Models, Mathematics, and Measurement: A Review of *Reconstructing Reality* by Margaret Morrison

[Philosophy of Science 83 (4): 627-633. 2016.] 

Margaret Morrison's new book contains the most detailed development to date of her much discussed view that models are autonomous objects mediating between theories and the world. But there is much more, including a wide-ranging discussion of the role of mathematical and computational representations in science, arguments that simulations are inextricably embedded in parts of modern experimental science, and a consideration of the role of idealizations and abstractions in models. Because of the wealth of detail, this book requires close attention, but the richness of ideas pays ample rewards. The detail is important because Morrison's conclusions are carefully limited in scope and those conclusions apply to systems that are considerably more sophisticated than the stock examples we often use in philosophy of science.

The book consists of three separate but dependent parts. The first is on the role of mathematics in scientific understanding, the second is on the role of models in science, and the third is on the increasing role of computer simulations in many sciences, accompanied by an extensive discussion of the relations between simulations and measurements. There is far more here than can be covered in one review, so I shall focus on one central thesis from each part of the book. There is much effective criticism of other philosophers' positions along the way, but I shall concentrate on the constructive elements of the theses.

One of the running themes that straddles both Chapter 2 and Chapter 6 is the interplay between mathematical and empirical routes to knowledge. Morrison argues in Chapter 2 that even when empirical content is stripped from certain models, mathematical techniques and frameworks can by themselves generate information about the represented system. The topics explored here are not whether there are purely mathematical explanations of physical facts but whether the mathematical models provide physical information over and above their role in calculations. Underpinning her arguments is a distinction between abstraction and idealization that runs throughout the first part of the book. Morrison draws the distinction in a novel way. For her, abstraction is a process that allows us to describe phenomena in ways that cannot be realized in the physical world, for example by appealing to infinite populations. An important feature of many models that use abstraction in this sense is that the mathematics employed is necessary for constructing the model. Idealization, in contrast, usually involves a process of approximation whereby the system can be de-idealized by using correction factors, such as when a term representing friction is added to a model of a simple planar pendulum.

This claim about the necessity of certain representational techniques is certainly true of some examples that have been proposed as mathematical explanations, such as Euler's proof of the impossibility of traversing the bridges of Königsberg without duplicating a bridge crossing. Without the concepts of an Euler circuit or an Euler path employed in the proof, we would not understand a wide class of related problems. Morrison wants to cast a wider net. One running example she uses is the thermodynamic limit in condensed matter physics. This is an abstraction rather than an idealization in Morrison's sense because the infinite limit is not an approximation to the finite case and the mathematics used in the Wilson-Kadanoff approach to renormalization is necessary for representing the system. She provides one of the clearest accounts available of why it is possible to omit details of the physical properties and interactions at the microlevel and to provide an explanation in terms of dynamical flows and fixed points. It is this material independence that opens the way to features that are characteristic of some kinds of mathematical explanation because information about the material base is not what is central to explaining the behavior of the system. It might be thought that what is going on is not really abstraction in Morrison's sense but abstraction away from any particular instance of the representation. Morrison sees the situation differently and one can see how both the emphasis on models and the role of physically unrealizable representations underpins her approach because using renormalization group methods "...involves transferring the problem from a study on a particular system S to a study of scale transformations such that the results depend only on the scaling properties. What that requires is a shift away from the phase space of the system to a space of models or Hamiltonians." (p. 75 ) This emphasis on the necessity of the mathematics does not involve treating such models as fictions because there are choices that can be made with modeling, including fictional modeling, that are not available for the necessary components of abstractions as Morrison has defined them. Given the prevalence of alternative models for any given system, it would have been useful to hear more about how we can know that a given mathematical representation is necessary but perhaps this is relative to a given representation structure as when certain axioms are shown to be necessary within a given axiomatization

Turning to Part II of the book, Morrison does not intend to present a general theory of representation, but one that is attuned to the differences between different types of models – indeed, her view is that such a general theory is not possible. Chapter 4 addresses the mediating role of models. She distinguishes two ways in which models can mediate between the world and our theories of it. In the first we lack knowledge of the target system and we construct a model to learn more about hypothetical features of that system. In the second case the starting point is the theory itself and the model is used to pick out abstract and idealized features of the theory to represent specific features of the target system. Because the term `represents' has multiple uses, the most appropriate way to interpret `represents' in Morrison's use is in the sense of `standing in place of' as when a representative sample stands in for the whole population. This use captures both the role of the model in standing in place of the theory and in its role of standing in for the real system.

Using a detailed examination of the modeling considerations that led to the development of Cooper pairs in the BCS model of superconductivity, she argues, persuasively in my view, that the model was well motivated by physical considerations rather than being ad hoc. As she puts it: "the fact that assumptions about Cooper pairing and interacting states are not derived directly from quantum theory does not, in itself, make them ad hoc, unless of course one classifies the construction of representative models as itself an ad hoc process" (p. 139). This is correct. Quantum theory is an extremely general representational apparatus and has to be supplemented with principles stemming from electrostatic theory, weak, strong, electromagnetic, and gravitational theory, statistical mechanics, and many other areas. Although there is no doubt that some models are conjectural and ad hoc, and Morrison notes that Maxwell's model of the ether was of that sort, most are constructed on the basis of specific knowledge of the system. Indeed, Morrison may have underemphasized the mediating role of models. She gives as examples cases where the size or distance of the target system prevents detailed knowledge of the system and the model represents how we assume the system behaves. This is a recognizable form of exploratory modeling (a term I believe is less provocative than `experimental models'), but even in cases where the system is directly observable, such as with models of cloud formation, most models have this assumptive character.

It is frequently said that anything can represent anything. Morrison effectively responds to this by noting that theories put constraints on what the space of physical possibilities can be, as do properties of the represented system. If models are mediating agents between theory and a system they are required to conform to those constraints. To this it might be replied that since we have discrete models of systems that theory says are continuous, and that models rarely represent properties of systems exactly, these constraints can be violated. The appropriate response to this objection is to note that since the mediating models have the function of linking theory to the system, they will be unable to carry out this function if they are incompatible with the data. For example, if a system producing binomially distributed data is incorrectly modeled by a power law distribution, the statistical model will be rejected using standard statistical methods. It is only in a completely trivial sense that the laissez faire view can be maintained. Morrison's position then entails that what counts as a model is not just a matter of stipulation that can be considered apart from the uses of a theory. As she says "The point here is that we can't use just anything to represent the helium atom if the goal is to understand its structure; nor is the decision based on purely pragmatic considerations.… [The users] of the theory determine what they want from the representation, but which representation will deliver on that request is not a decision that rests solely with them" (pp. 128-129).

Inconsistent models are evaluated at length in Part II. The primary goal is to investigate whether and how using many different models to describe the same system results in inconsistency and if it does, whether the inconsistency affects the epistemic status of the information that models give us. The problem of idealization is taken to be a variation on the problem of inconsistency because the idealizations in the current models and theories are usually inconsistent with a realistic description of the target system. Morrison argues that there is a difference between (a) the type of case when mutually inconsistent but complementary models that are consistent with fundamental theory are used to represent different aspects of turbulence and (b) the case of models of the nucleus for which the different models are inconsistent with background theory and where there is no real theoretical understanding that can be brought to bear allowing us to choose between the models. Morrison notes that both laminar and turbulent flow occur in fluids, but because each type results from different boundary conditions we should therefore not try to impose a uniform treatment on the motion of the fluid as a whole. This is not perspectivism but a rational response to reality. As always, the world determines what kinds of models are suitable or unsuitable for a given system.

The nuclear models case is where abstraction comes back into play. Here false assumptions are necessary to make accurate predictions, but not because of computational difficulties. A major problem is that physics tells us that there are interactions within the nucleus due to residual strong forces, which are short range, but these cannot be used to derive properties of nuclei. The contrast with models of turbulence is that whereas specific conditions, such as the geometrical configuration of obstacles to the flow can necessitate different models, in the nuclear case, there are inconsistent models for the very same system. However, I would dispute the moral that Morrison draws from her discussion of different models of the nucleus. She concludes that although the many models are mutually inconsistent, this is a scientific problem and not one to which philosophy has anything to contribute. One might differ on that score. Wave-particle duality was originally thought to involve inconsistent attributions of properties to quantum mechanical entities, but the Copenhagen interpretation, flawed as it was, did make progress in eliminating the inconsistencies and the interpretation, although constructed primarily by scientists, was recognizably philosophical in form and content.

There has been considerable discussion about whether data from computer simulations can replace data from material experiments, a topic Morrison addresses in Part III. To her credit, Morrison holds the view that pointing out that there are analogies between the simulation and experiment does not get us very far. What is important is what epistemic role in science the two types of data can play. Her aim is to argue that in some but not all cases computer simulations can provide data that serve as measurements of theoretical quantities and so have a similar epistemic status to experimental data. This once again involves the use of models as mediating devices. Morrison's argument strategy is to note that there is a close connection between models and experiment and since simulations are a particular type of modeling this gives us a connection between simulations and experiments. Furthermore, it is the function of models as measuring devices that is crucial in establishing models as experimental rather than as mere calculation devices because this allows us to classify the output of simulations as measurements. A key argument used to support these claims is that both experiments and simulations require multiple layers of models in order to be effective and her description of how the interplay between theoretical models, simulation models, and experiment established the existence of the Higgs boson is a powerful example in support of her position. The treatment is far and away the most sophisticated account of the role that simulations play in experiments and shows just how difficult it is to disentangle the two in particular cases.

One aspect of simulations is important to note. Morrison argues that new knowledge is produced by simulations because they allow us to draw out implicit content from the model. This is certainly correct, although I would argue that the implicit content made explicit is not independent empirical knowledge, even though the path from theory to simulation model is not the traditional deductive path. As well as the discretization of the theory to accommodate the degree of resolution of the simulation, approximations are frequently used that may or may not be scientifically justified, not to mention the difficulties involved in program verification, a topic Morrison explores in illuminating detail in Chapter 7. That discussion is the best philosophical assessment of verification and validation available and those interested in this important topic are urged to read it.

Morrison agrees that a material experiment is necessary in order to establish the existence of something. If we generalize this point to include data from a material source in nonexperimental contexts, it seems that the criteria for this are shifting. A new planet, currently called Planet Nine, has been postulated on the basis of observed alignments in the orbits of objects in the Kuiper Belt. The orbit and gravitational effects of the new planet were calculated using mathematical models and computer simulations. At the time of writing the observational search for the planet had just got under way. One might ask, is it necessary to observe the planet in order to justifiably claim that it exists? Skeptics will point to the historical example of Vulcan as a reason to deny that such a discovery can take place purely by calculation. But the incorrect prediction of the existence of Vulcan resulted from a fundamental defect in Newtonian mechanics, whereas the calculations underpinning the postulation of Planet Nine are sufficiently well-founded that one could argue we have high enough degree of certainty that the actual visual confirmation of the planet is superfluous. Visual confirmation is in any case not required since the existence of other types of astronomical objects has been established on the basis of different types of evidence. Perhaps the addition of simulation evidence to other types of acceptable evidence will eventually become acceptable or even count as stand-alone evidence.

The literature on these issues discussing the importance of materiality (experimenting on a system with the same material as that of the target system) or irrelevance thereof can be misleading and Morrison is quite right to reject the view that running simulations on material computers is a reason to accept the importance of materiality. Some measurements require access to intrinsic properties of a system, which is when the specific material is important; others do not. Morrison says "What I argue is that in some instances this notion of 'being in causal contact with the system' has no relevant epistemic or ontological implications for the way the outcomes are evaluated" (p.212). One reason for this is that it is rare in modern physics experiments to have a direct causal connection between the system and the data. The connection is almost always partially causal and partially computational with the computational aspects being based on substantive scientific models. As a result, the causal connection to the target is not the sole bearer of the epistemological burden. If this is true it has significant implications for scientific realists especially within the kinds of causally grounded accounts such as Chakravartty's semirealism within which the notion of causal contact with an entity is essential. So this is an issue not just about experiment but also about realism.

The real issue here is whether data from an experiment make a different epistemic contribution to science than do data from a simulation. What would help is some kind of categorization of the knowledge that can be produced by simulation as opposed to an experiment. One such category is estimating the value of a parameter in a model, a situation discussed by Morrison. Here is a simple example. We need to estimate the five parameters in the multinomial model for rolling a die.[1] We could either roll a real die or use a computer simulation which relies on a random number generator producing a multinomial distribution. The important difference between the two is that we have assumed into the simulation the value of the central parameters, which are the values of probabilities for the six outcomes. The real die can tell us whether there is a deviation from the equally probable values whereas the simulation cannot. This example shows that it is not just existence, but also the determination of empirical model parameters that cannot always be carried out through simulations. Yet the example is too

[1] We get the sixth parameter for free given normalization.

simple to capture what Morrison is after. In more complicated cases her claim is that in both simulation and experiment we are investigating and manipulating a model of the system and once again we must note the restricted scope of her conclusions. Morrison is discussing only what she calls particle simulations and she is not arguing for the conclusion that we can always replace experiments with simulations. Indeed, different types of experiments have different epistemic functions, just as do different kinds of simulations. Some explore only structural features of the system while others must be supplemented by empirically measured parameter values in order to get the desired results. Although she points out that measuring the value of critical exponents in second-order phase transitions is typically done by simulations, the universality of such exponents make this a different type of case than measuring the Young's modulus of a sample of stainless steel, which cannot be done purely by simulation. It is in determining the values of parameters that are type specific that the materiality of the experiment is important.

A great virtue of this book is that it can be read with profit by those who are not specialists in the philosophy of physics or the philosophy of biology. Anyone who is interested in how the uses of models, theories, simulations, and data have been radically transformed in contemporary science will benefit from reading Morrison's work. Using a deft blend of case studies and philosophical theory, it injects real scientific substance into contemporary discussions of representation, elevates the level of analysis of the epistemology of computer simulations, and contributes a distinctively different dimension to the debate about the role of mathematics in the natural sciences.