Modeling without representation

Citation for published version:
Isaac, AMC 2013, 'Modeling without representation' Synthese, vol 190, no. 16, pp. 3611-3623. DOI: 10.1007/s11229-012-0213-9

Digital Object Identifier (DOI):
10.1007/s11229-012-0213-9

Link:
Link to publication record in Edinburgh Research Explorer

Document Version:
Peer reviewed version

Published In:
Synthese

Modeling without Representation[1]

Alistair M. C. Isaac

Abstract

How can mathematical models which represent the causal structure of the world incompletely or incorrectly have any scientific value? I argue that this apparent puzzle is an artifact of a realist emphasis on representation in the philosophy of modeling. I offer an alternative, pragmatic methodology of modeling, inspired by classic papers by modelers themselves. The crux of the view is that models developed for purposes other than explanation may be justified without reference to their representational properties.

1. Introduction

The increasing popularity of mathematical and computer-based methods in science has inspired a growing interest in the epistemological status of models (Godfrey-Smith, 2006; Weisberg, 2007b). At the heart of this interest is a puzzle about how modeling works as a form of scientific reasoning. For example, a simple model of a market treats it as a competition between agents acting solely to maximize their expected utilities. But real human beings are not in fact utility maximizers (as demonstrated by many experiments, e.g. the ultimatum game, Camerer and Thaler, 1995). How then should we interpret this model of a market? Its assumptions seem to be false, but what possible scientific use could a fallacious model have?

This is the classic epistemological puzzle of modeling, a puzzle which arises when models are contrasted with the realist ideal for a scientific theory. Good theories are *true*, or veridically *represent* the world, and it is *in virtue of this representation* that they succeed in *explaining* natural phenomena. In contrast, models (frequently) fail to veridically represent the causal structure of the world, so *how can they explain?* A realist

[1] "Representation" is used throughout to mean the relationship between the assumptions of a model and the world, which I take to be the sense employed by Weisberg when he argues that the "essential" feature of modeling is that it "involves indirect representation and analysis of real world phenomena" (2007b, 209–10). This should not be confused with the mathematical sense of "representation" as characterized by representation theorems, see Section 3 for further discussion.

strategy for resolving this puzzle might attempt to justify the impoverished representational features of models, perhaps beginning with an analysis of the explanatory properties of idealizations (Weisberg, 2007a), or an account of how false models converge on true ones (Wimsatt, 1987). The present project outlines and defends an alternative strategy, one which sidesteps questions about representation and instead justifies modeling practice *pragmatically*.

I maintain that the classic puzzle rests upon a tenuous assumption, one entrenched in the realist perspective, but unnecessary and unwarranted in the context of modeling. This assumption is that successful science depends upon successful representation. On this view, the justification of modeling as a scientific practice must ultimately rest upon an analysis of how models represent: representation is conceptually prior to success. Ironically, this attitude runs contrary to the pragmatic methodology expressed in classic papers by modelers themselves (Section 2). I analyze an example of a model with purely pragmatic success conditions in Section 3 before returning to the question of explanation in Section 4. The recognition that explanation is just one amongst many goals of modeling motivates a new interpretation of the classic puzzle.

I think the main resistance to a pragmatic methodology of modeling is a supposed conflict with realism about scientific theories. If our realism forces us to build our account of all scientific practice upon representation, then we cannot accept a purely pragmatic methodology of modeling. In Section 5, I argue that there is no actual inconsistency here. We can have our representational cake and eat it too, so long as we endorse a pluralism about scientific practice. Not only can pragmatism and realism peacefully coexist as responses to distinct scientific methods, but the relationship between these methods is clarified by acknowledging that they rest upon distinct foundations.

2. A Pragmatic Methodology of Modeling

The foundations of a discipline explain and justify its practices. What explains and justifies the practice of modeling? The answer I defend here ties models to specific functions and judges the success of a model, i.e. justifies the practice of building it, in terms of its success at fulfilling its specific function. Examples of functions I have in mind are things like (i) generating testable predictions; (ii) offering a policy recommendation; or (iii) demonstrating how an unexpected phenomenon is possible. From this perspective, models are more akin to tools than theories and accordingly are judged by their success at getting the (relevant) job done.[2] This pragmatic methodology has precursors in the influential methodological writings of Milton Friedman (1953) and Richard Levins (1966).

2.1. Richard Levins

Population biologist Richard Levins defends a pragmatic methodology in his influential article, "The Strategy of Model Building in Population Biology" (1966), which argues that model building necessarily involves a tradeoff amongst three competing desiderata: realism, generality, and precision. Levins appears to treat these three desiderata symmetrically: realism does not have a privileged status, it is just one property a model might have more or less of. This interpretation is supported by Levins' remarks on model evaluation: "The validation of a model is not that it is 'true' but that it generates good testable hypotheses relevant to important problems" (430). So, the success of a model is assessed pragmatically, in the context of a particular problem, and does not directly depend on truth, or representational success.[3]

Levins identifies three goals of modeling: "understanding, predicting, and modifying nature" (422). The extent to which one reads Levins as defending a pragmatic methodology depends crucially on whether one takes these goals to be independently satisfiable. If so, then a model might be validated by success at prediction or modification without an antecedent assessment of its contribution to understanding. The justification of such a model would not need to appeal to its representational properties.

[2] Morrison and Morgan (1999) make a similar point when they stress that models should be understood as "autonomous," and that "what it means for a model to function autonomously is to function like a tool or instrument" (11). Note that while Morrison and Morgan emphasize the question of "representation" in understanding modeling success, they use this term in a broader sense than Weisberg and Godfrey-Smith, covering not only the model–world relation, but also the model–theory relation.

[3] Jay Odenbaugh (2006) has also stressed the pragmatism of Levins (1966), but Odenbaugh's claim and mine are different. He argues that the "necessity" of tradeoffs for Levins is pragmatic, not logical. I wish to emphasize the pragmatic nature of model evaluation in Levins, a different, though perhaps related, point.

This interpretation of Levins (1966) is somewhat heterodox. A more mainstream reading (e.g. Weisberg, 2006) takes the goal of understanding to have special status (625). Since understanding follows from explanations and explanations must be veridical, representation plays a fundamental role in modeling methodology (624). On this account, when realism is traded off against generality and precision, the importance of representation for evaluating a model is not also diminished. Instead, the modeler adopts a different "representational ideal" (633); she does not abandon fit with reality as a virtue, she merely changes the standards by which she assesses this fit.

My claim is not that Levins abandons realism, representational adequacy, or explanatory value as norms of modeling. The point is rather a more subtle one, about emphasis and conceptual priority. Levins (1966) deemphasizes realism and truth (and, consequently, representational adequacy) in favor of pragmatic considerations such as the generation of testable hypotheses.[4] One possible response to this shift in emphasis is a corresponding shift in priority, placing pragmatic considerations prior to representational considerations. This radical position has been explicitly defended by Milton Friedman.

2.2. Milton Friedman

Milton Friedman's "The Methodology of Positive Economics" (1953) served as a manifesto for the "Chicago School" and a target for its critics. Friedman defends an extreme pragmatic methodology for the practice of mathematical modeling. The caricature statement of his view is that models should not be judged by the realism of their assumptions, but only by the success of their predictions. A model of a market of interacting utility maximizers should be judged by its ability to predict the behavior of actual markets, *not* by the truth or falsity of its assumptions about agents.

This rather extreme reading of the article has been somewhat tempered by historical work into its origins. Daniel Hammond (2008), for example, ably argues that Friedman was responding to a trend in economic modeling which ignored any contact

[4] The exact import of this shift in emphasis for Levins' own research is unclear. Chapter 1 of Levins (1968) appears to support Weisberg's interpretation as it characterizes the practice of model building as one of abstracting from reality, though it still emphasizes that model validation always occurs relative to a specific purpose. Conversely, the conclusion of Levins and Lewontin (1985) defends a holistic and anti-reductionist worldview at odds with traditional conceptions of realism.

between models and empirical data. His ultimate position should not be interpreted as rejecting the value of realistic assumptions altogether (as these represent one contact with empirical data), but rather as emphasizing the importance of the comparison between predictions and data over and above that between assumptions and data. On this view, a model with unrealistic assumptions which accurately predicts economic behavior would have more value than a model with realistic assumptions which nevertheless fails to generate accurate predictions, but a model with both realistic assumptions and accurate predictions might *in some contexts* trump them both.

The crux of Friedman's argument is the claim that it is wrong to interpret the degree of realism of a model's assumptions as an independent source of evidence for the model. Conceptually, there is just one relationship between model and data, and predictive success should outweigh any supposed "falsity" of assumptions.

> [T]he relevant question to ask about the "assumptions" of a theory is not whether they are descriptively "realistic," for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions. The two supposedly independent tests thus reduce to one test. (15)

Friedman later clarifies his use of the term "theory," stating it includes an "abstract model" plus "a set of rules" defining the model's representational properties (24). The determination of which features of the model count as assumptions and which as implications will be found in these rules. Crucially, however, they cannot all be explicitly stated, and will in general vary with context. According to Friedman, the distinction between assumption and implication "is not . . . a characteristic of the hypothesis as such but rather of the use to which the hypothesis is to be put" (26).

Although we could simply read Friedman as defending a general instrumentalism,[5] I think it is more constructive to see him as arguing particularly about the status of models. Certainly, he is responding to many of the same issues which

[5] E.g. a "methodological instrumentalism" (Caldwell, 1982). Friedman has also been identified as a realist, however, e.g. Mäki, 2009. In assessing the implications of Friedman, 1953, for philosophy of science generally, it is important to remember that modeling is only one facet of his research; the other is data collection in the form of diachronic case studies. For a discussion of this side of Friedman's methodology, and a reading of his methods as a response to the Duhem-Quine thesis, see Schliesser, 2012.

motivated Levins: the complexity of the phenomena, the limitations of human computational abilities, the apparent necessity of tradeoffs between accuracy and generality, the distance between mathematics and physical reality, etc. Insofar as Levins and Friedman are addressing the same issues in scientific practice, they seem to agree that the measure of a model is in its implications, not in the realism (or lack thereof) of its assumptions.

On Friedman's view, a model can only be assessed in the context of a particular use or purpose. And in this context, it is evaluated in terms of its success at fulfilling that purpose. What then justifies constructing a model with unrealistic or fallacious assumptions? Simply that it gets the job done. A model of interacting utility maximizers is nothing more than math if considered in a vacuum. If an economist uses it to predict the effects of a change in tax policy on the price of gas, then it has a context and a function. *If this function is fulfilled, then the model is validated.*

3. An Example: Full-Employment Policy

Let's look at an example of modeling from Friedman's own research to see how a model might be validated without reference to its truth or representational status. We'll see that Friedman's model is validated by its success at analyzing a set of concepts relevant to policymakers. This analysis might plausibly be recast as an instance of mathematical "representation,"[6] but its success does not depend in any way on a representational relationship with the world. In fact, as a quick examination of subsequent literature demonstrates, the model may be taken to represent different features of the world, and with more or less success, without undermining its pragmatic success as a piece of concept analysis.

[6] A mathematical "representation theorem" proves for an axiomatically defined set of structures that every structure is isomorphic to one in a distinguished subset. In measurement theory, for example, a representation theorem can be used to show that every structure which satisfies the axioms of a ratio scale is isomorphic to the real line, thereby justifying our use of real numbers to represent the outcomes of measurements of length. Note that this notion of representation does not hold between a model and the world (since the world is not axiomatically defined), but holds rather between two (sets of) models. Friedman's argument could be recast as involving this type of representation if the concepts of instability and intervention are given an axiomatic characterization. For an extended discussion of the role of representation theorems in the philosophy of science see Suppes, 2002.

3.1. The Model

Friedman (1951) purports to address "full-employment policy," i.e. policy measures aimed at achieving 100% employment. Very rapidly, however, the article turns to abstract questions about the efficacy of countercyclic interventions on cyclic phenomena:

> Under what conditions will countercyclical action succeed in its objective of reducing instability? Under what conditions will it actually increase instability? How does its effectiveness depend on the magnitude of action? What is the optimum magnitude of countercyclical action? (117)

Friedman notes that currently popular models of the relation between employment and government expenditure (his primary target: a 1949 report to the United Nations) do not include temporal dynamics and so cannot be used to address these questions. This motivates the introduction of a new model.

Friedman's model treats income and the effect of policy on income as arbitrary functions evolving in time. Since his motivating questions involve stability and fluctuation rather than directed trends, he assumes these functions have a stable expected value at all times. He takes the variance σ^2 (the square of the standard deviation) as a measure of the magnitude of fluctuations. The general equation, then, is

$$Z(t) = X(t) + Y(t),$$

where $X(t)$ is income in the absence of a full-employment policy, $Y(t)$ is the effect at time t of the policy (*not* the effect of measures *taken* at time t, but the effect *at t* of action *already* taken), and $Z(t)$ is total income (122). He can now rephrase his initial questions about the efficacy of countercyclic intervention as questions about the relationship between the variances of X, Y, and Z:

> Under what conditions will the variance of Z (σ_Z^2) be less than the variance of X (σ_X^2), so that the countercyclical policy succeeds in its objective of reducing instability? Under what conditions will σ_Z^2 exceed σ_X^2? How does the difference between σ_Z^2 and σ_X^2 depend on the magnitude of countercyclical action, that is, on σ_Y^2? What is the optimum size of σ_Y^2? (123)

The key insight for answering these questions is that the variance of the sum of two variables depends upon their degree of correlation. In this case, the relevant formula is

$$\sigma_Z^2 = \sigma_X^2 + \sigma_Y^2 + 2r_{XY}\sigma_X\sigma_Y,$$

where r_{XY} is the correlation coefficient of *X* and *Y*. As we would expect, if correlation is positive ($r_{XY} > 0$), then fluctuations in *Y* reinforce fluctuations in *X* and instability increases. If there is no correlation between interventions and fluctuations in income ($r_{XY} = 0$), the variances of *X* and *Y* simply sum and intervention again increases instability. The crucial point of interest is that interventions may be negatively correlated with fluctuations in income as desired ($r_{XY} < 0$) and yet *still* increase instability. A simple transformation of this equation, division by σ_X^2, allows for more transparent analysis.

$$\frac{\sigma_Z^2}{\sigma_X^2} = 1 + \frac{\sigma_Y^2}{\sigma_X^2} + 2r_{XY}\frac{\sigma_Y}{\sigma_X}$$

Countercyclic intervention has a positive effect, i.e. decreases instability, whenever $\sigma_Z^2 < \sigma_X^2$, or whenever the left side of this equation is less than 1. This allows us to compute the answer to our first question, namely the conditions under which countercyclic policy succeeds, or those values of r_{XY} which ensure $\sigma_Z^2 < \sigma_X^2$. Whenever

$$-1 < r_{XY} < -\frac{\sigma_Y}{2\sigma_X}$$

the effects of the policy are stabilizing, and whenever

$$-\frac{\sigma_Y}{2\sigma_X} < r_{XY} < +1$$

the effects are destabilizing (124–5). Similar manipulations answer the rest of the questions on Friedman's list.

Friedman's article concludes with a discussion of the implications of his analysis for policy. The two variables affected by policy are σ_Y, average magnitude of the intervention, and r_{XY}, degree of correlation with fluctuations in income. He gives a precise characterization of how these relate to each other and to σ_X, including an optimal value for σ_Y given a value for r_{XY}, and vice versa. He emphasizes that static models of full-employment policy such as the 1949 U.N. model tacitly assume reaction to fluctuations is instantaneous (i.e. that $r_{XY} = -1$), or that a very low r_{XY} can be ensured by predicting with great accuracy "both the behavior of the system in the absence of action and the effect of action." As Friedman points out, "to date there is no reason for confidence in our ability to make such predictions" (129).

Nevertheless, once we acknowledge the difficulty in predicting fluctuations in employment, we can develop a positive strategy for lowering r_{XY}, and thereby succeed at the goal of stabilizing income.

> Whereas one method of controlling r_{XY} is to change the kind of action taken, another method is to limit the objective. The effect of action is clearly likely to be in the right direction much more frequently if action is taken to counteract only substantial movements in income than if it is taken to counteract mild movements as well. In the case of substantial movements the lag between action and its effects is likely to be much shorter relative to the movement itself—even if not in absolute terms—than for mild movements, and so r_{XY} is likely to be greater. (131)

Since a large fluctuation in income takes longer than a small fluctuation, our response to it is more likely to occur *relatively* early during the fluctuation, and therefore the degree of correlation will be closer to -1.

3.2. Validating Friedman's Model

What is the relationship between the representational properties of this model and its success? Does the classic puzzle arise for this model? Friedman's model does indeed "falsely" represent the world: *X* represents changes in income independent of intervention, *Y* represents the effect of interventions on income, yet these are certainly not the only two factors which determine total income. Nevertheless, I think it misinterprets Friedman's endeavor to put any evaluative weight on the model's representational properties. There is no puzzle about how the model succeeds despite representing the world falsely since the standards for assessing its success have nothing to do with its accuracy in representing the world.

To see this, let's look at where Friedman's model comes from. The mathematical model Friedman proposes is nothing more than an attempt to make precise the concepts of cyclic and countercyclic activity. One can see this by comparing Friedman's general questions about countercyclic policy with his questions about the relationship between the variances of X, Y, and Z. So, one step in the modeling practice, a step which requires validation, is the characterization of cyclic activity as fluctuations in the value of an arbitrary function. The validation of this step is purely a matter of concept analysis.

The second step for validating Friedman's model addresses the relationship between the abstract questions he asks and policy choice. The pertinent question here is not whether income actually fluctuates or whether these fluctuations correlate with employment. Rather, the crucial issue is whether policy makers perceive full-employment policy as a countercyclic corrective to fluctuations in income. If a match obtains between Friedman's theoretical characterization of the problem and the policymakers' perception of the problem, then his analysis succeeds in placing constraints on reasonable policy choice. This step is validated empirically, but does not require an assessment of the match between model and world.

Unfortunately, it is difficult to assess this empirical question. Large scale policy decisions are often made by committee (in the United States, for example, the twelve member Federal Open Market Committee), and while committee members may agree on the particular policy to implement, they may not agree on the rationale which justifies said policy (Friedman and Kuttner, 1996, 81). Nevertheless, we can find some measure of the success of Friedman's model by looking at its treatment in the subsequent literature on public policy. While that literature treats the model as successful at analyzing the limited set of concepts it considers, it has since acknowledged that a richer set of concepts is needed to address the concerns of policymakers.

Friedman's model supplemented previous analyses by explicitly considering the relevance of temporal dynamics for policy efficacy. Optimal policies of the sort considered by Friedman were soon recognized as implausible in the face of policymaker uncertainty, however, motivating the explicit inclusion of the expected rather than actual effects of interventions in the model.[7] Once policymaker uncertainty was explicitly considered, however, it was natural to take the further step of considering their risk aversion as well. Mitchell (1979), for example, defends a model which explicitly

[7] Brainard, 1967, 411. Brainard does not explicitly cite Friedman, but he takes a model equivalent to Friedman's (his equation (3)) as a starting point for his analysis.

includes risk aversion; Friedman's model falls out as a special case. So, as the optimal policy literature has developed, Friedman's analysis has become a small part in a richer analysis of a much more sophisticated network of concepts.

It is important to emphasize that the development of this literature would be incoherent if its methodology were founded on representation. The reason is that this sequence of modeling decisions depends on an equivocation between variance in the world and variance in the policymakers' access to information about the world.[8] To supplement Friedman's model, a model supposedly illustrating fluctuations in income, with parameters which characterize risk aversion, a property of policymakers, is to mix apples and oranges from a representational standpoint. From the standpoint of concept analysis, however, there is no problem. The basic irrelevance of representation here is further illustrated by the fact that Friedman's model is treated as one of optimal intervention on a fluctuating system; the question of employment policy in particular is simply ignored.

Of course, Friedman's model might also be applied in an empirical context, at which point its representational properties do become important. An example here is Friedman and Kuttner (1996), who perform a longitudinal analysis of U.S. monetary policy in the 1970s and 80s, asking why the Federal Reserve appears to have ignored a Congressional mandate from 1975 requiring it to explicitly set monetary growth targets. Two of the potential answers they consider are motivated by Friedman's model. In order to derive explanations for the observed data from the model, they take σ_Z^2 to represent prices (measured, e.g., by GDP), σ_X^2 to represent money supply, and σ_Y^2 to represent the effect of Federal Reserve interventions on money supply. Once this representation is locked in, Friedman's model turns out to fail as an explanation of the efficacy of U.S. monetary policy. *Not* because it fails as a conceptual analysis (Friedman and Kuttner acknowledge the model makes a valuable, if incomplete, policy point, 104–5), but because an empirical relationship between prices and money supply which used to obtain now no longer does. Friedman and Kuttner conclude this is because money demand, a factor not explicitly represented in Friedman's model, has become unstable.

[8] C.f. a related discussion in Brainard, 1967, 412–3.

In sum, the claim is not that representation plays no part in Friedman's model, but rather that the classic puzzle does not arise because the model's success conditions do not include veridical representation. In the literature on public policy, it constitutes one in a sequence of increasingly sophisticated models, all justified by their pragmatic success at analyzing the network of concepts relevant to policy makers. In empirical contexts, these models may be given a specific interpretation, and their success or failure at the goal of explaining the data may then depend upon their representational adequacy.

4. Explanation: One Goal Amongst Many

The pragmatic methodology of modeling on offer here won't work for models which are intended as explanations. This is because explanations have a normative status—they can be correct or incorrect. Not so with many of the pragmatically evaluated purposes to which models are put, e.g. generating testable predictions or policy recommendations. Testable predictions may be interesting or not, though this is a matter of opinion. In the context of the day to day life of a laboratory, however, *it is having a prediction to test which is important*, its correctness or not is determined *ex post facto* by the test itself. Even false predictions can be of value: the discrepancy between the rate of precession of the perihelion of Mercury predicted by Newtonian models and that which was observed provided evidence for general relativity, but it was evidence which could not be discovered without the precise calculations of Newtonian theory. Arguably, this discrepancy should be treated not just as a success for general relativity (in explaining it away), but also for Newtonian gravity, which succeeded in identifying it in the first place.[9]

Policy recommendations are similar. Of course, there are good and bad policy choices, but determining which is which is largely a matter of hindsight. *So long as there is agreement on the goal to be achieved*, there is no meaningful empirical standard by which to assess a policy recommendation other than its *ex post facto* success in achieving

[9] See discussion in Smith, 2002, especially p. 157; c.f. the general comparison between Newton's and Friedman's methodologies in Schliesser, 2005.

that goal. Consider, for example, two models of the earth's climate: one which has realistic assumptions (i.e. reflects our best theories of the causal interaction between local heat, wind change, ice albedo, width of ozone layer, etc.) but is poor at matching historical data, and a second which matches historical data much better, but contains arbitrary, uninterpreted parameters. Which is better as a model for guiding climate policy? A climatologist will say the first, a statistician, the second, but a politician is interested in the one which correctly predicts the outcome of her policy choice. By what criterion then should she choose between the two? Ideally we'd like a model which both reflects our causal knowledge *and* accurately predicts. If we don't have that, we are in the realm of Levins and Friedman, where the world is too complex, human limitations too great, and the best we can do is tradeoff amongst criteria of interest. In this case, the only definitive constraint is that parameters representing the intervention of the possible policy change must be present in the model. Beyond that, the question of how to determine the right model to trust appears to be extra-scientific.

Even in the case of explanation, the pragmatic approach can get a toehold. Of course, ultimately, we'd like our explanations to be true, and this means they accurately represent reality. But in many contexts, we are satisfied with something weaker, a "how possibly" explanation. And here, again, there is no question of correctness, but merely of pragmatic success: a "how possibly" explanation may sound plausible, or generate new predictions, or simply get the questioner to stop asking questions. Of course, a "how possibly" explanation may turn out to describe *how actually*; or it may not. But its success *as* a "how possibly" explanation is distinct from this future status it may one day attain, and must be evaluated differently.

Is this justification of modeling practice too quick? A mere sleight of hand which avoids the hard questions? Don't be fooled by this cursory discussion, there is much left to be done, but the methodological project which prioritizes contexts and purposes looks quite different from that which prioritizes representation. Rather than a taxonomy of representational ideals, we will need a taxonomy of context-sensitive scientific goals, and an analysis of the evaluative criteria used in each. The pragmatic methodology of modeling is not a quick fix, it is merely a different starting point for the journey. It does not ignore the "hard questions," but merely recasts them in a different foundational role.

5. Can We be Pragmatists about Models, but Realists about Theories?

I argued above that the test of a good model is that it works, that it does the job for which it was intended. Does this view necessarily lead to a thoroughgoing instrumentalism? If we reject a foundational role for representation in our methodology of modeling, are we thereby led to reject it in our philosophy of science as a whole? I think the answer to this question depends crucially on the status of models within scientific practice. If models are only one type of scientific construct amongst many, we can consistently maintain pragmatism about modeling and realism (or some other view, if we wish) about other aspects of the scientific endeavor.

Of course, the view that all scientific theories are just models (the semantic view) was quite popular in the latter half of the 20th century. In order to be coherent, however, this view must mean something quite different by "model" than either Levins or Friedman, both of whom use the term solely for sets of mathematical equations.[10] In fact, it was to some degree dissatisfaction with the semantic view, and a shift towards pluralism about scientific *practice*, which motivated the modern turn toward modeling (see especially Godfrey-Smith, 2006). So, at least in the context of the present debate, there seems to be no inconsistency in maintaining a pragmatic foundation for modeling, but a realist foundation for theories as the outcome of scientific inquiry more broadly construed.

What is the status, then, of questions about how models represent? On the account developed here, the direction of the classic puzzle is reversed. The classic puzzle asks: given that a model is false, how can we explain its success? The methodological pragmatist asks: given that a model is successful, what can it tell us about the world? If there is a representational discrepancy between a model and our theory of reality, then the

[10] Historically, the semantic view *has* meant something different, although its history is one of debate about how to characterize the notion of "model" and what role models should play in philosophy of science. Originally, "model" was meant in the logical sense of a set-theoretical structure (Suppes, 1960). More recently, the literature has turned toward a looser notion of model, in part because the set-theoretic notion was inadequate for characterizing the theory-world relationship (Giere, 1988). The crucial point for the present discussion, however, is just that modeling *as a practice* (and as discussed by Levins and Friedman) is not a plausible account of the full diversity of scientific theorizing.

model's pragmatic success should cause us to question the supposed truth of that theory!

Of course, some of this discussion of "truth" and "falsity" is overblown. Experiments such as the ultimatum game show us that humans are not utility maximizers *in the limit*. They do not necessarily undermine the claim that humans *approximate* utility maximizers. We might resolve the apparent conflict between a successful model of an economy which assumes humans are utility maximizers and the "fact" that they are not with some suitably qualified view, for instance that *in large-scale systems of economic interaction, the only feature of human behavior relevant for generating correct predictions is that it approximates utility maximization*.[11]

The possibility that a strictly false model might shed light on the current state of our knowledge of the world should not be underestimated, however. The initial reaction to Newton's theory of gravity was that it posited an impossible, and therefore fallacious, mechanism. Intelligent men had *learned* that action at a distance was not possible. The arguments were logically sound, the reasoning incontrovertible. Newton's theory then *must be false*; hence attempts, e.g. by Leibniz, to reconstruct it without the action at a distance. In the end, Friedman's criterion, empirical success, won out. We did not revise Newton's theory because of its "false" assumptions, *we revised our "knowledge" of the world because of its success*.

6. Conclusion

Even if the ultimate aim of science is explanation, along the way in day to day practice there are many subgoals and practical tasks to be achieved. Models are frequently employed to solve these tasks, and their success or failure at the task at hand is both the measure of their value and the justification of their design. If a discrepancy exists between the assumptions of a model and some assumed feature of reality, that certainly constitutes a puzzle: not necessarily a puzzle about the model's success,

[11] This is Friedman's answer. It is closely related to Weisberg (2007a)'s "minimalist idealization"—note, however, that the direction of the reasoning is different. For Weisberg an idealization is "the intentional introduction of distortion" (639); the scientist begins with the richness of reality, then *introduces* a distortion to examine only the causally relevant factors. On the pragmatic account, the model is constructed from simple mathematical parts to generate an answer to a practical question. We then *learn* from the success of this tool at satisfying its function which features of the world are relevant (or not) for generating the phenomenon of interest.

however, but perhaps one about our theory of the world. Such discrepancies can drive new theory formation, motivate new experiments, and correct and clarify our understanding of nature. Representation does not provide the foundation for modeling practice, rather modeling practice helps provide the epistemic foundation for our knowledge of scientific truths.

Acknowledgements A previous version of this paper was read at the Pacific APA. I am grateful to David Stump, Michael Weisberg, Ryan Muldoon, and three anonymous reviewers for their helpful comments. This research was supported by NSF grant no. 1028130.

Bibliography

Brainard, W. (1967) "Uncertainty and the Effectiveness of Policy," *The American Economic Review*, 57: 411–425.

Caldwell, B. J. (1982) *Beyond Positivism: Economic Methodology in the Twentieth Century*. George Allen & Unwin: Boston, MA.

Camerer, C. and R. H. Thaler (1995) "Anomalies: Ultimatums, Dictators and Manners," *The Journal of Economic Perspectives*, 9: 209–219.

Friedman, B. and K. N. Kuttner (1996) "A Price Target for U.S. Monetary Policy? Lessons from the Experience with Money Growth Targets," *Brooking Papers on Economic Activity*, 1996: 77–146.

Friedman, M. (1951) "The Effects of a Full-Employment Policy on Economic Stability: A Formal Analysis," reprinted in (1953) *Essays In Positive Economics*. University of Chicago Press: 117–132.

Friedman, M. (1953) "The Methodology of Positive Economics," in *Essays In Positive Economics*. University of Chicago Press: 3–46.

Giere, R. N. (1988) *Explaining Science: A Cognitive Approach*, University of Chicago Press: Chicago, IL.

Godfrey-Smith, P. (2006) "The Strategy of Model-based Science," *Biology and Philosophy*, 21: 725–740.

Hammond, J. D. (2008) "Friedman's Methodology Essay in Context," in *The Anti-Keynesian Tradition*, ed. R. Leeson, Palgrave Macmillan: 78–95.

Levins, R. (1966) "The Strategy of Model Building in Population Biology," *American Scientist*, 54: 421–431.

Levins, R. (1968) *Evolution in Changing Environments*. Princeton UP: Princeton, NJ.

Levins, R. and R. Lewontin (1985) *The Dialectical Biologist*. Harvard UP: Cambridge, MA.

Mäki, U. (2009) "Unrealistic Assumptions and Unnecessary Confusions: Rereading and Rewriting F53 as a Realist Statement," in *The Methodology of Positive Economics: Reflections on Milton Friedman's Legacy*, ed. U. Mäki. Cambridge UP: 90–116.

Mitchell, D. W. (1979) "Risk Aversion and Optimal Macro Policy," *The Economic Journal*, 89: 913–918.

Morrison, M. and M. S. Morgan (1999) "Models and Mediating Instruments," in *Models as Mediators*, ed. M. S. Morgan and M. Morrison, Cambridge UP: 10–37.

Odenbaugh, J. (2006) "The Strategy of 'The Strategy of Model Building in Population Biology,'" *Biology and Philosophy*, 21: 607–621.

Schliesser, E. (2005) "Galilean Reflections on Milton Friedman's 'Methodology of Positive Economics,' with Thoughts on Vernon Smith's 'Economics in the Laboratory,'" *Philosophy of the Social Sciences*, 35: 50–74.

Schliesser, E. (2012) "Inventing Paradigms, Monopoly, Methodology, and Mythology at 'Chicago': Nutter, Stigler, and Milton Friedman," *Studies in History and Philosophy of Science*, 43: 160–171.

Smith, G. E. (2002) "The Methodology of the *Principia*," in *The Cambridge Companion to Newton*, ed. I. B. Cohen and G. E. Smith, Cambridge UP: 138–173.

Suppes, P. (1960) "A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences," *Synthese*, 12: 287–301.

Suppes, P. (2002) *Representation and Invariance of Scientific Structures*. CSLI Publications: Stanford, CA.

Weisberg, M. (2006) "Forty Years of 'The Strategy': Levins on Model Building and Idealization," *Biology and Philosophy*, 21: 623–645.

Weisberg, M. (2007a) "Three Kinds of Idealization," *Journal of Philosophy*, 104: 639–

659.

Weisberg, M. (2007b) “Who is a Modeler?” *British Journal for the Philosophy of Science*, 58: 207–233.

Wimsatt, W. C. (1987) “False Models as Means to Truer Theories,” in *Neutral Models in Biology*, ed. M. H. Nitecki and A. Hoffman, Oxford UP: 23–55.