Social Ontology and Model-Building: A Response to Epstein

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Abstract

Brian Epstein has recently argued that a thoroughly microfoundationalist approach towards economics is unconvincing for metaphysical reasons. Generally, Epstein argues that for an improvement in the methodology of social science we must adopt social ontology as the foundation of social sciences—i.e. standing microfoundationalist debate could be solved by fixing economics' ontology. However, as I show in this paper, fixing the social ontology prior to the process of model construction is optional instead of necessary and that metaphysical-ontological commitments are often the *outcome* of model construction, not its starting point. By focusing on the practice of modeling in economics the paper provides a useful inroad into the debate about the role of metaphysics in the natural and social sciences more generally.

Keywords

social ontology, model-building, microfoundations, economic methodology, Brian 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

[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.

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 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

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[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.

[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.

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).

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* 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

[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.

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

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

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

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.

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

[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.

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*.

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.

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[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.

[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.

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 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 1999b). 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] 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

[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).

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 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

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

[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).

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 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.

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

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