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Contested Modeling: The Case of Economics

1 Introduction

Economics is a culturally and politically powerful and contested discipline, and it has been that way as long as it has existed. For some commentators, economics is the "queen of the social sciences", while others view it as a "dismal science" (and both of these epithets allow for diverse interpretations; see Mäki 2002). Economics is also a discipline that deals with a dynamically complex subject matter and has a tradition of reducing this complexity by using systematic procedures of simplification. Nowadays, these procedures involve for the most part building and using mathematical models (for an overview of the philosophical issues, see Morgan and Knuuttila 2011). In the dominant circles of the discipline, one is not regarded as a serious economist having a professional expert view on any given economic or social issue without having a model about it. Much of the power of the discipline and its characteristic contestations therefore involve models and modeling: the successes and failures of the dismal queen are those of modeling. The issues involved in economic modeling have been made particularly acute once again by the financial crisis of 2008–2009 and its aftermath: the discipline of economics is among the candidates for the major blame for failure.

I will first outline some thoughts about the characteristic disciplinary conventions that guide and constrain modeling in economics. I will then summarize my account of the very ideas of models and modeling. Finally, within the framework of that account, I will highlight some major issues of contestation and sketch the respective notions of potential success and failure in economic modeling with illustrations. These notions are motivated by my subscription to a (flexible and discipline-sensitive) realist philosophy of science (e.g. Mäki 2005).

2 Some characteristics of economics and economic modeling

The notions of model and modeling can be conceived broadly or more narrowly. Broadly conceived, they encompass activities such as theoretical modeling, laboratory experimentation, and computer simulation. These are examples of *surrogate reasoning* that share the strategy of using one thing (the surrogate object) to learn about another thing (the target object). While in other ways different from one another, the study of theoretical models, laboratory experiments and simulations is similar in being species of surrogate reasoning (for the similarities and differences between theoretical modeling and experimentation, see Morgan 2003; and Mäki 1992, 2005). More narrowly conceived, of these three categories, only theoretical modeling qualifies as proper modeling. The present paper has a narrow focus on theoretical models and modeling in economics.

The prevalence of modeling, whether more broadly or more narrowly conceived, is particularly salient in research fields that seek [a] to access targets that are complex (such as biology, ecology, climatology); [b] to access targets that are distant in time or space, very small, very large, very slow, very fast, or ethically awkward (such as cosmology, archaeology, evolutionary theory, nuclear physics, biomedicine); and [c] to access familiar but complex targets whose overall functioning is often unapparent, possibly for reasons such as those listed above in [a] and [b] (of this, economics is a prominent example).

Each discipline or type of discipline has its own ways – styles, routines, values, and conventions – of modeling, and they are not a simple function of the specific nature of its target domain. It is always somewhat risky to make generalizing claims about a discipline and its characteristic practices and the values guiding those practices, but I believe the following will be recognized as more or less accurate regarding much of economic modeling in the recent decades.

First, economic modeling is often theory-driven, shaped and constrained strongly by the dominant theoretical framework. This framework nowadays usually requires that models be built in terms of optimizing agents and equilibrium outcomes.

Second, the combination of parsimony and breadth is highly valued in economics. This means that there is an urge to increase the unification of diverse phenomena in terms of portable model structures or modeling principles, that is, structures or principles that can easily be transferred from one domain (or even discipline) to another (see Mäki 2001, 2009).

Third, what is typically highly valued in economic modeling is mathematical rather than numerical precision. It is not surprising therefore that analytical derivation tends to be preferred to computer simulation (see Lehtinen and Kuorikoski 2007).

Fourth, among the achievements of theoretical modeling economists often mention that models provide some "insight" into phenomena and the mechanisms which produce them; that they yield conditional predictions that state that if certain conditions were to prevail, then this or that would happen; and that they suggest how-possibly explanations that give account of ways in which some given phenomena might have come about (in contrast to how they actually did come about).

Fifth, the so-called Duhem-Quine problem of underdetermination of theory or model choice by the empirical data is particularly pressing in economics. In practice this often means that theoretical disputes are hard to settle by empirical means, and that theories and models that were thought to have been refuted by empirical evidence often make a comeback and enjoy long academic lives.

Sixth, the dominant streams of 20th century economics have for the most part been characterized by one-way disciplinary autonomy, that is, the relative reluctance to import to economics substantive ideas from other disciplines such as sociology or psychology.

Disciplinary conventions are not carved in stone, so they may occasionally be subject to modification or rejection. Some of the above disciplinary conventions of economics are being increasingly questioned and alternatives are being tried out. For example, the proportion of data-driven modeling and computer simulations, even if still relatively small, has been increasing, and there is growing interdisciplinary traffic flowing to economics from experimental psychology and cognitive neurosciences. In the aftermath of the crisis of 2008–2009, many economists have proposed that, in order to understand the mechanisms that tend to bring about this sort of crisis, economists should better do agent-based simulations (e.g. Farmer and Foley 2009) and incorporate "animal spirits" in their models, informed by cognitive sciences broadly conceived (e.g. Akerlof and Shiller 2009). Predicting the future of a discipline is always difficult, but economics may have started becoming more diverse in its disciplinary conventions than has been the case in the recent decades.

Whatever the future of the discipline may hold, its past has had one perennial methodological issue above others. This is the concern of unrealistic models and their assumptions – such as perfect competition, the fully informed self-seeking rational *homo economicus*, instantaneous and cost-free market adjustment in an institutional vacuum, international trade with two countries, two goods and two factors of production, and so on. Within and around economics, nothing compares to the most important methodological issue: what if any justification might be available to unrealisticness in models and their assumptions?

In dealing with this issue, economics have to overcome pressures and worries from two directions. There is the "phenomenological" pressure and the respective worry: *Does the world look like that?* This is the puzzlement among audiences such as beginning economics students and other uninitiated observers such as other social scientists. There is also the "practical" pressure and the related worry: *Does the model work?* This reflects the expectations among the policy-advising economists as well as consumers of economics such as policy makers and the general public as spectators of the performance of economics regarding its policy relevance, akin to other technologically oriented engineering disciplines.

The *phenomenological worry* derives from the fact that ordinary people, including students of economics, are also economic agents with amassed collectively shared experience and commonsense conceptions about the economy. The contrast between theoretical models and phenomenology is often stark. This discrepancy has two very different sources. First, economic models are formulated in terms of assumptions that radically idealize items in the commonsense experience, e.g. when the behaviour of ordinary people is portrayed as that of the fictional *homo economicus*. Second, economic models typically provide (invisible-hand) explanations, which are surprising and counterintuitive from the commonsense point of view, e.g. when free trade is modelled as benefiting all parties and when apparently irrational herd behaviour is modelled as arising from individual rationality.

The *practical worry* is equally pressing. Economics is regularly faced with charges of practical failures. This is an ongoing concern, but in every few decades the credentials of economics are questioned more seriously in public. In these situations, the challenge of academic accountability of the discipline is turned into one of broader public accountability. In fact, we have such a situation right now. On 16th July 2009, the *Economist* magazine wrote: "Of all the economic bubbles that have been pricked, few have burst more spectacularly than the reputation of economics itself."

The two worries – the phenomenological and the practical – imply that a commentator of economic models has to meet special challenges. The clash of theoretical models with commonsense views implies a need to understand the origins of the clash and the associated attitudes and arguments, including attempts to justify theoretical models not only as unproblematic, but also as superior to the commonsense conceptions. The successes and failures of economics in guiding economic policy likewise give rise to the call for explanation and justification of the varying practical performance of the discipline. For these purposes we need accounts of criticism and defence of, as well as success and failure in modeling. They are accounts of various kinds of contestation faced by economic modeling. Before these will be discussed, an account of modeling is needed.

3 The very ideas of model and modeling

The key idea of modeling is to examine one thing (the target) by examining another (the model). Using models is motivated by the circumstance that there is no direct and easy epistemic access to the target. A model is, at best, a surrogate object in the following way: By directly examining what happens in the surrogate object the investigator seeks to indirectly acquire information about the target object. The surrogate object is taken to stand for the target and must be required to be sufficiently similar with it for such information acquisition to be possible. Another way of putting this is saying the surrogate object *represents* the target object.

In many contexts, we are inclined to talk about models in simple dyadic terms such that one thing is a model of another. In order to understand what a model is, however, it is not enough to think of it in terms of a dyadic relation between two objects, the model and its target. Recent philosophical work has stressed the roles of two further components in constituting a model, namely an agent and a purpose: an agent considers or uses one object as a model of another object for some purpose (e.g. Giere 1999). The recent literature has also investigated the notion of representation in connection to models: indeed, models are typically conceived as representations of their targets. Yet there is no elaborate notion of model representation available that would express a consensus view. In my opinion adequate accounts of model and representation should be richer than has been customary. Further elements are needed in addition to agents and purposes associated with models. I have suggested such a richer idea of model representation that takes representation to have two aspects, those of representative and resemblance. It distinguishes between a model and its description, and it adds the ideas of audience and commentary to the overall notion (e.g. Mäki 2009a,b, 2011). Here is a formulation of this account of model representation:

[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.

There are several noteworthy features in [ModRep]. Nothing is a model without being used as such by some individual or collective *agent*. Use implies purpose. A model can be used for a variety of different *purposes*, such as predicting some future event or property with a certain degree of accuracy; isolating a fragment of a causal structure; exploring possible causal configurations; serving as a benchmark; refining a mathematical technique; designing a well-functioning institution; and so on. Reflecting the social nature of scientific inquiry, models are used in relation to various *audiences* – such as specialists in the same research field, students, policy makers, the curious general public – in order to pursue goals such as communicating information, teaching undergraduate students the core principles of conventional economic reasoning, and persuading some relevant audience to adopt a point of view. The choice of *model description* typically reflects the presumed expectations and competencies of the relevant audience. For example, advanced mathematical languages may be used when addressing expert scientists in the same field, while familiar metaphors and visualizations of various kinds may be relatively more effective when addressing beginning students and lay audiences.

The very idea of models as representations can be briefly summarized. I take representation to involve two aspects: that of representative and that of resemblance. A model represents (rather: is used to represent) a target by being (used as) its *representative*, by standing for it. This is the relatively more voluntary side of modeling: the modeler chooses (or chooses to build) the object that is then used as a representative of some target. This is not yet sufficient for representation: not just any object can reasonably represent the target object. Some further conditions or constraints must be met, and these are not entirely subject to the decision of the modeler. The key condition in [ModRep] is given by the second aspect of representation, that of *resemblance* or similarity with some target. This idea comes with two important qualifications. First, it is not required that the model actually does resemble the target, it is rather required that the model has *a chance of resembling* some target and that this *potentially prompts the issue of whether indeed the model does resemble*. Further inquiry may then settle this issue, but such inquiry is not required for establishing whether the object represents or not. Further inquiry *is* required for establishing whether the model *truthfully* represents.

Resemblance is a matter of how the model world – the world envisaged in a model – is related to the real world. The second qualification is that what really matters in modeling is not resemblance per se, but rather *relevant resemblance*. The notion of relevant resemblance combines ontological and pragmatic perspectives in modeling: resemblance is an objective matter of fact, while relevance derives from the modeler's interests and goals, purposes and audiences. Among the latter there are goals such as predicting the inflation rate with a degree of accuracy useful for economic policy makers; outlining the core structure of a bubble-generating mechanism in a way that is understandable for the electorate; showing that a result is robust to a change of an assumption so as to impress one's peers; unifying diverse classes of social phenomena so as to expand the academic authority of economics; designing a regime of regulation for the financial sector useful for legislators.

Relevant resemblance is always incomplete and imperfect. Complete and perfect resemblance is unattainable (and would be impractical anyway), and most partial and imperfect resemblances are irrelevant for a given purpose and audience. A model that relevantly resembles a target, resembles it in a specifically limited way that serves a purpose and helps to reach an intended audience. It highlights only some selected aspects of the real world and does this in some imperfect degree of accuracy. Relevance is a function of the pragmatic context of purposes and audiences, while relevant resemblance is a function of the pragmatic context together with the relationship between the model and the target object. It is the task of model commentary to point out what kind of relevant resemblance is being sought and perhaps achieved (and it is the task of philosophical analysis to investigate whether relevant resemblance can be interpreted as truth or not; for a positive answer, see Mäki 2011a).

This framework also helps to understand the role of false idealizing assumptions in modeling. They are among the descriptions of a model. If we take models to be imagined systems, it is obvious that such systems can be described in many different ways, such as in terms of verbal means, mathematical equations, graphs and diagrams and other visualisations. So idealizing assumptions can be considered as describing or even defining models rather than being statements about some real-world system. The challenge then is one of understanding and justifying such idealizations. The answer lies in the analogy between model and experiment, or what I have called the "experimental moment" in theoretical modeling.

Structurally, theoretical modeling is similar to laboratory experiments. Both aim at the isolation of some important relationship or mechanism – theoretical and material isolation, respectively. Both pursue it by controlling things other than those that are being isolated. Laboratory experimentation does this by *causally manipulating* those "other things" while theoretical modeling does the same by *making assumptions*. There is therefore an obvious sense in which theoretical models are thought experiments. (Mäki 1992)

Model commentary is an important part of modeling. It plays a key role in identifying and coordinating the other components of model representation. By specifying the purposes and audiences of the exercise, it fixes the standards of relevance. By illuminating the roles played by idealizing assumptions, it can dispel unnecessary suspicions about some models while helping raise legitimate doubts about others. For example, an informed commentary should be able to show differences between defending an assumption on grounds such as the following four (Musgrave 1981; Hindriks 2006; Mäki 2011b). First, a false idealizing assumption – such as that the information held by economic agents is symmetrical – can be defended by saying that it should be interpreted as the possibly true claim that the asymmetries in information are *negligible* for some given purpose. Second, an assumption can be defended by suggesting that the model is only *applicable* to domains where information is symmetrical or where the asymmetries are negligible. Third, it can be defended by suggesting that, ceteris paribus, the use of the assumption makes the modeling of some phenomenon (mathematically) *tractable* (or, in case it additionally distorts non-negligible facts, it should be criticized). Fourth, it can be defended by interpreting it as an *early-step* assumption that will be relaxed in later-step versions of the model; such a de-idealization is a way of de-isolating the model by bringing in previously excluded factors. This may aim at checking the robustness of the model's basic view of the world (see Kuorikoski, Lehtinen and Marchionni 2010) or bringing the model closer to being applicable to some specific domain.

4 Economic modeling contested: Three ways

Models are often contested by raising issues of relevant resemblance. A model – or a family of models, or the strategy of modeling – can be challenged by claiming that it fails the test of resemblance relative to some purpose and audience. Or the charge can be that the model commentary has failed to identify the functions of particular idealizing assumptions or the limits of applicability of a model, and so on. Using the account of models and modeling outlined above we can now identify three ways of contesting an economic model, a family of models, or a style of modeling (see Mäki 2009).

The most radical challenge questions the strategy of modeling in general as misguided simply because models and their assumptions are found to be so unrealistic that the strategy is judged to be unsuitable for accessing economic reality. This may manifest the phenomenological worry based of a perceived dissimilarity between the model worlds and the real world, suggesting that *unrealistic models cannot possibly serve as surrogate worlds* that might pave the epistemic way to the real world. To this suspicion my response has been, and continues to be, that unrealistic assumptions per se are no obstacle to successful surrogate modeling.

The second kind of contestation considers unrealistic economic models as potentially successful surrogate objects helpful for acquiring information about the complex real world, but *criticizes particular surrogate models for failure* in the task. While unrealistic assumptions in general are not an impediment to surrogate reasoning about the real world, particular unrealistic assumptions are taken to be the source of failure for they are responsible for the exclusion of important causal factors from a model. With respect to this charge, it is easy to agree that many models fail just in this way. In the recent years, many very important economic models were blamed for having failed in this manner, reinforcing the practical worry about misguided or missing policy advice.

The third variant of contestation puts forth the charge that *modeling has degenerated into producing and manipulating mere substitute systems* in contrast to surrogate systems. Research and reasoning are concerned with the properties of toy models only, with no further concern about how they relate to and might provide epistemic access to real world systems. Modeling becomes governed by consideration of tractability and mathematical convenience. Again, in response to this worry, it seems obvious that, among the many forces that govern economic modeling, there is also a temptation and tendency in economics to retreat to substitute modeling.

4.1 Can simple models with unrealistic assumptions serve as surrogate systems?

Simple economic models do not do justice to the rich and complex economic reality. Models involve idealizing assumptions that often severely distort the facts. Models depict closed systems, while the economic world is open and cannot be artificially closed. Therefore, theoretical modeling is a dubious strategy of inquiry in economics. This, or something along these lines, is a critique sometimes put forward against the possibility of successful economic modeling. The focus of this challenge does not lie on the particular idealizing assumptions used in particular models or model families, it rather lies on the strategy itself, that of simplification and idealization in economic modeling.

Consider a model of planetary motion that isolates a simple system that consists of one planet and the sun, both considered as mass points, excluding all other objects and properties and forces other than the gravity between the two included mass points. If the purpose of this model is to provide predictions with a certain degree of accuracy, and if it manages to provide them, it cannot be contested by raising a phenomenological worry just by pointing out that the idealizations of the model distort many features of the actual world.

Then consider the 2×2×2 model of international trade. It isolates a system of two countries, two goods and two factors of production (labour and capital), assuming that the factors are homogeneous and production technologies are identical between the two countries and exhibit constant returns to scale. Capital and labour can move within countries but not between them. Competition is perfect within countries, but firms are not considered in the model. There is no unemployment and there are no tariffs. The only difference between the two countries is their relative abundance of labour and capital. This simple Heckscher-Ohlin version of the 2×2×2 model isolates a mechanism of comparative advantage that generates outcome patterns in which capital-abundant countries export the products of capital-intensive industries, while labour-abundant countries export the good produced by their labour-intensive industries.

The assumptions of the simple Heckscher-Ohlin model are highly unrealistic and its implied prediction is inaccurate about the actual world. It may be hard to generally justify the idealizations as true negligibility assumptions, claiming that deviations from the facts are generally negligibly small for the predictions of the model to come out sufficiently correct. It may also be hard to find many empirical cases in which the distortions would indeed be negligible, so as to defend them as applicability assumptions. Therefore, they often serve best as early-step assumptions, which are to be relaxed and replaced by other more realistic assumptions. This is what has happened, both in a more piecemeal fashion and in more radical ways, which end up isolating different kinds of mechanism. "New trade theory" relaxes the assumption of constant returns to scale and assumes returns to be increasing. It brings firms to the model, but assumes them to be identical. "New new trade theory" allows for a diversity of firms and analyses their differential roles in relation to international trade. These developments suggest that the models are at least some of the time considered as surrogate systems. Unrealisticness as such does not undermine this ambition.

Perhaps the most striking example of this principle is what has sometimes been called the world's first economic model, J.H. von Thünen's *Der isolierte Staat*, a very simple and highly idealized model of the distribution of agricultural land use (von Thünen 1828; for an analysis, see Mäki 2011a). The model makes highly idealizing assumptions and implies a very idealized land use pattern of concentric rings. Among the idealizations, the region is assumed to be a perfect plain without mountains, valleys or navigable rivers; throughout cultivatable and of homogeneous fertility and climate; to have just one dimensionless town in the middle with a market on which the agricultural products will be sold; to be cut off from the rest of the world by a wilderness. Furthermore, transportation costs and land rents are assumed to be functions of the distance from the town (longer distances are associated with higher transportation costs and lower land rents). And naturally, agents are assumed to be rational maximizers, doing a perfect job in balancing the pull and push of the two magnitudes in deciding where to locate. The assumptions and implications of this simple model are false, but yet there is a fair chance that it manages to isolate a real mechanism that causally contributes to actual land-use patterns. The distortions by the assumptions might not be negligible if the purpose is to predict the outcome pattern with a relatively high

degree of accuracy, but they might be so for the purpose of isolating a fragment of the causal structure of the world.

An important condition for models to succeed as surrogate objects is for the model commentary to be informed about their capacities and limitations, their appropriate domains of application and the sorts of question they can be used to answer. There is a failure of model commentary in case a model is applied to domains to which it does not properly apply and is used for answering explanatory questions on which no illumination can be cast with that model. A good model commentary sees to it that models are applied in a way that promotes the pursuit of the goals for which they are fit.

Theoretical models in economics often provide *how-possibly explanations* – to be pointed out by a commentary. There is an observed pattern, such as a pattern of trade or of agricultural land use. One then suggests a model that depicts a mechanism that has possibly brought about the pattern. No claim is made at this point that the mechanism has actually generated the pattern. Indeed, it might have arisen in some other way as well (such as a land-use pattern having arisen as a result of centralized zoning). Models providing how-possibly explanations are surrogate models for they can be used for making claims about some real structural features of a domain of causes and effects. They often isolate mechanisms but are alone insufficient for determining whether those mechanisms are actually in operation and whether their operation is or is not modified or even overridden by other mechanisms. For these purposes, other models and an informed model commentary are required.

4.2 Failing surrogate models with failing unrealistic assumptions

In contrast to the suspicion discussed above, modeling is here not contested as a general strategy that in principle cannot succeed in generating reliable information about the real world. So the possibility of surrogate modeling is granted, but its actual implementation is judged as a failure. All models idealize and are simple, but bad models idealize and simplify too much or in a wrong way. The alleged reasons for such a failure can be many, such as mistaken background theories, incomplete or poor quality data, weak or misguided methods of testing, the tempting mathematical convenience of some idealizing assumptions, ideological bias, and so on.

What many of these criticisms share is the idea that *a model misses some causally important factors that should be modelled*. Another way of putting this is to trace the alleged failure of bad models back to some key assumptions that are claimed to be responsible for the failure. Those assumptions are idealizations that help exclude from the model world one or more factors that are causally important in the real world.

The flaws of economic models have been diagnosed with respect to the recent crisis in the same way. The two sets of models most often accused of major failure are efficient financial markets models and the macroeconomic DSGE (dynamic stochastic general equilibrium) models. They share the image of unregulated markets as efficient and basically self-correcting, and of economic agents as rational and well informed.

Models of efficient financial markets rely on assumptions such as zero transaction costs and perfect and symmetrical information between the agents. Such idealizations are instrumental in generating an image of the financial system in which market prices fully reflect all available information and in which there can be no bubbles in asset prices such as those of stocks or houses. This is a surrogate object that has the nice feature of being self-regulated and having the capacity of containing all relevant risks.

It then takes a major step to move from this surrogate world to the real world. This step can be taken in a variety of ways and on a number of grounds. One extreme and straightforward option would be to reject the model on phenomenological grounds, simply because the key idealizing assumptions seem to get the facts wrong. At the other extreme, without much further investigation, the model would be accepted as a true or useful surrogate system that is relevantly similar to the relevant target systems. It would be believed to get the important properties of the real financial system right – such as asset prices reflecting all available information, no bubbles being generated, and so the real-world financial system having the self-stabilizing properties needed for containing all risks.

The critics claim that economists or practitioners in the financial markets – enchanted by models of efficient markets – have been too hasty in concluding that there is relevant resemblance between the models and the real world, perhaps believing that informational imperfections in real-world markets are negligible. The critics believe there is no relevant resemblance at all, so the real-world imperfections are far from negligible. They argue that the properties of real-world markets may in fact be the reverse of those of the model-world markets:

> ... where the Efficient Markets Hypothesis suggests that financial markets provide a way of managing economic risk, the evidence suggests that they are actually a major source of risk. (Quiggin 2010, 51)

The charge might be put by saying that *economists have missed real-world risks by underestimating modeling risks*. The move from the model world to the real world

is typically far more difficult and risky than is the mere production of publishable results of the theoretical examination of models. It is these epistemic risks that may have been neglected.

The same complaint can be made about dominant macroeconomic models. In an interview in 2009 Nobel Laureate Robert Solow diagnoses their failures as deriving from their shared image of the economy that distort some basic facts:

> currently fashionable macroeconomics likes to formulate things in a way that inevitably endows the economy with more coherence and purpose than we have any right to assume.

By saying that, "without any right" the models "endow the economy" with properties that the economy does not have, Solow implies that macroeconomists have been careless risk takers in moving from examining their well behaving model worlds – in which there is a lot of "coherence and purpose" – to making claims about the less orderly real world.

The contested macroeconomic models rely on the image of financial markets being efficient, so no further inquiry is required to incorporate more nuanced assumptions about how the financial markets actually function. This is not the only objection. It is an instance of the more general complaint that the models leave out causally important factors and in doing so also miss important explananda. Those factors are causally important, because they are responsible for, say, the sort of crisis we have recently witnessed. Since the causes of such crises are not among the isolated factors in the models, their effects – the crises and their characteristics – cannot be explained or predicted. In the worst case, they cannot even be conceived within the framework of those models. This is, among many others, the main focus of the complaints levelled by Nobel Laureate Joseph Stiglitz and many others.

Macroeconomic models using representative agents miss the crucial causal factors that lie in things such as informational asymmetries, structure of financial markets, and corporate governance. These models therefore do not recognize phenomena such as excess indebtedness, debt restructuring, bankruptcy, and agency problems. Any model with these characteristics

> leaves out much, if not most, of what is to be explained; if that model were correct, the phenomena – the major recessions, depressions and crises that we seek to understand – would not and could not have occurred (Stiglitz 2011, 168).

These models fail to incorporate factors that are crucial for major macroeconomic fluctuations and instead focus on minor price distortions due to inflation. Macroeconomic models are better in explaining "the small and relatively unimportant fluctuations that occur 'normally', ignoring the large fluctuations that have episodically afflicted countries all over the world." Those models have failed, and are unable, to answer explanatory questions such as, Why have such fluctuations occurred? Why do disturbances get amplified? And why are recoveries so slow? (Ibid., 169.)

So the core of the contestation is one of failed isolation: the poor models have isolated factors of secondary importance and by idealizing wrongly have come to leave out many others that are crucial. The charge is not that models fail to represent or are not intended as surrogate objects but rather that "the conventional models inadequately modelled – and typically left out – many, if not most, of the key factors that played a central role in this crisis" (172). The issue is about the relevant resemblance between the models and the target phenomena, and the claim is that the issue has been unsuccessfully resolved. Given that relevance is determined by the explanatory urge to understand the behaviour of the bubbles of the current crisis, the verdict levelled by the critics is that for this purpose, models do not relevantly resemble their targets.

The reason why a model fails is that the causal factors it excludes are not negligible. This is what Stiglitz implies:

> Economists assumed that information was perfect even though they understood that it was not. Theorists hoped that a world with imperfect information was very much like a world with perfect information – at least so long as the information imperfections were not too large. (2010, 242)

The charge is here that economists dealt with the false perfect information assumption as a true negligibility assumption. But as Stiglitz reminds us, economists have no rigorous way of measuring the size of information imperfections – which makes estimating their negligibility even more difficult. This creates room for the role of sheer hope that they are negligible (yet Stiglitz himself does not hesitate to claim that information imperfection is not negligibly small).

The issue often becomes transformed into an issue of the purposes of modeling and the intended domain of their applicability.

> Is the purpose of an economic model to help us predict a little bit better how the economy is performing in 'normal' times – when things do not matter much? Or, is the purpose of an economic model to predict, prevent and manage big fluctuations and crises? (Stiglitz 2011, 168)

The criticism is often phrased by saying that the poor models deal with "special cases where market inefficiencies do not arise" (Stiglitz 2011, 166) or that they do not apply to economies that are capable of generating bubbles. In the imagined worlds of these models, agents are super-rational and fully informed, there are

markets for all goods and all risks extending infinitely far into the future and covering all risks ("one can buy insurance against every conceivable risk"). In such worlds, bubbles don't occur (Stiglitz 2010, 252). Careless epistemic risk taker economists then proceed to conclude that bubbles do not occur in real world economies, either.

Such careless risk taking reflects deficiencies in the model commentary that fails to inform modelers and model users about the structure of the modeling exercise and what it takes to successfully manage the epistemic risks in model application. Among other things, the commentary should give the obvious advice to build a pool of models from which one can choose and put in use those that are appropriate to the kind of case at hand – for example, a set of models for situations with bubble-generating mechanisms in operation and another set for other sorts of situation (cf. Colander 2010). This advice may fail to be given insofar as it cannot be easily reconciled with disciplinary conventions such as that of unification. Some observers suspect that behind such an uninformed model commentary there is an ideological bias: "Unfortunately, careful attention to the limitations of simplified models has not been the norm in the era of market liberalism." (Quiggin 2010, 109)

4.3 Substitute modeling

The type of challenge discussed in the previous section identifies possible modeling failure in the failed attempt to build models that would isolate the factors that are causally important for some major phenomena such as financial crises of the present type. While such a failure is a matter of a *failed attempt*, the one to be briefly discussed in this section is a matter of *failing to attempt*. This is the distinction between surrogate modeling and substitute modeling (Mäki 2009).

Surrogate modeling is motivated by epistemic ambitions that reach beyond learning about just the model. By directly examining the properties of the model, the modeler seeks to indirectly learn about some target. Resemblance between the model world and the real world is an issue that is prompted and perhaps settled. In surrogate modeling, the model system is intended – or found to serve – as a *bridge* to some real system (and may fail as such a bridge).

By contrast, *substitute modeling* is a degenerate activity that has no ambitions beyond dealing just with models. The modeler only examines the model and only learns about its properties, whereas the resemblance of the model world with some real world system is not prompted as a genuine issue to be resolved. Examining a model is a substitute for trying to indirectly access the real target. Criteria other than those indicating resemblance dominate the exercise. Rather than offering a bridge, the model remains an intellectual *island* unconnected to the real world.

There is a deeply rooted suspicion among many critics that much – or at any rate too much – economic modeling is of a substitute variety. According to this charge, economists too often only have an interest in examining the properties of their models and have no interest in checking how those properties relate to the properties of some important real world systems. At the time of crises, this charge regularly makes an appearance (e.g. Hodgson 2009).

This provides a framework for reading Nobel Laureate Paul Krugman's critical account of the sources of failure of economics in dealing with – anticipating and analyzing – the financial and economic crisis of 2007–2008. In a column in the *New York Times Magazine*, Krugman (2009) stated that

> [...] the economics profession went astray because economists, as a group, mistook beauty, clad in impressive-looking mathematics, for truth. [...]

This can be translated into the idea that economists have failed in dealing with the crisis because they have been busy with substitute modeling rather than surrogate modeling. Accordingly, economists have been preoccupied with the beauty and neatness of their models, expressed in impressive mathematics, while this has contributed nothing to the task of finding relevant truths about the real world.

Regarding the contents of these models, Krugman says economists have envisaged a fantasy world of perfectly rational agents in perfectly functioning markets, very far removed from the imperfections of the real world – and that this must change.

> When it comes to the all-too-human problem of recessions and depressions, economists need to abandon the neat but wrong solution of assuming that everyone is rational and markets work perfectly. The vision that emerges as the profession rethinks its foundations may not be all that clear; it certainly won't be neat; but we can hope that it will have the virtue of being at least partly right.

So the model worlds envisaged by economists – with perfect rationality and perfect markets, and therefore without the sorts of financial bubble that burst in 2008 – have been excessively neat and tractable. Such models permit relatively easy derivations of relatively unambiguous modeling results. Krugman might be taken to suggest that there is some sort of trade-off between neatness and truth, such that when trying to get their models closer to the truth ("at least partly right") economists will have to give up at least some of the neatness of their models.

We may develop this line of thought further by envisaging an extreme situation in which the virtues of neatness and tractability completely come to dominate modeling at the expense of other ("reality-oriented") virtues. Once a model world is sufficiently far from the real world, the modeler is tempted to pay all her attention to the properties of the models only and to ignore any further issues of resemblance with the real world. This would be degenerate substitute modeling.

This inclination could be generated or reinforced by an *excessive role of mathematical convenience or tractability* in modeling. Some idealizing assumptions in models are indeed made to serve mathematical tractability purposes (they are called "modelling tricks" by Krugman; cf. Mäki 1992; Hindriks 2006). In case tractability and negligibility do not coincide – in case the distortions brought about by those tractability idealizations are not negligible from the resemblance point of view – we have a possible source of failure (Mäki 2011b). John von Neumann – surely with no dislike for mathematics per se – was aware of these dangers:

> As a mathematical discipline travels far from its empirical source, or still more, if it is a second and third generation only indirectly inspired by ideas coming from "reality", it is beset with very grave dangers. It becomes more and more purely aestheticizing, more and more purely l'art pour l'art. This need not be bad if the field is surrounded by correlated subjects, which still have closer empirical connections, or if the discipline is under the influence of men with an exceptionally well-developed taste. But there is a grave danger that the subject will develop along the line of least resistance, that the stream, so far from its source, will separate into a multitude of insignificant branches, and that the discipline will become a disorganized mass of details and complexities. In other words, at a great distance from its empirical source, or after much "abstract" inbreeding, a mathematical subject is in danger of degeneration. (von Neumann 1947, 9)

Yet, we should not rush to any simplistic conclusions on this matter. The world of modeling – not just the world modelled – is complex and easily misunderstood. It is fairly safe to make the general observation that economists are happy with examining models and making claims about their properties in a rigorous manner, but are equally happy with saying nothing – or at most saying something very casual – about any real targets based on what they discover about models (cf. Sugden 2009). Yet, as such, this alone does not imply that economists are practicing substitute modeling. Let me explain why.

Talking about models as if they were the world is a natural aspect of model-based research strategy in all disciplines. Models easily become objectified or reified as the immediate targets of inquiry: their properties and behaviour are investigated and the results are reported in scientific publications. The important question is *what else* is going on in inquiry. The relevant dimensions of the possible answers to this question are the collective and the historical. There is the collective dimension: *What does the research community do as a whole?* And there is the historical dimension: *What will happen in later stages of research?*

Some portion of economic modeling could perhaps be saved from charges of substitute modeling provided one or both of the following two conditions were met: there is a well functioning division of intellectual labour such that while some economists only build and examine models, there are others doing the hard work of investigating how those models relate to the real world; and/or there is a historical sequence of bodies of research such that an earlier stage of study of model properties will in due time be followed up by the study of how those model properties relate to real world properties. Another way of putting this is to say that substitute modeling may only appear to be such, while in fact it is a phase of surrogate modeling considered in a broad enough collective and historical context.

Indeed, in their defensive commentary of apparent substitute models, economists often appeal to such collective and historical considerations. However, this is an all too easy move if nothing more specific is said about the two dimensions. The critic may grant the relevance of the collective division and historical ordering of tasks, and yet argue that economic modeling has recently failed just in this. The needs of policy are often urgent, so they cannot wait for some possible future generation of economists to do its share in bridging the gap between the models and the world. This would be a failure in the institutions of modeling – its rules and conventions, incentive structures and industrial organization.

5 Conclusion

I have outlined three sets of ideas. First, I have articulated what I think are among the dominant disciplinary conventions that guide economic modeling. Second, I have sketched a general account of modeling as ontologically and pragmatically constrained epistemic activity. Third, without trying to be exhaustive, I have provided a rough partial typology of three ways of contesting economic modeling: questioning the use of unrealistic assumptions and thereby the strategy of modeling as such; questioning the use of particular unrealistic assumptions and models conceived as surrogate objects; and questioning the allegedly degenerate practice of substitute modeling.

The boundaries between the three ways of contestation are not always clear and sharp. For example, it is not always easy to tell a surrogate model used for offering a how-possibly account from a substitute model governed by goals other than reasoned truth about the world. More generally, the difference between the two may be hard to tell, because the collective and historical dimensions of

excuse allow for flexible interpretations: there is no unambiguous and uncontroversial way of fixing the required sort of division of research labour and the permitted time lag between examining a model and checking how it relates to the real world. Yet I believe something of the sort I have suggested might serve as a beginning for drawing a map within which various ways of contesting modeling might find a place.

As I see it, modeling is a powerful and indispensable method of *managing complexity* in a discipline like economics. At the same time, it is extremely important to recognize the difficulties of *managing the risks of modeling*. As Keynes said, in the long run we are all dead. The critic of modeling might add that in the long enough run, we may all be killed by some deep economic disaster – the possibility which economists failed to conceive and the actual occurrence which they failed to anticipate just because they were too fond of their simplistic model worlds.

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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.

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Preface

In 2006, within the Academy of Sciences and Humanities in Hamburg, the working group *Models, Simulations, and the Reduction of Complexity* was founded. In this group, scientists from various disciplines – economics, engineering science, history of science, mathematics, medicine, philosophy, physics, psychology, sociology – cooperate in order to analyze methodological and epistemological problems connected with the use of models and simulations in an interdisciplinary framework. As a first public event, the members of this group, in cooperation with Stephan Hartmann (then at Tilburg University, The Netherlands), organized an international conference on *Models, Simulations, and the Reduction of Complexity* that took place at the University of Hamburg on 18–19 March, 2010. During this conference, eight selected model building and simulation projects from different disciplines from the natural, engineering, and social sciences were presented. Each presentation was commented on by a philosopher of science specializing in problems of model construction and simulation, and trained in the respective discipline. The main task of the commentators was pointing out and analyzing methodological, discipline-specific peculiarities, as well as any interdisciplinary parallels of the modeling and simulation techniques applied. The subsequent discussions focused on different strategies used for the reduction of complexity in the various disciplines, on the relation between models and underlying theories, and on the possibility for one discipline to learn from the techniques and strategies used in others. The essays and commentaries assembled in this volume are revised and extended versions of the papers and comments presented at this conference.

It is a pleasure to thank all contributors for their excellent papers and commentaries and for undertaking the task of preparing a revised version of their contribution for this volume. Furthermore, we wish to thank the Academy of Sciences and Humanities in Hamburg for generous financial and organizational support. In particular, we would like to thank its former president, Professor Heimo Reinitzer, and Dr. Elke Senne for their interest in the project and continuous support. Finally, we are grateful to Ms Victoria Pöhls for preparing the index and the final manuscript.

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Content

Ulrich Gähde and Stephan Hartmann

Introduction

Modern science is, to a large extent, a model-building activity. In the natural and engineering sciences as well as in the social sciences, models are constructed, tested and revised, they are compared with other models, applied, interpreted and sometimes rejected or replaced by a better model. Some models help scientists to systematize huge amounts of data, coming from experiments or generated through computer simulation, and to extract information out of them. Other models are developed with the aim to explain a puzzling scientific phenomenon – a task that typically requires a number of clever idealizing assumptions and, more and more, the use of computer simulations. By now it is uncontroversial that scientific models are indispensable for solving scientific problems. While some philosophers (such as Ronald Giere (1999) and Bas van Fraassen (1990)) think that science can do without laws, it seems utterly impossible for science to do without models.

The extraordinary importance of models in science has not gone unnoticed by philosophers of science. Starting in the 1960s, scholars such as Peter Achinstein (1968) and Mary Hesse (1963) focused on simple models, such as the billiard ball model of a gas, to illustrate various philosophical claims about, for example, the role of metaphors and analogies in science. Others, most notably Patrick Suppes (1969), explored the connections between scientific models and mathematical (model-theoretical) models and stressed the role of models in the analysis of data ("models of data"). Later, beginning in the 1980s and initiated by seminal contributions by Nancy Cartwright (1983), Ronald Giere (1988), Ian Hacking (1983) and Bas van Fraassen (1980), increasingly complicated scientific models, from physics as well as from the special sciences, gained center stage, and new questions, for example about the relation between theories and models, came to the fore. This debate led to a rethinking of many traditional topics in the philosophy of science, including the nature of confirmation, explanation, and the structure of scientific theories, as well as the role of approximations, idealizations and intertheoretic relations. For a detailed overview of these debates, we refer the reader to the survey article by Frigg and Hartmann (2012). Bailer Jones (2009) gives a book-length discussion of models in science, including an intriguing account of the history of the philosophy of scientific models.

In order to narrow down this tremendously broad and rich field of study, we decided to focus on the modeling of complex systems. All natural and social sciences are concerned with such systems, and it is here where one of the great advantages of model-building becomes especially vivid: Modeling helps scien-