12. Core models in macroeconomics

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1. INTRODUCTION

Macroeconomics is governed by a stronger methodological discipline than just about any other academic field. At the same time, the culture of shunning and disparaging methodological discussions is prevalent and perhaps even stronger than in other parts of economics. Accordingly, and as has been noted by various economic methodologists (e.g. Dow 2012, p.107), it is difficult to find explicit defences of the mainstream approach. It is somewhat unfortunate because such methodology by silence allows macroeconomists to sleepwalk into their methodological positions (see Hoover 2010, 2012, 2015b). The main defences for what has become the DSGE approach were written more than 40 years ago (Lucas 1976; Lucas & Sargent 1978). More recent explicit defences include Chari and Kehoe (2006), Wickens (2010), Christiano et al. (2018), Kehoe et al. (2018), and perhaps Reis (2018).

In recent years, however, the societal pressure resulting from the perceived failure to handle the financial crisis has provoked more methodological reflection by mainstream macroeconomists than before. This chapter provides a description and a philosophical evaluation of such mainstream methodological writings.

Duarte and Lima evaluate the mainstream macroeconomic methodological narrative as follows:

> The operating premise in assembling the present volume was that the internal narrative of mainstream macroeconomics is likely to be unreliable, its principal function being to buttress a particular, historically contingent methodological argument. (Duarte & Lima 2012, p.4)

This is a strong claim in that it presupposes a clear answer to the question: what is mainstream macroeconomic methodology? I take this to be a genuine question that requires further enquiry.[1] Yet, Duarte and Lima's warning must be taken seriously: it could turn out that what we see in explicit macromethodological discussions is not what actually governs the methodology of the field.

In 2017, Vines and Wills organized a conference on macroeconomic methodology. The final papers came out in the *Oxford Review of Economic Policy* (*OREP*) 2018. They sent all the participants a set of questions to which they wanted the papers to relate. They asked whether the current core model is appropriate, and if not, what macroeconomists should include in a new core model. They further specified that by a core model they meant Christiano et al. (2005) or Smets and Wouters (2007). Various authors responded. Some were willing to reject the core model altogether, while others proposed modifications and extensions to it.

It is evident that many macroeconomists seem to take for granted that there is a 'core' model, and that there should be one in the future as well. Olivier Blanchard provides an explicit statement of this view:

> The pursuit of a widely accepted analytical macroeconomic core, in which to locate discussions and extensions, may be a pipe-dream, but it is a dream surely worth pursuing. If so the basic modelling choices of DSGE are the right ones … Starting from explicit microfoundations is clearly essential; where else to start from? Ad hoc equations will not do for that purpose. (Blanchard 2016)

I find this intriguing because Blanchard simultaneously posits that Dynamic Stochastic General Equilibrium (DSGE) models are 'seriously flawed'. How could modelling choices be obviously right, but the end result seriously flawed? He seems to take for granted that there is some benefit from having a core model. But what exactly is this benefit? Two interrelated features of the methodological discussion make the task of figuring this out difficult. First, the authors in the *OREP* 2018 methodological issue as well as elsewhere use the notion of a core model in several different ways. As we will see, the notion of a *core model* is related not just to microfoundations but also at least to identifiability, Duhem–Quine problems, the needs of communication, the way in which central banks use models in their deliberation, graduate teaching in macroeconomics, tractability, DSGE models, and general equilibrium modelling. Second, I have never seen any economist make an effort to define the notion of a core model, possibly because I have also never seen any economist make an effort to defend the need for a core model. Instead, these issues are taken for granted. These two features make the enterprise of writing this chapter a highly speculative one. Why, then, is it justified to ask the question: what could be the methodological argument for having a core model?

There must be such an argument, and it has to be rather convincing because, as Wren-Lewis (2016, 2018) has argued, there are costs to having a core model. These costs derive from the lack of diversity that could pertain to any kind of framework to which strict adherence is required. Consider the following train of thought. Soon after the apex of the financial crisis several central

bank policy-makers lamented that they could not use macroeconomic theory for dealing with the imminent problems. For example, Trichet (2011) said the European Central Bank (ECB) had to resort to history rather than macroeconomic theory (see also Buiter 2009). According to Krugman (2018), the central banks often resorted to the old IS–LM framework, i.e. a framework that was developed in the 1930s and that has not been taken seriously in research for decades.[2] The problem was that since macroeconomic theory had been developed by focusing only on one kind of model, the relevant models for dealing with the crisis were never developed. Wren-Lewis (2016, 2018) claims that this is a problem that could have been avoided: had the macroeconomics profession not been so adamant about having microfounded models, developing the so-called 'structural econometric models' would have led to the development of macromodels that take financial frictions into account. More generally, if it is likely that one cannot know beforehand what kind of processes lead to the next economic crisis (and Blanchard 2018 explicitly endorses this view), it is likely that the best way of preparing for it is to let several different lines of research coexist in macroeconomics. But this means precisely that there should not be any core model (in at least one sense of the term).

I take it that the best genuine methodological argument for having a core model could be related to the ease of comparing results from different models, and thus the possibility of figuring out what is causing what in macromodels and in reality. Before examining in more detail how the argument could work, and assessing it, it is necessary to see what other possibilities there are.

2. THE MANY FACES OF CORE MODELS

Here is a list of possible meanings for a 'core' model:

1. A codification of what should be regarded as the most important characteristics of macroeconomies (Vines & Wills 2018).
2. A benchmark (Lengnick 2013; Blanchard 2018).
3. DSGE models (Blanchard 2018).
4. Empirical results only concern the non-core elements. Bank of England's 2005 core-non-core model: the core is shielded from empirical testing (Harrison et al. 2005).
5. Whichever model a central bank uses the most in its decision-making (e.g. Lindé 2018).
6. A simple enough model to be taught to graduate students (Blanchard 2018; Reis 2018).
7. A general equilibrium model (Lindé et al. 2016, p.2249).
8. A common formal language of communication (Blanchard 2016; Kehoe et al. 2018).

9. A Kuhnian exemplar.[3]

Many of these interpretations are related to others. The fact that there are several possible but interrelated meanings indicates that macroeconomic modelling faces several different kinds of institutional, theoretical and practical demands. One problem of the methodological discussion in macroeconomics is that the notion of a core model is used in many ways, and it is not clear how the methodological arguments for various particular interpretations of a core model are to be evaluated.

It is important to find out whether having a core model has epistemic benefits because there is the possibility that economists want to have a core model mainly for non-epistemic reasons. For example, it is obvious that the more macroeconomists agree on the core structure of the economy, the easier it is for policy-makers to see why they propose a particular policy (see Duarte 2012). To put it bluntly, the more unanimous macroeconomists are, the more influence the field will have as a whole on policy and on people's economic views – at the very least this is what they themselves seem to believe.

This perspective was explicit in a collection of methodological reflections in the *American Economic Review* in 1997. Various authors (Blinder 1997; Eichenbaum 1997; Solow 1997; Taylor 1997) responded to the question: is there a core of practical macroeconomics that we should all believe? The reason for mentioning the word 'practical' here refers to the fact that macroeconomics is used in central banks' decision-making bodies, government departments, international agencies, and corporations with little interest in developing macroeconomic theory. Some commentators note that central banks often deliberate until they come up with an agreement on the causal forces relevant for their decision (see Faust 2009). Nevertheless, the process of providing a conditional prediction or a policy analysis always starts from a 'baseline', a first run of the main model in the bank. As far as I understand, when macroeconomists talk about the 'workhorse model' (e.g. Lindé et al. 2016), they refer to such model-use, and they could equally well talk about the 'core' model.

Most central banks adopted DSGE models only after the model was developed into an estimable form that was reasonably in agreement with the data (Smets & Wouters 2003, 2007; Christiano et al. 2005). Given that the policy-makers are in the business of solving practical problems, there is a long tradition of using a 'suite of models' in their decision-making: they use several different and incompatible models simultaneously.[4] Thus, even if the monetary policy committees need to come to an agreement at least once in a month, most central banks use several models in addition to judgment to come up with such an agreement. Thus, such monetary policy-making needs have never required a single model and a single agreed view on the structure of the economy.

I conclude, then, that even though some supporters of DSGE models would like to substitute such judgment-making in central banks with the core model, the fact that many central banks use the DSGE model does not yet provide an independent epistemic argument for it.

The interpretation (7) requires some explication. Macroeconomists commonly talk about 'satellite models' (see e.g. Vlcek & Roger 2012; ECB 2016). These are models that concern particular blocks of the whole economy that can be analysed separately from the general equilibrium core model because the feedbacks are rather negligible. These models are used to feed information into the general equilibrium model or to provide independent tests of them. One meaning of a core model is thus that it refers to those elements and processes of the economy that cannot be reasonably studied in isolation from the other major macroeconomic influences. If this is all that the notion of a 'core model' means, the quest for having one is obviously justified because it is necessary to study general equilibrium effects in macroeconomics. However, it seems equally clear that core models are not just any models that study general equilibria.

Consider now the needs of graduate education in economics (6) together with the notion of a DSGE model (3). Macroeconomics is a complex and a technically demanding field. The models have to represent the linkages between different markets. At the same time, the economy as a whole can be thought to be subject to a variety of different stochastic exogenous shocks. Finally, the economy evolves in time, making various dynamic relationships crucial for analysis and prediction. The models must thus be Dynamic and Stochastic, and they must concern the General Equilibrium. If one describes DSGE models in this way, it seems obvious that a major part of macroeconomics has to be dealing with a DSGE framework. However, the existing DSGE models provide a very particular way of implementing the required model-structure.

The main constraint for macromodellers is that the models are exceedingly complex. The complexity implies that modellers must make various choices concerning variables and the associated macroeconomic mechanisms, and the solution and estimation methods. DSGE models cannot be solved analytically and system-level estimation is obviously more difficult than one equation at a time. Mathematical tractability is thus the overarching constraint, and it takes years to properly learn to use such models. Consequently, in practice, graduate students are taught to deal with macroeconomic questions with only a few different model-kinds. Here a core model provides a simple enough platform for graduate students.

The characterizations of 'core models' that we have seen thus far provide explanations for why macroeconomists might hanker for core models for pragmatic reasons that have nothing to do with the possible epistemic justifications.

They do not provide a convincing methodological rationale for the demand for a core model.

There are at least three interrelated ways of arguing for epistemic benefits of core models: interpretations (1), (2) and (8): (1) Having a benchmark is required in order to solve Duhemian problems. (2) Having a commonly accepted view about the most important causal relationships leads to a methodological consensus, and methodological consensus has benefits. (8) A core model is needed for efficient communication. Before turning to a detailed investigation of interpretations, it is necessary to clarify the difference between arguments for having a core model, and arguments for the particular model that is taken to be the core at the moment. In a paper that concludes the methodological messages from the contributions in the *OREP* 2018 issue, Vines and Wills (2018) note that the various macroeconomists who took part in writing the methodological essays agreed that the core model of Smets and Wouters (2007) has problems. They summarize the way in which the core model should be modified as follows. Financial frictions have to be included, rational expectations should be relaxed, the models should include heterogeneous agents, and finally, the model and the additions should be underpinned with appropriate microfoundations. For now, it is not necessary to go into the details of this agreement. It is important to bear in mind, however, that although there has been extensive methodological discussion on the last three of these, many but not all current DSGE models continue to use rational expectations (RE) and a single representative agent (RA). At the same time, everybody agrees that the core model that incorporates RE and RA is inappropriate. The difference between arguing for having a core model, and arguing for some features of what is now taken to be the core model is thus that the former are arguments for a particular kind of research tradition with the associated research heuristics, while the latter are arguments for particular model-kinds.

The relevance of this distinction is evident if one considers various ways of interpreting the notion of a codification of the most important characteristics of macroeconomies. First, the most important characteristics of macroeconomies could consist of well-known empirical regularities that every macromodel must be able to explain (e.g. Blinder 1997). This is often cashed out in terms of the notion of stylized facts.[5] Second, one could also mean that the core consists of a set of methodological agreements, a consensus on methodology. More particularly, one could also refer to the core as particular elements in macromodels, especially the assumption of intertemporal optimization as expressed in an Euler equation. Here the 'core' would refer to model-elements common to a large number of models, just like in the philosophical literature on robustness (e.g. Weisberg 2006; Kuorikoski et al. 2010).[6] A core macromodel, then, is a set of core elements exemplified in a coherent model. It is perhaps better to call it a core model family because a set of core elements can be exemplified in

a variety of ways – recall that Vines and Wills pointed out both Christiano et al. (2005) and Smets and Wouters (2007) as core models. This notion of a core model is closely related to Kuhn's concept of an exemplar. Kuhn initially described paradigms in various ways. In the third edition of *The Structure of Scientific Revolutions*, he wanted to substitute the term paradigm with exemplars defined as 'concrete problem-solutions that students encounter from the start of their scientific education' (Kuhn 1996, p.187). The term paradigm then kept its meaning as a group's shared commitments.

For an example of seemingly arguing for a broad methodological approach, let me quote the defenders of the methodological status quo at some length:

> [A]lthough macroeconomists often hold heterogeneous beliefs about how promising any particular mechanism may be in accounting for features of the data or about the benefits of any particular policy, they agree that a disciplined debate rests on communication in the language of dynamic general equilibrium theory. By so doing, macroeconomists can clarify the origins of any disagreement and hence make progress on how to settle it. For example, when two different views are justified by fully specified quantitative models, it is relatively easy to pinpoint which key parameters or mechanisms are at the heart of the differing conclusions for policy. Hence, future work can attempt to discern which is in greater conformity with the data. (Kehoe et al. 2018, p.163)

What is defended here is a core framework for study that allows macroeconomists' disagreements to be handled in a disciplined way. They argue that having such a framework has an epistemic benefit that consists in helping to pinpoint where the problems lie so that empirical evidence can dissolve conflicts – interpretations (1) and (8) seem to be compatible with this kind of claim. In principle, for this kind of justification, a core model can have just about any particular features. What is important is that the formal language is flexible and sufficiently clear to allow communication. Strictly speaking, a core model as an exemplification of common model-elements is different from a language of communication or a modelling framework. Yet, it does not seem to be the case that Kehoe et al. are merely defending a language or a framework. Their admonition to solve the disagreements with data is somewhat disingenuous: they are simultaneously arguing for explicitly parameterized models, and this implies that only DSGE models are allowed into the empirical comparison. The set of relevant datasets is limited to a few variables in DSGE models due to the problem of stochastic singularity. But the dissenters like Fair (2020) and Favero (2007) argue precisely that such limitations are responsible for a highly questionable empirical methodology. In other words, given that they disagree on which data are relevant and which empirical methodology is correct, the suggestion by Kehoe et al. can be taken to be arguing for a broad methodological approach only if one disregards the methodological disagreement over the

relevance of the consequences of stochastic singularity. Although Kehoe et al. are seemingly arguing for a common framework of study, what they identify as the epistemic benefit from having a framework can be achieved only by using a particular kind of model which is taken to be contentious by those who do not use it.

Ghironi (2018) argues that DSGE analysis does not even require the most standard Euler equation that ties expected growth in the marginal utility of consumption to the real interest rate. More generally,

> It is useful to establish benchmark, transparent results in simplified frameworks that can then guide our understanding of the implications of working with more realistic assumptions. But nothing in the DSGE approach constrains us to using any of those ingredients. (p.211)

This is one way of arguing for core models as benchmarks (2). In Ghironi's version, the argument is related to core models as codifications of what ought to be regarded as the most important macroeconomic relationships in an interesting way:

> If a SEM [simultaneous equation model] or a toy model make it possible to address the question of interest, my view is that we only stand to learn from comparing their results to those of different types of DSGE models … When results are similar, we will perhaps feel more comfortable about them, and when they differ, we will have new research questions to ponder. (2018, p.214)

Is Ghironi arguing for having a common framework of analysis, or for the particular core model that is now prevalent? He clearly takes the answer given by DSGE models to be prima facie correct. If he did not think it is correct, it would be difficult to see why there would be no need for further investigation if a model provides a result consistent with the DSGE framework. But then, if the DSGE results are correct by default, why is one allowed to change the framework by removing the core parts?

Now, is the DSGE framework a codification of the most important characteristics of macroeconomies (1) according to the defenders of the framework? It is difficult to answer this question because they are seemingly defending a broad framework in terms of a flexible language, but at the same time, they seem to take for granted that features of the framework are so obviously right as to be beyond question. One must also distinguish between the most important causal forces in the economy, and a codification of the methodology, even though it is obvious that the methodology one chooses limits what kind of causal forces can be studied with it. It is easy to understand that those who think that the current DSGE core model does not study the most important relationships are willing to reject the DSGE core model altogether (e.g. Stiglitz

2011, 2018; Hendry & Muellbauer 2018). For example, if you think that issues of coordination and distribution of wealth are highly important in understanding business cycles, then it does not seem to make sense to work with a representative agent model that is assumed to be in general equilibrium at every instant. If one believes that problems of aggregation (see e.g. Geweke 1985) are severe, one is not likely to study a representative agent model because it gives distorted results in addition to preventing the study of those problems.

To the best of my understanding as an outsider, many macroeconomists seem to think that it is impossible to come to a widespread agreement on the most important causal characteristics of macroeconomies. At the same time, there is considerable concern over achieving a consensus on methodology (see e.g. De Vroey & Duarte 2013). Indeed, various macroeconomists seem to take for granted that progress requires consensus (see e.g. Blanchard 2000, 2009; see also Duarte 2012). After the neoclassical synthesis, the 1980s were commonly viewed as a horrible period of methodological disarray (see e.g. De Vroey 2016). Goodfriend and King (1997) argued in favour of the new neoclassical synthesis, and Goodfriend (2007) and Woodford (2009) argued for supplementing this framework with Bayesian estimation. As we can now see, this new synthesis survived the financial crisis in that DSGE modelling continues to be developed by theorists and used by central banks in 2020.

Is there a solid methodological rationale for hankering for such a methodological agreement? One way to study this question would be to let the historical evidence speak by comparing the contributions in the 1980s to the times before and after to see whether the methodological disagreements hampered macroeconomics from progressing. The problem is just that such a historical account requires a clear notion of progress, and it may be difficult to formulate one. As background information, it is important to remember that some representatives of the new classical school (Lucas and Prescott in particular) were commonly viewed (by the Keynesians at the time) as intolerant, ignoring criticisms and refusing to converse with anybody who did not agree with their methodology. It is clear that such bad behaviour hampers any science because methodological reflection and criticism becomes impossible: if nobody ever needs to change his or her mind about methodology because everyone is allowed to work in a methodological silo, wildly incorrect ideas may blossom. From this perspective, the macroeconomists' call for core models as a way of implementing synthesis is understandable. However, it is possible that such disruptive features were a historical accident that had to do with the mindset of some of the key players. In other words, it does not seem to be the case that avoiding such disruptive practices requires a commonly agreed methodology.

3. SOLVING DUHEMIAN PROBLEMS?

Efficient methodological communication requires that one can compare the models with each other. Let us thus consider the idea of core models as benchmarks. Having benchmarks may indeed provide genuine epistemic benefits. The idea is roughly the following. Macroeconomics is all about figuring out what causes what in the economy. At the same time, it is very difficult to study such questions because the economy is an enormously complex system, the structure of which is also changing in time. Furthermore, since similar macro-outcomes may be the result of several different individual behaviours, and several possible mechanisms translating such behaviours into macro-outcomes, it is important to be able to see how any given modification to the model affects the outcomes. It is thus commonly agreed that the Duhemian problems are more severe in macroeconomics than in many other fields.

A benchmark core model is a set of assumptions and the associated model-outcomes that are known by all the practitioners in the field. The benchmark yields comparative information about the relevance of various components to the results in a family of models. This information derives from robustness of or change in results as some of the assumptions are either changed, removed or added to the benchmark. The point with having a benchmark model is thus related to efforts at solving Duhemian problems. Modifying or adding elements to the benchmark shows how various factors affect the economy – at least in the model. The role of the core model is to help in identifying which of the elements are responsible for the results when the core is modified or if elements are added to it (e.g. Blanchard 2000). What is driving the results can be inspected from the comparison between the core model and the modification.

I want to make it clear that I don't believe in the Duhem–Quine thesis if it is taken to posit that it is impossible to falsify hypotheses in science due to the fact that one can always blame the auxiliary assumptions rather than the hypothesis itself. I believe it is indeed possible to allocate confirmation and disconfirmation into individual assumptions (see Lehtinen 2016, 2018 for details). As an illustration, consider an embarrassing episode for economic methodology. Cross (1982) argued that the linchpin of monetarism, the stable demand for money, cannot be falsified due to the possibility of laying the blame on a huge number of auxiliaries. As it happened, during the year in which Cross's paper was published, the velocity of money became highly volatile, and has remained so ever since (see e.g. De Vroey 2016, p.90). To put it simply, all of a sudden, the empirical result on which monetarism depended, ceased to be robust. This single piece of evidence has practically buried monetarism in the old Friedmanian sense. I do not, however, either think that

Duhemian problems are always easy to solve. Indeed, macroeconomists are grappling with such problems every day.

Let us now see in a simple manner how the model comparisons work, and thus how the Duhem–Quine problems are solved. Suppose the core model M consists of assumptions A, B, C and D. It also entails various results, say, R_1, R_2, and R_3. We can then write

$$M = (A, B, C, D) \vdash R_1, R_2, \text{and } R_3$$

If a model modification changes D into D′ but leaves the rest of the model intact, and if all the results remain the same, then we learn that changing D to D′ has no effect:

$$M' = (A, B, C, D') \vdash R_1, R_2, \text{and } R_3$$

If, instead, some results do change, then we learn that it did matter:

$$M' = (A, B, C, D') \vdash R'_1, R_2, \text{and } R_3$$

This representation is, of course, highly stylized. In macroeconomics, the models incorporate hundreds if not thousands of assumptions, and results are usually evaluated in terms of impulse response functions for important variables generated by the DSGE model, and the impulse response functions given by a vector autoregressive (VAR) representation constructed from the empirical data (see Duarte and Hoover 2012, pp.241–4). None of these details matter for illustrating the general logic of model comparisons for which the above representation is sufficient. The problem with not having a core model can be depicted as follows. Suppose that R_1 is an empirically problematic result while R_2 and R_3 are not. Suppose that we provide a completely different model M_{AB} that can be described as follows:

$$M_{AB} = (X, Y, Z, W) \vdash R'_1, R_2, \text{and } R_3$$

Even though the model is able to do better than the core model M in accounting for R'_1, this does not help us much because we don't know why it does so. The problem is that since the models differ in too many ways from each other, it is difficult to pinpoint which assumption or assumptions exactly are responsible for the results. Hence, it is difficult to learn in a systematic way what causes what even in the models. There is thus indeed an epistemic argument to be made for enabling comparability of models. Furthermore, the argument requires models to be at least somewhat similar to each other.

Yet, I am not convinced that such model comparability requires having a benchmark core model that is based on a DSGE platform. To see why, let us now consider two different and additional ways of achieving comparability that are not tied to the DSGE framework. First, Wieland et al. (2012) and

Wieland (2012) provide a framework for comparing the predictive abilities of different macromodels, the Macroeconomic Model Comparison Initiative. They have created a webpage (http://www.macromodelbase.com/) onto which macroeconomists are solicited to provide their computer simulation codes for their models. The paper and the website provide instructions for translating any model, including VAR models, into a comparable format.

The second example, agent-based macroeconomics, requires a separate treatment.

4. AGENT-BASED MACROECONOMICS AND BENCHMARK CORE MODELS

Agent-based macroeconomics has become a vibrant research field in the recent years (e.g. Dilaver et al. 2018; Dosi & Roventini 2019; Haldane & Turrell 2019). It provides a flexible approach while retaining the possibility of providing a microfounded model. Various authors have thus argued that agent-based models could be introduced in order to solve the tractability problems associated with constructing a new core model (e.g. Caverzasi & Russo 2018; Haldane & Turrell 2018).

It is difficult to find explicit criticisms of agent-based macro by the mainstream macroeconomists, even though it seems clear to me that they have methodological objections to this approach. Otherwise, it would be difficult to explain why there are plenty of agent-based macro papers around, but none in the top economics journals.

Blanchard (2018) worries that 'agent based modellers have not provided a core model'. He seems to be referring to the core model as a codification, known to all, of the most important macroeconomic relationships, that functions as a benchmark. With this is mind, I take Blanchard's statement to be expressing the idea that agent-based modellers should work hard to come up with a canonical version of an agent-based model that could provide a benchmark for evaluating what causes what in the economy. The idea would be that if the agent-based modellers provide a benchmark in this sense, then the mainstream should have no objections to adopting it.

Blanchard's claim that agent-based macro has not produced a core model could be interpreted in several different ways. First, it could be just a matter of noting that agent-based modelling is not yet mainstream, and that perhaps it could be. As noted before, a core model must be known by all the practitioners. Second, there are indeed several different families of agent-based macromodels (see Fagiolo & Roventini 2017, or Dawid & Delli Gatti 2018 for reviews). They are thus not even comparable among themselves. If agent-based models were to be accepted by the mainstream, one family of models would have to gain considerable traction at the expense of others so as to become the new

core model. Third, these families of agent-based models are not just different from each other; they are, rather, different from the current core DSGE model. This makes it difficult to evaluate what exactly is driving the results in these models. In particular, it makes it difficult for those who are working on modifying the core DSGE model to learn from them.

The agent-based modellers have recognized the problem, at least since Windrum et al. (2007). Indeed, the need for a benchmark is often recognized and several authors have tried to come up with a benchmark model (e.g. Caiani et al. 2016). Lengnick (2013) even tries to provide a benchmark model that minimizes the number of new assumptions. It incorporates merely some commonly accepted relaxations of the assumptions in the core DSGE model.

I find the paper by Gobbi and Grazzini (2019) to be a more promising stepping stone to creating a new core model. I will discuss it in some detail because it provides a counterexample to the idea that the DSGE approach can be justified by appealing to its allegedly unique ability to provide a benchmark. Recall that Kehoe et al. (2018) and Ghironi (2018) argued for core models in terms of providing a benchmark but then also presupposed that the DSGE model is the only proper benchmark. Gobbi and Grazzini provide an agent-based model that tries to mimic the standard DSGE model as closely as possible. In the basic version the *only* difference from Woodford's (2013) New Keynesian–DSGE model is that the model is solved by letting a finite number of agents interact rather than by imposing an equilibrium assumption. The results of this model are similar to Woodford's model as well as other to DSGE models – indicating that perhaps the way in which the models are solved is not so crucial after all. In the Gobbi and Grazzini model, D′ refers to the solution method in terms of agent interaction, and the rest of the model is identical to a DSGE model. Let us represent Woodford's model M_W as follows:

$$M_W = (A, B, C, D) \vdash R_1, R_2, \text{and } R_3$$

and Gobbi and Grazzini's model as follows:

$$M_{GG} = (A, B, C, D') \vdash R_1, R_2, \text{and } R_3$$

Gobbi and Grazzini are surprisingly silent about these results, perhaps because criticizing the assumption of instantaneous equilibrium has been such an important part of the self-understanding of the agent-based macro community: this result provides an example in which the assumption of an equilibrium does not matter very much in the end, because changing it yields results that are robust with respect to the DSGE benchmark.

They also construct a version of the model in which the assumption of rational expectations is relaxed by providing the consumers with a perturbed signal on the aggregate productivity (C′). Now the results are different:

$M_{GG}' = (A, B, C', D') \vdash R'_1, R_2$, and R_3

This version of the model shows that expectations make a difference, and that the more realistic cognitive capacities in the agent-based model also yield better empirical results. More generally, this paper shows that it is possible to build agent-based macromodels that are so similar to the DSGE models that the argument for holding onto the DSGE models in order to enable comparisons in terms of a benchmark is not quite convincing.

5. THE ARGUMENT FROM CLARITY, MICROFOUNDATIONS AND RATIONAL EXPECTATIONS

Some other economists surely think that the problem with agent-based models lies deeper: it is intrinsically difficult to see what causes what in an agent-based model, and thus the modellers will never be able to come up with an acceptable core model that can be used to figure out which parts of the model are responsible for the results. To the extent that agent-based models are indeed 'black boxes', I take this to be a real problem (see Lehtinen & Kuorikoski 2007). However, although it may be difficult, it cannot be intrinsically impossible to find out what causes what in an agent-based model. It is possible, for example, to study the process in which the results emerge by separately reviewing each time-step in the model.

I admit that I have never seen the black box argument made by a mainstream macroeconomist. It is, rather, an argument that one can see in various discussions on the status of agent-based models in science (see also Haldane & Turrell 2019). I attribute this idea to some macroeconomists, however, on the grounds that some of the arguments presented in favour of having microfoundations emphasize this kind of issue.

Agent-based models mostly hide the process in which the individuals interact. This is perceived to be a major problem by macroeconomists. Consider a recent criticism of heterogeneous agent New Keynesian (HANK) DSGE models:

> But the source of the differences between heterogeneous agent New Keynesian (HANK) and representative agent New Keynesian (RANK) economies, and the extent to which these differences are a general result rather than a consequence of particular modeling assumptions, remain obscure. While these papers have highlighted striking differences in the behavior of HANK and RANK economies, the lack of analytical tractability makes it hard to pinpoint exactly which features are responsible for these differences. (Acharya & Dogra 2020, p.1113)

This criticism is targeted at the HANK version of the DSGE model rather than at agent-based models, but it seems obvious that the indicated problem is more serious in agent-based models. Even the modeller may have some difficulty in figuring out what depends on what in these models. In contrast, many macroeconomists defend using microfounded models by appealing to the fact that such models bring 'clarity', that they 'keep the logic straight' and that they allow the modellers to see which variables respond to policy. Accordingly, the top journals in macroeconomics have developed a culture in which the general equilibrium consequences must be explicitly derived in every model even if this has already been done in previous publications.

There are various versions of this argument about clarity. Tony Yates' blog comment captures a common orthodox way of looking at things:

> Microfounded models are models which tell an explicit story about what the people, firms, and large agents in a model do, and why. What do they want to achieve, what constraints do they face in going about it? My own position is that these are the ONLY models that have anything genuinely economic to say about anything.[7]

Perhaps the following widely quoted aphorism provides another example: '… if you have a coherent story to propose, you can do it in a suitably elaborate DSGE model' (Chari et al. 2009, p.243). I added the qualifier 'perhaps' because it is not clear whether this aphorism is an argument for the DSGE model as a general language, or whether 'coherence' refers to rational expectations or to the requirement of microfoundations.

Let me briefly recap the standard methodological argument for microfounded macromodels. According to the Lucas (1976) critique, old Keynesian macroeconometric models were not able to identify the main causal relationships because they assumed that a change in (monetary) policy would change people's behaviour according to rules expressible as aggregative equations with fixed parameters. But if people were rational, their change of behaviour would change the aggregate equations themselves, and consequently, the aggregate macroeconomic relationships would not be invariant to such policy changes. Thus, Lucas recommended that one must build models that are based on individual (intertemporal) utility maximization and use rational expectations. The underlying methodological idea is that unlike behaviour, the parameters that govern individual preferences and technology will not change as a result of policy changes. These parameters are thus thought to be 'structural'.

Given that the Lucas critique is commonly taken to be related to the problem of identification, and the problem of identification is said to be related to underdetermination (e.g. Windrum et al. 2007), let me explain why I have chosen to use the term 'Duhemian problems' instead of 'identification problems'. The problem of identification arises in econometrics when two or more different

models entail the same parameter values, or when a model is consistent with several parameter values. In such cases, the models are said to be observationally or empirically equivalent (e.g. Hoover 1988, p.18; 2001a, p.15; Canova & Sala 2009). A parameter is said to be (point) identified if, given the model, it is uniquely determined from what one knows about the model and the data. There are various different notions of identification, and ways of solving identification problems (see Lewbel 2019 for an extensive review). The difference between trying to solve Duhemian problems and trying to solve identification problems is that methods of identification are limited to making particular identifying assumptions in econometrics (on causal order, on what can be taken as fixed and so on) while one may try to solve Duhemian problems also by comparing models within a model family or even by comparing two completely different theoretical frameworks (e.g. Gobbi and Grazzini 2019).

When macroeconomists talk about 'microfounded' models, they seem to mean that the models must be based on utility maximization. Given that most microfounded models are based on the representative consumer, it does not seem to be necessary to literally build an explicit story about what people do and why. Instead, as Kevin Hoover (1988, pp.243–4; 2001b, p.85; 2015b) has emphasized for decades, the representative agent is an aggregate construction that faces an aggregate rather than an individual budget constraint. Given that representative agent models must ignore individual differences in how the economy is perceived (indeed all individual differences), many macroeconomists seem to interpret the notion of microfoundations as referring to the assumption of rationality rather than to explaining the macro with the micro.[8] If this interpretation is correct, despite the appearances, requiring microfoundations may have very little to do with methodological individualism or with what real people want to achieve. For this reason, it is possible to think that insofar as agent-based models are not based on utility maximization, they are not 'microfounded' despite the fact they explain the macroeconomic events from the ground up. 'Microfoundations' means modelling the aggregate construction of the representative agent with microeconomic theory, rather than building a model that starts with real people's intentions. When Yates above is using the expression 'genuinely economic', I take him to be referring to the idea that economic theory is a formalization of rational behaviour. Requiring microfoundations on the grounds that microfounded models provide an explicit account of what people do and why is thus tantamount to believing that the neoclassical microeconomic theory is a true account of individual behaviour when they make decisions relevant for macroeconomics. Formulating microfoundations in terms of behavioural microeconomics will not be acceptable for those who think 'microfoundations' are necessary for macromodels.

Some critics of 'microfoundations' (e.g. Stiglitz 2018; Wren-Lewis in blogs like http://mainlymacro.blogspot.fi/2013/12/microfoundations-illusion

-of-necessary.html) use the term 'wrong microfoundations' to describe the standard rational account of microfoundations (as in e.g. Chari et al. 2009). I interpret the use of this term to indicate that they cannot be referring to rational microfoundations: if rational microfoundations are wrong, then microeconomic theory must be wrong.

The rational expectations hypothesis requires that the distribution of expectations in the model is identical with the objective probability distribution of outcomes. It follows that deviations between expectations and the model's predictions do not have systematic patterns. Systematic mistakes about the structure of the economy are ruled out by assumption. Rational expectations are often thought to be an extension of individual rationality into expectation formation. They are an important part of the clarity argument for the following reason. A model with rational expectations implicitly covers the whole economy, and the implications of the representative agent's behaviour have to be derived in every market assumed to be in general equilibrium. This is why it may be taken to deliver consistency restrictions across the various equations (e.g. Hansen & Sargent 2005). These cross-equation restrictions are then used to identify the causal structure in the models. When the DSGE models are empirically tested, these identifying assumptions are crucial, and the results obviously hinge on them. In other words, this version of the clarity argument concerns the most important methodological assumptions in macroeconomics: rational expectations, microfoundations, Lucas critique and identifiability.

Let us now relate the argument to the quest for a core model. The orthodox account of microfoundations can be taken to provide a reason for having a core model: a core model is 'clear', and it has microfoundations because this is ultimately what is taken to guarantee identifiability: the 'deep' parameters concerning tastes and technology are believed to be fixed. In other words, those who believe that only microfounded models are acceptable for conditional predictions trust that these are the parameters that one can assume to be unchanged by changes in policy. I venture to speculate that the role of clarity in this argument is the following: clarity comes from assuming rationality on the part of the agents. Using rational expectations together with intertemporal optimization guarantees that there is only one way in which agents may behave. In contrast, as Wickens (2010) notes, there are several ways to behave in an irrational manner. Assuming that some particular parameters are structural is taken to provide a way of solving Duhemian problems:[9] microeconomic theory is used to identify the causal structure in econometrics. Such identification obviously relies on the truth of the microtheory. Furthermore, this theory would surely not be used in identification if there were several ways of cashing out what individual behaviour entails for identification.

Is this argument plausible? Given that the argument concerns several different issues in macroeconomic methodology, I cannot provide a full discussion

of all of them in this chapter. One question concerns whether the parameters on preferences and technology are really structural (e.g. Ericsson & Irons 1995; Estrella & Fuhrer 2002). Another concerns whether rational expectation hypothesis (REH) provides a consistent methodology. Frydman and Goldberg (2007, 2008, 2013) argue that it doesn't: if market participants have heterogeneous expectations, then a model that employs rational expectations will be systematically wrong about some aspects of the economy. If one formulates expectations by ignoring the heterogeneity of others' expectations, this may lead to systematic mistakes because other people's beliefs affect their behaviour. Given that empirical evidence usually indicates violations of REH (e.g. Coibion et al. 2018), Frydman and Goldberg's argument is known to be empirically relevant. Unlike various other criticisms of the REH, this one challenges the rationality of REH.

Hurtado (2014) provides an interesting discussion of whether DSGE models respond to the Lucas critique in a convincing way. He shows that using the current DSGE models in the context of 1970s stagflation would not have been able to escape the Lucas critique: the models would have provided similar analyses to what was derived from the Phillips curve at the time. Furthermore, the results show drift in several supposedly structural parameters such as the elasticity of labour supply to real wages.

Finally, and perhaps most importantly for our purposes, the identifiability assumptions in DSGE models are commonly considered to be questionable (see Nakamura and Steinsson 2018 for a review). It has become commonly acknowledged that the likelihood functions derived from DSGE models are usually flat (Fukac & Pagan 2006; Solow 2008; Canova & Sala 2009). What this means is that the empirical studies cannot distinguish between different models of the economy.

These critical observations explain why there seems to be significant disagreement on empirical methodology in macroeconomics. The critics call the empirical DSGE approach the 'pre-eminence of theory' (see e.g. Juselius 1999, 2004, 2011; Hoover et al. 2008; Spanos 2009), while the DSGE modellers tend to think that alternatives are not able to identify causal relationships at all, and this is indeed one possible interpretation of the above aphorism.

DSGE modellers believe that figuring out what depends on what in the economy requires a particular internal structure for a model. They argue that it is impossible to infer macroeconomic causes without an a priori theoretical model. This is what grounds the practice of using identification restrictions from the theory when modellers estimate theoretical parameters from reduced forms. The calibrationists (e.g. Prescott 1986; Kydland & Prescott 1991) assume that one can insert some parameter values on the basis of antecedent empirical studies and from an a priori theoretical model, and then simulate the model.

Kydland and Prescott can also be taken to espouse interpretation (4): the core parts of the microfounded theory are not taken to be subject to falsification. Kydland and Prescott write, for example, 'Unlike the system-of-equations approach, the model economy that better fits the data is not the one used. Rather, currently established theory dictates which one is used' (1991, p.174). Hoover (1995) uses the term 'core theory' in making a Lakatosian characterization of Kydland and Prescott. I take it that the expression 'core theory' refers to the idea that what is common in a number of models is the shared theory. Kydland and Prescott posit that all 'model economies' are false by definition, and on these grounds, they do not even try to 'determine the true model' (1991, p.170). In other words, they do not even try to find a true or a best econometric model. Their models are based on a core theory which they consider so much more reliable than empirical data that they take it to be non-falsifiable. I do not deny that all models are false if what one means is just that they are never complete and that they always also idealize. Furthermore, even Lakatosian cores may be allowed to be false if one can use such core components in constructing models that are confirmed by empirical evidence. However, rejecting the empirical evidence when it is inconsistent with the established theory does not follow from these admissions.

If the argument that it is impossible to infer causal relationships without a theoretical model were to be correct, it would provide an epistemic argument for having a core theory. Does it provide such an argument? It seems to me that it crucially hinges on the idea that the core theory is true. But given that there now seems to be consensus that it is not just false, but also that it should be changed, I don't really see how anybody could seriously propose that it does provide an epistemic argument.

Although the empirical practice in macroeconomics has changed since the 1990s in many ways, and the real business cycle theory is largely abandoned, these considerations continue to be relevant because the pre-eminence of theory is clearly still present in the practice of DSGE modelling.

It is also noteworthy that while many other mainstream macroeconomists tout the benefits of clear communication as one of the main arguments for the DSGE models, Blanchard (2016) considers DSGE models as poor communication devices on the grounds that '… for the more casual reader, it is often extremely hard to understand what a particular distortion does on its own and then how it interacts with other distortions in the model.' Here Blanchard is indicating that one has to work with the DSGE model so as to be able to figure out what depends on what.

Caballero (2010) argued that the DSGE approach 'has become so mesmerized with its own internal logic that it has begun to confuse the precision it has achieved about its own world with the precision that it has about the real one' (p.85). Caballero's point is that it is not sufficient to be able to figure out what

causes what in the model; one also has to be able to establish that the identified causal relationships hold in reality.

Identification is commonly considered to be hard in macroeconomics. The DSGE approach uses the microeconomic theory for identification in the sense that some of the parameters are calibrated with the theory before estimation begins.[10] But if the whole system is misspecified – and it is commonly argued to be misspecified – the derived estimates will be biased. The DSGE approach thus ultimately relies on a strong belief in the correctness of the microfounded Ramsey model with rational expectations. The problem is just that if one does not identify the model in this way, there does not seem to be any guarantee that an empirical investigation even finds the (technology) shocks that the statistical model identified with theory finds in the data (see Juselius & Franchi 2007).

More generally, there seems to be widespread disagreement about which shocks drive the economy (see Chari et al. 2009; Kocherlakota 2010; Hoover 2015a; and Ramey 2016 for an extensive review). DSGE models often assume 'habit persistence' so as to fit the model to the data, and preferences are subject to an autocorrelated shock. It is perhaps not particularly surprising, then, that the empirical tests are not able to properly distinguish between the magnitude of the elasticity of intertemporal substitution, habit persistence and the auto-correlation coefficient of the preference shock (Iskrev 2010). In other words, the DSGE framework provides a convincing account for finding out what depends on what only if one believes that the microfounded representative agent model with REH correctly describes the economy. But then, if we are to believe Vines and Wills' conclusion from the *OREP* essays, the consensus is that this is not a correct codification of the economy.

6. 'HANK' MODELS AND MICROFOUNDATIONS

Consider a recent defence of DSGE modelling by Christiano et al. (2018). In a working paper version,[11] they defend the questionable representative agent assumption:

> So why would anyone ever use the representative agent assumption? In practice analysts have used that assumption because they think that for many questions they get roughly the right answer. For example the answer that the standard DSGE model gives to monetary policy questions hinges on a key property: a policy induced cut in the interest rate leads to an increase in consumption.

But then they admit that

> the Euler equation is satisfied … in all dates and states of nature … There is overwhelming empirical evidence against this perspective on how consumption decisions are made.

They note that the primary channel by which monetary policy-induced interest rate changes affect consumption in standard DSGE models is by causing the representative agent to reallocate consumption over time. In other words, these authors first claimed that a policy-induced cut in the interest rate increases consumption despite the fact that this assumption is empirically rejected in numerous studies, because you 'get roughly the right answer'. Given that, empirically speaking, this seems to be the wrong answer, I interpret them as indirectly arguing for microfounded models: a representative agent model yields an increase in consumption as a result of an interest rate cut, and the Lucas critique implies there is such an increase. Thus, the model gives roughly the right answer in that the representative agent model delivers the kind of results that microfounded models are supposed to deliver. The representative agent is included merely for tractability.

This is a rare example of an orthodox methodological defence of a particularly questionable assumption. However, it disappeared from the published version of the paper. In its stead, there is a short discussion on how HANK models like Kaplan et al. (2018) and Ravn and Sterk (forthcoming) study the monetary transmission mechanism in such a way that only a part of the population changes its intertemporal allocation of consumption. In other words, these new models abandon the representative agent framework in its pure form.

I bring this discussion to the fore because there are several ways to interpret what has happened. For the defenders of the DSGE approach these developments are proof of the flexibility of the approach. They also claim that the cutting edge of the literature is driven by micro data and assessed with micro and macro data. Kehoe et al. (2018) confidently argue that any *new* parameters or included features must be explicitly disciplined by direct evidence.

Let us take these methodological claims (from Kehoe et al. 2018 and Christiano et al. 2018) at face value and see what they imply if we combine them. If the HANK models were introduced because they are better able to explain the data, which micro data exactly are they referring to? Empirical results indicating a small coefficient for the intertemporal elasticity of consumption to the interest rate have been known since the 1980s. If these are the relevant empirical micro data, then it seems that, from now on, the assumption of intertemporal optimization is falsified according to some defenders of the DSGE approach. I believe that if heterogeneous agent models are now com-

monly accepted as the remedy for this empirical failure, this will fundamentally change the field of macroeconomics.

The HANK models are not microfounded in the sense of providing an account in which all individuals are assumed to be rational in the same sense. They are microfounded in the sense of providing a credible story about what different parts of the population take as constraints, and what the different agents want. The difference between these two ways of understanding microfoundations matters, because 'once one allows for heterogeneous agents together with asymmetric information, it is difficult to take fundamentals-driven, rational expectations seriously as a benchmark assumption' (Miller 2011, p.22). Note that the HANK models pose this challenge even though replacing one representative agent with two or three does not really solve the problems of aggregation. Even the rationally behaving part of the population must make an ad hoc assessment of which share of the population is not maximizing an intertemporal utility function in order to formulate rational expectations. In other words, formulating rational expectations becomes very complex.

These are reasons why the HANK models provide significant methodological hope for the critics of the DSGE approach. After decades of criticisms that seemed to have very little effect on the DSGE modelling choices, there are now models that get rid of the representative agent, and in so doing, they also open the floodgates to models that are only partially microfounded, and in which the very notion of rational expectations does not seem to be easy to apply. The HANK models represent a significant rupture from the tradition of DSGE modelling because they explain data that are inconsistent with the current core model with rational microfoundations and expectations. More importantly, they literally abandon a part of what has taken to be a core component in macromodels, viz. intertemporal optimization by the representative agent. Time will tell whether these models modify the DSGE approach so much that the traditional methodological components (i.e. REH and intertemporal maximization as a microfoundation) are no longer required by reviewers in top economics journals.

7. CONCLUSIONS

This chapter has followed an unusual approach in methodology: I have taken an informal notion from the mainstream methodological discussions, the core model, and tried to see whether some of the ways in which economists talk about it could be turned into bona fide methodological arguments. It is obvious that having a core model has some obvious pragmatic benefits for graduate training, central bank decision-making and the general influence of macroeconomics. However, since working with too homogenous a set of models

also carries risks, it is important to see whether having a core model also has genuine epistemic benefits.

I have studied the possibility of such benefits by discussing three ideas: (1) having a benchmark is required in order to solve Duhemian problems. (2) Having a commonly accepted view about the most important causal relationships leads to a methodological consensus, and methodological consensus has benefits. (3) A core model is needed for efficient communication.

These three benefits could all be reaped by any kind of common framework of study. If these arguments are convincing, in principle such benefits could be attained with a non-DSGE framework. I take it that this is why even those who are critical of the DSGE framework do not object to the use of the term 'core model'. I have argued that it is indeed important to have a benchmark model in order to be able to see what depends on what, but that one can just as well use an agent-based model for making pairwise comparisons. I do not have a clear answer to (2) because providing a convincing one would have to use resources from fields on which I claim no particular competence: history of macroeconomics and the part of social epistemology that studies the consequences of diversity in epistemic communities.

However, we have seen that the notion of a core model is often used to refer to some particular features of DSGE models. If the 'core' means a central part of microfounded DSGE models, consisting of an intertemporally optimizing representative agent that has rational expectations, then the three ideas above obtain a new meaning. For example, the first idea becomes the view that we need a DSGE benchmark (with intertemporal optimization and REH) in order to solve the Duhemian problems in macroeconomics. This view is, of course, just the traditional argument for rational microfoundations. The second idea becomes the view that consensus is beneficial, and that consensus in only possible with the current DSGE model. The third becomes the view that efficient communication requires a DSGE model.

One may find that my exercise in mainstream exegesis is futile because nobody has explicitly argued for the benefits of having a core model. But that is precisely the problem. Although the quality of the discussion is generally good, it is somewhat frustrating to read macroeconomists' methodological writings because the reader has to guess what the arguments are arguments for. In particular, we have come across several cases where arguments are presented for a general platform of communication or comparison, but then it turns out that the argument presupposes the correctness of the microfounded DSGE.

Those not working with the current core DSGE model are not convinced that it correctly depicts the true structure of the economy. It seems to me that only those who believe in the correctness of the optimizing microtheory as implemented in the microfounded representative agent model take the argu-

ment for a DSGE benchmark seriously. This leaves the possibility of using the DSGE framework as a benchmark even though one admits that it does not provide a codification of the most important relationships. To put it differently, the microfounded rational expectations benchmark is known to be false whether or not it is supplemented with new Keynesian bells and whistles. If the new benchmark consists of HANK models, the macroeconomists will abandon the requirement of rational microfoundations. What will come in its stead? One possibility is that there is no agreed model-structure but instead a more extensive set of data benchmarks that have to be satisfied.

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NOTES

1. See Colander et al. (2004) for an analysis of how the mainstream typically changes.
2. The IS–LM (investment savings–liquidity preference money supply) framework has persisted in central banks and in economic education (Colander 2004; De Vroey & Hoover 2004) even though it has not been the subject of much research. The empirical version of the New Neoclassical Synthesis model could be taken to be a candidate for an alternative core model. In a three-equation version it has an IS curve; a Phillips curve, and an interest-rate rule which may be reformulated as an LM curve.
3. This interpretation was proposed by Kevin Hoover.
4. The Bank of England seems to be the only major central bank to use a DSGE model exclusively in their deliberation.
5. Thanks to John Davis for bringing this into my attention.
6. I admit that the textual evidence for this interpretation is somewhat scant. Recall, however, the quote from Blanchard above, and see Stiglitz (2018) and Caballero (2010). After having written a draft of this chapter, I asked Blanchard, Vines and Wills how they interpret the notion of a core model. Blanchard responded that a model is a core when it is used as a starting point to which one can add extensions. He appealed to his undergraduate textbook (Blanchard 2019), in which the

IS–LM framework is presented as a core, and the extensions consist of expectations and open-economy considerations.

7. See https://longandvariable.wordpress.com/2013/12/15/why-microfoundations-have-merit/. This quotation also appears in Kuorikoski and Lehtinen (2018).
8. See De Vroey (2012), Duarte (2012), and Frydman and Goldberg (2008) for accounts of Lucas's methodology that note how, according to Lucas, any theory that assumes irrationality is the 'wrong theory'. This expression appears in Lucas (2001).
9. Del Negro and Schorfheide (2013) make the argument but not by appealing to the notion of a 'core'. They speak about the 'coherence' of the model. See also Wren-Lewis (2011).
10. I do not mean to say that this exhausts the empirical methods used in macroeconomics. The theory may also provide priors for a Bayesian estimation or the theory-based parameters may be chosen so as to minimize the distance between the impulse response functions from the model and a VAR representation of the data, and so on.
11. Unfortunately, I can no longer find this version of the paper on the Internet. The reader is asked to trust me that the text is directly copy-pasted from an earlier version of this paper.

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