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REDUCTIONISM IN ECONOMICS:
CAUSALITY AND INTENTIONALITY IN THE MICROFOUNDATIONS
OF MACROECONOMICS

BY KEVIN D. HOOVER

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CENTER FOR THE
HISTORY OF POLITICAL ECONOMY
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Reductionism in Economics: Causality and Intentionality in the Microfoundations of Macroeconomics*

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Abstract
of
Reductionism in Economics:
Causality and Intentionality in the Microfoundations of Macroeconomics

In many sciences – physical, but also biology, neuroscience, and other life sciences – one object of reductionism is to purge intentionality from the fundamental basis of both explanations and the explanatory target. The scientifically relevant level – ontologically and epistemologically – is thought to lie deeper than the level of ordinary human interactions. In the material and living world, the more familiar is the less fundamental. In contrast, the economic world of day-to-day life – the world of market interactions – appears to be the relevant level. Macroeconomics is thought to provide an account that is above, not below or behind, ordinary economic decision-making. An advantage of a macroeconomic account is that it is possible to employ causal analysis of the economy as a whole analogous to the causal analysis of physical systems. The fear of many economists is that such analyses are untethered to ordinary economic decision-making. The object of reductionism in economics – the so-called *microfoundations of macroeconomics* – is adequately to ground or replace higher level causal analysis with an analysis of the day-to-day interactions of people. The object is not to purge intentionality, but to reclaim it. The paper will attempt to understand the key issues surrounding the microfoundations of macroeconomics from a perspectival realist perspective that elucidates the relationship between economists' methodological preference for microfoundations and need for macroeconomic analysis – that is, between economists' respect for the intentional nature of economic life and the need for a causal analysis of the economy. The paper favors metaphysical humility and methodological pragmatism.

JEL Codes: B41, B40, E00, E10

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I. 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, 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 2012a).

The American philosopher John Dewey ([1925]1958, ch. 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” (pp. 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 as 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. 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 indices such as the GDP deflator or the consumer price index (CPI) 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 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. 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*. 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. Evaluation of the claim that microfoundations are necessary to a successful macroeconomics is my target.

II. Geschichte des Makroökonomie Wie Es Eigentlich Gewesen

Philosophers of economics as well as macroeconomists have frequently been guided by distorted histories of macroeconomics, often drawn from textbooks at several removes from the original sources. A philosophically useful history requires some contextualization. It will be helpful to educate the reader a little on the objective history of macroeconomics. Naturally, I am aware that I have a point of view – an ax to grind – so that I am open to the ridicule of those who mocked the position of some of the heirs to the southern side in the American Civil War who called for “an objective history of the Civil War from the Southern point of view.” But one advantage of my allegiance to perspectival realism is that I can both understand the pitfalls of such a history and nonetheless maintain that it is a defensible project (Hoover 2004).

Economists are generally happy to refer positively to methodological individualism, but they have little truck with, for example, 19th century debates in which individualism is arrayed

against Hegelian or Marxist social forces. Even Karl Marx when wearing his economist's hat recapitulates and amplifies the detailed Ricardian individual analysis.¹ The issue of the reducibility of macroeconomics to microeconomics began in the 1930s almost immediately with Ragnar Frisch's introduction of the distinction in 1933 (see Velupillai 2009). The use of "microfoundations" as name for the reduction was introduced only in the 1950s and gained currency only in the 1970s (see Hoover 2012b). Frisch (1933a, pp. 172-173) did not view see microeconomics as foundational for macroeconomics. On the contrary, he saw microeconomics as a kind of local analysis that required a macroeconomic context in which to proceed in much the same way that a physicist might characterize the movement of a particular charged particle against the background of field that was the aggregate product of the interaction of many other charged particles but which was not, for purposes of analysis, resolved into its constituent particles. Keynes (1936), who is often credited with founding macroeconomics, although he never used the term, actually gives a more detailed account of individual behavior than does Frisch. Frisch himself was more concerned with the possibilities of practical analysis and does little further to investigate the relationship of the macroeconomic to the microeconomic (see Hoover 2012b). At just about the same time, Jan Tinbergen, who shared the first Nobel Prize in economics with Frisch, was the first to construct econometric models for whole economies using aggregate data (Morgan 1990, ch. 4).

Several important threads in the history of macroeconomics converge in the figure of Lawrence Klein. Klein's doctoral dissertation is an interpretation of Keynes's *General Theory*

¹ It is true that Ricardo and other classical economists frequently were concerned about distribution of income by social class, which gave a starting point for Marx's vision of a clash between Capital and Labor; yet the underlying machinery is all individual. Jevons (1871) employed the notion of a "trading body," which he thought of as an individual or a firm or a country; yet he applied the same marginalist individual analysis in each case, as if each were a decision-maker with preferences and, like modern economists, presupposes the grounding of that analysis in individual behavior without demonstrating how to construct the connections (see Herfeld 2013).

(1936), which includes a mathematical reconstruction of its main relationships as an aggregate general-equilibrium system – thus, articulating a formal theoretical basis for macroeconomic models of the kind that Tinbergen was already beginning to formulate.² After finishing his dissertation, Klein visited the Cowles Commission in Chicago, which was committed to the agenda laid out by Frisch in the founding of the Econometric Society – the cooperation of economic theory, mathematics, and statistics in the establishment of a new quantitative economics (Frisch 1933b, p. 1). Following the lead of Trygve Haavelmo (1944), a student of Frisch, who gave the first systematic methodological account of the foundations of econometrics, the Cowles Commission treated the problem of econometric modeling, as one of using theory as a tool for articulating the causal structure of a model and then of using statistics to tie that model to the empirical data. Their notion of “theory” was broad and not very prescriptive.

Nonetheless, the Cowles Commission also pursued the somewhat distinct project of the mathematical formulation of Walrasian general-equilibrium models, which were abstract, idealized models of whole economies built up from the decision problems of individual agents. Klein stood more on the systems-estimation side of Cowles; yet he explicitly embraced the Walrasian sensibility as well. On leaving Cowles, Klein embarked on a life-long project of constructing ever larger models of the economy, beginning with a three-equation proof-of-concept model in 1950 and ending with Project LINK, a current effort of the United Nations to integrate national macroeconomic models into a world model.³

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

² Klein’s 1944 dissertation was published as *The Keynesian Revolution* (1947).

³ Klein, who was awarded the Nobel Prize in 1980, died on 20 October 2013.

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 commercial. Following the path of greater and greater causal articulation, the models evolved from the three to twenty-five 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 policymaker aimed at certain *targets* by choosing the settings of *instruments*. The macroeconometric model provided the machinery for conducting counterfactual analysis of the connection of between instruments and targets.

Klein, Tinbergen, and other macroeconometric modelers were called “Keynesians” because of their having adopted the aggregative architecture of Keynes’s *General Theory*, Keynes’s own skepticism of the macroeconometric project to the contrary notwithstanding.⁴ And Keynesian macroeconometrics was the dominant approach until the early 1970s. Seen through political or ideological glasses, the main story was the debate between “monetarists,” associated with Milton Friedman, and policy-oriented Keynesians, such as James Tobin, Paul Samuelson, and Robert Solow. But seen through the glasses of economic science, the central challenge to the Klein/Tinbergen program was the so-called “Lucas critique” (Lucas 1976; see also Hoover 1988, ch. 8, sections 8.3-8.4). Put broadly, Lucas’s point was that the aggregate relationships modeled by macroeconometricians were the product of the individual behaviors of individuals. Those behaviors were intentional. And contrary to the implicit assumption of the

⁴ The modelers typically followed the formal interpretations of Keynes’s theory due to Hicks (1937) and Modigliani (1944). See Keynes (1939) for his view on macroeconometric modeling (also Morgan 1990, ch. 4)

engineering approach to policy, the policymaker was not an outsider to economy: not only did the policymaker react to data generated by intentional agents, those agents themselves had every reason to try to understand and predict the actions of the policymaker 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).

III. The Reductive Impulse versus Reductionism

No economist really dissents from ontological individualism of the form that holds that individual behavior lies behind economic phenomena, so that macroeconomic phenomena are in

some sense emergent. The details of this emergence are unclear, but even economists who disagree with the implications of the Lucas critique for practice do not look to any sort of mysterious emergence grounded in occult influences. Emergent properties often seem perfectly explicable. Keynes points to a famous fallacy of composition: an increase in an individual's marginal propensity to save (i.e., fraction saved out of an incremental increase in income) increases his total savings *ceteris paribus*; yet a simultaneous increase in propensities to save of all individuals leaves national saving unchanged. The explanation is that the *ceteris paribus* clause is necessarily violated in aggregate, since an accounting constraint requires that savings equal investment in aggregate; and, if investment is unchanged, total savings cannot increase. There is no mystery in this since income, which an individual might in isolation take to be fixed, is not fixed in aggregate. An attempt to increase savings lowers expenditures, in turn reducing incomes to the sellers of goods, which limits their expenditures, reducing other seller's incomes, and so forth. This is the downward version of the Keynesian multiplier process that was much discussed with respect to stimulus packages in the wake of the recent financial crisis.

In other attempts to evaluate the case of microfoundations, I have applied Searle's (1995) analysis of collective intentionality to demonstrate that social structures could emerge non-mysteriously from individual behavior in a way that created social institutions that were independent of *any* individual mind and, yet, not independent of *all* minds (Hoover 2009). And I argued that such emergent institutions might in some cases underwrite the application of an analysis of efficient cause to economic data closely analogous to the application to physical processes in which intentionality was at best a side consideration. I do not want to recapitulate that argument any further here.

Rather I want to point out that Lucas's call for the elimination of macroeconomics was based on an argument that we could derive the detailed connection between microeconomics and macroeconomics and not on an argument that we actually knew how to do it. Absent the details, the assertion that macroeconomics can in fact be grounded in microeconomics is a matter of faith or hope. Yet, nonetheless, it is one that very few macroeconomists actually call into question. Even those, such as I, who do call it into question feel the force of Lucas's argument tugging on their intuitions. Lucas has forced us all to take a stand with respect to microfoundations. There are two distinct stances, which I will refer to as the *reductive impulse* and *reductionism*.

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, p. 16) famous definition: "Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses." The standard approach of microeconomics is to express for any particular 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, p. 57). Later, he explored the foundations of aggregation theory (Klein 1946a, b). His preferred approach was to view microeconomics and macroeconomics as both explanatorily successful and as each employing a distinct set of conceptual categories. He then asked whether there was a formal aggregation procedure consistent with the constraint of simultaneous consistency with 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 section I).

It soon became clear 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 macroeconometric 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. [Klein 1992, p. 184]

Klein's approach does not represent the abandonment of the goals of reduction. He and most of the Keynesians of the 1950s through 1970s 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 macroeconometric model was subjected to an individual, microeconomic analysis.

To take an example: Simon Kuznet's 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.⁵

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 to the macroeconomic problem that would allow the researcher to identify relevant variables and qualitative relationships and to expose underlying inconsistencies between macroeconomic relationships and individual behavior. There is no appeal to a deductive relationship between the individual and the aggregate. 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.

⁵ See Hoover 2012c for a textbook account of the permanent-income hypothesis, its evidence, and implications.

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 macroeconomics makes 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.

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 thorough-going 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 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, though it may ultimately fail, gives me some hope of succeeding, while an analysis that starts with physics (or even neurobiology) gives me none.

Intentional accounts are not, however, causal in the sense of efficient or “billiard-ball” 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.

In contrast to the reductive impulse of the Keynesian macroeconometric modelers, Lucas's own response to the Lucas critique is an example of reductionism – the idea that no aggregate relationship is secure unless it has been built up from deeper, fundamental principles. On Lucas's view, the entire scientific enterprise of macroeconomics was an intellectual mistake; and if microfoundational reductionism succeeds “the term ‘macroeconomic’ 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” (Lucas 1987, pp. 107-108).⁶

IV. Against Eschatological Justification

The Lucas critique is the hinge of the history of macroeconomics over the past fifty years. Early generations of Keynesian 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

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.

technology as given – and a plausible, adequate microfoundational account was held to be proof against the Lucas critique.

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 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 aggregate could be computed for this economy by simply following the data collection procedures of government statistics bureaux, 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 is practically implementable. 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 analogy that is a variant of an argument from Hillary Putnam (1975) regarding square pegs and round holes. 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 apparatus 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 pressure to the top of the building. Unless one is really a thorough-going 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 apparatus 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 minimus* 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 explain how instruments that instantiate that mechanical advantage can be constructed.

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 (see Hoover 1995 for details). 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, as opposed to their *nominal* or monetary (directly observable values). The general price level is the key concept involved in converting nominal to real quantities, so that the same conceptual divide is recapitulated between the familiar, experiential nominal quantities and the refined objects that populate macroeconomics. And just as with the problem of lifting a 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 policymakers.

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. (This is not the same as analyzing an individual in a context – which was Frisch's idea of how to do microeconomics – for, in that case, at each stage, the entire economy is represented.) 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 limit case of a microeconomic general-equilibrium model (e.g., an Arrow-Debreu model) in that, in its empirical application, it posits a single agent and attributes the entire national GDP, entire provision of labor, entire ownership of capital, and so forth to the single agent. A large number of *empirical* representative-agent models have been seriously

proposed. They are widely held to be immune from 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; its agents are still representative of aggregates and not individuals.

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 to the data and hoped for a macroeconomic model that was not inconsistent with microeconomics; the microfoundationalist see their primary allegiance 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 the microeconomics and macroeconomics rules that out – hence the eliminativist nature of reductionism in macroeconomics.

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 which step by step will inevitably bring the model closer to the agent-by-agent microeconomic model of the whole economy – an elaboration which 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. 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 behinds 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, p. 1189]

Lucas does not notice that this admission undermines the methodological basis for microfoundational 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 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). 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 one 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 rescalings of all prices), Walras's law (i.e., the sum of the value of all excess demands is zero), and that demand rises as price falls (i.e., that demand curves *ceteris paribus* income effects are downward sloping) (see Kirman 1992).⁷ 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

⁷ In Walras's law goods in excess supply are treated as negative excess demands.

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. The reasoning of the representative-agent modelers would be analogous to a physicist attempting to model the macro-behavior of a gas by treating it as 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 that they will 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.

V. A Plea for Humility

Reductionism in macroeconomics, the program of the microfoundations of macroeconomics, faces a two-fold 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, though 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 motivated 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 over-claim. 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 and not 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*. 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, pp. 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.⁸ Insisting on reductionism and

⁸ It may be objected that representative-agent models are routinely tested against empirical data. While it is true that they are routinely compared to such data and used to interpret it, the representative-agent framework itself is never placed at risk. At most, other aspects of the model may be modified in the face of recalcitrant data. Defending a particular variant of the representative-agent model, Prescott (1986) defends the representative-agent model in the face of recalcitrant data by arguing that the data has not yet caught up with the theory. Referring the techniques initially used to test representative-agent models (typically in the general class of rational-expectations models), Sargent (2005, pp. 567-568) 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

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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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.

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