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CHAPTER 14

MICROFOUNDATIONS AND THE ONTOLOGY OF MACROECONOMICS

KEVIN D. HOOVER

1. THE IDEOLOGY OF MICROFOUNDATIONS

For more than fifty years, economic pedagogy has been structured around the division between macroeconomics and microeconomics.¹ Virtually every undergraduate economics major and graduate student begins his course of study with core classes in these two fields. Yet, in the conceptual schema of professional economists—surprisingly, perhaps, especially among macroeconomists—macroeconomics occupies an equivocal place.

The typical concerns of macroeconomics—such as national output, employment and unemployment, inflation, interest rates, and the balance of payments—are among the oldest in economics, having been dominant among the problems addressed by both the mercantilists and classical economists, such as David Hume, Adam Smith, David Ricardo, as well as even earlier writers. These concerns co-existed with ones that we now regard as characteristically microeconomic, such as the theory of prices exemplified in the labor theory of value of the classical economists or the theory of marginal utility of the early neoclassical economists. Questions about the relationship between these two groups of concerns could

hardly be articulated until a categorical distinction between macroeconomics and microeconomics had been drawn.

Although John Maynard Keynes did not use the term *macroeconomics*, the category of macroeconomics entered the consciousness of economists as a result of the publication of Keynes's *General Theory of Employment Interest and Money* (1936).² *Macroeconomics* is used in at least two senses. First, Keynes (1936, 292–293) contrasted economic analysis that assumed that aggregate output was fixed and addressed the decisions of individual firms or workers and analysis that explained output as a whole, as opposed to the output of a particular firm. Second, macroeconomics has also been viewed simply as the economics of aggregate quantities—of GDP rather than cars, of the unemployment rate rather than the hiring decision, of the inflation rate rather than the price of Coca-Cola. These two characterizations of macroeconomics are different but not necessarily contrary. The interplay between them, indeed, explains some of the issues that arise in macroeconomics.

Once a distinction had been drawn—whether or not it was a crisp one—the question of the relationship between microeconomics and macroeconomics was immediately on the table (see Hoover 2002). Wassily Leontief (1936), in one of the earliest responses to the *General Theory*, attacked it for its inconsistency with a microeconomic account of general equilibrium. The Keynesian Lawrence Klein (1950, Chapter 3 and 192–199) saw the provision of microeconomic foundations for each of the main Keynesian functions (consumption, investment, and money demand) as part of the essential agenda of the Keynesian revolution.

Keynes himself analyzed the individual behavior that he understood to lie behind these functions. Yet, he also thought about how the behavior of the individual related to aggregate variables. For instance, Keynes's (1936, 169–172) account of the speculative demand for money depended on the individual's assessment of whether current interest rates are below or above the rate that he subjectively regards as normal. Individuals plunge into money when rates are below normal, since any return to normal implies a capital loss on holdings of interest-bearing bonds; whereas they plunge into bonds when rates are above normal in pursuit of the implied capital gain. Keynes argued that a smooth curve—rather than the knife-edge behavior of the individual—should relate aggregate money holdings inversely to interest rates, since subjective judgments about the “normal” interest rate vary among individuals.³

That economists had to think carefully about the differences between individual and aggregate behavior was once an article of faith and was clearly encapsulated into elementary textbooks (for example, in Samuelson's (1948) pathbreaking *Principles of Economics*) with reference to fallacies of composition. Yet seventy years after the publication of the *General Theory*, the reigning ideology of macroeconomists is that macroeconomics is secondary to microeconomics. Reflecting a common feature of ideologies, adherents of the reigning ideology have a variety of understandings of in what exactly the secondariness of macroeconomics consists and different degrees of allegiance to the ideology itself.

At least three broadly reductionist views of the relationship of macroeconomics to microeconomics are commonly encountered.

- One view holds that there is no useful distinction between macroeconomics and microeconomics. This is encapsulated in Robert Lucas's (1987, 107–108) desire to eliminate the distinction altogether.
- A second view defines macroeconomics, not by its methods or its conceptual scheme, which are no different from those of microeconomics, but by the range of problems to which it is addressed—for example, to monetary and fiscal policy. On this view, macroeconomics is distinct from other subfields in the same way that, say, industrial organization is distinct from labor economics, while both are subfields of microeconomics.
- A third view does admit different methods and approaches, but sees macroeconomics as only a pragmatic compromise with the complexity of applying microeconomics to economy-wide problems. This view asserts that macroeconomics reduces to microeconomics in principle but, because the reduction is difficult, we are not there yet.

These three views all contribute to the widespread belief that sound macroeconomics stands in need of *microfoundations*.

In referring to microfoundations for macroeconomics as an ideology, I use *ideology* both in the neutral sense of a more or less coherent set of beliefs guiding the collective activity of macroeconomic research and in its pejorative sense of false consciousness—a collective illusion shared by macroeconomists. My contention is that, even in its neutral sense, the ideology of microfoundations rests on a mistake about the ontology of the social world; whereas, in its pejorative sense, it shares the characteristic, common to political ideologies, of serving as a tool of persecution and intellectual repression. The ideologue searches for ideological purity. Since the consciousness is false, ideology in this sense is bound to be a muddle—but a deeply pernicious muddle.

The adherents of view 1 or 2 frequently also hold view 3 as well. And view 3 underwrites a kind of tyranny of the future, which is typical of totalitarian politics: a vision of heaven on earth justifies any misdeed today as long as it aims toward the future good, even when the path between the here-and-now and the future is obscure. I have elsewhere referred to this trope as *eschatological justification* (Hoover 2006a).

The reductionist impulse in macroeconomics is frequently referred to as *methodological individualism*. The term is not apt. Practical macroeconomics does not consist of true microeconomic models—that is, of models in which the behavior of macroeconomic aggregates is derived from the composition of the behaviors of individual economic actors. This would obviously be a very difficult way to approach the economy, posing problems of a similar nature to trying to explain the formation of hurricanes molecule by molecule, applying the established principles of Newtonian mechanics.

In practice, macroeconomists generally accept the representative-agent model as a workable microfoundation. The representative-agent model takes economy-wide aggregates (GDP and its components, price indices, average interest rates, and so forth) as if they were the equivalent to the similarly named variables associated with individual agents (the incomes of individual workers, the products of individual firms, the prices of those products, the interest rates at which an individual borrows, and so forth). The representative agent is just a microeconomic agent writ large. He maximizes utility subject to a budget constraint given by the national-income identity. He simultaneously maximizes profits subject to an aggregate production function. And the forms of these functions are identical to the forms that have proved tractable in microeconomic analysis.⁴

It is, in fact, microeconomists who have shown clearly that there is no valid reason to believe that the functional forms that apply to individuals will describe aggregates of individuals. For example, even if every individual is governed by Cobb-Douglas utility and production functions with individual goods as their arguments, it would be extraordinary for the macroeconomy to conform to Cobb-Douglas functional forms with aggregate consumption, GDP, capital, and labor as their arguments.⁵

Once we recognize that representative-agent models do not deal with individuals, we see that macroeconomics mimics the forms of microeconomics without successfully implementing an individualist methodology. This is so obvious that one must wonder in what the attraction of microeconomic forms consists. I conjecture that the real underlying motivation is not methodological but ontological. The ontological mistake of macroeconomics is to believe that the objects of macroeconomic analysis are not ontologically independent. Macroeconomists fear that they are not dealing with solid economic entities unless they can trace the route along which those entities reduce ontologically to intentional agents, individuals who make decisions in light of their preferences, goals, and beliefs. But, since this is an impracticable task, they emphasize the connection of the aggregate to the individual by aping the analytical forms of microeconomics.

The fears and strategies of economic reductionism are, then, quite different from those in other sciences. Many biologists, for example, are deeply afraid of functional or teleological explanations. They are not happy with explanations that rely on anything that attributes beliefs, goals, or other intentional states to individual organisms, much less to species. In contrast, the economist finds it hard to see any analysis as "economic" unless it deals in intentional states. The point of microfoundations is to recapture the teleology that appeared to be missing in macroeconomics.

A successful ontology of macroeconomics must recognize the fear that motivates the program of microfoundations. Macroeconomists are, of course, correct that there is an important sense in which macroeconomic behavior must emerge from microeconomic behavior. Macroeconomic aggregates are not free-floating Platonic forms or, as Searle (1995, 25) puts it, a "Hegelian world spirit." A successful

ontology must provide an intelligible account of the connection between the individual and the aggregate.

I want, therefore, to turn to two questions: First, is there a successful ontology of macroeconomics? Second, what implications does (or should) this ontology have for practical macroeconomics?

2. ON WHAT THERE IS IN THE MACROECONOMY

2.1 Supervenience and Reductionism

I begin with my own earlier view of the ontology of macroeconomics (Hoover 1995; 2001a, Chapter 3; 2001b, Chapter 5). I argued that macroeconomics *supervenied* on microeconomics. That is, while the conceptual structures of microeconomics and macroeconomics were distinct, any particular arrangement of microeconomic features would always generate exactly the same set of macroeconomic features.

Supervenience is usually regarded as a form of weak reductionism that aims to preserve the fundamental object of reduction of the macro to micro. For example, philosophers of mind who want to reduce the mental to the physical while recognizing that there is a gap between mental and physical concepts appeal to supervenience (Kim 1978). But just, as I previously noted, biologists seek reductions to purge their subject of intentionality whereas economists seek reductions to recapture intentionality, as an economist I sought to use supervenience in an antireductionist manner. The goal was not to reduce macroeconomics to microeconomics, but to show that macroeconomics could have an ontological anchor in the individual, while preserving ontological independence for causally interacting aggregates. The central idea was that, in moving from the microeconomic to the macroeconomic, we do not preserve, in Uskali Mäki's (1996) evocative term, the "ontic furniture" of the microeconomic world. The conceptual shift, I argued, was evident in the units of measurement. The price of goods has the dimension of dollars/unit of good, whereas the general price level, which is often thought to be analogous to a microeconomic price, has the dimension of current-period dollars per base-period dollar. Similarly, a real good has a physical unit, while the supposedly analogous macroeconomic quantity, real GDP, has only a monetary unit (base period-dollars). My argument, then, was that macroeconomic aggregates could be seen as emergent properties of the macroeconomy—ones that would not exist without the underlying microeconomic agents—but ones that, like mental properties emerging from physical properties—were ontologically distinct. But why should this relationship not be regarded as a species of reduction? Why should

this distinction not be merely conceptual rather than ontological? In particular, why should we not just view the macroeconomic aggregates as (rather imperfect) summaries of the microeconomic data? Why should we regard them as “real” in the sense of existing *externally* (i.e., independently of any individual human mind) and objectively (i.e., unconstituted by the representations of macroeconomic theory) (see Hoover 1995, 236; 2001b, 109).

One argument against reduction of macroeconomics to microeconomics extends David Levy’s (1985) argument that microeconomic actors necessarily employ macroeconomic concepts in their decision making. For example, anyone trying to calculate how much to put aside for his children’s education needs to form expectations of the course of inflation, where *inflation* is a macroeconomic concept. Since these macroeconomic concepts are not those of an outsider who is observing and summarizing the microeconomic facts, but are those of individual agents who are making the microeconomic facts, it would seem that macroeconomic concepts are, in fact, constitutive of parts of microeconomic reality. A reductionist use of supervenience requires that the microeconomic and the macroeconomic belong to separate domains, but here they cannot be separated.

In addition to this argument against reductionism, I also tried to give positive arguments for the external reality of macroeconomic aggregates. First, I applied Ian Hacking’s (1983, 22–24) manipulability criterion—“if you can spray them [i.e., electrons], then they are real”—to macroeconomics. A positive example of such a manipulation is the Federal Reserve’s use of controlled movements in one macroeconomic aggregate—the real interest rate—to manipulate another—the yield curve. Second, I argued that the strategy of idealization in the construction of scientific models (see Nowak 1980 and Hoover 1994) can be successful empirically only if the models isolate the *essential* primary factors involved in causal processes. The success of an idealized model, therefore, amounts to an argument in favor of the existence of these primary factors. All successful macroeconomics, including any successful representative-agent models, trade entirely in macroeconomic aggregates. Despite their microeconomic trappings, the claims that representative-agent models are successful amount to implicit claims for the existence of macroeconomic aggregates. And *pace* Rosenberg (1992), I believe economics generally, and macroeconomics particularly, to have substantial empirical success.

2.2 A Critique of Supervenience

Julian Reiss (2004) argues that supervenience provides a poor framework for understanding the micro-macro relationship in a realist ontology of macroeconomics. He argues, first, that the classic applications of supervenience are reductionist and require that the concepts at the micro and macro levels be disjoint. An antireductionist twist to a supervenience argument, particularly one that argues that macroeconomic concepts are employed at the microeconomic level, is misguided

to the point of triviality. All that is left of the original notion is that there is a determinate many-to-one relationship between the individual and the aggregate, in which the same configuration of individual elements always fix the configuration of aggregate elements, although more than one configuration of individual elements might fix the same configuration of aggregate elements.

In part, Reiss's objection is that no one fails to accept that the macroeconomic behavior is ultimately the product of microeconomic behavior. He elaborates the objection, however, by challenging the ability of the microeconomic to fix the macroeconomic. He does so by appealing to Levy's observation that microeconomic agents use macroeconomic concepts. In particular, if agents use a price index to calculate real quantities or inflation rates that are salient in their individual decision making, and if, as is perfectly clear, there is no uniquely correct price index, then, the same set of background microeconomic facts might lead agents to make different decisions, depending on which price index they use, and consequently generate a different set of macroeconomic facts.

Reiss's argument that supervenience is inapplicable is offered constructively, in the sense that he supports a realist macroeconomic ontology and finds that an appeal to supervenience is a distraction that weakens the argument. Reiss mis-prizes the supervenience argument because he underestimates the ontological fears of macroeconomists. The lesson of the Lucas (1976) critique of macroeconomic models is that estimated relationships will be stable in the face of changing policy, and, therefore, useful in policy analysis, only when they capture the "deep parameters" of the economy, which we can appropriately translate into the "fundamental ontological building blocks of the economy." Lucas's assumption is that these deep parameters are necessarily microeconomic. The only way to calm the ontological fears of macroeconomists is to demonstrate that macroeconomic aggregates can stand in relationships governed by deep parameters without being severed from the microeconomic, for it is precisely the view that aggregates are not tightly connected to individual decision makers that drives the microfoundational ideology. Supervenience was offered as a balm—one way of clarifying that macroeconomic aggregates could be causally autonomous without becoming disconnected from individual behavior.

Reiss claims that the multiplicity of aggregation schemes (e.g., multiple price indices) undermines the supervenience account in the sense that he denies that the exact same micro facts must generate the exact same macro facts. The argument appears to be that, since the same micro facts can support different price aggregation schemes and each can be used to generate a different measure of inflation, then the micro facts have not "fixed" the macro facts about inflation. But this argument seems to hinge on the conflation of the measuring instrument with the thing measured and to appeal to a limited view of what constitutes the micro facts. Inflation as a category is something that we might measure through different schemes. One may be better than another, and yet, neither is perfect at capturing our theoretical conception. The claim about micro fixing macro is not about any

particular imperfect measure, but about the object of measurement. The diversity of aggregate measures does not address the point at issue.

Further, even if the measures themselves disagree (inflation on the CPI is 3 percent per year, whereas on the GDP deflator it is only 1.5 percent per year), once we know what the measuring schemes are, they are still all fixed by the micro arrangements. If a particular micro configuration delivers CPI of 3 percent and GDP deflator of 1.5 percent, any repetition of that configuration would also deliver those same values. The values of these and other measures may all differ, but they differ in the same way for the same set of micro arrangements. And the distribution of assessments of macroeconomic quantities among different individuals is one of the pertinent microeconomic facts. It is only when we conceive of this distribution as not among the fixed facts that Reiss's argument gets any traction.

While Reiss underestimates the vital importance of meeting the ontological fears of macroeconomists, he may be correct that the effort to meet those fears with a nonreductive supervenience account is so alien to established usage that the term *supervenience* ought to be dropped. The point that macroeconomics can be causally autonomous without slipping its microeconomic moorings remains important nonetheless.

In retrospect, the argument that macroeconomic concepts are fundamentally involved at the microeconomic level may be too weak to support the status of macroeconomics. It may mistake an epistemological problem for an ontological one. Yes, any account of microeconomic behavior must recognize that individuals need to use aggregate concepts as a means of dealing with the complexity of the environments in which they make decisions. Consequently, there are no useful accounts of the microeconomic that do not involve aggregate variables. This can be true, however, without these variables existing independently of the representations of the individuals.

The first step in my attempt to establish the external reality of macroeconomic aggregates was to establish the independence of the conceptual framework of macroeconomics from that of microeconomics. To the degree that this was established through dimensional analysis of micro and macroeconomic quantities, it now seems to rest on too weak a reed. Dimensional analysis does point a genuine distinction between the micro and the macro, but it is the same distinction that can be found, for example, in the relationship between microphysical and macrophysical quantities, and does not point to consequences at the macro level for intentions that are the heart of economists' insistence on microfoundations for macroeconomics. Consider a physical example—the relationship between the momenta of molecules in a gas and the pressure as it figures in the ideal gas laws. In order to derive the ideal gas laws, the kinetic theory of gases makes use of deeply relational facts, namely, the assumed probability distribution of the momenta of the underlying molecules. Physicists tend to overlook the manner in which the assumption of a probability distribution adds a superindividual element into an erstwhile reductionist enterprise. The social elements in economics are relational

facts among individuals. And analogous to physics, reductionist economics leaves out the characteristically social elements of economics. But as with the distributional assumptions in the derivation of the gas laws, we need to say something more about how these social relations work to generate macroeconomic phenomena. The question is whether we can provide a richer, but still intelligible account of the connection between the individual and the aggregate, which incorporates the specifically social features of economics.

2.3 The Construction of Social Reality

John Searle (1995) offers one such account.

The fundamental notion of microfoundations is that all macroeconomic facts ought to be shown to arise out of individual choice. Advocates of microfoundations see economic institutions, then, as lacking any independent ontological status; such institutions are, rather, essentially the epiphenomena or summary of the interactions of ontologically distinct individuals—"human molecules" in Tjalling Koopmans's evocative coinage (in excerpt from Koopmans in Hendry & Morgan 1995, 515; used as the title of Nelson 1992). The actions of such individuals are guided by their own beliefs, desires, and other intentional states, so that the intentions of another individual enter my own calculus only through my beliefs about what they might be.

An example shows how familiar this picture is to economists, as well as suggesting that there might be alternatives. Roger Guesnerie (1992, 1254) divides the justifications of the rational-expectations hypothesis into two types—*eductive* and *evolutive*. "Eductive explanations rely on the understanding of the logic of the situation by economic agents; they are explicitly or implicitly associated with the mental activity of participants aiming at 'forecasting the forecasts of others.'" Eductive rational expectations are the expectations of human molecules whose intentional states are radically isolated from the intentional states of others. In contrast, evolutive expectations are based on observations of economic behavior: "[e]volutive explanations put emphasis on the learning possibilities offered by the repetition of the situation; they are associated with the study of convergence or more or less ad hoc learning processes." The important point for us is not the *ad hoc* nature of the learning processes but the public nature of what is learned. Forming expectations on the basis of inductions about the process governing, say, prices puts little weight on an individual's intentional states and is, in principle, intersubjective. An eductive rational expectation is my own belief; an evolutive rational expectation is an attempt to characterize a fact in the world and could be something that I read in the newspaper. Even if I were to publish my belief, by the time that it is in print, it simply becomes an additional piece of information on which the holders of eductive rational expectations will ply their forecasting logic.

In opposition to the strictly individual intentionality of eductive rational-expectations, Searle tries to make sense of collective intentionality. As well as

"I believe" or "I intend," Searle argues that the states that "we believe" or "we intend" are part of the mental repertoire of every individual. The choice is not between strict reductionism and an untethered Hegelian spirit. Rather, individuals can possess individual or collective intentions, and collective action is one human possibility.

For example, when Exxon bought Mobile Oil, this was not an individual action in which the CEO's intentional state was "I intend to buy and believe that the chief financial officer and other parties intend to buy," and so for each relevant party. Instead, the intentional state of the CEO and the other parties was "we (i.e., Exxon) intend to buy." The intentions remain in the mind of each individual—no supermind is invoked. It is just that each individual intends to act collectively. The operative pronoun is *we*, not *I*. Each individual plays a particular role, and the intention is framed within the context of constitutive rules. If John Jones were the CEO, then a key intentional state belongs to John Jones, but it is meaningful to the purchase only because of his role as CEO and not because he is John Jones *per se*.

Another example: It is only within the constitutive rules of Major League Baseball that Barry Bonds can hit a home run, much less surpass Babe Ruth's or Hank Aaron's home-run records. And while in the 2006 season, Barry Bonds may have been the only one poised, to break these records, which makes his position seem individual, his role as the cleanup hitter for the San Francisco Giants could have been filled by other people. There is an independence between the role that an individual fills in a collective endeavor and the individual who fills it.

Searle rejects the notion of collective action as the complex outcome of the interactions of atomic intentionality—a form of intentionality for which educative rational expectation provides a paradigm. First, while collective social facts exist only as the result of representations in some individual minds, typically no single mind is necessary or sufficient for their existence. In that sense, they are not subjective, but are experienced as objective facts not dissimilar to the facts about physical objects. Social objects (e.g., governments, money, or universities) are "placeholders for patterns of activities" (Searle 1995, 57). Collective intentionality is about ongoing activities and the possibility of more. And the patterns of activities themselves, such as a corporation or government, are different in kind from the individuals that play specific roles within them.

The independence of the collective level from particular individuals is also mirrored in physical sciences. Putnam (1975) argues that many physical explanations work only at a macro level. Why is it, for instance, that a square peg 15/16 inches on a side will pass through a square hole 1 inch on a side but not through a round hole 1 inch in diameter? The answer appeals to the microstructures of the peg and the board in which the holes are cut only to the degree that, whatever those structures are, they imply that the peg and board are rigid. No microanalysis can explain, in any general and, therefore, scientific manner, what is easily understood on the basis of macro-level geometry. Batterman (2001) uses a case study

of rainbows to argue that scientific explanation quite commonly relies on higher level relationships that are indifferent to most of the fine details of the underlying microstructures.

Searle's second point is that the rules constitutive of social facts form a background, which is not maintained by conscious (or unconscious) intentions. A tyro baseball player may intend with each of his actions to follow the rules of baseball, but an experienced player simply intends various actions within the context of the game—bunting or stealing a base, for example. While the experienced player may be described as following the rules of the game, because his behavior conforms to the rules, he need not constantly intend to do so. The background framework of constitutive rules, not maintained by intentional states, accounts for the inert objectivity of social facts.

Searle finds many economic examples of social facts that are constructed through collective intentionality—money, for instance. Individual, engraved, green pieces of paper are nothing but paper except to the degree that they serve as markers in a set of constitutive rules involved in exchanging goods and storing purchasing power through time. In context, the paper serves as money only to the degree that a sufficient number of people believe that it serves those functions. Because the collective intentions are what give the paper its monetary character, it is also clear that the paper itself is dispensable. Metal or entries in a bank's computers might also serve as money, provided that people accepted them as markers in the set of constitutive rules. In this sense, money depends on the representations of individuals, but it does not depend on the representations of any particular individual.

Erstwhile paper money could become demonetized if a sufficient number of people ceased to believe that it could serve these monetary functions. Some of the police powers associated with state-issued money are aimed at removing the causes (such as counterfeiting) that might undermine people's belief or faith in the monetary character of the money tokens (paper or electronic).

Money, then, is epistemologically objective in that it exists independently of my representations, though not independently of all representations. And it is ontologically subjective—not subjective in the sense of not really existing, but in something more like the sense in which color is subjective. Colors are real; but without observers, light would have a frequency though not a color. Money, as anyone who has found himself "a day late and a dollar short" will attest, exists as a social fact. And while such a social fact depends on our representations and intentional states collectively, one encounters the possibilities of money or the constraints of a lack of money as palpably as one encounters the stone when building a wall or stubbing a toe.

Again, the literature on the microfoundations of money provides just the contrast case against which Searle argues. Search models of money (e.g., Jones 1976 or Kiyotaki & Wright 1989, 1993) seek to explain which objects become money and how they acquire their monetary properties with an appeal exclusively to individual intentionality. In a search model, an object becomes monetized through

a process of each agent's individually regarding it as valuable, and believing that others also regard it as valuable, as an intermediate step in trade, one that reduces the expected costs of finding a suitable trading partner. Monetization is, therefore, closely related to the eductive justification of rational expectations in which the expectations are calculated as forecasts of the forecasts of others. Money in such accounts has no intrinsic value or, if it has intrinsic value because it is a real good like gold, its intrinsic value is not essential to its monetary character. Rather, its monetary character is sustained by all the agents in the economy solving a complex utility maximization problem in which the monetary character of the money good emerges as an equilibrium in which the particular good is the unique (or nearly unique) intermediary in all trades. Search models provide a paradigm for all microfoundational accounts of economic institutions.

Money as an institution is, on the microfoundational account, in need of perpetual intentional maintenance. Its status is fragile and would collapse easily if the optimizers shirked. In arguing that collective intentionality is exercised in a background of constitutive rules, Searle denies that economic institutions must be so persistently maintained by the right intentional states. Searle argues that ordinary economic actions are not the product of rational choice exercised through well-ordered preferences, but operations within internalized rules. The institution of money, on this view, need not be perpetually recreated by the decisions of economic agents in the manner of the search models, but can be taken as a given part of the background (objective relative to any particular individual), the context in which, say, individual shoppers decide to buy this or that particular good.

The problem of macroeconomic ontology has two levels. The first is to establish that there are social facts or institutions that ontologically transcend individual economic agents. The second is to establish that the macroeconomic aggregates are examples of such social facts and to determine their character. Searle's account so far addresses the first level. Social institutions are the product of collective intentionality, but they may form only the background that delineates the possibilities and constraints on individual actions. Can we use Searle's account to address the second level of the problem of macroeconomic ontology?

Searle (1995, 137–147) argues that, besides the mental causation and behaviorism ("billiard ball causation") familiar in much of social science, we should admit a third kind of causation, which recognizes the causal role that the background of constitutive rules plays in determining the actions and outcomes of individuals.⁶ The argument amounts to recognizing that, in some cases, the particular intentional actions of individuals are less important to determining outcomes than the constraints that limit their behavior. The well-known argument of Stigler and Becker (1977) that, with no reference to utility functions, we know that demand curves must slope down because of the budget constraint illustrates how the background can determine the character of the outcome independent of intentional states. Similarly, Herbert Simon's (1957, Part IV, especially chapters 14 and 15) "bounded rationality" is in part driven by the role of constraints. Heiner (1983)

argues that predictable behavior emerges from bounded rationality and background constraints essentially because something like Searle's background dominates individual preferences.

It is a small step to macroeconomic aggregates. Driven by constraints, the behavior of aggregates may sometimes be hardly related to the specific decisions of individual agents. Traffic flow or electricity demand can be characterized as aggregates, and the traffic engineer cares no more about the individual's car trip nor the electric company's load manager about the individual's decision to switch on the air conditioning than does the hydrologist about the individual water molecule. Not only would it be too hard to work from molecules (chemical or human) to the properties of the aggregates, it is beside the point (see Batterman 2001 for an extended treatment of physical examples).

And so, too, with the economy. Money regarded from the point of view of its large-scale movements in the national income or flow-of-funds accounts does not depend on any of its particular representations (i.e., on whether it takes the form of notes or coins in the cash register or of an ATM transaction), and the particular actions of individuals matter not at all relative to the average behavior of all individuals. While these characteristics are, in Searle's view, the product of the causal influence of the constitutive rules (e.g., that purchases are limited by the monetary resources of the individual), they seem to underwrite something quite like the "billiard ball causation" of physics, in the sense that individual intentions do not play any essential role in understanding the particular outcomes, even though intentional agents provide ineliminable substrate for the economic aggregates. The applicability of billiard-ball causation explains why a differential equation may characterize a traffic flow or the course of GDP growth.

That it is beside the point in certain sorts of causal analysis to inquire into the behavior of the individuals that constitute the aggregates is why I previously focused on dimensional analysis: The aggregate price level and the prices of individual goods are not the same and, indeed, not even commensurable (Hoover 1995). Although that characterization remains correct, it is not distinctively economic. The point applies equally to physical aggregation—the difference between the mechanics of molecules and the aggregate characteristics of pressure and temperature. But molecules do not have intentions. And with economic agents we have the extra obligations to explain what happens to intentions. Searle's account of social facts and a background of constitutive rules helps to explain the limited reach of individual intentions at the level of the aggregates and, therefore, supplies a piece that was missing in the earlier account.

Limited reach does not imply that individual intentions have no role to play in macroeconomics. To some extent, Searle may overstate the case in purging social institutions of a role for individual intentional states. Much of macroeconomics is about the interaction of policy makers with the aggregate economy, and, as Searle (1995, 138–139) agrees, the intentional actions of the policy maker may be pertinent to the macroeconomic outcome. An influential individual need not be a policy

maker, but may be an opinion maker. If Bill Gates or George Soros or Warren Buffet takes a publicly observable action in response to an individual intentional state, it may have macroeconomic consequences, hardly different in character to policy actions directed by George Bush or Ben Bernanke.

But I would go further than this. The individual intentional state of the typical economic agent may have no noticeable effect on the aggregate. This is a version of the small-relative-to-the-market idealizing assumption that is frequently employed in microeconomics as perfect competition or in international finance as the small-country assumption. While no one agent may affect the outcome, collectively agents operating in more or less the same direction surely will.

The point is nicely made in a passage of Anthony Trollope's (1869, Chapter 25) novel *Phineas Finn*. In the novel, Phineas, a member of Parliament, tries to convince Mr. Bunche, a minor political operative, not to engage in a street demonstration in favor of a certain bill, on the grounds that his action, while possibly dangerous to himself and his family, could not possibly affect the outcome. Bunche replies:

"Look here, Mr Finn; I don't believe the sea will become any fuller because the Piddle runs into it out of the Dorsetshire fields; but I do believe that the waters from all the countries is what makes the ocean. I shall help; and it's my duty to help."

Bunche's position is a direct retort to the economic analysis that concludes that it is irrational to vote, since the marginal vote cannot affect the result and, therefore, the expected benefits of casting a ballot are outweighed by even trivial costs of going to the polls. Mr. Finn goes on:

"It's your duty as a respectable citizen, with a wife and family, to stay at home."

To which Bunche replies:

"If everybody with a wife and family was to say so, there'd be none there but roughs, and then where should we be? What would the Government people say to us then? If every man with a wife and family was to show hisself in the streets tonight, we should have the ballot before Parliament breaks up, and if none of 'em don't do it, we shall never have the ballot. Ain't that so?"

Standard economic analysis of voting employs the model without collective intentionality. Mr. Bunche anticipates Searle in seeing that he can join with a group each to intend as a collectivity of voters. And what is more significant for us, the aggregate outcome, although it is not significantly altered by the individual identities of the voters, is determined by their collective intentional states.

In this respect, macroeconomic aggregates are importantly different from physical aggregates. How they relate to one another depends on the collective intentional states of underlying individuals, and explains why there are sources of instability in the relationships among economic aggregates not found in the relationships among physical aggregates. This is, of course, the insight behind Lucas's (1976) critique of macroeconometric policy analysis. Lucas argued that individual economic agents who understood the role of economic policy would make

different choices and, therefore, would contribute to different aggregate outcomes as policy rules changed. As a result, the policy maker was not entitled to take the relationships among aggregates as a stable background that could be used reliably to predict the effect of a policy action.

The Lucas critique appears, then, to be compatible with a Searlean account of macroeconomic aggregates. The standard reaction to the Lucas critique, starting with Lucas himself, is to argue, first, that the only way to stabilize the relationships among macroeconomic aggregates is to base the analysis in the causal bedrock of "deep parameters" and, second, that those deep parameters are the ones that govern the tastes and technology of individual economic agents (Lucas 1980, 708–712; cf. Sargent 1981, esp. 233, 242). The second part of the reaction reflects the ideology of microfoundations and ignores the superindividual quality of economic facts and institutions based in collective intentionality. On the one hand, some of the social features that underwrite economic aggregates may be those for which the behavior of particular individuals is irrelevant, so that the deepest relevant parameters appear in institutional arrangements that form the background of constitutive rules in which individuals operate. On the other hand, macroeconomic aggregates sometimes may reflect the collective outcome of the intentional states of individuals operating within those rules. This second consideration implies that the analysis of what a typical individual might do may sometimes give insight into the collective outcome: The individual optimization problem is not wholly irrelevant to macroeconomics. But the first consideration undercuts the case that one can *deduce* the aggregate outcome from the behavior of a typical individual without regard to background of constitutive rules or economic institutions. There is no basis for the idea that a single agent or group of agents, just like one of the actual individuals but blown up to economy size, provides a legitimate basis for deducing the behavior of economic aggregates. The focus of early macroeconomists on fallacies of composition is warranted by the recognition that the collective intentionality generates a set of causal structures that is systematically distinct from the underlying individuals.

3. IMPLICATIONS FOR APPLIED MACROECONOMICS

At the end of Section 1, I posed two questions: First, is there a successful ontology of macroeconomics? Second, what implications does (or should) this ontology have for practical macroeconomics? If a macroeconomic ontology grounded in a Searlean account of collective intentionality permits us to answer yes to the first question, where does this leave us with respect to the second question?

Empirical economics from Davenant and King in the seventeenth century to today has largely dealt in aggregate quantities. It is only recently that detailed

individual data or data from economic experiments has become available. Some eighteenth and nineteenth century economists (e.g., Tooke, Jevons) tried to use aggregate data to understand the causal structure of the economic world; yet, economists of the day were frequently skeptical. John Stuart Mill, for example, gave a detailed account of how to infer causation from experimental data with his canons of induction (Mill 1851), but he regarded the economy as too complex to be dealt with in an experimental manner. For Mill, the method of economics had to be *a priori*, based on the logic of the economic situation. Since deductions from theory led to conclusions about behavior in isolation from a huge range of complicating factors, they—at best—indicated *tendencies* of the economy. Economic statistics could illustrate these economic tendencies, but the failure of the data to conform to the theory could not provide evidence against the theory—too many complicating causes might explain away the deviations. Economics, for Mill, was an “inexact and separate science” (cf. Hausman 1992). Extreme versions of Mill’s *apriorism* are found in such later economists as Robbins (1935) and Von Mises (1966). Their vision of economics as the science of the allocation of scarce resources to their optimal uses (optimization subject to constraints) provides the underlying vision of modern microeconomics.

Many nineteenth- and early twentieth-century statisticians and economists also doubted the applicability of statistics to economic data. Such data rarely appeared to conform to the laws of probability: They were not derived from controlled experiments and, when observational, rarely conformed to common probability models. They were, for example, almost never normal and frequently displayed trends or correlations between successive time-series observations.

Trygve Haavelmo (1944) initiated the modern period in econometrics with the idea that economic data could be divided into a systematic component (the economics) and an unsystematic component (the residuals). The residuals formed by conditioning the data on its systematic causes could be regarded as a random sample, conforming to the laws of probability, and be subjected to ordinary statistical analysis. The essential problem in Haavelmo’s account of econometrics was to identify the systematic component. The identification problem had been recognized well before Haavelmo (see Morgan 1990, Chapter 6; Hendry & Morgan 1995, Section 3).

Supply and demand provide the classic illustration. Supply depends on price, and demand depends on price; the observed quantity and price are the intersection of the supply and demand curves. If all we observe are the prices and quantities at these intersections—even if, because of random shocks, these are not constant—how can we learn the shape of the supply and demand curves? If we happen to know that supply (say, of corn) depends on weather, as well as price, and demand depends on income, as well as price, and if weather and income are themselves variable, then variations in weather will shift the supply curve independently, tracing out the demand curve, and variations in income will shift the demand curve, tracing out the supply curve. In the argot of econometrics, the supply and demand

curves are *identified conditional on the restrictions* that weather does not appear in the demand curve and income does not appear in the supply curve.

How—aside from common sense—do we justify these identifying restrictions? Haavelmo and later the Cowles Commission (Koopmans 1950 and Hood & Koopmans 1953) sought the source of the restrictions in economic theory. But economic theory, in their usage, was hardly more than organized common sense. It was not usually called upon to provide any detailed understanding of underlying causal mechanisms. Rather, it merely suggested a list of factors that might affect one aggregate function or curve but not another. And this list may or may not be suggested by a microeconomic model. The macroeconometric models of the 1950s and 1960s, identified in this framework, can be thought of as articulating “billiard-ball” causal relations among aggregates.

Liu (1960) was skeptical of what Sims (1980, 1) later referred to as “incredible” identifying restrictions. Both Liu and Sims suggested ignoring theory in favor of purely statistical characterizations of the data—a position that Sims later modified substantially (see Hoover 2006b). Although Sims’s criticism had important practical consequences for macroeconometrics, it was the Lucas critique that sounded the knell for the first-generation macroeconometric models. Lucas’s advocacy of microfoundations was essentially a reaction to the billiard-ball causality of the macroeconometric models, motivated by the desire to place intentional agents into empirical macroeconomics.

One reaction to the Lucas critique was to apply the representative-agent model to aggregate data. The optimization problem of the representative agent implies a set of restrictions—more complicated than the exclusion restrictions typically employed in earlier econometric models—that identify the equations of the optimization problem itself. This is an ideological response in the sense that, even within the terms of the individual intentionality of microeconomic theory, we know that aggregates do not conform to relationships that mimic, on a larger scale, microeconomic relationships (see Section 1).

What is more, if we take the representative-agent models seriously, then they imply more restrictions on the data than are needed for identification, and such overidentifying restrictions can be tested. For the most part, estimates using actual data reject the overidentifying restrictions. This would come as no surprise to Mill. And one camp of modern macroeconomists have adopted the Millian response of privileging a priori economic theory. The calibration methodology of Kydland and Prescott (1982, 1991) starts with economic theory, supplies values to key parameters of both the systematic and random components, and then simulates the model. The output of the models is then compared to the statistical characteristics of the actual data.

The calibration methodology has substantially more modest goals than the earlier macroeconometric methodologies. It is not generally used to explain or to predict the specific paths of observed variables. Rather it is used to explore the generic operating characteristics of a simulated economy, to explain what sort of

tendencies might arise in an economy with, for example, different policy rules or different sources of random variation. In focusing on such generic characteristics, the calibrationists follow Mill: They hope that the models shed light on the actual data, but they do not regard the data as capable of falsifying the theory that informs the model.

Calibrationists use a representative-agent framework—in part, because it is tractable. The problem is not merely a technical one. Despite offering eschatological justifications for their models, their project of true microfoundations is hopeless. Consider a physical analogy. A hydrologist might model turbulence by constructing a computer simulation that models something on the order of 10^{27} to 10^{30} molecules (i.e., a billion billion billion to a billion billion billion billion) individual molecules according to the laws of mechanics. He sets the simulation running and observes what sorts of macrophysical outcomes are produced.⁷ Why should the truly committed microfoundationalist not attempt the analogous exercise and create a calibrated model of individual agents?⁸ Our ontological analysis suggests that such an exercise would not capture the features of the macroeconomy. It fails to make room for collective intentionality and its products, the background of constitutive rules that shapes the causal interactions of economic aggregates. Indeed, it would obscure the collective intentionality that lies behind the very phenomena that interest us when we pose macroeconomic questions. Individuals not only create social objects through collective intentionality, but they act and choose within the constraints of such structures. The ontological problem is that, contrary to the microfoundational ideology, physical things and individual actors are not all that exist independently.

The behavior of aggregates is driven both by the constraints of constitutive rules and by the collective intentionality of individual agents. Here I equivocate on *collective*, taking it both to refer to Searle's "we believe" and "we intend" and to the straightforward aggregation of the decisions guided by the intentional states of individual agents. The first sense helps to make aggregates different in kind from their microeconomic analogues; the second confirms the insight of the microfoundationalists that individual intentions do matter to aggregate behavior. I do not see any way that we can know a priori how to strike the balance between the relevance of individual intentions and constraints of collective intention. This is a question for empirical research in macroeconomics.

In Hoover (2006b) I distinguish between two methodological visions for empirical investigation in macroeconomics. The *Walrasian* method is the bottom-up approach of the engineer, who starts with the foundations in microeconomics and works to the superstructure of macroeconomics. No estimates of macroeconomic relationships can be secure unless they reflect the correct articulation of the economic structure. We have to start from first principles. The *Marshallian* method is the top-down approach of the archaeologist. Like the engineer, the archaeologist believes he is dealing with a well-defined structure, but his problem is literally how to *discover* it. He works down toward the foundations and sees no warrant for inferring what they must be like in advance of the investigation.

The Marshallian approach is not atheoretical. Nor is it opposed to micro-foundations, in the sense of looking for deeper structures behind the relationships of macroeconomic aggregates. It simply does not assume that the useful level on which to locate such structures is necessarily the level of the microeconomic agent. The Marshallian approach is, then, very close to what Auyang (1998, Chapter 1) refers to as "synthetic microanalysis." Early rational-expectations models provide an illustration of synthetic microanalysis (e.g., Sargent & Wallace 1975).

These models were essentially IS-LM-aggregate supply models, not representative-agent models claiming identification from microfoundational restrictions. The fundamental insights of such models are, first, that expectations should play a role in individual choice (for example, in labor supply or investment demand), and, second, that expectations that differed systematically from realizations would provide profit opportunities that individual agents could easily exploit. If we were truly dealing with individuals, we would want to understand the process of expectations formation, and we would want to model it in such a way that expectations would converge on actual realizations. But in these early rational-expectations models, this is not what is done. Instead, the aggregate implications of expectations are modeled in the form of a restriction that the mathematical expectation of a variable (X_t), conditional on available information (Ω_{t-1}), does not differ from the realization of that variable by more than a random error:

$X_t - E(X_t | \Omega_{t-1}) = \varepsilon_t$, where $E(\cdot | \cdot)$ is the conditional expectations operator and ε_t is an identically distributed, independent random-error term. A crucial assumption is that the information set (Ω_{t-1}) includes the structure of the model itself. This amounts to defining the expectation of X_t ($\equiv X_t^e$) as the forecast of X_t that is generated by the model itself.

The variable X_t^e is not observable, except in the sense that we can back it out of the model. It is not the average expectation of individual agents nor even an aggregate, if by aggregate we mean something that is composed of smaller parts. Rather it is a system characteristic, the numerical manifestation of the restriction that neither the econometrician, the purveyor of the model, nor the policy maker, who might be a character in the model or a user of the model in the real world, can have a systematic informational advantage over individual agents. As such X_t^e does not possess causal capacities of the type championed by Cartwright (1989) that can be carried from one context to another. It is not a cause or effect, but a summary statistic without causal efficacy. The aggregate rational-expectations hypothesis is not the hypothesis that expectations are formed in any particular manner; rather it is the claim that the true causal variables of the macroeconomy are related to one another in a particular nonlinear manner, captured by what macroeconometricians refer to as *cross-equation restrictions*.

The rational-expectations hypothesis is an example of synthetic microanalysis because it begins with an insight into the macroeconomic implications of individual economic behavior. Like its closely related sister, the *efficient-markets hypothesis* of finance (Sheffrin 1983, Chapter 4), it conjectures that profitable or utility-improving

options are not left unexploited. But it is a systems characteristic that operates on a plane distinct from individual choices. It in no way depends on the particular detailed economic histories of individual agents. We can, of course, compare the realizations of X_t^e to, say, individual reports or surveys of individual expectations. We would be surprised if the rational-expectations hypothesis were empirically verified at the macroeconomic level and, at the same time, movements in individual expectations failed to track X_t^e . Nevertheless, X_t^e does not directly characterize any individual expectation nor even their means or other statistical features. It is a feature of the macroeconomy that reflects the intentionality of individual economic agents without being reducible to their individual intentional states.

4. THE IRONY OF IDEOLOGY

Reductionism in physical and life sciences faces a number of challenges. There are causally significant macro features of the world that do not seem to depend on the details of the underlying micro arrangements. Reductionism in economics faces a similar challenge, albeit with additional complications. Economics is an intentional science. Whereas physical and life sciences fear anthropomorphic, teleological, or intentional explanations, economics would be denatured without them. As a human science, it demands that observed behavior be connected to goals, choice, and other intentional states. Economists are skeptical of billiard-ball causation because it omits the human side of human agents and their behavior. Recent economics embraces the program of microfoundations, which denies the independent existence of macroeconomic quantities. The challenge for any antireductionist macroeconomics is to provide an account that both assigns an independent ontological status to microeconomic individuals and to macroeconomic aggregates *and* provides an intelligible account of the connection between the intentional states of the individuals and the behavior of the aggregates.

We have argued that Searle's (1995) account of social facts as a background of constitutive rules created through collective intentionality and constraining the actions of individuals provides a rich understanding of the independence of super-individual economic structures from particular economic agents while preserving channels through which the beliefs, intentions, choices, and actions of individuals influence the behavior of macroeconomic aggregates. The irony of the program of microfoundations is that, in the name of preserving the importance of individual intentional states and preserving the individual economic agent as the foundation of economics, it fails to provide any intelligible connection between the individual and the aggregate. Instead, it embraces the representative agent, which is as close to an untethered Hegelian World (or Macroeconomic) Spirit as one might fear in the microfoundationalist's worst nightmare.

NOTES

1. Presented at the conference on Issues in the Philosophy of Economics, University of Alabama at Birmingham, 19–21 May 2006. Thanks to Paul Teller and Harold Kincaid for useful discussions on an earlier draft.

2. Fittoussi and Velupillai (1993) attribute the coinage of *macroeconomics* to Ragnar Frisch in 1931. But that is not right; Frisch used the term *macrodynamics*, albeit in a similar sense. A discussion on the History of Economics Society's listserv indicated that Böhm-Bawerk referred to the *microcosm* and the *macrocosm* of a developed economy in a sense quite similar to modern usage of *microeconomics* and *macroeconomics* as early as 1891; while the earliest use of *macro-economic* (with the hyphen) recorded on the JSTOR journal archive is in an article by J.M Fleming (1938).

3. LeRoy (1983) provides a persuasive reading of Keynes on the relationship between the individual and aggregate marginal efficiency of capital—the key to the investment function.

4. I have described a pure representative-agent model, which is in fact very common. But the point that I making applies equally to most so-called heterogeneous-agent models, in which a small number of types of agents are representative of large groups in the economy.

5. See Hoover (2001a, Chapter 3) and the references therein for a fuller discussion.

6. By “billiard ball causation,” Searle (1995, 139) refers generally to nonintentional efficient causes.

7. I am not committed to this being workable research strategy for the hydrologist. Philosophers and physicists (e.g., Auyang 1998 and Batterman 2001) suggest reasons why the behavior of individual molecules, treated merely as interacting individuals, may not account for the features of the observed macrophysical data. However that may be, the problem for the hydrologist will not turn on the intentional states of the molecules.

8. In fact, agent-based simulation models have been constructed (e.g., Tefatsion 2006), though none with the scope of typical calibrated macroeconomic models and, to the best of my knowledge, none by the school of economists most committed to the ideology of microfoundations.

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