Methodology and Microfoundations: A New Argument for an Autonomous Macroeconomics

By

Nadia Ruiz
M.A, Kansas University, 2017
M.A, University of Texas at El Paso, 2013
B.A, University of Texas at El Paso, 2011

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Chair: Armin Schulz, PhD

Corey J. Maley, PhD

Sarah Robins, PhD

John Symons, PhD

David Slusky, PhD

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The dissertation committee for Nadia Ruiz certifies that this is the approved version of the following dissertation:

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Chair: Armin Schulz, PhD

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Abstract

Although microeconomics and macroeconomics seem to differ in their object of study—microeconomics studies how economic individual agents make decisions and how those decisions interact, while macroeconomics studies that overall ups and downs in the economy as a whole (Hubbard and O'Brian, 2015) —there is a significant tradition in economics that argues that macroeconomic model building requires microfoundations. The core motivating my dissertation thesis is whether this tradition is right. Specifically, the core questions I address in here are (a) to what extent macroeconomic model requires microfoundations, and (b) what grounds the need for microfoundations (or its absence). I assess these question by focusing on microfoundations *purely* as a methodological practice— i.e., as a method for X scientific purpose, does macroeconomic models need microfoundations?

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Chapter I: Introduction

Although microeconomics and macroeconomics seem to differ in their object of study—microeconomics studies how economic individual agents make decisions and how those decisions interact, while macroeconomics studies the overall ups and downs in the economy as a whole (Hubbard and O'Brian, 2015) —there is a significant tradition in economics that argues that macroeconomic model building requires microfoundations. That is, "macro" entities' behavior like whole economies or governmental institutions need to be shown to be derivable for the choice patterns of individual economic agents, e.g., consumers, firms, or households. The core motivating my dissertation thesis is whether this tradition is right. Specifically, the core questions I address here are (a) to what extent do macroeconomic models require microfoundations, and (b) what grounds the need for microfoundations (or its absence).

Philosophers of economics who argue against the need of microfoundations, recently, focus on the ontological status of macroeconomic and microeconomic entities (Epstein, 2014, 2015; Reiss, 2004; Hoover, 2001, 2006, 2008, 2015). [1] However, as I show throughout this dissertation, these arguments are too heavily focused on ontological concerns. That is, they focus on questions regarding the metaphysical relationship between macroeconomics and microeconomics, e.g., whether macroeconomic facts supervene of microeconomic facts or whether the latter grounds macroeconomics facts. Note that my problem with these types of

[1] The need for microfoundations is related to the Lucas critique, which was made by the economist Robert Lucas. The Lucas critique argues that the relationship between economic variables observe in past data or in macro-econometric models are not reliable for economic policymaking. This is because macroeconomic policymaking needs to consider that people in the economy form rational expectation about future events (Lucas, 1972). Lucas played a pioneer role in developing microeconomic foundations for macroeconomics based on rational expectations. Although my arguments in here are not directed to the Lucas critique and my arguments in here should be consider as a philosophical take concerning the microfoundationalist debate, it is important to point out the role it plays for economists against or in favor of the microfoundationalist approach.

arguments has nothing to do with the value of metaphysics in philosophical debates. However, the issue is that since metaphysical arguments tend to be controversial (and the case in economics is no exception), their conclusions cannot directly be tied to empirical facts, which, ultimately, digress from an important question: "how must macroeconomists build models?" I argue that these types of arguments pay too little attention to a practice-based approach of macroeconomic (scientific) modelling. One of the main purposes in my dissertation is to re-direct the microfoundationalist debate in philosophy of science to a more practice-based approach.

Since economics is, without question, a model-based science, assessing the microfoundations debate from a practice-based approach on scientific modelling is crucial. Although there is much debate surrounding the nature and practice of economic models, there are two features about models that have been accepted which I used throughout my dissertation. First, there are no clear rules or algorithms determining how to construct suitable models (Morgan and Morrison, 1999; Cartwright, 1983, 1999; Weisberg, 2012). Second, models are constrained by their design—e.g., the elements of a model are rule bound in accordance with the purpose the model was built for (Morgan, 2012; Cartwright 1999). Thus, focusing on these features of microfoundations from the perspective of methodological practice— i.e., as a method for X scientific purpose, does macroeconomic models need microfoundations? —reveals a re-conceptualization of the debate.

In *chapter two* I assess Brian Epstein's argument against microfoundations. Epstein argues that the microfoundationalist approach is unconvincing because economists overlook the influence ontological commitments have on scientific practices, specifically, ontological individualism—i.e., for an improvement in economic methodology we must adopt social ontology as the foundation of social sciences. In this chapter, I show that fixing the social ontology prior to the process of model construction is optional instead of necessary. Looking closer at the process of model-building, it becomes clear that when assessing the target-system, it is most often likely that having a fixed ontology would not help the modeler. Furthermore, I argue that metaphysical-ontological commitments are often the *outcome* of model construction, not its starting point. The conclusions of my arguments here go beyond the need for microfoundations in economics, and affect the entire metaphysics-first picture of science.

In *chapter three* I focus on Kevin Hoover's argument for an autonomous macroeconomics. I point out that the latter is only plausible given his metaphysical account for the nature of macroeconomics—i.e., his ontological distinction between natural and synthetic macroeconomic aggregates. I show that his *ontological* dichotomy between macroeconomic aggregates is a measurement problem instead, and that it is therefore not clear that his macroeconomic methodological approach gets off the ground. Also, I shed light on a different approach to macroeconomic modeling (Dan Rodrick's account). Although it might not answer *completely* why microfoundations are not necessary in macroeconomic models, the latter account illustrates best, methodologically, the question of why (and when) to give up microfoundations.

Chapter 4 can be considered a continuation of Rodrik's arguments about economic methodology. However, in this chapter I focus on the accounts that philosophers of science have expanded on Rodrik' claim—"model diversity is an epistemic virtue in economics". First, I provide an argument against Aydionant's account, which states that model diversity (many models targeting different causal factors) secures better economic explanations, and Veit's model pluralism account, which states that: for any scientific goal *z* scientists require multiple models of aspect *x* of phenomenon *y*. I argue that model diversity as a methodological virtue does not entail epistemic completeness, at least not necessarily. Furthermore, I argue that *model diversity* (not to be confused with *model pluralism*) not only improves economics as a discipline but is also a crucial step in justifying the relationship between an economic model and the policy making process. I make the latter clear by using the microfoundationalist approach in macroeconomics as an example.

In *chapter 5,* I conclude with insights gathered from chapters of this dissertation. Specifically, I stress the value of looking closely at economic methodology and economists' practices in the philosophy of economics and in the philosophy of science more generally.

Chapter II: Social Ontology and Model-Building: A Response to Epstein

1. Introduction.

Epstein (2014, 2015) has recently argued that a thoroughly microfoundationalist approach towards economics is unconvincing for metaphysical reasons. He argues that the debate of whether macroeconomic models need microfoundations could be solved if economists fix their ontology—i.e. give up ontological individualism. Specifically, once macroeconomists recognize that macroeconomic phenomena are constituted by more than their individual aggregates, their models will become more compelling, both predictively and explanatorily. Concomitantly, Epstein thus argues that in order to improve the methodology of the social sciences, we must adopt social ontology as the foundation of the social sciences.

From the get-go, it is important to acknowledge the role that Epstein's *The Ant Trap* book has had among philosophers and social scientists. In particular, it is often seen to have re-energized "a long-standing yet stagnant debate about the proper foundations of the social sciences" (Di Iorio and Herfeld 2018) by putting metaphysics at the heart of the social sciences. Acknowledging this is important, as Epstein's book thus connects to the larger, ongoing debate about the role of metaphysics in the sciences (on the role of metaphysics in the sciences see e.g. Lowe 2002; Wilson 2006; Kincaid 2013; for debates specifically on the role of metaphysics in the social sciences see e.g. Searle 2009; Sudgen 2016; Ahmed 2016).

However, it turns out that there are good reasons for thinking that Epstein's social ontology-based account fails to resolve the status of microfoundations in the practice of economic modeling. I argue that fixing the social ontology prior to the process of model construction is optional instead of necessary. Furthermore, I argue that metaphysical-ontological commitments are often the *outcome* of model construction, not its starting point.

In this way, my argument here goes beyond the need for microfoundations in economics and affects the entire metaphysics-first picture of science. Indeed, by focusing on the practice of modeling in economics—though this is also interesting in and of itself—the paper provides a useful inroad into the debate about the role of metaphysics in the natural and social sciences more generally. Looking at concrete examples of how Epstein's framework could—or could not—actually be used in practice makes for a new and useful way to appreciate a number of the issues with this framework.

The paper is structured as follows. In section 2, I explain how I understand "microfoundations." In section 3, I lay out Epstein's two most relevant arguments. In section 4, I explain that prioritizing social ontology seems optional instead of necessary when it comes to model construction. In section 5, I conclude.

2. Microfoundations

Although microeconomics and macroeconomics differ in their object of study—microeconomics studies how people make decisions and how those decisions interact, while macroeconomics studies the overall ups and downs in the economy as a whole (Hubbard and O'Brian 2015)—there is a tradition in economics that argues that macroeconomic model building requires microfoundations. The idea is that economic models, in order to be compelling, need to derive all of their conclusions from the choice patterns of individual agents (Frydman and Phelps

2013): consumers, households, firms, or governmental bodies (Gindis 2009; Schulz 2016).[2] For this reason, microeconomic equilibrium theory is taken to make for the overarching theoretical framework in economics: its basic principles are seen to give us the mechanisms and major causal factors with which economics is concerned. These principles include: (1) the theory of consumer choice, which comprises three postulates—rationality, consumerism, and diminishing marginal rates of substitution; (2) the theory of firm, which also comprises three postulates—diminishing returns, constant returns to scale, and profit maximization, and (3) the theory that markets tend towards equilibrium (Hausman, 1992).[3] The commitment to microeconomic equilibrium theory as the core of economics then entails that generalizations about choice or other economic phenomena are *ad hoc* and should be avoided if they are not derivable from microeconomic equilibrium theory.[4]

For example, a model that simply assumed that an economy has a (collective) savings rate of 30%—as is done in some classic macroeconomic models, such as the Solow Growth Model (see e.g. Jones 2002)—would be considered *ad hoc* because an analysis of the consumption choices of individuals in terms of consumer theory is absent (Hausman 1992). Instead, this savings rate should be derived by explicitly considering the intertemporal consumption decisions of individual consumers (see e.g. Romer 1990). So, we might assume that individual consumers

[2] Note that whether something is seen as microeconomic entity in and of itself does not depend on its size or the number of members that constituted them. Rather, what matters is that they are taken to be individual agents taking decisions, which relationships are focal for economics.

[3] While this need not be the only way of spelling out these principles, it is sufficient for present purposes.

[4] Note that defendants of microfoundations need not be committed to a replacement of macro-explanations by micro-explanations. This is because, one, the relationship between economic models and economic explanations is not so straightforward (e.g. read Hausman 1992; Morgan 2012; Morgan and Morrison 1999). Two, there are, also, many different views about the nature of scientific explanation (e.g. Salmon 1984; Hempel 1965; Khalifa 2012; Potochnik 2015; Pérez-González 2020). Thus, it is possible that some economists who advocate for mechanistic explanations would find compelling to explain a macroeconomic phenomenon by its causal-mechanical macro-relations.

have a utility function of this form: $max_C \int_{t=0}^{\infty} \frac{C^{1-\sigma}-1}{1-\sigma} e^{-\rho t} dt$ [5] this is a utility function that expresses the extent to which individuals preferer to consume more rather than less, and the extent to which they prefer to consume sooner rather than later—i.e. this function, for the most part, allows an analysis of individuals' spending and saving behavior. From this, we further assume that these consumers then maximize this function, which yields an individual savings rate. Then, interestingly, we can argue that the consumers in the economy are all similar in these ways so that the national savings rate would be equal to this individual savings rate. (Alternatively, we can allow these consumers to differ in some ways and take the national savings rate to be the average of their individual savings rates). The key point is that the national savings rate is derived from that of the individual consumers. In other words, this shows how some macrovariables (e.g. national savings rate) can be derived from a set of microvariables (e.g. individual's intertemporal utility function) in order to give microfoundations to the national savings rates.

It is furthermore worth noting here that the commitment to microfoundations in economics shares a number of similarities with a commitment to individualism in the social sciences more generally (for more on the latter, see e.g. Lukes 1970). Indeed, a commitment to either explanatory individualism (EI)—which says that social phenomena are best or only explicable by appeal to individuals' behavior, actions and/or interactions—or ontological individualism (OI)—which says that there is nothing more to social phenomena above and beyond facts about individual people—seems to be the main reasons why many economist advocate for the need of microfoundations (see also Hoover 2001).

[5] Where C is the amount of output consumed, σ is the extent to which consumption utility is decreasing, and ρ is the rate of intertemporal substitution.

Now, according to Epstein (2015) it is especially OI that is crucial to the commitment to microfoundations. He argues that since "commitments about the nature of the entities in science––how they are composed, the entities on which they ontologically depend—are woven into the models of science" (Epstein 2015, 41), the microfoundationalist "explanatory strategy carries with it a commitment to a particular ontology of the social world" (Epstein, 2015, 46). On the flipside, this means that, according to Epstein "ontological mistakes lead to scientific mistakes" (Epstein, 2015, 41). In this context, therefore, doubts about OI translate directly into doubts about the plausibility of the commitment to microfoundations. Epstein further argues that there indeed *are* reasons to have doubts about the truth OI. The next section makes this clearer.

3. Epstein's Arguments

Epstein thinks that there are several reasons to revise the individualist social ontology common in economics, i.e. to give up OI. In the first place, macroeconomic facts do not supervene on microeconomic facts, as we frequently encounter changes in the macroeconomic domain without changes in the microeconomic domain. Epstein points to the following example to illustrate this:

A. The mob ran down Howe Street.

B. Bob, Jane, Tim, Joe, Linda, … and Max ran down Howe Street.

If *A* supervened on *B*, it should not be possible to change *A* without changing *B*. As a matter of fact, though, one can think of changes in *A* without changes in *B*. For example, maybe Bob, Jane, Tim, Joe, Linda … and Max ran down Howe Street because there was a free Radiohead concert. There is no mob running down Howe Street, nonetheless, Bob, Jane, Tim, Joe, Linda, … and Max ran down Howe Street: a mob seems to not just depend on a group of people running and gathering in a same location, but on something else as well. Cases like this show that just because social-level entities are constituted (made) of individuals it does not entail all social entities and/or phenomena must be understood and studied in terms of the individual properties that constituted them. OI in social science (in general not just in economics) must be reconsidered (Epstein 2015).[6]

Because of the existence of examples like the above, Epstein argues that we cannot trust ontological individualism. Instead, he thinks that we need to "engage in a more careful metaphysics", which "is best done from scratch" (Epstein, 2015, 49). Epstein's social ontology consists of a metaphysical toolkit in which grounding and anchoring relations determine the nature of social facts—i.e. a social ontology in which social-level phenomena are not reducible and not fully determined by the individual parts that constituted them.

Grounding is a relation in which the most fundamental fact—lower-level set of facts— is the metaphysical reason for why that set of higher-level facts is the case, i.e. grounding relations state the building blocks of social facts. Fact *A* (Bob, Jane, Tim, Joe, Linda… and Max ran down Howe Street) grounds fact *B* (the mob ran down Howe Street). Saying that fact *A* grounds fact *B* means that fact *B* depends on fact *A*, which, also means that fact *A* "metaphysically makes" fact *B* the case. Note that this is not a causal relation, it is not that fact *A* caused fact *B*. Instead, fact *A*

[6] Epstein (2014) also argues that even if supervenience were to hold, advocates of microfoundations need to make clear that the microeconomic properties on which macroeconomic properties supervene are in fact ontologically basic. The problem is that microeconomics not only focus on individuals' choice patterns, but also, households, firms, and governments, which look like macroentities themselves. It is not clear what counts as individualistic in economics (Epstein 2014). This argument is not so central here because I take it to be an argument concerning the nature of microeconomics. If Epstein is right here, defenders of microfoundations would just need to change the microfoundational base they are relying on—not give up on the microfoundationalist project altogether.

is the reason why *B* is the case (e.g. the fact "I am not married" grounds—makes the case— the fact "I am a bachelorette").

Beyond this, social facts' building blocks need a reason for *why* they are these building blocks. For example, the reason why fact *B*—a piece of paper— counts as a United States Dollar (USD) is because of society's collective acceptance of a constitutive rule that "being printed by the BEP grounds what is being a USD." [7] This frame principle anchors the existence of what grounds being a USD. In other words, for a set of facts *A* to anchor a frame principle *A'* is to say that those facts are the metaphysical reason of why that frame principle is the case (Epstein 2015).

Epstein therefore thinks that it is by fixing the grounding conditions and the rules (i.e. the anchoring frame principles) that set up the grounding conditions of a social fact that we get the building blocks for modeling in social sciences (Epstein 2015). For example, "if we are interested in modeling financial markets, we may just want to take the set of financial kinds fixed, anchored as they are, and see how changes in the world affect facts about them" (Epstein, 2015, 128). That is, prior to building a model an economist must first establish what fixes the grounding conditions of this new entity. To do this, she needs to look at the relevant constitutive rules (the anchoring frame principles), such as contracts and practices of financial trades. Doing this will help her understand what the thing she is modeling really *is*. In turn, this will ensure she is building the correct models for it. However, it is precisely this last set of inferences that I will question in the rest of this paper.

4. Model-Building in Practice: A Response to Epstein

[7] Epstein bases this somewhat on Searle (1995).

In this section I will use Weisberg's target-directed modeling account (2013) in order to assess to what extent a fixed social ontology is necessary *prior* to model building. After laying out the outlines of the account, I show that although having a fix on the right ontology seems at first beneficial/necessary for at least one of the elements of ("target-directed") modeling, deeper attention of this element illustrates that issues are not so straightforward. Next, I argue that metaphysical / social ontological conclusions are generally anyway better seen as the outcome of model-building, not its starting point.[8]

4.1. Weisberg's Account of Target-Directed Modeling

According to Weisberg's widely accepted account, scientific modeling is about constructing a model of a specific *target-system*. Weisberg understands a target-system as a "single real system" which is an abstraction of a phenomenon in the world. Modelers decide which aspects of the phenomenon they consider relevant: they focus on some of the phenomenon's static and dynamic properties while abstracting away from other ones (Weisberg 2013). Target-directed modeling is thus not about constructing models about "real-world" phenomena *per se* but about constructing models of a target-system.[9]

Target-directed modeling involves three distinct elements: the development of a model, the analysis of the model, and the targeting of the model to a real-world system. Note that although Weisberg describes these as three conceptually distinct processes, he acknowledges that in practice they might happen together. I discuss this more in detail in section 4.3.

[8] Indeed, related arguments against and for Epstein's approach to the social sciences have been made in the literature (see e.g. Lauer 2017; Schaffer (forthcoming); Lohse 2017).
[9] Target-systems' nature has been crucial for the arguments regarding the model-world relation (read Suárez 2003; Frigg 2009; Weisberg 2013), but only few have acknowledged the importance of giving an account of the relation between a target-system and the process of generating a model. Elliot-Graves (2014, 2020), has an account of this process; however, although her account helps us understand this process, it fails to describe cases in which scientists choose a target-system because of mathematical tractability purposes. This will be crucial in what follows below.

Model development is an active process in which, one, scientists either construct or borrow a structure—mathematical descriptions, equations, or graphs—to represent a target-system. Two, they adjust the structure's features so as to best represent the target-system's properties of interest. For example, differential equations are often used to represent how economic variables change over time and are fine-tuned according to the system in question. Three, scientists develop a construal: they formulate their intentions about how their model structure should be interpreted. Construals consist of: 1) the model's scope—the target-system's features that are intended to be represented in the model; 2) an assignment—the specification regarding how the target-system's properties are to be mapped onto the model;[10] 3) two kinds of fidelity criteria—the dynamical fidelity, which specifies how close the model's predictions must be in relation with the real-world phenomenon; and the representational fidelity, which specifies how close the model's internal structure must match to the real-world phenomenon's causal structure (Weisberg 2013).

The *analysis of the model* depends on the modeler's goals with respect to the model, but generally consists in developing a representation of the static and dynamic properties of the model, allowable states of the model, transitions between states, what initiates transitions between states, and how states and transitions depend on one another (Weisberg 2013).[11] Scientists have access to these by analyzing the mathematical structures and/or computer simulations outcomes. What is central in this element is analyzing and understanding the behavior of the model *as it has been specified*.

[10] For example, in economics, an equilibrium model could be used either as a supply and demand model for price determinations or as a labor market model for the determination of wages.
[11] Note also that although there are some general characteristics of target-directed modeling analysis, the analysis can take many different forms depending on the type of model or pragmatic factors.

Finally, *model-target comparison* consists of theorists actually comparing a model with the target-system.[12] Given how the target-system was defined, scientists see how their model fits that target. The fidelity criteria—dynamical and representational—will be used to specify more precisely which properties of the target the model must fit and to what degree they must fit them.

For example, assume an economist is interested in modeling the behavior of a consumer *A* making a choice between two bundles of commodities *x* and *y*. The modeler would begin by constructing or simply choosing a mathematical structure—for example, the idea that A's preferences over x and y are described by the utility function $U = x + y$ and that A faces a budget constraint of $I = px * x + py * y$ (Hausman 1992). After the structure has been set up, the modeler then engages in analyzing the mathematics of the model: for example, they might find the maximum of U, subject to the budget constraint. Finally, the modeler looks at empirical data to compare its model and conclude whether her model meets her fidelity criteria. If not, they might adjust the utility function or budget constraint in some way.

4.2 Metaphysics and Model Development

Given the above, it may seem obvious that fixing the metaphysics first might not be very useful for the second and third element of target-directed modeling. Determining the appropriate metaphysics first is not needed in the *analysis of the model*, because in this element, the main focus is on carrying out the type of analysis a scientist has in mind for her study. The target system is already specified; the heart of this element is just developing an understanding of the static and dynamical states of the model. Similarly, in the *model-target comparison*, fixing the metaphysics first is not needed because scientists' interest here is just to measure the degree of fit

[12] Note that not all modeling engages in model-target comparison. For instance, biologists sometimes engage in hypothetical modeling which is the practice of modeling nonexistent targets—e.g. exponential growth models (Weisberg 2013). This is not so central in what follows, though.

of the model to the target system, given the models' fidelity criteria. (Given the fact that target-directed modeling is an iterative process, there are a few further complexities to note here, though; section 4.3 will return to this.)

However, fixing the metaphysics first might seem crucial for the *development of the model*. One can argue that having the correct ontology is necessary for the development of the model because it is only by having an accurate picture of the phenomenon in question that scientists can choose a target-system correctly and accurately borrow / construct a mathematical structure. After all, choosing the metaphysically correct target-system seems to ensure that the model's representational capacity is strong: the scientist will then be modeling the relevant phenomenon in an accurate way. While target-directed modeling does not involve modeling real-world phenomena per se, it at least concerns *aspects* of real-world phenomena (Bailer-Jones 2003; Giere 2004; Contessa 2007). Thus, working out the metaphysics (social ontology) first may seem crucial for the development of the model because it ensures scientists model the phenomena as they really are. Contrary to this, model-building using the wrong (social) ontology entails inaccurate model representations: they would model the phenomena in inaccurate ways.

However, the issues here are more complex than this lets on. In the main, this is because there are many reasons why modelers choose target-systems; accurate representation of parts of the world is just one of them. On the one hand, Knuuttila argues that, on many occasions, scientists "learn from the construction and manipulation of models quite apart from any determinate representational ties to specific real-world systems they might have" (2011, 14). In other words, the epistemic value of some models is not in their being able to represent real-world systems, but in facilitating the study of certain more general scientific phenomena. Often scientists avoid overly complex models—even if highly accurate—in favor of simpler models that are highly idealized and abstracted. This is because these simpler models can facilitate the study of certain phenomena: the simpler models give us a better *understanding* of certain aspects of the world (Elgin 2011). In these cases, modelers do not need to fix the metaphysis first, as accurately representing real-world phenomena is not the goal here.

On the other hand, target-systems often are constructed specifically in in certain ways because in these ways they become mathematical tractable (see e.g. Alexandrova 2006; Batterman 2009). This comes out clearly by focusing on the practice of idealizations. In many cases, modelers use idealizations to fix the target-systems' features so as to conform with mathematical structures. In other words, it is not that modelers find/borrow a mathematical structure that best fits the target-system, but instead the target-systems' features are chosen *just* because these are the ones that are mathematical tractable.[13]

Now, it is true that, sometimes, modelers rely on idealizations that merely distort or simplify reality in harmless ways; these have become known as Galilean idealizations (McMullin 1985; Cartwright, 2007). It is also true that these types of idealization seem to fit quite well to an account like Epstein's: for Galilean idealizations, some grasp of the target-system's basic ontology is required. Scientists at least need to know the constituents and central features of the phenomenon at interest so as to distort and simplify it in their model. However, the key point to note here is that not all types of idealizations used in economics are Galilean idealizations.

William Stanley Jevons's work in modeling economic behavior (Jevons 1871) is a good example of the appeal to idealizations because of considerations of mathematical tractability.[14]

[13] Alexandrova (2006) also argues that many assumptions are introduced into economic models just so as to facilitate mathematical derivations (see also Knuuttila and Morgan 2019, Cartwright 1999, 1999b).
[14] In here I follow Morgan's (2006).

Jevons's "economic man" is a model-idealized human that gains enjoyment/pleasure from consumption of goods. His model-building process seems to be the following. First, he decided to use Jeremy Bentham's account of utility—i.e. a psychologically-based account of utility. Then, he decided that from the seven dimensions of Bentham's account only intensity, duration, certainty/uncertainty, and propinquity/remoteness were relevant for the problems economics attempts to solve. Jevons next reduced these four into two dimensions of feeling, i.e. duration/intensity of pleasure and duration/intensity of pain. It seems that he reduced these four to two *so as to* be able to diagrammatically represent the dimensions of pleasure in a two-dimensional space. Jevons's model then showed how humans gain pleasure (utility) from consuming goods and how that pleasure (utility) declines with more units of the same good consumed.

For the purpose of my argument, Jevons's model-building process—from shaping/designing a target-system and choosing/developing the mathematical structure—shows that instead of choosing a mathematical structure that best represents the target-system's properties, Jevons seemed to choose the target-system's properties *because of* the availability of a compelling and easy to use mathematical structure. So, Jevons chose Bentham's utility account instead of Mill's—which sees human behavior as mainly motivated by a desire for wealth, accompanied by the two negative motivations of dislike of work and love of luxury—only because he thought of Bentham's as a better choice to be formalized mathematically. "[T]he mathematical forms are imposed for convenience of representation and its subsequent usage, rather than because mathematics is the form in which economic man's behavior is best and most accurately represented" (Morgan 2006, 13). In other words, Jevons reduced Bentham's utility account to two features, not for any substantive reason, but just because of the demands of the mathematical structures he was working with.

It might here be objected that, although Jevons chose to reduce Bentham's utility account to two dimensions for mathematical tractability reasons, he actually did think of pleasure and pain as two of the essential features of economic behavior. That is, it may be thought that he not only chose Bentham's account because it was easier to model, but also because the latter is the most compelling ontological account of human (psychological) behavior. Thus, this shows the benefits of working out the metaphysics *prior* to model building after all.

However, even if that were so, it remains true that Jevons saw these features as constrained by the relevant mathematical structure. Put differently: we may grant that Jevons did use Bentham's psychological account of utility partly because of his ontological commitments. However, it is still the case that Jevons *also* chose to focus on duration/intensity of pleasure and duration/intensity of pain *because these were easier to model* (i.e. more mathematically tractable) than propinquity and certainty. Indeed, he may well have thought the other two features of utility (propinquity and certainty) are metaphysically *more* important than the ones he in fact focused on. Still, for pragmatic reasons, he chose duration/intensity of pleasure and duration/intensity of pain. This shows that what is mathematically tractable is just as important for makes for a good model as what matches our ontological commitments.[15]

In short: although it may seem that fixing the metaphysics first is important in the development of the model, as I have shown, this is not always the case. There are other things scientists consider in the process of model development. On the one hand, a model's

[15] Similarly, Alexandrova's derivation facilitators assumptions in economic models that are introduced to facilitate mathematical derivations (Alexandrova 2006).

representational capacity is not the only thing that gives it epistemic value. On the other, idealizations in models sometimes have nothing to do with ontological commitments.[16]

4.3 The Active Process of Model-Building

Although Weisberg's target-directed modeling describes the three elements as conceptually distinct processes, he states that in practice they might happen simultaneously. That is, he notes that the actual practice of model construction is more like a *trial and error* process (Weisberg 2013). Also, he describes this process to be an active process in which modelers try different things until they finish the construction of their model. For example, they might start with a loose idea of the target-system, then choose a mathematical structure, then figure out that the chosen structure does not have the features necessary to represent the target-system. They might then change the target system, or the structure used to accommodate the target, or their construals (maybe the fidelity criteria need to be changed) (Weisberg 2013).

Acknowledging this is important, because it shows that fixing the metaphysics is not what scientists need to do first, as argued by Epstein. There is no book of rules or an algorithm that determines what the model-building process is for making a suitable representation of the real target-system (see Cartwright 1983; Boumans 1999; Morgan and Morrison 1999; Morgan 2012). The target-system can be the *result of* the process of building the model. It is during the process of constructing the model that scientists figure out which sets of features define fruitful research targets and which do not (Weisberg 2007, 2013; Elliot-Graves 2012). The same can be said

[16] Also, there seems to be a problem with what is thought about de-idealizing. De-idealization is not a simple process of removing and adding back features, there is more going on when a modeler decides to add back detail or take away the distortions (read Knuuttila and Morgan 2019; also, Hausman 1990).

about idealizations. Coming up with idealizations is an active process that moves between the choice of target-systems' features and structures' features.

For an example of this, consider Ricardo's model of farm production (see Morgan 2012). Ricardo started his project with two main questions: what is the nature of rent, and what problems are caused by population growth? Ricardo seemed to have had an idea about how some elements of these phenomena *might* behave, and then started by "giving form to them and making them rule bound" (Morgan 2012, 74). The key thing here is that, although Ricardo *acquired* a better idea regarding the nature of rent by focusing on some set of fixed classical economic principles, he could not have got to that understanding without first trying out different arrangements for the information he had at hand (economic principles plus his own new intuitions) (Morgan 2012). Put differently, Ricardo's findings are the result of his model-building process and were not the starting point of it. Ricardo did not follow a fixed set of instructions for model-building. Most importantly, Ricardo's own surprise with his findings comes out clearly from the following quote: "this is a view of accumulation which is exceedingly curious, and has, I believe, never been noticed" (Ricardo, 1815, 16). The key point here then is that Ricardo's model-building process did not start with Ricardo fixing the nature of rent—i.e. working out and specifying a fixed set of features describing what sort of relation rent entails— and then made a model of this; rather, he played around with some assumptions and ended up with an understanding of the nature of rent that was not available before his model's outcomes. Thus, this shows that the model-building process is an active process that looks more like a trial and error process. Also, it shows that is not just that scientific modeling is surprising and elucidating but that our metaphysical /social ontological discussions are the result, precisely, of modeling practice.

5. Conclusion.

I have argued that Epstein's metaphysics-first approach surrounding his argument against the commitment to ontological individualism in the social sciences—and that of microfoundations in economics–fails to resolve the debate. Specifically, I have shown that addressing metaphysical questions first is neither necessary nor useful for the process of model-building. I have also shown that a better understanding of the relevant metaphysical issues is often the *outcome* of model-building, not its starting point. For these reasons I take that the question about microfoundations cannot be answered by accounts such as Epstein's. Instead, it is an implication of this paper that the question of whether macroeconomic models should be built on the basis of individual agents' preferences / choices or not is *methodological* in nature—i.e. instead of asking whether microfoundations are metaphysically compelling, we should ask whether they make for good modeling practice in macroeconomics[17]— and should be addressed as such.

[17] For a further reading on this reconceptualization of the microfoundationalist debate read Ruiz and Schulz (forthcoming).

Chapter III: More Methodology, Less Metaphysics: A Response to Hoover's Argument Against Microfoundations

1. Introduction

In several publications, Kevin Hoover argues against the eliminativist microfoundationalist approach towards macroeconomics, which advocates for the euthanasia of macroeconomics, i.e., since macroeconomic models must show to be derivable from the choice pattern of individual economic agents, microeconomic theory is enough for the study of economic aggregates (Hoover, 2001, 2008, 2009, 2010, 2015).[18] Hoover's arguments, throughout, emphasize that is economists' metaphysical misconception concerning methodological individualism, which, ultimately, leads them to a misguided macroeconomics methodology. In turn, Hoover (2001) lays a *methodologically* autonomous macroeconomics account—structural causal analysis—however, the latter is only plausible if one accepts and/or commits with Hoover's metaphysical account for the nature of macroeconomics, i.e., specifically on the view that macroeconomic aggregates supervene on microeconomic properties, but they are not reducible to the later. [19]

Although Hoover's recent arguments against eliminative microfoundations approach focus more on the representative agent, in here I solely focus on Hoover's set of arguments for his structural causal analysis account in macroeconomics. Specifically, in this paper, first, I argue

[18] Not only Hoover (2001, 2008, 2009, 2010, 2015) and Epstein (2014, 2015) have arguments that questioned the reliability of microfoundations. Wren-Lewis (2007, 2018), also, questions the epistemic trade-offs between the value of holding ontologically microeconomic agents as fundamental in economics (macroeconomic modelling must be consistent with microeconomic theory) and empirical data (consistency among the model findings and data).
[19] Although Hoover most recent work does not emphasize on ontological distinction among macroeconomic aggregates, his arguments rely on the metaphysics behind macroeconomics—e.g., DSGE representative agent's accuracy and precision (Hoover, 2015, 2021). However, I focus on Hoover's ontological distinction among macroeconomic aggregates to illustrate that macroeconomic methodology is better off without any strong metaphysical commitments.

that Hoover's *ontological* dichotomy between macroeconomic aggregates is not compelling and that it is therefore not clear that his macroeconomic methodological approach gets off the ground. After lying out what goes wrong with Hoover's account, I shed light to a different approach—Rodrick's 'model choice' account— to macroeconomic modeling, which although it might not answer *completely* why microfoundations are not necessary in macroeconomic models, I argue, the latter account illustrates best, methodologically, the question to why (and when) to give up microfoundations or not.

The structure of the paper is as follows. In section 2, I explain what I mean by microfoundations, and discuss the ontological commitment that motivates these. In section 3, I present Hoover's macroeconomic aggregates ontological difference argument. In section 4, I draw on Rodrik's model choice selection account as a methodological argument for the absence (or need) for microfoundations. In section 5, I conclude.

2. Microfoundations

The theoretical idea behind microfoundations is that microeconomic equilibrium theory shapes the theoretical enterprise in economics because its basic principles give us the mechanisms and predominating causal factors with which economics is concerned.[20] Hausman (1992) sketches these principles as follows: (1) the theory of consumer choice, which comprises three postulates—rationality, consumerism, and diminishing marginal rates of substitution; (2) the theory of firm, which also comprises three postulates—diminishing returns, constant returns

[20] As in physics, biology, neuroscience and general social sciences the microfoundationalist approach in economics sheds light to a related and much discussed reductionists debates—i.e., whether higher level phenomena can be reduced (theoretically, explanatory, ontologically) to fundamental lower-level entities (Fazekas, 2009; Bechtel, 2007; Wimsatt, 2007; Kincaid, 2015; Nelson, 1984).

to scale, and profit maximization, and (3) the theory that markets tend towards equilibrium.[21] In other words, economic models need to derive all of their conclusions from the individual economic agents' choice behavior. Note that in this context economic agents such as, institutions, firms, and households, for example, can be taken to be individual economic agents with choice behavior too (Gindis, 2009; Schulz, 2020).

Commitment to microeconomic equilibrium theory as the core of economics entails that generalizations about choice or other economic phenomena are *ad hoc* and should be avoided if they are not derivable from equilibrium theory. For example, Keynes (1936) stated that the marginal propensity to consume is less than one is often regarded as *ad hoc* because an analysis of the consumption choices of individuals in terms of consumer theory is absent (Hausman, 1992). Contemporary work in macroeconomics has been devoted in showing how to provide microfoundations for macroeconomic models.

Note that the commitment to microfoundations in economics is similar with the commitment to methodological individualism in the social sciences more generally (for more on the latter, see e.g., Lukes 1970; and Kincaid, 2015). Methodological individualism consists of two theses, (i), a commitment to explanatory individualism which states that phenomena are best or only explicable by appeal to individuals' behavior, actions and/or interactions—(ii), ontological individualism which states that there is nothing more to social phenomena above and beyond facts about individual people. Although it seems that a combination of these two theses evoke by methodological individualism are economists' motivation for the need of microfoundations,

[21] While this need not be the only way of spelling out these principles, it is sufficient for present purposes since this gives the non-economist reader a feasible idea of the microfoundations motivation and its evolution—e.g., representative agent and endogenizing variables.

microfoundationalist arguments (for and/or against) have focused more generally on ontological individualism[22] (see Epstein, 2014, 2015; Lohse, 2017; Lauer, 2017). Hoover (2009) explains that this is because microfoundationalists' motivation rests on the idea that for a compelling empirical science the objects of study must not turn out to be *"free-floating Platonic forms"* or *"Hegelian world spirits".* In other words, microfoundations allegedly give us a comprehensible account about the connection between individuals and macroeconomic aggregates, granting the latter as a compelling empirical science. In this paper, precisely, I want to illustrate that metaphysical accounts—as supervenience or emergence—cannot be taken at face value when incorporate them into the scientific domain's methodology.

3. Hoover's Macroeconomics Without Microfoundations

Hoover argues that commitments to ontological individualism in macroeconomics is not accurate, and that is why a compelling methodological account of non-microfounded macroeconomics might be possible. Hoover (2001) provides an approach to a causal analysis in macroeconomics, which, he argues, separates macroeconomics and microeconomics methodologically. However, as I show later in the paper, since his methodological account relies on realist accounts of causation—"structural causal analysis presupposes a realist ontology for causality" (Hover, 2001, p. 109) —and macroeconomic entities, Hoover's methodological account falls short.

[22] Note that the defendants of microfoundations need not be committed to a replacement of macro-explanations by micro-explanations—e.g., Hoover (2009) has a similar claim to the following explanation I will give. This is because, one, the relationship between economic models and economic explanations is not so straightforward (e.g., read Hausman, 1992; Morgan 2012). Two, since there are many different views about the nature of scientific explanation, it is possible to find compelling to explain macroeconomic phenomena by focusing mainly on macroeconomic relations (e.g., Salmon, 1984; Hempel, 1965; Khalifa, 2012, Potochnik, 2015).

Hoover presupposes a realist account of causality —i.e., "causes are real properties of variables in structures" (Hoover, 2001, p. 109). He describes a causal structure as "a network of counterfactual relations that maps out the underlying mechanisms through which one thing is used to control or manipulated another" (Hoover, 2001, p. 24). This ability to control and manipulate economic mechanisms, according to Hoover, is especially valuable for the role of economists as policy advisors (Hoover, 2001). In other words, Hoover takes causes to be real properties of variables in a structure (e.g., money flows alter expenditure, prices rise in the face of rising expenditure).

A critique to Hoover's causal structure account is beyond the purposes of this paper. What matters here is just that Hoover's methodological account of macroeconomic entities depends on whether causal powers are accepted as real features of macroeconomic entities. This is important because, if macroeconomic entities turn out to be just the sum of their microeconomic parts, then macroeconomic entities' causal efficacy is determined by these (microeconomic) parts; and, thus, eleminitivist microfoundations holds. On the other hand, if macroeconomic entities' causal powers differ from microeconomic entities' causal powers, then "one of the central rationales for the program of microfoundations for macroeconomics would be eliminated" (Hoover, 2001, p. 109). That is, macroeconomics is not fully determined by its microeconomic parts. Thus, microfoundations cannot be derive in such cases.

For this to be plausible Hoover must show that macroeconomic entities are not merely aggregations of microvariables. In other words, Hoover must avoid the overdetermination problem (see Kim, 2005). So, for macroeconomic entities to have causal powers on their own, macroeconomic entities should meet the realist account of economic entities. Whatever economic entities there are exist externally—i.e., independently of any human mind—and objectively—i.e., un-constituted by representations of any theory.[23] Since the microfoundationalist approach denies that macroeconomic entities exist objectively because these exist in reference to the relations implied by microeconomic theory, Hoover's first step is to show that the existence of macroeconomic entities do not depend fully in microeconomic theory. (I will discuss this more in depth in section 4). The causal structural analysis account depends on the success of this.

Hoover draws an ontological distinction among macroeconomic aggregates—e.g., *natural aggregates* from *synthetic aggregates*. *Natural aggregates* are just simple sums or averages, which are measured in the same units as the individual components that constituted them, i.e., they share the same dimensionality. For example, the *national interest rate* is calculated on a simple average of rates paid by all insured depository institutions for which data is available (the resulting aggregate's dimensionality in this case is %, which is the same dimensionality of all the constituents available).

By contrast, *synthetic aggregates* are fabricated out of components, whose structure is altered (Hoover, 2001, p. 113). Synthetic aggregates' components do not share the same dimensionality with the other components because the components need to be added / averaged to estimate the aggregate, components need some adjustments to their dimensionality—i.e., what Hoover refers as "components' structure gets altered".[24] The natural rate of unemployment (NRU) is an example of a synthetic aggregate. It goes beyond counting (and then averaging) the

[23] Hoover used Maki's (1996) discussion of realism in economics, specifically, his distinction between semantic realism and ontological realism in economics.
[24] Hoover, elsewhere, pointed out to me that a better description of synthetic aggregates is that these are not quantities that are dimensionally distinct but quantities that cannot legitimately be understood as derivable from the microconfiguration of the economy without adding an independent macro premise to the derivation. This, I think, emphasize best my argument in 3.1,

number of people currently not in paid labor. NRU determinants include facts about the expected future of the economy, changes in labor force characteristics, technological progress, changes in labor market institutions, actual wage settings, and changes in government policies (Krugman and Wells, 2009). That is, there is not direct relationship with its components, thus, giving microfoundations to all of NRU's macrovariables might not be plausible.

According to Hoover, the above ontological distinction among aggregates exposes two things. One, since synthetic aggregates composition goes beyond just adding all the components together, there is no straight relationship between the aggregate and its components. Two, consequently, the latter shows that giving microfoundations to synthetic aggregates will be inadequate. [25] But is this enough to solve the question about microfoundations? I think it is not enough.

3.1 Are There Two Types of Aggregates?

Hoover's distinction presents problems that make his methodological account fall short. Before I develop on this issue, it is important to note that Hoover's ontological classification allows economists to derive microfoundations to natural aggregates. This is because natural aggregates do seem to bear a relationship with its individual components, so deriving microfoundations its plausible. Additionally, the latter entails that deriving microfoundations to natural aggregates might have some epistemic justification. Economists that value theoretical

[25] It goes beyond the scope of this paper to give a separate argument to Hoover's second step, in which he aimed to account proof for the fundamentality of synthetic aggregates. Hoover uses Hacking's manipulability criterion, which states that an entity exits if there is a procedure used to manipulated parts outside the domain of the theory this entity is embedded in and such procedures such entity is used as an instrument—e.g., "real rate interests and the yield curve are synthetic aggregates, and both are entities with causal powers in some economics theories" (Hoover, 2001, p. 123). Although this step depends on Hoover's ontological distinction, it can also be applied whether macroeconomic aggregates are taken as fundamental or not. If they are the focal variable in a model, they will be able to manipulate other variables, hence, passing the Hackings manipulability criterion.

coherence—i.e., macroeconomic relationships are consistent with microeconomic theory axioms such as microeconomic equilibrium theory[26]—have enough reasons to derive microfoundations to natural aggregates.

Whether microfoundations are available to one type of aggregate and not to the other is not where my concern is. My main concern is to show that Hoover's distinction is not clear. Hoover claims that since synthetic aggregates are fabricated of components whose structure gets altered, this makes it implausible to derive microfoundations (Hoover, 2001). Ultimately, this is what makes synthetic and natural aggregates ontologically distinct. But a closer look shows that there are reasons why the natural / synthetic distinction is unlikely to make for an ontological distinction. Indeed, this distinction speaks more to differences in the ease with which a macroeconomic variable can be *measured*, and not to an ontological difference among aggregates.

To see this, begin by considering the Natural Rate of *Unemployment*. NRU is said to be the variable / point that describes the level of unemployment that would exist when the labor market is said to be in equilibrium. However, to model and measure NRU, we need to estimate when the labor market is in equilibrium: we would need to determine the wage / price level that balances supply and demand for labor. Unsurprisingly, doing that is difficult (McAdam & Morrow, 1999; Reiss, 2001; Stockhammer, 2004). However, this is not a point about the ontological constitution of the unemployment rate. It has to do more with economists' purposes and abilities: they need to find a model or measurement procedure that allows them to get to an estimate of the unemployment rate that matches what they want to use it *for*. Therefore, in some cases,

[26] For a further discussion on internal and external consistency read Wren-Lewis (2007, 2011, 2018).

measuring unemployment by adding the number of working age people failing to get a job might well be enough, while for others it might be more appealing to use a measurement procedure as the one engaged with NRU. In short: the unemployment rate could be treated as either a natural or synthetic aggregate.

More generally, it is not clear that there is one specific method for aggregating micro-variables: in different circumstances, this is best done by simple sums and averages, linear functions, more complicated differential equations, etc. So, it is not clear that Hoover's distinction makes an ontological distinction among aggregates. In particular, the purpose of converting one thing into another form is often instrumental (regarding Hoover's claims about sharing dimensionality among variables in an aggregate) —i.e., applying necessary measurement operations to obtain common ground for later.[27]

Also, most measurement procedures involve some alterations to constituents, and this is just one step of the measurement procedure. Macroeconomists as scientists need to make decisions regarding scale adjustments / calibration when measuring *all* theoretical concepts. For instance, according to Tal (2020), measurement model-based accounts state that any measurement outcome is just "claims about the values of one or more quantities attributed to the object being measured and are typically accompanied by a specification of the measurement unit and scale and an estimated of measurement uncertainty". So, it seems that *"what makes"* synthetic aggregates different is a lack of agreed upon or official measurement units and / or scales. This is an issue of synthetic aggregates holding a lower degree of measurement epistemic fidelity, not an issue that can grant an ontological distinction.

[27] An example is to convert kilograms (kg) to pounds (lb) an approximation, which has been previously stablished and accepted, is used—i.e., 1 kg. corresponds to 2.2 lb. *approximately.*

In the background of this point is the fact that economic phenomena are complex, in the sense that one economic phenomenon is constituted by many factors. To be able to construct compelling models that serve as measuring instruments, economists must idealize or abstract some features. Determining which factors should be abstracted or idealized does not depend entirely on the nature of economic phenomena, but on what aspects we want to model or measure. To assume that synthetic aggregates will be modeled focusing only on the factors that cannot be microfounded, would be a mistake because it is blind to the fact that some synthetic aggregates still count with macrovariables that can be modeled with the aim to measure some aspects that microfoundations can be derived. In short, it seems that to give microfoundations or not depends on either methodological limitations or methodological goals.

The key problem with Hoover's argument is that with no clearer distinction between aggregates, his methodological approach suffers the consequences. This is because it is not clear that synthetic aggregates in fact exist fully independently from microeconomics—i.e., a macroeconomic aggregate which no microeconomic relationship can be shown— hence, there will not be macroeconomic causes to analyze independently from microeconomics. Unless the latter is one among the macroeconomists goal with the macroeconomic aggregate model/measurement. In shorth: Hoover's methodological account cannot start without the success of his ontological argument. In the following section I make clear that for an argument against microfoundations it is better to just drop the ontological argument.

4. Modelling at the Right Level: A Non-Ontological Approach to Microfoundations

Instead of following Hoover, I want to suggest that the debate about microfoundations should stray from ontological considerations and instead start from the fact that macroeconomic models are built for specific purposes (either practical or theory development purposes). This is not to say that the metaphysics behind microfoudnations is not an important issue to inquire about, however, the point I want to make in this section is that the inputs from the best macroeconomics' metaphysics of the time do little to resolve or give a clearer answer to why or why not macroeconomic models need to provide microfoundations. As I highlight below (and has been widely accepted) is not necessary for scientific models to hold an isomorphic relation between the target-system model and real-world phenomenon (read Morgan, Morrison, & Skinner, 1999; Morgan, 2012; Weisberg, 2013; Knuuttila, 2011). So, why is it that the microfoundations debate still being discussed under such considerations? That is, microfoundations as best way to represent macroeconomic phenomena. Contrary to this, the microfoundations debate needs to be discuss and understand taking into consideration (seriously) macroeconomists scientific practices (methodology)—by questioning and analyzing such practices.

For example, macroeconomists have the option to keep using their benchmark models—e.g., Aggregate Supply-Aggregate Demand (AS-AD) model, Dynamic Stochastic General Equilibrium (DSGE) model, endogenous growth model, etc.— or substitute the model for another (Rodrick, 2015). This approach allows the question of microfoundations to be about the purpose of the model. Depending on the purpose of the model, it's possible that some macroeconomic models would be best without microfoundations, and vice versa.

I suggest that Dani Rodrick's model choice approach in economics is a better starting place (Rodrick, 2015). His account offers a procedure of selecting a model that best fits the question/purpose the model was built for. In what follows, I briefly explain Rodrick's account.

4.1 Rodrik's Model Choice Account

Rodrik's model choice account is constituted by three steps: (1) selecting candidate models, (2) identifying critical assumptions, and (3) carrying out verification procedures. *Selecting candidate models* involves finding a set of simple and mathematically tractable models that plausibly represent a given target and specific purpose. Each plausible model provides a narrative of the cause-and-effect and if-then relationships (Rodrick, 2015, p. 19). Next, *identifying critical assumptions*: although economic models involve many assumptions, not all are critical. Economists must identify the *critical* assumption that *if* modified in an arguably more realistic direction, it would produce a substantive difference in the model's conclusion (Rodrik, 2015). Finally, *carrying out verification procedures*: there are four types of verification, and each consists in the process (or skill) of moving back and forth between the model and the real world. The *skill assesses the fitness* of the model with respect to the specific context, purpose, or policy consideration the model was built for.

These verifications are meant to confront the model with empirical evidence instead of first principles (Rodrick, 2015). *1) Verifying critical assumptions.* Economists must check whether the critical assumption is fundamental for the question the model is assumed to answer (Rodrick, 2018). For example, for the question, *"How does a tax increase effect the price of cigarettes?"*, product homogeneity (the assumption that economic consumers have no preference among different cigarette brands) is not a critical assumption. Whether different cigarette brands are perfect substitutes or not would have no effect on the result of the model. In contrast, for the question, *"What is the effect of price controls on the cigarette industry?"*, product homogeneity becomes a critical assumption because consumers' preference among different cigarette brands can show a decrease of consumption in some brands, for example. *2) Verifying Mechanisms*. A model's operative mechanism needs to be verified as being empirically relevant to the real-word working of such mechanism. In a competitive model, a firm's supply and market price is the operative mechanism—e.g., when an industry restricts supply, the market price goes up; when supply is increased, the market price goes down. There are plenty of real-world examples of shocks to supply having had observable effects on prices (Rodrick, 2015). *3) Verifying direct and indirect implications* of critical assumptions. Whether the model's results approximate a variable is subject to direct verification that aims to verify whether the incidental implications are broadly consistent with observed outcomes.

4.2 Microfounded / Not-Microfounded Macroeconomic Models

Accepting that model diversity in economics is an epistemic virtue[28] (see Emrah Aydinonat 2018; Veit 2020) changes the microfoundationalist approach in macroeconomics modelling from an ontological concern to a methodological concern. Note that I am not advocating for Rodrick's account as *the* modeling approach in economics. It would work with other model choice approach too, for example, model selection theory.[29] What is compelling from Rodrik's account is that emphasizes that model diversity in economics should be seen as an epistemic virtue instead of a constraint.

However, that is not to say that everything goes. It is important to establish what constitutes an adequate choice process. As it has been pointed out that Rodrick's step of identifying critical assumptions does not work in the DSGE macroeconomic model (see Grune-Yanoff and Marchonni 2018; Kuorikoski and Lehtinen 2018). DSGE models have core critical assumptions that cannot be changed. For intertemporal optimization, for example, alteration would only be

[28] In chapter four I discuss in detail model diversity's methodological virtue.
[29] Read Rocherfort-Maranda, 2016; Sober, 2002; Foster & Sober, 1994.

possible “if the whole DSGE framework is abandoned” (Kuorikoski & Lehtinen 2018). Also, altering DSGE’s representative consumer critical assumption (i.e., making it more realistic) makes the DSGE model mathematically unattractable (Kuorikoski & Lehtinen 2018).[30]

The above objections suggest that macroeconomics is not a good candidate for Rodrick’s model choice account. But these objections undermine the epistemic virtue of model diversity in economics. Economic phenomena is diverse. Economists need to build different models targeting same phenomena in which different variables, parameters, and exogenous and/or endogenous relationships are the focal point of the model in order to explain and make predictions. Thus, although it might be true that DSGE’s critical assumption cannot be altered, it does not entail that all macroeconomic models would have the same issue. It seems plausible that either a microfounded economic growth model or a non-microfounded economic growth model can be developed for a specific purpose. In short: the above objections are specifically to one of Rodrick’s steps, but that should not dissuade economists and philosophers of science from attending to the epistemic virtues of model diversity.

Note that, although here I rely on Rodrcik’s account, I am not blind to the fact that his procedure needs further specifications. What would happen if you were faced with two models that verifiably hold the same degree of empirical accuracy or inaccuracy and respond to the question equally (see also Grüne-Yanoff & Marchionni 2018)? What should drive the economist’s choice? This is an important question that philosophers of economics and science should explore more (e.g., Ruiz and Schulz, forthcoming). My point here is to illustrate that

[30] Note that another issue with DSGE models is that the conclusions’ direct implications are far away from reality, which bring little value—e.g., DGSE models’ representative agent (see Hoover 2015; Faust 2009; Kirman 1992). As Rodrick points out, if these cannot be confirmed, economists must rethink the value of keeping them, since there might be more useful models with respect to the question the model is used for.

questions about the microfoundationalist approach in economics can have different answers when considering the epistemic virtue of model diversity. Whether or not to include microfoundations would not depend on models accurately (i.e., more realistically) representing the phenomenon in question. As Rodrick stated, "it is not immediately clear to me that assuming a representative household with an infinite lifetime is always a better representation of individual-level behavior—i.e., appropriately 'microfounded'— than assuming fixed savings propensities for different groups of consumers" (Rodrick 2018). But it would depend on methodological concerns such as prediction, empirical relevance, explanation, data fitness, policy goals, etc., instead. In other words, it's about picking models that work for the task at hand; we might sometimes go with microfoundations, and sometimes not.

For example, the expectations-augmented Philips curve (NAIRU) is a benchmark model used in monetary policies. This model is microfounded because it introduces adaptive expectations, which predict or explain an agent's behavior based on their past experiences and expectations for the same event. The expectations-augmented Philips curve uses these to explain that agents will associate high inflation with rising salaries and will adjust their behavior on past experiences, ultimately influencing the effect of the monetary policy. But the NAIRU model's explanations and predictions do not necessarily entail better results. For example, Stockhammer (2004) compares a NAIRU model and a Keynesian model to European unemployment in which the Keynesian approach performed more successfully than NAIRU (also read Smets, Christoffel, Coenen, Motto and Rostagno, 2010). Rodrick might argue that, since modeling is question-dependent (as it is context-dependent), different countries, which entail different policy concerns, might call for different models (Rodrick 2018). The question of the need for microfoundations changes when it is considered from a methodological perspective.

5. Conclusion

I showed that Hoover's distinction between natural and synthetic macroeconomic aggregates is a difference of measurement procedures instead of an ontological difference. By showing this, one must accept that his methodological account—causal structural analysis— does not gets off the ground. Illustrating the problem of metaphysical-based argument against microfoundations. Also, by considering Rodrik's model choice account it became clearer that the problem with the microfoundationalist approach is not only economists' commitment to ontological individualism *per se*. The problem, also, it is with some of the methods used to provide microfoundations—e.g., the representative–agent optimal maximizer with adaptive expectations (Hoover, 2001, 2008, 2015).[31] The problem with this method is that it fails to fulfill its objective: to model the decision problem for each agent in the economy.[32] Both of these problems are the result of seeing modeling with only one purpose—i.e., accurate representations. Both of these methods employ to model macroeconomic aggregate phenomena could be assessed differently if it is accepted that models' goas go beyond representation. For the microfoundationalist approach, the latter, reveals that it is plausible that sometimes microfoundations are needed and sometimes they are not.

[31] Something similar also has been argued regarding DSGE models: although microfoundations might be a useful tool, they are not the ultimate goal of macroeconomic models. That is, if the macroeconomic model makes the wrong aggregated simplifications, then the microfoundations derived from them will not be compelling (Korinek 2017).
[32] Instead, the representative-agent method assumes that the choices of all the diverse agents can be considered as the choices of one *"representative"* standard-utility-maximizing individual whose choices would be the same as the aggregate choices of all the heterogenous individuals (Kirman 1992; Hoover, 2001, 2008, 2015).

Chapter IV: Economic Model Diversity and Policy Making

1. Introduction

Although economists use a variety of models to make sense of diverse and complex economic phenomena, and thus count with (or can build) a variety of models to make sense of these, it has not prevented them to adopt *benchmark* models — e.g., Supply-Demand model, Dynamic Stochastic General Equilibrium (DSGE) model, IS-LM model, endogenous growth model, etc.[33] Science adopting models as fundamental to their object of study is not uncommon nor a reason to criticize its methodology. Nonetheless, the 2008 financial crisis engendered suspicion about some of macroeconomics' benchmark models. People wondered why they did not "see it coming:" why did macroeconomists fail to predict the nature, timing, or severity of the crisis (Bernanke, 2010)?[34] In other words, the public became skeptical of macroeconomics methodology. Almost 15 years after the 2008 financial crisis, macroeconomists have not given up on their benchmark models; not even the DSGE model, which was one of the most criticized (Stiglitz, 2018; Korinek, 2018; Bruine de Bruin *et al*, 2010). It is true that changes to the DSGE model have been made, but the revisions seek only to account for the criticism pointed out—e.g., to incorporate or account for heterogenous agents instead of a homogenous representative agent (Kaplan, Moll, and Violante, 2018). In short, macroeconomists seem to be patching a sinking boat.

[33] Although in this paper I, mainly, focus on macroeconomics, it does not mean that there are no benchmark models in microeconomics. In fact, the microfoundationalist approach is derived from the commitment that economics modeling must derived all of their conclusions from the choice patterns of individual economic agents.
[34] According to several economists, macroeconomists failure to predict the financial crisis is not the only (or main) responsible of such event. For instance, it has been pointed out that financial institutions managers' greed played a major role (Suranovic, 2010). Lack of knowledge to the new financial instruments (Keim, 2012). Although all these have enough justified reasons, in this paper I focus to the criticism to macroeconomics modelling.

The 2008 financial crisis is an unfortunate example of what happens when models are used dogmatically (Rodrik, 2015). Using models dogmatically "make practitioners, with time, become overconfident of the models' epistemic relevance, which leads to errors —e.g., errors of omission and errors of commission" (Rodrik, 2015).[35] To prevent this, economists should embrace *model diversity* as an epistemic virtue. Economists should employ this skill to navigate among models and alter and/or substitute them for a model that best fits the model's purpose (Rodrick, 2015, 2018). Thus, in this paper, I argue that *model diversity* (distinct from *model pluralism*) not only improves economics as a discipline but is also crucial in justifying the relationship between economic models and the policy making process.

The structure of the paper is as follows. In section 2, I explain what is meant by "model diversity" and why it should be differentiated from "model pluralism." In section 3, I expand on the methodological virtue of model diversity for policy making purposes. In section 4, I briefly lay out an example of model diversity focusing on the microfoundationalist approach in macroeconomics modeling. In section 5, I conclude.

2. Model Diversity Is Not Model Pluralism

Philosophers of science have been slowly embracing pluralistic accounts in science and scientific methods.[36] *Scientific pluralism* in philosophy of science is motivated by the limitations of accounts seeking "the unity of science," or the idea that the different scientific domains can be

[35] Rodrik thinks these errors are the consequence of identifying a model with the model—i.e., benchmark models. Errors of omission are products of the inability to see troubles looming ahead. Errors of commission are products of fixating on a particular view of the world, which makes economists "complicit in policies whose failure might have been prevented" (Rodrik, 2015, pp. 152).
[36] For pluralism as a program in science read Ruphy, 2011; Giere, 2006; Longino, 2006; Galison and Stump, 1996; for pluralistic accounts in specific scientific practices, such as explanation, read Mitchell, 2002, 2003; Gijsbers, 2016; about models, read Weisberg, 2007, Morrison, 2011, Veit, 2020; about computer simulation, read Edwards 2010, Weart, 2010.

unified into one single theory. In other words, pluralistic scientific practices are motivated by the limitations of accounts seeking *the* explanation, *the* model, *the* theory, etc. (e.g., Philip Kitcher's explanatory unification account [Kitcher, 1981, 1989]). However, due to the complexity found in the natural and social world, unity seems implausible. Therefore, philosophers of science have pointed out that scientific practices must develop multiple explanations or models to fully obtain what is at the heart of the research. In scientific domains that overlap at different levels of organization—e.g., explanations in biology at the cellular level *and* the organ level (Braillard and Malaterre, 2015) — scientific pluralism has been widely adopted, or at least discussed more generally.

Note that is not the purpose of this paper to argue against pluralism—specifically model pluralism. Personally, I am sympathetic to such pluralistic accounts. Instead, in this section the purpose is twofold. One, "model pluralism" as it has been developed lately by Veit (2020, 2021) and Aydionant (2018), who base their account around Rodrik's view of "model diversity as an epistemic virtue," misinterprets and strays away from model diversity's *epistemic virtue*. (I point out the issues with both accounts in section 2.2.) Two, both accounts are implicitly committed to a sort of epistemic completeness that ultimately undermines the main aim of "model diversity," disguising its relationship with policy making.

2.1 Model Diversity

Since literature on model diversity is limited, it is sufficient for our purposes to understand what Rodrik implies by "model diversity" throughout his book. First, the call for model diversity started as a criticism of economists' tendency to commit to 'benchmark' models—i.e., when economists confuse *a* model for *the* model (Rodrick, 2015). Only focusing on one set of leading models precludes economists to focus on the different perspectives economics models have to offer. This is problematic because economists will only focus on the set of conclusions stressed by these benchmark models, overlooking the variety of economic phenomena (Rodrik, 2015, pp. 198). Economists, instead, need to take advantage of the "multiplicity of models tailored to a variety of settings" (Rodrik, 2015, pp. 11). Embracing the idea that models can be built based on a specific purpose, context, or question allows economists not only to improve economics' epistemic power, but also to give better practical advice—i.e., policy. Although a specific purpose focus on a specific phenomenon, different models answer to this differently. Thus, economists need to evaluate which model best fits the purpose. The upshot from the above is that, in economics, model diversity has not been used to obtain any sort of epistemic completeness. Put differently, it is not a scientific methodology that seeks a set of models that would exhaustively predict, explain, and/or teach economic phenomena best.

For example, in economics, besides *competitive* market models, one can also find *uncompetitive* market models—e.g., monopolies, duopolies, monopolistic competition, just to mention some—oligopoly market models—e.g., Cournot model, Bertrand model, Stackelberg model. Just as there are models about types of uncompetitive models, there are models with mathematical structures that track different causal structures' dimensions, like static versus dynamic models and simultaneous versus sequential moves models. The upshot of all these examples is that is not necessary that, for example, market price can be only model as a static model (market demand and supply at a particular point in time) but also it can be model as a dynamic pricing model (how demand fluctuations relate to market price). These two models are neither complements of each other nor do they fully explain or predict market price. Instead, these two models model market price differently depending on their scientific purpose.

The second point to consider for a better understanding of model diversity is that, as Rodrick emphasizes, different models of *x* phenomenon cannot be assessed as one being "right" and the other "wrong." Three models of working markets are just "three different visions of how markets function (or don't)…The correct answer to almost any question in economics is: It depends. Different models, each equally respectable, provide different answers" (Rodrik, 2015, pp. 17). Model diversity's epistemic virtue thus relies on the variety of models already at hand and the ability to adapt or build on existing models depending on the purpose the model is going to be used for. Ultimately, model diversity's epistemic virtue is that it gives economists power *to choose a model that best fits a chosen purpose*. Considering this more seriously, this explains why most of Rodrik's book corresponds to the development of a model choice account (laid out in chapter 3).

2.2 Model Pluralism

As I have shown in section 2.1, Rodrik did not lay out a clear picture of what he meant by "model diversity as an epistemic virtue in economics." Because of this, recent accounts of model diversity have been developed. These accounts, however, improperly elaborate on model diversity as stated by Rodrik. These accounts conflate model diveristy with model pluralism (implicitly or explicitly). This is troublesome. It is important to keep them separate because (1) as a method, they entail a different process, and (2) their epistemic virtue is different.

A philosopher who elaborates on model diversity is N. Emrah Aydinonat. He argues that model diversity secures better economic explanations. This is because economists build models with the aim (among others) to find the right set of models to explain the phenomenon at hand, and most economic phenomena are the result of multiple causal factors. A single model cannot give us a complete explanation (Aydinonat, 2018). In other words, economists cannot provide an exhaustive explanation citing all the relevant causal factors of x phenomenon with one model. Thus, different models—each targeting a specific casual factor or answering different *what-if* questions about the phenomenon in question—are a means to better explanations in economics. "Model diversity" in economics, according to Aydinonat, allows the latter.

Aydinonat's account of model diversity for better economic explanations, however, resembles explanatory pluralism. It is accepted in neuroscience, for example, that fully explaining a phenomenon requires the integration of various mechanistic explanations at different levels (Craver, 2002).[37] Aydinonat's account digresses and embellishes model diversity's focal goal: the power to choose *a* model for *a* purpose. Instead, his account of "model diversity" for better economic explanations is just a version of *explanatory* pluralism, which aims for an exhaustive explanation of a particular event. The difference is that, instead of a number of mechanistic explanations, economists must provide a number of different models targeting different causal factors for an *'exhaustive'* explanation of a *x* phenomenon.

A problem with this account is that it only focuses on explanation. There are other goals a model can be built for, goals that do not *necessarily* involve the integration of a variety models to achieve some purpose. Another problem is that, since a set of models are needed for an explanation of *x* economic phenomenon, it is not clear how many models are needed to explain a phenomenon and how different these models would need to be from each other. What about constructing models on non-major causal factors in a phenomenon? Where does one draw the line concerning how many models are needed to obtain 'better" explanations in economics?

[37] For explanatory pluralism in biology read Braillard, P. and Malaterre, 2016.

On the other hand, Walter Veit (2020, 2021) dubs 'model pluralism' an account based on and expanded from Rodrik' model diversity, but incorrectly overelaborates on it.[38] Model pluralism also foments what I have been calling "epistemic completeness," or the idea that multiple models guarantee an exhaustive explanation, representation of a target system, precise predictions, etc. "Model pluralism" (strong model pluralism),[39] according to Veit, consists of two theses:

> "(i) any successful analysis of models must target sets of models, their multiplicity of functions within science, and their scientific context and history and (ii) for almost any aspect *x* of phenomenon *y*, scientists require multiple models to achieve scientific goal *z*" (Veit, 2020, pp. 93).

Thesis (i) is a *how-to* thesis—i.e., how philosophers of science must study to further understand the practice of scientific modeling— which I do not see as a controversial. In fact, my minor objection to thesis (i)—if we can call it an objection—is that it is not new that philosophers of science focus on actual practices of modeling across the sciences, their different functions (epistemic and non-epistemic), scientific context and history. Philosophers of science have also engaged in debates concerning models' ontological status (read Mäki, 1994; Morrison and Morgan, 1999; Zeidler, 2000; Knuuttila, 2009; Odenbaugh, 2009; Frigg, 2010; Peschard and Van Fraassen, 2018; Morrison, 2011; Weisberg, 2012). Another minor objection is that suggesting

[38] Although Veit's 'model diversity' entails both a descriptive claim—"models and modeling practices in science are incredibly diverse" (Veit, 2020, pp 92)—and a prescriptive claim— "model diversity is to be sought and embraced (Veit, 2020, pp. 92) —he concludes that referring to both "interchangeably when speaking of model pluralism" (Veit, 2020, pp. 92) is unproblematic. Note that the issue I found in Veit's account has nothing to do with a linguistic mix up.

[39] In this paper is not necessary to give an exhaustive description of Veit's account. However, it is important to note that he identifies four forms of model pluralism—weak, weakly moderate, moderate, and strong. From these, he identifies Rodrik's *model diversity* and Aydinonat's account with what he dubs *weakly moderate model pluralism*—i.e., as "each phenomenon has many different aspects, and scientists need different models to explain/predict these different aspects of a single phenomenon" (Veit, 2020, pp. 96). Since I take this version to concur with pluralistic accounts of scientific practices (modeling) in general, I have decided to focus on *strong model pluralism* to highlight why model diversity is not model pluralism.

that an analysis of models *must always* target sets of models for this to be successful seems odd. Scientists or philosophers of science can explore, and gain knowledge from, the practice of modeling by analyzing one model in a specific domain at the time.

For example, a philosopher of science that won a scholarship to study scientific models can organize her grant in the following way: during year 1, she can study modeling in climate science; during year 2, she can study endogenous economic growth models; during year 3, she can study nuclear models in physics, etc. It is not clear to me how analyzing sets of models would, necessarily, improve each one of these research grants. It seems to me that by analyzing each type of model, one at a time, scientists and philosophers of science can implicitly make comparisons with their past knowledge. In other words, I think it is odd to require scientists to make explicit that each time they assess a model it has been compared (or it must be compared) to a set of models. I think this is already implicitly practiced, and that it should not be a *condition* for scientists or philosophers of science, but an *option*. This is all I can say about thesis (i).

Thesis (ii) is controversial and entails a different epistemic virtue (goal) from model diversity's epistemic virtue. Thesis (ii) prescribes that, independently of scientific goal *z*, scientists require multiple models of aspect *x* of phenomenon *y*. If one of the specific scientific goals is explanation or prediction, thesis (ii) seems non-controversial. Although some scientists need several models to accomplish these goals, I do not think this is a necessary condition. For example, it is actually the case that, in climate science modeling[40] (computer simulation), multiple models for forecasting purposes are used. Since the climate system is complex and technology is limited, is not possible to calculate all the equations for every cubic meter,

[40] Similarly, large-scale computational models (multiequation models) to forecast the economy and predict the effects of monetary and fiscal policy are used in most central banks.

variable, and parameter. Scientists thus divide the climate system into layers (smaller models) to calculate pressure, humidity, wind, temperature, etc., and to give a fair approximation (Parker, 2018; Lenhard and Winsberg, 2010). But what about other scientific goals—for example, policy making? I come back to this in section 3.

So, what is wrong with thesis (ii)? In fact, not much. Its main problem is that, as described by thesis (ii), model pluralism's epistemic virtue enforces 'completeness:' *the* explanation, *the* most accurate prediction, *the* representation of a target-system, etc. There is an implicit assumption that scientists would need multiple models to explain *x* of phenomenon *y*. The reason seems to be that of providing an exhaustive explanation. We see a similar situation when representation is the scientific goal. Veit makes this point explicit when emphasizing mistakes assumed of *Schelling model of segregation* constitution—the mistake is that the Schelling model is taken as one single model (Veit 2020) and not as "a whole cluster of models that are related to each through genealogical origin and similarity" (Yilkoski and Aydinonat, 2014).

The problem is that model diversity is not just about multiple, numerous models that help achieve a scientific goal.[41] To suggest that diversity gives you exhaustive epistemic goals is a mistake. Model pluralism and model diversity must be considered separately for a better understanding of their epistemic virtue. To allow these two methodologies to be used interchangeably is like mistaking Latin American countries as an integrated multi-culture. This

[41] Mäki (2018) points out that model diversity should not be confused with the number of models: "It is one thing to have *a large number of models of the same kind*, and quite another to have *many diverse kinds of models"*. This is hinting that diversity needs to be considered on its own and not as a synonym with pluralism.

limits the understanding of the diversity and variety of regions and cultures, each one with its own richness. The same is at risk by conflating model diversity and model pluralism.

Another problem is the generality in which *"scientific goal z"* is portrayed. For some scientific goals a pluralistic approach to modeling is something that is needed and should be done. But this might not apply to all scientific goals. Scientists often are only interested in merely providing a partial account of aspect *x* of phenomenon *y*. The number of models needed depends a bit more on the specific scientific goal *z*. What the model is going to be used for? What is the question the model is trying to answer? Contrary to this, model diversity guarantees that you have at least two models to choose from. Compare which one best serves the purpose, revise its empirical accuracy with the purpose at hand, and use the one that fits best (Rodrik, 2015, 2018; also read Reiss, 2016; Parker, 2020).

The issue is not whether model pluralism is a bad methodological approach. Rather, the issue is that model diversity plays an important (and different) epistemic role for policy making, which I will make clearer in the next section. Thus, model pluralism and model diversity must keep separate. Economists can choose among the variety of models specifically with respect to the policy at stake. It is one thing to say that many models (model pluralism) are needed for *z* policy; it is another to say that there are a variety of models (model diversity) to choose from for *z* policy.

3. Model Diversity and Policy Making

Although Rodrik develops a model choice account, which evaluates and assesses how to select a model for z purpose, in this section I will not expand on his model choice account because that would digress from what is at stake here. That is, a defense of model diversity's methodological virtue and its relationship with policy making. Instead, I focus on two things. One, model diversity aids economists in improving communication concerning why they propose a specific model for a policy.[42] The step I am envisioning most likely happens *implicitly* in the model-construction/model-choosing process. Think of such step as an epistemic justification for a model to be used for the policy at hand. Two, I highlight some plausible features that are relevant in the model-choosing/policy making process.

In better words, the epistemic justification I propose concerns economists (scientists in general) and policy makers engaging in a dialogue in which both evaluate models and choose the one that best serves the purposes at stake for the development of a policy. This is important because economists must have certain knowledge about what the aim is with the initiative/intervention the policy makers seek. Similarly, policy makers must be aware of the model's constraints. For example, each must talk about why *X* methodology is preferred and whether such preference is share or not must be communicated. Also, employing this step secures that, in the policy writing process, a description of the method/procedure/approach used to get the result is not the only "scientific" assessment made on behalf of the economists. Policy proposals need a more detailed explanation concerning why *X* methodology is preferred instead of *Y* methodology—i.e., *X* methodology will strengthen the policy acceptance chances. For example, the policy advice might be received better. Economists and scientists must ensure that their voices are understood accurately, and highly mathematical complex models might not be accepted or understood.

[42] Note that what I am proposing here is not an alternative to cost-benefit analysis. The latter is a policy evaluation, not an evaluation of the model.

Economists have, or can learn, the skill of evaluating models. However, note that economists (and scientists in general), when evaluating a model, do not only focus on the representational adequacy of the model, but also on how well it *fits a purpose*. Most importantly, note that adequacy for *x* purpose can be an epistemic purpose—that is, explanation or prediction—and/or a non-epistemic purpose, such as purpose policy (practical purpose). Although the intended purpose is a practical one, an epistemic purpose related as well: "the intended contribution of the model is epistemic: it is expected that the model's serving one or more epistemic purpose will, in the context of a more extended activity, facilitate the achievement of the practical purposes" (Parker, 2019, pp. 460). Therefore, a dialogue between economists and policy makers must continue throughout the policy making process. In other words, facilitating the achievement of the practical purpose is precisely what needs to explicitly be pointed at when choosing between models.

The above dialogue needs to assess two models (but not necessarily two—it can be one, three, four, etc., the point is that economists' skill to navigate among models allows them to do this) for a policy initiative. Economists and policy makers must consider whether there is a suitable relationship with "target T, user U, methodology W, circumstances B, and goal P jointly" (Parker, 2019, pp. 464).[43] This is a crucial point for economists as contributors to public policy. Economists' role must not end at the epistemic (methodology) phase. Economists need to engage in more active and present conversations throughout the policy making process. For instance, suppose economists choose to use model A because this gives us a better picture of consumers behavior, but since the models' mathematics are a bit more complex[44]due to the

[43] For a detailed explanation of Parker's model adequate-for-purpose account read Parker, 2019, 2021.
[44] To say that REG's mathematics are a bit more complex does not entail that one must always chose models with simpler mathematics. The point is that, for X purpose, simpler explanations are favored for specific target users. The

endogenization of some macrovariables, the targeted audience has issues understanding its implications, and thus the policy is not approved. Parker (2019) acknowledges that this is a case in which the model is an inadequate model for purpose *x*: “although M’s equations are very accurate, they are so complex that explanatory information is not silent to users (U)”. In other words: economists need to navigate among models (not default to the most preferred model), assess which seems to fit the policy purpose, and then communicate the epistemic imports of the model to the policy maker to stablish the relationship described above.

Also, when it comes to policy making, economists and scientists in general must not uphold the choice of methodology (model) by themselves. There must be features of the user (U) which do not only concern economists and policy makers as target audience. For example, the audience of a policy could be obscured or hidden from economists working on the model for the policy. This is not absurd to imagine. Economists and scientists have been criticized for spending too many hours on their research and too little time with the public. Objections claiming that it is a mistake to expect all economists and scientists to engage in public affairs do not affect what I am proposing. It is true that not all economists or scientists must engage in public affairs. There is a reason they chose to be scientists instead of politicians. However, those who choose to engage in public affairs via policy making must acknowledge the need for a better process to communicate the epistemic findings behind their methodology and how it best fits the purpose at hand (policy X). The latter is only achievable by establishing a suitable relationship with the policy maker (and all the features mentioned above).

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tradeoff depends on the adequacy with which the model fits with the relationship among the model adequate-for-purpose’s features.

This also shows that choosing between different models does not depend on the attractiveness of a given epistemic or methodological feature alone—e.g., whether the macroeconomic model is microfounded accurately to the movement. Instead, it would depend on which model better fits the policy purpose at hand. It can be decided that intermediate cases are best: some *limited* form of microfoundations are found to lead to the best combination for the policy at hand. That is, from a policy perspective, it may be best to endogenize just the national savings rate, but not to endogenize the rate of technological progress. Once again, choosing a model for a specific scientific goal does not imply that there must be a general account to do so—i.e., it's a case-to-case manner.

What matters here is to establish the value for economists and scientists to engage in an epistemic justification for using one model instead of the other. The epistemic justification is just one of the starting steps to secure that the "writing" of the policy starts with the model that best addresses the policy question. This would limit justifications (or lack of) to use models because of ideologies, political purposes, or popularity of use. It might turn out that, according to *x'* policy purposes, model A is the model that best fits *X* policy instead of model B, or vice versa. Focusing on model diversity, model evaluations such as Rodrik's account and/or Parker's account would prevent errors that happen when "economists (and those who listened to them) became overconfident in their preferred models of the moment…they forgot about the other models" (Rodrik, 2015, pp. 159), or because "refraction of an intellectual trend in academic economics through the political process sometimes leads to a set of ideas being too dominant and too long-lived in the policy world after the academic bandwagon has rolled on" (Coyle, 2022, pp 72).

4. An Example

To make the arguments above clearer, it is best to look at an example. Suppose that, in a think tank, an economist and a policy maker are working on a policy initiative to stabilize economic growth. To do this, they are considering two models of economic growth—one with and one without microfoundations. The first model—a version of the Solow model—treats technological progress as an exogenous variable that grows at a fixed rate g. The second model—a version of Romer's endogenous growth model—treats technological progress as the result of intentional investment decisions made by profit-maximizing agents (Romer, 1990; Jones and Vollrath 2002).

Now, for certain epistemic purposes, the second microfounded model may be more compelling: it provides an account of why and how technological progress occurs. However, in this case, the first model may be preferrable. In virtue of the fact that it is easier to understand by the targeted audience, policy interventions that increase the rate of technological progress—say, increased funding for public universities—can be more easily justified to policy makers and the wider public. In another context, though, where we are trying to decide among different policies for increasing technological progress, the second model may be preferrable. This shows that, when it comes to model choice, there need not be one right answer: it depends on the context in question.

5. Conclusion

In this paper I showed that although model pluralism counts with a compelling epistemic virtue, this is not (and it should not be used interchangeably with) model diversity. Model pluralism's methodological virtues is that which I dub *epistemic completeness*, whereas model diversity's methodological virtue is the ability *to choose a model for a purpose.* The latter does not necessitate multiple models to obtain *x* purpose. Moreover, I expand on model diversity's place in policy making. The ability to choose between models prevents economists from using models dogmatically, as argued by Rodrik. I showed that a model evaluation account (based on Parker's account), when choosing between models for a policy, allows economists and policy makers to engage in a more transparent policy making process. When evaluating an adequacy-for-purpose model, economists need to consider aspects beyond the epistemic goals of the model, including features of the target audience.

Chapter V: Concluding Remark

The chapters in this dissertation address the microfoundationalist approach in economics from the perspective of a practice-based philosophy of science. By focusing on the details of how economists (and scientists in general) build models in practice, new insights concerning the microfoundationalist debate (and scientific modeling in general) become available. In what follows I summarize the three core insights from my dissertation.

1. Microfoundations: Not Either / Or

As argued in chapters two and three, most of the arguments in favor of, or against, the microfoundationalist debate rely on the metaphysics of macro- and microeconomics—i.e., whether macroeconomic phenomena are just the result of economic individual agents, and whether the latter therefore must be explicitly taken to be the foundation economic models. This framing suggests the debate is an either/or question. In contrast, by considering the question about microfoundations from a purely methodological perspective, it becomes clear that whether economic models require microfoundations should not be seen to be an either/or question. It is possible that sometimes, macrovariables can and should be seen as a product of the decisions of individual economic agents, but also these same macrovariables sometimes can be treated as independent from their microeconomic parts. As I point out in chapters three and four, the microfoundationalist argument depends on the scientific or policy purposes of the model.

2. Metaphysics and Science

Chapter two illustrates that addressing metaphysical questions (specifically concerning social ontology) is neither necessary nor useful in the process of model building in economics (or science in general). It is most often the case that relevant metaphysical issues are the outcome of model-building processes (and other scientific practices), not its starting point. Note that I am not implying that metaphysical debates have no value or role in economic or scientific debates (or in philosophy of science or social science). Rather, the point is that it is not necessary to solve these debates first so as to progress in the (social) sciences—and that the latter can help us with the resolution of the former.

3. Economic Methodology and General Philosophy of Science

I argue that philosophical debates concerning scientific modelling should look more at the work done in economics. Understanding how economists use scientific tools—e.g., models, ceteris paribus clauses, robustness analysis, econometrics, causal structures, randomized controlled trials, etc.—to generate knowledge about the economic world can help shape and provide useful insights for how to assess general issues in philosophy of science.

4. Further Research

The relationship between economic methodology and policy making (a key aspect of chapter four) is an important area that needs to be explored further. Specifically, there is an open question concerning whether economists—who are both practitioners of science and contributors to public policy—have responsibilities that go beyond their epistemic responsibilities. This not only highlights the need for understanding the significance of economic practice, but also the *moral* and *policy-based* significance of economic practice.

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