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

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Abstract

The paper highlights the importance of epistemological reasoning in economics by stressing the practical implications of different epistemological starting points. Most importantly, these epistemological starting points determine how one chooses among competing explanations and determine accepted research methods.

The argument gets illustrated by identifying recent epistemological shifts in the development economics literature and by highlighting their methodological implications: After the realist research program of the Cowles Commission, a shift towards less theory-dependent instrumentalism took place. Recently, this movement has come under critique and a return to a realist program focusing on the identification of economic mechanisms is discussed. We present and discuss two potential methodological answers to such an epistemological reorientation that can be found in the literature: the New Keynesian program with its focus

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on DSGE models, and the complexity approach with its agent-based models.

(136 words)

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1 Introduction

Whether the assumptions of economic models should be descriptively accurate is one of the most significant debates in economics. Since assumptions are always to some degree descriptivesly inaccurate, economists usually refer to the particular 'nature of economic theory' (e.g. Blume and Durlauf (2006)) to justify their use of 'unrealistic' assumptions.

What is this particular feature of economic theory? How could we understand a real system by using a model which is not a one-to-one depiction of this system, but is only an imperfect representation of it?

This is an epistemological question[1] and over time, economists have developed very different strategies to answer it.

These strategies range from instrumental[2] approaches such as Friedman's justification of models as prediction instruments (Friedman, 1953)[3], through hybrid approaches 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)[4] or Mäki's idea of "models as isolations and surrogate systems" (Mäki, 2009a). All these approaches represent different ways of using models to generate new knowledge.

[1]Epistemology is the area of philosophy that deals with questions on the necessary and sufficient conditions for the creation of 'knowledge' (Steup, 2014).

[2]The term 'instrumentalism' (or 'instrumentalist approach') 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).

[3]There has been a debate on whether Friedman's essay could also be interpreted in a realist way (Mäki, 2009b), in particular if one shares Mäki's distinction between *realism* and *realisticness*. But it seems that many economic philosophers and Friedman himself agree that the fundamental orientation of the 1953 article is instrumental (Boland, 1979, 2010).

[4]Sugden (2000, p. 12) explicitly says that his concept represents a realist position, and not an instrumentalist one. Mäki (2009a) confirms this by pointing out to the similarities between Sudgens *credible worlds* (which are characterized by some form of similarity to the real world) and his own realist concept of MISS.

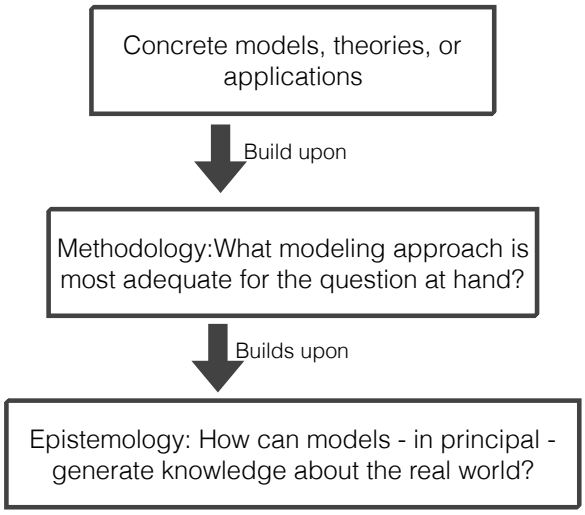

Figure 1: An illustration of the structure of economic research that highlights the hierarchical dependency among the different levels of theory: for example, *every* methodological consideration must *always* refer to epistemological arguments. While the existence of the relationship is a logical necessity, only a historical investigation of this relationship is able to elucidate its particular nature.

To what extent, one may ask, are such epistemological (i.e. meta-theoretical) elaborations relevant for the daily modeling challenges economists usually face? We will argue that they are very 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 are epistemological arguments since they refer to the adequateness of the different methods to *generate knowledge*. In other words: methods must fit the epistemological approach, and different methods fit to different epistemologies. The following example from applied macroeconomics illustrates the practical relevance of such meta-theoretical considerations:

There is an increasing debate on whether macroeconomic models should be made *agent-based*. Agent-based models (ABM) allow more realistic assumptions than general equilibrium models, but they 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 prevalent epistemology of economics is changing, then so should the methodological toolkit that is accepted for economic research.

We will see later how the dominant epistemology of economics has indeed undergone important changes in the recent past: after a certain trend towards an instrumentalist way of modeling until the 1990s, in particular in the field of development economics, economists now implicitly rediscover the (realist) concept of mechanism-based explanations. Although we welcome this trend, much confusion could be avoided if economists appreciated more the value of epistemology and made their epistemological considerations more explicit. The first goal of the paper is therefore to highlight and describe these changes of the prevalent epistemology of economics.This exercise will then allow us to address its second goal: to elaborate on the methodological implications of this recent change towards mechanism-based explanations and to assess the different methodological reactions of the economics community.

The rest of this paper is structured as follows: Section 2 summarizes two ideal epistemological strategies to use formal models in economics and paves the way for the application that follows. Section 3 investigates the recent implicit change in the epistemology of development economics.Section 4 discusses the methodological implications of this shift and assesses two different reactions the economics profession has undertaken recently. Section 5 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 fundamental question. For the sake of the argument we simplify the matter by focusing on

two epistemological ideal types:[5] According to what we call the instrumentalist approach, for example, the realism of assumptions *per se* is an 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. The most famous claim for such an approach in economics is Friedman (1953) and this view is still prominent in several economic research communities.

The concept that we want to contrast this instrumentalist approach with is more in the philosophical tradition of realism. *Mechanism-based* explanations are not primarily concerned with generating adequate predictions (although good mechanism-based theories often can provide reasonable predictions). Rather, they prioritize the identification of the particular economic, social or cognitive mechanisms having caused the *status quo* over the provision of predictions. But mechanism-based explanations differ from purely descriptive analysis. Such analysis is concerned with one particular case and aims 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: mechanisms are no universal laws, but 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

[5]See Reiss (2012) for a more extensive discussion of potential demarcation lines between realism and instrumentalism.

already the close relation between epistemology and methodology.[6]

The distinction between instrumental and realist approaches is not always straightforward and many distinctions are fuzzy(Reiss, 2012). For example, the fact that considering adequate mechanisms may often increase the ability of a model to predict the future. The following example should make the distinction clear: Let us consider the way one could investigate the structure of social interactions or networks within an economic model. From empirical investigations we know that social networks are usually highly clustered, consist of different communities, have small diameters and heavy tailed degree distributions. It would be useful to recreate such networks computationally and to consider them explicitly in economic models. In such a case one needs do develop algorithms that grow a network. Such an algorithm may resemble social mechanisms that could also be operating in reality. Alternatively, it may 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 networks with a particular heavy tailed degree distribution (Barabasi & Albert, 1999): 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. Consequently, vertices that already have a large number of connections, are likely to acquire even more connections. This mechanisms has many names, e.g. preferential attachment or the Mathew effect. Despite being abstract, it usually comes with a natural interpretation and may well represent (or at least approximate) mechanisms operating in reality.

Another important algorithm developed by Clauset, Moore, and Newman (2008) seeks to generate networks resembling the community structure that can often be observed in real world networks. In contrast to the Barabasi-Albert model, the algorithm proceeds

[6]Note that neither economists seeking instrumentalist or mechanism-based explanations can be considered to be more reflected on their underlying epistemology *per se*. Also neither an economist seeking mechanism-based explanations nor someone seeking instrumentalist explanations needs to believe in the idea of a truthfully description of the real world (see also section 5, or Reiss (2012) for a different opinion on this).

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. 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 'mechanism-based' algorithms. Many other properties of interest must be generated via algorithms that do not resemble real world mechanisms. From an instrumental viewpoint the fact that does not represent any drawback. For those seeking mechanism-based explanations, however, this situation motivates further research on mechanism-based algorithms.

While usually not discussed explicitly in economics papers, the practical implications of these two ideal epistemological approaches are huge. In particular, they provide different answers to two important and practically highly relevant questions: firstly, how much importance is given to the descriptive realism of assumptions. secondly, how can we discriminate between two competing explanations that are both consistent with the data?

2.1 The importance of the descriptive adequacy of model assumptions

Explicit (and graphical) meta-frameworks can facilitate the identification of epistemological differences. They also help to illustrate why the descriptive adequacy of assumptions is more important if one seeks mechanismic explanations and methods used to this end must be flexible enough to formalize a wider range of assumption than methods useful for an instrumentalist investigation. Therefore, I will introduce a meta-framework for thinking in terms of models (based upon Miller and Page (2007) and Mäki (2009a)).

According to Mäki (2009a) – who refers extensively to Giere (1988) – models have two 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, 2009a). 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.

What the essential properties of a system are is a question of *ontology*, i.e. a question of what actually exists. It is important to make clear what kind of system one wishes to investigate and what are its essential properties before reasoning about how knowledge about these systems could - in principle - be generated.[7] In cases were the essential properties of the SUI do not have an epistemic counterpart in the model, it 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, 2009a).

It is therefore not surprising that people from different ontological starting points judge models differently. For example, critical realists may find it difficult to analysis parts of the economy they consider being open systems (or being characterized by what they call 'demi-regularities') with formal models that in themselves are closed systems. Instead they argue for different ways of describing (i.e. representing) the reality. The only possibility to ensure a productive exchange among the different communities is transparency and the willingness to deal with the ontological and epistemological implications and assumptions of models explicitly.

To this end,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 of the 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. We therefore call g the *complexity reduction*

[7] See G. M. Hodgson (2015) for a nice introduction to essentialism.

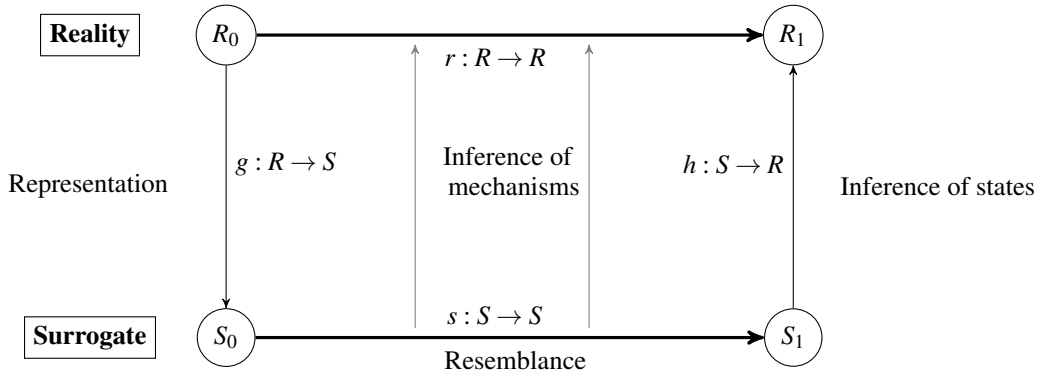

Figure 2: A development of Mäki's MISS concept, where we interpret modeling as a mapping process (following Miller and Page (2007)) and distinguish between the inference of mechanisms and states.

function of the model. Note that S_0 could consist, for example, of sentences, equations, or algorithms, depending on the type of model. Note also that figure 2 captures a universal feature of models: every model (or description if one wishes to avoid the term 'model') necessarily reduces the complexity of the real world, simply because "any (discursive or formal) theory must necessarily abstract or isolate" (G. Hodgson, 2006, p. 128).

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 directly. The process of model exploration may take the form of solving a system of equations or conducting a *Gedankenexperiment*, depending on the type of the model (i.e. formal or verbal).

Figure 2 nicely illustrates the different ways models are expected to generate knowledge depending on whether one refers to instrumentalist or mechanism-based explanations: a model that tries to provide mechanism-based explanations 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. As I will argue more extensively in section 4, this is also the reason why methods suitable for the study of mechanisms must allow for a more flexible complexity reduction function: the form of representation constrains the number of mechanisms that can be conjectured in the resulting model.

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 but the outcome it produces. Therefore, the flexibility of the complexity reduction function here is less of an issue. 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.

Besides putting different weight on the adequacy of the representation aspect of a model, different epistemologies also require different concepts to distinguish among competing explanation that provide equally good predictions.

2.2 How 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 common to use *Occam's razor* to discriminate among them: if two procedures provide equally good predictions, the simpler procedure should be preferred. Because of its reference to prediction, Occam's razor is particularly useful if one is working within an instrumentalist approach. 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, a common phenomenon referred to as *equifinality*. Secondly, determining the complexity (or the simplicity) of a mechanism is 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 (Bunge, 1962) as a discrimination device: one explanation can be said to be deeper than another if it uses more detailed and plausible mechanisms to explain an 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 explanations for this pattern: The first 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 by assuming substitution effects due to the inferiority and superiority of goods (e.g. Dixit and Stiglitz (1977), Wadman (2000)).

However, as Witt (2001) points out, 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. the need to collect food and shelter), or they can be non-physiological and non-homeostatic (e.g. the want for group conformity or status seeking). Witt proposes an explanation on the grounds of his *continuity hypotheses* according to which the origins of preferences are to be explained by evolutionary theory. 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.[8]

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.[9]

Using Occam's razor as an alternative discrimination device would be less clear: since 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 judgement (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 epistemologies.

The next section applies the concepts described above and shows that the dominant epistemology in economics has actually changed during the last decades.

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 focusing on the field of development economics after WWII.

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

[9] The concept of deep explanations also provides an additional answer to the question posed by Spiegler (2011): "why should one use a bounded-rationality model if one can get the same results with a standard rational choice model?" Yes, one can get *almost everything* as the result of a rational choice model, but this would not be a good practice since one would always get the mechanisms wrong.

3.1 From realism to instrumentalism...

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). It can be considered the vantage point for the research program of the Cowles Commission that was dominant in the post-war period until the late 1980s. 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. This requires 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). This means that the primary interest of the Cowles Commission program was to identify (and quantify) social mechanisms.

This approach later was criticized for the underlying theory, most famously through the Lucas Critique (1976) according to which 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 disagreement 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 (or, speaking with figure 2: a particular complexity reduction function): 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.

Two important streams of research marked a departure from this strategy. One on the macroeconomic, the other on 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 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. 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 future states of reality (i.e. working out the relationship $s \circ h$).

Secondly, on the microeconomic level, the methodological movement towards randomized controlled experiments (RCT) from the 1990s was motivated by a similar scepticism 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.[10] 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 microeconometrics.

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

3.2 ...back to realism?

This *epistemological* shift away from mechanism-based explanations is now beginning to be criticized:

For example, both Deaton (2010) and Grüne-Yanoff (2016) stress that without a direct consideration of economic mechanisms, results from empirical studies can hardly be generalized. Deaton shows that without identifying 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. Successful intervention always requires knowledge of the underlying mechanisms (Schelling, 2005; Deaton, 2010; Rodrik et al., 2004; Hallak & Levinsohn, 2004; Grüne-Yanoff, 2016): 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 (Deaton, 2010).[11]

Also, the literature on the relationship between economic growth and trade focused on determining the correlation between various trade policies and economic growth and it is not clear how and through which channels international trade affects economic development (Hallak & Levinsohn, 2004). This makes it very difficult to develop adequate trade policy. Rodrik et al. (2004) also 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.

An ultimate argument in favour of mechanism-based explanations is that only a reference to concrete mechanisms provides us with more means to judge the plausibility of an

[11]Further examples, in particular for policies focusing on nudges, can be found in Grüne-Yanoff (2016)

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 impossible to say whether it is a causal relationship or an omitted variable that has caused this result.

If one accepts these epistemological arguments in favor of mechanism-based explanations, one must ask whether particular methodological changes must accompany this shift.

4 Methodological implications of mechanism-based explanations

One of the key objectives of this paper is to emphasize that research methods and epistemologies must fit 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 2.1 suggests at least two criteria for successful modeling techniques when it comes to the conjecturing of social mechanisms:

The first property concerns its ability to formalize flexible assumptions. Otherwise, it is not possible to conjecturing 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 modeling 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) or 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 would not be a mechanism-based explanation any more.

A second feature that is helpful for generating mechanism-based explanations is that of modularity. This means that it should be possible to extend 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.[12] 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 often causes confusion about the functioning of the model. Instead, one should start with a simple model as possible and add more mechanisms sequentially. Of course, understanding the effect of a mechanism in a model is different to identify causal mechanisms in reality. Inferring from the behaviour of a formal model to the functioning of the real world involves a process of interpretation and reflection, otherwise one would conflate epistemology with ontology. However, understanding better the necessary and sufficient conditions for an outcome to occur in the model can help tremendously in reasoning about reality. Better understanding reality or to make policy decisions is ultimately a matter of judgement to choose among and relate different causal structures suggested by different models to each other (Dow, 2004).

How can economists react to the demands of mechanism-based explanation? Two

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

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.[13] 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.

A 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 behavioural economics and complexity economics are two examples for research communities that currently pursue this strategy. Both approaches have departed significantly from mainstream economic theory. In particular, they try to avoid using utility maximizing rational agents 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

[13]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).

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 for having proposed policy measures that have led to the period of stagflation in the 60s and 70s. The response of Lucas and other economists was the concept of rational expectations. The resulting real business cycle (RBC) models 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 (Fagiolo & Roventini, 2012).

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)).[14]

[14] 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).

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 try to expand their theory with a small, but accepted set of methods within 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. Many of the researchers have a background in the natural or computer sciences. This indicates why the willingness of complexity scholars to give up established economic concepts is much higher than of New Keynesian economists. What holds the heterogeneous complexity community together is the (ontological) 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 computational experiments to derive the systemic implications of this initial configuration.

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 that could not be developed in a general equilibrium model. [15]

But the flexibility of ABM may also considered a major weakness of these models. 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.[16] In other words, while some consider the optimization-cum-equilibrium approach in orthodox economics to be a 'straitjacket' (Farmer, 2013, p. 383), others may consider it a useful 'disciplining device'.

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 & 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.[17] Again, this both represents a strength that, if not sufficient information is available, can turn into a weakness if the model specification becomes arbitrary.

4.3 Discussion

The central claim of this paper has been that methods must fit epistemologies and that there are good reasons to change the underlying epistemology in development economics towards a focus on mechanism-based explanations. Consequently, some change in the methodology

[15] But it does not mean that using ABM *automatically* represents a departure of an instrumenalitst epistemology, let alone a more radical departure than DSGE models. As a flexible tool, it is compatible with a wide range of epistemological orientations others than those of complexity economics.

[16] 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.

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

of economics must also take. How do two of the most prominent methodological reactions fit to a re-focusing on mechanism-based explanations? Complexity economics and ABM certainly represent a potentially more radical shift towards mechanism-based explanations than the New Keynesian approach.

As mentioned above, mechanisms are to be understood as "sequences of states within a given system" (Bunge, 2004), and a mechanism-based 'explanation' therefore means the conjecturing of such a sequence.

For particular mechanisms to be conjectured, the representation of a model must be sufficiently adequate. For example, if a particular method – such as DSGE modeling – does not allow one to express true uncertainty, then all mechanisms for which true uncertainty is important cannot be conjectured by such a method. Mechanisms involving the social learning of ecologically rational heuristics that allow agents to make decisions within a context of true uncertainty, for example, could not be conjectured using DSGE models. For the purist advocate of mechanism-based explanations this is a serious drawback of this method. She would either reject the use of such a method entirely, or at least call for methodological pluralism such that this kind of situation can be analysed with different methods. For a less purist advocate of mechanismic explanation, or even an instrumentalist, this inability of the method is not *per se* a problem – only if the effect of true uncertainty could not be replicated by the particular method she would started to be worried.

Thus, the potential flexibility of the complexity reduction function in ABM translates into greater potential for conjecturing mechanisms – a clear advantage of this method. One might argue, however, that because of their analytical core, DSGE models are better off when it comes to the *verification* of a mechanism *within* the model. To address this issue in the ABM approach, greater transparency, better verification methods and a closer collaboration between empirical and theoretical work is needed. One way to achieve the latter point lies in the concept of evidential pluralism, as suggested by Moneta and Russo (2014): within such an approach, ABM can capture the *mechanistic* knowledge demanded by the authors, while the entire explanation also makes use of sufficient *statistical* knowledge about the system under investigation. The potential role of ABM within 'evidential

pluralism' is certainly an important subject for future research.

5 Summary and outlook

Epistemological reasoning is an important part of economic research. This fact is not yet adequately reflected in the typical structure of economic research papers which usually do not discuss epistemological issues explicitly. Given the important practical implications of epistemological orientations, this is a very unfortunate state of affairs.

To illustrate this practical relevance of epistemology we have looked at significant changes of the epistemological orientation in development economics in the recent past. The changes from realism to instrumentalism and back that have taken place created the need for methodological innovations since different epistemologies fit to different methodologies. Given the current tendency towards mechanism-based explanations, we have discussed two different methodological reactions that could accompany this epistemological shift, at least for macroeconomics: on the one side, New Keynesian economists argue for the use of more sophisticated DSGE models, while many complexity economists favour the use of agent-based models.

Both approaches represent a movement towards a more realist assessment of the economy, but they differ in the extent of this movement: The New Keynesian approach remains much more in the realm of neoclassical economics than the complexity approach.

As in this case, where members of the respective research community are very critical of the methods used by the others, we believe that many methodological debates in economics are at the core about epistemology: researchers prefer different methods because 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. [18]

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 mechanismic explanations are sought for. Economists that aim for mechanismic explanations, and there seem to be ever more of them, should rather strive for *deep explanations*.

Furthermore, 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 about epistemological orientations usually cannot be finally resolved within a reasonable timespan, and the example of development economics discussed above showed that there is often a back and forth between different dominant epistemologies, different epistemological orientations and their methodological implications should be tolerated within the profession (see also Dobusch and Kapeller (2012)).

Our elaborations above indicated that there is not only no 'one correct model', but probably not 'one correct epistemology'. Rather, a pluralism of different epistemological (and consequently methodological) approaches must be not only tolerated but actively supported. For such a pluralism to be productive, however, it is important that epistemological considerations are made more explicitly such that a productive exchange among the different approaches becomes feasible. Otherwise, an unproductive pluralism of many different, isolated communities, all with their own accepted set of models and unable to understand what is going on in other communities, will be the inevitable result.

[18] Of course, the set of accepted complexity reductions within a scientific community are also determined partly not only by ideology, but also by the institutions and power relations within the scientific community. Particular players have an interest that certain methodologies are considered as 'standard' and others are not accepted methods of research. And certain positions in the scientific system (e.g. journal editors of heads of scientific associations) give such players the influence needed to safeguard their interests by affecting the dominant epistemology and the set of accepted research methods. Such sociological aspects of the problem were not part of the present article, but would be a most fruitful element for future research.

It is the important task of economic methodologists to support a productive pluralism by identifying the different epistemologies and to relate different research approaches to each other (see already Elsner (1986)). Arguments against such a productive pluralism, 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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