Microfoundations and Methodology: A Complexity-Based Reconceptualization of the Debate

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Abstract

In a number of very influential publications, Brian Epstein and Kevin Hoover (among other authors) have recently argued that a thoroughly microfoundationalist approach towards economics is unconvincing for metaphysical reasons. However, as we show in this paper, this metaphysical / social ontological approach to the debate fails to resolve the status of microfoundations in the practice of economic modelling. To overcome this, we argue that endogenizing a model—that is, providing microfoundations for it—correlates with making that model more complex. Specifically, we show that models with more microfoundations tend to have more parameters or variables. This matters, as there are well established methodological reasons for preferring models with fewer parameters or variables—ceteris paribus. We therefore conclude that, from a practice-based point of view, microfoundations are only defensible to the extent that they significantly improve the ability of the relevant model to fit the data of interest. In this way, we arrive at a practice-based, methodological reconceptualization of the debate surrounding the need for microfoundations in economics.

5 *Conclusion*

1 Introduction

Whether economic models, in order to be seen as compelling, need to be given 'microfoundations'—that is, whether all the conclusions of the models need to be shown to be derivable from the choice patterns of individual economic agents—is a classic debate in economics, and one that shows no signs of abating (Epstein [2015]; Hoover [2010], [2015], [2009]; Elster [1982]; Kincaid [2015]; Zahle & Kincaid [2019]).[1] In the recent literature, the predominant approach towards this debate is in terms of metaphysical considerations: what is the best social ontology that should underlie economic models (Epstein [2015], [2014]; Hoover [2009]; but see also Kincaid [2015]; Sugden [2016])? Further, in the recent literature, the most widely accepted answer to this question is that a purely individualist micro-ontology is unable to do justice to all the phenomena being studied by economists (Epstein [2015]; Hoover [2009]).

However, as we make clearer in this paper, this metaphysics-first approach in fact fails to compellingly resolve the debate surrounding the need for microfoundations in economics (see also Kincaid [2015]; Little [1998]; Sugden [2016]). Because of this, we argue that we need to adopt a novel, purely methodological reconceptualization of this debate based on the *complexity* of models with and without microfoundations. The upshot of this complexity-based methodological approach to this debate is a new way of looking at the issues that can help bring the debate closer to its resolution.[2]

[1] The importance of this debate goes beyond questions of economic methodology per se and also speaks to the demand for an individualistic, reductionist social science more generally (Rosenberg [2012]; Epstein [2015]; Watkins [1952]; Elster [1982]). However, the focus in this paper is strictly on economics (though many of the conclusions carry over to the more general case).

[2] It is of course true that the individualism / holism debate has often been framed as methodological in nature (see for example Elster [1982]; Kincaid [2015]; T. Jones [1996]). We return to this point momentarily; the claim in the text is

The structure of the paper is as follows. In section 2, we explain what we mean by 'microfoundations'. In section 3, we present the currently popular metaphysical arguments against the need for microfoundations, and argue that these arguments fail to fully make the case against the latter. In section 4, we reconceptualize the debate to formulate a purely methodological approach towards these issues. We conclude in section 5.

2 Microfoundations

The basic idea behind the commitment to microfoundations in economic modelling is that, in order to be compelling, economic models need to derive all of their conclusions from the choice patterns of individual economic agents (Mas-Colell et al. [1996]; Hoover [2015], [2009]; Little [1998]; Sugden [2016]; Phelps et al. [1970]; Frydman & Phelps [2013]). These agents can be consumers, firms, or even governmental entities: as long as a firm or governmental entity can be seen as a genuine economic agent of its own, it can be part of the microfoundations of the model (Gindis [2009]). (If a firm or governmental entity cannot be seen as a genuine economic agent of its own, its behavior needs to be derived from that of the genuine economic agents, whatever they turn out to be: see for example Coase [1937]; Williamson [1971]; Kurozumi [2010]. We return to this point below; see also Epstein [2015].) Depending on the way economic models are individuated, this therefore means that non-microfounded models need to be replaced or at least supplemented by microfounded-models (except, perhaps in a limited set of pedagogical circumstances). Whether macro-models need to be replaced by micro-models, or merely supplemented by the latter, turns on whether a macro-model M_1 that is 'supplemented' by a micro-model M_2 should be seen to make

just that *the kind of methodological reconceptualization argued for here* is new and different from the metaphysical focus that has been popular of late.

up a new model M' or whether it retains its status as M_1. However, since settling this issue is not important for our purposes, we leave it open here.[3]

The details of the choice procedures that micro-founded models rely on can vary from model to model: while it is still quite common to assume that consumers and firms are fully informed expected utility or profit maximizers (Hausman [2012], [1992]; Mas-Colell et al. [1996]; Gul & Pesendorfer [2008]; Phelps et al. [1970]), different theories of choice could be and increasingly are adopted (Gigerenzer & Selten [2001]; Bleichrodt & Wakker [2015]; Barberis [2013]; Frydman & Phelps [2013]). The key point is again just that, independently of exactly what economic agents are taken to be and independently of exactly how they are assumed to make decisions, everything that occurs in the model needs to be able to be derivable from these decisions.

A good example of this kind of commitment to microfoundations is Romer's ([1990]) model, which 'endogenizes'—that is, provides microfoundations for—the classic Solow growth model (for a good introduction to the latter model, see for example C. Jones [2002]). In the classic Solow growth model, it is assumed that GDP (Y) is determined by three-factor national production function:

$$Y = f(A, K, L), \tag{1}$$

[3] Something similar also holds for issues concerning theoretical reduction and explanation. To see this, note first that the relationship between economic models, on the one hand, and economic theories and explanations, on the other, is not straightforward (Hausman [1992]; Morgan [2012]; Morgan & Morrison [1999]; Maki [2009]). Second, note that there are many different views about what constitutes for a theoretical reduction or an explanation (on explanation, see for example Potochnik [forthcoming]; Machamer et al. [2000]; Salmon [1984]; Hempel [1965]; Khalifa [2012]; Grimm [2010]; for a good overview on reduction, see Sterelny & Griffiths [1999], chaps. 6-7). The upshot of these two points is that defenders of the need for microfoundations in economics need not be committed to theoretical reductionism in economics—for example, because they don't think there are many (macro or micro) theories in economics to begin with—or to the replacement of macro-explanations with micro-explanations—as they might think that scientific explanation is causal-mechanical, and are happy to admit that there can be causal-mechanical macro-relations. Microfoundationalism is inherently a view just about the *practice of economic modeling* (though it may of course have implications about various other questions—depending on the relationship between models and the best accounts of explanation and theoretical reduction for the issues at hand).

where K is the national stock of capital, L is the national stock of labour, and A is ‘total factor productivity’—that is, the domestically available technological frontier. It is assumed that there are decreasing returns in all the factors (so that doubling these factors would lead to *less* than doubling the output), that A and L grow at fixed and constant rates g and n respectively, and that K accumulates as a result of the difference between a fixed, exogenously given savings rate s and a fixed, exogenously given depreciation rate d. Skipping over some algebraic details not relevant here, it can be relatively easily shown that, in the equilibrium, GDP per head (Y/L) will grow at a rate that is equal to g—that is, the rate of technological progress.

Now, for present purposes, it is crucial to note that the classic Solow model lacks microfoundations. In particular, the savings rate s and the rate of technological progress g—both of which are key features of the model—are simply taken to be exogenous parameters. They are not derived from the choices of individual consumers or producers; indeed, the latter are completely absent from the model.

This, though, can be changed. In particular, Romer ([1990])—a paper that heavily contributed to its author winning the Nobel prize in economics—assumes that there are a number of identical producers, which jointly produce GDP (Y) according to the Cobb-Douglas-like production function

$$Y = \int_{i=0}^{\infty} H_Y^{\alpha} L^{\beta} x(i)^{1-\alpha-\beta} \, di, \qquad (2)$$

where the i is the range of available input technologies, $x(i)$ is the amount of technology i used in production, L is labour, and H_Y is the amount of human capital employed in production. (2) thus states that GDP is determined by the amount of labour used for production, the amount of human capital (schooling, etc.) used for production, and the amount of technology *i* used for production—for all the different available technologies *i*.

The use of any technology requires paying a price of *p(i)* to the patent holder. Firms then maximize profits by setting, for each technology *i*:

$$p(i) = (1 - \alpha - \beta) H_Y^{\alpha} L^{\beta} x(i)^{-\alpha-\beta}. \tag{3}$$

Behind (3) is just the idea that firms set the marginal cost of producing with technology *i*—*p(i)*—equal to its marginal revenue (which is just the first derivative of the production function for technology *i*, with respect to *i*.)

Crucially, though, the range of available technology *A* is not taken for granted, but assumed to grow depending on the amount of human capital employed in technology production H_A. In turn, the latter is determined by solving the profit maximization problem

$$max_x \, [(1 - \alpha - \beta) H_Y^{\alpha} L^{\beta} x(i)^{1-\alpha-\beta} - r\eta x], \tag{4}$$

where *r* is the interest rate and η is the amount of output units needed to produce 1 unit of an input good *i*.[4] (4) thus states that people decide how much of their human capital to devote to technological innovation by maximizing the difference between the revenue they can get from production with a given technology and the costs of using this technology—which is just the costs of the capital (the interest rate) times the amount of output need to produce the relevant quantity of good *i*.

[4] Note that (4) is just the maximization of revenue (*p(x)x*) minus cost ($r\eta x$).

Finally, we need to consider the consumer's intertemporal consumption decisions:

$$max_C \int_{t=0}^{\infty} \frac{C^{1-\sigma} - 1}{1-\sigma} e^{-\rho t} dt, \tag{5}$$

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. Equation (5) is a relatively standard utility function expressing the extent to which people prefer to consume more rather than less, and the extent to which they prefer to consume sooner rather than later. (4) and (5) together can be used to determine how much time and effort is allocated to labour and the acquisition of human capital.

The upshot of all of this is something that looks a lot like the Solow growth model (with human capital):

$$Y = (AH_y)^{\alpha}(AL)^{\beta}K^{1-\alpha-\beta}\eta^{\alpha+\beta-1}, \tag{6}$$

where A is the upper limit of available production inputs i (that is, the extent of the technological frontier). Note that (6) can be written as $Y = f(A, K, L, H)$—that is, equation (1) with the addition of human capital. However, what were exogenous macro parameters in the Solow model—like s and g—are now given choice-theoretic foundations: they are derived from the choices of consumers and producers over different allocations of labour, human capital, and time paths of consumption.

In this way, the key features of models with microfoundations become clearer. First, adding microfoundations to a model requires that the model is built from the *choice-theoretic* ground on up. The issue is not one of getting rid of *all* exogenous parameters (which would not be possible anyway): in Romer's microfounded form of the Solow model, for example, we still have exogenous parameters in the shape of the agent's intertemporal substitution rate ρ, the capital discount rate *d*, the rate with which output goods can be translated into input goods η, and the shape of the agent's utility function σ (among others). It is just that these latter exogenous parameters either represent features inherent to an individual agent's choice patterns or aspects of the *non-social* world (such as the physical features of capital goods), rather than social features that are unconnected to the choices of an individual economic agent.

Second, adding microfoundations to a model generally involves complicating it. We need to add choice-theoretic derivations for those aspects of the model that were not part of the choice-theoretic foundation to begin with. Note that, in principle, it is entirely possible that adding such derivations reduces or leaves the same the amount of 'assumptions'—parameters or variables—the model relies on. (In section 4, we return to some more technical issues surrounding this use of 'assumptions'.) For example, in some cases, it may be possible to assume that there is a representative consumer with an exogenously given intertemporal substitution rate ρ, and to use this rate *instead of* a formerly exogenously given national savings rate *s* (and it may even be possible to also derive other formerly exogenous parameters from this parameter ρ). However, while this sort of situation is possible—and entirely consistent with the arguments that follow below—the more typical cases involve microfoundations that increase the number of parameters or variables of the model.

To see this, note that, whatever exactly is true about the metaphysical nature of economic phenomena (a point to which we also return momentarily), it is uncontroversial that individual decision-making has something to do with—that is, is relevant for—most economic questions.

After all, economics is a social science focusing on human interactions with their social and non-social environment (Hausman [1992]). Given this, moving away from microfoundations generally means summarizing or otherwise aggregating individual agential effects, and adding microfoundations requires spelling out the individual agential effects that are connected with the relevant social phenomena—that is, de-aggregating the macro-assumptions. This is bound to increase the number of assumptions (parameters and variables) the model relies on: if a variable or parameter really *aggregates* individual agential effects, there have to be several such effects to be aggregated. Indeed, if a macro-parameter or variable were fully replaceable with a micro-parameter or variable (as in the ρ / s illustration just sketched), it is not clear why it would not be a micro-parameter or variable to begin with: these then appear to be merely differently labelled representations of the same social phenomenon (see also Hoover [2010], [2015]). (Similarly, if a given parameter is not social in nature at all—say, because it represents the weather, or, as above, physical facts about the depreciation of capital—then a model featuring such a parameter should not be seen to be lacking microfoundations. See also the response of Di Iorio & Herfeld, 2018 to the arguments of Epstein, 2015.)[5] At any rate, even if this kind of one-for-one endogenization is possible in some cases—for example, where all the member of the relevant population are identical in terms of their savings rate—this should not be seen to be typical; *in general*, endogenizing a model should be expected to lead to an increase in the amount of assumptions underlying the model.

The above example makes this very clear. In the original Solow growth model, we merely need to specify α, β, s, g, n, and d and track K. In Romer's model, by contrast, we need to specify α, β, σ, ρ, d, η, as well as track variables K, C, H_A, H_Y, $p(i)$, $x(i)$, (among others). The original Solow

[5] However, that said, the same sort of complexity-focused concerns laid out below can be applied here, too.

model contains about 10 variables and parameters that need to be tracked (depending on the exact version of the model in question), whereas the Romer model contains about 15 variables to be tracked (again depending on the exact version of the model in question)—a 50% increase.

In short: adding microfoundations to a model requires spelling out the agential factors that underlie the relevant social phenomena, and typically (though not necessarily) increases the assumptions the model is based on. With this in mind, it becomes possible to assess whether providing microfoundations—so understood—is something that economists should strive for (as is often supposed: see for exampleZahle & Kincaid [2019]; Elster [1982]; Watkins [1952]; Frydman & Phelps [2013]). The next section begins this assessment by critically evaluating the currently most popular, metaphysically-based ways of approaching this question.

3 Beyond Metaphysics: The Need for a Reconceptualization of the Debate

In the recent literature, the major (though not the only: see for example Kincaid [2015]; Little [1998]; Sugden [2016]) approach towards the question of the need for microfoundations in economics has been a metaphysical one: issues of social ontology are thought to be the key fulcrum with which to resolve this question (see for example Epstein [2015]; Epstein [2014]; Hoover [2010], [2015], [2001], [2009]; see also Sugden [2016]). (Put more broadly, the debate surrounding methodological individualism and holism in economics has come to be seen as ultimately resting on considerations of ontological individualism and holism.) There is no question that the arguments that have been given here are sophisticated and complex, and deserve close scrutiny (see for example Di Iorio & Herfeld [2018]; Hawley [2019]; Schaffer [forthcoming]). However, for present purposes, a detailed scrutiny of their strengths and weaknesses is not necessary. Rather, we here merely seek to show that, even if they are successful as *metaphysical arguments*, this is not sufficient for them also being able to resolve the *social scientific* dispute surrounding the need for microfoundations in economics: the relationship between methodological questions and even the best metaphysical arguments is not straightforward. In order to establish this, we begin with a brief summary of these metaphysical arguments.

In one of the most widely discussed treatments of the individualism / holism debate in the social sciences, Epstein ([2015], 2014]) argues that macro-level social entities or events (like a US Dollar or the Great Recession) neither supervene on nor are fully constituted by any individual-level grounding facts. To be a US dollar is not just a matter of being a piece of paper of a certain shape and size, with a particular print on it, and which was made in a specific place by a specific set of people. Rather, these 'grounding' facts also need to be 'anchored' in the *collective acceptance of* these facts being necessary to be a US dollar (the 'frame principle' of these grounding facts).

Because of this, Epstein concludes that ontological individualism is false. Since the grounding facts of social entities are anchored in macro-level facts—the collective acceptance of the relevant grounding facts—economic models that try to reduce macroeconomic facts (like the inflation rate) to facts about individuals (like their intertemporal consumption preferences) should be seen to be suspect. There is just more to the former than what is allowed for by the latter: we cannot hope to get at the true nature of the inflation rate (say) by reducing it to the choices of individuals. We need to treat it as a macro-level entity in its own right (Epstein [2015], [2014]).

Somewhat relatedly, Hoover ([2010], 2015], 2009]) also argues that reducing at least some macro-level entities to micro-level ones is not convincing. His argument is based on a distinction between synthetic and natural aggregates. Natural aggregates are just statistical summaries of their individual components. For example, the national interest rate might be seen as just the average of different interest rates charged by different banks. By contrast, synthetic aggregates involve more radical transformation of their individual components—they change the metaphysical nature and measurement of their components. For example, the national rate of unemployment goes beyond counting the number of people currently not in paid labour, and includes facts about the expected future time-path of the economy, the nature of 'paid labour', and what we, collectively, should take as the natural growth rate of the economy (Reiss [2001]). That is, synthetic aggregates add something to their individual components, and thus push the resulting aggregation away from a mere addition of the individual components. (In this way, the additional elements of synthetic aggregates bear a resemblance to the frame principles in (Epstein [2015]).)

Hoover ([2010], 2015], 2009]) then goes on to argue that since a number of macroeconomic models take such synthetic aggregates to be causally efficacious, we should not attempt to reduce them to individual decisions only. After all, it is in the very nature of synthetic aggregates that they transform their components in the process of aggregation. If synthetic aggregates only played unimportant roles in economics, and could thus easily be excised from economic models, the fact that they cannot be easily broken into individualistic components would not matter greatly for the plausibility of microfoundationalism. However, given that they do play an essential role in some economic models—by making causal differences, they must be taken at face value: as genuine macro-level entities.

As just noted, qua metaphysical arguments, these are sophisticated accounts of the issues that raise many interesting questions. However, here, we want to point out a key limitation of these arguments. This limitation concerns the fact that the connection between metaphysical arguments like these and (social) scientific practice is not clear (a point that has also been noted for example by Sugden [2016]; Di Iorio & Herfeld [2018]; see also Maudlin [2007]). Importantly, this limitation goes beyond the claim that economists need to be seen as instrumentalists whose methodological goals reduce to the prediction of behaviour, without regards to the truth of the modelling assumptions employed (Friedman [1953]; Gul & Pesendorfer [2008]; see also Reiss [2012]). The latter viewpoint is now widely seen as unconvincing for a number of different reasons (see for example Hausman [2012], [2008]; Angner [2018]; Glimcher et al. [2005]). Rather, the point here is that the relationship between the metaphysical truths about economic reality and the best modelling approach towards that reality is not straightforward. This is so for two reasons.

First, economic models (and scientific models in general) are constrained by and embedded in practical considerations beyond metaphysical truths (Morgan [2012]; Morgan & Morrison [1999]; Cartwright [1999]; Weisberg [2013]). They are *tools* with which to investigate economic reality. As such, their usefulness is not strictly correlated with their metaphysical accuracy. Most obviously, models may need to involve idealizations and abstractions: despite the fact that a given economic phenomenon *P* may have some metaphysical feature *F*, an economic model may be more compelling if it assumes that *P* does *not* have *F*. A major—though not necessarily the only—reason for this is that issues surrounding the ease of use of a model are key to the latter's defensibility (Weisberg [2013], [2006]; Levins [1966]; Odenbaugh [2006]; Orzack & Sober [1993]). There is just no reason to think that an accurate representation of *P* is necessarily the best or only base for a useful model of *P* (Cartwright [1999]; Morgan [2012]; Morgan & Morrison [1999]; Wimsatt [2007]; Weisberg [2013]).

Of course, ceteris paribus, accurate models are preferable to inaccurate ones. However, the key point to note is that other considerations also matter (Levins [1966]; Weisberg [2006]; Odenbaugh [2006]; Orzack & Sober [1993]). Given that economic reality is complex, epistemically quite opaque, and involving many uncertainties, the fact that an account of economic reality is metaphysically compelling need not, by itself, imply that it is also socially scientifically compelling. In particular—to foreshadow some remarks that will be made clearer below—such an account may be overly complex given the kind of data at our disposal. We may be able to make better sense of economic reality by considering a simpler or only partially accurate account of it.

This point is strengthened by the second reason for why metaphysical arguments are only tendentiously connected to (social) scientific practice: namely, the fact that we generally are surer about our social scientific modelling needs than about the metaphysical nature of (social) reality. Metaphysical arguments tend to be controversial, and the present case is no exception. For example, while Hawley ([2019]) is generally very sympathetic to Epstein's ([2015]) approach, she also argues that the frame / grounding distinction it relies on is not convincing. Much the same holds for Schaffer ([forthcoming]). Relatedly, the metaphysical discussion surrounding the nature of supervenience relations—a key element of Epstein ([2015], 2014]) and Hoover ([2010], 2015], 2009])—is subject to much controversy (see for example Shapiro [2004]; Funkhouser [2014]).

Of course, there is controversy surrounding many arguments in many areas of philosophy and the social and natural sciences. However, metaphysical arguments, by their very nature, are particularly prone to this kind of controversy. These arguments deal with issues that cannot be straightforwardly tied to empirical facts (which is precisely what makes them metaphysical after all), and there are thus fewer constraints in assessing them.[6] This is not to say that anything goes as far as these arguments are concerned (see for example Sider [2012]). Rather, our point is that, in cases where there is a conflict between (social) scientific, practice-based reasons *for* modelling a given phenomenon *P* as *X*, and metaphysical reasons *against* modelling *P* as *X*, it is far from obvious that the latter should win out.

[6] This is a classic point in the literature (see for example Carnap [1950]; Sidelle [2009]). It does not imply that there is no connection between metaphysics and empirical considerations whatsoever (see for example Maudlin [2007]); it just implies that this connection is tendentious at best.

Because of these two points—that is, the independence and strength of methodological, practice-based reasons for modelling social scientific phenomena in certain ways—resting the attack on the need for microfoundations in economic modelling on metaphysical / social ontological considerations, no matter how sophisticated these considerations are, is unlikely to end the debate here. Despite the best efforts of Epstein and Hoover (and other arguments like theirs) microfoundationalist approaches may well continue to be thought viable and defensible modelling strategies in economics. For this reason, we propose a reconceptualization of this debate that makes these methodological considerations central. That is, instead of asking whether microfoundations are metaphysically compelling, we are asking whether they make for good *methodological practice* when it comes to economic modelling. While reconceptualization is also hinted at by other authors (Kincaid [2015]; Sugden [2016]), they develop this idea very differently from how we do here.[7] In particular, our argument turns on the methodological principle that overly complex models are to be avoided, unless the increase in complexity is sufficiently compensated by a better fit to the data.[8] The next section makes this clearer.

4 A Methodological Reconceptualization of the Microfoundations Debate

Our reconceptualization begins by emphasizing a point that, while noted before (see for example Zahle & Kincaid [2019]; Sugden [2016]), is not always given the attention it deserves.[9] This point consists in the fact that the question of whether economic models require microfoundations should not be seen to be an either / or question, but to consist of two *graded* questions: (a) *when* do

[7] Indeed, this point may even be quite in line with Hoover's own thoughts on these issues (personal communication).
[8] Note also that this is not a point about *explanation*—the issue is not whether macro-models can identify causes, say, and thus provide causal explanations. The issue is whether and when, for reasons surrounding good modeling practice, we should require economic models to have microfoundations. See also note 3.
[9] Indeed, a related point has also been made in the literature surrounding explanatory individualism / holism: see, for example, T. Jones ([1996]).

economic models require microfoundations?; and (b) *how deep* do these microfoundations need to go? Question (a) is a reflection of the fact that there is no reason to think that either all economic models need microfoundations or that none do. It is possible that, sometimes, macro-entities can and should be seen as merely epiphenomena or the decisions of individual economic agents (say), but, at other times, they should be treated independently of the latter.[10] Question (b) is a reflection of the fact that it is not entirely obvious what the choice theoretic 'bedrock' is on which all economic models is meant to rest. Are *firms* genuine economic agents of their own, or do these need to be reduced further, too (see for example Coase [1937]; Williamson [1979]; Nelson & Winter [1982]; Winter [1988]; Schulz [2016], [2020])? What about government entities like central banks? What about households? Again, there need not be one answer that holds for all cases—sometimes, it may be appropriate to see firms as fully autonomous, bedrock economic agents of their own, at other times, this may not be the case.[11] The microfoundations debate really comes down to the fact that we are still lacking practice-focused answers to these questions.

Making explicit that the microfoundations debate is gradualist in nature is important, as it paves the way for the next—and central—step in the reconceptualization of this debate. This step returns to the point made in section 2 that the extent to which an economic model is microfounded will tend to correlate with the *complexity* of that model: endogenizing aspects of a model typically means adding assumptions—parameters or variables—to that model. As noted earlier, this correlation is not necessarily perfect (that is, it need not be the case that, in every case, the more microfounded a model is, the more complex it is), but it is likely to be positive nonetheless. It is

[10] Note that this is also implicit in Hoover's distinction between natural aggregates—which can be reduced to their constituents—and synthetic aggregates—which cannot.
[11] Epstein ([2015], 2014]) also notes that it is not obvious where the line between micro-level entities and macro-level entities should be drawn.

now necessary to expand the discussion of section 2 by distinguishing two aspects of this increase in complexity.

The first aspect concerns the fact that microfounded models are likely to be computationally less tractable. By adding assumptions—parameters or variables—to a model, that model is likely to become harder to solve analytically; it may even turn out that computational approximations to the solutions of the model are more difficult to obtain. So, for example, linear models might become non-linear models, and the computing power necessary to obtain approximations to the solution of microfounded models might increase beyond that of most current computers (for more on this sort of issue, see for example Lenhard [2019]; MacLeod & Nersessian [2018]; Winsberg [1999]; Lenhard & Winsberg [2010]; May [1974], [1972]).

However, while undoubtedly important and worthy of further discussion, this aspect of the increase in the complexity of (typical) microfounded models is less central in the present context. This is so for two reasons. On the one hand, it is less well studied, and its methodological implications are therefore not as well known. How methodologically problematic are increases in computational intractability? How do these computational considerations trade-off against other good-making features of models? As yet, it is not clear how to answer these questions (though see also Lenhard [2019]; Winsberg [1999]). On the other hand and most importantly, there is already much to say concerning the second, structural aspect of the complexity increase of microfounded models. The latter is therefore in focus here; it can be combined with a detailed treatment of the methodological implications of increases in computational intractability at a later date.

The second aspect of the increase in complexity of microfounded models concerns the basic fact that such models typically contain more assumptions—parameters or variables—than non-microfounded models. This point was already mentioned in section 2 above, but it is now important to make the details of this increase in complexity slightly more precise.

In general, this increase will *not* be akin to the one involved in moving among nested models in the literature on model selection (Burnham & Anderson [2002]; Abraham & Ledolter [2006]; Hitchcock & Sober [2004]; Rochefort-Maranda [2016]; Forster & Sober [1994]). In particular, the increase in complexity that comes from endogenizing the national savings rate is not like moving from a linear model ($y = ax + b$) to quadratic model ($y = a'x^2 + b'x + c'$)—that is, it does not concern two models, one of which can be obtained from the other by setting some of its parameter values to 0. After all, as just noted, the issue here (normally) is not one of simply *adding* micro-parameters or variables to an economic model; the issue is more one of *translating* macro-parameters or variables *into* micro-parameters or variables.[12] Because of this, the complexity increase in the case of microfounded economic models will typically be more in line with comparing *non-nested* models with different sets of parameters. Two versions of this kind of comparison are central in this context.

On the one hand, we could be comparing a model with one parameter for a given aspect of economic reality with one that has many parameters for this aspect of economic reality. So, instead of modelling the national savings rate with one exogenous parameter s, we could also model it with separate savings rates s_i for each consumer i, and which are then averaged. This is a familiar case in many sciences (see for example Forster & Sober [1994]; Hitchcock & Sober [2004]). For instance, this is exactly parallel to the case in many applications of phylogenetic inferences, where we can either assume that all branches of a phylogenetic tree have the same length (that is, that the chances of evolutionary changes happening between successive branching events are equal

[12] Of course, it is possible that some endogenous models are strictly nested in non-endogenous models. The point in the text is just that this is will not *generally* be so.

throughout the tree), or assume that all branches have different lengths (that is, that the chances of evolutionary changes happening between successive branching events are different in different parts of the tree) (Felsenstein [2004]).[13]

On the other hand, we could be moving from a linear model in two dimensions ($y = ax + b$) to one in three ($y = a'x + b'z + c'$) (that is, where the y intercept b has been endogenized with another linear function $b = b'z + c'$). For example, in a model with an exogenous savings rate s, we could replace this rate with a *function* that takes the consumers' rates of intertemporal substitution for granted, and which embeds the latter in a particular utility function, the maximization of which leads to the national savings rate s. In this way, by endogenizing the y intercept, we are both increasing the number of parameters and the number of dimensions of the model: we move from a model with two parameters (a and b) and one independent variable (x) to one with three parameters (a', b', and c') and two independent variables (x and z).[14]

More generally: by endogenizing a model, we are likely (though not guaranteed) to add parameters or variables to the model—apart from also making it less computationally tractable. This matters, as, *ceteris paribus*, less complex models are methodologically preferable to more complex ones (Hitchcock & Sober [2004]; Forster & Sober [1994]; Rochefort-Maranda [2016]).

[13] Note also that this is very different from Hoover's point laid out earlier. On the one hand, the issues here cross-cut Hoover's distinction between natural and synthetic aggregates. For example, if the savings rate of the individual consumers can be taken to be part of the micro-base of an economic model, then we could endogenize the natural savings rate by averaging over the savings rate of the individual consumers. In that case, though, we would still be replacing one parameter—that national savings rate—with many parameters—the savings rate of consumer 1, the savings rate of consumer 2, etc. On the other hand, the issues here are anyway methodological, not metaphysical. The issue is not that, typically, we *cannot* reduce macrophenomena to microphenomena; it is merely that doing so typically increases the complexity of the model.

[14] While we could of course define a new variable $w = x + z$, this will not always be theoretically meaningful. At any rate, this would still amount to adding an extra parameter b''.

Other things being equal, models with fewer parameters or variables are methodologically superior to ones with more: model simplicity is a methodological virtue.[15]

The main reason for this is that more complex models (that is, models with more parameters or variables) have more degrees of freedom in fitting to a given set of data. Assuming that the data have been generated by a probabilistic process—a nearly universally plausible assumption—this makes it more likely that the model 'overfits' the data. The model will fit the probabilistic noise of the generating process as well as its core, informative part, rather than just fitting the latter (Burnham & Anderson [2002]; Hitchcock & Sober [2004]; Forster & Sober [2011], [1994]; Schwarz [1978]; Rochefort-Maranda [2016]). In other words, more complex models are in danger of missing the forest for the trees: they fail to exclude the noise in the data generating process, and thus fail to get an accurate representation of the 'signal' of that process.

This also leads to a second reason for why more complex models are methodologically inferior to less complex ones. Because of the fact that they are less likely to hone in on the signal of a given set of data, they are also less likely to accurately predict unknown (future) data. In order to make concrete, empirically testable predictions, abstract economic models generally need to be fitted to a given data set. Given that more complex models are more likely to confuse signal and noise in such a data set, more complex models have less guidance for the prediction of future data. Their predictions are biased by being based too much on the noise in the initial data set that was used to ground assignments to the open parameter values in the model. Put differently, by getting an overly

[15] For a more general account of the value of appeals to simplicity, see Sober ([2015]). Note also that T. Jones ([1996]), in assessing the debate surrounding explanatory individualism, appeals to simplicity (among other pragmatic considerations). (However, the way the appeal to simplicity is spelled out in that paper is quite different from the way it is done here.)

good fit to the current data, more complex models are likely to end up with underfitting future data (Forster & Sober [1994]; Zucchini [2000]; Hitchcock & Sober [2004]).[16]

In short, both for misrepresenting the core features of the data generating process and for failing to be predictively compelling, more complex models are methodologically suspect relative to less complex ones—*ceteris paribus*. The *ceteris paribus* qualification here is important, in that it is (of course) not the case that a simpler model is *always* preferred to a more complex model. In particular, the model's goodness of fit to the data also matters: we want our models to fit any given data set as well as possible. What this means is that the key point here is *not* that we should always opt for the simplest available model. Rather, the upshot to take away from these points is that, from a methodological point of view, our task as researchers is to *balance* a model's goodness of fit with its complexity (Forster & Sober [1994]; Hitchcock & Sober [2004]; Sober [1988]; Rochefort-Maranda [2016]).

Now, it needs to be noted that the nature of this balancing is a point of contention in the literature, and might well depend on the details of the case. For example, the Akaike Information Criterion (AIC) penalizes extra parameters slightly differently compared to the Bayesian Information Criterion (BIC), and both of these differ from what Likelihood Ratio (LHR) tests call for (Zucchini [2000]; Bretthorst [1996]; Forster & Sober [2011]; Burnham & Anderson [2002]; Schwarz [1978]; Goodman & Royall [1988]; Royall [1997]; Stone [1974], [1977]). Similarly, exactly what is required for this trade off to be made also differs among model selection methodologies: AIC requires strictly nested models, whereas BIC or LHR-based methodologies

[16] More complex models are not *guaranteed* to overfit the data, and neither are they *guaranteed* to be predictively poor (Hitchcock & Sober [2004]; Rochefort-Maranda [2016]). Whether they are depends on the circumstances in which they are constructed, and the details of the data generating process. This is an important point to which we return momentarily.

allow for comparisons among non-nested models (like the ones sketched above) (Burnham & Anderson [2002]; Abraham & Ledolter [2006]; Smith [1992]).[17]

However, for present purposes, these differences ultimately are not greatly important. What is key here is just that it is widely acknowledged that there *is* this trade-off between goodness of fit and model complexity. That is, what matters here is that it is widely acknowledged that, holding goodness of fit fixed, increasing model complexity is to be avoided. Or, put differently, it is widely acknowledged that increases in model complexity need to be compensated for by sufficiently strong increases in the goodness of fit of the relevant model. The exact reasons behind the acceptance of this trade-off can be left open here: this paper takes the current statistical-methodological consensus as given.[18]

Noting this is crucial here, as it brings the methodological reconceptualization of the microfoundations debate to its conclusion. As just noted, models with microfoundations will tend to be more complex than those without. They are likely to contain extra parameters or variables that are abstracted out in the less microfoundationalist model. (Importantly also, if a particular microfoundationalist models is less or equally complex to a non-microfoundationalist model, the following steps need to be adjusted accordingly, but the overall methodological lesson remains the same.) Second, given this, and given the fact—also just noted—that, ceteris paribus, less complex models are methodologically preferred to more complex models, we need to ask if the extra complexity of microfoundationalist models is needed, given the fit to the data that the model can

[17] Also, the *statistical* problem of model selection—which concerns precisely defined sets of equations—and the *economic* problem of model selection—which concerns more loosely specified sets of equations—are slightly different from each other. This means that the latter needs to be translated into the former, thus introducing another layer of complexity here. However, this does not fundamentally affect the conclusions derived in the text: the balancing of complexity and fit in theoretical economics may often be more complex than in straightforward statistical-econometric situations, but it still needs to be done.

[18] In this way, this paper avoids Sober's ([2002]) charge that some model selectionist frameworks—like likelihoodism—lack an epistemic foundation.

achieve. If this fit is not significantly better, we have a reason to avoid adding microfoundations to the model. By contrast, if the fit *is* much better, we *have* a reason to add these foundations.

Now, as just noted, the technical details of exactly *how* to make the comparison of the goodness of fit / complexity relationship of different models is complex, and will need to be assessed on a case-by-case basis. However, for presented purposes, this is not central. Rather, the key point here is just *that* we should make such a comparison. Put differently, the key point here is that, from a methodological point of view, the question is not whether, metaphysically, macro-entities need to be given microfoundations: the question is whether adding these microfoundations sufficiently improves a model's ability to fit data so as to compensate for its increased complexity. That is, we are happy to admit that, in some cases, all of the relevant macro-entities can be spelled out in terms of the decisions of individual economic agents. However, this does not mean that it is also *methodologically reasonable* to do so: for spelling out the microfoundations of these macro-entities might increase the model's complexity to such an extent that it fails to be able to accurately represent the signal in noisy data, and thus becomes less predictively successful. There is just no a priori reason to think that all microfounded models must fit the data significantly better than all non-microfounded models. This is not the case in other sciences, and there are no grounds for thinking that economics is an outlier in this respect.

Again, the comparison with the situation in many phylogenetic inferences is telling. We may agree that, in reality, no two branch lengths are exactly the same—but this does not mean that we should always treat them as being different. Sometimes, assuming that they are the same simplifies the model without leading to major losses in its ability to fit the data (Felsenstein [2004]). The same is true in economics: it may be true that, *metaphysically*, the national rate of technological progress should be seen to be the consequence of the investment, production, and consumption decisions of individual economic agents. However, this does not mean that treating it in this way is also *methodologically* warranted, given the increase in complexity it comes with. Note that our point here is not to argue that Romer's ([1990]) model fails to improve on Solow's. Rather, our point is that, from a methodological point of view, it matters whether the increased complexity of Romer's model is compensated by a sufficiently better fit to the empirical data—not by the question of whether providing microfoundations for the Solow growth model is *metaphysically possible*. Two more general lessons about this point deserve to be made here.

First, this way of approaching the microfoundations debate is very much in line with the point noted earlier that this debate should not be seen to be an either / or matter. In particular, there is no reason to think that no microfoundationalist model will ever fit the data sufficiently better to warrant its extra complexity: some such models may well do so. Similarly, it is possible that intermediate cases exist, where some microfoundations are found to lead to the best combination of fit and complexity, but full microfoundations or no microfoundations do not. So, for example, it may turn out that, methodologically, it is best to endogenize the national savings rate, but not to endogenize the rate of technological progress. In this way, the gradualist ground of the microfoundations debate can be placed on rigorous and clear footing.

Second, it is again important to note that the upshot here is not a return to a pure instrumentalism of the sort defended, for example, by Friedman ([1953]) (see also Gul & Pesendorfer [2008]). Rather, our contention is to make the microfoundations debate a *methodology*-first debate. By focusing on the point that more microfounded models tend to be more complex, we obtain a methodological fulcrum with which to approach the debate surrounding the need for microfoundations in economics. Since more complex models are in danger of overfitting the data, we should opt for models with extensive microfoundations only to the extent that these increases in complexity are compensated for by a sufficiently better fit to the data. While, as noted earlier, this is indeed likely to also lead to better predictions, the latter is merely an *implication* of our arguments, and not their central claim. Our point is that there is a trade-off between fit and complexity, and not merely that the we ought to look for models with the best predictions. Of course, we agree that the latter is useful. However, we also want to argue that, in order to determine which model has the best predictive outlook, both a model's ability to fit a given set of data and its complexity need to be taken into account.[19]

5 Conclusion

We have argued that the currently popular metaphysically-based way of approaching the debate surrounding the need for microfoundations in economics fails to resolve this debate. Given that the relationship between metaphysics and social scientific practice is not straightforward, a compelling metaphysics appears neither necessary nor sufficient to ground a compelling social scientific methodology. For this reason, we have then argued that the debate surrounding the need for microfoundations in economics needs to be reconceptualized. Adding microfoundations to an economic model—that is, endogenizing some of its assumptions—typically (but not necessarily) adds parameters or variables to the model, and thus makes it more complex. However, more complex models are, ceteris paribus, methodologically suspect. In particular, a model's goodness of fit needs to be traded-off against its complexity (though it is a matter of some controversy exactly how to make this trade-off). In this way, we arrive at a new way of looking at the microfoundations debate: microfounded models are defensible to the extent that they achieve a significantly better fit to the data. The issue is not whether, for metaphysical reasons, economic

[19] Note also that not all model selection frameworks in fact focus on a model's predictive success (Schwarz [1978]; Goodman [1999]; Goodman & Royall [1988]; Stone [1974], [1977]).

models need to be fully microfounded. Rather, the issue is when which degrees of microfoundations are methodologically most compelling.

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