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# Reductionism in Economics: Intentionality and Eschatological Justification in the Microfoundations of Macroeconomics

Kevin D. Hoover\*†

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Macroeconomists overwhelmingly believe that macroeconomics requires microfoundations, typically understood as a strong eliminativist reductionism. Microfoundations aims to recover intentionality. In the face of technical and data constraints macroeconomists typically employ a representative-agent model, in which a single agent solves the microeconomic optimization problem for the whole economy, and take it to be microfoundationally adequate. The characteristic argument for the representative-agent model holds that the possibility of the sequential elaboration of the model to cover any number of individual agents justifies treating the policy conclusions of the single-agent model as practically relevant. This eschatological justification is examined and rejected.

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**1. Reductionism and the Practice of Macroeconomics.** A value of the philosophy of science for a field such as macroeconomics is that it brings into the light the nature of some of the unreflective practices of macroeconomists and allows them to be held up to critical scrutiny. I would like to consider macroeconomics from a broadly pragmatic perspective. It is “pragmatic,” first, because I am concerned with practice; second, and more specifically related to traditional pragmatism, because I find the meaning of terms in their implications for action; and, finally, because I see some versions of pragmatism as committed to a perspectival realism in which truth is not called into question, but truths are always expressed from a point of view (Hoover 2012c).

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The American philosopher John Dewey (1925/1958, chap. 1) compares the best practices of science with the typical practices of the philosophy of his day. He argues that science begins in experience conceived as ordinary human interactions with the world, which it tries to account for through a process of abstraction and creation of theories, or “the refined, derived objects of reflection” (3–4), and then brings those objects of reflection to bear to provide some level of mastery over the original experience—that is, some level of understanding and some instruments of control (Godfrey-Smith 2007). Dewey’s criticism of the practice of philosophy was that it took the first step—it moved from experience to the refined objects of reflection—but too rarely took the second step of bringing its theoretical constructs back in contact with experience. Too often, philosophy ended up assigning a superior reality to its constructions and explaining away, rather than explaining, the experience from which it started.

Reductionism generates refined objections of reflection in spades. One gloss on reductionism is that the “special sciences”—to use a loaded and invidious term—are particularizations or localizations of some more fundamental science and stand in a chain of dependence. An extreme version of such a view can be iterated until the most fundamental science is reached: macroeconomics reduces to microeconomics, which in turn reduces to psychology, which reduces to biology, which reduces to evolutionary biology, which reduces to chemistry, which reduces to atomic physics, which ultimately reduces to the most fundamental physics, whatever that turns out to be. Neither the nature of the dependence relationship for reductionism nor the scientific interest in establishing it is clear. Some possibilities include

- A. *elimination*: if we fully understood psychology, then we would do without economics altogether; eliminativism frequently allows that the “higher level” theory might employ more convenient or more manageable concepts but would maintain that they are strictly speaking dispensable;
- B. *ontology*: we just want to establish that economics does not trade in any mysterious or inexplicable stuff;
- C. *explanation*: for example, economic categories and concepts are not to be eliminated, but psychology explains why they are what they are.

I take the pragmatic view to reject eliminativism (A) to the degree that it hangs on the qualifier “if we fully understood.” On the one hand, eliminativism may express a commitment to a promise without evidence for its future redemption. For example, if we in fact do not (yet?) fully understand how psychology underwrites economics, then the “if” in the qualifier merely marks our faith-based commitment.

On the other hand, especially with the caveat that we could retain higher-level concepts for convenience, the dependence relationship may collapse into a request for an explanation of why the higher-level categories and concepts are what they are (C). But this second case is implicitly rejects eliminativism. If the reducing theory is truly more basic, then the enduring convenience of the reduced theory is not something to be taken for granted but stands in need of explanation. The problem here in Deweyan terms is that, if we can give no account of the convenience, then we fail to tether the refined object of reflection—the reducing theory—back to the experiences that motivated the reduction in the first place. If we can give such an account, then we are no longer eliminativists.

I take the pragmatic view—at the least—to accept only a very qualified version of ontological reductionism (B). An extreme view that asserts that all economics is reducible to the most basic physics *because at root there is nothing else but the physical* without actually constructing the reduction strikes me as either question begging or a religious or metaphysical (in its common pejorative sense) commitment. No one knows how to reduce economics to fundamental physics—or even to psychology—in a manner that preserves and accounts for its target problems and its explanatory success. So, other than table-thumping or faith, how can we be sure that the ontology of physics or psychology is sufficient? Ontological claims are about what there is in the world. They seem to be empirical in a broad sense.<sup>1</sup> It is, therefore, hardly consistent with an empiricist commitment for ontological claims to outrun the concrete achievements of a program of grounding higher-level concepts in something presumed to be more basic.

Before we stray too far from economics, let me narrow my focus. My concern will not be anything so sweeping as the claim that economics reduces to psychology, much less to fundamental physics. Rather, I want to address a reductive claim within economics itself—namely, the claim that macroeconomics reduces to microeconomics. *Macroeconomics* is the study of whole economies (national or global) without particular attention to individuals that those economies comprise. It is, thus, typically an analysis of aggregated data: gross domestic product (GDP) rather than individual production or income, price indexes such as the GDP deflator or the consumer price index rather than the prices of particular goods and services, the market rate of interest or the term structure of interest rates (yield curves) rather than the interest rate contracted on specific loans or financial assets. *Microeconomics*, in contrast, focuses on the individual worker or consumer

1. Peirce (1931, para. 184), following Bentham, refers to sciences that employ the empirical evidence of ordinary life as *cænoscopic*. Cænoscopic science, he holds, includes metaphysics.

and the particular firm or product. Microeconomics trades in such familiar concepts as supply and demand—and somewhat less familiar ones such as cooperative games. Macroeconomics is the servant of business-cycle forecasting, the analysis of economic growth, and monetary and fiscal policy.<sup>2</sup> By far the greatest number of practicing macroeconomists believe that there is a reductive dependence of macroeconomics on microeconomics known universally as the *microfoundations of macroeconomics*.<sup>3</sup> The issue of reductionism is not merely a philosopher's concern. Economists themselves see microfoundations as doing real work in economics by restricting which theories or models are acceptable and conditioning their empirical implementation (Hartley 1997; King 2012). Mainstream economics accepts an eliminativist reductionism that, ideally, would offer an agent-by-agent account of the economy as a whole. Such an ideal is unattainable and most “microfoundational” models rely on the device of a *representative agent* whose decision problem stands for the whole economy, the use of which is justified as the first step toward the ideal. Importantly, the representative-agent model is taken to be practically relevant because it is an early stage in the progressive elaboration of the microfoundational model that ultimately would reach the ideal. I challenge the claims that microfoundations are required for a successful account of the economy as a whole, that the representative-agent model provides microfoundations, and that the representative-agent model is entitled to practical deference because of its relationship to yet-to-be-developed detailed microfoundational models. I also argue that the same considerations that appear to motivate the representative-agent model warrant an explanatory reductionism that, in contrast with the eliminativist program, is compatible with an autonomous macroeconomics.

The argument proceeds in stages. First, as a matter of historical fact and disciplinary self-conception, economics is grounded in individual decision making. Macroeconomics, as an account of the economy as a whole, immediately raises the question of the relationship of macroeconomics to microeconomics. In fact, the question spawned a number of different “microfoundational programs” (Hoover 2012b). The earliest macroeconomists were inclined to the view that only an abstract or idealized account of the relationship could be provided and that, therefore, practical macroeconomic

2. The classification as microeconomics or macroeconomics of disaggregated general equilibrium, which models the decision problems of individual agents embedded in a comprehensive integrated system, is ambiguous to the point that it has proved difficult to pin down in official classifications of economic literature by the American Economic Association (see Cherrier 2014).

3. Janssen (1993, 1998, 2008) provides overviews of the issues surrounding microfoundations.

problems compelled economists to work with aggregates organized in a causal framework quite close to that of physical mechanics. The second generation of macroeconometric modelers sought both to create consistency between the analysis of aggregates and the microeconomic analysis of individual people and firms and to improve the empirical performance of their models through an investigation of analogies and implications of individual behavior and disaggregation of empirical quantities. They were driven by a *reductive impulse* to relate the aggregates to intentional behavior to the degree that it was practically possible, given the restrictions of available data and theoretical analysis. There was a top-down approach in which the need for empirical results in support of practical policy advice was the foremost consideration and limited the degree of practicable disaggregation.

The second stage is to examine the challenge to the top-down strategy embodied in the so-called Lucas critique, which argued that the top-down approach was impossible because it failed to integrate the implications of intentionality thoroughly and consistently into macroeconomics analysis. The Lucas critique called for a radical *reductionism*—a bottom-up approach in which the behavior of aggregate quantities was derived deductively from the characterization of individuals.

The third stage is to note that the advocates of reductionism face exactly the same conflict between the desire to account for intentional behavior and the needs of practical policy: the Lucas critique points to radical reductionism, but the conceptual and empirical resources for such reductionism simply do not exist. The preferred strategy is to offer highly simplified models in which a single agent or a few types of agents are modeled as individual optimizers yet take economy-wide aggregates as both the resource constraints and the targeted choice variables in their optimization problem. These so-called representative-agent models now dominate mainstream macroeconomics. It is easy to dismiss the representative-agent model as offering only a simulacrum of microeconomics, since no agent in the economy really faces the decision problem they represent. Seen that way, representative-agent models are macroeconomic, not microfoundational, models, although macroeconomic models that are formulated subject to an arbitrary set of criteria (Hoover 2001b, chap. 3). But this view of them has not proved persuasive to economists. Rather, economists have been persuaded by the argument that the representative-agent model is the first step toward a detailed microfoundational model—a radical reductionist model—and that because the remaining steps can be articulated in principle, whatever the practical barriers, the policy and predictive consequences of the representative-agent model itself are entitled to practical deference. I maintain that this argument, which I call *eschatological justification*, is specious but that its attractiveness to economists arises from the need simultaneously to respect

the intentional character of economics and to provide something like the “billiard-ball” causality implicit in the earliest macroeconomic models.<sup>4</sup>

## 2. The Problem of Microfoundations in the Origin of Macroeconomics.

The issue of the reducibility of macroeconomics to microeconomics began almost immediately with Ragnar Frisch’s introduction of the distinction in 1933 (see Velupillai 2009). The use of “microfoundations” as a name for the reduction was introduced only in the 1950s and gained currency only in the 1970s (see Hoover 2012b). Frisch (1933, 172–73) did not view microeconomics as foundational for macroeconomics. He was more concerned with the possibilities of practical analysis and did little more to investigate the relationship of the macroeconomic to the microeconomic (see Hoover 2012b).<sup>5</sup> At just about the same time, Jan Tinbergen, who shared the first Nobel Prize in economics with Frisch, constructed the first econometric models for whole economies using aggregate data (Morgan 1990, chap. 4).

Several important threads in the history of macroeconomics converge in the figure of Lawrence Klein. Klein joined an interest in theoretical Walrasian, agent-by-agent, general-equilibrium models with technical expertise in rapidly developing econometric techniques for empirical modeling and with a deep understanding of the architecture of the Keynesian analysis of the economy as a whole (an analysis that is now referred to as “macroeconomics,” even though Keynes never used that term; Klein 1947).

The macroeconomic modeling programs of Klein and Tinbergen from the 1950s on can be seen as treating economic analysis as an engineering problem. One goal was to increase the causal articulation of the models—that is, to obtain and model more and more disaggregated data. For example, rather than investment, a model might distinguish among types of investment, such as plant and equipment, structures, and inventories of finished goods, and each of these categories might be further refined, so that, for example, structures could be subdivided into residential, industrial, and

4. In referring to “billiard-ball” causality, I do not mean to oppose intentionality to causality in general but merely to indicate that, in making no meaningful reference to intentionality, the causal account implicit in these particular macroeconomic models is similar to the causal accounts employed in the physical sciences.

5. Frisch argued that an individual-by-individual, commodity-by-commodity “macrodynamic” model (essentially an intertemporal Walrasian general-equilibrium model) would be, at best, a theoretical abstraction that could never be usefully linked to data. A substantial effort after 1933 was directed toward microfoundational programs that employed the approach that Frisch explicitly rejected—in particular, toward models that aimed to generate Keynesian outcomes such as involuntary unemployment—within an agent-by-agent, mathematical general-equilibrium framework (Weintraub 1979). These programs are not our direct concern, since, as Frisch foretold, they have never had any substantial empirical or practical policy-relevant orientation (Hoover 2012b).



commercial. Following the path of greater and greater causal articulation, the models evolved from the 3–25 equation range to hundreds or even thousands of equations.

A second engineering goal was to use the models as a guide to management of the economy. Tinbergen (1956) developed an explicit methodology of policy evaluation in which the policy maker aimed at certain *targets* by choosing the settings of *instruments*. The macroeconomic model provided the machinery for conducting counterfactual analysis of the connection between instruments and targets.

Klein, Tinbergen, and other macroeconomic modelers were called “Keynesians” because of their having adopted the aggregative architecture of Keynes’s *General Theory* (1936), Keynes’s own skepticism of the macroeconomic project to the contrary notwithstanding (1939/1983). And Keynesian macroeconomics was the dominant approach until the early 1970s. The central challenge to the Klein/Tinbergen program was the “Lucas critique” (Lucas 1976/1981; see also Hoover 1988, chap. 8, secs. 8.3–8.4). Put broadly, Lucas’s point was that the aggregate relationships modeled by macroeconomists were the product of the behaviors of individuals. Those behaviors were intentional. And contrary to the implicit assumption of the engineering approach to policy, the policy maker was not an outsider to economy: not only did the policy maker react to data generated by intentional agents, but those agents themselves had every reason to try to understand and predict the actions of the policy maker and to incorporate those understandings and predictions into their behavior. The Keynesian policy modeler treated the economy as a causal mechanism that would invariably transmit the settings of policy instruments as causal stimuli to target variables as causal effects. Since policy was guided by preferred goals, policy actions were not random or *sui generis*. To the degree that they were systematic or predictable, the individuals in the economy would adjust their behaviors in light of the policy. Any change in policy (a change in the “policy rule”) was then likely to be met with a change in the relationship among the aggregate variables. Thus, contrary to the assumption of the Keynesian policy modeler, the relationships embedded in aggregate macroeconomic models would not be invariant to policy actions. Tinbergen’s target/instrument framework was bound to fail in models in which the articulation stopped at an aggregated level.

Lucas suggested that the path forward was to understand aggregate outcomes as the product of individual microeconomic decisions, taking only “tastes and technology” as given. Lucas’s program was eliminativist. While the idea of microfoundations for macroeconomics had been pursued in various ways since the 1930s, it is only with the Lucas critique that macroeconomists typically began to insist that models without microfoundations lacked scientific *bona fides* (Hoover 2012b). Lucas’s proposal is an explicit endorsement of radical reductionism. In his view, the entire scientific



enterprise of macroeconomics was an intellectual mistake, although one made under the duress of the Great Depression (Lucas 1987, 108). If the program of microfoundational reduction succeeds, he argues, “the term ‘macro-economic’ will simply disappear from use and the modifier ‘micro’ will become superfluous. We will simply speak, as did Smith, Ricardo, Marshall and Walras, of *economic theory*” (107–8).<sup>6</sup>

Lucas should not be interpreted as making an unusually vigorous expression of the reductionist impulse but as making an altogether stronger claim that Keynesian econometric models suffer from “fatal” flaws and are of “no value in guiding policy” (Lucas and Sargent 1979, 50; cf. Lucas 1980, 705 [esp. n. 80] and 712). Security lies not simply in seeking underlying mechanisms but in actually starting from individuals: “Notice that, having specified the rules by which interaction occurs in detail, and in a way that introduces no free parameters, the ability to predict individual behavior is nonexperimentally transformed into the ability to predict group behavior” (Lucas 1980, 711).<sup>7</sup> Lucas’s reductionism rapidly became standard among “new classical” macroeconomists, who reiterated his claim that the absence of individualist microfoundations was fatal for Keynesian aggregative macro-econometric models (e.g., Hoover 1988; Plosser 1989, 51; King 2012, chap. 6).

**3. Intentionality and Causality.** No economist really dissents from ontological individualism of the form that holds that individual behavior lies behind economic phenomena. Klein is the exemplar of the reductive impulse. He took microeconomics as essentially on the right track and paradigmatic of what economics is. He did not dissent from Lionel Robbins’s (1935, 16) famous definition: “Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses.”<sup>8</sup> The standard approach of microeconomics is to express for any par-

6. In fact, Smith and Ricardo spoke of *political economy* and not of economics. Lucas’s position is not that the topical or policy concerns typical of macroeconomics should disappear but only that the theoretical account of the phenomena that they address must be microeconomic. Twelve years later, the prominent new Keynesian economist Michael Woodford (1999, 31) declared the success of Lucas’s reductive program: “modern macroeconomic models are . . . derived from the same foundations of optimizing behavior on the part of households and firms as are employed in other branches of economics.”

7. Sargent (1982) provides a comprehensive statement of the reductionist position.

8. Robbins’s formulation is found in nearly every introductory microeconomics textbook today. And what I am calling the standard approach, which embodies that formulation, is general enough and widely seen among economists to be general enough, at one extreme, to cover game theory, which stresses the interaction of two or a small number of agents, each constraining the other, and, at the other extreme, perfectly competitive general equilibrium, in which each agent is taken to be small relative to the market and, hence, able to choose as if his own decisions did not affect the market. Lucas (1980, 711

ticular individual a fixed ranking of ends (a utility function or profit function) that is maximized with respect to a constraint expressing alternative uses (a budget or technology constraint). This approach defines the core of microeconomics. Klein simultaneously took standard microeconomic theory and aggregate Keynesian macroeconomic theory as each holding in its own domain. In his dissertation he explicitly criticized Keynes for failing to provide a detailed account of the relationship of individual economic decisions and aggregate outcomes (Klein 1947, 57). Later, Klein explored the foundations of aggregation theory (1946a, 1946b). His preferred approach was to view microeconomics and macroeconomics as both explanatorily successful and each employing a distinct set of conceptual categories. He then asked whether there was a formal aggregation procedure jointly consistent with the constraints of both conceptual structures (see Nelson [1984] and Hoover [2010] for a discussion of this approach to aggregation). The approach did not aim at elimination but at explanation of the connection (a type C reductive dependence; see sec. 1).

It soon became clear that there were practical and theoretical difficulties to approaching the relationship of macroeconomics to microeconomics through direct aggregation (e.g., Gorman 1953), and Klein turned his attention to a more workable program—namely, to the progressive causal articulation and disaggregation of macroeconomic models already described. Klein himself describes his attitude: “In contrast with the parsimonious view of natural simplicity, I believe that economic life is enormously complicated and that the successful model will try to build in as much of the complicated interrelationships as possible. That is why I want to work with large econometric models and a great deal of computer power. Instead of the rule of parsimony, I prefer the following rule: the largest possible system that can be managed and that can explain the main economic magnitudes as well as the parsimonious system is the better system to develop and use” (1992, 184).

Klein’s approach does not represent the abandonment of the goals of reduction. He and most of the Keynesians of the 1950s–70s continued to believe that, even if no detailed aggregation of microbehavior to macrobehavior could be worked out, incompatibility between macrobehavior and microbehavior was a blemish on their models. Every behavioral function of the macroeconomic model was subjected to an individual microeconomic analysis. The Keynesian macroeconomic modelers looked for models with a billiard-ball causal structure that would support policy advice—one in which a change in a policy instrument would be predictably and reliably related to a change in targeted outcomes. Causal and intentional accounts are

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n. 11) specifically notes the wide scope of microeconomics (e.g., game theory, as well as market models) with reference to his advocacy of microfoundations.

not in general at odds, but their goal was to discover the way in which intentionality conditioned the relationships among the variables such that, once known, explicit reference to the intentions of individuals in the economy (other than the policy makers themselves) would not be needed when offering causal analyses of the policy problem. The causal articulations that they sought were explicitly analogous to those appropriate for the physical sciences or engineering.

To take an example: Simon Kuznets discovered in the early 1940s that, in data aggregated by decades, a stable fraction of aggregate income was devoted to aggregate savings, while, in data within a decade, the fraction of savings rose as income rose. Every influential approach to this puzzle began with an analysis of the individual decision problem. The ultimately dominant approach of Kuznets and Friedman suggested that an individual should calculate the asset value of expected future income and set savings as a fixed fraction of *permanent income*, defined as the implicit income stream of the assets (Friedman 1957). The puzzle was resolved for the individual: all transitory income should be saved, linking the measured savings rate to the level of income year by year, but over decades, transitory income would average out, so that only the fixed savings rate from permanent income would be measured.<sup>9</sup>

Such individual analysis was not used to place direct restrictions on the aggregate relationships but was instead used qualitatively. Some examples:

- self-employed people typically have larger fluctuations year by year than wage earners, so it might prove to be a productive strategy to disaggregate consumption and income by employment status;
- durable goods (e.g., cars or washing machines) return their services over time and, hence, have an asset value, similar to investment goods—in effect, savings in a physical rather than financial form—while nondurable goods (e.g., food and electricity) are consumed very quickly, so it might be useful to model durable and nondurable consumption separately;
- the asset value of expected future income and the implicit income from those assets (financial and physical) depends on the rate of interest as a measure of opportunity cost, so it might turn out that interest rates should be a variable in any macroeconomic savings relationship.

The utility of these exercises was not supposed to be to provide a basis for direct aggregation but to provide an analogy between an individual's decision problem and an aggregate relationship that would allow the researcher

9. See Hoover (2012a) for a textbook account of the permanent-income hypothesis, its evidence, and implications.

to identify relevant variables and qualitative relationships and to expose underlying inconsistencies between macroeconomic relationships and individual behavior. Kuznets, Friedman, and others who took this approach did not attempt to provide any strict deduction of the properties of the aggregates from the microeconomic analysis of multiple individuals in any practical case.<sup>10</sup> The relationship is rather a qualitative analogy and would have to be tested empirically at the aggregate level before it would be accepted as compelling.

The reductive impulse is essentially a methodological attitude that we should look for the explanations of things by trying to articulate the mechanisms that make them arise and that we should continue the process of articulation as far as feasible and pragmatically useful. The appeal of reduction in biology, and even in mind sciences, is that reduced explanations eliminate teleology by purging intentionality. In economics, it is just the opposite: the appeal of reduction is to recapture intentionality. Some macroeconomic analyses make little or no reference to intentionality, and it is precisely for that reason that Lucas sees them as defective: the object of the Lucas critique is to recapture intentionality. Lucas and his collaborators and followers, who came to be called “new classical” economists, reject the notion associated with Klein’s reductive impulse that there is any way station between macroeconomics and the individual that could adequately account for the influence of intentionality on causal articulation.<sup>11</sup> The only stopping point is the individual agent.

What explains the appeal of intentionality as a theoretical desideratum for economics? The first consideration is familiarity: historically, economics began with a folk psychology. The second is success. It is common to denigrate the predictive and explanatory success of economics (e.g., Rosenberg 2012; Rosenberg and Curtain 2013). Often it is compared to the stunning success of some areas of physics. One argument in favor of a thoroughgoing reductionism is that all the greatest successes of natural science have employed a reductive strategy, so that we ought inductively to conclude that this is the way forward in social sciences as well. Aside from a general objection to the validity of inductions of this sort, the success of physics in its own domain is irrelevant to its success in a social domain. If, for example, I want to predict your route home from work, an intentional account, although it may ultimately fail, gives me some hope of succeeding, while an analysis that starts with physics (or even neurobiology) gives me none.

10. Notwithstanding Klein’s (1946a, 1946b) investigations of the theory of aggregation, which are not in fact applied to actual data either in the those articles or in later modeling exercises.

11. See Hoover (1988) for a comprehensive account of the new classical macroeconomics.

Intentional accounts are not examples of billiard-ball causation analogous to physically efficient causation. Yet the lesson from the example of the permanent-income analysis of savings is that the effort to explore the problem intentionally may help us to articulate constraints and relatively stable social structures that allow us to apply an analysis of efficient causation, setting detailed intentions aside as second order. This is not to deny the Lucas critique. Rather it is to suggest that its reach may be sufficiently moderated in aggregate data that there are useful macroeconomic relationships to model that are relatively invariant. It suggests that macroeconomics will always have a local character: the general templates of macroeconomics may be similar across a large variety of cases, but the quantitative details are likely to be country and temporally specific—a conjecture that is borne out by the experiences of macroeconometric modelers.

The Lucas critique is the hinge of the history of macroeconomics over the past 50 years. Early generations of Keynesians had preferred aggregate relationships that were compatible with microeconomic analysis, but they rarely—if ever—imagined that empirically relevant aggregate relationships were conceptually or feasibly logically deducible from microeconomic premises. The Lucas critique heralded a methodological revolution: after 1976, any model, analysis, or measuring framework was likely to be summarily rejected unless the researcher could offer a microfoundational account—that is, one grounded in individual choice, taking only tastes and technology as given—and a plausible, adequate microfoundational account was held to be proof against the Lucas critique.

**4. Against Eschatological Justification.** The microfoundational turn poses two interrelated issues for macroeconomics: first, the question of what counts as plausible, adequate microfoundations; second, the adequacy of economic science to support practical policy analysis. The strident logic of the Lucas critique implies an uncompromising reductionism. Limited empirical resources and the demands of practical policy require compromises. The new classical macroeconomics adopts a particular compromise, the representative-agent model, but treats it not as the abandonment of reductionism, which would amount to a strategy similar to Klein's, but as a model the practical conclusions of which are warranted by its being the first step in a thoroughly reductionist program. We now turn to examination of their strategy.

The short answer of the microfoundational reductionists to the question of what counts as plausible, adequate microfoundations is that those that adhere to the template of optimization of preferences subject to constraints are the right place to start. But that is a minimal and not very discriminating constraint. There is a fantasy vision of the connection of macroeconomics to microeconomics. It starts by assuming that we can model the decision problem for each agent in the economy. If this is done fully, then we need only

fill in initial conditions about the preferences of agents, the available technology, and the distribution of primary resources, and the model will recapitulate the actual economy or predict the future economy. Macroeconomic aggregates could be computed for this economy by simply following the data collection procedures of government statistics bureaus, but these aggregates would be seen to be epiphenomenal and not explanatory of any of the economic events of the world. This is the economist's analogue to Laplace's deterministic fantasy about the physical world. What makes it a fantasy is partly that no one imagines that it can be practically implemented. Indeed, I would conjecture that it is computationally impossible. But aside from those considerations, would the implementation of the fantasy be scientifically helpful?

Let me begin to answer that question by appeal to an analogy.<sup>12</sup> Imagine that we have an engineer who needs to lift a large object to the top of a building. One approach is to imagine an apparatus of ropes and pulleys that can be analyzed by the physical laws of simple machines that are found in the most elementary physics textbooks. The conceptual architecture of these laws refers to pulleys and cords but not to their material constituents. Practical engineering takes certain generic facts about the materials (e.g., stiffness and tensile strength) as constraints. There may need to be some degree of de-idealization—for example, taking into account that actual cords have thickness and weight. But beyond some level, the details do not matter. Pulleys might be fiberglass, metal, or wood; cords might be ropes or cables. The theory of simple machines provides a resource for the engineer that is both explanatory of the relevant facts and useful for prediction and counterfactual analysis.

Contrast this with a reductionist fantasy account. We go back to fundamental physics—let us say to Newton's laws rather than to relativity and quantum mechanics. We specify initial conditions at some earlier time—and we then do what? Provided that our commitment to reductionism has not turned us into determinists, we might search exhaustively over feasible interventions and then select from among the interventions that end up with a future in which our object is on top of the building. Here is the difficulty: how do we make sense of the notion of a feasible intervention, given that the various apparatuses can be instantiated through a wide variety of microstructures?

We may, perhaps, stumble upon a *recherché* or arcane mechanism that through just the right set of actions lifted the object without any familiar mechanism: snapping our fingers just so and at just the right time results in just the right cascade of molecular collisions that our object is lifted by air

12. The analogy is a variant of Hilary Putnam's (1975) argument that it is the geometrical and not the microconstitutive properties of square pegs that prevent them from passing through round holes, the diameter of which are equal to the lengths of the sides of the pegs.

pressure to the top of the building. Unless one is really a thoroughgoing determinist and all of that is preordained, including our own “intervention” of snapping our fingers just so and the “counterfactual” analysis itself, it is hard to imagine interventions fine enough to work in such a way. So, more plausibly, our investigation will conclude only when we discover interventions that generate from microstructures apparatuses that have a familiar macrostructure. Our search criterion over the intervention space amounts, then, to looking for pulley-and-cord-like machines.

The reductionist procedure is impractical, and, even in any fantasy that preserves a reasonable facsimile of the engineer’s intentional intervention, we end up appealing to conceptual categories (the abstract analysis of simple machines) that bear no useful conceptual relationship to the physical analysis of its constituent parts, except the *de minimis* one that actual machines must be instantiated in materials with physically adequate properties. The material prerequisites do not explain the mechanical advantage employed by the engineer. At most they mark out a class of materials in which to instantiate instruments that possess mechanical advantage.

That the physics of simple machines is conceptually distinct from the reductionist microdescription does not, I think, introduce any mystery or leave any unaccountable residuum. The physics of simple machines provides a resource to the engineer that the microphysical account fails to provide for conceptual, even more than practical, reasons.

Economics presents similar cases. A prime example is the general price level, from which we get economy-wide measures of inflation. It shares a name with the familiar prices of goods, but it is, in fact, not an object of experience but one of Dewey’s “refined objects of reflection.” While it is in practice measured by pricing bundles of representative goods, its dimensions are not even commensurate with the dimensions of the prices of particular goods (Hoover 1995; 2001a, chap. 5). The general price level bears a similar relationship to its constituent prices, as the pulley does to its constituent materials: the macro cannot in either case be instantiated without the micro, yet, there is a conceptual divide such that the macro provides conceptual resources that are not available from the micro in isolation. Fundamental macroeconomic theory relies on concepts such as the *real* values of GDP, consumption, the money stock, interest rates, and so forth. The general price level is the key concept involved in converting *nominal* or monetary values to real values, so that the same conceptual divide noted with respect to the price level is recapitulated between the familiar experiential nominal quantities and the refined objects that populate macroeconomics.<sup>13</sup> And just as with the problem of lifting a

13. For example, one way of measuring nominal GDP is to add up all the incomes of all the people in a country. Each component is directly observable from accounting records or tax receipts and is recorded in the national accounts in currency units. Real GDP con-



heavy object, the reductionist fantasy of the microfoundationalist fails to provide the conceptual resources for evaluating an economic stimulus or the monetary policy of quantitative easing or other of the typical macroeconomic concerns of policy makers.

No one really disagrees with the impracticality of either the physical reductionist's or the economic microfoundationalist's fantasies. Yet, the microfoundationalist's fantasy has a powerful hold on macroeconomists. They recognize that an agent-by-agent reconstruction of the economy is not feasible, but they argue that it is something that we could do "in principle" and that the in-principle claim warrants a particular theoretical strategy. The strategy is to start with the analysis of a single agent and to build up through ever-more-complex analyses to a whole economy. In the extreme case in which there is only a single agent, the microfoundationalist strategy is referred to as the *representative-agent model*. The representative-agent model is distinct from the Robinson Crusoe limit case of a microeconomic general-equilibrium model of an autarkic island economy. Rather than positing a very small economy of a one agent, in its empirical application, the representative-agent model posits a single agent who faces a decision problem of the same form as Robinson Crusoe but who, in contrast, takes all the resources (capital and labor) of the heterogeneous population of the national economy as his budget constraint and whose production and consumption choices are meant to correspond to the aggregate of their individual choices (see Hartley [1997] and King [2012] for histories and methodological analyses of representative-agent models).<sup>14</sup> A large number of empirical representative-agent models have been seriously proposed. They are widely held to be immune to the Lucas critique, because the representative agent is analyzed using the usual forms of microeconomics.

The underlying logic of the representative-agent model has rarely been either explained or explicitly defended. When pressed, advocates usually refer to the model as a first step along a path to a truly satisfactory model (e.g., Chari and Kehoe 2008). They point to recent work on heterogeneous-agent models as further steps on the same path. Heterogeneous-agent models are, however, still conceptually distinct from true agent-by-agent models; their agents are still representative of aggregates and not individuals.

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verts nominal GDP to the value of money in a base year, where the unit of value depends on the construction of the price index, so that the units of real GDP are fundamentally baskets of goods and not currency units, despite being expressed in what might be mistaken for a currency unit (e.g., "2010 constant dollars").

14. Lucas (1978, 1430) had already employed the representative-agent assumption in theoretical work, and it was rapidly adopted in empirical studies using aggregate data: "Analysis of dynamic, stochastic general equilibrium models is a difficult task. One strategy for characterizing equilibrium prices and quantities is to study the planning problem for a representative agent (see Lucas 1978)" (King and Plosser 1984, 366).

The microfoundational strategy that begins with the representative agent is the exact opposite of the strategy of the Keynesian macroeconometric modelers. The Keynesians saw their primary allegiance as to the data and hoped for a macroeconomic model that was not inconsistent with microeconomics; the microfoundationalists see their primary allegiance as to a microeconomic account and hope for an empirical model that is not inconsistent with the aggregate data. One might imagine a meeting in the middle, but the difference in conceptual structure between microeconomics and macroeconomics rules that out—hence, the eliminativist nature of reductionism in macroeconomics. This point is, in fact, understood by new classical macroeconomists. Sargent notes that the difficulties in developing a model that consistently applies microeconomic templates to highly disaggregated data are formidable and “unfortunate, but [do] not seem to argue in favor of models that purchase superficial realism at the cost of making numerous implicit assumptions that violate the principles that emerge from the simple abstract models that we do have” (1982, 384 n. 11).

The implicit argument in favor of representative-agent models as empirically relevant to aggregate economic data runs something like this: a representative-agent model is not itself an acceptable representation of the whole economy (in part because it does not allow us to analyze some questions, such as heterogeneity of preferences or the distribution of income), but it is a first step in a program that step by step will inevitably bring the model closer to the agent-by-agent microeconomic model of the whole economy—an elaboration that we can understand in principle—and, therefore, we ought to take the empirical predictions of the representative-agent model seriously, we ought to ground our practical counterfactual policy analysis in such models, and we ought to reject models that either are not representative-agent models or do not stand further down the path to the fully elaborated model. I call this argument *eschatological justification*: it is the claim that there is a plausible in-principle game plan for a reductionist program and that the conclusions of early stages of that program are epistemically warranted by the presumed, but undemonstrated, success of the future implementation of the program in the fullness of time.

While rarely spelled out in detail, the argument is taken seriously.<sup>15</sup> In a status report on quantitative macroeconomics published in one of the high-

15. A referee has suggested that the claim that the eliminative microfoundationalists are engaged in eschatological justification may raise a straw man, suggesting that advocates of representative-agent models, in fact, take empirical success as the warrant of their models—pursuing something like what I have referred to as the reductive impulse. The attribution of eschatological justification in this case requires not only that we should seek eliminative microfoundations but that, even recognizing the empirical inadequacies of current representative-agent models, we should prefer their policy conclusions. Chari and

est profile economics journals, the authors illustrate the direct utility of various representative-agent models for real-world policy analysis, and they stigmatize approaches that are not grounded in the same principles: “The practical effect of the Lucas critique is that both academic and policy-oriented macroeconomists now take policy analyses seriously only if they are based

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Kehoe (2008, 248), discussing practical empirical applications, make the case that models that build from the “ground up” to increasingly complete coverage from a representative-agent base have a “special virtue” relative to those that disaggregate from the top down, even when they both embody the same level of empirical aggregation. They thus reject following the reductive impulse to achieve explanatory reduction in favor of first steps toward an eliminative reduction. Such explicit statements are uncommon, yet the most pervasive features of any scientific methodology are frequently implicit rather than carefully articulated—economics is no different. We can, however, look for the hallmark of eschatological justification—namely, that the expected future success of a modeling strategy implies practical deference to models at the current stage of development, even in cases in which those models may fail on other desiderata. Here is a concrete case: central banks around the world employ dynamic stochastic general-equilibrium models, a species of representative-agent model, to give actual policy guidance. (And they were widely criticized for doing so—by politicians and the press—in the wake of the policy failures of the worldwide financial crisis that began in 2008.) An economist at the Bank of England provides an unusually explicit explanation: “The pre-microfoundations approach puts the stress on data consistency: models that are not consistent with the data (in an econometric sense) should be rejected. In contrast, the Bank of England’s new model embodies a quite different approach. Internal consistency is vital, because only then can we be sure that relationships are consistent with the axioms of microeconomic theory. Econometric consistency is not essential . . . but instead is a pointer to future theoretical development” (Wren-Lewis 2007, 47–48). A key point here is that these models were actually used to guide policy. This is not an unusual case, but in fact is implicit in mainstream practice. That eliminative microfoundations are the mainstream ideal has already been established. Beyond that, however, they are used to dismiss alternatives that are successful on other relevant desiderata, despite an acknowledged need for future development. In his own work (and in that of a vast number of economists), Sargent—even in cases meant to be relevant to policy—takes the representative-agent model as *prima facie* immune to the “theoretical presumption” maintained against nonmicrofoundational models that they will founder on the Lucas critique. The nonmicrofoundational models should be rejected in favor of the currently available “simple abstract models,” despite technical hurdles to further development. Similarly, Kydland and Prescott (1991) reject the notion that empirical success can be judged on “how well the model mimics historical data.” Rather, “the degree of confidence . . . depends on the confidence that is placed in the economic theory” (171). And that theory is explicitly committed to eliminative reduction. It is critical to notice that eschatological justification does not suggest that representative-agent models are empirically adequate even in the eyes of their proponents but only that they are entitled to empirical and practical deference because of their place in the research program that is identified with the ideal endpoint of a fully eliminative reduction. They are, in fact, routinely compared to data. Such comparisons, however, do not place the underlying theory at risk. At most, other aspects of the model may be modified in the face of recalcitrant data. With respect to a particular variant, Prescott (1986) defends the representative-agent model in the face of recalcitrant data by arguing that the data have not yet caught up with the theory. Referring to the techniques initially used to

on quantitative general equilibrium models in which the parameters of preferences and technologies are reasonably argued to be invariant to policy” (Chari and Kehoe 2006, 4). I believe that eschatological justification stands behind many philosophical defenses of reductionism. But the cobbler ought to stick to his lasts: here I argue only that it is what stands behind a ubiquitous practice in macroeconomics.

Eschatological justification does not provide a valid argument for the salience of representative-agent models. It nonetheless seems to provide methodological solace to macroeconomists. Practitioners fully understand that the end point of an agent-by-agent model is not possible:

Kahnemann and Tversky haven’t even gotten to two people; they can’t even tell us anything interesting about how a couple that’s been married for ten years splits or makes decisions about what city to live in—let alone 250 million. This is like saying that we ought to build it up from knowledge of molecules or—no, that won’t do either, because there are a lot of subatomic particles—we’re not going to build up useful economics in the sense of things that help us think about the policy issues that we should be thinking about starting from individuals and, somehow, building it up from there. (Lucas, in Hoover and Young 2013, 1189)

Lucas does not notice that this admission undermines the methodological basis for his reductionist claims.

One defense of microfoundations deeply embedded in the strategy of eschatological justification is to suggest that the barriers to the implementation of the reduction are merely practical. However, a case can be made that a barrier is no longer “merely practical” when the game plan for achieving the end stage cannot be spelled out in any detail past the first few shallow moves. Be that as it may, there is a stronger objection on the table. Analysis using the representative-agent model employs an analogy between the behavior of a single agent and the agents collectively in a whole economy. For example, the representative agent is typically endowed with a utility function from precisely the same family as those typically assigned to individual agents in

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test representative-agent models (typically in the general class of rational-expectations models), Sargent (2005, 567–68) recalled: “My recollection is that Bob Lucas and Ed Prescott were initially very enthusiastic about rational expectations econometrics. After all, it simply involved imposing on ourselves the same high standards we had criticized the Keynesians for failing to live up to. But after about five years of doing likelihood ratio tests on rational expectations models, I recall Bob Lucas and Ed Prescott both telling me that those tests were rejecting too many good models.” Subsequently, such models were brought into contact with data by using calibration methods in which the representative-agent assumption itself is never at risk. (For a trenchant criticism of the abandonment of established econometric standards in the assessment of representative-agent business-cycle models, see Heckman and Hansen [1996].)

microeconomic analysis. Do we have any good reason to accept the analogy? Microeconomists have long known that the answer is no.

Exact aggregation requires that utility functions be identical and homothetic (Gorman 1953).<sup>16</sup> Translated into behavioral terms, it requires that every agent subject to aggregation have the same preferences (you must share the same taste for chocolate with Warren Buffett), and those preferences must be the same except for a scale factor (Warren Buffet with an income of \$10 billion per year must consume 1 million times as much chocolate as Warren Buffet with an income of \$10,000 per year). This is not the world that we live in. The Sonnenschein-Mantel-Debreu theorem shows theoretically that, in an idealized general-equilibrium model in which each individual agent has a regularly specified preference function, aggregate excess demand functions inherit only a few of the regularity properties of the underlying individual excess demand functions: continuity, homogeneity of degree zero (i.e., the independence of demand from simple rescaling of all prices), Walras's law (i.e., the sum of the value of all excess demand is zero), and that demand rises as price falls (i.e., that demand curves *ceteris paribus* income effects are downward sloping; see Kirman 1992).<sup>17</sup> These regularity conditions are very weak and put so few restrictions on aggregate relationships that the theorem is sometimes called the "anything goes theorem."

The importance of the theorem for the representative-agent model is that it cuts off any facile analogy between even empirically well-established individual preferences and preferences that might be assigned to a representative agent to rationalize observed aggregate demand. The theorem establishes that, even in the most favorable case, there is a conceptual chasm between the microeconomic analysis and the macroeconomic analysis.<sup>18</sup> The reasoning of the representative-agent modelers would be analogous to a physicist attempting to model the macrobehavior of a gas by treating it as a single room-size molecule. The theorem demonstrates that there is no warrant for the notion that the behavior of the aggregate is just the behavior of the individual writ large: the interactions among the individual agents, even in the most idealized model, shapes in an exceedingly complex way the behavior of the aggregate economy. Not only does the representative-agent model fail to provide an analysis of those interactions, but it seems likely that they will

16. Hands (2014) provides a valuable historical discussion of these issues.

17. In Walras's law, goods in excess supply are treated as negative excess demand.

18. To be clear, the theorem does not rule out that there could be a function of aggregates the maximization of which coincides with observed aggregate outcomes; rather, it rules out that we have any reason to expect that such a function would recapitulate the presumed forms of individual utility functions or bear any simple, reliable relationship to them, which is what would be needed to justify the representative-agent model as it is actually implemented in macroeconomics.

defy an analysis that insists on starting with the individual, and it is certain that no one knows at this point how to begin to provide an empirically relevant analysis on that basis.

**5. Recapitulation.** Reductionism in macroeconomics, the program of the microfoundations of macroeconomics, faces a twofold challenge. First, the agent-by-agent analysis that is its natural end state, at the least, cannot be practically implemented. Second, even if it could, it would fail to provide the right conceptual resources for the problems that motivate macroeconomics in the first place. Macroeconomics requires different conceptual resources because the interactions of individuals generate stable relationships that are not simply the sum of individual behaviors regarded atomistically, and these relationships in aggregate are frequently independent of the details of the individual behavior. This is no more mysterious than that the critical properties of pulleys and gears as simple machines are independent of their material constituents, even though their functionality may be conditioned by those constituents: a plastic gear in a toy car operates on the same mechanical principles as a steel gear in a race car, although a plastic gear would fail in a race car. Macroeconomic relationships are not simply blown-up versions of microeconomic relationships but possess structure that places aggregates into causal relationships with other aggregates. Although these causal relationships are frequently independent of the individuals whose microeconomic interactions bring them into existence, they are, like the gears, conditioned by their constituents—in particular, by their intentionality, which is why the Lucas critique cannot be ignored even by those who reject Lucas's version of reductionism. And there is always the question: why particular macroeconomic relationships are what they are.

The push to answer that question is the source of the reductive impulse. Klein's program was guided by the reductive impulse to get behind and explain macroeconomic relationships. Its aim was analysis—disassembly of the mechanism to account for its working one piece at a time. In contrast, Lucas's program is constructive, privileging a particular set of components—namely, optimizing individuals. Its aim is to assemble them into macroeconomic relationships. The difficulty is that no one knows how to do that, as Lucas himself now accepts. The representative-agent model is a backhanded acknowledgment of that fact, based on the unarticulated—and nearly magical—belief that, if economists copy the forms of microeconomics with aggregate data, then somehow the result will come out right even though they pay no attention to the relationships among the interacting individuals.

Reductive microfoundations fails in a way that is analogous to Dewey's criticism of the failure of some systems-building philosophy. It fails to bring its refined theoretical objects back into touch with the experiences that mo-



tivated our concern in the first place. It tries less to explain than to explain away. The representative-agent model fails to reconnect to relevant experience and practice because the posited connector is nothing but an analogy, and microeconomics itself (aggregation theory) has shown it to be a defective analogy.

I must be careful not to overclaim. Representative-agent models may be the source of fruitful analogies if they are not taken too literally and not thought to constrain in detail the admissible behavior of aggregates. Used that way, they are just another tool for pursuing the reductive impulse, analogous to Friedman's analysis of individual consumption being used to suggest that certain aggregate properties might be worth investigating, but they do not provide the foundations of reductionism. The results of any analogical insights that they might provide must be brought back into contact with the actual explanatory problems of macroeconomics. Do they improve prediction? Do they adequately guide policy? Do they illuminate the structure of the institutional and social relationships that are reflected in aggregate data? I am skeptical, but the possibility that they may cannot be ruled out a priori. Success would be measured through testing at the aggregate level, and failure to conform to a microeconomic template would not constitute a priori grounds to dismiss a competing model.

**6. Coda.** We ought to be humble and not claim the fruits of scientific success until we have actually planted the trees, nurtured them, and brought in the harvest. Criticizing certain philosophical systems, Dewey writes about intellectual humility:

The claim to formulate a priori the legislative constitution of the universe is by its nature a claim that may lead into elaborate dialectic developments. But it is also one which removes these very conclusions from subjection to experimental test, for, by definition, these results make no differences in the detailed course of events. But a philosophy that humbles its pretensions to the work of projecting hypotheses for the education and conduct of mind, individual and social, is thereby subjected to test by the way in which the ideas it propounds work out in practise. In having modesty forced upon it, philosophy also acquires responsibility. (Dewey 1909, 97–98)

It is not just philosophy per se; a science such as economics may suffer from a lack of humility. Following the reductive impulse is a humble way to proceed. Insisting on reductionism and dismissing scientific work because it fails to meet the constraints of reductionism is, absent a practicable connection of the supposed basic theory to the world in which practitioners live, immodest and perhaps irresponsible as well.



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