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19 Unrealistic models, mechanisms, and the social sciences

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Abstract. Assessing the status of unrealistic models in economics and the social sciences more generally is an intellectual puzzle and a practical problem of some import. I argue that attempts to provide a general defense of and metric for evaluating unrealistic social models is misguided. Rather, compelling assessments have to look at specific models used for specific purposes and in specific empirical contexts. I outline how arguments can sometimes be made that unrealistic assumptions do not matter and that indeed on occasion they are essential. Across the discussions I look at the claim that mechanisms are needed for unrealistic models and argue that again there is no universal answer–sometimes they are and sometimes not.

Keywords: realism, models, idealization, philosophy of social science.

1 Introduction

Some highly unrealistic economic models were behind various policy pronouncements of economists in the period leading up to the great crash of 2008. Those pronouncements we now know were very far off target, and some blame for the 2008 downturn probably has to go to those models and those who purveyed them. It seems obvious to many in retrospect that these models should not have been trusted because of their quite unrealistic assumptions. Yet simplified models are absolutely essential in much of science and much of social science. So in evaluating unrealistic models it is important to avoid throwing the baby out with the bathwater. Yet how to do that is still very much an open question in the social sciences.

This paper tries to help us decide when we should throw the bathwater out without worry because social science models are so distant from reality and to tell when models tell us something despite their irrealism. My theme is that these problems are unlikely to be solved on general grounds; instead a host of different issues and techniques are involved depending on the kinds of models, their purposes, the empirical domains involved, and the testing techniques used. There is of course a vast philosophy of science literature on models, no doubt some of it of relevance to debates in the social sciences about unrealistic models. In a short paper I cannot pretend to tease out all of the possible connections, though my general approach is that the problem of unrealistic models cannot generally be decided on general conceptual grounds, that it must be attacked by looking for local methods for specific kinds of models, and to show examples of how that can be done. I am doubtful that the problems can be handled by first giving a general account of model representation and using that then to decide which unrealistic models explain and which do not–a general account of model representation seems unlikely (van Fraassen, 2008), and even if we had that, it is 1.) not clear what it would say about unrealistic models and 2.) local detail would likely still be essential to any assessments. My claims are also local in that they are mostly about specific kinds of models and applications in economics and some applications in political science, areas I know best. While some of my claims may generalize to the use of models across other sciences, I am not going to assert that here.

However, in a sense there are some more general morals lurking in my discussion, because evaluating unrealistic models overlaps with general questions about the place of mechanisms. Mechanisms have been offered as a tool for dealing with unrealistic models in the social sciences (Mäki, 2009) and I look at specific kinds of cases where they can help. Still my conclusions will be that evaluating the importance of mechanisms, as in evaluating unrealistic models, depends strongly on the local details involved. As I have argued elsewhere (Kincaid, 2011, 2014), there are multiple independent sense of mechanisms and there is no reason they should all the same import. One of my main and more novel conclusions perhaps is that there are circumstances where providing mechanisms for unrealistic models leads to error; in some cases models need to be unrealistic to be believable.

Throughout I presuppose no general account of what models are (and doubt that there is one to be had). I take the term "model" in the way it is commonly used by social scientists, where the paradigm case comes from economics which overwhelmingly thinks of models as an a equation or set of equations which is accompanied by a verbal narrative linking the model to economic phenomena. I think my conclusions can also be applied to other senses of models such as Schelling's physical checkerboard model of racial segregation.

The paper is organized as follows. Section 2 elaborates on the importance and nature of the issues just sketched above and then looks at some all purpose attempts to evaluate the status of unrealistic models and argues that they are insufficient. Sections 3-5 then look at specific circumstances where parts of economics and political science can successfully ignore the irrealism of their models. My goal is to defend the strong thesis that unrealistic social scientific models sometimes provide us with well-confirmed explanations, where the explanations in question are causal explanations. I make two claims: 1.) that there are methods by which we can show that the irrealism of models does not matter in producing well-confirmed causal explanations and 2.) that sometimes only unrealistic models can provide well-confirmed causal explanations. Claim 1.), I should note, is not the standard and true point that some factors can be left out because they are of negligible importance. The kinds of cases I have in mind concern factors that can be causally *very* important. Rather the idea is that given the right kind of model and method of verification, even causally important factors can be neglected.

Section 3 looks at competitive equilibrium and game theory models, arguing that the typical unrealistic assumptions about the abilities of agents do not necessarily undermine the equilibrium explanations given because some of those explanations do not depend on cited presuppositions about how agents reach equilibrium. Section 4 looks at instrumental variables and related techniques that can ground arguments that models can leave out variables without sacrificing validity, though economists err in thinking that such arguments can be purely statistical. Section 5 demonstrates cases where leaving out causes that have significant effects is essential, not just defensible.

2 Issues and past approaches

Macroeconomic models used in the run up to the 2008 crisis (and still used) illustrate the problem of unrealistic models vividly. These models are called dynamic stochastic general equilibrium (DSGE) models. General equilibrium means they are supposed to consider all market interactions at once. They are dynamic in the obvious sense that they are supposed to describe changes over time and they are stochastic in that they describe probabilistic relations. These models are the dominant trend in economics because they are supposed to be grounded in mechanisms involving constrained optimizing of individuals, unlike traditional Keynesian models whose ties to individual behavior is less determinate.

However, the actual ties to individual behavior of DSGE models are much less clear than the rhetoric advertises. DSGE models depend essentially on “representative agents”: for instance, they assume that all consumers can be treated as if they were one agent. In other words they assume that there is only one consumer. They do so for tractability reasons. However, it is widely known that aggregate consumption mirrors individual choice only under stringent assumptions that are seldom met (Kirman, 2010). Aside from this aggregative assumption, DSGE models also assume that we can treat all firms as a single agent and that financial markets are fully efficient, and that relations are linear.

So these models are not making assumptions that certain factors only play a small role and can be ignored nor just transforming complicated relationships into somewhat more tractable simpler ones. They are starkly at odds with reality. Thus the pressing question is how we can explain the real world using them. Models as unrealistic as the DSGE models are not an exception in economics and political science. Two work horses in economic analysis are competitive general equilibrium models and game theory models, with the latter increasingly important in political science as well. Both appear to rest on assumptions about agents that are often highly unrealistic. How can they produce well-confirmed explanations–or do they?

Before I address these questions it is important to note that models may serve multiple functions other than providing well-confirmed explanations. For example, models may:

1. be essential for tractability–to get any way of systematically thinking about the phenomena.
2. be used to discover new possible phenomena.
3. be used to show how certain phenomena might come about—Schelling's models of residential segregation are a favorite example cited in this regard (see Ylikoski & Aydinonat, 2014).
4. be used to get a clearer formulation of a verbal argument. This is certainly quite true of the motivation for models in economics.
5. be used to identify robustness—to show that with weakened or different assumptions results still hold (Kuorikoski, Lehtinen, & Marchionni, 2010).

None of these uses of models are committed to the idea that models have to provide well-confirmed causal explanations of specific real world phenomena. So, given these goals, unrealistic models are not necessarily a problem.

Relatedly, unrealistic social science models may also be unproblematic in that their purpose is just to provide successful forecasts or predictions of the data. There is much science that counts success as the ability to trace variables through state space without a pretense of providing realistic causal mechanisms. Of course, it is a very open question whether some or any social science models provide the ability to predict variables through state space with anything like the success of some natural sciences. Certainly some parts of macroeconomics at least hope to provide prediction models without any claim to have realistic causal explanations. However, if unrealistic models are able to show such predictive success, then it seems that they tell us something about how the world is despite their unrealistic assumptions. This defense of unrealistic models needs to be distinguished from instrumentalism, because it does not claim that all social science is only about predicting observables but only that some models do. A charitable reading of Milton Friedman's (1953) classic paper might see this thesis as one of his more defensible points.

Despite these various functions that unrealistic models can perform, we still of course would like them to provide well-confirmed explanations; social scientists routinely claim that their models can do so, as witnessed by the fact that they are willing to make policy recommendations based on them. Thus while recognizing that the function of models in science is many and that questions about realism may not always be problematic, the status of unrealistic models in the social sciences is still pressing.

There are of course many defenses of unrealistic models in the social sciences, especially in economics, extant in the literature. None of them are entirely convincing. I want to sketch some from social scientists and some from philosophers as a way of setting up the approach that I think is more fruitful and that I apply in the rest of the paper.

A first response frequently found among social scientists is to dismiss the problem outright as unimportant on the grounds that all models misrepresent, as claimed recently for example by political scientists Clarke and Primo (2012). This "be happy, don't worry attitude" will, of course, not do if we want models to give us well-confirmed explanations of how the social world actually works. Since some models with unrealistic assumptions clearly do not tell us how the world works, we need to know how to tell those that do from those that do not. So the very same social scientists who advance this view about models will go on to argue in their actual work that some models, namely their own, are better than others; the question is how that judgment is grounded. They will also use their models to make policy recommendations, implicitly implying that they are close enough to reality to allow causal inferences.

Another common claim from producers and consumers of unrealistic models in the social sciences is that despite their departure from reality they provide "insight." The problem here is how to know when "insight" is anything more than a warm and fuzzy intellectual feeling? What does the insight come to? The idea can be developed into an account of explanatory understanding of possibilities as in Ylikoski and Aydinonat (2014) mentioned above. Yet that still does not tell us how can we use it to ground real world causal knowledge like that needed for policy advice.

Morgan (2012) in a recent interesting book of case studies of economics models defends unrealistic models by claiming that economists provide narratives and use tacit knowledge to tie them to reality just as scientists do in the natural sciences. While this is no doubt true in some sense, the open question is still when and how unrealistic social models do so *successfully*. There are well-developed methods in the natural sciences for tying unrealistic models to reality (see for example Wilson, 2006) but the question is whether the social sciences have any real parallels. Talk of providing narratives and using tacit knowledge is a promisory note that needs to be filled out, something Morgan only hints at. I will make some efforts in that direction below.

Finally, an important trend argues that unrealistic social science models are nonetheless explanatory when they capture or isolate the essence of a causal mechanism. This view has been argued for by Cartwright (1989), at length by Mäki (2009), and very recently by, on my view, one of the more methodologically sophisticated economists, Rodrik (2015). These approaches seem to me fundamentally on the right track. But the question is how and when do we know that unrealistic models actually isolate causal mechanisms? How do we know that a causal process in a model isn't disrupted in reality when other left out factors are involved? These authors by and large do not address that essential question, and it is this question that I am pursuing. What a "causal mechanism" comes to is also often left unspecified.[1]

I want to argue that answering this question about when we really have isolated a real causal process has to be largely a local affair, where the local parameters include the specific structure of the model in question and the specific techniques used to access it. A lack of realism may be no problem for some model structures and a serious obstacle for others; a lack of realism may be no problem for some models using certain techniques and a serious obstacle when using other methods. Mechanisms may be essential in some senses and some cases but not in others. Relatedly, I want to argue that it is unlikely that questions about unrealistic models have general, purely statistical noncausal answers, a view social scientists often harbor.

3 Showing that unrealistic assumptions are not part of the explanation

My first example of unrealistic models that can be nonetheless well-confirmed really builds off the previous point that questions of about the realism of models have to be cognizant of what models are used for. Two paradigm cases of unrealistic models in economics are perfect competition and game theory models, workhorses of the profession. Both kinds of models seem to make incredible assumptions. Perfect competition models often assume among other things:

- complete markets over all goods at all times (e.g. used umbrellas in 2027)
- individuals with complete preferences over all goods (e.g. those umbrellas)
- every buyer and seller is a price taker
- commodities are infinitely divisible
- buyers and sellers have full information

Typical game theory models assume:

- that agents can calculate the relevant best response equilibrium strategy, e.g. the subgame perfect Bayesian Nash equilibrium

[1] We could put this approach in terms of the idea of a partial isomorphism, one standard way of talking about model representation. However, we would still need to know which isomorphism and how the isomorphism obtains if the rest of the model is false.

- there is common knowledge of the payoffs, strategies and all the specific players

A considerable amount of ink has been spent on various twists and turns trying to justify such assumptions. However, there is good reason to think that in some applications, these modeling assumptions are no worry (Smith (2008) is the main inspiration for this insight). They are no worry because they need not be part of the explanation that is given. How can that be?

The insight is that assumptions of the perfect competition and game theory models may just be assumptions the *analyst* –the economist or political scientist–uses to identify equilibria. However, in certain empirical applications, the explanations are equilibrium explanations that make no commitment to what process leads individuals to find equilibrium. The analyst uses the unrealistic assumptions to identify an equilibrium. Then experimental studies are used to show that individuals will reach the equilibrium predicted from the models. So Smith shows in multiple experimental setups that subjects reach competitive equilibrium in continuous double bid auctions. Economists identify competitive equilibrium from assumptions such as that every individual is a price taker, there are an indefinite number of individuals in the market, and so on. Obviously these assumptions are not necessary for a competitive equilibrium to exist, since the experimental setup is nothing like this. Nonetheless competitive equilibrium is reached. Thus the unrealistic assumptions are useful to the analyst, but they are not part of the explanation of the experimental results.

Binmore (2007) does something similar for game theory. For example, in a finite, two person zero sum game the Nash equilibrium first proved by von Neuman was the minimax value. Borel first proposed the solution but had no proof; von Neuman proved it was the Nash equilibrium. Binmore notes that his subjects are probably not smarter than Borel, but given repeated play find their way to the minimax equilibrium.

These explanations work by showing that all individuals are making best responses to the actions of others. Equilibrium persists because deviation from it would produce inferiior results for individuals. You may or may not think these are causal explanations (cf Sober, 1983). However, the evidence for them comes from carefully designed experimental setups where the equilibrium behavior predicted by the model is observed (Smith is a Nobel prize winner and Binmore among the most highly respected experimentalists in the field). These explanations are a species of optimality analysis, but they do not have the kind of complications that plague optimality in nonexperimental situations (Orzack & Sober, 2001). For example, the costs and benefits individuals face are well known because they are set by the experimenters, a decided advantage over observational studies.

Notice the role mechanisms play in these cases. A mechanism in the sense of the causal process that leads individuals to find equilibrium is not needed. We don't know how they get there but we have strong evidence that they do. On the other hand, mechanisms are given in the form of appeal to best responses to the actions of others.

This defense of unrealistic competitive equilibrium and game theory models need not generalize to their use in other settings. When we have only observational evidence of behavior outside the lab, we are on much shakier grounds in claiming that observed behaviors are optimal equilibrium behaviors. Then the lack of a realistic mechanism that would lead to an optimum–for example, appeal to individuals calculating subgame perfect equilirium by backward induction–matters.

4 Causal methods to compensate for unrealistic models

One standard worry about unrealistic models in the social sciences is that confounding variables have been left out. This is a reasonable concern. I want to show in this section that there are ways and circumstances in which such concerns can be shown not to prevent well-confirmed explanations of unrealistic models. I have in mind three approaches: instrumental variables, sensitivity analyses, and the use of structural equation models to establish causal effects rather than causal effect size.

Instrumental variables are a way to defend unrealistic models. The idea of instrumental variables, for those who are unfamiliar, is that there are certain causal variables such that, if we have the right causal structure, we can make reliable estimates of causal influence even if there are confounding variables that our model leaves out. Leaving out important causes makes the model unrealistic. But given the right information, instrumental variables can show that our model nonetheless provides well-confirmed causal explanations. To use instrumental variables, we need to show that there is a variable influencing the alleged cause but not the dependent variable or effect. We can then use the variability in C caused by the instrument I to estimate its influence on E independently of possible confounding variables effecting C and E. When these conditions are met, standard regression methods –two stage least squares–can reliably estimate the influence of C on E despite ignoring the confounder. So this is a case of a good argument for an unrealistic model.

However, note the nature of the argument: it is a causal argument invoking a quite specific causal structure, not a purely formal statistical argument. Econometricians are often confused about this, defining an instrumental variable as one where the instrument is strongly correlated with the cause and not correlated with the effect. But, as usual, causes cannot be gotten from correlations, and as Reiss (2005) has shown, it is easy to imagine situations where an instrumental variable is not correlated with the outcome or the error term and is correlated with the cause being studied, but the instrumental variable is not a cause of the variable in question.

However, economists often just want an instrumental variable to be able to do consistent estimation in the technical statistical sense, since unknown confounders can produce estimates from samples to populations that do not converge on the correct estimate as the sample size increases. Instruments that only meet the statistical criteria do suffice for this goal, which is a rather different project than providing well-confirmed causal explanations. This is an instance of my claim that whether realism matters depends on the purpose of the model and the methods being used: if you want to confirm a causal explanation with left out confounders, you need a realistic causal model; if you only are using the model to make consistent estimates about a population the realism is not necessary.

A related but alternative way of arguing that we can leave out unknown confounders comes from sensitivity analyses. Sensitivity analyses provide arguments that even if there were confounders that are left out, they would have to take on unreasonably high values to completely undermine the causal relations postulated in the model. Suppose that I believe that *C* causes *E* but that I also worry that *CC* is a common cause confounding the relation. I can ask how large must the relation between the common cause *CC* and the *C* and *E* relation be to make the correlation between *C* and *E* to be entirely spurious. There are both structural equation and regression based ways of testing this hypothesis.

Note that sensitivity analysis here is much more helpful than what is commonly called robustness analysis. Robustness analysis is common in economics. It involves seeing whether conclusions from models hold up under changed assumptions. Yet such analyses can move from one unrealistic assumption to another and there is no guarantee that a more robust model is likely to be more reliable guide to reality.

Finally, structural equations, using the Pearl causal calculus approach, provide another sense in which we can argue that unrealistic models can nonetheless provide well-confirmed causal explanations despite unrealistic assumptions. Typically, a structural equation model in the social sciences assumes that the relations between variables are linear. But if my goal is to provide evidence for causation rather than the *size* of causal parameters, I can use such a structural equation model even if the linearity assumption is wrong. Causal relations entail dependencies and independencies among the variables but makes no requirements on their functional forms. So long as the requisite dependencies and independencies show up, it does not matter what their functional form is and thus it does not matter that I have estimated an unrealistic linear model.

Note again the nuanced role of mechanisms in these three approaches to dealing with unrealistic models. Instrumental variables allow us to confirm a causal relation without knowing the confounder(s) so in that sense we do not need mechanisms. Yet we need to have evidence that the instrumental variable does not causally influence the effect. In that sense we need to know about mechanisms. Much the same holds for sensitivity analysis: it allows me to find evidence for a causal relation without knowing what the confounders are–to that extent I do not need to know what the mechanism is. Finally in the case of inferring causal effects (but not causal effect sizes), I do not need to know the details of the causal relations–the precise functional forms of their relations–and in that sense need a minimal description of the mechanism. Yet I need more information if inferring effect sizes is my goal and so then mechanisms carry more weight .

5 Showing that irrealism is a necessity

So the moral of these three examples is that with sufficient information we can confirm unrealistic models, but that information is causal and not just formal or statistical and in some ways mechanisms are irrelevant and in other ways needed. I want to now show that sometimes models that *leave things out* –unrealistic models—are better at getting us well-confirmed causal explanations than models that are more realistic in that they do not leave causes out. The context I have in mind is the traditional multiple regression frame work that is the work horse across the social sciences. The kinds of models I have in mind here are the more empirical models involved in statistical testing, not the abstract theoretic models often involved in economics.

Standard practice for social scientists using multiple regression is to include "control variables" or "covariates" when looking at a specific causal relation of interest. The general tendency is to err on the side of including more such variables rather than fewer. The thought is that doing so gives more realistic models that will help eliminate confounding.

However, this "include everything in the model approach"—which seems like a move towards greater realism—can be the enemy of well-confirmed causal explanations. How so? In two common causal situations, including covariates using multiple regression techniques can result in seriously erroneous causal claims. Here are two clear situations:

1. including mediator variables as controls in regressions
2. including collider variables as controls in regressions

We have mediators when we simply have a causal intermediary between a more distal causal and the final effect we want to explain. A collider is a variable that two or more other variables cause. They "collide" on their common effect.

In both cases having a more realistic model that includes mediators or colliders, given that we are using multiple regression techniques to look for causes, leads to error. If I control for an intermediate variable—hold it fixed—then I remove the connection between the distal cause and the final effect. I will conclude that there is no relation between the distal cause and the effect when there is in fact one. If I control for a collider, I create correlations between its two causes that are spurious; holding the collider constant creates a spurious connection. Thus including the collider leads to mistaken causal conclusions. So in both cases, using multiple regression techniques, I should be less realistic in the sense of leaving such variables out of my models when I go to test them.

These kinds of problems have important real world effects. For example, economists have used data sets from most countries in the world to examine the causes of economic growth. Generally what they have done is to treat all possible causes of growth as independent factors and regressed measures of growth on these variables. However, this is an extremely simple model. Most likely there are complex causal relations between the independent variables causing growth. That means it is likely that the regressions in cross country studies are controlling for mediating causes and colliders. So these studies are quite likely to eliminate connections that are there by controlling for intermediate causes and to create spurious correlations by controlling for colliders. In my previous work (Kincaid, 2014) I have shown this to be the case: the simple additive model where every possible cause is controlled for fits the data very badly; models with more complex causal structure do much better. I show that the standard naïve models which typically find that education has no influence on growth are not well supported by the data; models that allow for mediation and colliders find that education does indeed contribute to growth and are much better fits to the covariances found in the data.

So to conclude, I hope to have shown at least two things. Treating the question of unrealistic models as a set of more specific questions about particular models using particular methods allows us to make progress on the issues in a way perhaps not possible if the issue is approached in perfectly general way. Similar conclusions hold about the role of mechanisms: their importance and place varies according to the kind of causal claims being made and the methods used to support them.

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