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Models, Representation, and Economic Practice

Commentary on Uskali Mäki

Few, if any philosophers of economics and economic methodologists have been brought up without being nurtured on Uskali Mäki's writings on idealisation, models, realism, truth, isolation and many other aspects of economic methodology. I certainly have been. In graduate school, his article 'Scientific Realism and Some Peculiarities of Economics' (Mäki 1996) was presented to me as a classic, and I still use it to teach my own students about realism. It is therefore a particular pleasure to have been given the opportunity to provide some thoughts on Mäki's latest on models and idealisation.

The aim of Mäki's paper is three-fold. First, he outlines a number of epistemic virtues economists seek in models – such as being constrained by theory, being parsimonious, broadly applicable, couched in mathematics, uninfluenced by findings in other disciplines as well as providing insights into phenomena and their generative mechanisms – as well as obstacles to their realisation, e.g. the Duhem-Quine problem. Second, he gives a new formulation of his account of modeling and defends some of its aspects. Finally, he discusses three challenges critics have posed to economic modellers and either rebuts or sustains these challenges. In my comment I will focus on the account of modeling and how it deals with the three challenges.

1 Representation by Models

For convenience let me repeat Mäki's account of representation here:

[ModRep]
Agent *A* uses (imagined) object *M* as a **representative** of (actual or possible) target *R* for **purpose** *P*, addressing **audience** *E*, at least potentially prompting genuine **issues of resemblance** between *M* and *R* to arise, describing *M* and drawing inferences about *M* and *R* in terms of one or more **model descriptions** *D*, and applies **commentary** *C* to identify and coordinate the other components.

[ModRep] departs, more than any other account, from the usual way of thinking of representation as a two-place relationship between a model and a target. Why two additional places should be included is straightforward. Nothing is a model of something else without being stipulated as such – or ‘*used* as a representative’ as Mäki states. Thus, agency is essential. And there is no way to tell whether a model is a good one, or whether it is an accurate or adequate representation, without specifying a purpose.

The reasons for including the other aspects are less conspicuous, despite what Mäki remarks on pp. 91–94. One way to understand them would be to hold that representation is not a four-place relationship between a model, a target, a user or agent, and a purpose, but rather a seven-place relationship that also includes an audience, a description and a commentary.

If so, we may ask why we need an audience in addition to a purpose. I was taught about the solar system using a mainly mechanical model that had a big light bulb at its centre to represent the sun. I do not think that model was of much scientific use. Its purpose was to help the teacher getting across some basic astronomic knowledge to the students. The purpose, properly specified, includes the audience. We could ask: does the representation relation change when the audience changes? Surely not when the solar system model is used between 9 a.m. and 10 a.m. for one set of students and between 10 a.m. and 11 a.m. for another. But it would make a difference if a crazy scientist, who would use the model to draw inferences about how to send a probe to Mars, replaced the students. However this would be because the model was built for the purpose of education (or, more specifically, of educating grade-eight grammar school students in the UK with such-and-such a background and …) and not for calculating the trajectory of Mars probes. Thus, purpose includes audience.

The same is true for the commentary. A commentary can draw our attention to the fact that a model was built for one purpose and not for another. David Colander suggested it might be a good idea, if an economic model included ‘warning labels to prevent the model from being misused’ (Colander 2010). The fact that a model was built for one purpose and not another makes some applications of the model instances of misuse, not the commentary. A different commentary does not change the representation relation as long as the purpose stays the same.

Finally, it is true that models always occur under a certain description. But it is noteworthy that the descriptions define the model. It is not the case that the same model M has a different representation relation to its target R when the description of it is changed. Rather, the model has changed – and it is *qua* that change that the representation relation may or may not have changed.

Therefore, Mäki cannot mean that representation is a seven-place relationship. Instead, I would suggest, the somewhat Baroque account is meant to remind us that representation is a complex scientific activity that cannot be reduced to the simply minded search for similarity relations between one object (the ‘model’) and another (the ‘target’). This, to my mind, is entirely correct and an important point to make.

The representation relation itself has, according to Mäki, two aspects: the ‘representative’ aspect and the aspect of ‘resemblance’. The agent decides whether an object is a representative of another. This is why I stated above that agency was essential. At this stage there are not yet limits to what can be used as a model of something else. If I point to some sprawling weeds in my garden and say: ‘These weeds are a model of world capitalism!’, then the weeds *are* a model of world capitalism. *Whether* something is a model of something else is decided by fiat or use. If someone countered my exclamation with: ‘No, they are not!’, he would not have understood the rules of the game. But of course, that something is a model of something else does not automatically mean that it is also a *good* model of it. This is why in addition to representativeness we need resemblance.

Whether or not any given model is also a good model of its target depends in part on the purpose of its use. If my intention is to suggest that capitalism takes over even the remotest corners of the world economy, like the weeds are taking over my garden, then they might well be a good model. But certainly I will not learn many useful things about the causes of world capitalism’s behaviour by examining my weeds.

Now, while Mäki does not address this issue explicitly, his paper suggests that he takes the circumstance whether the model resembles its target to be a fact about the relation between the model and its target. Pragmatic factors determine, which aspects of the model (or the model/target relationship) are relevant. But once this issue is settled, facts alone determine whether a model bears ‘resemblance’ to its target, and thereby whether the model is a good one.

In my view, this gives context and purposes a too small role to play in representation. Resemblance is not a natural kind whose presence is determined by the facts alone. Any two objects are similar and dissimilar in uncountable ways. We need context and purpose to determine not only what aspects are relevant, but also in what sense model and target should resemble each other. Paul Teller makes the point succinctly as follows (Teller 2001: 402):

> In short, once the relevant context has been specified, for example by saying what is to be explained or predicted and how much damage will result from what kinds of error, the needs of the case will provide the required basis for determining what kind of similarity is correctly demanded for the case at hand. More specifically, similarity involves both agreement and difference of properties, and only the needs of the case at hand will determine whether the agreement is sufficient and the differences tolerable in view of those needs. There can be no general account of similarity, but there is also no need for a general account

> because the details of any case will provide the information, which will establish just what should count as relevant similarity in that case. There is no general problem of similarity, just many specific problems, and no general reason why any of the specific problems need be intractable.

2 Challenging Economic Models

Mäki discusses three sets of criticisms commentators have levelled against economic models. The first challenge is that economic models cannot be epistemically useful, because they simplify and idealise. The second holds that the particular assumptions a particular model makes may be unsuitable for a particular task at hand. The third criticism concerns the economics profession at large and maintains that economists practise modeling too much (or even exclusively) for its own sake rather than with specific (policy or other practical) applications in mind. Let us consider these in turn.

(A) Simple and idealising models (SIMs) cannot be epistemically useful.

To begin with a disclaimer, I do not know anyone who makes the criticism at this very general level, and Mäki does not provide a single reference to anyone who does. The charge is highly implausible: all models simplify and idealise in myriad ways (for a classification, see for instance Wimsatt 2007: 101–102), and it would be hard to maintain that no model is epistemically useful. Perhaps the charge is somewhat more specific: all *economic* models simplify and idealise too much *relative to economic reality*. Again, I know no one who would hold such an implausible view. Tony Lawson (1997, 2003) comes close, but his criticism concerns the mathematisation of economic models, not models (or simplification/idealisation) *per se*. All other critics I am aware of make more nuanced remarks, remarks concerning specific modeling strategies and specific domains of application.

Mäki nevertheless provides two defences. First, even the most highly simplifying and idealising assumptions may be considered to be mere ‘early step assumptions’, which are to be replaced by more realistic assumptions at a later stage. Second, SIMs often provide ‘how-possible explanations’.

Neither line of defence is entirely convincing. The ‘SIMs play a heuristic role for future models that are epistemically useful’-defence leads into a dilemma. Either simplifications and idealisations can be relaxed so as to make models more realistic and thereby epistemically useful or they cannot. If they can, one would have to be able to tell a good story of what precise heuristic role the SIMs play on the road to better models (why do we build epistemically useless models if we can have useful models?), and I would imagine this will be no mere trifle. More importantly, however, one could simply ignore SIMs and focus on those models that are useful. The criticism would amount to saying no more than '*some* models are not useful'. But that's hardly a criticism.

Or the simplifications and idealisations cannot be relaxed. But then models involving them could not play a heuristic role for better models. Either way, this line of defence leads straight into a *cul-de-sac*.

The other defence is that SIMs can provide 'how-possible explanations'. But this notion has the modal operator in the wrong place. A 'how-possible "explanation"' is not an explanation. It is *possibly* an explanation. Suppose an implication of a SIM is a claim of the following form: 'In situation *S* (which can be described by conditions *s*1, *s*2, ..., *s*n), factor *C* causes outcome *E*'. This allows us to provide a more precise characterisation of what a SIM is: a model, which entails causal claims that are true under conditions rarely or, more frequently, never found empirically. Typical examples of such conditions include a continuum of economic agents, agents who are perfectly rational, agents with an infinite lifespan, businesses located on a line that has neither depth nor breadth, consumer goods that have a single property and so on.

There is indeed a sense in which SIMs make a possibility claim, *viz*. they show that *it is possible that C* causes *E*. It is important to see, however, that this is a very weak sense of possibility, something like logical or conceptual possibility. SIMs do not prove an existence claim of the form: 'There is an empirical situation S^e in which *C* causes *E*'.

I have given an account of how models, resulting in those possibilities, can be epistemically useful (Reiss 2008: Ch. 6). Essentially, if everyone in some epistemic community at some point in time is convinced that it is impossible that *C* causes *E*, it might well be useful to learn that there are conditions, even though non-empirical conditions, under which *C* does cause *E* because now we have a reason to investigate empirically whether *C* causes *E* in situations that interest us. A SIM, in my 2008 terminology, gives *prima facie* evidence for a causal claim: a licence to further investigate it. (Till Grüne-Yanoff 2009 gives a very similar albeit more detailed account.)

But this account is not Mäki's. To show that a causal relation is logically or conceptually possible is not to explain anything. Take the famous Akerlof lemons model in which asymmetric information brings about market failure (Akerlof 1970). Mäki is of course right to say that Akerlof provides an account of how market failure might come about. But that account explains no single instance of market failure. Rather, it gives a possible or *potential* explanation. A potential explanation is not a genuine explanation unless all other potential explanations have been ruled out. Therefore, the 'how-possible explanation' defence does not work, either.

(B) Specific SIMs simplify and/or idealise too much or in the wrong way (to be useful for the task at hand)

For this charge to have any bite, one must couple it with the empirical claim that such 'bad' models are typically used in epistemically or practically important applications or both. One does not have to go far afield to find some evidence for that empirical claim in the current situation in which blaming economists and their modeling practices for the financial crisis of the late 2000s has become an academic fashion (see for instance Acemoglu 2011; Akerlof and Shiller 2009; Cassidy 2009; Colander 2010; Colander et al. 2009; du Plessis 2010; Hodgson 2011; Kirman 2010; Lawson 2009, Ormerod 2010; Roubini and Mihm 2010; Stiglitz 2009, 2010, 2011). Mäki joins this choir, but goes beyond many of the other commentators for he provides a general methodological account why it is the case 'that macro and financial economists helped cause the crisis, that they failed to spot it, and that they have no idea how to fix it' (*The Economist*, July 16 2009): their models exclude non-negligible causal factors.

Here Mäki's realism stands in the way of a more nuanced analysis. Economists are not in the business of building models that represent all and only those factors that are causally relevant for outcomes of interest. They are in the business of building models that describe and predict, explain and underwrite policies. Non-negligible causal factors will no doubt play *some* role in such models. But, depending on the purpose, such factors will often play an attenuated or negligible role.

We all know that one does not need causality for predictive success. For a classical philosophers' example, the barometer reading reliably predicts the storm without causing it. If the goal is to predict a storm, there is little reason to model all the causally relevant factors for storm. It is indicator variables we need, and barometers are good indicators.

The problem for the causal realist is that factors that *cause* outcomes of interest are never essential and often of limited usefulness. Explaining phenomena is the best test case, because of the tight semantic connection between 'causes' and 'explains'. But even for explanation causation is not essential. Though highly successful, the causal account of explanation is not the only existing one. Most famously, there is the alternative account of explanation as unification (Friedman 1974; Kitcher 1985; 1989). This is, of course, not an argument in itself, as the unification account might just be wrong. But what is important to understand is, that the causal account is difficult to square with the fact that all models idealise heavily and yet appear to be explanatory – and are often taken to be explanatory by economists and many other scientists (see Reiss 2012 for a discussion; for a defence of the causal account in the light of idealisations, see Strevens 2007). The causal account is also difficult to square with the fact that some relations seem to require non-causal, but explanatory relations such as constitution (Ylikoski 2011). The least we should take from these considerations is that not all successful explanations are causal.

Finally, it is clear that successful *descriptions* do not always require causal information (for an argument to the effect that causal-mechanistic information is not always helpful for description, see Reiss 2007), and it has been argued that causal relations are not needed for policy analysis (Leuridan et al. 2008).

The upshot of this discussion is that a more nuanced argument is needed to support sweeping claims of the sort 'economists' models helped cause the crisis'. There is no unique recipe for failure one might say. Omitting non-negligible causally relevant factors in a model may well be a reason for failure. Only in the light of a specific use of the model and if an argument to the effect that the omission was essential for the failure is available, we can determine whether this is so.

(C) Modeling for modeling's sake

The final challenge is normative. A model, as we have seen, is good or bad only in the light of the purpose pursued with its construction and use. Any related methodological criticism is consequently instrumental: we criticise models not as such but rather as means to given ends. But many methodological debates concern in fact the ends themselves: do we want models to describe and predict or shall we seek explanation in addition (famously, Friedman 1953)? Shall we, perhaps, ultimately seek only explanation, because prediction is impossible and description is subsidiary (e.g., critical realism: Lawson 1997; 2003; the new mechanists: Elster 2006; prediction is impossible: McCloskey 1998)? *Is* accurate description of merely instrumental value or is it an important end in itself (e.g., Sen 1981)?

The present charge concerning the aims of economic modeling is that economists too often engage in the pursuit of models with primarily non-empirical virtues at the expense of the more empirical description/prediction/explanation/policy analysis. What these non-empirical virtues are is not quite clear. In a widely cited op-ed piece, Paul Krugman lamented that economists were 'mistaking beauty for truth' (Krugman 2009; see also Juselius 2009). Mäki puts it differently: economists too often pursue *substitute* in lieu of *surrogate* modeling. A model is usually a model for or of something. An animal model is called such because it is examined in order to make predictions about other animals, usually humans. The mechanical model with the lamp at its centre I mentioned above was a model of the solar system. Mäki calls this kind of modeling practice in which a model is used as a stand-in for something else, 'surrogate modeling'. In surrogate modeling, we examine one system to learn about another, because the latter is epistemically inaccessible for technological, financial or ethical reasons.

'Substitute modeling' works without target systems of interest. The model system's properties are examined, but not for the sake of learning about another system. The model system's properties are examined for the sake of learning about the model system.

Mäki calls this activity 'degenerate' (p. 101). This is a little too fast, however, according to Mäki's considerations that follow. Perhaps there is some sort of division of labour going on between 'theoretical' economists devising models and investigating their properties and 'applied' economists using the models to describe, predict and explain phenomena of interest, and to prepare policy.

Something goes wrong only when the discipline as a whole becomes one of substitute modellers – because no one is left to apply the models to our urgent practical and policy problems (p. 104).

Though I share Mäki's concern in principle, I would like to add that divisions of intellectual labour of the proposed kind often only happen to the detriment of the practical goals of a science (Kitcher 2001; Cartwright 2006; Reiss 2008: Ch. 5). Simply because of the way in which science proceeds, one cannot easily separate the more theoretical role of constructing problem-solving templates of wide applicability and the more applied role of using these templates for solving concrete problems. New models always build on old models; and if all the models there are were built with a particular purpose in mind, it is very difficult to build new models for different purposes (Biddle and Winsberg 2009). One would have to start from scratch. But letting applied scientists build models that are useful to them from scratch is exactly what the proposed division of labour is meant to prevent.

Robert Sugden senses this problem when he writes (Sugden 2009: 25):

> In the light of Schelling's argument about social mechanisms, however, I cannot claim that theorists who make such claims are necessarily committing methodological errors or failing to act in good faith. It is just that the approach of looking for significant mechanisms while not trying to explain anything in particular seems unlikely to be productive.

My point is stronger: to build models with no particular application in mind *is* to commit a methodological error – as long as the aims of economics are considered to be largely practical.

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