurately represent other aspects of the system. This division will allow us to place conditions on when an idealization or non-theoretical assumption can coexist with a limited realist conclusion.

To fix some terminology, I will think of a model as a wholly mathematical entity combined with a series of propositions about how the parts of the mathematical entity correspond to the physical features of a system. For example, in a configuration space model of some system of n physical particles, the mathematical entity might be some subset of the set of $\mathbf{R}^{3n+1}$ (3n+1-tuples of real numbers). The propositions that are part of this model will then relate the first three entries in each triple to spatial coordinates of a physical particle and the last entry to the time at which these particles are at that spatial position. Taken together the mathematical entity and these propositions impose a vast array of conditions on the system. These conditions are the content of the model. When these conditions are met, we say that the model accurately represents the system.

In the example given it might seem that all the features of the mathematical entity are paired up with features of the physical system. However, a bit of reflection shows that there are all sorts of mathematical features of this mathematical entity that have no impact on the content of the model. For example, it may be that there is an equation in 3n+1 variables that is satisfied by all and only the 3n+1-tuples in the model. But this is irrelevant to whether or not the model accurately represents the system because the propositions that relate the mathematical entity to the system make no appeal to this mathematical property. For any model, there will always be some surplus mathematical structure, i.e. mathematical properties of the mathematical entity that do not figure into the representational content of the model.

In many cases of modeling we can think of a theory as picking out a large class of models. This class might include all mathematical entities satisfying a given equation or series of equations, along with the propositions relating some of the features of these entities to aspects of a physical system. A theory of classical physics, for example, might lead to a series of subsets of $\mathbf{R}^{3n+1}$ that satisfy the equations of the theory. Each model will then represent a system with a number of admissible trajectories, i.e. trajectories consistent with the laws of the theory. This class is completely impractical to work with, and so some measures must be taken to isolate some smaller collection of models whose accuracy we have some chance of assessing. It is precisely here that steps related to idealization and other kinds of non-theoretical assumptions threaten our confidence that we end up with an accurate model. For suppose I add a condition on my models that is not tied to the theory. Now it looks like I have simply shifted my attention to a new model M that is unrelated to the models that I began with. And whatever success I might have working with M will have no implications for the truth of the conclusions that I draw from that model's success. If the imposed condition is otherwise unmotivated, then I seem to have no reason to think the model I have ended up with is accurate.

The way out of this difficulty is to recognize that we can adopt a more nuanced conception of the accuracy of a given model. On this new approach, we will say that a model is accurate with respect to aspect A of the system when its content concerning A is correct. As we have seen, the content of a model is a product of two features: the mathematical entity and the propositions relating features of the entity to aspects of the system. Clearly a model can be accurate with respect to aspect A without being accurate with respect to aspect B. Given this independence, if we move from model M_1 to model

M_2 while preserving the claims about aspect A, then we can assess M_1's accuracy with respect to aspect A by checking M_2's accuracy with respect to aspect A. This is so even if model M_1 and model M_2 differ in dramatic ways. In particular, model M_2 may involve non-theoretical assumptions. As long as these non-theoretical assumptions leave the claims about aspect A untouched, model M_2 can be a reliable guide to the A-aspect accuracy of model M_1.

To illustrate the consequences of this approach, let's see how it can be used to defend the widespread idealization of treating a discrete system as continuous. Suppose that our best theory of fluids represents some fluid as composed of discrete particles moving rapidly, colliding with each other and the boundaries of some container and preserved together by some complicated nexus of gravitational and chemical forces. In many cases, we use a model that represents the fluid as continuous, i.e. as being composed of point-like particles whose motions are not due to chemical forces. My claim is that even though the content of this model disagrees with any model that fits with the theory, it can still be the case that the model agrees with some models that fit the theory in some aspects. For example, the two models might agree on whether or not a piece of wood would float on the surface of the fluid. If we could track the differences between the two models and ensure that the two models agreed in this respect, then the success of the idealized model in predicting that the wood would float is a reason to think that the non-idealized model is accurate with respect to this aspect of the fluid.

It would be foolish, of course, to try to extend this argument into an argument that the non-idealized model is accurate with respect to all aspects of the fluid that it represents. So, the template that we have developed cannot by itself vindicate our hope that there is a model associated with the theory that is completely accurate. At the same time, there is every reason to think that other idealized models could be developed that would match our original model in some other respects. If this is repeated enough times, we may have reason to think that the model that accords with the theory is accurate in a wide range of respects.

Here, then, is a rough outline of the sort of argument that can provide a scientist with a reason to think there is a model that accurately represents some aspect of some system under consideration:

> (1) There is a successful theory T whose models include a model M that represents aspects $A_1, \ldots, A_n$ of system S.
> (2) There is a model M' that agrees with M with respect to its representation of aspect A_1 of S.
> (3) M' is accurate with respect to aspect A_1.
> Therefore, M is accurate with respect to aspect A_1.

III.

Establishing instances of premises (1)-(3) in any particular case can be extremely challenging, and scientists have developed any number of different techniques to convince themselves and their colleagues of such premises. I will focus on three different modeling techniques which correspond roughly to three different goals that a scientist might have at a given stage of inquiry. These different goals will further clarify the different kinds of aspects of a system that I have been alluding to so far. The first concern that a scientist might have is to uncover the ultimate causal mechanisms responsible for a phenomenon of interest. A second concern would be to make accurate predictions or retrodictions about observable features of the system. The third concern I will discuss is to isolate structures that recur in a type of system and that may or may not track unique underlying ultimate causal mechanisms. With some scientific problems, some of these concerns may come together. For example, with the continuous fluid model discussed above, we were interested both in making an accurate prediction (would the piece of wood float?) and in isolating a recurring structure of some fluids (wood will float on it). But our idealization erased the representation of the ultimate causal mechanism responsible for the floating and replaced it with point-like elements that work differently. In this respect, then, our continuous model is inaccurate and would be a poor choice to try to understand the causal mechanisms responsible for floating.

As this case illustrates, accurate prediction of observable aspects of a system does not require a model that represents underlying causal mechanisms. More generally, our best predictive model for some observable phenomenon may not represent the causes of that phenomenon. Our best causal model may be very bad at making accurate predictions. Finally, we may have a model that accurately isolates a recurring structure without accurately representing the causes or making accurate predictions about observable features.

Examples of cases where different models cooperate to generate accurate representations of different aspects of a system are not too difficult to come by if we consider a system organized on several different spatial and temporal scales.[3] An ecological system, say, will have a large number of very small physical parts. According to our best physical theory, these parts are responsible for the ultimate causal mechanisms that give rise to the observable features of the system, e.g. how many organisms there are. But our best predictive model of how many organisms there will be over time will not make reference to these causal mechanisms. Instead, it may consider predator-prey relationships that erase the internal physical complexity of each organism. Finally, we

[3] Here I have been strongly influenced by Batterman 2002 and Wimsatt 2007.

may develop a model that seeks to account for a recurring feature of ecosystems of some kind. As explained by Weisberg, for example, biologists noted an asymmetric response to "a general biocide" across predators and prey (Weisberg 2006). This phenomenon was robust in the sense that it did not depend on many of the particular features of the system. For our purposes, the key point is that the models used to explain this recurring phenomenon do not accurately represent the ultimate causal mechanisms at work in the system.[4] At the same time, they fail to provide predictions about the behavior of any particular ecosystem because the whole purpose of developing this model was to understand a phenomenon that recurred across ecosystems. So, we have a case where three different kinds of models can be accurate in different respects. As long as we are clear on what kind of modeling purpose is in play, each kind of model can be used to support the conclusion that we are getting this or that aspect right about the system.[5]

IV.

Up to now I have been somewhat cavalier in talking about the articulation of the models of our best theory through appeal to non-theoretical assumptions like idealizations. A reasonable worry about the limited realism so far presented is that my optimism may depend simply on laziness. That is, if I actually worked through even one example of how this works in any detail, I would quickly see that there is no reason to think that the instances of the premises of my argument template are ever met. The charge of lazy optimism is indeed a charge that has been leveled by Mark Wilson against

[4] Weisberg sometimes speaks of revealing "causal structure" (Weisberg 2006, 739) through robustness analysis. This goes beyond what I would count as an ultimate causal mechanism, although it is not clear if this difference is anything more than terminological.

[5] Here I follow Parker 2007, but her conception of model pluralism may differ in some respects from the view defended here.

a view similar to my own (Wilson 2006). Let's see how this objection works and if it can be countered.

One dimension of Wilson's concerns turns on the need for boundary conditions for a determinate model to be selected. Strictly speaking, these conditions will always be non-theoretical assumptions according to the way I have been using the term. This is because they do not follow from a theory whose scope includes all systems of a given kind. Boundary conditions not only impose a spatial boundary around a system which may be more or less artificial, but generally involve unrealistic claims about exchanges across the boundary. For example, we may treat our system as completely isolated. No actual systems are completely isolated and so the need for boundary conditions raises a special problem for my proposal.

An initial response to Wilson's challenge based on boundary conditions is to claim that there still remain cases where the false or otherwise unmotivated assumptions about the boundary leave unaffected the content of the models concerning other aspects of the system. If this is ever the case, and scientists can be in a position to establish that it is the case, then the premises of my argument template can be justified and some form of limited realism remains possible. To return to our fluid case, in our continuous model of the fluid we tacitly imposed the boundary conditions that the fluid could not escape through the walls of the container and that there was no loss of fluid through the top of the container. This boundary condition is of course false as there is sure to be some evaporation. But I hope it is not too difficult to see that getting this aspect of the system wrong does not undermine its accuracy with respect to the aspect of the system that we are concerned with, i.e. the wood floating. Adopting a model with these boundary conditions is perfectly consistent with showing agreement with some theoretical model in some respects. It would be a different story if we could not divide up the content of the model in the way that I have been assuming or if the inaccurate boundary conditions related directly to the aspect of the system we were concerned with. But in those cases where we can partition the content in this way, no barrier to limited realism arises.

Wilson has a more fundamental worry about this approach, however, which brings in more general problems concerning concept possession and reference to physical properties. Wilson uses the patchwork character of our modeling practices to question what he calls a classical approach to concepts and their referents. According to this approach, it is unproblematic to assume that some of the concepts that we employ univocally track physical properties. By ostension or some other direct means, the classicist aims to attach our concepts to the world in such a way that they pick out the same physical property across time and scientific context. This "classical gluing" then generates univocal contents for the claims made by scientists, independently of the particular representational practices that they are then engaging in. Wilson rejects classicism based on the disconnect between this picture of concept-property pairing and the involved techniques that working scientists have developed to actually understand physical properties. In one of his most convincing examples he explains how the concept of hardness picks out different physical properties for different materials. The classical dogma that these sorts of concepts pick out a single property is thus exposed.

Now it is precisely this classical picture that I have assumed so far in my description of models and my argument for limited realism. To see why, notice that I identified models with the combination of a mathematical entity and a series of propositions relating the parts of these models to physical properties. Without these propositions, the model would just be a mathematical entity. So far there is no explicit reliance on the classical picture, although my failure to spend any time explaining what these propositions are or how they work suggests classical optimism. But classicism is presupposed when I assumed that models motivated by widely divergent assumptions overlapped in content with respect to some aspects of the system. That is, the models ended up representing the same physical properties even though they work quite differently. To take what might seem to be a trivial example, I assumed that both the discrete and the continuous model could be models of the very same fluid. If fluidity turns out like hardness, then these models do not represent the same aspect of the same system. If this happens, then the argument for limited realism that I have sketched breaks down.

While a full reply to Wilson's rejection of classicism would be quite involved, a defense of limited realism is easier to envision. The basic point of disagreement for any particular case is whether or not the propositions that are constitutive of one model transfer over to the propositions that are constitutive of the other model. In the simplistic example of a discrete and continuous model of a fluid, I think it is unproblematic that the two models overlap in content to some degree. In other cases, there may be more debate, and resolving this debate would be a necessary step in justifying the premises of the argument for that instance of limited realism. It may happen, for example, that the hardness claims of the two models are so divergent that there is no univocal concept of hardness that figures in both models. When this happens, we cannot be confident that the two models are equally accurate with respect to hardness. We may wind up with fewer justified cases of limited realism, but I do not see why such problems should lead us to abandon a properly qualified defense of scientific realism. In fact, it seems that Wilson allows the point I am making here when he rejects an "indiscriminate holism" (Wilson 2006, 69) that would insist that the content of two models can never overlap.[6]

V.

To bolster my proposal I conclude with a brief case study concerning a recent debate at the intersection of meteorology, climatology and public policy. This is the controversial question of whether climate change (or global warming) causes an increase in hurricane intensity. Here we see a combination of the three modeling purposes that I have so far emphasized. To begin with, there is a question about causation, which presumably should bottom out in the fundamental particles of the system and their physical properties. Clearly there are also predictive aspects to the debate as we would predict that hurricane intensity would increase if climate change does in fact cause it to increase. Finally, we have a case of a recurring structure when we consider hurricanes. Each hurricane is different, but hurricanes are a well-defined collection of meteorological phenomena that arise across a wide range of meteorological systems.

I will take as my primary focus a recent article by Curry, Webster and Holland called "Mixing Politics and Science in Testing the Hypothesis that Greenhouse Warming is Causing a Global Increase in Hurricane Intensity" (Curry et. al. 2006). The article summarizes the objections that the authors received to an earlier *Science* article that argued for a genuine causal connection (Webster et. al. 2005).[7] Beyond this, they provide some methodological reflections on how such disputes should be resolved as well as

[6] Here I aim in part to correct my previous discussion of Wilson in Pincock 2005.
[7] H. R. Chang was a co-author in Webster et. al. 2005 but not in Curry et. al. 2006.

some practical suggestions for how scientists can communicate productively with the media.

Curry et. al. begin by presenting their “central hypothesis”: “greenhouse warming is causing an increase in global hurricane intensity” (Curry et. al. 1026). This hypothesis is then analyzed into three “subhypotheses”, which are then considered separately:

> 1) the frequency of the most intense hurricanes is increasing globally;
> 2) average hurricane intensity increases with increasing tropical SST [sea surface temperature];
> 3) global tropical SST is increasing as a result of greenhouse warming (Curry et. al. 1026).

They continue by noting that “The central hypothesis implies a causal chain 3→2→1 and therefore depends upon the validity of each of the three subhypotheses” (Curry et. al. 1026). That is, greenhouse warming causes global tropical SST to increase, which in turn increases average hurricane intensity. As a result, the observed increase in hurricane intensity can be attributed (in part) to global warming. An additional conclusion is that further greenhouse warming will produce further increases in hurricane intensity.[8]

The theories at play in this example range from the fundamental theory of fluid dynamics to more specialized meteorological theories concerning the origin and propagation of hurricanes through to the theory of global warming as a byproduct of human activity. The challenges posed by this issue can be traced in part to the involvement of phenomena from dramatically different scales. On the microscale, there are the molecular interactions between water and air. On some medium scale, we have the conditions necessary for the formation of hurricanes from tropical depressions. Finally, on the largest scale, we have the global increase in SST temperature that is

[8] There is an unfortunate equivocation concerning “hurricane intensity” in this argument which raises concerns analogous to ones raised by Wilson about “hardness”. I cannot pursue these complications here due to space constraints.

attributed to human activity. The way in which all these scales are interrelated is tracked in the different sorts of models that are used to resolve the issue.

The point I want to make with this example is that there is no single theory and no single model that is used to justify the authors' central hypothesis. Instead, different theories and different models are used to handle each of the three subhypotheses. This will not be a problem if the scientific arguments can be interpreted in terms of the different aspects of the system in line with my argument template from section II. While it is far from clear that the scientists have gotten things right here, I will argue that this is the sort of argument that should convince us of its conclusions if the individual steps can be justified.[9]

The authors justify subhypothesis 3) that global tropical SST is increasing as a result of greenhouse warming by appeal to the 2001 Intergovernmental Panel on Climate Change Third Assessment Report along with "subsequent climate modeling studies" that appeared in 2004 and 2005. These publications proceed by describing highly complicated models that are meant to simulate the global atmosphere-ocean evolution given some specified initial and boundary conditions. Typically these models represent the Earth as a grid, where each cube in the grid is treated as unstructured except for meteorological magnitudes like temperature and pressure. When these models are evaluated with respect to the 20th century, it turns out that only the models that include some factor tied to human activity can reproduce the known data. Thus because "the global surface temperature since 1970 (including the trend in tropical SSTs) cannot be reproduced in

[9] Additional support to this conclusion, based on different representations of hurricane strength, is offered in Emanuel 2005 and Sriver and Huber 2006. The latter paper is especially interesting from a methodological point of view as the authors "test more robustly the hypothesis" (Sriver and Huber 2006, p. 2) by employing different models than Emanuel 2005. The independence of these models is questioned by Maue and Hart 2007, but Sriver and Huber 2007 offers a convincing reply.

climate models without the inclusion of anthropogenic greenhouse gases", 3) is to be preferred over the "null hypothesis" that global tropical SST is *not* increasing as a result of greenhouse warming. The important thing to note here is that the models in question here do not represent any hurricane activity, let alone hurricane intensity. So, it is not possible to directly justify the central hypothesis using just these sorts of models.

This shows that a different collection of models is needed to justify hypothesis 2) that average hurricane intensity increases with increasing SST. Here Curry et. al. appeal to a theory of hurricane intensity that relates SST to the potential energy of a hurricane that forms, as well as a 2006 paper which "clarified the relationship between seasonally averaged hurricane intensity and seasonally averaged tropical SST on an individual ocean basis" (Curry et. al. 1029). A review of this latter paper indicates that this conclusion depends on a model that delineates the different contributions to the intensity of the hurricanes that formed. Our best theory of hurricane intensity is used to isolate four factors that contribute to increased hurricane intensity, including SST increase.[10] The meteorological data are then analyzed to see which factor is most responsible for the long-term trend in hurricane intensity. While short-term variability in the other three factors is part of the explanation of the short-term variability in hurricane intensity, the authors conclude that only the long-term trend of SST increase is responsible for the long-term trend of hurricane intensity increase.

Finally, hypothesis 1) that the frequency of the most intense hurricanes is increasing globally is supported primarily by consulting records of hurricane activity since 1970. Here there are potentially troubling uncertainties in the data as we would

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[10] The other three factors are "increasing specific humidity, minimal vertical wind shear, and negative stretching deformation" (Hoyos et. al. 2006, 94).

expect that our ability to find hurricanes has increased since 1970, and so the upward trend in the number of the most intense hurricanes may be merely due to our better technology. The authors thus provide what we could call a model of the data that they have at their disposal and conclude that there is no good reason to suspect that this systematic error has been committed (Curry et. al. 1028-1029). Clearly the models deployed in this analysis of the data are distinct from the models used to justify hypotheses 2) and 3).

Here, then, we see that different sorts of models are used to justify a scientific claim with broad scope.[11] If each model is considered using its entire content, then the steps in this argument are contradictory and presumably prove nothing. This shows that the cogency of the scientific argument for the ultimate hypothesis depends on the content of each model being exploited only in a very limited fashion. There is no overarching theory that could be used to motivate each of these steps, but taken individually there is a detachable claim from each part that shows something about reality. When these claims are combined, we can have a compelling conclusion about the real world.

References

Batterman, Robert (2002), *The Devil in the Details: Asymptotic Reasoning in Explanation, Reduction, and Emergence*. New York: Oxford University Press.

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

[11] Compare the apparent pessimism about these sorts of conclusions in Oreskes et. al. 1994 and Oreskes & Belitz 2001.

Cartwright, Nancy (1999), *The Dappled World: A Study of the Boundaries of Science*. New York: Cambridge University Press.

Cat, Jordi (2005), “Modeling Cracks and Cracking Models”, *Synthese* 146: 447-487.

Curry, J.A., P.J. Webster and G.J. Holland (2006), “Mixing Politics and Science in Testing the Hypothesis That Greenhouse Warming is Causing a Global Increase in Hurricane Intensity”, *Bulletin of the American Meteorological Society* 87: 1025-1037.

Emanuel, K. (2005), “Increasing destructiveness of tropical cyclones over the past 30 years”, *Nature* 436: 686-688.

Giere, Ronald (2006), *Scientific Perspectivism*. University of Chicago Press.

Hoyos, C.D., P.A. Agudelo, P.J. Webster and J.A. Curry (2006), “Deconvolution of the Factors Contributing to the Increase in Global Hurricane Intensity”, *Science* 312: 94-97.

Jones, Martin R. & Nancy Cartwright (eds.) (2005), *Idealization XII: Correcting the Model. Idealization and Abstraction in the Sciences*. Amsterdam: Rodopi.

Maue, R.N. and R.E. Hart, “Comment on R. Sriver and M. Huber (2006)”, *Geophysical Research Letters* 34: L11703.

Morrison, Margaret (2005), “Approximating the Real: The Role of Idealizations in Physical Theory”, in Jones and Cartwright (eds.) (2005), 145-172.

Oreskes, N., K. Schrader-Frechette and K. Belitz (1994), “Verification, Validation and Confirmation of Numerical Models in the Earth Sciences”, *Science* 263: 641-646.

Oreskes, N. and K. Belitz (2001), “Philosophical Issues in Model Assessment”, in M.G. Anderson and P.D. Bates (eds.), *Model Validation: Perspectives in Hydrological Science*. Wiley, 23-41.

Parker, Wendy (2007), “Understanding Pluralism in Climate Modeling”, *Foundations of Science* 11: 349-368.

Pincock, Christopher (2005), “Conditions on the Use of the One-dimensional Heat Equation”, in G. Sica (ed.), *Essays on the Foundations of Mathematics and Logic*, Vol. 2. Polimetrica, 67-79.

Psillos, Stathis (1999), *Scientific Realism*. New York: Routledge.

Sriver, R. and M. Huber (2006), “Low frequency variability in globally integrated tropical cyclone power dissipation”, *Geophysical Research Letters* 33: L11705.

Sriver, R. and M. Huber (2007), “Reply to comment by R.N. Maue and R.E. Hart”, *Geophysical Research Letters* 34: L11704.

Van Fraassen, Bas (1980), *The Scientific Image*. Oxford University Press.

Webster, P.J., G.J. Holland, J.A. Curry and H.R. Chang (2005), “Changes in Tropical Cyclone Number, Duration, and Intensity in a Warming Environment”, *Science* 309: 1844-1846.

Weisberg, Michael (2006), “Robustness Analysis”, *Philosophy of Science* (Proceedings) 73: 730-743.

Wilson, Mark (2006), *Wandering Significance: An Essay on Conceptual Behavior*. Oxford University Press.

Wimsatt, William (2007), *Re-Engineering Philosophy for Limited Beings: Piecewise Approximations to Reality*. Harvard University Press.