From realism to instrumentalism - and back? Methodological implications of changes in the epistemology of economics

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11 June 2016

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Abstract

We identify epistemological shifts in economics throughout the 20th century and discuss their methodological implications. After the realist research program of the Cowles commission and Lucas' rational expectations approach, several economists became dissatisfied with economic theory and initiated a shift towards instrumentalism. Recently, this movement has come under critique and a return to a realist epistemology focusing on identifying economic mechanisms is suggested. Such epistemological changes have important practical implications: they affect the discrimination among competing explanations and determine which research methods are accepted. We illustrate this by studying epistemological and methodological changes in development economics throughout the last century. (100 words)

Keywords: mechanism-based explanations, realism, instrumentalism, agent-based computational modeling, New Keynesian economics

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1 Introduction

Developing models is an essential element of economic theory. The descriptive accuracy of their assumptions has always been subject to a lively debate. Since building models with 100 per cent descriptively accurate assumptions is not feasible, economists usually refer to the particular 'nature of economic theory' (e.g. Blume and Durlauf (2006)) to justify their use of 'unrealistic' assumptions.

Yet what is the particular feature of economic theory that makes models useful despite not being 100 per cent descriptively accurate representation of reality? This is an epistemological question and fortunately economists have developed a number of different strategies to ensure the epistemic usefulness of their models. The approaches range from instrumental[1] positions such as Friedman's justification of models as prediction instruments (Friedman, 1953), through hybrid positions such as the idea of models as analogies to reality (Gilboa, Postlewaite, Samuelson, & Schmeidler, 2014), to realist positions such as Sudgen's concept of models as "credible worlds" (Sugden, 2000) or Mäki's idea of "models as isolations and surrogate systems" (Mäki, 2009).

To what extent, one may ask, are such meta-theoretical elaborations relevant for the daily modeling challenges economists usually face? In this paper we will argue that they are indeed relevant since epistemological concepts have direct implications for economic methodology, i.e. the question of which modeling tool is adequate to the concrete question at hand. This is illustrated in figure 1: by deciding on the final modeling approach, every application builds upon an implicit or explicit comparison of potential modeling approaches. The arguments put forward in this comparison always rely upon an (implicit or explicit) epistemological orientation.

We may consider the discussion about the usefulness of agent-based models (ABM) in

[1]The term 'instrumentalism' here refers to the particular positivist epistemological position. It must not be confused with the pragmatist concept of instrumentalism. The latter is not mainly concerned with epistemology but the construction of policy instruments that aim at increasing the well-being of people (Hayden, 2006, chapter 2). Pragmatism is still of considerable importance in the school of evolutionary-institutional economics and can - on the epistemological level - be considered a particular form of realism. As it is interpreted in evolutionary-institutional economics (which is deeply rooted in Darwinian theory) it puts a major emphasize on the identification of causal mechanisms and is thus perfectly compatible with the concept of mechanism-based explanations introduced below. See e.g. Hodgson and Knudsen (2004, p. 433ff).

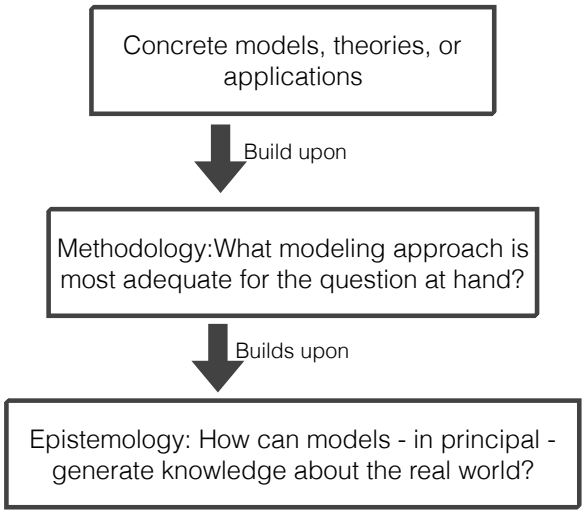

Figure 1: An illustration of the structure of economic research.

economics as an example: these models allow more realistic assumptions than general equilibrium models, but usually do not allow for a closed form solution of their equations. For economists sticking to Friedman's idea of models as prediction machines, the ability of ABM to allow for more realistic assumptions does not have any value in itself. They may therefore be much more reluctant to the application of ABM than someone who considers models to represent 'credible worlds' where the adequateness of assumptions matters. This means that if the underlying epistemology of economics is changing, then so should the methodological toolkit that is accepted for economic research.

In this paper, we will show how the epistemology of economics has indeed undergone important changes during the past decades: after a certain trend towards an instrumentalist way of modeling, in particular in the field of development economics, economists now implicitly rediscover the (realist) concept of mechanism-based explanations. Although we appreciate this trend, much confusion could be avoided if the corresponding epistemological considerations were made more explicit. One goal of the paper is therefore to highlight and describe these changes in the epistemology of economics. This will then allow us to elaborate on potential methodological implications of this change, assess the different reactions of the economics community, and to derive some implications for future research.

The rest of this paper is structured as follows: Section 2 describes two ideal epistemological strategies to use formal models in economics. In particular, we introduce the concept of mechanism-based explanations and demarcate it from instrumentalist approaches a la Friedman (1953). Section 3 investigates the implicit change in the epistemology of economics in the past decades. For the sake of brevity and intuition we will focus on the field of development economics. Section 4 discusses the methodological implications of this a shift and assesses two different reactions the economics profession has undertaken. Section 5 then summarizes the paper and illustrates the relevancy of the results for future research.

2 Instrumentalist and mechanism-based explanations

How can we give epistemic meaning to models of which the assumptions are at odds with reality? In the course of the post-WWII period, different answers have been given to this very fundamental question. One well-know answer refers to instrumentalist philosophy: according to the instrumentalist approach, the degree of realism of assumptions is a completely irrelevant factor for judging the usefulness of models. Instead, a model is useful if it is able to provide reasonably accurate predictions about the system under investigation. The particular mechanisms that operate in reality and that have caused the *status quo* are of no interest as long as the model is able to predict the *status quo* by some arbitrary procedure. Probably the most famous advocate of such an approach in economics is Milton Friedman in his famous essay from 1953. Despite criticism from various angles, this view is still very prominent in several economic research communities.

The concept that I want to contrast this instrumentalist approach with is more in the philosophical tradition of realism. In contrast to an instrumentalist explanation, *mechanism-based* explanation is not primarily concerned with generating adequate predictions (although good mechanism-based theories often can provide reasonable predictions). Rather, it prioritizes the identification of the particular economic, social or cognitive mecha-

nisms having caused the *status quo* over the provision of predictions. But mechanism-based explanations differ from a purely descriptive analysis. The latter would be concerned with one particular case and would aim for a very detailed exposition of the particular events leading from one situation to another. Instead of being focused on particular case studies, mechanism-based explanations are part of theories of medium-range: while mechanisms do not constitute universal laws, they are expected to have a certain generality (Hedström & Swedberg, 2005).

Thomas Schelling defines a mechanism as a "a plausible hypothesis, or a set of plausible hypotheses, that could be the explanation of some social phenomenon, the explanation being in terms of interactions among individuals or between individuals and some aggregate" (Schelling, 2005, p. 33). As mechanisms are to be considered "sequences of states within a given system" (Bunge, 2004), the 'explanation' mentioned by Schelling means the identification of such a sequence. Of course, identifying such a sequence is not straightforward and involves a couple of methodological difficulties. This illustrates already the close relation between epistemology and methodology.

The distinction between instrumental and realist approaches is not always straightforward (in particular, because considering the correct mechanisms may often increase the ability of a model to predict the future). But the following example from network economics makes the fundamental distinction clear: There are is now an increasing number of empirical results on how economic networks look like. For example, many social networks are highly clustered, consist of different communities, have small diameters and heavy tailed degree distributions. It would be nice to recreate networks with these properties computationally. In such a case one needs do develop stochastic algorithms that grow a network. Such an algorithm may be built by resembling particular social mechanisms that could also be operating in reality and be one reason that social networks have these properties. Such an algorithm may also use artificial mechanisms of which we know that they do not operate in reality, but that nevertheless create the networks with the desired properties.

For example, one of the most famous models in network theory is Barabasi and Alberts'

method to generate scale free networks, i.e. networks that have a particular heavy tailed degree distribution (Barabasi & Albert, 1999). Roughly speaking, in this model new vertices are added step by step and get randomly connected to existing vertices. The probability that a new vertex gets connected to an existing one is a increasing function of the degree of the existing vertex. This means that vertices that already have a large number of connections, are likely to acquire even more connections. This mechanisms comes in many facets and names, e.g. preferential attachment or the Mathew effect. Despite being abstract, it usually comes with a natural interpretation for real economic systems.

Another important algorithm developed by Clauset, Moore, and Newman (2008) is used in the context of stochastic block models and seeks to generate networks that resemble the community structure that can often be observed in real world networks. In contrast to the Barabasi-Albert model, this algorithm has several degrees of freedoms that can be estimated. The algorithm then proceeds by randomly generating communities (i.e. groups of vertices whose connections follow a similar statistical pattern) and then connecting them such that in the end the network quite accurately resembles several large-scale statistics obtained from real world networks. But in contrast to the preferential attachment model above, there is no natural interpretation in the sense that the algorithm tells us anything about the mechanisms that have generated the network in reality.

Unfortunately, only a fraction of relevant network properties (such as the heavy-tailed degree distributions) can be recreated using these kind of 'mechanism-based' algorithms. Many other properties of interest must be generated via algorithms that do not resemble real world mechanisms.[2]

In network theory, as in any branch of economics, such instrumental algorithms are useful. Yet they represent a different epistemological position than the mechanism based-explanations.

In particular, these two ideal approaches to economic theorizing provide different answers

[2]From an instrumental viewpoint the fact that does not represent any drawback. For realists, however, this situation motivates further research on mechanism-based algorithms.

to two important and practically highly relevant questions of economic theorizing: firstly, how can we discriminate between two competing explanations that are both consistent with the data? Secondly, how and what can we infer from models to reality?

2.1 Two ways to discriminate among competing explanations

Suppose there are two models or procedures that perform equally well in predicting the dynamics of the system under investigation It is a common strategy to use *Occam's razor* for the discrimination among them: if two procedures provide equally good predictions, the simpler procedure should to be preferred. Because of its reference to prediction, Occam's razor is particularly useful if one relies on an instrumentalist epistemology. It is less suitable for discriminating among mechanism-based explanations. There are at least two reasons for this: firstly, several mechanisms can have the same implications. This phenomenon is referred to *equifinality* and is common in both natural and social systems. Secondly, determining the complexity (or the simplicity) of a mechanism is difficult and often impossible given the fact that most measures for complexity that come to mind are uncomputable in such contexts (see already Bunge (1962)).

A viable alternative would be to use the 'deepness' of an explanation as a discrimination device: one explanation can be said to be deeper than another if it uses more detailed and plausible mechanisms to explain the observed fact. The following example makes this distinction clear:

"Why does a rise in *real* per-capita income lead to increased per capita expenditure on consumption?" This is a central question if one wishes to understand the sustainability of positive growth rates: if people would not increase their consumption, positive growth rates are unlikely to exist in the long run. Let us consider two possible theoretical explanations for this observed pattern: The first one is a classical approach that assumes agents maximizing their utility for a given preference relation which, among others, must obey the axiom of local non-satiation. This assumption is necessary for the results of classical demand theory

to hold (Mas-Colell, Whinston, & Green, 1995). Starting from these assumptions there is a number of studies that derive the empirically observed consumption patterns as an equilibrium solution. In particular, a changing proportion of goods consumed if the real income rises can be introduced by considering substitution effects due to the inferiority and superiority of goods, the latter being part of the preference relation assumed in the beginning. A considerable number of results could be obtained using this kind of reasoning (e.g. Dixit and Stiglitz (1977), Wadman (2000)).

However, as Witt (2001) points out, these studies do not explain where the assumed preference relations come from. In the strict sense, the majority of the explanatory content of these studies comes from the assumption of local non-satiation (Witt, 2001, p. 24). But where does this assumption come from, despite being a 'standard assumption of microeconomic theory" (Mas-Colell et al., 1995)? As argued by Witt (2001, 2010), consumption always serves the satisfaction of certain needs and wants. These needs and wants can be either physiologically determined and thus homeostatic (e.g. for the need to collect food and shelter), or they can be non-physiological and non-homeostatic (e.g. for the want for group conformity or status seeking). Witt goes on to propose an explanation on the grounds of his *continuity hypotheses* according to which the origins of preferences are to be explained by evolutionary theory rather than being assumed. He refers to the limited cognitive resources of human beings and the need for social cognitive learning which is then the vantage point for the social construction of symbolic commodity characteristics. Because of these symbolic properties, which are the results of a social coordination process, some products become capable of serving the individual need for social status. As a consequence, the demand for these (often rather expensive) goods rises. [3]

Of course, the first type of model is much simpler than the second one, and both reach the same result. But the model by Witt (2001, 2010) considers the mechanisms underlying the formation of the preference structure. In the classical case they are simply assumed. Witt's model therefore ultimately leads to a 'deeper' explanation of the changing consumption in times of economic growth.

[3] This is a very reduced form of the overall argument. Refer to Witt (2001) and Witt (2010) for a fuller description and a formal model.

Using Occam's razor as an alternative discrimination device would be less clear: while the first type of model is 'simpler' and both approaches come to the same conclusions, one may be tempted to use Occam's razor to support the first one. Defenders of Occam's razor could then argue that the second theory explains the observed facts better than the first one, but then they would lack a concept giving substance to this judgment (i.e. to make clear what 'better' does mean in this context). As the concept of deep explanations is not useful in the context of an instrumentalist epistemology, Occam's razor is rather useless in the context of a realist epistemology. This again represents an important practical implication of the choice among different epistemologies.

2.2 Two ways of making models meaningful

To illustrate how instrumentalist and mechanism-based explanations differ with respect to the way they give epistemic meaning to economic models, I will introduce a meta-framework for thinking in terms of models (based upon Miller and Page (2007) and Mäki (2009)). This framework later helps to highlight the different methodological implications of different epistemological orientations.

According to Mäki (2009) models have two different important aspects: the representative aspect and the resemblance aspect.

For a model to be a representation of reality means to serve as a *surrogate* for the real world (Mäki, 2009). This means that the model is built as an image of the real world. Successfully building a model that represents the real world is a first step to *understand* it. One may already fail at this stage because it is also possible to study a model for its own sake - for the beauty of its math or because one likes the story associated with the model. One may also fail in building a model as a surrogate because one misses some of the *essential properties* of the system one wishes to investigate.[4] In these cases, the model

[4] A discussion of the concept of essential properties would lead us too far into the area of economic ontology, an area even more abstract and fundamental than epistemology. See Hodgson (2015) for a nice introduction to essentialism.

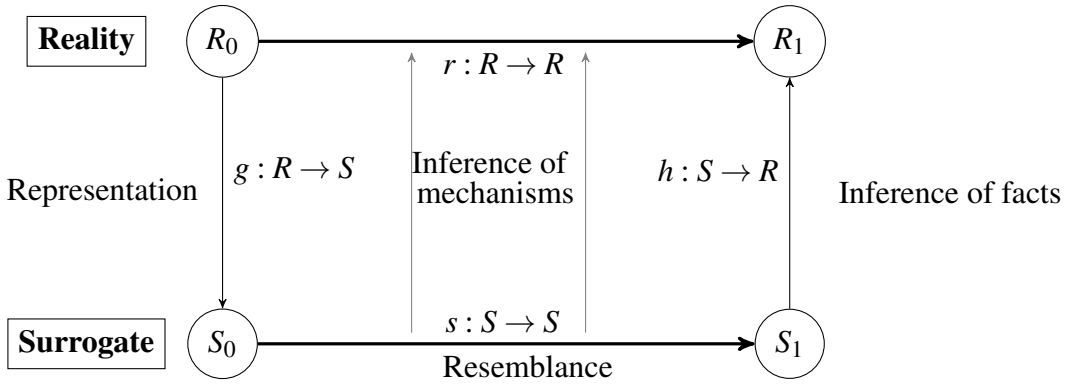

Figure 2: A development of Mäki's MISS concept, where we interpret modeling as a mapping process (following Miller and Page (2007)) and distinguish between the inference of mechanisms and facts.

is not to be considered a surrogate, but rather a *substitute* that is studied *instead* of the real world, not as a *measure to understand it* (Mäki, 2009).

At this point it is intuitive to interpret our meta-framework as the mapping process in figure 2: The process of building a model as a representation of reality means to reduce the complexity of the real world. The process of reducing the real world R to the model can be described by a function $g : R \to S$. For the state real world at time t_0 (denoted R_0 in figure 2) it gives a corresponding model in state S_0. The surrogate (or the model) is always less complex than the real world. It therefore seems reasonable to call g the *complexity reduction function* of the model.

The second aspect of a model is the resemblance aspect. It is related to the process of model exploration. Model exploration is the activity of studying the behavior of a model. To speak with figure 2, it means to study the transition function of the model, denoted by s. This function transforms the state of the model at time t_0, denoted by S_0, to the state at t_1, denoted by S_1. The general idea is to learn something by the model exploration about the behavior of the real world, i.e. the transition function r that is, in its completeness, too complicated to be understood by the scientist directly.

Figure 2 nicely illustrates the different ways models get epistemic meaningfulness de-

pending on whether one refers to instrumentalist or mechanism-based explanations: a model that tries to provide mechanism-based explanation is built in a way such that the transition function in the model (s) isolates a particular mechanism operating in the real world (r). This similarity can be assessed by comparing the intermediate outcomes of the model with that of reality, by seeking the competence of domain experts, or by referring to the results of other disciplines, e.g. psychology. In contrast, a model that is mainly concerned with providing useful predictions does not necessarily resemble the transition function function of reality in any sense. In the extreme case, it employs some very abstract machine learning algorithms that manipulate the state of the model such that it matches the state of the real world in the future as closely as possible.

In the post WWII period, both approaches were practiced within the economics profession. In the next section we will explain that there have been a number of implicit changes between the two ideal types and that these epistemological changes are usually accompanied with changes in the methodologies employed by economists.

3 Mechanism-based and instrumentalist explanation in development economics

The shift between instrumental and mechanism-based explanations and their relation to economic methodology can be nicely illustrated by locking at the history of thought on economic development in the post-WWII period.

3.1 From realism to instrumentalism: development economics in the 20th century

An important starting point for economic theory in the post-WWII period was Haavelmo's seminal contribution on the "Probability Approach in Econometrics" (Haavelmo, 1944), a paper that can be considered the vantage point for the influential research program of the Cowles Commission. According to the exponents of the Cowles Commission, there are

causal mechanisms operating in the real world, and quantitative, econometric techniques should be used to identify these mechanisms. To do so, it is important to make several *a priori* restrictions before analyzing the data. These restrictions (e.g. about the functional forms and the closure of the model) must come from economic theory (Moneta, 2005).[5] This means that the primary interest of the Cowles Commission program was to identify (and quantify) social mechanisms.

This approach later was later criticized for the underlying theory, most famously through the Lucas Critique (1976) according to which the macroeconomic models used by the Cowles Commission neglect the rationality and adaptability of economic agents at the micro level. Lucas and others argued for the use of quantitative models built upon the rational expectations hypothesis. This would guarantee that agents respond to policies in an optimal way. But as nicely shown by Moneta (2005), there was no dissence between the adherents of the Cowles Commission and adherents of the rational expectations research program that economic (causal) mechanisms should be the final subject of economic research. It might therefore be fair to say that Lucas and his colleagues were demanding a particular form of theoretical underpinnings for empirical work: the theory should be microfounded and based on the rational expectations hypothesis. Or, put differently, their concern was more of a methodological, rather than epistemological kind.

Later, two important streams of research marked a departure from this strategy. The first was mainly concerned with the macroeconomic level, the second with the microeconomic level. Firstly, the macroeconomic research program launched by the seminal work of Sims (1980) was motivated by the observation that restrictions based on economic theory - including the rational expectations hypothesis - often seem to be arbitrary and not guided by empirical data. Adherents to this program therefore tried to do their research with at least *a priori* theory as possible. Consequently, their reference to concrete mechanisms was marginal. On the methodological side, they preferred estimating unrestricted reduced form

[5]Theory here plays the role of an instrument to identify empirical facts. On this function of theory in 'modern' economics see e.g. **graca16**

equations and using nonparametric approaches over the analysis of structural equations and parametric estimation techniques. The underlying epistemological shift was a movement away from mechanism-based towards instrumental, and prediction oriented explanations (see Moneta (2005) for a similar claim). Or, speaking with figure 2, the ultimate goal of research was not to infer the mechanisms operating the real world (i.e. to study the relation between functions s and r), but 'merely' to infer facts of reality (i.e. working out the relationship $s \circ h$).

Secondly, on the microeconomic level, there was an similar skepticism against conventional economic theory. The methodological movement towards randomized controlled experiments (RCT) was motivated by this skepticism and a change in the underlying epistemology: According to Deaton (2010), economists stopped believing that economic theory could provide a reasonable base for program evaluation. Rather than from economic theory, the explanatory power should come from elaborated randomization procedures that then allow the application of standard econometric techniques to identify the effect of the policy program.[6] According to Pawson and Tilley (1997), who assessed the success of RCTs outside economics, the RCT-way of program evaluation focuses on *whether* a program has worked or not - without explicitly tackling the question of *why* it worked or not. The latter would require a theory of the underlying mechanisms - and this recourse to theory is exactly what many of the proponents of RCT wanted to avoid (Deaton, 2010).

In both cases, the underlying shift in the epistemological orientation of the researchers went hand in hand with important methodological changes: a shift to reduced form equations in macroeconometrics, and a shift towards RCT in microecometrics. When this shift took place, the change in the underlying epistemology received less attention than the methodological innovations.

[6]Rodrik, Subramanian, and Trebbi (2004) explain nicely why randomization and instrumentalization does not represent any theory on its own.

3.2 ...back to realism?

Today, this *epistemological* shift away from mechanism-based explanations is beginning to be criticized:

For example, both Deaton (2010) and Grüne-Yanoff (2015) stress that without a direct consideration of economic mechanisms, results from empirical studies can hardly be generalized. Deaton shows that if a RCT is not designed in a way that it identifies the particular mechanisms that were the causes for the success or failure of the project it is not clear whether the project can be expected to be successful in another instance. This relates to a second argument for mechanism-based explanations: successful intervention and always requires knowledge of the underlying mechanisms (Schelling, 2005; Deaton, 2010; Rodrik et al., 2004; Hallak & Levinsohn, n.d.; Grüne-Yanoff, 2015): Deaton (2010) uses the example of a poverty reduction program using conditional cash transfers that has been found successful in Mexico. Without knowing why and how the program has worked, however, it is impossible to say whether it would also be successful in, say, Kenia: the different cultural and institutional framework in Kenia may affect the mechanisms that have led to the success in Mexico in such a way that the project looses its positive effect. To make a reasonable prediction one must study the interaction effects among different mechanisms and the economic environment - and this requires mechanisms to be considered explicitly in both theory and practice (Rodrik et al., 2004; Deaton, 2010). [7] Rodrik et al. (2004) focus more on the macroeconomic aspects of economic development. They stress that policy makers regularly operate in second-best environments. Therefore they must adjust their measures to the institutional setting they are operating in - and that this adjustment process requires knowledge of the (macro- and microeconomic) mechanisms at stake.

A similar argument is made Hallak and Levinsohn (n.d.) who refer to the literature investigating the relationship between economic growth and trade. This literature did not pay much attention to the mechanisms underlying this relationship but - similar to the microeconomic RCT literature mentioned above - focused on determining the correlation

[7] Further examples, in particular for policies focusing on nudges, can be found in Grüne-Yanoff (2015)

between various trade policies and economic growth. This has led to the unsatisfying state of affairs that it is not clear how and through which channels international trade affects economic development. Therefore they call for an approach that "focuses on and models the specific mechanisms through which trade or trade policy operate instead of looking directly at the macroeconomic outcomes"(Hallak & Levinsohn, n.d., p. 24). Only then successful policies are likely to be developed.

An ultimate argument in favor of mechanism-based explanations is that only a reference to concrete mechanisms lets us judge the ultimate plausibility of an explanation: Hedström and Swedberg (2005) exemplify this with the empirical result that exposure to electromagnetic fields increases the frequency of childhood leukemia. Without the provision of a plausible (in this case bio-physical) mechanism explaining this relationship it remains, however, impossible to say whether it is a causal relationship or an omitted variable that has caused this result.

All these contributions criticized the shift away from mechanisms mainly from an epistemological perspective. It therefore remains the question whether, and if yes, which, changes on the methodological level should accompany a reorientation towards mechanism-based explanations.

4 Methodological implications of mechanism-based explanations

Research methods and epistemologies must fit to each other. Not all methods are compatible with every epistemological approach (Gräbner, 2015). If one is interested in identifying particular social mechanisms one should not rely too much on machine learning algorithms which may do an excellent job for providing good predictions. On the other hand, models that seem to capture important mechanisms of the real world but potentially showing chaotic behavior are not well suited to provide (point) predictions. It is thus unlikely that research methods developed in an instrumentalist environment can be successful in identifying social mechanisms. What does that imply for development economists if we follow the arguments of section 3.2 and assume that a shift back to more mechanism-based explanations is adequate? The meta-theoretical framework from section 2.2 suggests at

least two criteria for successful modeling techniques when it comes to the identification of social mechanisms:

The first property of an adequate modelling technique concerns its ability to formalize flexible assumptions. Otherwise, it is not possible to identify more complex (and interesting) social mechanisms. The reason for this can be derived directly from figure 2: Successfully isolating a mechanism in a model means to establish a similarity between the transition function in the model (s) and the transition function of reality (r). Establishing such a similarity requires a certain degree of accuracy in the representation process. For example, for a modelling approach that is built upon a representative agent with rational expectations, several mechanisms are *a priori* excluded from consideration: all mechanisms involving true uncertainty (e.g. heuristic formation) of those based on the structured interaction of heterogeneous agents cannot be implemented directly in such models. One may be able to replicate the consequences of these mechanisms. But this is no mechanism-based explanation in the strict sense.

A second feature that is helpful for generating mechanism-based explanations is that of modularity. This means that it should be possible to extent the complexity of a model gradually to include additional mechanisms without changing the overall functioning of the model. Neoclassical economists usually try to follow this demand by relying on a small number of core models that they extend to match other more specific situations.[8] This makes it easier to understand where the reasons for different modelling outcomes come from: one implements a model with and without the mechanism under consideration and compares the outcome of the two models. If the rest of the model remains the same (which is the definition of modularity), one has successfully isolated the effect of this additional mechanism. This is particularly important if one is interested in what we termed deep explanations above: starting with a highly complex model from scratch usually only causes confusion about the functioning of the model. Instead, one should start with a simple

[8]This is the so called TAPAS approach: *take a previous model and add something.*

model as possible and add more mechanisms sequentially.

How did economists react to these demands? Two main strategies can be distinguished and both are currently pursued in the economics community:

The first, more conservative, methodological reaction could be a renewed focus on models with rational expectations and a close relation between empirical research and economic theory. On the macroeconomic level, this is the strategy associated with the New Keynesian paradigm and their DSGE models. On the microeconomic level, Modern Behavioral Economics that tries to identify deviations from rational behavior and alter the utility functions of agents to capture these deviations come to mind.[9] Such a strategy has both advantages and disadvantages: on the one hand it bears the danger of not adequately considering the criticism that ultimately led to the departure towards more instrumentalist methods: Sims and other economists tried to minimize the impact of economic theory because they were not satisfied with it (Deaton, 2010). Adherents of the New Keynesian approach and modern behavioral economists need to take this criticism seriously. On the other hand, a new focus on rational expectation modeling would help to preserve previously obtained knowledge and to relate the new work with existing results. Also, economists are usually well trained in rational expectation modelling so a reorientation would not require much additional training.

The second, more innovative but risky methodological reaction would be the attempt to broaden the theoretical and empirical toolbox of economics and to consider the use of new research methods that meet the demands for mechanism-based explanations, but also consider the criticism of earlier approaches. Classical behavioral conomics and complexity economics are two closely related research communities that are currently pursuing this strategy. Both approaches have departed significantly from mainstream economic theory. In particular, they try to avoid using utility maximizing rational agents

[9]Modern Behavioral Economics can be distinguished from the Classical Behavioral Economics, which tries to develop an alternative to the optimization approach based on the study of how humans actually solve daily decision problems (Kao & Velupillai, 2015). This alternative paradigm has its historical roots in Herbert Simon's distinction between procedural and substantive rationality (Simon, 1981).

as their fundamental entity and seek to use closure strategies that are more general than the classical equilibrium. Again, such an approach has advantages and disadvantages: on the one hand it bears the danger of inter-paradigmatic losses, makes it difficult to relate to existing models, and requires economists to learn methods that are not yet part of most graduate programs. On the other hand, such an approach would allow the consideration of very different assumptions and would facilitate the direct investigation of the economic implications of many social and cognitive mechanisms that cannot be captured in the conventional *maximization-cum-equilibrium* approach.

In the following we will discuss the two movements in more detail.

4.1 The New Keynesian way: a close integration of theory and empirics

DSGE models were developed as a response to the Lucas critique that criticized Keynesian macroeconometric models for ignoring individual rationality and to have proposed policy measures that have led to the period of stagflation in the 60s and 70s: macroeconometric models suggested a negative relationship between unemployment and interest rate (the original Phillips Curve). Therefore governments were thought of facing a trade off between unemployment and inflation. But it turned out that these models confounded correlation with causation: if people are smart and adaptive, they anticipate the effects of an expansive monetary policy of the state and adjust their consumption and investment decisions accordingly. In such a case, higher inflation does not yield a stimulus for the economy and the employment rate does not change. The response of Lucas and other economists was the concept of rational expectations. The resulting real business cycle (RBC) models - the second big syntheses in macroeconomics in the 20th century (Trautwein, 2014) - built upon rational agents optimizing over time, thus forming expectations consistent with the model. Today's state of the art are New Keynesian DSGE models that address the shortcomings of the RBC literature by including market frictions and financial markets. They are well-grounded in neoclassical microeconomics and effectively bring together theoretical and empirical economists: the literature on how to calibrate and estimate DSGE models is huge. But while new estimation techniques are developed frequently and

by different statistical paradigms (in particular both frequentist and Bayesian approaches), unfortunately, there are still some severe problems with the way DSGE models are usually confronted with empirical data (see e.g. Fagiolo and Roventini (2012)). This is certainly a key challenge for further development of the models.

Also, while proponents of the New Keynesian paradigm refer to the effort undertaken to add new mechanisms to DSGE models (e.g. by introducing a moderate level of agent heterogeneity (Massaro, 2013)), critiques argue that the technical design of DSGE models poses strict limits to the mechanisms considered in these models (e.g. Leijonhufvud (2014)).[10]

In the end, the New Keynesian approach in macroeconomics seems to be what Kuhn (1962) considered the repair of an established research paradigm: New Keynesian economists consider themselves as progressives and try to expand their theory with a small, but accepted set of methods. But at the same time they are also conservative in the sense that they build their theory upon the well-known *optimization-cum-equilibrium* approach. While they constantly work on relaxing particular assumptions and on integrating new mechanisms into their models, other mechanisms will ever remain outside their reach. Interestingly, they justify this by a peculiar mixture between instrumentalist and realist epistemology: adding mechanisms to DSGE models is usually considered positive (which is a realist argument), but the fact that some mechanisms are just outside the reach of their modelling frameworks is not considered a bad thing, as long as the models serve their purpose (which is an instrumentalist argument).

4.2 The Complexity approach: rethinking economics from the bottom up

In contrast to the DSGE research community (that consists mainly of genuine economists), the complexity approach to economics is an interdisciplinary movement (Arthur, 2010). Many of the researchers have a background in the natural or computer sciences. This indicates why the willingness to give up established economic concepts of complexity scholars

[10] A summary of the most common criticisms of DSGE models are given in Colander, Howitt, Kirman, Leijonhufvud, and Mehrling (2008) or Fagiolo and Roventini (2012).

is much higher than in the New Keynesian community. What holds the heterogeneous complexity community together is the believe that the economy is a complex adaptive system. And while the methodological toolkit of complexity economists is very diverse, a significant number of researchers use agent-based computational models (ABM) to study the economy.

The idea of ABM is to express the fundamental economic entities (in particular economic agents) as software objects, to specify their relations among each other and with their artificial environment, and then to conduct numerical experiments to derive the systemic implications of this initial configuration.

In contrast to the New Keynesian paradigm, the historical roots of ABM are less well known: An early antecedent of ABM is system dynamics modeling initiated by Forrester (1967) that, while being concerned only with aggregated entities, introduced the concept of simulation into the social sciences. At the same time, Orcutt (1957) and others developed the first 'microsimulations' to assess the effects of tax changes on individual households. These models, however, did not consider the interactions among the micro entities and the system as a whole. The key feature of direct interaction was introduced into computational modelling by John von Neumann and others working on cellular automata. The most famous application in the social sciences was the segregation model of Schelling (1971) that illustrated how the *interaction* among different individuals causes interesting dynamics on the aggregated level. As the agents became more and more complex in the sense that their portfolio of behavior increased, this literature developed into what today is commonly referred to as ABM. A key figure in this development was Herbert Simon and his distinction between procedural and substantive rationality (Simon, 1981). This distinction provided the vantage point for more elaborated artificial agents and the motivation to develop constructive alternatives to the computationally infeasible concept of 'Olympian rationality' (Simon, 1981) that is underlying the rational expectations literature. One important milestone was the computational tournament of Axelrod and Hamilton (1981), in which they studied the success of different strategies in the iterated prisoners' dilemma

and presented the now famous strategy 'tit-for-tat'. This was the starting point for a close collaboration between game theorists and agent-based modelers, partly culminating in the subfield of 'algorithmic game theory' (Nisan, Roughgarden, Tardos, & Vazirani, 2007). Another milestone in this transformation into modern ABM is certainly the 'Sugarscape' model developed in Epstein and Axtell (1996) where a large number of heterogeneous and boundedly rational agents directly interact within a topological space and produced so called emergent properties on the system level.

Because ABMs are solved numerically they do not necessarily rely on the concept of economic equilibrium. Indeed, one of the major motivations for using ABM is the wish to study economic dynamics outside equilibrium. Other important arguments in favor of ABM concern their ability to study the mutual dependencies of different ontological levels (Gräbner & Kapeller, 2015) or to directly represent the decision making of boundedly rational agents (Tesfatsion, 2006). This flexibility represents a huge potential for studying the economy from a realist perspective, and to develop mechanism-based explanations for economic phenomena.

On the other hand, this flexibility is also considered a major weakness of the approach. To use the example of human decision making: since there is only one way to be rational, but infinitely many ways to be boundedly rational (Gigerenzer, 2016), one needs very good reasons for the specification of an ABM.[11] On the other hand, this unambiguity of rationality might itself be an illusion. As Gigerenzer (2016) makes clear, the statement that there is only one way to be rational holds only in situations of risk - as soon as there is uncertainty, there is no 'best' way of making a decision, yet alone to optimize one's utility, any more, and the consideration of different heuristics becomes inevitable.

In any case, model verification and validation must play a key role in the ABM approach. As for DSGE modelling, the literature on empirical estimation and calibration is growing tremendously (Fagiolo, Moneta, & Windrum, 2007; Brenner & Werker, 2007; Guerini &

[11] While some consider the optimization-cum-equilibrium approach in orthodox economics to be a 'straight-jacket' (Farmer, 2013, p. 383), others may consider it a useful 'disciplining device'.

Moneta, 2016). In contrast to DSGE models, ABM can be calibrated not only on the macro level, but also on the micro level, in particular with respect to the agent behaviour and other intermediate results.[12] Again, this both represents a strength that, if not sufficient information is available, can turn into a weakness if the model specification becomes arbitrary.

Summarizing, complexity economics and ABM represent a more radical way towards a stronger focus on mechanism-based explanations than the New Keynesian way of modeling. On the other hand, this approach also comes with new difficulties and critical challenges and it is still much less accepted by orthodox economists. [13]

5 Summary and outlook

We have argued that during the post WW-II period the epistemological orientation in economics has undergone significant changes. Since direct epistemological debates in economics are rare, these changes from realism to instrumentalism and partly back remained largely implicit. We have argued that this is not helpful for the progress of economics since epistemological orientations have important practical implications: different epistemologies provide different means to discriminate among competing explanations and favor different methodologies.

We have highlighted this relationship in the context of development economics. In particular, we discussed two different methodological reactions towards a renewed focus on mechanism-based explanations: the New Keynesian way of modeling and the complexity approach to economics.

Both approaches represent a movement towards a more realist assessment of the economy, but they differ in the extent of this movement. One important implication of this is their different methodology. As in this case, we believe that many methodological debates in economics are at the core about epistemology: researchers prefer different methods because

[12] Or, speaking again with figure 2: ABM allows for much more flexible complexity reduction functions.

[13] This has important practical implications: it is much more difficult to publish ABM in top economic journals compared with comparable general equilibrium models. Funding for research through economics associations is also more difficult to obtain and the skills that are needed to do ABM are usually not part of the standard economics curriculum.

they differ in they way they compare the quality of theories and in their strategy to make their models meaningful.

Therefore, a more explicit epistemological debate could advance many of the deadlocked dialogues among competing research communities. Such a dialogue could also help identifying differences in ideology that may also explain the difficult relationship among various research communities. Such hidden ideologies can be identified through an epistemological debate and their influence could - hopefully - be reduced.

Beyond this clarification the paper highlighted a number of meta-theoretical concepts that are useful for economic theorizing: we explained why an excessive focus on Occam's razor as a mean to discriminate between competing explanations can be misleading if realist explanations are sought for. Economists that have a realist orientation, and we believe there are many of them, should rather strive for *deep explanations*. To this end, a more open and critical assessment of the methodological toolkit available to economists is desirable. Contrary claims that, for example, proclaim the end of methodological debate in macroeconomics and see the New Keynesian way of modeling as only viable way of doing macroeconomics (e.g. Goodfriend (2007)), are not helpful for the progress of economics. This relates to another, more abstract, implication of taking epistemological debates seriously: tolerance for different research approaches in economics is very important. Since disputes abut epistemological orientations usually cannot be finally resolved (at least within the lifetime of the researchers involved), and the historical perspective taken above revealed that there is often a back and forth between different dominant epistemologies, approaches that differ in their epistemological orientation should be tolerated within the profession (see Dobusch and Kapeller (2012, 4) for a similar claim). In such a pluralist environment, the study of the history of economic thought play an essential role in identifying the different epistemologies and to relate different research approaches to each other (see already Elsner (1986)). Claims to the contrary, such the statement of Goodfriend (2007), not only increase the danger of theoretical and methodological lock-ins - they also make the economics profession less innovative, and in the end less successful in explaining reality - regardless of how 'explanation' is to be understood.

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