

## SEVENTEEN

### Does Macroeconomics Need Microfoundations?

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As I observed in the first lecture, I chose Pissarides's model as a paradigm of the modern macroeconomic model for a variety of reasons: the clarity of its goals and exposition; the manner in which it attempted to relate its theoretical construction to empirical facts (at least in principle); and, by no means the least important reason, because it was the model that Nancy Cartwright held up as an example of a nomological machine in economics. A number of fellow economists, however, question whether Pissarides's model really is a *macroeconomic* model. Because it appears to model the decision problem of the individual worker and the individual firm, some economists regard it as a microeconomic model. But this is all the better for my purposes because there is a persistent refrain in recent macroeconomics that the only acceptable macroeconomic models are those that have adequate *microfoundations*.

The idea of microfoundations did not originate with the new classical macroeconomics, but the manner in which the new classical macroeconomics has dominated the agenda of macroeconomics over the past quarter century has firmly cemented it in the minds of virtually all economists. Lucas puts it clearly when he longs for an economics that does not need the prefixes "micro" or "macro" – sound economics is held to be microeconomics, and any macroeconomics that is not just a shorthand for the manner in which microeconomics is applied to certain problems is held to be bad economics.<sup>1</sup>

Lucas advocates the euthanasia of macroeconomics and has spent most of his career supplying pills to hasten the demise of the once proud models of the macroeconomic era. It has taken time, but we have reached the point at which there are graduate students for whom John Hicks's IS/LM model

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is just a dim memory from an undergraduate textbook and whose first lecture in their graduate macroeconomics courses began with a Hamiltonian describing the dynamic optimization problem of what appears to be an individual agent. Gradually, undergraduate textbooks are following suit, and even the econometric forecasting models of the United States Federal Reserve System have undergone surgery to remove the IS/LM model that once was the beating heart of their more than two hundred equation system. That the profession has sworn allegiance to the ideal of microfoundations is beyond doubt. The question before us is whether they are right to do so.

### Some History

The earliest empirical economics is macroeconomics. The word “economics” derives from a Greek word meaning the management of the household. The earliest name for our subject, “political economy,” consciously drew the analogy between the management of the household and the management of the state. But the politics of the seventeenth and eighteenth centuries was somewhat different from the politics of the nineteenth, twentieth, and twenty-first centuries. The transition to individualism was incomplete, and it was not uncommon for the political theorists of the day to think more of the social hierarchy as king, aristocracy, merchants, farmers, peasants, and so forth with little regard to the role of the individual. The early statistical researches of William Petty, Gregory King, and Charles Davenant were aimed not at understanding the economic behavior of particular people but at determining the capacities of England and Ireland to support the military ambitions of the English king. The models of François Quesnay and the Physiocrats, which bear many structural and methodological resemblances to modern macroeconomic models, went a step further. Again, appealing to the division of French society into broad classes (nobility, farmers, artisans), they gave normative advice to the French king on how to direct the economy in a manner that would enlarge his military capabilities.

The macroeconomic models of the seventeenth and eighteenth centuries were not supplanted all at once in a wave of individualism. The seeds had to be planted. The beginning of wisdom was the notion promoted by Adam Smith and the great Scottish political economists that the source of social welfare was the individual welfare of the ordinary man. We are so used to the idea that economics is about harnessing individual self-interest for social harmony and to attributing this idea to Smith, that we forget how limited were his claims for individualism. We remember the “Invisible Hand,” but this image appears only once in the *Wealth of Nations*, in the context of

foreign trade (and in two other instances in Smith's noneconomic works). Bernard Mandeville, early in the eighteenth century, in *The Fable of the Bees*, put the point that private vice (greed) could promote public virtue far more clearly than did Smith. But Smith took a dim view of Mandeville. Smith, David Ricardo, and the other classical economists were mainly concerned with market phenomena, and the individual played a relatively weak analytical and empirical role in their arguments.

With marginalism in the middle of the nineteenth century, the analytical ground shifts more clearly to the individual, but market phenomena remain the focus of William Stanley Jevons and the English political economists. It is in the work of the French economists Augustin Cournot and Leon Walras that the individual is truly made the analytical center of economics and the problem of how individuals coordinate socially, usually ascribed to Smith, takes center stage.

The political philosophy of the late nineteenth century is marked by debates over the relative explanatory role of individualism versus superindividual categories. Marxists led the way. For them, classes determine men, rather than men determining classes. (Yet, one should note that Karl Marx's economics owed its analytical framework to Smith and Ricardo and so was tainted, at least as far as they went with it, by individualism.) Austrian economics presented a clear contrast in which Carl Menger and, in the twentieth century, Ludwig von Mises, Friedrich von Hayek, and others espoused *methodological individualism*: the doctrine that the only well-grounded explanations of social phenomena were ones that appealed to the actions and behaviors of individuals.

English and American economics maintained an incomplete individualism. Although Alfred Marshall managed to kill the "political" that had long modified "economy" in the name of our discipline, his object was more to refocus attention on the analytics of the subject rather than on the applications. (The term "political economy" has been reborn in the past twenty years, though it conveys a very different sense now than it did in Smith's time.) Marshall discussed the particular firm and the particular worker or consumer. But, like his English and Scottish forefathers, he did so mainly to illuminate markets. The analyzed individual is meant to typify individuals in general. It is to Marshall, with his discussion of the "representative firm," that we owe the idea of the *representative agent*.<sup>2</sup> Still, Marshall's markets are not economy-wide, but are focused on particular products. Economics by 1930 appears mainly to be microeconomics. Yet, the proto-macroeconomics of the earlier time did not completely vanish. It is clearly evident in theoretical discussions of money, especially of the quantity theory, which never

succeeded in finding adequate grounding in individual analysis. And it is evident in empirical discussions of business cycles, which were regarded as economy-wide phenomena.

So things stood in the mid-1930s, when John Maynard Keynes was writing the *General Theory*. Keynes did not invent macroeconomics, nor did he use the term. (As far as I can discover, Ragnar Frisch was the first to use the term, in 1931, though it became current only after the Second World War.)<sup>3</sup> Keynes, nevertheless, clarified the distinction between what we now call macroeconomics and microeconomics and made it possible for us to ask the question, how are the two related? As is evident in his discussion of the consumption function (the marginal propensity to consume follows from a “fundamental psychological law”), investment (entrepreneurs optimize with respect to opportunity costs), and the demand for money (speculators anticipate capital gains or losses), Keynes follows Marshall in looking to the individual decision problem for illumination. These appeals to individual behavior remain in the service of aggregate explanations. Despite the fact – largely ignored in potted histories – that he stresses the heterogeneity of individual responses as a central feature of aggregate behavior, Keynes never explores the relationship between the individual and the aggregate in any really systematic way.

Microeconomics so dominated economic thinking in 1936 that the cry for microfoundations for the newly resurgent macroeconomics was almost immediate. Jacob Viner and Wassily Leontief wrote microeconomic criticisms of the *General Theory*.<sup>4</sup> Lawrence Klein, in his *Keynesian Revolution*, thought it necessary to discuss the microeconomic underpinnings of the principal Keynesian aggregate functions.<sup>5</sup> The history of the first twenty-five years of postwar macroeconomics is largely the hanging of micro-economic flesh on the skeleton of interpretation of Keynes’s *General Theory* formalized in Hicks’s aggregate general-equilibrium, IS/LM model. James Dusenberry, Milton Friedman, and Franco Modigliani tried to explain the microeconomics of consumption; William Baumol and James Tobin, the demand for money; Dale Jorgenson, investment; Don Patinkin, labor; and so forth.<sup>6</sup>

Beginning with Robert Clower’s dual-decision hypothesis and Robert Barro and Herschel Grossman’s fixed-price models, the urge for microfoundations began to infect the general-equilibrium framework.<sup>7</sup> It is no longer enough that each function have an individualistic foundation; since individuals are assumed to be making choices to generate each function separately, those choices really ought to be coordinated and consistent. This is a hard problem with heterogeneous agents. The modern representative agent, which is essentially a homogeneity assumption, made his appearance first

in these models. At more or less the same time, Lucas and Leonard Rapping began to model unemployment as an optimization problem. Lucas made consistent optimization in general equilibrium the centerpiece of his monetary model published in the *Journal of Economic Theory* in 1972.<sup>8</sup> Strictly speaking, this model is not a representative-agent model. Yet, it is highly idealized and assumes that all individuals are fundamentally identical. From there, it is only a short step to the representative-agent models that have dominated new classical macroeconomics since the early 1970s.

### Reductionism

So much for a brief history of the movement for micro-foundations in economics. What are the intellectual roots of this urge to ground macroeconomics in the individual? It has analogies in other sciences. The nature of scientific explanation is a hotly debated subject among philosophers and scientists. One plausible view is that a theory is explanatory when it achieves parsimony: if a complex phenomenon can be reduced to some smaller number of governing principles, then we regard the complex phenomenon as having been explained.

In the eighteenth century the ideal gas laws were formulated. The Boyle-Charles law states that

$$pV = nRT,$$

where  $p$  is pressure,  $V$  is volume,  $n$  is the number of moles of the gas,  $R$  is the universal gas constant, and  $T$  is temperature. As the name suggests this law is an idealization of the results of empirical observations and holds with a high degree of accuracy at moderate temperatures and low pressures.

The gas law appears to be an approximate truth about physical reality, but nevertheless physicists were not happy with its *sui generis* quality. The solution is found in the kinetic theory of gases, which provides an account of the gas laws as a deduction from Newtonian mechanics. The kinetic theory is also based on an idealization: the gas is assumed to be composed of molecules regarded as perfectly elastic point masses. With the added assumption that the velocities of the molecules are distributed according to a particular random distribution – that they are equally likely to move in every direction – it is possible to derive the gas laws. Temperature corresponds to the mean energy of the molecules and pressure to the mean momentum transferred by contact with the walls of the containing vessel. The kinetic theory of gases thus constitutes a *reduction* of the macrophysical gas laws to the microphysical Newtonian mechanics.

Notice two features of this reduction. The first is that it is not micro all the way down. In addition to Newton's laws, the kinetic theory relies on a statistical assumption – that is, an implicitly macro assumption. Also, notice that the categories that apply to Newton's laws and to the gas laws are very different. A single molecule has momentum and energy, but it does not have pressure or temperature. To make the derivation work, it is necessary to identify aggregate properties of the collection of molecules (their *average* energy and momentum) as corresponding to the macro properties (temperature and pressure) that have quite different sensible characteristics. The phenomena of temperature and pressure can be thought of as *emergent properties* of the aggregation of molecules.

Reductionist strategies are pursued throughout science. Recently, in biology, a lot of effort has been directed to reducing macrobiological phenomena to the micro principles of genetics and organic chemistry. But even here, the effort is controversial, with one wag saying: “the only way to reduce biology to chemistry is through death.”<sup>9</sup> The philosophical mind/body problem has, in the age of neuroscience, also generated a debate over reductionism. The issue is whether mental states can be completely explained by knowledge of brain states. Even if they could, the issue of the phenomenological difference between the two levels is larger here than it is with respect to the gas laws. Seeing a beautiful woman does not seem to be the same kind of thing as any pattern of neuron firings. Vision and, to a greater degree, aesthetic appreciation appear to be emergent properties, even if there is a reduction.

The situation is even more complex than that. You and I can see the same thing even though our brain states are not the same. Similarly, you can see the same thing at different times even though your brain state is different at each time. There is no one-to-one mapping between the macro phenomena of mind and the micro phenomena of brain states. This observation has led to the notion of *supervenience*. Mental states are said to supervene on brain states in the sense that any time one could exactly reproduce a certain brain state and collateral conditions, the same mental state would occur, even though that mental state may occur for other configurations of brain states as well, and even though the appropriate phenomenological descriptions of the mental state are completely different from those of the brain states. Supervenience guarantees the autonomy of the macro level in the sense that it ensures that one can rationally use an independent language and categories to describe the macro level and that one should not expect to find unique deductions from the micro to the macro. Yet, it also underscores the connection between the micro and the macro: no macro state exists unless an appropriate micro state exists. Supervenience has been offered both as a

way of eliminating the need for reduction and as a justification for a weaker form of reduction. Which way one looks at it partly depends on what one views as threatened.

### Economics and Methodological Individualism

So what about reductionism in economics? Whether economic explanations must be reductive depends in part on how one defines economics. An older tradition defines it with respect to certain areas of human life. The classic definitions can be summarized in a word: *plutology*, the science of wealth. John Stuart Mill writes:

Writers on Political Economy profess to teach, or to investigate, the nature of Wealth, and the laws of its production and distribution: including, directly or remotely, the operation of all the causes which the condition of mankind, or of any society of human beings, in respect to this universal object of human desire, is made prosperous or the reverse.<sup>10</sup>

Similarly, Alfred Marshall writes:

Political Economy or Economics is a study of mankind in the ordinary business of life; it examines that part of individual and social action which is most closely connected with the attainment and with the use of the material requisites of wellbeing.

Thus it is on the one side a study of wealth; and on the other, and more important side, a part of the study of man.<sup>11</sup>

Modern economists almost all follow the much different definition of Lionel Robbins:

Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses.<sup>12</sup>

Economics is, in Robbins's view, the science of choice. Economics is, in modern terminology, microeconomics.

Once microeconomics is seen as defining the very nature of economics, any macroeconomic phenomenon will be seen to need a reductive explanation. Of course, it is one thing to want such an explanation and quite another to have it. It is obviously impractical to dispense with measurements of temperature and pressure and to keep track of the velocities of each and every molecule even in a relatively small volume of gas. Similarly, it is absurd to think that practical economics can trace the decisions and constraints facing each individual agent in the economy. I call this the *Cournot problem*, because the first clear statement of it is found in Cournot's

*Researches into the Mathematical Principles of the Theory of Wealth* (1838). No one really denies the Cournot problem; the only question is what to do about it.

Notice that the motivations for seeking a reduction are different in economics than they are in biological sciences. Biologists are suspicious, for instance, of mental explanations because they involve *intentional* states: beliefs, purposes, desires, will, goals, and so forth. Human mental life is *teleological*; that is, it is directed to ends. The reduction of the mental to the neurological is appealing to scientists precisely because neurons, chemicals, molecules, genes, and such do not have ends or intentional states. Reduction banishes teleology. In economics, it is just the reverse. Macroeconomic relations, say as represented in Okun's law, which relates changes in the unemployment rate to the growth rate of gross domestic product (GDP), are not obviously intentional anymore than the gas laws are. But if macroeconomic relations are regarded as the products of human action, this could be seen as a defect. The goal of reducing macroeconomics to microeconomics is to recapture human intentions. Reduction reclaims teleology.

The difference is clear in what is probably the most influential paper in macroeconomics in the postwar period: Lucas's "Econometric Policy Evaluation: A Critique."<sup>13</sup> Lucas criticized the empirical macroeconomics of the day – especially the large-scale macroeconometric forecasting models – on the basis that their equations captured transitory correlations in the data that would not remain stable in the face of changes in policy regimes. His idea is that people make choices subject to constraints that include their best expectations of government policy. If the government uses the macroeconomic models to guide its policy choices, it will surely find that the models fail as soon as it changes its policy, because agents will adapt to the constraints of the new policy. Projecting macroeconomic relationships estimated in the past into the future implicitly assumes that the policy of the past continues. But if the government uses those projections to guide changes in its policy, then it assumes that people expect the old policy, even while a new policy is in place. People are not stupid, so the past projections are bound to fail. The most common response to the Lucas critique (for example, in the program of Lars Peter Hansen and Thomas Sargent and, more recently, in real-business-cycle models) was to argue that economic projections would be secure only if they were grounded in a deep analysis of the decision, problems faced by individuals, including their detailed understanding of the structure of policy.<sup>14</sup> A model was said to be secure from the Lucas critique only if it was grounded in relationships built up from the "deep parameters" corresponding to tastes and technology. Only



well-specified optimization problems were supposed to provide a secure basis for economic prediction. In other words, macroeconomics must be reduced to microeconomics. The conviction that macroeconomics must possess microfoundations has changed the face of the discipline in the last quarter century.

That the argument for microfoundations should have been so successful rhetorically is, I think, puzzling. For it ignores the obvious difficulties in empirical implementation posed by the Cournot problem. As I said before, no one believes that economists can practicably trace the decision problems of millions of individuals and aggregate them to discover macroeconomic behavior. The intellectual triumph of microfoundations is grounded not in methodological individualism (that is, in a strategy of basing all empirical explanations on the behavior of individuals) but in *ontological individualism* (the conviction that the only real entities in the economy are individuals). Who could disagree with that?

Well, I would. Unfortunately, the full argument for this position would take us further down a metaphysical byway than any group of economists is likely to want to go. Still, I would at least like to poke a few holes in the presumption that ontological individualism is necessarily correct. The fear of the ontological individualist is that if he says that macroeconomic entities like GDP or the general price level are real, he must also say that they are independent of the individual people who constitute the economy. The second claim is, of course, obviously wrong, but ontological individualism does not follow from denying it.

The relationship between microeconomics and macroeconomics could be one of supervenience. Any identical reconfiguration of the agents in the economy and their situations results in the same configuration of the macroeconomic entities in the economy, but the mapping is not one to one. What is more, the supervenience of the macroeconomy on the microeconomy is not just a weak form of reductionism. This is because of intentionality at the microlevel. Individuals have to make plans and decisions on the basis of expectations about the future. In so doing, they face precisely the same problem that is faced by the economist from his detached perspective: the economy is too complex for a detailed microeconomic account to inform the construction of expectations. Individuals, just like economists, face the Cournot problem. When I try to figure out how much money to put aside to pay for my daughters' college education, I must make guesses about future inflation and interest rates, as well as about my own income. I cannot do that by constructing a realistic computable-general-equilibrium model of the economy. Instead, I use simple macroeconomic models (indeed, crude

time-series models, such as one that says that future interest rates will be the average of past interest rates). But this means that I cannot completely reduce macroeconomics to microeconomics. Microeconomics of the real world necessarily uses macroeconomic models and concepts as an input. The macroeconomy supervenes on the microeconomy but is not reducible to it.

### Aggregation and the Illusion of a Microeconomic Ontology

While I am convinced that the impulse that made the microfoundational argument succeed is ontological and not methodological, it would be absurd not to acknowledge the methodological sea change in macroeconomics after the Lucas critique. Macroeconomic models look like microeconomic models (hence the reaction that my use of Pissarides's model provoked among my colleagues). The same techniques, the same mathematics, the same language is used. But this is truly puzzling. The physicist who has successfully reduced the ideal gas laws to the kinetic theory of gases does not then abandon the language of pressure, temperature, and volume when working with gases or try to use momentum, mass, and velocity as the principal phenomenological categories for discussing the macroscopic behavior of gases.

But economists have taken a different tack. They have typically started with the microeconomics of the individual and then asked to what degree the lessons learned at that level can still apply to aggregates of individuals. There is, in consequence, a vast literature on the theory of aggregation. The general conclusion of this literature is that aggregation in which the macro looks like the micro can occur only under circumstances so stringent that they could never be fulfilled in the real world except by the merest chance. I want to argue something even stronger than that; namely, that even what appears to be perfect aggregation under ideal circumstances fails. But, first, let us consider the lessons of aggregation theory as they stand.

Economics is about heterogeneous things. In microeconomics we choose how to allocate our consumption among different goods or how to allocate factors of production used to make those goods. In both cases, we consider physical things of disparate natures and somehow have to make them equivalent. The role of utility functions or profit functions is to give us a common denominator, a basis for choosing among goods that otherwise are little alike. Similarly, when we calculate nominal GDP, we cannot add up the disparate goods until we have given them a common denominator – typically, money. Real GDP is even one step further removed, as we correct the

monetary unit of measurement for changes in its own value by constructing a notion of a *general* price level.

Now, the first question asked in aggregation theory is, when is aggregation perfect? – that is, when can two disparate goods be added together and treated analytically as if they were but one good? The criteria are typically economic, not physical, though the first example may seem physical. Suppose that we have a certain quantity of coal and a certain quantity of oil. Coal and oil differ on many dimensions; but, if the only difference of material importance to us is the amount of heat they produce (which dimension matters is the economic criterion), then we can measure each in British Thermal Units (BTUs), rather than in tons or barrels, and add them up in those units. This is the case in which, up to a factor of proportionality, the goods are perfect substitutes. Similarly, in any case in which goods are perfect substitutes on the relevant dimensions, we can aggregate them.

Oddly, the polar opposite case works as well. Consider the manufacture of water through burning hydrogen and oxygen. It takes exactly two moles of hydrogen and one mole of oxygen to make one mole of water. We cannot vary the formula. Hydrogen and oxygen are not substitutable; they are perfect complements. But we can aggregate perfectly by counting bundles of hydrogen and oxygen into bundles:  $2\text{H} + 1\text{O} = 1$  water bundle.

Generally, however, except in these extreme cases, perfect aggregation is not possible. The reason is economic. If goods are neither perfect complements (in which case no change in the mix of the goods is possible) nor perfect substitutes (in which case no change in the mix of goods matters), then the mix of goods can be changed and still yield the same output or utility. How that mix changes depends on relative prices. As the price of a good rises, we purchase less of that good and more of its substitute. This is the basis for the common claim, going back to Hicks, that we can treat bundles of goods as composite commodities, so long as their relative prices do not change: the so-called *composite commodity theorem*.<sup>15</sup>

The composite commodity theorem is true as far as it goes, but notice how special are the assumptions on which it is based. We generally regard prices not as exogenous variables given outside the economic system, but as one of the important products of economic coordination. The proofs of the existence of a general equilibrium, going back to Kenneth Arrow and Gerard Debreu, demonstrate that there is a set of prices that coordinates economic activity. The prices are not themselves parameters, but change as the true parameters (tastes and technology, if we go back to Lucas's formulation) change. The composite commodity theorem, therefore, holds only when

the relevant underlying parameters do not change. How relevant can that be for interesting economic analysis?

Let us illustrate the problem with an extremely simple example. Consider an economy with two consumers and two goods. These goods can be either two goods in a single period or one physical good that can be consumed in two different periods. It does not matter which interpretation we take for the example to work, although the second one is directly relevant to a number of intertemporal macroeconomic models. Let each individual ( $i$ ) choose the goods ( $c_1$  and  $c_2$ ) by maximizing a Cobb-Douglas utility function:

$$u^i = \log c_1^i + \alpha^i \log c_2^i \quad (17.1)$$

subject to a budget constraint

$$y^i - c_1^i - pc_2^i = 0, \quad (17.2)$$

where  $y$  is exogenously given income, and  $p$  is the price of good 2 in terms of the numeraire, good 1. The demand for good 1 is

$$c_2^i = \frac{y^i}{1 + \alpha^i}. \quad (17.3)$$

Letting the superscripted, lower-case letters designate variables that apply to individual agents and upper-case or unsuperscripted letters, variables that apply to aggregates, the idea of the representative-agent model is simple. If equation (17.3) gives the demand for the individual for good 1, then the aggregate demand for good 1 is

$$C_1 = \frac{Y}{1 + \alpha}. \quad (17.4)$$

But, in our simple economy of only two agents, it is easy to check exactly what the aggregate form, of the demand for good 1 should be. It is merely the sum of the two individual demands, so that

$$\begin{aligned} C_1 &= c_1^1 + c_1^2 = \frac{y^1}{1 + \alpha^1} + \frac{y^2}{1 + \alpha^2} = \frac{(1 + \alpha^1)y^1 + (1 + \alpha^2)y^2}{(1 + \alpha^1)(1 + \alpha^2)} \\ &= \frac{Y + \alpha^1 y^1 + \alpha^2 y^2}{(1 + \alpha^1)(1 + \alpha^2)}, \end{aligned} \quad (17.5)$$

since  $Y = y^1 + y^2$ . In general, equation (17.5) does not have the same form as equation (17.4). In fact, the only circumstances in which (17.4) and (17.5) are identical in form is when  $\alpha^1 = \alpha^2 = \alpha$  – that is, when all agents have identical tastes.

As a rule, the conditions are even more stringent than that. I purposely chose a very tractable utility function. The Cobb-Douglas utility function is

homothetic; that is, its indifference curves are each parallel blowups of the indifference curves closer to the origin. Equivalently, the income-expansion paths (that is, the locus of tangencies between indifference curves and budget constraints as the budget constraint is moved outward to reflect increasing income and constant relative prices) are all straight lines through the origin. And this is what the theorists tells us: some technical details and caveats to one side, perfect aggregation from individual agents to a representative agent requires that all agents have identical utility functions and that these be homothetic. Why? Because in these cases, income distribution is not relevant. Because of homotheticity, the ratios of goods consumed by any one individual remain the same whether that individual is rich or poor. And because utility functions are identical, the ratios of goods consumed are the same for any individual. In such circumstances, for a fixed aggregate income, redistributing that income among the individual consumers will not affect demands for individual goods and, therefore, will not affect relative prices. In that case, the conditions of Hicks's composite commodity theorem apply, and we can add up individual quantities to form economy-wide aggregates without loss of information.

Although the example that we have looked at is extremely simple, it carries a very general message. The conditions of exact aggregation are strong and almost certainly never fulfilled in any practical instance. Why should one accept the representative-agent model and the facile analogy from the micro to the macro? Indeed, recently, a number of economists – Rolf Mantel, Hugo Sonnenschein, and Debreu – have shown that theoretically there is no such analogy.<sup>16</sup> No matter how well behaved the microeconomic functions may be, the aggregate functions, given distributional variations, are essentially unrestricted and need not take a form that is derivable in any simple way from the form of the underlying micro functions. This means, for example, that if every underlying production function is Cobb-Douglas, there is no theoretical reason to conclude that the aggregate production will also be Cobb-Douglas. Conversely, if the aggregate production function for an economy is Cobb-Douglas (which to a first approximation it appears to be for the U.S. economy), there is no reason to believe that this tells us anything at all about the shape of the underlying production functions.

There is a strong belief, expressed not only in the ordinary practice of macroeconomics but in the methodological writings of philosophers of economics, that aggregation does not alter the fundamental categories of economics. Whereas in physics molecules have one sort of description and gases, even though they are aggregations of molecules, quite another, in economics real GDP is much like any other real good. Uskali Mäki makes

the point I wish to oppose by saying that economics does not add to the “ontic furniture” of the world given to common sense.<sup>17</sup> This is, I think, an illusion that arises because of the view that perfect aggregation represents a possible limiting case of actual aggregation. The possibility of perfect aggregation suggests the analogy of real GDP to an individual good. If, for example, relative prices are constant (that is,  $P_j/P_k$  is constant for all  $j$  and  $k$ ), then  $\sum_{j=1}^n P_{j,t} Q_{j,t}$  (where the  $t$  in the subscript indicates the base time, period  $t$ ) can be normalized by choosing the units for the  $Q_{j,t}$  so that each  $P_{j,t} = 1$ . Then, nominal GDP at time  $n$  can be written

$$\sum_{j=1}^n P_{j,t+n} Q_{j,t+n} = P_{t+n} \sum_{j=1}^n Q_{j,t+n}. \quad (17.6)$$

Under the assumed conditions  $P$  is unique. Some conclude, therefore, that in this limited case, one can treat the summation on the right-hand side of equation (17.6) as a natural aggregate quantity analogous to an individual quantity. The conditions for constant relative prices are almost certainly never fulfilled; but, even if they were, the summation is not analogous to an individual quantity. The general price level  $P$  in (17.6) still has the dimension period- $n$  dollars/period- $t$  (i.e., base period) dollars. To sum heterogeneous goods, they must still be converted to a common denominator, and in this case, the summation still has the dimensions of period- $t$  dollars. This would be more perspicuous if (17.6) were written as

$$\sum_{j=1}^n P_{j,t+n} Q_{j,t+n} = P_{t+n} \sum_{j=1}^n 1_{j,t+n} Q_{j,t+n}, \quad (17.7)$$

where the subscripted numeral 1 is a place holder for the dimensional conversion.

One might regard perfect aggregation as the idealization of typical aggregation in which quantities are affected by changing relative prices. The upshot of the argument here is that the aggregate remains analogous to the macro gas of the ideal gas laws and is not obviously some natural extension of a single underlying molecule. The ideal gas laws fit well only within a limited range of temperatures and pressures. Outside that range, they, vary in a manner than can be accounted for using the kinetic theory of gases by adding more realistic assumptions about the volume of individual molecules and the forces acting between them. The equivalent in macroeconomics is found in the efforts of Alan Kirman and Kathryn Dominguez and Ray Fair, among others, to account for distributional effects in macroeconomic relationships.<sup>18</sup>

### The Strange Career of the Representative-agent Model

Given what we know about representative-agent models, there is not the slightest reason for us to think that the conditions under which they should work are fulfilled. The claim that representative-agent models provide micro-foundations succeeds only when we steadfastly avoid the fact that representative-agent models are just as aggregative as old-fashioned Keynesian macroeconometric models. They do not solve the problem of aggregation; rather they assume that it can be ignored. While they appear to use the mathematics of microeconomics, the subjects to which they apply that microeconomics are aggregates that do not belong to any agent. There is no agent who maximizes a utility function that represents the whole economy subject to a budget constraint that takes GDP as its limiting quantity. This is the simulacrum of microeconomics, not the genuine article.

This seems transparently obvious. So why have intelligent economists come to believe so fervently both in the necessity of microfoundations and in the efficacy of the representative-agent model in providing them? Let me offer a speculation. One of the earliest examples of modern dynamic economics is found in Frank Ramsey's optimal savings problem.<sup>19</sup> In this problem, Ramsey considered the problem of saving for an economy and imagined it to be a social planner's problem in which the utility function represented social preferences, without conjecturing how these might be related to the preferences of the members of society. Ramsey may well have thought (in the manner of Keynes) that the wise men of Cambridge could be trusted to know what was best for society independently of any direct knowledge of the lower classes. Push-pin may have been as good as poetry for Jeremy Bentham; but Bentham was an Oxford man. In Cambridge the poets ruled and aspired to rule the world. On Cambridge assumptions, there is no problem with what Ramsey did.

By the early 1950s, the general-equilibrium model had been more thoroughly developed and analyzed. The two theorems of welfare economics were established:

1. Every perfectly competitive general equilibrium is Pareto efficient; and
2. Every Pareto-efficient allocation can be supported as a perfectly competitive equilibrium for some set of lump-sum transfers.

These two theorems appear to promise an isomorphism between social planner problems that choose Pareto-efficient allocations and perfectly competitive equilibria. In fact, this isomorphism provides a powerful technical tool for the solution of dynamic optimization problems, because it is often

easier to define a social planner's problem and a Pareto-efficient outcome, and then to ask how to decentralize it, than it is to solve for the competitive equilibrium directly (a trick common in the literature on real-business-cycle models).

Notice that there is a sleight of hand here. Only rarely do macroeconomists care about the redistributions needed to decentralize the social planner's problem. It is fine to ignore redistributions when they do not matter – that is, when all agents are identical and have homothetic utility functions. Once again, the macroeconomists have slipped in unwarranted microeconomic assumptions, as well as, implicitly, assumptions about the shape of the social planner's function. But, if we take the notion of decentralization seriously, we know that everyone cannot be alike. Furthermore, not only does aggregation theory tell us that we do not know how the social planner's function might relate to the underlying utility functions, the older Arrow Impossibility Theorem tells us that, for reasonable assumptions, no social planner's function exists that respectfully and democratically aggregates individual preferences.<sup>20</sup> Thus, the idea of the representative agent appears to arise naturally in dynamic macroeconomic models as a kind of benign extension of Ramsey's social planner in the face of the two welfare theorems. But this idea is plausible only when the macroeconomist fails to take microeconomics seriously.

Could we, nevertheless, not regard the representative-agent model as an idealization? It may be a good way to think about macroeconomic problems when the losses due to aggregation are relatively small. Let us accept that, but notice that whether or not the representative-agent model is a good thing depends now entirely on its contingent empirical success. It may work; it may solve the Lucas critique; it may not. We just have to see. There is no longer a point of principle involved. The advocate of the representative-agent model has no right to attack other macroeconomists for failing to provide microfoundations, for he fails to provide genuine microfoundations himself.

My guess is that the representative-agent model may help in pointing to some sorts of qualitatively useful relationships. But it is unlikely to provide useful quantitative restrictions on the behavior of macroeconomic aggregates. The reason can be seen by thinking about the way in which Marshall used the idea of the representative firm. For Marshall, the representative firm was not the average, or even median, firm, but a firm that typified firms at a point in their life cycle at which the extreme behaviors associated with very small or very young firms, on the one hand, or very large or very old firms, on the other hand, could be set aside. If we can analogize back to



the physicist's ideal gas, Marshall wanted to describe the usual behavior of a gas molecule under certain ideal conditions. The use of representative-agent models in modern macroeconomics attempts something quite different. It attempts to describe the behavior of the gas (its pressure and volume), not by considering seriously how the molecules behave in aggregate, but by analyzing the gas as if it were one big molecule subject to the laws that in fact govern real molecules. This is a category mistake: pressure and volume are descriptions of the properties of aggregates – properties that individual molecules either in reality or idealized to colossal size do not possess as isolated units.

On the analogy with gases, we should conclude that what happens to the microeconomy is relevant to the macroeconomy but that macroeconomics has its own descriptive categories and may have its own modes of analysis. It is almost certain that, just as in the case of gases, no genuine microfoundations can ever be provided for macroeconomics that do not make concessions to the macrolevel in the form of statistical assumptions about the distributions of important microeconomic characteristics. And, given those concessions, it is almost certain that macroeconomics cannot be euthanized or eliminated. It shall remain necessary for the serious economist to switch back and forth between microeconomics and a relatively autonomous macroeconomics depending upon the problem in hand.

### Suggested Readings

As observed in this lecture, the history of microfoundations is a long one. The modern obsession with microfoundations as the sine qua non of macroeconomics can be dated to Robert E. Lucas, Jr.'s "Econometric Policy Evaluation: A Critique" (originally published in Karl Brunner and Allan H. Meltzer [eds.], *The Phillips Curve and Labor Markets*, vol. 1 of Carnegie-Rochester Conference Series on Public Policy, Amsterdam: North-Holland, 1976, and reprinted in Lucas's own *Studies in Business Cycle Theory*, Oxford: Blackwell, 1981). An excellent methodological study of the necessity of microfoundations is found in Maarten Janssen's *Microfoundations: A Critical Inquiry* (London: Routledge, 1993).

More particularly, the modern ploy of providing microfoundations through the representative-agent model is brilliantly attacked in Alan Kirman's "Whom or What Does the Representative Individual Represent?" *Journal of Economic Perspectives* 6(2) (1992), 117–36, and, with a rich historical perspective, in James Hartley's *The Representative Agent in Macroeconomics* (London: Routledge, 1997).

## Notes

1. Lucas (1987), pp. 107–108.
2. Marshall's notion is, as we will see, substantially different from that common in modern macroeconomics; see Hartley (1996, 1997).
3. Frisch used the term in his lectures; Erik Lindahl may have been the first to use it in print in 1939; see Fitoussi and Velupillai (1993).
4. Viner (1936) and Leontief (1936).
5. Klein (1947).
6. Dusenberry (1949), Friedman (1957), Modigliani and Brumberg (1954), Baumol (1952), Tobin (1956, 1958), Jorgenson (1963), and Patinkin (1965).
7. Clower (1965) and Barro and Grossman (1971).
8. Lucas (1972).
9. Vercelli (1991), p. 243.
10. Mill (1848/1911), p. 1.
11. Marshall (1920), p. 1.
12. Robbins (1935), p. 16.
13. Lucas (1976).
14. Hansen and Sargent (1980).
15. Hicks (1946), p. 46.
16. Kirman (1992) and Hartley (1997).
17. Mäki (1996).
18. Kirman (1992), Dominguez and Fair (1991).
19. Ramsey (1928).
20. Arrow (1951).

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