The Representative Agent in Macroeconomics

James E. Hartley

Routledge Frontiers of Political Economy

ROUTLEDGE

THE REPRESENTATIVE AGENT IN MACROECONOMICS

Representative agent models have become a predominant means of studying the macroeconomy in modern economics. Surprisingly, there has been little discussion in macroeconomic literature about the propriety or usefulness of such models. This volume aims to evaluate the use of these models in macroeconomics.

Examining the comments made by new classical economists, the book identifies three related justifications for the modern use of representative agent models: they are a means of avoiding the Lucas critique, they aid in the construction of Walrasian models, and they provide microfoundations for macroeconomics. The author illustrates how the representative agent model is inadequate to these tasks: it does not solve the Lucas critique, does not help create good Walrasian models, and does not provide microfoundations. He evaluates the goals themselves, finding that the Lucas critique is unworkable in its present form, Walrasian methodology is not particularly useful for macroeconomic study, and rigorous microfoundations of the sort representative agent models are presumed to provide are neither possible nor particularly desirable. Representative agent models are, therefore, neither a proper nor a particularly useful means of studying aggregate behavior.

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In the first place he made short work of my "chronological snobbery," the uncritical acceptance of the intellectual climate common to our own age and the assumption that whatever has gone out of date is on that account discredited. You must find out why it went out of date. Was it ever refuted (and if so by whom, where, and how conclusively) or did it merely die away as fashions do? If the latter, this tells us nothing about its truth or falsehood. From seeing this, one passes to the realization that our own age is also "a period," and certainly has, like all periods, its own characteristic illusions. They are likeliest to lurk in those widespread assumptions which are so ingrained in the age that no one dares to attack or feels it necessary to defend them.

C. S. Lewis, *Surprised by Joy*

To look backward for a fleeting moment at the much despised Victorian era might do less harm than the artists of our day believe.

Huntington Hartford, *Has God Been Insulted Here?*

To Janet

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My thanks to the American Economic Association for permission to use "Retrospectives: The Origins of the Representative Agent," *Journal of Economic Perspectives,* vol. 10, no. 2, Spring 1996: pp. 169–77. Chapter 2 of this work is a slightly revised version of that paper.

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Part I

WHY REPRESENTATIVE AGENTS?

1

INTRODUCTION

MACROECONOMICS

This is a book about macroeconomics. "To me, the most extraordinary thing regarded historically, is the complete disappearance of the theory of the demand and supply for output as a whole, i.e., the theory of employment, *after* it had been for a quarter of a century the most discussed thing in economics." Perhaps even more extraordinary is that this quotation is taken from a letter written in 1936 by John Maynard Keynes (reproduced in Moggridge, 1973, p. 85). I suppose we should now be talking about the redisappearance of macroeconomics.

Instead of theories of the demand and supply for output as a whole, modern macroeconomics is dominated by models of a different sort. One of the most widely used methods of studying the macroeconomy, if not the predominant method, is the representative agent model. In these models, the macroeconomy is studied not by working out theories regarding how aggregate economies behave, but rather by working out theories regarding how an individual behaves and transferring these rules of behavior to the aggregate level.

Oddly, there has been extraordinarily little discussion in the macroeconomic literature about either the propriety or usefulness of representative agent models as a means of studying the macroeconomy. There are no widely cited justifications for its use; there are assorted critiques of its use, but while these critiques were relatively well known among microeconomic theorists, there is little evidence that macroeconomists took much notice. Alan Kirman's 1992 article in the *Journal of Economic Perspectives* criticizing the use of the representative agent model brought these microeconomists' critiques into the spotlight, but to date there is still little evidence that macroeconomists have taken much notice.

The purpose of this book is to evaluate thoroughly the use of the representative agent model in macroeconomics. It pulls together the scattered justifications for using such models and evaluates them. The conclusion from this inquiry is that representative agent models are neither a proper nor a particularly useful means of studying aggregate behavior.

Before starting this evaluation, however, let us be very clear about what this book is and what it is not. While the foregoing discussion may make this book seem like a methodological tract, it is methodology of a particular sort, perhaps best described in the title of Mayer's (1995) recent book, *Doing Economic Research.* Our aim here is to think through how economists do study the macroeconomy, both evaluating current practice and providing insights into alternative practice. The aim is to be a practical guide to the use and abuse of the representative agent model and other methods of studying macroeconomics.

On a related note, this book is not one of the ever-increasing number of "definitive critiques of neoclassical economics," all of which aim to show that neoclassical economics is fundamentally flawed and should be replaced by something else. The discussion in this book is solidly neoclassical; the criticisms of current practice and the suggestions for future practice are criticisms and suggestions from within the family. While external criticisms of neo-classical macroeconomics are of interest, this book is not one of them.

WHAT IS A REPRESENTATIVE AGENT MODEL?

The phrase "representative agent" is sufficiently generic that it can be used to describe a wide variety of possible constructs. The representative agent discussed in this book is that used as a modeling device in the new classical macroeconomics.

In these models, the optimization problem of a "representative" agent is explicitly written down and solved. For example, consider the problem of a representative consumer. We start by specifying the utility function and budget constraint for the consumer. We posit that the consumer will maximize his utility subject to the budget constraint and solve out the maximization problem in order to get some form of demand function for the consumer. The solution is thus an individual demand curve. In a new classical representative agent model, this well-defined and mathematically derived *individual* demand curve is used as the exact specification of the *aggregate* demand curve. To fit the requirements of a new classical representative agent model, the aggregate curve must be exactly the same as the rigorously derived individual curve.

It is the fact that the aggregate curves are derived from individual maximization problems that separates new classical representative agent models from other sorts of models. As an example, we can compare the models used to examine the permanent income hypothesis in Friedman (1957) and Hall (1978). Both models start by thinking about how individual consumers make consumption decisions and both end with an aggregate relationship between income and consumption. However, the part in between is very different. In Friedman's model, discussed more completely in Chapter 9, the consumer's problem is examined to see if individuals look at more than current-period income when making consumption choices; Friedman thinks about the consumer problem as a means of introducing the idea that people are actually concerned with their income over several periods when making a decision about current-period consumption. Friedman then goes on to posit an aggregate relationship between aggregate permanent income and aggregate consumption. In moving from the individual consumption function to aggregate consumption, Friedman makes it very clear that the functional form of the aggregate consumption function will not be the same as that for the individual consumption function. In fact, Friedman makes no attempt to derive the functional form of the aggregate consumption function, nor does he even try to derive theoretically what variables belong in the aggregate function. In discussing the aggregate consumption function, Friedman explicitly recognizes that such things as "utility factors" (e.g., age, family composition) and the ratio of nonhuman wealth to permanent income will vary between people. Because of these heterogeneities, the aggregate consumption function is not simply an aggregate version of the individual consumption function.

In Hall's model, discussed more completely in Chapter 11, the form of the individual and aggregate equations is identical. The aggregate consumption function is derived by solving the individual's maximization problem, finding the individual's consumption demand function and inserting aggregate values instead of individual values in the function. In new classical terminology, Hall's model is rigorously derived from an optimization problem at the level of the individual's objective function and constraints; Friedman's model is not so derived.

Thus, not everything that gets labeled a "representative agent" is a new classical representative agent model. For example, Friedman (1969) contains a discussion of how the money holdings of a "'representative' individual" will change after a helicopter drops money on a community. However, the representativeness of Friedman's "representative individual" is irrelevant. All Friedman uses the agent to show is that the individual will immediately be holding more money than he desires to hold and thus he will try to spend the excess. To avoid the discussion of how much will be spent and where the agent will end up from becoming too abstract, Friedman is using the "representative individual" as a numerical example. Nothing in Friedman's paper would change at all if Friedman had used a "nonrepresentative individual." The analysis and implications would be identical. There is no sense in which Friedman's "representative individual" resembles the representative agent in a new classical representative agent model.

We can distinguish between a simple use of the phrase "representative agent" and the sort of representative agent model that constitutes the subject of this book, by thinking of the latter as new classical representative agent models. As we shall see in Chapter 3, the modern form of the representative agent model came into vogue with the rise of the new classical macro economics. It has become the predominant model in new classical work in general and real business cycle work in particular.

However, its use is not limited to models generally described as new classical. Much of what is commonly called new Keynesian work also uses representative agent models. Thus, when we discuss new classical representative agent models, the phrase "new classical" should not be construed to mean "models in which policy does not matter" or "models using the Lucas supply function" or any other such thing. Instead, it is simply meant to be an indicator of models of the type that are used extensively in the new classical literature. Thus, the evaluation of models in this book applies with equal force to many models in the new Keynesian fold.

PLAN OF THE BOOK

Before we can start to evaluate the merits of using representative agent models to study the macroeconomy, we must first understand why users of such models advocate their use. It would be very convenient to be able to turn to the series of papers written by others in which the case for using representative agent models to study the macroeconomy is convincingly set forth. Unfortunately, such a series of papers does not exist. What does exist is a large set of introductions, paragraphs, and parenthetical asides that, when brought together, set forth the rationale for using representative agent models.

The natural starting point is an exploration of the origin of the representative agent. Alfred Marshall was the first person to use a representative agent in economics. This original use was vigorously attacked by Marshall's contemporaries and the construct largely vanished from economic discourse. Marshall's original justification and these early criticisms are discussed in Chapter 2.

As interesting as this discussion of the earliest representative agent model may be, it is not the final authority in an evaluation of its modern use. Chapter 3 explores the rise of the new classical representative agent model. By examining the comments made by prominent new classical economists, we can find three related justifications for the modern use of representative agent models. Briefly, these justifications are that representative agent models allow us to avoid the Lucas critique, that they are a powerful means of constructing Walrasian (or general equilibrium) models, and that they are a means of providing microfoundations for macroeconomics.

The rest of the book is an examination of these three arguments. The discussion has two components. First, in each of the three cases, we will see that if we adopt the goal implied by the rationale, for example, if we adopt the goal of providing microfoundations, then the representative agent model is inadequate to the task. In other words, the representative agent model does not solve the Lucas critique, it does not help in the creation of Walrasian models, and it does not provide microfoundations.

The second component to this discussion is an evaluation of the rationales themselves. Are these three goals useful for macroeconomic study? We shall see that none of these three standards of good economic models is necessarily useful, and that there are alternative standards, firmly within the neoclassical tradition, that may well be more useful for a study of the macroeconomy. The Lucas critique is unworkable in its present form; Walrasian methodology is not as useful as Marshallian methodology for macroeconomic study; and rigorous microfoundations of the sort that representative agent models are presumed to provide are neither possible nor particularly desirable.

Thus, in the end, the representative agent model fails to be a good method of studying macroeconomics in two ways. First, the goals which proponents of the use of representative agent models have enunciated are not necessarily useful or attainable goals for the study of macroeconomics. Second, even if one agreed with the aims of the proponents, the representative agent model does not help meet the aims.

Specifically, Chapter 4 examines the Lucas critique. The chapter explores the exact nature of the critique. It shows that representative agent models are every bit as susceptible to criticism as being subject to the Lucas critique as are the Keynesian aggregate models to which the critique was originally applied. Moreover, the Lucas critique as it is commonly stated is an extremely nihilistic standard for an economics model and thus is necessarily abandoned in actual economic research and should be abandoned in economic rhetorical critiques.

Chapters 5 through 7 explore the methods of Walras and Marshall and necessarily delve into methodological issues. But the study of proper method is of interest to philosophers (which is what we are, if we are going to be of any use to anyone) so the chapters go into a little detail.[1] We begin by exploring the nature of Walrasian methodology, evaluating both its aims and how the representative agent assumption fits into this general framework. We find a very poor fit; while Walrasian models may be interesting, Walrasian models incorporating representative agents are vastly less interesting. We then explore an alternative method, namely, Marshallian methodology. We show that Marshallian methodology can be profitably used to study the macroeconomy, and in fact may be more profitable than Walrasian methodology. We also show that the representative agent assumption can play a reasonable role in such a methodology. The final chapter in this section demonstrates that the new classical methodology is Walrasian, and thus the representative agent assumption is not a useful means of constructing good new classical, Walrasian models.

Chapters 8 through 13 examine the issue of microfoundations. The first order of business is to figure out why anyone would even want microfoundations; why not just study the macroeconomy itself? Chapter 8 looks at one answer to this question, that provided by Austrian economists, who have a very stringent set of guidelines for what constitutes a microfoundational model. We find that new classical representative agent models do not meet Austrian requirements for microfoundations. In Chapter 9, we turn to the traditional case for microfoundations. We find that there has been virtually no rigorous justification for the proposition that microfoundations of the sort advocated by the new classical economists are interesting or possible. We find only vague justifications for a bland sort of microfoundations. The chapter leads to the conclusion that the case for rigorous, new classical-style microfoundations has not yet been made and thus need not be accepted, while the case for the bland, vague microfoundations is simply a matter of taste. *De gustibus non est disputandum* – there is no disputing about tastes.

Chapter 10 begins the examination of whether representative agent models are capable of providing microfoundations. First, the aggregation problem is explored. There seems to be a vague belief by many that the representative agent model bypasses the aggregation problem. Chapter 10 shows that this is simply not true. Insofar as the rationale for microfoundations is a desire to bypass the aggregation problem, representative agent models fail to provide microfoundations. Chapter 11 turns to the issue of endogeneity and exogeneity. It demonstrates that representative agent models inherently confuse the status of economic variables. Chapter 12 explores whether representative agent models are even capable of providing a foundation in modern microeconomic theory. An exploration of current microeconomic theory shows that representative agent models do not now and can never provide microfoundations in current microeconomic theory.

We then turn to the desirability of microfoundations. Can we ever provide microfoundations and if so, would we want to do so? Chapter 13 presents several arguments why microfoundations is not a useful criterion for good macroeconomic study.

We conclude in Chapter 14 by noting the demise of representative agent models and asking what comes next.

2

THE ORIGINS OF THE REPRESENTATIVE AGENT

THE BIRTH OF THE REPRESENTATIVE AGENT

Representative agents were born in Marshall's *Principles of Economics.* This chapter examines their birth. How and why did Marshall use the construct of the representative agent? His use is of interest to us for two reasons: (a) Marshall's use of the representative agent is very different from and more limited than its current use; and (b) despite its relatively limited use, Marshall's representative agent was vigorously assaulted as a useless and misleading construct, most notably in a 1928 essay by Lionel Robbins. This criticism hit its mark. Decades later, one commentator wrote, "It is now more than twenty-five years since Professor Robbins's famous article on the representative firm finally drove that concept from the pages of economic textbooks" (Wolfe, 1954, p. 284).[1]

Marshall limited the application of the concept to the idea of a representative firm. He considered applying this construct to consumer theory – what we would call today a "representative consumer" – but decided against it. As Marshall once noted, "I think the notion of 'representative firm' is capable of extension to labour; and I have had some idea of introducing that into my discussion of standard rates of wages. But I don't feel sure I shall: and I almost think I can say what I want to more simply in another way" (in Pigou, 1956, p. 437).

The representative firm makes its appearance in Marshall's *Principles of Economics* in the discussion of the conditions of supply.[2] Marshall defines the representative firm in the following manner:

> We shall have to analyse carefully the normal cost of producing a commodity, relatively to a given aggregate volume of production; and for this purpose we shall have to study *the expenses of a representative producer* for that aggregate volume. On the one hand we shall not want to select some new producer just struggling into business, who works under many disadvantages, and has to be content for a time

> with little or no profits, but who is satisfied with the fact that he is establishing a connection and taking the first steps towards building up a successful business; nor on the other hand shall we want to take a firm which by especially long-sustained ability and good fortune has got together a vast business, and huge well-ordered workshops that give it a superiority over almost all its rivals. But our representative firm must be one which had a fairly long life, and fair success, which is managed with normal ability, and which has normal access to the economies, external and internal, which belong to that aggregate volume of production; account being taken of the class of goods produced, the conditions of marketing them and the economic environment generally.
>
> (Marshall, 1920 [1961], vol. 1, p. 317)

The representative firm is thus the vehicle by which Marshall will study the supply of goods.

To clarify Marshall's notion of the representative firm, let us explicitly note what it is not. It is not some statistical construct; it is not, for example, created by dividing total supply by the number of firms in the industry. It is not a giant superfirm that is assumed to produce all of the aggregate output. While the above quote makes it appear that Marshall is thinking of a real firm, he is not; he explains, "We have to consider the conditions of the representative firm rather than a given individual firm" (ibid., p. 805).

It is useful to step back and consider why Marshall found it necessary to invent the concept of the representative firm in the first place. To do this, we must examine his vision of the notion of supply. Marshall wanted to construct an industry supply curve which would show how much supply would be forthcoming at any given price. He recognized that the supply of a good depends on the costs of producing the good. However, he was troubled by the existence of firms of vastly different sizes within any given industry. Due to internal economies of scale, these differently sized firms could bring the same good to market at vastly different costs. So which of these costs determined the unique selling price of the commodity?

Modern economic analysis tends to assume that the supply price is determined by the marginal, or least profitable, firm. Any firm that cannot bring a product to market at or below this cost will not produce. Marshall, however, recognized that there will be firms which are just starting out that will be content to produce with negative profits for a while in the hope of establishing a position in the industry and thereby making positive profits later on. Thus, the supply price cannot be assumed to be the cost of these least-profitable firms. Similarly, Marshall believed that there will be older firms which are well established and making positive economic profits.

Thus, the industry supply price would be lower than the costs of newer firms that are hoping to rise, but higher than the costs of older firms that may be stable or declining. Given only this information, it would be impossible to pinpoint the industry supply curve. The representative firm was invented to fill this gap. Marshall defined it as the firm whose costs of bringing its product to market are exactly the same as the industry supply price; i.e., the representative firm is the firm that is making zero economic profits.

Thus, Marshall's representative firm is quite similar to the description of firms in a competitive equilibrium. In fact, Marshall's intellectual heir, Pigou, used a similar construct and called it "the equilibrium firm" (Pigou, 1928). The following description of the representative firm makes this similarity clear:

> Let us call to mind the "representative firm," whose economies of production, internal and external, are dependent on the aggregate volume of production of the commodity that it makes; and, postponing all further study of the nature of this dependence, let us assume that the normal supply price of any amount of that commodity may be taken to be its normal expenses of production (including *gross* earnings of management) by that firm. That is, let us assume that this is the price the expectation of which will just suffice to maintain the existing aggregate amount of production; some firms meanwhile rising and increasing their output, and others falling and diminishing theirs; but the aggregate production remaining unchanged. A price higher than this would increase the growth of the rising firms, and slacken, though it may not arrest, the decay of falling firms; with the net result of an increase in the aggregate production.
>
> (Marshall, 1920 [1961], vol. 1, p. 342)

Marshall created the representative firm to abstract from the idiosyncrasies of individual firms and the vagaries of industry supply. Or, as Blaug (1985, p. 391) notes, "Marshall's device of the representative firm allows him to state the conditions for equilibrium of total output in an industry without requiring at the same time that all the member firms of the industry be in equilibrium." Marshall's fundamental purpose was to avoid the need to assume that all firms are alike. He wanted to be able to describe a single industry equilibrium with a single market price without having to assume that all firms are producing in exactly the same manner.

The representative firm was created for this very specific purpose, and only for this purpose. Marshall makes no attempt, here or elsewhere, to use the representative firm to derive other results.

There is a telling clue to Marshall's thinking about the limitations of the representative firm in his *Industry and Trade* (Marshall, 1920). Appendix N in this work is subtitled "The recent increase in the size of the representative industrial establishment in America" (p. 846). This would seem to be the perfect place to examine Marshall's use of the representative firm. However, after the subtitle, the representative firm is not mentioned in this appendix. Instead, it discusses a broad range of national data from 1850 to 1910, including such aggregates as the number of business firms in operation, the amount of capital and labor employed in the nation, the aggregate level of wages and sales, and so on.

The discussion of the size of businesses deals in broad aggregates. For example:

> The wages bill of factories etc. increased between 1900 and 1910 considerably faster than the number of workers; but not nearly so fast as the value of the total output of products (which of course includes the cost of the material used); or of the net product (which is the value added to the material by the process of manufacture).
>
> (Marshall, 1920, p. 847)

In this appendix, Marshall had at his disposal all the data he needed to construct a statistical profile of a representative institution, along with a subtitle promising a description of the representative firm – and yet he refrained from constructing one.

Marshall uses the representative firm solely as an abstract notion designed to avoid problems arising from the diversity of firm size. He does not view the representative firm as a tangible entity with a life of its own. Marshall's representative firm is a very limited notion indeed.

THE CRITIQUE AND ABANDONMENT OF THE REPRESENTATIVE FIRM

To most economists today, Marshall's use of the representative firm would seem innocuous enough. Indeed, Marshall's reliance on the representative firm pales in comparison to that of modern economists. But as noted in the introduction, this notion of the representative firm quickly elicited a great deal of criticism.

There were several bases upon which the criticisms of Marshall's representative agents were built. The first of these was that the notion was rather ephemeral. As Robbins (1928) put it:

> The Marshallian conception of a Representative Firm has always been a somewhat unsubstantial notion. Conceived as an afterthought . . . it lurks in the obscurer corners of Book V [of *Principles*] like some pale visitant from the world of the unborn waiting in vain for the comforts of complete tangibility. Mr. Keynes [1924] has remarked that, "this is the quarter in which in my opinion the Marshall analysis is least complete and satisfactory and where there remains most to do."
>
> (Robbins, 1928, p. 23)

Even some of Marshall's defenders expressed similar misgivings: "[P]ossibly it is nominalistic to speak of a unit which varies in identity, size and organization as being representative" (Maxwell, 1929, p. 632).

The second prong of the attack on the representative firm was that such a construct was unnecessary, that the use of this construct gained nothing. Robbins (1928, p. 28) notes, "There is no more need for us to assume a representative firm or representative producer, than there is for us to assume a representative piece of land, a representative machine, or a representative worker." Sraffa (1926) made this line of argument most forcefully. Recall that Marshall created the representative firm in order to explain how an industry with diverse firms could generate a single market price. Sraffa, in a precursor to the monopolistic competition literature, argued that the equilibrium is in general determinate, even without the assumption of a representative producer. Moreover, Sraffa argued that the equilibrium would not in general have a uniform price; different producers would charge different prices for similar goods, and thus there was no need for the assumption of a representative producer. In Sraffa's view, Marshall's aim of establishing a unique industry supply curve was simply misguided. There is no need to presume that all firms in a given industry will have the same price to establish the existence of an equilibrium.

The argument that the representative firm was an ephemeral construct that serves no real purpose might have been enough to warrant its exclusion from professional discourse. But the critics of the representative firm did not stop there. They went on to argue that the representative firm is a misleading construct as well.

The first way in which the representative firm is misleading relates to situations of industry growth. Marshall (1920 [1961], vol. 1, pp. 316, 459–60) argued that as the industry grew, the representative firm grew proportionally; this assumption allows the supply curve to stay relevant when market demand is increasing. Young (1928) noted a fatal flaw in this reasoning. One of the dominant features of economic growth is the furtherance of the division of labor. Just as pins were formerly made by only one individual and later by multiple individuals, products that formerly were made by one firm will later be made by multiple firms, each producing only a part of the former product. This division of labor is problematic for the idea of the representative firm:

> With the extension of the division of labor among industries the representative firm, like the industry of which it is a part, loses its identity. Its internal economies dissolve into the internal and external economies of the more highly specialized undertakings which are its successors, and are supplemented by new economies.
>
> (Young, 1928, p. 538)

Thus, Young showed that Marshall's representative firm was unable to take account of any type of economic expansion other than the simple enlargement of the existing manufacturing process.

However, Marshall's representative firm is ultimately unable to be used to explain even this type of economic growth. In fact, it turns out that even if all firms are identical to one another, Marshall's representative firm may be unable to show the effects of economic growth. Robbins (1928) notes that an increase in production can arise from an increase in the production of all existing firms, in which case the representative firm grows with the industry, or it can arise from an increase in the number of firms, in which case the representative firm does not grow with the industry. Both outcomes are equally plausible. Thus, during economic growth, "the representative firm may cease to be representative and its cost curve cease to be significant" (Robbins, 1928, p. 31).

Perhaps the most devastating criticism of Marshall's representative firm was that it led to confusion about the nature of the average firm. Since the representative firm is such an ephemeral construct, it is very easy to lose track of what exactly it entails. As Robbins put it:

> The whole conception, it may be suggested, is open to the general criticism that it cloaks the essential heterogeneity of productive factors – in particular the heterogeneity of managerial ability – just at that point at which it is most desirable to exhibit it most vividly.
>
> (Robbins, 1928, p. 33)

The problems in this vein began with Marshall himself. Recall that the reason he concocted the representative firm was to be able to write down a single supply curve for an industry with diverse firms. Implicit in this argument is the assumption that the supply curve will be that of the representative firm. But why will the supply curve of the representative firm, rather than that of some other firm, correspond to that of the industry? Marshall explains it thus:

> Anyone proposing to start a new business in any trade . . . if himself a man of normal capacity for that class of work, . . . may look forward ere long to his business being a representative one, in the sense in which we have used this term, with its fair share of the economies of production on a large scale. If the net earnings of such a representative business seem likely to be greater than he could get by similar investments in other trades to which he has access, he will choose this trade. Thus that investment of capital in a trade, on which the price of the commodity produced by it depends in the long run, is governed by estimates on the one hand of the outgoings required to build up and to work a representative firm, and on the other of the incomings, spread over a long period of time, to be got by such a price.
>
> (Marshall, 1920 [1961], vol. 1, pp. 377–8)

Marshall is here arguing that the expected profits from running a representative firm determine the level of capital investment in the industry and thus the market price. If a manager sees the representative firm making positive economic profits, he will enter the industry, raising the quantity supplied of the good and thereby lowering the market price. Thus, the arbitrage of managerial ability ensures that the market price coincides with the costs of the representative firm.

This line of reasoning neglects the fact that managers have varying abilities. Both the inferior and superior managers must work somewhere. As Davenport notes:

> This evidently takes the representative firm to be something like an average firm; it is here said that any average man who concludes that in the trade in question he would turn out to be an average man, will go into the trade if he notices that the average man in that trade is doing better than average men outside. True, as a doctrine of opportunity cost; but it does not need the assumption of average men to be true. Any inferior or superior man will act in precisely the way outlined, if he believes that men of his grade are finding the trade in question more remunerative than other trades to which he has access. And there is nothing in any case to indicate that the cost of this average man will coincide with the price of the product, or to indicate that the cost of the marginal man will not so coincide.
>
> (Davenport, 1908, p. 377)

There is thus nothing about Marshall's analysis that explains why the market price will coincide with the costs of the representative firm. It is only by implicitly assuming that all managers are of average ability that he can argue that arbitrage of managerial ability will drive the market price to coincide with that of the representative firm. Davenport is simply noting that there is nothing in Marshall's analysis to drive the conclusion that the supply price will be that of the average (or representative) firm rather than the inferior (or marginal) firm. Instead, it is equally plausible that the arbitrage of inferior managerial ability will drive the supply price to the costs of the inferior firm. By thinking about market equilibrium in terms of the average firm, Marshall seems to have forgotten about essential heterogeneities in managerial ability. The same problem arose in the works of Marshall's followers, in particular those of Henderson (1922) and Robertson (1927). Robbins (1928, pp. 34–5) deflated these arguments succinctly: "Mr. Henderson should reflect that if all entrepreneurs were *at least* of average managerial ability, they would at once cease to be average."

These criticisms of Marshall's representative firm were fatal. Marshall's use of the representative firm did have its defenders, notably Pigou (1928) and Robertson (1927, and in Robertson *et al.,* 1930). However, as Wolfe's (1954) comment shows, these defenders were ultimately unsuccessful. A telling example of the widespread dismissal of the representative agent notion is seen in Schumpeter's paean to Marshall in *Ten Great Economists.* Schumpeter (1951, pp. 99–100) rather matter-of-factly lists the representative agent as one of the "treacherous" constructs that "cover rather than mend the logical difficulties" which economists encounter. Schumpeter apparently felt no compulsion to justify this indictment.

MODERN LESSONS

The representative agent thus arose from very humble beginnings. Marshall's reliance on the representative firm was extremely light compared to its modern use, but yet it still provoked sharp criticisms. But in more recent times the representative agent has made a remarkable comeback, becoming one of the most pervasive assumptions in economics. How did this assumption rise from ignominy to omnipresence? One might presume that the problems noted by Marshall's critics were solved, allowing a new, improved representative agent to be unashamedly used. However, many of the criticisms of Marshall's representative economic agent apply with equal force to its modern counterparts. Let us take up each of the lines of criticism in turn.

The notion of a representative agent is no more corporeal today than it was when Marshall first used it. The list of unanswered (and, possibly, unanswerable) questions is lengthy. What exactly is a representative agent? What does it mean to be "representative"? Does "representative" simply mean "average," or does it mean something else? In a group of firms or agents with, say, 100 characteristics, how many of these characteristics must be well-reflected by a representative agent? And so on.

Consider two examples. Suppose that the marginal propensity to consume is 0.9 for 90 percent of the population, and 0.5 for the rest. What should be the marginal propensity to consume for a representative agent? Should it be 0.9, to reflect 90 percent of the population? Should it be the average weighted by population – in this case, 0.86? Should it depend on how much the two groups spend; i.e., if the 10 percent of the population with a marginal propensity to consume of 0.5 does 50 percent of the consuming, should the representative agent have a marginal propensity to consume of 0.64? Do any of these ideas capture what we mean by a "representative" agent?

The questions become even harder when we consider more complicated examples. Consider an example of risk aversion.[3] Consider a world where 90 percent of the population is risk averse and 10 percent is risk neutral. Our task is to define the risk preferences of a representative agent.

We might go about our task by trying to measure the risk premium on bonds. In this case, the characteristics of the bond market are relevant. In our hypothetical world, 90 percent of the bonds are issued by the government and carry no risk. The remaining 10 percent of the bonds are issued by corporations and are considered risky. For simplicity, assume that all bonds have the same expected yield and that all people hold the same number of bonds.[4]

In this world, the risk premium on corporate bonds will be exactly zero. After all, as long as there is a positive risk premium, the risky bonds would be unambiguously more desirable to the risk neutral investors. They will keep buying risky bonds, and driving down the return, until they own all the risky bonds and (in the conditions of this example) the risk premium is eliminated.[5]

Imagine that we now construct a representative agent model to study risk premiums, as was done in Mehra and Prescott (1985). What value should we use for the risk aversion parameter? Is the representative agent risk averse? If we say no, then our representative agent does not represent the 90 percent of the population that is risk averse. If we say yes, and build a representative agent model with a risk averse agent, then that model will predict a positive risk premium on corporate bonds. Our model will be unable to understand why no risk premium is observed in actual data.

So we might try to fix up the model somehow, to get rid of the predictions of a risk premium. Suppose we find a way to model an agent who is risk averse but yields a model without a predicted risk premium. We know beforehand that this model has not given the proper explanation. The real reason there is no risk premium is the heterogeneity of the population. There is no representative agent model that can model this heterogeneity.

The problem here is general. No representative agent can model this heterogeneity. With a heterogeneous population and multiple types of agents and bonds, there will always be difficulty in measuring risk premiums.

Relaxing the assumptions eliminates the extreme case of no measured risk premium in the aggregate, but does not eliminate the basic problem. We can, for example, allow for a continuous distribution of risk averseness in the population. In such a case, the risk premiums on the risky bonds will be exactly the risk premiums demanded by the marginal purchaser, not some sort of average purchaser in the general population. Thus, unless we define the representative agent as the marginal purchaser, the measured risk premiums will be different than predicted. Defining the representative agent as the marginal purchaser, however, means that the agent is quite ephemeral. Any change in the distribution of the riskiness of bonds, for example, will result in a change in the representative agent; the coefficient of risk aversion for the representative agent would be endogenously determined by the market distribution of bond risk.

This gets us back to the whole means by which we are defining representativeness. Do we really think of representativeness as merely an average? If we do not, what is it? It is extremely hard to give a consistent and empirically usable definition of what a representative individual is like. If we create an agent that is some sort of statistical average, the model using it will not necessarily explain aggregate data. If we try to define the representative agent as the marginal purchaser, it becomes a will-o'-the-wisp whose taste parameters are endogenously determined by market characteristics.

The second criticism leveled at Marshall's representative firm assumption was that it was unnecessary. Even if that criticism was justified against Marshall's use of the concept, it would have little to say about whether the assumption of a representative agent or firm is necessary in other uses.

However, it remains true that representative agent models still have difficulties dealing with problems of growth. Representative agent models are still unable to cope with the problem that growth can occur either because the representative firm grows or because there are simply more representative firms. This is a very important distinction in most economic work. Unless returns to scale are constant, growth is very different if it is caused by more firms or by the same number of firms producing more goods.

Most economists would agree that we do not live in a perfectly linear, constant-returns-to-scale economy. As Hahn (1973, p. 12) has remarked, "For it now seems to me clear that there are logical difficulties in accounting for the existence of agents called firms at all unless we allow there to be increasing returns of some sort." However, if nonlinearities are important, the representative agent framework is completely unable to account for growth. When representative agents are used in macroeconomic models, one of two equally unpalatable assumptions is commonly made: either everything is assumed to be linear or the number of agents is fixed exogenously. Neither assumption is acceptable to most economists, but the representative agent framework forces one of these assumptions to be made in any framework with economic growth.

The most devastating criticism of Marshall's limited use of the representative firm also applies to its modern counterparts. By their very design, representative agent models conceal heterogeneity, whether it is important or not. Economists who would never automatically assume that the important characteristics of all policy regimes are homogeneous routinely assume that heterogeneity among agents is unimportant. However, one should no more automatically assume that heterogeneity of agents is irrelevant than automatically assume that regime changes are irrelevant.

3

ARGUMENT FOR THE NEW CLASSICAL USE OF REPRESENTATIVE AGENT MODELS

THE RETURN OF THE REPRESENTATIVE AGENT MODEL

Given the history described in the last chapter, how did the representative agent model become so prevalent in modern macroeconomic research? In this discussion, we want to do two things. First, we want to trace the reintroduction of representative agent models into new classical macroeconomics. How did the representative agent become so prominent in new classical macroeconomics? Hoover (1988) identified three tenets he considered key to understanding new classical macroeconomics: (a) agents' decisions are based on real magnitudes; (b) agents are in continuous equilibrium; and (c) agents have rational expectations. Nothing about these tenets mandates the use of a representative agent model, so why did research in the new classical program become so dependent on the use of this construct? In fact, Hoover's encyclopedic discussion of new classical macroeconomics has very little discussion of the representative agent construct.[1] Similarly, the more recent explanation of the new classical macroeconomics in Snowden *et al.* (1994) devotes only one paragraph to the representative agent assumption. This relative lack of emphasis on the representative agent model is not gross negligence, but rather simply a reflection of the fact that there is very little in the new classical program that hinges on the use of representative agent models. The important assumptions can be and have been incorporated into pure macroeconomic models, and most of the important results can be and have been derived in purely macroeconomic models. So if the representative agent model is neither necessary nor sufficient for the aggregate implications of new classical macroeconomics, how did it come to be used so extensively?

While looking at the papers in which the representative agent model was first used, we also want to examine the arguments presented to explain why using representative agents is either desirable or interesting. Why is it desirable to model macroeconomics indirectly, via representative agents, rather than by the simpler, direct macroeconomic models of the past? The answer is not obvious. A simplistic use of Occam's Razor would seem to say that if you can get the same result using either a simple macroeconomic model or a complex representative agent model, the former is more desirable.

We will trace out a threefold rationale for using representative agent models. At the most superficial level is the Lucas critique. Most of the stated rationales for using representative agent models emphasize this idea. However, in and of itself, the Lucas critique is insufficient to justify representative agent models. Probing further, we will see a related argument for an explicit use of the Walrasian tradition of creating general equilibrium models to study economic phenomena. Finally, there is a strongly held belief in the need to provide microfoundations for macroeconomic models.

Examining these rationales is more problematic than one might assume. To date, none of the principals has written a serious defense of using the representative agent construct in macroeconomics. The discussion below is based upon assorted comments scattered throughout the new classical literature. Sargent has come closest to providing an extensive discussion of the rationale for representative agents, and he certainly has provided many of the tools necessary for solving out these models. Kydland and Prescott have also provided some important discussion of the rationale for these models. However, most uses of these models simply presume that the reader understands why the representative agent approach is better than a pure macroeconomic approach; Hall (1978), for example, is an early new classical representative agent paper with absolutely no discussion of the modeling revolution in which he was participating.

THE FIRST NEW CLASSICAL REPRESENTATIVE AGENT MODEL

In his discussion of modern business cycle theory, Lucas (1977, p. 16) notes that “In moving from these general considerations to a more specific theory, it will be helpful to consider as an example a ‘representative’ agent.” The footnote at the end of this sentence lists a set of papers in which “many of the arguments in this and subsequent sections have been developed more extensively” (ibid.). The earliest papers listed are those in the Phelps *et al.* (1970) volume; the only paper there which uses a representative agent is Lucas and Rapping (1970). None of the later papers listed in this footnote uses a representative agent. Thus, the Lucas and Rapping paper not only can be considered one of the first new classical papers, but also the starting point of the use of representative agent models in new classical macroeconomics.

Lucas and Rapping (1970) set up a model in which a representative agent is used to determine the nature of labor supply. Specifically, the representative household maximizes utility which is based on current and future consumption (C) and labor supply (N):

$$U(C, C^*, N, N^*)$$

where an asterisk indicates the future value. Utility is assumed to increase with consumption and decrease with labor supply. The representative household maximizes utility subject to the budget constraint:

$$\mathrm{P} \cdot \mathrm{C} + \left(\frac{\mathrm{P}^*}{(1+r)}\right)\mathrm{C}^* \leq \mathrm{A} + \mathrm{W} \cdot \mathrm{N} + \left(\frac{\mathrm{W}^*}{(1+r)}\right)\mathrm{N}^*$$

where A is the initial level of nonhuman assets and r is the nominal interest rate. Lucas and Rapping then note that this problem will yield a current labor supply function:

$$\mathrm{N} = \mathrm{F}\left(\mathrm{W}, \frac{\mathrm{W}^*}{1+r}, \mathrm{P}, \frac{\mathrm{P}^*}{1+r}, \mathrm{A}\right)$$

Assuming F is homogeneous of degree zero in its arguments, we can deflate by the current price level to get the equivalent expression:

$$\mathrm{N} = \mathrm{F}\left(\frac{\mathrm{W}}{\mathrm{P}}, \frac{\mathrm{W}^*}{\mathrm{P}(1+r)}, 1, \frac{\mathrm{P}^*}{\mathrm{P}(1+r)}, \frac{\mathrm{A}}{\mathrm{P}}\right)$$

To get the signs of the first derivatives here, Lucas and Rapping make certain regularity assumptions (future goods and leisure are substitutes for current leisure, leisure is not inferior, the asset effect is small), yielding:

$$\frac{\delta \mathrm{F}}{\delta\left(\frac{\mathrm{W}}{\mathrm{P}}\right)} > 0, \frac{\delta \mathrm{F}}{\delta\left(\frac{\mathrm{W}^*}{\mathrm{P}(1+r)}\right)} < 0, \frac{\delta \mathrm{F}}{\delta\left(\frac{\mathrm{P}^*}{\mathrm{P}(1+r)}\right)} < 0, \frac{\delta \mathrm{F}}{\delta\left(\frac{\mathrm{A}}{\mathrm{P}}\right)} < 0$$

Finally, based on the labor supply function, Lucas and Rapping posit a log-linear relationship:

$$\ln\left(\frac{\mathrm{N}_t}{\mathrm{M}_t}\right) = \beta_0 + \beta_1 \ln(w_t) - \beta_2 \ln(w_t^*) + \beta_3\left[r_t - \ln\left(\frac{\mathrm{P}_t^*}{\mathrm{P}_t}\right)\right] - \beta_4 \ln\left(\frac{a_t}{\mathrm{M}_t}\right)$$

where M is an index of the number of households, w = W/P, w^* = W*/P* and a = A/P, and using the fact that $\ln(1 + r)$ is approximately equal to r. Lucas and Rapping note that the assumptions above imply β1, β2, β3, and β4 are all positive.

An aggregate production function is used to generate labor demand. Lucas and Rapping then go on to develop a three-equation structural model, with labor supply, the first-order condition on labor, and unemployment as the dependent variables. They also present a two-equation reduced-form model with wages and employment as the dependent variables.

After developing this theoretical structure, Lucas and Rapping move on to an empirical section in which they test the theory. They argue that the empirical results presented in the body of the paper are striking: “The theory has thus provided us with an extremely sharp prediction on the way the variables examined are related, and these predicted relationships have been confirmed by the 1930–1965 data" (Lucas and Rapping, 1970, p. 282). In particular, Lucas and Rapping emphasize that the configuration of signs predicted by their structural model is one of 1,728 possible outcomes, implying that the fact that the empirical results have this exact configuration of signs is quite noteworthy.

But is it? If we look a little closer at the representative agent model developed by Lucas and Rapping in the theoretical section and at the model empirically tested in the empirical section, we notice a difference: neither the interest rate term nor the asset term is included in the empirical section. Why were they dropped? Lucas and Rapping explain: "[T]here is some reason to believe that the asset effect on labor supply is minor . . . and, for this reason, this variable was originally excluded from our tests. Later we introduced some rather unsatisfactory 'proxies,' with generally poor results" (ibid., p. 267). This may or may not be objectionable; it is empirically difficult to come up with reasonable proxies for household assets; and thus it is at least defensible to omit a poor proxy from a test of a theory.

However, when we look at the interest rate, we find another story. Lucas and Rapping also explain why they omit the interest rate: "Similarly, while results with a nominal interest rate, rt, are reported, our most satisfactory models exclude this variable, and it will be dropped from the discussion that follows" (ibid.). By looking at the results presented in the Appendix of the Phelps volume paper (but not included in the 1969 *Journal of Political Economy* version of the paper), we can see why Lucas and Rapping would want to drop the interest rate: the coefficient has the wrong sign.[2] Now having an empirical estimate come out with the wrong sign is not in and of itself a monumental failure. However, when Lucas and Rapping emphasize how significant it is that the model presented in the body of their paper not only has all of the predicted signs, but that this is one of 1,728 possible sign configurations, the fact that the model they present in the body of the paper is not the one that they derive in the theoretical section of the paper becomes significant. The matter is particularly significant since the only reason given for dropping the interest rate from the empirical test is that the empirical results without interest rates are more "satisfactory."

So, far from having astounding empirical support for the model they developed, the empirical support is rather mixed. By dropping two of the variables from the regression, Lucas and Rapping were able to make it look as if the empirical support for their model was much better than it actually was. The first new classical representative agent model was thus portrayed as having stronger empirical support than it in fact had.

On the other hand, the Lucas and Rapping representative agent model is very different from its descendants. In effect, Lucas and Rapping are simply writing down a typical worker's utility maximization problem, and then using the information from it to guess at the signs in a macroeconomic regression. There is no attempt rigorously to derive a macroeconomic relationship as the solution of the maximization problem of a representative agent. Rather, Lucas and Rapping have a purely macroeconomic equation estimating the labor supply and are using a microeconomic model to guess at what variables should be in the equation. This procedure is little different than that which Keynesian macroeconomic model builders had been using for decades.

So the Lucas and Rapping paper is not really the introduction of modern representative agent models into macroeconomics; it is more the introduction of the phrase "representative agent" into the new classical discourse. The introduction of models in which macroeconomic equations are rigorously derived from consumer and firm optimization problems came later.

THE LUCAS CRITIQUE

Much of the early new classical macroeconomic work was primarily done with macroeconomic models. Even the famous (or infamous, depending on your point of view) policy ineffectiveness models (Sargent and Wallace, 1975, 1976) and the unpleasant monetarist arithmetic models (Sargent and Wallace, 1981) were pure macroeconomic models. (It should be noted that the authors were not necessarily fond of the fact that they were using macroeconomic models. Sargent and Wallace (1975, p. 241) begin the paper by lamenting the "deplorable feature" that the model is not derived from microeconomic objective functions.) So it is clear that there is nothing about the incorporation of rational expectations or the policy ineffectiveness results that mandates the use of a representative agent model.

Where did representative agent models get their start then? A natural place to look for an answer is in the paper that explained how to solve out such models. Hansen and Sargent (1980) were devoted to describing solution routines for the relatively complicated representative agent models. The introduction to that paper leaves little doubt about these authors' motivation for studying representative agent models:

> This paper describes research which aims to provide tractable procedures for combining econometric methods with dynamic economic theory for the purpose of modeling and interpreting economic time series. That we are short of such methods was a message of Lucas's (1976) criticism of procedures for econometric policy evaluation. . . . The implication of Lucas's observation is that instead of estimating the parameters of decision rules, what should be estimated are the parameters of agents' objective functions and of the random processes they faced historically.
>
> (Hansen and Sargent, 1980, p. 7)

A careful examination of exactly what Lucas argued in his 1976 paper seems warranted. He begins by arguing that an economy at time t can be defined as a set of three vectors: (a) the set of endogenous variables, y_t; (b) the set of exogenous variables, x_t; and (c) the set of random shocks, ε_t. Consider the simple model:

$$y_{t+1} = \beta x_t + \varepsilon_t \tag{3.1}$$

With a model of the form in equation (3.1), policy analysis is simple. One merely puts new values of the exogenous *x*s into the model, and derives the new *y*s. For example, if one of the *x*s is the number of paper clips purchased by the federal government, a researcher could find the effect of different levels of paper clip purchases on, say, GDP by changing the value of the corresponding *x*.

The Lucas critique is that this sort of exercise is fundamentally flawed. In studying policy changes in this manner, we are assuming that β is held constant across policy regimes. There is no reason to assume that the parameters in β will be unaltered when policy regimes change. When people are aware of the regime change, they may well change their behavior. In such a case, agents would no longer follow the decision rules from the old policy regime when the new policy regime is instituted. The new values of the βs in equation (3.1) need to be determined before the effect of thse policy change can be predicted.

Thus, changes in the policy regime both alter the future values of the xs and may alter the values of the βs. A proper specification of the macroeconomy would be:

$$y_{t+1} = \beta(\lambda) \cdot x_t + \varepsilon_t \tag{3.2}$$

where λ is the policy regime. This model takes account of both effects of the policy change.

While the Lucas critique presents us with an interesting theoretical problem, it does not point the way to a solution. The critique argues that we need to get at $\beta(\lambda)$, but does not indicate how to do so. Others were quick to supply the answer. The basic premise of the new classical solution was, using the title of Sargent's (1982) paper, to go "Beyond Demand and Supply Curves in Macroeconomics":

> For if the presence or [sic] the cross-equation restrictions implies that the private decision rules change systematically with descriptions of the dynamic environment and of government rules, a successful theoretical analysis requires understanding the way in which optimizing agents make their decision rules depend on the dynamic environment in general. The econometric ideal of discovering objects that are structural, in the sense that they are invariant with respect to the class of policy interventions to be analyzed, imposes that criterion for success.

> The upshot is that the analyst's attention is directed beyond decision rules to the objective functions that agents are maximizing and the constraints that they are facing, and which lead them to choose the decision rules that they do.
>
> (Sargent, 1982, p. 383)

Thus, at the heart of the new classical research program is an attempt to bypass the Lucas critique. The equations in traditional macromodels – Sargent's "Demand and Supply Curves" – are too superficial since changes in policy regimes will alter these curves in ways that may be unpredictable. So a good macromodel will go beyond these curves to agents' objective functions. If we model a person's utility function, we can predict how he will respond to hypothetical policy regime shifts. In other words, while *a priori* we cannot say how employment changes with a change in monetary policy, if we know how each person and firm responds, we can deduce the aggregate result.

In short, the new classical means of solving the Lucas puzzle is to go one step further and search for the "structural equations" that underlie the macroeconomy. In this manner, regime shifts can be contemplated. For if we start with structural equations rather than with the curves that come from them, the Lucas critique does not apply. Structural equations are, by definition, immune to regime changes.

Sargent's *Macroeconomic Theory* provides a nice illustration. In the first edition (1979), the chapter on consumption contains a long discussion similar to that used in Friedman's (1957) work on permanent income. The discussion is focused on aggregates. In the second edition (1986), the consumption chapter is completely rewritten and focuses on a representative agent model like that used in Hall (1978). Why the change?

> The process of building the theory of consumption described above began as a conservative attempt to modify the Keynesian consumption function to incorporate the distributed lag needed to match the correlations in the data. This enterprise eventually led to the insight that the decision rule for consumption at t as a function of the agent's information at t – the consumption function – ought not to be regarded as invariant with respect to alterations in the process for taxes, labor income, and the interest rate that face the agent. It followed that the formerly widespread practice of holding consumption functions invariant while simulating the effects on consumption of alternative tax and income processes contradicted the theory. This was Lucas's critique of econometric policy evaluation. This insight, which has revolutionary consequences for the practice of macroeconomics and econometrics, emerged naturally and logically from the process of refining the Keynesian consumption function to bring it into line with dynamic theory.
>
> (Sargent, 1986, p. 378)

Representative agent models are thus an attempt to model rigorously the structural relationships in an economy. If we cannot simply start with macroeconomic equations, then we need to start with microeconomic agents. The first step is to write down the problem faced by the microeconomic agent in terms of fundamental parameters. This agent is assumed to be representative, and the solution to this problem is assumed to hold for the macroeconomy.

However, the Lucas critique is not a sufficient reason for using representative agent models. There are many purely macroeconomic models (e.g., the Sargent and Wallace papers mentioned earlier) that incorporate changing expectations about policy. So we must search for something more fundamental as a rationale for using representative agent models.

THE WALRASIAN TRADITION

A second, related motivation for the use of representative agent models is a desire to build Walrasian general equilibrium models of the economy. Sargent notes, "Since in general one agent's decision rule is another agent's constraint, a logical force is established toward the analysis of dynamic general equilibrium structures" (Sargent, 1982, p. 383). Or as Kydland and Prescott put it:

> By general equilibrium we mean a framework in which there is an explicit and consistent account of the household sector as well as the business sector. To answer some research questions, one must also include a sector for the government, which is subject to its own budget constraint. A model within this framework is specified in terms of the parameters that characterise preferences, technology, information structure, and institutional arrangements. It is these parameters that must be measured, and not some set of equations. The general equilibrium language has come to dominate in business cycle theory, as it did earlier in public finance, international trade, and growth. This framework is well-designed for providing quantitative answers to questions of interest to the business cycle student.
>
> (Kydland and Prescott, 1991, p. 168)

The goal of economics is thus the development of comprehensive models of the economy. This focus on modeling is the legacy of Walras, who advocated the creation of a "pure" economic model, devoid of real world distractions.

In writing down "pure" models, economics ends up being rather abstract. This abstractness presents a real danger. For if scientific discovery is not refutable by empirical data, what is to insure that scientific discoveries are in any sense correct? The answer is logic. In the scientific realm, inquiry must proceed according to the strictest rules of logic. Thus, if A is true, a scientist asks what must *necessarily* follow from A.

The necessity of rigorous logic is amplified by the fact that economics is a social science. As such, experiments are not repeatable. In physics, Newton's Second Law of Motion is testable by repeated experimentation in a laboratory. There is no similar means of discovering the true relationship between money and income. As Debreu writes:

> Being denied a sufficiently secure experimental base, economic theory has to adhere to the rules of logical discourse and must renounce the facility of internal inconsistency. A deductive structure that tolerates a contradiction does so under the penalty of being useless, since any statement can be derived flawlessly and immediately from that contradiction.
>
> (Debreu, 1991, pp. 2–3)

We now arrive at the goal of the model. If the foregoing is true, then economists can do no better than develop models of the true underlying structure of the economy. Walras called such a study "pure" economics. We want models which start with the basic facts we know to be true (Walras suggests value in exchange) and rigorously, logically build up models of the economy. Walras' *Elements of Pure Economics* (1926) is an exemplary demonstration of this approach; Arrow and Debreu have developed the modern heir. Note that the point of both of these models is not to allow practical empirical testing. The goal is to explain the true behavior of the economy. Economists can then study how reality fails to live up to the ideal.

There is, however, a very real problem with this methodological approach: it is difficult to use. The wealth of detail in a full-scale Walrasian, general-equilibrium model is immense; e.g., there are n agents, each with his own objective function; m different commodities; and so on. There are far too many agents in the economy to model each one individually. It thus becomes impractical, if not impossible, to study the effects of policy or market imperfections without some simplification. At this point, the representative agent comes to the rescue. By using a representative agent framework, we need only specify a small number of functional forms:

> Rather than carrying along the number of firms and the number of households as additional parameters, which is a nuisance, we use the standard device of "representative" agents. The substantive aspect of this device is to build in the assumption that all firms are alike and all households are alike, while technically it serves to eliminate the need to carry along the numbers of each kind of unit.
>
> (Sargent, 1979, p. 371)

Moreover, by using a representative agent, we can then use a wide variety of powerful mathematical techniques, which are unusable in models with heterogeneous agents, to develop a rich variety of economic models. In particular, we can exploit the fundamental theorems of welfare economics:

To determine the equilibrium process for this model, we exploit the well-known result that, in the absence of externalities, competitive equilibria are Pareto optima. With homogeneous individuals, the relevant Pareto optimum is one which maximizes the welfare of the stand-in consumer subject to the technology constraints and the information structure.

(Kydland and Prescott, 1982, p. 1354)

Or, more specifically:

> The theorems of Bewley (1972) could be applied to establish existence of a competitive equilibrium for this l_∞ commodity-space economy. That existence argument, however, does not provide an algorithm for computing the equilibria. An alternative approach is to use the competitive welfare theorems of Debreu (1954). Given local nonsaturation and no externalities, competitive equilibria are Pareto optima and, with some additional conditions that are satisfied for this economy, any Pareto optimum can be supported as a competitive equilibrium. Given a single agent and the convexity, there is a unique optimum and that optimum is the unique competitive equilibrium allocation. The advantage of this approach is that algorithms for computing solutions to concave programming problems can be used to find the competitive equilibrium allocation for this economy.
>
> (Prescott, 1986, p. 12)

Furthermore, we can exploit the fact that a competitive equilibrium can be computed as a social planning problem. It is relatively simple to solve out the problem faced by a social planner; we need only imagine that the social planner is maximizing the utility of the representative agent. This is the rationale offered by Hansen and Sargent (1990, p. 7): “We use a standard method of computing a competitive equilibrium by solving a Pareto or fictitious social planning problem, a method that was used for this type of model by Lucas and Prescott [1971]. It can be verified that the *aggregate quantities* that solve the Pareto problem are the aggregate competitive equilibrium quantities.” (Also, see Hansen and Sargent, forthcoming; and Cooley and Prescott, 1995.)

The Walrasian goal of rigorous, pure economic models thus seems to be furthered using a representative agent. While Arrow and Debreu’s model is a helpful construct, it is far too complex to be used regularly. The representative agent framework allows an economist to generate a huge variety of models, differing in whatever way is thought to be important. For example, consider the goal of studying the effects of money. By using representative agents we can quickly develop models with cash in advance constraints, legal restrictions, or a finance constraint. The differences and similarities of these worlds can be rapidly examined.

In short, if our goal is the development of pure models of the economy, representative agents enable us to reach this end quickly. Furthermore, it allows for an immense variety of models, without the need to develop the same level of detail found in Walras or Arrow and Debreu.

MICROFOUNDATIONS

Fundamentally, both the Lucas critique and the Walrasian tradition are arguments for incorporating rigorous microfoundations into macroeconomics. Such an argument seems familiar to modern economists. Nowadays, economists seem to have an instinctual belief that macroeconomics is in need of a microeconomic foundation.

This belief in the necessity of microfoundations undoubtedly stems from the seemingly trite observation that only humans can act. All macroeconomic events are the accumulation of millions of decisions made by individual people. From this observation, it seems to follow immediately that if we are ever going to understand the macroeconomy, we need to understand the microeconomic behavior which forms its basis.

How does this seemingly trite observation lead to a belief that we need a rigorous modeling of the microeconomic agents? After all, even old-style Keynesian macroeconomists believed that only agents can act. The answer here is intrinsically tied up with the arguments for the Lucas critique and Walrasian modeling:

> The final and most telling step of [the effort to incorporate rational expectations into macroeconomics] was the insight of Robert E. Lucas and Edward Prescott [1971] that the content of optimizing dynamic economic theory was to deliver cross-equation restrictions across the distributed lags in decision rules, on the one hand, and the equations for the motion of the variables that appear in agents' objective functions and which they care about predicting on the other hand. This meant that when one conducted a thought experiment involving a change in one of the exogenous laws of motion, some or all of the behavioral relations – decision rules – of the model would change. . . . Lucas and Prescott's insight about the cross-equation nature of restrictions on behavioral relations drives the analyst toward explicitly formulating dynamic general equilibrium models at the level of objective functions, constraint sets, and market clearing conditions of their counterparts.
>
> (Sargent, 1982, pp. 382–3)

Once again, however, we run into the same problem. It is tedious and difficult to try to understand the motivations of millions of diverse individuals. It is here that the representative agent comes to the rescue. By rigorously modeling the decision-making process of a single agent and assuming these rules hold in the aggregate, we simultaneously bypass the need to model millions of different agents while still grounding our macroeconomic model in microeconomics.

DISCUSSION

We have identified three motivations for using representative agent models in macroeconomics. However, the three arguments above are not as distinct as we have made them appear.

The Lucas critique is intricately tied up with an appeal to microfoundations. Recall that the reason purely macroeconomic models change with regimes is that agents' behaviors change. The proposed solution to this problem is to directly model the behavior of individual agents, which is exactly the same as saying we need to provide microfoundations for our macroeconomic models.

There are also similarities between the Walrasian tradition and microfoundations. Microfoundations proponents argue that we need to ground all of economics in microeconomic models of the economy; Walrasian methodology also calls for rigorous microeconomic models of the whole economy.

Despite the similarities between these three arguments, we will continue to keep them separate for expository reasons. We want to see how well the representative agent modeling strategy meets each of these three standards. In all three cases, we will find that representative agent models do not meet the goals for which they are constructed; i.e., they do not solve the Lucas critique, they are not a good basis for Walrasian models, and they do not provide for microfoundations.

Part II

THE LUCAS CRITIQUE

4

BEYOND TASTE AND TECHNOLOGY PARAMETERS IN MACROECONOMICS

THE LUCAS CRITIQUE

As we discussed in Chapter 3, perhaps the most frequently mentioned rationale for using representative agent models in macroeconomics is the Lucas critique. In this chapter, we want to examine carefully the Lucas critique and see how it relates to representative agent models.

Lucas (1976) begins by arguing that an economy at time t can be defined as a set of three vectors: (a) the set of endogenous variables, y_t; (b) the set of exogenous variables, x_t; and (c) the set of random shocks, ε_t. Traditional macroeconomics would use these vectors in a model of the form:

$$y_{t+1} = \mathrm{F}(y_t, x_t, \theta\, \varepsilon_t) \tag{4.1}$$

where θ is a set of fixed parameters. The task for the economist is to define the set (F, θ). This is usually done by specifying the functional form, F, and then empirically deriving the values of the parameters in θ.

Let us be more concrete. Consider the simple model:

$$y_{t+1} = \beta x_t + \varepsilon_t \tag{4.2}$$

The functional form, F, is explicitly stated by the fact that y_{t+1} depends only on x_t. A different F could be:

$$y_{t+1} = \alpha y_t + \beta x_t + \varepsilon_t \tag{4.3}$$

In equation (4.2), the θs are the βs; in equation (4.3), the θs are the αs and the βs. Note that the Fs and the θs are not completely independent entities. The choice of F will affect the values of θ. One could start with the functional form in (4.3) and derive the form in (4.2) with the appropriate values of θ, i.e., if the αs are all zero. However, in practice, the Fs and θs are distinct. A researcher chooses F; i.e., he chooses whether to derive values for α (equation (4.3)) or to set them *a priori* equal to zero (equation (4.2)). Given the functional form, the θs are then derived.

With a model of the form in equation (4.1), policy analysis is simple. One merely considers a policy change, determines how the *x*s will be affected, puts the new *x*s into the model, and derives the *y*s. For example, if one of the xs is defense spending, a researcher could find the effect of different levels of defense spending on GDP merely by inserting the appropriate values.

Lucas' contention is that the model in equation (4.1) is misspecified. Equation (4.1) assumes that θ is held constant across policy regimes.[1] There is no reason to assume that the parameters in θ will be unaltered when policy regimes change. For example, one of the θs could incorporate the mean inflation rate, π. In any given regime, π could be considered a constant by the agents in the economy. However, if the government changes its policy in such a fashion that the mean inflation rate either rises or falls, and if agents are aware of this change in π, then equation (4.1) would incorrectly predict the outcome. Equation (4.1) holds π constant and merely alters the *x*s. Agents would not act in the manner predicted by the model if they were aware of the policy change, i.e., agents' behavior would change with the change in regime.

The net result is that changes in the policy regime have two effects: (a) the future path of the *x*s is altered; and (b) the values of the θs may change. Thus, a proper specification of the macroeconomy would be:

$$y_{t+1} = \mathrm{F}(y_t, x_t, \theta(\lambda), \varepsilon_t) \tag{4.4}$$

where λ is the policy regime. This model explicitly takes account of the two effects of a change in policy.

As we noted in the previous chapter (p. 24), the Lucas critique presents us with an interesting theoretical problem, but it does not solve it. The most prominent solution in new classical work has been to try to go "Beyond Demand and Supply Curves in Macroeconomics." In other words, regime changes can only be studied if we go beyond the aggregate curves and search for the "structural equations" that underlie the macroeconomy. By starting with structural equations, we bypass the Lucas critique. Structural equations are, by definition, immune to regime changes.

The result of this exercise is a well-defined maximization problem consisting of "deep" technology and taste parameters. The strategy works only if such parameters can be identified. The rest of this chapter is an exploration of whether new classical representative agent models do in fact identify invariant technology and taste parameters.

THEORETICAL CONSIDERATIONS

Reconsider the formulation of a macromodel suggested by Lucas:

$$y_{t+1} = \mathrm{F}(y_t, x_t, \theta(\lambda), \varepsilon_t) \tag{4.4}$$

where y is a vector of endogenous variables, x is a vector of exogenous variables, λ is the policy regime, and ε a set of random shocks. Until now, we have followed Lucas in defining the term θ only vaguely as "parameters." What exactly are these "parameters"? The examples of θ given by Lucas consist of such things as the percentage of income considered to be transitory, the form of the expectations of future tax cuts, and the mean inflation rate. Thinking of parameters such as these, we can easily see the validity of Lucas' argument. The values of these parameters obviously depend on the policy regime.

However, Lucas does not treat all parameters alike. In equation (4.4), $\theta(\lambda)$ is a vector consisting of all parameters. But Lucas does not consider all parameters to be dependent on policy. In Lucas' exposition, there exist two distinct types of parameters: those dependent on the regime and those constant across time. The latter are taste and technology parameters. Lucas is lumping these two types of parameters into the single vector, $\theta(\lambda)$.

We should note here that exactly what Lucas was arguing in his 1976 paper is open to debate. Some readers may believe that the discussion in this section is actually what Lucas was arguing. For reasons delineated in the next section, it is my impression that Lucas' 1976 critique was the narrower form presented above. However, it is important to note that nothing in the argument presented below depends in any way on what Lucas intended to say in 1976. If the following description of the form of macromodels is what was intended by Lucas, the analysis of deep technology parameters is unaffected.

Let us now separate the two types of parameters that Lucas grouped into θ. Let the economy be characterized as

$$y_{t+1} = \mathrm{F}(yt, xt, \theta(\lambda), \mu, \varepsilon_t) \tag{4.5}$$

Let θ be those parameters contained in decision rules that vary with regimes, e.g., all the variables that Lucas considered when formulating his critique. Let μ be a portmanteau vector of variables contained in the agent's objective functions governing tastes and technology. The distinction between θ and μ is exactly that of Hansen and Sargent (1980, p. 91): "The implication of Lucas's observation is that instead of estimating the parameters of decision rules, what should be estimated are the parameters of agents' objective functions. . . ."

It is from this distinction between θ and μ that new classical representative agent models arose. Traditional macro models were formulated in terms of θs, which were considered to be constant. The heart of the Lucas critique is that it is improper to hold θ constant. So representative agent models were devised in an attempt to create macromodels with variable θs.

Representative agent models are an attempt to model the aggregate economy as if it were a single entity. These new representative agent models were grounded in the "deep" parameters of tastes or technology; i.e., it was attempted to base them on the parameters in

μ. This, then, is the meaning of Sargent's "Beyond Supply and Demand Curves." Representative agent models are believed to be taken one step further, going beyond the θs into the μs.

So what exactly are the parameters in the μ vector? They are usually argued to be the deep parameters of taste and technology. Recall that the purpose of reformulating models in terms of μ is that μ can reasonably be assumed to be constant across regime changes.

What sorts of parameters meet these conditions? The laws of physics certainly provide technological constraints which are immune to regime changes. However, it is hard to imagine an economic model that takes as its starting point the speed of light. Similarly, it is easy to imagine a constancy in human behavior which provides a basis for a utility function, but it is more difficult to give precise numerical values to these constants.

The goal of a macroeconomic model is thus to attain the form:

$$y_{t+1} = F(y_t, x_t, \theta(\lambda), \mu\varepsilon_t) \tag{4.5}$$

However, it is difficult, if not impossible, to identify exactly the parameters in μ. So, in economic modeling, we are forced to use proxies for the variables in μ. While (4.5) is the ideal, macroeconomic models are actually of the form,

$$y_{t+1} = F(y_t, x_t, \theta(\lambda), \phi\varepsilon_t) \tag{4.6}$$

where φ is a vector of proxies for the parameters in μ. The idea behind φ is that while we would like to directly model the deep parameters in objective functions, we can at best model emanations (or even emanations from penumbras) of these deep parameters. While we would like to model the process by which a person decides what choices to make, perhaps the best we can do is to model his marginal propensity to consume.

Proponents of representative agent models argue that they are hitting the ideal of equation (4.5). In actuality, they are writing models of the form of equation (4.6). However, equation (4.6) is a poorly formulated model. In it the changes in the parameters in θ with regimes are acknowledged, but φ is considered to be a constant. If φ was actually μ this would be appropriate, but φ does not contain deep parameters. *A priori,* there is no reason to assume that the variables in φ are immune to regime changes.

The proper specification of the macro model is thus:

$$y_{t+1} = F(y_t, x_t, \theta(\lambda), \phi(\lambda), \varepsilon_t) \tag{4.7}$$

In other words, we need to recognize explicitly that what we call an agent's deep taste or technology parameters are actually merely approximations of the truly deep parameters and, thus, can vary with regime.

Lucas thus narrowed his focus too much. In arguing that θ can move with policy regimes, Lucas argued that traditional macromodels were on shaky theoretical ground. However, the new classical attempts to formulate representative agent models on solid ground have also failed. Traditional macromodels may have unjustly held the parameters in θ fixed, but, if so, representative agent models are equally unjust in holding the parameters in φ fixed.

AN EXAMINATION OF LUCAS (1976)

Some readers may believe that the reformulation of the Lucas critique presented above is merely a rewording of Lucas' point. It is my contention that this view is wrong; the ideas presented here are not what Lucas propounded in 1976. The goal of this section is to demonstrate the limited nature of the Lucas critique. We should note again that the conclusions of this chapter regarding the inability of the representative agent model to solve the Lucas critique problem are completely unaffected by whether we are merely restating the Lucas critique or not.

A natural place to begin this examination is to look at how Lucas' argument was interpreted by researchers attempting to avoid its strictures. The point here is to show that the interpretation of Lucas (1976) outlined above is similar to the interpretation of other researchers. Sargent is, of course, the most obvious interpreter: "The implication of Lucas's observation is that instead of estimating the parameters of decision rules, what should be estimated are the parameters of agents' objective functions and of the random processes that they faced historically" (Hansen and Sargent, 1980, p. 7). Similarly:

> In dynamic contexts, a proper definition of people's constraints includes among them laws of motion that describe the evolution of the taxes they must pay and the prices of the goods they buy and sell. Changes in agents' perceptions of these laws of motion (or constraints) will in general produce changes in the schedules that describe the choices they make as a function of the information they possess.
>
> (Sargent, 1981, p. 213)

Thus, these proponents of formulating representative agent models explicitly state their belief that by formulating models in terms of preference or technology parameters, the Lucas critique is bypassed. In short, the Lucas critique is viewed as restricting its focus to the expectational part of decision rules.

It can, of course, be argued that the proponents of formulating representative agent models have a vested interest in interpreting the Lucas critique narrowly. So consider another interpretation of the Lucas critique, that of Sims:

> Lucas argues that, since a policy is not really just one change in a policy variable, but rather a rule for systematically changing that variable in response to conditions, and since changes in policy in this sense must be expected to change the reduced form of

> existing macroeconometric models, the reduced form of existing models is not structural even when policy variables have historically been exogenous – institution of a nontrivial policy would end that exogeneity and *thereby change expectation formation rules* and the reduced form. . . .
>
> To summarize the argument, it is admitted that the task of choosing among policy regimes requires models in which explicit account is taken of the *effect of the policy regime on expectations.*
>
> (Sims, 1980, pp. 12, 14, emphasis added)

In other words, Sims interprets Lucas in exactly the same way as Sargent does: as focusing exclusively on expectations variables and not on taste and technology parameters.

Of course, the fact that others have interpreted Lucas' point narrowly does not in and of itself justify such an interpretation. What needs to be determined is whether such an interpretation is consonant with the Lucas paper.

The heart of the question at hand is exactly what kinds of variables Lucas was referring to when he discussed θ. In other words, did Lucas intend θ to refer to expectational variables alone or did he mean it to include taste and technology parameters as well? The obvious place to look is the definition of θ in Lucas' (1976) paper. Unfortunately, θ is defined rather vaguely. The extent of the descriptions of θ are as a "fixed parameter vector" (p. 21) and "behavioral parameters" (p. 40). Additionally, Lucas states, "The function F and parameter vector θ are derived from decision rules (demand and supply functions) of agents in the economy" (p. 25). Note that θ is argued to come from decision rules, not objective functions. We have here our first indication that θ is defined as not including parameters of utility or profit-maximizing functions. Clearly, however, this is a slender reed on which to build an argument.

As Lucas' definition of θ offers little indication as to his meaning, we must seek other avenues of inquiry. Fortunately, one is at hand. In his paper, Lucas gives three examples of his point about regime changes. By closely inspecting each of these examples to see which parameters Lucas argued were subject to alteration with regime and which were held constant, we gain an insight into his meaning. This inspection is certainly not an attempt to glean facts from air; presumably, the purpose of Lucas' examples was to aid others in understanding what he meant.

The three examples are models of consumption, taxation and investment demand, and the Phillips curve. These models are used to show how policy regime changes will alter standard economic models. An examination of the three cases reveals a striking similarity. In every case, the variables that Lucas argues will change with regime are those relating to expectations. Furthermore, in every case, taste and technology parameters are held as constants.

Consider each model in turn. In the consumption model, the parameter Lucas argues is variable is a weighting term on income. Changes in this parameter indicate changing perceptions about the amount of current income that is permanent versus that which is transitory. Thus, policies that change an agent's future income will alter his expectations of what percentage of his future income is permanent. However, in the same model we find that the discount rate and the marginal propensity to consume permanent income (both taste parameters) are fixed constants.

The second model shows the effects of taxation on investment demand. The focus of Lucas' attention here is the expectation of the investment tax credit at time *t*. Changes in the process governing taxation will alter firms' expectations of the future values of this credit. However, in this model there are several fixed technology parameters: the output–capital ratio, the depreciation rate, the cost of capital, and the slope of the demand function.

Finally, in the Phillips curve model, Lucas examines the variability of several expectational variables: the mean inflation rate, the variance of the general price level about its expected value, and the persistence of inflation. Technology parameters are once again held constant, namely the intertemporal substitution possibilities in supply, the variability of relative prices in supply, and the nominal or permanent supply.

A definite pattern has emerged. In every case, the only variables with which Lucas concerns himself are those relating to agents' expectations of future variables. Furthermore, in every case all taste and technology parameters are regarded as constant. The issue here is not whether it is proper to hold these taste or technology parameters constant; the issue is the pattern of the types of variables which are invariably held constant.

The matter becomes especially clear when we further contemplate the taxation and investment demand example. In it, the government manipulates the investment tax credit. Lucas argues that expectations about the future values of the tax credit will vary. However, one of the "technology" parameters held constant is the cost of capital. This is improper; changes in the investment tax credit will change the demand for capital and in all but the most degenerate of cases will change the cost of capital. Thus, in making his point about how θ needs to vary with policy, Lucas improperly holds a technology parameter constant across a contemplated regime shift.

We can finally turn to Lucas' own observations on his 1976 paper in the Introduction to *Studies in Business Cycle Theory* (1981). He writes:

> The paper stressed the importance of identifying structural parameters that are invariant under the kinds of policy changes one is interested in evaluating; and in all of the paper's examples, only the parameters describing "tastes" and "technology" were treated as having this property. This presumption seems a sound one to me, but

> it must be defended on empirical, not logical grounds, and the nature of such a defense presumably would vary with the particular application one has in mind.
>
> (Lucas, 1981, pp. 11–12)

This, however, is precisely why Lucas' 1976 paper is too weak. In it, Lucas considers it proper to hold taste and technology parameters constant in the hope that they are invariant. The contention of this chapter is that such hopes are in vain. Furthermore, Lucas' 1981 argument that the invariance of taste and technology parameters should be decided on empirical grounds is in fundamental contradiction to the underlying premise of his 1976 paper. If it is an allowable research strategy to decide empirically which taste parameters can be held constant, then it should also be allowable to decide empirically which expectational (or, for that matter, any other type of) variables can be held constant.[2] Lucas, however, is reduced to arguing that as a theoretical matter one class of parameters cannot be held constant, but as an empirical matter another class of parameters may be held constant.

AN EXAMPLE

The purpose of this section is to illustrate the argument made in the previous sections via a thorough examination of a prototypical new classical representative agent model. The goal here is twofold. First, we want to see if new classical representative agent models live up to the standards of a good model set forth by their proponents. Do representative agent models actually avoid the strictures of the Lucas critique? We will find that they do not. We then want to dig deeper and examine the possibility of improving these models so that they could meet these standards.

The model we will examine is that found in Sargent (1981). This model was chosen for several reasons. First, the model is embedded in a paper whose primary goal is to set down the criteria of a good model. Sargent is explicit about this goal. The paper begins, "This paper explores some of the implications for econometric practice of a single principle from economic theory. This principle is that people's observed behavior will change when their constraints change" (Sargent, 1981, p. 213). We have thus chosen as our foil a fundamental defense of the representative agent model methodology.

Sargent's model makes an admirable example for other reasons. It is a large enough model to allow us to raise several interesting points, but it can be outlined with relative brevity. It is similar in style to many other representative agent models. Furthermore, as the paper explicitly sets down the criteria for a good model, we are able to compare the model to the standards explicated in the same paper.

Readers with a thorough understanding of the issues raised here will have little trouble finding similar problems in other new classical representative agent models. The problems discussed below are common.

Sargent is very explicit about the proper means of modeling economies:

> The practice of dynamic econometrics should be changed so that it is consistent with the principle that people's rules of choice are influenced by their constraints. This is a substantial undertaking and involves major adjustments in the ways that we formulate, estimate, and simulate econometric models. Foremost, we need a stricter definition of the class of parameters that can be regarded as "structural." The body of doctrine associated with the "simultaneous equations" model in econometrics properly directs the attention of the researcher beyond reduced-form parameters to the parameters of "structural equations," which presumably describe those aspects of the behavior of people that prevail across a range of hypothetical environments.
>
> (Sargent, 1981, p. 214)

In short, the goal is to ground models in parameters that are not changed by regime shifts. What types of parameters are these?

> There is a general presumption that private agents' behavior and the random behavior outcomes both will change whenever agents' constraints change, as when policy interventions or other changes in the environment occur. The most that can be hoped for is that the parameters of agents' preferences and technologies will not change in the face of such changes in the environment. If the dynamic econometric model is formulated explicitly in terms of the parameters of preferences, technologies, and constraints, it will in principle be possible for the analyst to predict the effects on observed behavior of changes in the stochastic environment.
>
> (Ibid., pp. 215–16)

Thus, models are to be formulated in terms of taste and technology parameters. These are the variables in the vector μ described in the previous section. As long as models are grounded firmly in the stable variables in μ, invariance can be assumed.

Sargent goes on to explain why a formulation of models in terms of taste and technology parameters is needed:

> Past dynamic econometric studies should usually be regarded as having been directed at providing ways of summarizing the observed behavior of interrelated variables, without attempting to infer the objectives, opportunities, and constraints of the agents whose decisions determine those variables. Most existing studies can be viewed, at best, as having estimated parameters of agents' decision rules for setting chosen variables as functions of the information they possess. . . . Dynamic economic theory implies that these decision rules cannot be expected to remain invariant in the face of

> policy interventions that take the form of changes in some of the constraints facing agents. This means that there is a *theoretical presumption* that historical econometric estimates of such decision rules will provide poor predictions about behavior in a hypothetically new environment.
>
> (Ibid., p. 216, emphasis added)

Note specifically the last sentence. There is a "theoretical presumption" that all estimates of parameters derived from historical evidence will change with regimes.

We thus have here a very explicit outline of the exacting requirements of a good model. Sargent proceeds to develop a representative agent model which he claims lives up to these standards and thus provides a solid basis for modeling. We want to examine this claim. Does this representative agent model, which has been carefully crafted to be invariant to regime shifts, change with policy? In other words, is the representative agent model truly formulated in terms of the invariant taste and technology parameters of μ? In order to answer this latter question affirmatively, it must be the case that the parameters held constant can reasonably be expected to remain invariant during plausible regime changes. Moreover, if we are going to estimate the numerical values of these technology parameters using historical evidence, we must assume that they have been invariant to previous regime shifts, otherwise there will be a "theoretical presumption" that the technology parameter is not constant.

However, we do not wish to stop with a demonstration that the parameters in Sargent's representative agent model properly belong to the vector φ and thus change with policy. We want to explore the deeper question of whether or not it is possible to recast Sargent's framework in terms of truly invariant parameters. The ultimate issue here is that while it sounds nice to say that models should be formulated in terms of invariant technology parameters, is it possible to do so? We will find that at the very least such a grounding is difficult, if not impossible.

The model is of a representative firm which uses a single capital input, *k,* to produce output, *y,* according to the equation

$$y_t = fk_t + n^{-1}\varepsilon_t, \quad f > 0 \tag{4.8}$$

where *n* is the number of firms in the economy, with aggregate capital stock, K, and ε is a random shock. The representative firm maximizes the following:

$$\begin{aligned} \mathrm{E}_0 \sum_{t=0}^{\infty} \beta^t \Big[& (\mathrm{A}_0 - \mathrm{A}_1 f \mathrm{K}_t - \mathrm{A}_1 \varepsilon_t + \mathrm{A}_2 \mathrm{D}_{It} + u_t)(fk_t + n^{-1}\varepsilon_t) \\ & - w_t k_t - \left(\frac{d}{2}\right)(k_{t+1} - k_t)^2 \Big] \end{aligned} \tag{4.9}$$

subject to

$$K_{t+1} = H_0 + H_w(L)W_t + H_D(L)D_t + H_\varepsilon(L)\varepsilon_t + H_u(L)u_t + H_1K_t \tag{4.10}$$

$$\delta_w(L)W_t = V_t^w \tag{4.11}$$

$$\delta_u(L)u_t = V_t^u \tag{4.12}$$

$$\delta_D(L)D_t = V_t^D \tag{4.13}$$

$$\delta_\varepsilon(L)\varepsilon_t = V_t^\varepsilon \tag{4.14}$$

Additionally, the demand curve for output is

$$P_t = A_0 - A_1Y_t + A_2D1_t + u_t \tag{4.15}$$

The variables in the above equations are as follows (omitting time subscripts):

E = the expectations operator

L = the lag operator

β = the discount rate

Y = aggregate income = ny

P = the price of output

w = the rental rate on capital

W = a vector whose first element is w; the remaining elements are variables which help to predict future ws

D_1 = a vector of random variables in the industry demand schedule

D_2 = a vector of random variables that helps predict future values of the variables in D1

$D = [D_1\ D_2]'$

u = a random shock to demand

d = the cost of adjusting the capital stock

$$\delta_i(L) = I - \sum_{j=1}^{r_i} \delta_{ij}L^j, \qquad i = w, u, D, \varepsilon$$

$$V^i = \text{white noise error for } i, \qquad i = w, u, D, \varepsilon$$

$$H_i(L) = I - \sum_{j=1}^{r_i - 1} H_{ij}L^j, \qquad i = w, u, D, \varepsilon$$

Sargent argues that this is a model of the economy that is well grounded in the agent's optimization function. Policy regime shifts can be characterized as a change in one of the δs. Since the model is fundamental, by altering the δ in a suitable fashion, we can accurately predict the change in the firm's behavior when policy changes. We have, in other words, solved the problem raised by Lucas.

Sargent specifically considers three examples of interventions and how they would alter the model. First, a tax on output sales would alter δ_D. A tax on the input would alter δ_w. A change in the process by which the pretax rental rate is determined would also alter δ_w.

We should briefly note how this method of examining regime changes differs from that which Sargent finds objectionable. Suppose we are interested in the effect of differing series of demand shocks, u. Sargent argues that a traditional macromodel would merely posit a new series of demand shock realizations, i.e., a different set of values for the variables in u, and then churn out the result. The Lucas critique argues that this process is improper since agents would alter their behavior with the change in the nature of the demand shock. Sargent's model gets around this criticism by changing δu with the change in u. Thus, agents' decision rules change with regimes. We are not pretending that agents think we are in the old regime when the new regime is enacted.

We now wish to take up in turn each of the fixed parameters in the model: β, n, d, f, r_w, r_u, r_D, r_ε, A_0, A_1, A_2. Each of these parameters is considered to be an invariant technology parameter. However, there is no theoretical reason to assume that any of them is truly constant across previous regime changes or contemplated future regime changes. In terms of equations (4.5) and (4.6), these variables are properly in the vector φ, not μ.

We will begin by examining the ubiquitous discount rate, β. At first, holding β constant seems benign; after all, it is a constant in a multitude of models, new classical and others. But carefully consider exactly what β represents. For a profit-maximizing firm, the discount rate is nothing other than the real interest rate (or, more properly, the real after-tax interest rate). Firms of the sort in Sargent's model do not have utility functions wherein they can be said to prefer profits today over profits tomorrow for esoteric, hedonistic reasons. The only reason a profit-maximizing firm prefers profit today is that such profit today is worth more than an equivalent dollar amount tomorrow. The stream of income on a bond is always discounted at the interest rate; the stream of profits to a firm should be discounted in exactly the same manner.

A recognition of the true variable represented by β immediately drives the conclusion that it is improper to hold it fixed. A wide variety of policies have in the past, and can in the future alter, real interest rates; e.g., unless the Fisher effect holds perfectly, raising the inflation rate lowers real interest rates. A firm's discount rate is clearly not a deep parameter.

A second parameter that is improperly held constant is n, the number of firms in the industry. Once again, it takes only a moment's reflection to realize that the number of firms is not immune to regime changes. Indeed, n cannot be realistically considered to be a constant even in the absence of regime changes. There are more firms at the peak of a business cycle than at the trough. So any policy leading to an expansion or contraction of the economy has in the past, and will in the future, alter n. Similarly, government's taxing and spending decisions affect n. For example, the recent ill-fated luxury tax in the United States wiped out the yacht industry. Unless new firms arise one for one with the demise of yacht producers, the total number of firms has changed.

Consider next the fixed parameter d, the cost of adjusting the capital stock. (To be precise, $(d/2)(k_{t+1}-k_t)^2$ is the cost of changing the capital stock. However, d is the constant component of the cost, e.g., raising capital by one unit always implies a cost of $d/2$, raising capital by 2 units implies a cost of $2d$, etc.) This parameter is also improperly held fixed; adjustment costs do differ when policy changes. For example, recent environmental legislation has certainly changed the costs to firms of changing their capital stock, in addition to changes in the cost of capital itself. Changes in the capital stock now must involve search costs for "cleaner" capital, changes in the physical characteristics of a factory to accommodate differently shaped machines, new training for labor on how to operate the new machinery, and so on. Similarly, if the input was labor, affirmative action programs would alter the costs of hiring new workers.

Thus, it is improper to consider d a fixed number. However, Sargent does temper his use of a constant d by noting: "it is straightforward to modify the model to incorporate much richer dynamics by generalizing the nature of the adjustment costs" (Sargent, 1981, p. 218). What Sargent means by this statement is not really clear. There are two possible interpretations. First, he could be noting that the term $(d/2)(k_{t+1}-k_t)^2$ is too simplistic. One could for example allow for an exponent on d other than one, have more lags of the capital stock included, etc. However, a more general constant is still a constant. The problem with these reformulations is that they still do not allow adjustment costs to change with regimes.

Instead, Sargent could be arguing that d should in reality not be considered a constant, but rather a better model would have d be a function of even deeper variables. This argument is saying that since d is in the vector φ, we need to find the deeper parameters in μ which drive d. Let us for a moment take up the task of finding deeper parameters for d. Since d is too shallow a parameter, we desire to ground adjustment costs in a deeper foundation. What would such a model of adjustment costs look like? In short, what are the deeper, fixed parameters on which adjustment costs depend? It is difficult to come up with any ideas. These deeper parameters can have nothing to do with the nature of the capital stock or labor quality itself, the conditions of capital or labor supply, the structure of the portion of a firm's bureaucracy that is responsible for changing the capital stock or labor demand, or government regulations regarding the changing of capital or labor. All of these characteristics have changed and will change with regimes. We are seeking invariant characteristics relating to adjustment costs. If they exist, they seem to be quite ephemeral and thus, to put it mildly, difficult to quantify.

Next, consider f, the output per unit of capital. Once again, this is a parameter that should not be fixed. Any government policy that increases or decreases firm efficiency, e.g., affirmative action laws, has in the past and will in the future alter f. However, let us again try to remedy the problem of a constant f by considering how f could be reformulated as a function of deeper, invariant parameters. Once again, this is difficult to do. We cannot use any parameter that is related to the capital or labor itself. Yet the nature of the capital or labor is the determining factor of its productivity. It seems to be impossible to ground f in invariant parameters.

The exact nature of the parameters r_u, r_D, r_w, and r_ε is questionable. These parameters are the number of relevant lags in the $\delta_i(L)$ polynomials. It is unclear whether Sargent intended these parameters to be variable or fixed. They are never included in Sargent's lists of parameters. Making matters confusing, Sargent sometimes lists the polynomials $\delta_i(L)$ as parameters (which would include the r_is) while other times he refers to the δ_1s as the parameters (which would exclude the r_is). Presumably, Sargent meant to consider the r_is on the same plane as the δ_is. Both these parameters are part of the structure determining the realization of exogenous shocks. They are included in the firm's objective function since firms form expectations of the future on the basis of the nature of these shocks. Recall that this is the place where Sargent bypasses the Lucas critique. It makes no sense to argue that policy regimes can change only the δ_is, but the r_is must remain constant. Consistency demands that the r_is and δ_is be treated the same. Since Sargent's entire argument is that the δ_is change with policy, there is presumably no argument to noting that the r_is change with policy as well.

Finally, consider the demand function in equation (4.13). The parameters A_0, A_1, and A_2 are held constant, implying a completely stationary demand curve. Obviously, this is not reasonable. Sargent is not oblivious of this simplification. In a footnote, he remarks, "Specifying a demand schedule with interesting dynamics would complicate the presentation but not alter the basic message of our example" (Sargent, 1981, p. 220). Sargent would thus have no objection to noting that the demand curve has in the past and will in the future change with policy.

However, the inclusion of a degenerate demand curve in this model is an indicator of the new classical view on deep taste and technology parameters. Imagine a paper similar in style to Sargent's, but with the following change: the demand side is grounded more firmly in deeper parameters, but the laws of motion of exogenous shocks are not rigorously incorporated into the firm's objective function. In essence, this new paper simplifies one aspect of the model in order to deepen another part. There is little doubt that Sargent (and Lucas) would vehemently argue that such a model is without value as it is subject to the Lucas critique. The contrast is quite illuminating. On the one hand, Sargent condemns traditional macromodels as being too superficial, while on the other hand he asks us to take his model (with its heroic simplifications) seriously as an example of good modeling technique. What exactly is the difference? The only difference is in the aspects of the model that are simplified. Models in which expectational variables are held immune to past and future regime shifts are believed to be invalidated by the Lucas critique. Yet a model which purports to be "formulated explicitly in terms of the parameters of preferences, technologies, and constraints" and thus useful "for the analyst to predict the effects on observed behavior of changes in the stochastic environment" (Sargent, 1981, pp. 215–16) and thus a model that avoids the strictures of the Lucas critique, contains a fixed demand curve.

None of the parameters in Sargent's representative agent model is actually a deep taste or technology parameter. Thus, this representative agent model fails to meet the standard of being immune to past and future regime changes. Every fixed parameter in this model has changed in the past and will change in the future with changes in policy. There is a "theoretical presumption" that historical evidence of the numerical values of these parameters is invalid since the numerical values have changed in the past with regime changes. There is also a "theoretical presumption" that these technology parameters will change with future regime changes in general and particularly those regime changes commonly studied in macroeconomics, e.g., changes in the inflation rate or changes in tax rates or structure. Thus, if we accept Sargent's argument that the Lucas critique must be taken seriously, then we cannot use Sargent's representative agent model as a good model.

ADDITIONAL NOTES

In this section we shall consider several related issues. The first is the relationship between this argument and Kydland and Prescott's (1982) real business cycle model. The Kydland and Prescott model uses a representative agent framework to develop a comprehensive model of the economy. Like Sargent, they attempt to ground their model in the deep taste and technology parameters of the economy.

There is a fundamental oddity in the Kydland and Prescott model. Its most notable feature is that business cycles are solely driven by technological shocks. However, the technological shocks never change the technology in the model. The following are all considered fixed technological parameters: the capital–labor ratio, the elasticity of substitution between capital and inventories, the shares of capital and inventories, the depreciation rate, and the fraction of the resources allocated to an investment project from the jth stage to the last. With all of these factors fixed, it is hard to fathom exactly what constitutes a technological shock.

In the model, the technological shocks function as a scaling factor on output. In other words, exactly the same worker and capital produce more or less output from period to period depending on the shock. It might be more proper to call such shocks "productivity shocks." However, while it may be plausible that business cycles are driven by technology shocks, it is more difficult to believe that they are caused by fluctuations in the productivity of a given technological process.

In sum, the Kydland and Prescott model contains a fundamental inconsistency. The authors have attempted to ground the model in a fixed technology while at the same time having technological change drive the business cycle. Once again, we find that the fixed parameters are not as deep as they need to be.

Until now, all our examples have been of technological parameters. The technological parameters generally assumed to be deep have been shown to be rather shallow. In fact, the very existence of deep technological parameters is questionable. The situation in representative consumer models is only slightly better. The parameters generally used in modeling utility functions in macroeconomics are also rather shallow.

We will not take the time to explore thoroughly a model of utility maximization by a representative consumer. The arguments used here are identical to those used about production models. However, it is worth considering a couple of taste parameters that are relatively important in new classical representative agent models in general and real business cycle models in particular. Both the intertemporal elasticity of substitution and the intertemporal discount rate are often used as fundamental, and thus fixed, parameters in these models.

It is not surprising that economists would like to consider taste parameters as invariant. That economists should do so was the argument in Stigler and Becker (1977). Rather than simply explain everything as a matter of changing tastes, it is a goal of economics to use a framework in which tastes are fixed and relative price changes explain behavior.

This sort of argument leads directly to the belief in invariant taste parameters. This chapter does not intend to dispute the existence of such parameters. The point here is simply that while invariant taste parameters do exist, the fact that we call something a taste parameter does not make it invariant.

In particular, what macroeconomists are willing to accept as an invariant taste parameter is not necessarily one at all. To make this argument concrete, we will look at the model used in Cooley and Prescott (1995).

The model is a standard real business cycle model in which utility is maximized subject to production constraints. To make the exposition here simpler, we will set both the population growth rate and the long-term real growth rate equal to zero. (None of the following analysis would be altered in any way if we followed Cooley and Prescott in allowing for nonzero values.) The basic model is then:

$$\text{Max} \sum_{t=0}^{\infty} \beta^t u(c_t, l_t)$$

subject to

$$c_t + x_t = e^{z_t} k_t^{\theta} h_t^{(1-\theta)}$$

$$k_{t+1} = (1 - \delta)k_t + x_t$$

$$z_{t+1} = \rho z_t + \varepsilon_t$$

The parameters are:

c = consumption
x = investment
k = capital stock
h = hours worked
l = leisure = $(1 - h)$
z = technology parameter
β = discount rate
δ = depreciation rate
θ = capital share in production

The first utility function specified by Cooley and Prescott is:

$$u(c, l) = \frac{(c_t^{(1-\alpha)} l_t^{\alpha})^{1-\sigma} - 1}{1 - \sigma}$$

Cooley and Prescott state that α is the share parameter for leisure and $1/\sigma$ is the intertemporal elasticity of substitution.

Consider the parameter σ Is it reasonable to assume that this parameter meets the Sargent standard, i.e., is it reasonable to assume that such a parameter has been invariant to past regime changes and will be invariant to future changes? If it truly was the intertemporal elasticity of substitution, then perhaps it would be reasonable. However, σ is actually the willingness of the agent to substitute leisure today for leisure tomorrow. The difference is crucial.

To see the superficiality of this parameter, consider a regime change in which the relative cost of leisure-time activities requiring extended vacations is raised, for example, from an increased gas tax. Suppose that the effect of this regime change is that it makes agents less willing to work more today in order to have more leisure in the future. Stigler and Becker would presumably argue that this regime change is properly considered to be a change in relative prices; the price of an extended vacation rises, so people buy fewer of them. In this case, there is no change in tastes.

However, where would such a regime change show up in the Cooley and Prescott model? It would not affect the production side of the model; the production function, the capital accumulation process, and the productivity shock are all unchanged. So, in this model, it must change one of the parameters in the utility function. The parameter σ measures the willingness of an agent to work more today in exchange for leisure tomorrow. In this model, the numerical value of σ is what changes as a result of this regime shift.

How can we reconcile the Stigler and Becker argument with the argument that the taste parameter σ would change with the regime? We simply note that what Cooley and Prescott call the intertemporal elasticity of substitution is actually more than that. In fact, σ is an agglomeration of the true taste parameters governing the intertemporal elasticity of substitution and the relative prices that govern the exact choice of leisure; it is in fact a parameter reflecting the outcome of the labor–leisure choice. For any given regime, σ is a fixed value. However, with different relative prices, the willingness to substitute leisure today for leisure tomorrow changes and hence the numerical value of σ in this model will be different. The invariant elasticity of substitution is only a part of what determines the numerical value of σ relative prices are also important.

In fact, this discussion is being rather charitable to Cooley and Prescott. They do not actually use the production function above in their simulations. Instead, they assume that $\sigma = 1$, and use the utility function

$$u(c_t, l_t) = (1 - \alpha) \log c_t + \alpha \log l_t$$

In this model the only "deep" taste parameter governing utility is α the share of time spent in leisure activities. The amount of time an agent chooses to work is certainly not a deep parameter. Changes in the tax rate have an obvious impact on α Yet Cooley and Prescott use α as if it were an invariant parameter in their model.

We can see similar problems in another "taste" variable that is widely used as an invariant parameter in representative agent models. All intertemporal utility maximization problems contain a discount rate. This parameter expresses a person's willingness to trade happiness (or more properly, utility) today for happiness tomorrow. As with firms, people's discount rates are usually considered constant not only across regimes, but across time periods as well.

However, the discount rate is not a fundamental characteristic of man. A person's perspective on the future can change for a whole host of reasons. There are policies that can increase or decrease the weight people put on future happiness. Perhaps the most striking example of this fact is the societal breakdown in the inner cities. Gilder (1981) describes the situation nicely:

> Edward Banfield's *The Unheavenly City* [1968] defines the lower class largely by its lack of an orientation toward the future. Living from day to day and from hand to mouth, lower class individuals are unable to plan or save or keep a job. Banfield gives the impression that short-time horizons are a deep-seated psychological defect afflicting hundreds of thousands of the poor.
>
> There is no question that Banfield puts his finger on a crucial problem of the poor and that he develops and documents his theme in an unrivaled classic of disciplined social science. But he fails to show how millions of men, equally present oriented, equally buffeted by impulse and blind to the future, have managed to become far-seeing members of the middle class. He also fails to explain how millions of apparently future-oriented men can become dissolute followers of the sensuous moment, pursuing a horizon no longer than the most time-bound of the poor.
>
> (Gilder, 1981, p. 70)

Gilder goes on to argue that this change in horizons is due to government welfare policies. (Murray, 1984, 1988, has made the same point at some length.) Readers who reiect the conclusions of the conservatives Gilder and Murray are referred to Michael Harrington's *The Other America* (1966), in which he argues that there exists a "culture of poverty" with a concomitant "warping of the will and spirit." Regimes which exacerbate or alleviate this culture will therefore alter the "will and spirit." Thus, researchers who have examined such things find government policies that have in the past or can in the future induce changes in discount rates.

How can we reconcile these arguments for changing discount rates with the Stigler and Becker (1977) argument that tastes do not change? The reconciliation is simply a matter of acknowledging that what shows up as the discount rate in economic models is once again a combination of the true taste parameter reflecting impatience and assorted prices that govern expectations about the future.

The point here is not that it is impossible to conceive of invariant taste parameters. It is simply that the taste parameters used in many representative agent models cannot be reasonably asserted to be fixed.

In a similar vein, consider a point made by LeRoy (1991). Sargent *et al.* base their claim to solve the Lucas critique on the incorporation of rational expectations into their models. When a policy variable changes, agents incorporate this change into their expectations of the future.

LeRoy argues that models of this form do not actually incorporate rational expectations, but rather are based on stationary expectations. Agents do not take account of expected future changes in the policy variable. Rather, agents believe that the current policy regime will exist in perpetuity. This is an additional manner in which these models fail to live up to the standards of being immune to regime changes. (Sims, 1987, makes a similar point.)

Oddly, Sargent (1993a) acknowledges this point, yet he proceeds to justify such models on a rather astounding basis. Referring to his own work (Sargent, 1993b, ch. 3) on the end of hyperinflations, Sargent argues, "That the price stabilizations occurred so rapidly perhaps provides some reason for thinking that we don't make much of an error by ignoring the possibility that the prospects for a regime change occurring should really have been built in when analyzing the initial regime" (Sargent, 1993a, p. 28). Would the same apply to ignoring other effects of regime changes? Is it also possible we have some reason for thinking that we don't make much of an error by ignoring the *"theoretical presumption* that historical econometric estimates of such decision rules [ignoring the Lucas critique] will provide poor predictions about behavior in a hypothetically new environment" (Sargent, 1981, p. 216, emphasis added)? If so, then what was all the fuss about?

Finally, even if we could isolate the truly "deep" taste and technology parameters for a representative agent, it is not necessarily true that the resulting model is invariant to regime changes. For example, Kupiec and Sharpe (1991) examine the effect of imposing margin requirements on stock market volatility. They unambiguously show that if the underlying investors are heterogeneous, then the actual change in volatility will be different from the change predicted by a representative agent model. They conclude, "Uncovering the 'deep parameters' of a representative agent model may be insufficient or even useless for macrofinancial policy analysis" (Kupiec and Sharpe, 1991, p. 728). Until we have explored the aggregation problem in Chapter 10, we will have to defer an exploration of the reason that heterogeneity in the population being represented can make the representative agent model inherently incapable of modeling the effects of regime changes.

REPRESENTATIVE AGENTS AND THE LUCAS CRITIQUE

Lucas (1976) tells us that old-style Keynesian macroeconomic models are fatally flawed. Such models do not incorporate the effects of regime changes on the fixed parameters in the model. Thus, policy predictions using these models are not reliable; the fixed parameters may change with every change in policy. Moreover, the historical estimates of the numerical values of the fixed parameters are not reliable; there is no reason to assume that the historical numerical values will be the same as the future numerical values. Lucas (1976) thus results in a theoretical presumption that old-style Keynesian macroeconomic models are not useful.

If the argument in the previous paragraph is true, then representative agent models suffer the same fate as their Keynesian siblings. As we have seen throughout the chapter, representative agent models do not contain invariant taste and technology parameters. Thus, the policy predictions using a representative agent model are similarly not reliable; the fixed parameters may change with any change in policy. Moreover, the historical estimates of the numerical values of the fixed taste and technology parameters are not reliable; the future numerical values may similarly be different from the historical estimates. If Lucas (1976) gives us a theoretical presumption that old-style Keynesian macroeconomics models are not useful, then it also gives us a theoretical presumption that representative agent models are not useful.

For purposes of solving the problems detailed by the Lucas critique, representative agent models buy us nothing. They are in no way a theoretical improvement over old-style Keynesian macroeconomic models in this regard. Both types of models have invariant parameters that are improperly held fixed when simulating regime changes. Thus, Lucas could just as easily have been talking about representative agent models (instead of Keynesian macroeconomic models) when he wrote:

> simulations using these models can, in principle, provide *no* useful information as to the actual consequences of alternative economic policies. These contentions will be based not on deviations between estimated and "true" structure prior to a policy change but on the deviations between the prior "true" structure and the "true" structure prevailing afterwards.
>
> (Lucas, 1976, p. 20)

IS IT TIME TO DISPENSE WITH THE LUCAS CRITIQUE?

So where does this leave us? The Lucas critique is an unacceptable standard by which to judge macroeconomic models. Sargent has argued that there is a "theoretical presumption" that the estimated values of decision rules will change with policy. However, there is exactly the same "theoretical presumption" that estimated values of shallow taste and technology parameters will change with policy. The Lucas standard is thus inconsistent; it focuses on the problems associated with one type of parameter while turning a blind eye to the same problems in other parameters.

Some may argue that even though the Lucas critique is inconsistent, it is still an appropriate standard. After all, Lucas was correct in arguing that Keynesian economists improperly used the Phillips curve by ignoring the effects of regime changes on decision rules. However, the fact that the Lucas standard was appropriate in the discussion of Phillips curves does not imply it is appropriate everywhere. There is no reason to assume that shifts in the parameters in decision rules are empirically more important than those in utility or production functions. Recognizing that Lucas'contention about the Phillips curve does not immediately apply everywhere is in no way an exoneration of Keynesian macromodels. Lucas' argument that these models improperly held expectations fixed is still valid.

So the natural option seems to be to broaden the Lucas standard to include taste and technology parameters. In other words, we could argue that any model which does not allow for an accurate prediction of the effects of any regime shift is improper. In short, we acknowledge that what have been commonly called taste and technology parameters are not deep. So we set up a new standard that calls for models based on truly deep parameters.

This is a rather nihilistic standard. In order to have models based on truly deep parameters, we must first identify these parameters. As we have seen, such identification is at best extraordinarily difficult. This is therefore an impractical standard. There can be no model of reality that is a perfect representation of reality. No model can ever live up to the standard of being immune to all plausible regime shifts.

While the Lucas critique (and its more general counterpart presented here) is a powerful *theoretical* argument, the evidence of its *empirical* importance is mixed. The reason for this mixed empirical support seems almost trivially obvious. Surely, regime changes have important effects on parameters in some cases but not in others; i.e., there are some regime shifts that will cause some models to break down, but not all regime shifts will have large (or even measurable) effects on all models.

We are thus in need of a more pragmatic standard. A good maxim for economists to abide by would be: If it is obvious that a particular regime change will alter a particular parameter, do not hold the parameter fixed when contemplating this change. This rule is at once weaker and stronger than the Lucas rule; it is theoretically weaker, as not all fixed parameters are subject to criticism, but it is empirically stronger, as it forces economists to think seriously about each parameter they wish to fix.

This more pragmatic standard would be effective in rebutting the more egregious models used to study regime shifts. For example, the simple Keynesian Phillips curve models are subject to the same criticism from this standard as from the Lucas standard. However, this more pragmatic standard might help to prevent many bad models being used in the first place. By aiming broadly, the Lucas critique induces a somnolence in policy-oriented economists. Since no model can live up to the impossible standard of being immune to all regime shifts, often little attention is paid to the general idea. Alogoskoufis and Smith have succinctly stated the impact of the Lucas critique on economists doing empirical work:

> [The Lucas critique] has largely been ignored by the majority of applied econometricians, who, after paying lip service to it, go on to do exactly what Lucas criticized. According to an influential recent survey of recent developments in

> macroeconomics, "that critique has not been shown to be of any empirical significance in accounting for the failures of econometric models . . ." (Stanley Fischer, 1988, p. 302). This type of reasoning may be one of the reasons why many econometricians go on ignoring it.
>
> (Alogoskoufis and Smith, 1991, p. 1254)

While the new classicals seem to have won the rhetorical war, the practice they abhor continues apace.[3]

The more pragmatic standard is not so unwieldy; all it asks is that fixed parameters be carefully considered. Had this practice been the rule rather than the exception, a lot of poor predictions of the effects of policy could have been avoided.

Moreover, this more pragmatic standard calls into question the entire approach of the Lucas critique. Lucas was arguing that models in which expectations of the processes governing stochastic variables were held fixed when the processes varied were improper. He argued that the correct process must be rigorously incorporated into an agent's decision process. However, it was not noted that in this argument Lucas is slipping into an extreme form of the rational expectations hypothesis. Such a modeling strategy implicitly assumes that all agents instantly know the true processes that govern all stochastic variables. This modeling strategy is immediately censored by our more pragmatic standard. If agents either do not know about the regime change or, if they do know, do not adjust their expectations immediately, it is improper to model them as if they did. In fact, it is every bit as improper to pretend agents' expectations don't change when they do as to pretend agents' expectations do change when they don't.

Part III

THE WALRASIAN TRADITION

5

WALRASIAN METHODOLOGY

WALRASIAN AND MARSHALLIAN TRADITIONS

Since the Lucas critique provides an inadequate justification for using a representative agent model in macroeconomics, we now turn our attention to the second rationale delineated in Chapter 3. The Walrasian tradition in general, and in particular its modern incarnation, the Arrow–Debreu general equilibrium model, are often invoked as a justification for the use of representative models.

At the turn of the century, Leon Walras and Alfred Marshall proposed radically different economic methodologies. This chapter and the next two explore these arguments. This methodological debate between Walras and Marshall is far from sterile. Much of current economic practice is grounded in one of these methodological bases and much of what passes for debate in economics is merely a difference of methodological framework. For example, in discussing modern macroeconomics, Mayer (1993c) distinguishes between formalist theory and empirical science theory; Mayer's division largely corresponds to the division between Walrasian and Marshallian methodology. A proper understanding of the difference between these methodologies would do much to lay bare the foundations of current economic debates.

Our ultimate aim is to see how the representative agent assumption fits into these different methodologies. We will begin with a brief overview of Walras' method.[1] We then explore the nature of assumptions in this methodology, focusing on the representative agent assumption in particular. We conclude by looking specifically at the Arrow–Debreu general equilibrium model. Since new classical economists see themselves working in the general equilibrium tradition, we want to examine how the representative agent fits into such models.

WALRASIAN METHODOLOGY

Walrasian methodology is the clear precursor to the Arrow–Debreu general equilibrium model. As we will see below, Walras advocated the mathematical derivation of a rigorous, complete model of the economy.

Walras divided the study of economics into three parts: pure, applied, and social. He described these parts as "the True, the Useful, and the Just" (quoted in Jaffe, 1956, p. 127). From that description, it isn't hard to see which was Walras' first love; he devoted his life to the study of "the True," most notably in his master-work, *Elements of Pure Economics*. For Walras, the heart of economic study is a study of the pure economy:

> The pure theory of economics ought to take over from experience certain type concepts, like those of exchange, supply, demand, market, capital, income, productive services and products. From these real-type concepts the pure science of economics should then abstract and define ideal-type concepts in terms of which it carries on its reasoning. The return to reality should not take place until the science is completed and then only with a view to practical applications. Thus in an ideal market we have ideal prices which stand in an exact relation to an ideal supply and demand. And so on.
>
> (Walras, 1926 [1954], p. 71)

That description of economics study is notable for several reasons, not least its similarity to the idealized Arrow–Debreu world. Also notable is the prominence of the "ideal" economy. Elsewhere Walras argued, "the absolute or rigorous perfection is the hallmark of science. We are now in the domain of science; and therefore, in this domain, we look for the absolute or perfection" (quoted in Jaffe, 1980, p. 530). The economy to be studied here is indeed a "pure" economy, the "absolute" or "perfect" economy.

The study of pure economics is thus not a study of real world institutions; it is not an empirical science. *Elements of Pure Economics* is remarkable for its high level of detailed mathematical derivations of a pure economy and its near total lack of references to any real world economy.

The primacy of the study of the pure economy does not mean that Walras was uninterested in the real world. In fact, he argued that the real world should be studied once "the science is complete" (Walras, 1926 [1954], p. 71). However, the turn to the real world is not some sort of check on the accuracy of the pure model just completed. Rather, the real world is referenced in "applied economics," which is exactly what the name suggests: the pure theory applied to the real world. "Pure theory is the guiding light for applied theory. . . . When we have traced out the plan of a normal organization of production and distribution, we shall see clearly where the actual organization is satisfactory and where it is defective and must be modified" (quoted in Hutchison, 1953, p. 211). Pure theory, being "True," is a useful benchmark for the real world; a lack of correspondence between the pure theory and the real world indicates defects not in the theory, but in the world.

There is no independent check on the model's conclusions. Walras justified this approach by appealing to geometry:

> Everyone who has studied geometry at all knows perfectly well that only in an abstract, ideal circumference are the radii all equal to each other and that only in an abstract, ideal triangle is the sum of the angles equal to the sum of two right angles. Reality confirms these definitions and demonstrations only approximately, and yet reality admits of a very wide and fruitful application of these propositions.
>
> (Walras, 1926 [1954], p. 71)

If, in the real world, one does not find circles with equal radii, one does not alter the theory of the circle. So it is to be with economics. Economists should seek to construct the "true" model of the economy. Walrasian models are thus not subject to our battery of econometric tests. It is impossible to empirically verify or deny the conclusions or the accuracy of the model; they are simply held to be true.

The severity of this break between reality and economic theory is illustrated by a mistake Walras made in later editions of *Elements.* Milton Friedman (1955) has noted that as Walras progressed in the development of his model through successive editions of *Elements,* he (Walras) lost sight of the real world counterparts of the concepts in his model. In the first three editions of *Elements,* Walras carefully distinguished savings (flows) from consumption goods (stocks). However, in the fourth edition, Walras suddenly treats savings as merely another good. Friedman argues:

> Surely, the explanation must be that when Walras made the change in the fourth edition, he no longer had the system and its meaning in his bones the way he did when he developed it; he was taken in by considerations of pure form; the substance which the form was to represent was no longer a part of him. It would be hard to find a better example of the nonsense to which even a great economist can be led by the divorce of form from substance.
>
> (Friedman, 1955, p. 908)

This error is indeed illustrative. By the fourth edition, Walras was clearly studying his model of the economy, rather than the economy itself.

Walras clearly saw a distinction between the type of economics he was propounding and an empirically based economics. Nowhere is his disdain for facts better seen than in the following passage:

> I am an idealist. I believe that ideas reshape the world after their own image and that the ideal a man conceives for his century commands the attention of all humanity. . . . In this respect, I am swimming against the current of my century. Facts are now in fashion: the observation of facts, the investigation of facts, the acceptance of facts as laws. In stormy times, political power falls into the hands of the ignorant masses. Art, science, philosophy are swept away. Facts become masters; empiricism triumphant reigns supreme. Analytical minds closely study the explosion and wait for chaos

> gradually to take over as an object of fond description and serene glorification. As for me, I will have no part in this. . . . I take comfort in my ideal – it is my refuge against the avalanche of brute facts; and if my century crushes me, as the universe Pascal's reed, at least I shall have been spared being part of the century.
>
> (Quoted in Jaffe, 1980, pp. 532–3n)

Walras was thus seeking an ideal; he sought to construct the "pure" or "True" economy. In the end, he believed he was successful. "I am a man of science: I have sought the truth; I believe I have found it; I am recompensed" (quoted in Jaffe, 1956, p. 123).

THE ROLE OF MATHEMATICS

We can gain further insight into Walras' ideas about what constitutes good economics by examining his thoughts on the role of mathematics in economics. Moreover, this discussion will help illuminate the great differences between Walrasian and Marshallian methodologies. These two authors had radically different conceptions about the proper use of mathematics, and this is apparent from even a cursory glance at their writings. This difference is perhaps best enunciated in a letter Marshall wrote to Walras in 1889 (reprinted here in its entirety):

> I have to thank you very heartily for your new edition of *Elements d'Econ. Pol.* I have not myself retired from the conclusion that I think I communicated to you some time ago, viz that the right place for mathematics in a treatise on Economics is the background. But I think it is most desirable that different seekers of truth should take different routes; I rejoice much that the pure mathematical route is being developed by your great ability and energy.
>
> (Quoted in Jaffe, 1965, vol. II, p. 355)

Walras never responded to this letter; in fact, no further correspondence occurred between the two. This was surely due in part to the fact that "such was L.W.'s [Leon Walras'] passion for the application of mathematics to economics that he could never brook the slightest qualifying observation on this supreme object of his affections" (Jaffe, 1965, vol. I, p. 531). (Walras would later refer to Marshall as "that great white elephant of political economy": quoted in Jaffe, 1971, p. 272.)

As should be obvious by now, Walras held the use of mathematics in high esteem. He thought economics was essentially a mathematical topic: "Value in exchange is thus a magnitude, which, as we now see, is measurable. If the object of mathematics is to study magnitudes of this kind, the theory of value in exchange is really a branch of mathematics which mathematicians have hitherto neglected and left undeveloped" (Walras, 1926 [1954], p. 70). On this basic building block, Walras constructs his entire system.

Thus, for Walras, economics is mathematics. Mathematics is "a tool that is not merely useful, but indispensable" (ibid., p. 206). It is foolish to try to deal with economics in a nonmathematical way. In fact, it is impossible to do so. Any attempt at a nonmathematical economic treatise is merely an attempt to disguise the vital role mathematics is inherently playing in the background.

Walras' love affair with mathematics was intense, so intense that it gave even his mathematically oriented contemporaries reason to pause. During Walras' life, Edgeworth was also doing prominent work in mathematical economics. In 1889, he reviewed *Elements:*

> Though Edgeworth agreed with L.W. [Leon Walras] "in his plea for the use of mathematical reasoning in economics," he was afraid that L.W. had prejudiced the case by his advocacy, because of his "use of symbols in excess of the modest requirements of elementary mathematical reasoning," and because of his rash applications of the mathematical method to questions of policy. . . . [Edgeworth went on to add,] "His scheme of dosing the circulation by a nicely calculated injection of supplementary currency reminds us of the tailors in Swift's Laputa, who went through laborious mathematical computations in order to determine the measurements of a suit of clothes, which after all fitted very ill."
>
> (Jaffe, 1965, vol. II, pp. 339–40, citing Edgeworth, 1889)

Walras was undeterred by such criticism. He consistently viewed mathematics and economics as integrally intertwined:

> We count to-day I do not know how many schools of political economy. . . . For me, I recognize but two: the school of those who do not demonstrate, and the school, which I hope to see founded, of those who demonstrate their conclusions. It is in demonstrating rigorously the elementary theorems of geometry and algebra, then the theorems of the calculus and mechanics which result from them, in order to apply them to experimental ideas, that we realize the marvels of modern industry.
>
> (Walras, 1892, p. 54)

In short, without mathematics there is no economics.

Walras' emphasis on the need for mathematics is integrally related to his general methodological prescriptions. In order to build up a pure model of the economy, every step of the process must be rigorously derived and correct. Long chains of verbal reasoning are horribly prone to admit errors both small and large. Long chains of mathematical reasoning are relatively simple to check for complete accuracy. Starting from a simple proposition, one can work forever in the realm of mathematical reasoning and still ensure that the conclusion rigorously and correctly follows from the premises. There is no room for debate in this realm; either your mathematics were done correctly or they were not. The model derived is not open to criticism; we know the model is correct because it was correctly derived. In what sense can such a mathematical derivation be empirically verified? How do you use econometrics to see if two plus two really does equal four?

ASSUMPTIONS IN WALRASIAN METHODOLOGY

Our attention now turns to the role of assumptions in the Walrasian model. In a Walrasian model, there are two types of assumptions that can be made. First, there are assumptions about the true structure of the economy. In order to build a mathematical model of the pure economy, we must start somewhere. Let us call these assumptions on which the model will be built the structural assumptions.

For example, in *Elements,* Walras begins with value in exchange. He assumes that the value at which one good exchanges for another reflects their relative worth. Since the relative value of two goods is a mathematical expression, Walras then proceeds to build mathematically upon this foundation.

It is crucial that structural assumptions be accurate. False assumptions of this type will lead to false models of the economy. In this case, a test of the assumptions is in some sense a test of the model. If an assumption on which a model has been built is wrong, then we know the model is wrong. The problem here is that the logical processes we use to build our models are very exacting. If we start from a false premise, logically we will follow it to conclusions which cannot be assumed to be true. Take a simple example. Suppose that the true value of x is 1. Now, when I go to build my model I make the assumption that x is 2. From this assumption, I know that x^2 is 4 and x^3 is 8. My conclusions are, of course, wrong; both x^2 and x^3 are really 1. The problem with my model is not in the analysis. The analysis rigorously follows the canons of logic; assuming x to be 2, the conclusions would be correct. The problem with the model is that the assumption is wrong.

This argument that structural assumptions must be accurate if they are to be useful seems to be in contradiction to Friedman's (1953b) famous dictum that the realism of assumptions is irrelevant and, even more remarkably, the more unrealistic the assumptions of a model, the better. However, as we will see in the next chapter, Friedman was using Marshallian methodology, and thus this prescription is only meaningful when using that methodology. The accuracy (or lack thereof) of Friedman's statement when using Marshallian methodology has no bearing on its accuracy when using the completely different Walrasian methodology.

There is a second type of assumption in the Walrasian framework. There is in the real world a large number of complex institutions 'which are irrelevant to the underlying economic structure. The fundamental economic structure would be unaltered if these institutions either did not exist or existed in a different form. Thus, as the Walrasian goal is a model of the underlying structure, a researcher may make assumptions about these institutions. In some cases it may be desirable to assume they simply don't exist, while in others it may be desirable to assume they exist in a different form.

Let us call this second type of assumption a superficial assumption. The important aspect about superficial assumptions is that they are exactly what the name says, superficial. It is necessary that the real world institutions being modified by these assumptions are truly irrelevant to the underlying structure of the economy.

An example of a superficial assumption is found in Walras' model. Walras developed a model in which the economy is essentially a barter system and all transactions take place at market clearing levels. Recall that one of his structural assumptions was that the value of exchange between two goods reflects their relative worth. Since goods are assumed to exchange at their relative worth, it does not matter how these exchange rates are determined; all that matters is that the exchange rates exist and that trades take place at those rates. Whether prices are set by actual competitive forces or by an auctioneer is irrelevant. Thus, Walras does not bother with elaborate explanations of how these exchange values come to be determined; the mechanism is irrelevant. *If* Walras' structural assumption that goods exchange at values reflecting their relative worth is true, then his assumption that the precise mechanism by which these values are determined is not terribly important is superficial.

The distinction between structural and superficial assumptions is whether or not they must be true, i.e., whether or not they must match reality. As we have seen, structural assumptions must be in accord with reality, or the model will be based on false premises. However, superficial assumptions will never be true; by their very nature they are assuming that certain real world institutions are not the way they are.

Assuming away messy complications enables a researcher to get to the heart of the economy; assumptions of this type will always be untrue. However, once a researcher is examining the structure of the economy, it is no longer permissible to make false assumptions for the reasons enunciated above. In short, it is permissible to assume away irrelevant complications, but it is not permissible to make false assumptions about the underlying structure of the economy.

Creating a good Walrasian model is thus very difficult. Every assumption made must be classified as either superficial or structural. This is not an easy distinction to make, as it involves determining whether the matter is crucial or not.

Even if this distinction between superficial and structural assumptions can be made, all the economist's problems are not solved. While superficial assumptions need not be given more thought, structural assumptions are another matter. Structural assumptions must be accurate, but there is no easy means by which a structural assumption can be shown to be accurate. There are and can be no tests to determine the accuracy of a Walrasian model. This is not an irrelevant detail. If incorrect structural assumptions are made, then the model built upon them is of little value. No matter how much work is spent building up a rigorous model on the false structural assumption, it all comes to naught.

THE REPRESENTATIVE AGENT ASSUMPTION

We now wish to examine the relation between Walrasian methodology and the representative agent assumption. We want to examine if the representative agent assumption is ever justified in a Walrasian setting, and if it is, under what circumstances it can properly be used.

If the representative agent assumption is to be used it must be classified as either a structural or a superficial assumption. We will take up each of these possibilities in turn.

It is immediately obvious that the representative agent assumption cannot be considered structural. The fundamental feature of structural assumptions is that they are true. A Walrasian model that is based on a structural assumption which is not, to use Friedman's (1949, p. 91) phrase, a "photographic description of reality" cannot be relied upon. Now, we know that the real world is not populated by representative agents; there is undeniably heterogeneity among both real people and real firms.

Thus, if the representative agent assumption is to be used in a Walrasian framework, it must be considered a superficial assumption. The propriety of using the representative agent assumption in this manner is not as clear-cut as in the former case.

Whether or not the representative agent assumption is usable in a Walrasian framework depends solely on the answer to this question: Is heterogeneity among people and firms an irrelevant, complicating factor or is it an essential part of the real economy? The use of a representative agent in a Walrasian framework is imposing the very powerful assumption that the important features of the economy are unaffected by the fact that real people and firms differ from one another. If this assumption is true, then there is no problem with using the representative agent hypothesis. However, if it is not true, then using a representative agent in a Walrasian model is unjustified. The results from a Walrasian model improperly using the representative agent assumption are valueless.

Now, we know that in the actual world people are different. If we believe that the representative agent assumption is useful in a Walrasian model, we must also believe that our actual economy would not look very different than a world composed of clones or identical robots. However, few economists, and few other people for that matter, believe that heterogeneity among people is completely irrelevant. There are very good reasons for this belief; as we will see in Chapters 10 and 12, heterogeneity plays a very large role in most of modern economics. As a quick example, we need only think of the increasing prominence of models of asymmetric information to see this point.

Given that few, if any, economists truly believe heterogeneity is irrelevant, why is the representative agent employed in Walrasian models? As we saw in Chapter 3, the point of using the representative agent assumption was to make it simpler to develop Walrasian models.

> Rather than carrying along the number of firms and the number of households as additional parameters, which is a nuisance, we use the standard device of "representative" agents. The substantive aspect of this device is to build in the assumption that all firms are alike and all households are alike, while technically it serves to eliminate the need to carry along the numbers of each kind of unit.
>
> (Sargent, 1979, p. 371)

It is simply that keeping heterogeneous agents in the model is a "nuisance" and thus it is nice to "eliminate the need" to keep track of all those different people. Eliminating this "nuisance" makes models much easier to develop and solve. "The advantage of this approach is that algorithms for computing solutions to concave programming problems can be used to find the competitive equilibrium allocation for this economy" (Prescott, 1986, p. 12).

But why do we want easily developed Walrasian models? If the whole point of a Walrasian model is to develop a pure model of the true economy, of what value is it that the model is *simple to develop*? Simplicity may be a desirable feature of an explanation of economic phenomena, but it is not necessarily a desirable feature of the means of developing a structure of the underlying economy. While we want developed models to be simple, it does not follow that we want models that are simple to develop.

Indeed, the whole notion of trying to find means of easily developing Walrasian models is reminiscent of a bargain sale with hawkers proclaiming, "Science for cheap!" This "bargain science" is a curious notion. Consider its application in a field like cartography. It would be much simpler to develop maps if the world could be assumed to be flat. Easier, yes; correct, no. For the fact remains that the world is not flat, no matter how much we may wish it were so. As with cartography, so with economics.

Thus, when it comes to the Walrasian goal of a complete model of the underlying, true structure of the economy, simplicity is not the dominant concern. The whole notion that if Walrasian models were easier to develop, the world would be a better place is fallacious. Walrasian models are of value in proportion to their accuracy, not their ease of creation.

THE FUNDAMENTAL THEOREMS?

There is another way in which the representative agent model seems amenable to Walrasian methodology. Walrasian models are generally models of a competitive equilibrium. Instead of modeling a full-scale competitive economy (which is hard), we might prefer to exploit the fundamental theorems of welfare economics to get around the problem. If we can find a Pareto optimum, then we know that that allocation can be supported as a competitive equilibrium. To find a Pareto optimum, we can solve out a fictitious social-planner problem. What kind of social planner should we choose? Well, why not have the social planner maximize the welfare of the representative agent? Hansen and Sargent (1990) succinctly explain:

> We use a standard method of computing a competitive equilibrium by solving a Pareto or fictitious social planning problem, a method that was used for this type of model by Lucas and Prescott [1971]. It can be verified that the *aggregate quantities* that solve the Pareto problem are the aggregate competitive equilibrium quantities. Also, the *value function* along with the optimal law of motion for the Pareto problem determine the competitive equilibrium *price system.*
>
> (Hansen and Sargent, 1990, p. 7)

Hansen and Sargent (forthcoming) go step by step through the process by which the social-planner problem is interpreted as a decentralized competitive equilibrium. First, they solve out the optimal resource allocation problem by assuming there is a social planner maximizing the utility of the representative household subject to resource constraints. Then they find the set of prices for which the social-planning problem can be supported by a competitive equilibrium. Thus, we have a representative agent whose utility is being maximized as a social planner's problem which yields a solution that is Pareto optimal, and finally, by adding in prices, we arrive at a competitive equilibrium.

Note that this line of reasoning is a very different method of justifying the use of the representative agent in a Walrasian model than the one we discussed in the previous section. Earlier, we examined whether it made sense to simply assume that heterogeneity among agents was irrelevant. Now, we seem to bypass the need for that assumption; by finding a Pareto optimum, we know we have arrived at a competitive equilibrium. On the surface, it seems as if we have never imposed any condition on heterogeneity or lack thereof among consumers. As long as we posit that the social planner maximizes the utility of the "representative" agent, then we can get a competitive equilibrium.

However, there is many a slip 'twixt the cup and the lip. First, there is a very large difference between the statement "the Pareto optimum can be supported by a competitive equilibrium" and the statement "the Pareto optimum can be supported by a competitive equilibrium that I find interesting and somehow related to the actual economy." There are googols of hypothetical competitive equilibriums out there; is the one found by a representative agent problem interesting?

After showing that the social-planning problem derived by maximizing the utility of the representative agent can be supported by a competitive equilibrium, Hansen and Sargent go on to examine explicitly how to turn the representative agent economy into an economy with heterogeneous consumers:

> We build on some of the ideas of Gorman (1953), who provided necessary and sufficient conditions for a heterogeneous agent economy to aggregate to an "equivalent" *representative consumer* economy. We study a class of heterogeneous agent economies that satisfy Gorman's conditions for aggregation, and for which the Gorman style equivalent representative consumer economy is identical to the single consumer economy that we have been studying in this book.
>
> (Hansen and Sargent, forthcoming, p. 171)

The Gorman conditions will be discussed with aggregation below (Chapter 10), so for now we will just look at what kinds of agent this representative agent economy allows for.

The Hansen and Sargent model actually allows for only very limited heterogeneity. Agents can have different initial endowments and different levels of utility at which "bliss" is achieved. That's it; every single other aspect of these "heterogeneous" consumers is identical. (Why further heterogeneity cannot be incorporated into the model is discussed further in Chapter 10.)

Now suppose the economy we think is interesting has consumers that are a bit more heterogeneous than the Hansen and Sargent model allows. What do we get when we use the Hansen and Sargent model? The social planner is maximizing the welfare of a representative agent who is not representative. We thus get a Pareto optimum for the representative agent who is not representative. We can then support *a* competitive equilibrium, but there is no reason to assume that this competitive equilibrium would be the one churned out in a model with the heterogeneous agents we think are interesting. In fact, using the representative agent to get a competitive equilibrium in this case is equivalent to picking a Pareto optimum at random and saying that the competitive equilibrium supported by this random Pareto optimum is interesting.

Similarly, in the Hansen and Sargent framework, the fact that there is even limited heterogeneity among consumers is irrelevant to calculating the aggregate quantities. Using the Hansen and Sargent framework, you get exactly the same aggregate outcome if all of the initial resources are held by one agent as you would if the initial endowments were equally dispersed throughout the population or by any other possible distribution scheme. Thus, if we believe that heterogeneity is important for aggregate outcomes, that a different set of initial endowments will result in a different aggregate outcome, then even the limited heterogeneity allowed in the Hansen and Sargent model is not terribly interesting.

Thus, this alternative way of using the representative agent to generate Walrasian-style competitive equilibriums does not bypass the need for assuming that there is no interesting and relevant heterogeneity among agents. Instead, this argument merely hides the need for that assumption.

However, even if we are willing to ignore these sorts of problems, we run into yet another problem here. The fundamental theorems of welfare economics are razor-edged results. Only under certain conditions can we say that a Pareto optimum can be supported as a competitive equilibrium. For example, we need to assume that all agents are price takers and there are no externalities. If we think that interesting economies have some agents with market power or some externalities, then once again the equilibrium churned out by maximizing the welfare of the representative agent is *an* equilibrium, but not necessarily an interesting equilibrium. As Hahn and Solow (1995, p. 2) recently argued, "It is true that an Arrow–Debreu equilibrium is an allocation that maximizes a special social welfare function, but that is not the case, for instance, when some insurance markets are absent, or indeed when any even mildly realistic phenomena are included." The Hahn and Solow book is an attempt to bring some of these "realistic phenomena" back into macroeconomic discourse. It is interesting that they felt compelled to drop the representative agent model in introducing these phenomena:

> For instance, we cannot adopt the "representative agent" approach that simply assumes the model economy to solve and carry out the infinite-time optimization problem of a single, immortal, foresighted worker-owner-consumer. That approach cannot seriously be said to *conclude* that economic fluctuations are nonpathological, because it has already assumed just that. Because we want to preserve at least the option of concluding that the economy may behave in a deplorable way, even if wages and prices are flexible, we have to choose some other line of argument.
>
> (Hahn and Solow, 1995, p. 10)

Similarly, Greenwald and Stiglitz (1986) give a striking litany of the problems inherent in the line of reasoning that goes from finding a Pareto optimum to knowing how the actual economy works:

> The paper thus casts a new light on the First Fundamental Theorem of Welfare Economics asserting the Pareto efficiency of competitive equilibrium. The theorem is an achievement because it identifies what in retrospect has turned out to be the singular set of circumstances under which the economy is Pareto efficient. There is not a complete set of markets; information is imperfect; the commodities sold in any

> market are not homogenous in all relevant respects; it is costly to ascertain differences among the items; individuals do not get paid on a piece rate basis; and there is an element of insurance (implicit or explicit) in almost all contractual arrangements, in labor, capital, and product markets. In virtually all markets there are important instances of signaling and screening. Individuals must search for the commodities that they wish to purchase, firms must search for the workers who they wish to hire, and workers must search for the firm for which they wish to work. We frequently arrive at a store only to find that it is out of inventory; or at other times we arrive to find a queue waiting to be served. Each of these are "small" instances, but their cumulative effects may indeed be large.
>
> (Greenwald and Stiglitz, 1986, pp. 259–60)

In this paper, Greenwald and Stiglitz work through a model showing that the competitive equilibrium in the economy is not Pareto optimal, and that there is a government policy that can make Pareto improvements.

This "problem" of an inability to incorporate things like externalities into the Pareto-optimum–competitive-equilibrium framework seriously restricts the usefulness of Walrasian models of this type. There is no reason to expect a Walrasian model to mimic the performance of the actual economy. Walras was aware of this, which is why he saw himself as creating models of "pure" economies to which the real economy could be compared. The problem only arises when we try to depict the Walrasian model as a picture of the actual economy. The representative agent model has been portrayed in recent years as a real description of actual economies. Such a portrayal is not in keeping with the rationale for building Walrasian models.

Ignoring these problems, however, let us consider the question of whether the representative agent model is actually getting us a Pareto optimum. It is trivial to note that if the utility of the representative agent is maximized, then there is no better allocation for the representative agent. Can we then assume that if we use policy to make the representative agent better off, we are making the agents being represented better off? No. Jerison (1990) showed it is possible to have a situation in which the representative agent prefers situation A to situation B, but every single agent in the economy prefers situation B to situation A. (Kirman, 1992, provides a nice, simple illustration of Jerison's result.)

Jerison's result provides another serious kink in the chain of reasoning that leads from the representative agent model as a social-planner problem to a competitive equilibrium. When we maximize the utility of the representative agent, we are assuming that we are at a Pareto optimum, and thus at an allocation which the agents being represented will arrive at in competitive trading. However, if the allocation is not a Pareto optimum for the agents, then the actual agents could do better. If, on the other hand, we look for a situation in which the actual agents are at a Pareto optimum, then it is possible that this allocation will not provide the maximum utility to the representative agent. Which Pareto optimum is more interesting: a Pareto optimum of the actual agents in the economy or a Pareto optimum for a constructed "representative" agent? If the former (surely the former?), then the representative agent framework may be of little value in calculating Pareto optima and hence in calculating competitive equilibria.

In the end, the case for using the representative agent assumption in a Walrasian framework is considerably weakened. If we think heterogeneity does not matter, then at best, using a representative agent model allows us to achieve the Walrasian ideal of a "pure" economy. We can use such a model as a point of comparison for the actual economy, but there is no justification for assuming that the actual economy looks anything like this "pure," representative agent economy. However, if we believe that heterogeneity is not irrelevant, then the representative agent cannot legitimately be used in a Walrasian framework.

6

MARSHALLIAN METHODOLOGY

INTRODUCTION

This chapter presents an exploration of Marshallian methodology. Its ultimate aim is to discuss how the representative agent assumption fits into economic inquiry. As in the chapter on Walrasian methodology, we begin with a brief exploration of the general framework of Marshallian method.

The most defining statement of Marshall's methodology is found in Appendix D of *Principles of Economics*. The general plan is as follows:

> Induction, aided by analysis and deduction, brings together appropriate classes of facts, arranges them, analyses them and infers from them general statements or laws. Then for a while deduction plays the chief role: it brings some of these generalizations into association with one another, works from them tentatively to new and broader generalizations or laws and then calls on induction again to do the main share of the work in collecting, sifting and arranging these facts so as to test and "verify" the new law.
>
> (Marshall, 1920 [1961], vol. I, p. 781)

It is the combination of induction and deduction that Marshall emphasizes; quoting Schmoller, he notes, "'Induction and deduction are both needed for scientific thought as the left and right feet are both needed for walking'" (ibid., p. 29).

The starting place in Marshallian methodology is thus a close look at real world economic institutions. The wealth of factual evidence in *Principles* stands in sharp contrast to the dearth of such evidence in *Elements of Pure Economics*: "Marshall's familiarity with the factual aspects of business and industry, his concreteness in factual discussions, his wealth of detail, are nothing short of astounding. Rarely does he offer a generalization that is not profusely and illuminatingly illustrated in the descriptive-factual field" (Davenport, 1935, p. 4).

Having gathered facts about the real economy, economists can turn their attention to figuring out how to interpret them: "[T]he true analytical study of economics is the search for ideas latent in the facts which have been thus brought together and arranged by the historian and the observer of contemporary life" (Marshall, 1897, p. 133). Similarly, deduction is described as the "laborious plan of interrogating facts in order to learn the manner of action of causes singly and in combination" (Marshall, 1885, p. 171).

However, Marshall cautions against letting the deductive reasoning process take us too far: "The function then of analysis and deduction in economics is not to forge a few long chains of reasoning, but to forge rightly many short chains and single connecting links" (Marshall, 1920 [1961], vol. I, p. 773). In any long chain of reasoning, there is a multitude of places to go astray. By keeping the chains of reasoning short, one can test a theory against reality at each small step. This prevents inquiry from wandering too far from the truth. There is thus no room in Marshall's methodology for Walras' book-long deductive exercise.

Finally, the product of the analytical process is compared to empirical evidence. The results need to be "verified" by comparing them to the real world. When a newly worked out theory conflicts with empirical evidence, it is the theory that is presumed to be in error.[1]

While Walras disparaged "brute facts," Marshall at times seems to idolatrize them. Nevertheless, Marshall finds both ideas and facts to be vital to economics. It is in the phases of induction that an economist must use facts, while in the deductive phase ideas are crucial. Just as there is a need for both induction and deduction, economists must use both ideas and facts:

> It seems strange to me to be asked my views as to the study of pure economic theory; as tho' that were a subject on [which] I were fit to speak. . . . The fact is I am the dull mean man, who holds Economics to be an organic whole, and has little respect for pure theory (otherwise than as a branch of mathematics or the science of numbers), as for that crude collection and interpretation of facts without the aid of high analysis which sometimes claims to be a part of economic history.
>
> (Quoted in Coats, 1967, p. 133)

Nevertheless, there is little doubt that in any conflict between an idea and a fact, Marshall's sympathies are naturally inclined toward the latter. Theory is necessary because "Facts by themselves are silent" (Marshall, 1885, p. 166). But theory is more akin to a toy than to a tool:

> If we shut our eyes to realities we may construct an edifice of pure crystal by imaginations, that will throw side lights on real problems; and might conceivably be of interest to beings who had no economic problems at all like our own. Such playful

> excursions are often suggestive in unexpected ways: they afford good training to the mind: and seem to be productive only of good, so long as their purpose is clearly understood.
>
> (Marshall, 1920 [1961], vol. I, p. 782)

Theorizing may enable economists to set up toy economies which may shed light on real world problems. Marshall succinctly summarized his notions of the usefulness of theory in a letter to Edgeworth, which just as easily could have been sent to Walras: "In my view 'Theory' is essential. . . . But I conceive of no more calamitous notion than that abstract, or general, or 'theoretical' economics was economics proper: and by itself sometimes even – well, not a very good occupation of time" (quoted in Pigou, 1956, p. 437). Marshall is concerned that theory could drift away from the factual events it seeks to explain. This is the reason he limits the deductive process to "short chains and single connecting links."

In a similar manner, Marshallian methodology is sharply distinguished from Walrasian methodology by the agents it studies. Walras looked for ideal counterparts to actual people; Marshall strove to "deal with man as he is: not with an abstract or 'economic' man; but a man of flesh and blood" (Marshall, 1920 [1961], vol. I, pp. 26–7). Walras' discursion into the realm of the ideal has no place in Marshallian methodology. In fact, such pure theoretical diversions are of little use:

> [T]he direct and formal study of facts, perhaps mainly those of his own age, will much exceed the study of mere analysis and "theory" in its demands on the time of any serious economist; even though he may be one of those who rank most highly the importance of ideas relative to facts.
>
> (Marshall, 1920 [1961], vol. I, p. 778)

Note specifically the use of the word "serious" in the above quotation. If economists are to do anything of value, they clearly must deal with real institutions.

MATHEMATICS IN MARSHALLIAN METHODOLOGY

As with Walrasian methodology, we can gain much insight into Marshallian methodology by examining Marshall's remarks on the use of mathematics in economic study. We also vividly see the contrast between Marshallian and Walrasian methodologies.

Marshall was much more temperate in his use of mathematics than was Walras. He viewed mathematics as an extremely useful tool: "But a training in mathematics is helpful by giving command over a marvelously terse and exact language for expressing clearly some general relations and some short processes of economic reasoning; which can indeed be expressed in ordinary language, but not with equal sharpness of outline" (Marshall, 1920 [1961], vol. I, p. 781).

The usefulness of mathematics is limited, however. There are only certain areas of economics where it can be used; there are others where it should never be used. The problem is that only certain economic concepts are easily expressed in mathematical terms while many are not:

> For many important considerations, especially those connected with the manifold influences of the element of time, do not lend themselves easily to mathematical expression: they must either be omitted altogether, or clipped and pruned till they resemble the conventional birds and animals of decorative art. And hence arises a tendency toward assigning wrong proportions to economic forces; those elements being most emphasized which lend themselves most easily to analytic methods.
>
> (Marshall, 1920 [1961], vol. I, p. 850)

Thus, it makes no sense to restrict oneself to using mathematics alone in expressing economic ideas; such a practice would only warp economic theory.

Marshall was always painfully careful to avoid abusing mathematics. In *Principles* the mathematics was confined to a mathematical appendix. Marshall consistently believed that ideas easily expressed in words should be so expressed. Mathematics was only a tool:

> But I know I had a growing feeling in the later years of my work at the subject that a good mathematical theorem dealing with economic hypotheses was very unlikely to be good economics: and I went more and more on the rules – (1) Use mathematics as a shorthand language, rather than as an engine of inquiry. (2) Keep them till you have done. (3) Translate into English. (4) Then illustrate by examples that are important in real life. (5) Burn the Mathematics. (6) If you can't succeed in 4, burn 3. This last I often did.
>
> (Quoted in Pigou, 1956, p. 427)

Thus, while mathematics may be a powerful tool, enabling economists to work through complex issues, it is still necessary to translate mathematical derivations into words. Mathematics should neither be used excessively nor avoided completely: "The real question is not whether it is *possible,* but whether it is *profitable* to apply mathematical reasonings in the moral sciences. And this is a question which cannot be answered *a priori;* it can be answered only from the experience of those who make the attempt" (quoted in Whitaker, 1975, p. 266).

Marshall's comments on mathematics should not be lightly dismissed as the rantings of an economist unable to cope with the increasing mathematization of economics in his time. Pigou (1953) makes this point forcefully:

> And it is essential to remember here that Marshall was himself a first-class mathematician, second wrangler when Lord Raleigh was senior. Objections from people innocent of mathematics are like objections to Chinese literature by people who cannot read Chinese, and are not worth listening to. But objections from Marshall are in an entirely different class and deserve a most careful and respected hearing.
>
> (Pigou, 1953, p. 7)

Marshall's remarks are thus not a simple diatribe against increasing rigor in economic reasoning. Rather, he is arguing that in order to be rigorous, economists must not focus exclusively or even extensively on mathematical derivations. Marshall simply feared that a heavy reliance on mathematics would result in an economic theory that ignored much of what was important.

The limited use of mathematics thus fits in very well with Marshallian methodology. It can be an extremely useful tool during the stage of deduction when new theories are being worked out. However, getting the mathematical solution is insufficient; afterwards, the mathematics must be translated into English and tested to see if they are correct.

THE ROLE OF ASSUMPTIONS

The role of simplifying assumptions in a Marshallian framework is rather straightforward. We begin economic inquiry by partitioning off a portion of the economy because the totality of the economy is extraordinarily complex. We will generally run into the same problems of complexity when we turn to a study of a portion of the economy. So we simplify our subject further via assumption.

There is obviously a difference between good and bad assumptions. Simply put, a bad assumption is one which causes the researcher to reach an incorrect conclusion. A simplifying assumption that does not induce wrong conclusions is good, since it both makes the deductive part of economic research easier and may thereby allow for further insights into the workings of the economy.

For example, if I want to examine the effects of US government economic policy during the incipient stages of the Great Depression, it may not matter that the presidency changed hands in 1933. So I could safely ignore the existence of a new President and assume a constant dictator. If the effects of government policy were truly not affected by Roosevelt's assumption of the presidency, then I have made a good assumption. However, if I want to examine how the economic policies of Republican Presidents differ from those of Democratic Presidents, the change in presidency in 1933 is not an irrelevant datum. Assuming a constant dictator in this case would induce a false conclusion and is thus a bad assumption.

Thus, the use of a simplifying assumption changes with the subject matter. A researcher must evaluate the usefulness of a particular simplifying assumption in a particular case since not all simplifying assumptions are appropriate for all cases. Simplifying assumptions can be used if they allow a researcher to bypass irrelevant details and get to the heart of the matter but do not corrupt the analysis. We measure the conclusions gained from a model using simplifying assumptions by their usefulness. Do the conclusions reached from a simplified model improve our insights into the workings of the real economy as measured by our predictive ability? If they do, then the simplified model can be profitably used.

Because of the importance of testing in the Marshallian framework, the danger of incorporating bad assumptions in the analysis is mitigated. If a researcher makes a bad assumption, the implications from the analysis will be flawed. When the predictions from the model are not empirically verified, the researcher will have to revisit the model to discover what is wrong. This process of testing and revision will eventually root out the bad assumption.

This role of simplifying assumptions is also part of the reason for arguing against long chains of deductive reasoning. If the deductive part of research gets too complicated and the model's implications are empirically invalidated, then there are a vast number of possible errors in the analysis. By reducing the deductive leap from one set of empirical generalizations to the next, it is much easier to determine which simplifying assumptions may be leading a researcher astray.

This process is the heart of the Marshallian method. Before we look at the role of the representative agent, it is worthwhile to clear up some common misperceptions. The process of separating out a portion of the economy for analysis is often derisively referred to as partial equilibrium analysis. The next section explores this characterization. Then we turn to an examination of Friedman's methodological statements. These statements probably have been discussed more times than any other methodological statements in economics. We can gain a key insight into both Friedman's statements and Marshallian methodology when we recognize that they are, in fact, the same.

PARTIAL EQUILIBRIUM

Marshallian methodology is often characterized as partial equilibrium analysis. We need to consider both what exactly is meant by this assertion and whether it is true.

As it is used today, the term "partial equilibrium" is rather pejorative. The foremost characteristic of partial equilibrium models is believed to be that they leave crucial parts of the economy out of the analysis. A model of the market for oranges in which other fruits are not mentioned is the image often conjured up by the words "partial equilibrium." Clearly, the argument would run, partial equilibrium is vastly inferior to the more encompassing general equilibrium analysis.

Now, this characterization of partial equilibrium analysis may or may not be deserving of such condescension, but it is certainly not an accurate portrayal of Marshallian methodology. The assertion that a Marshallian research strategy ignores important parts of the economy is false. The ultimate goal of a Marshallian research strategy is to understand all the interconnections among variables in the economy. Research which ignores truly important aspects of the real economy is flawed, since it will necessarily lead to wrong conclusions.

However, Marshallian methodology argues that it is impossible to study all the important relationships in the economy at once. So its method is to segregate out small parts of the economy and study them in isolation. This is where the rap on partial equilibrium finds its smoking gun. But the rap ignores what comes after this stage. A proper application of Marshallian methodology then puts the isolated portion of the economy back into the whole. The interconnections between this now more fully understood portion and the balance of the economy should be completely studied. Once the part is put back, the whole of the economic puzzle should again be studied to see where further refinement is needed.

The fact that Marshallian methodology is distinct from the pejorative characterization of partial equilibrium can be seen clearly with our simple example. Consider the previously mentioned study of the orange market in which the price of other fruit is ignored. Would Marshallian methodology find such a study satisfactory? Clearly, it would not. An analysis which completely ignores other goods would yield wrong predictions about the economy, and thus be rejected. However, does this mean that a study of the market for oranges alone is without value? Clearly not. For if we are ever to fully understand the market for oranges, we need to know a great deal about both the demand for and supply of oranges and the demand for and supply of other pieces of fruit. However, it would be far too complex to study simultaneously both the market for oranges and the market for every other piece of fruit. So we can study the market for oranges alone, then put this new information to use in an analysis which includes other fruit. The predictions from the analysis that is augmented by the study of the market for oranges should be better, i.e., provide better predictions, than an analysis which omits this new information.

The underlying issue here is what constitutes good economic research. Marshallian methodology does not insist on having everything derived from first principles in every single case. Instead, small parts of the whole are studied and tested. Only after we gain greater understanding of the small parts, does an understanding of the whole emerge. The "partial" in "partial equilibrium" does not mean that everything else is unimportant. "Partial" simply means that only part of the economy is being studied at a particular time; in fact, because of the complexity of the economy, it is impossible to study the whole directly, so one must study a simple part of the economy.

Thinking about Marshall's analysis in this manner enables us to make some sense of Colander's (1995) paper title, "Marshallian General Equilibrium Analysis." Marshall never envisioned that one would do partial equilibrium analysis and then simply stop.

MILTON FRIEDMAN

We turn now to some comments about the methodology of Milton Friedman, particularly his remarks in "The Methodology of Positive Economics" (1953b). It is not my intention here to plow through the issues of exactly what Friedman was or was not saying in this paper. That subject has been studied extensively.[2] Rather, I wish to make a few observations about Friedman's methodology that are pertinent to our analysis here.

First and foremost, we need to recognize explicitly that Friedman's methodological comments must be interpreted in the light of Marshallian methodology. That Friedman's methodology is Marshallian is unquestionable. This is best seen in two papers Friedman included in *Essays in Positive Economics* (1953a), where "The Methodology of Positive Economics" was published. In the first of these, "The Marshallian Demand Curve" (1949), Friedman states his views on the respective merits (or lack thereof) of Marshallian and Walrasian methodologies. His comments here are uniformly positive about Marshallian methodology and uniformly negative about Walrasian methodology. As an example of the latter: "It [Walrasian analysis] yields no predictions, summarizes no empirical generalizations, provides no useful framework of analysis" (Friedman, 1953a, p. 92). (We should note that Friedman was not always so absolutely derogatory in his remarks about Walrasian methodology. In "Leon Walras and His Economic System", 1955, p. 906, he notes that Walrasian methodology is useful for the "bird's-eye view" it provides of the economy.) The essay "Lange on Price Flexibility and Employment: a Methodological Criticism" (1946) also indicates Friedman's unambiguous Marshallian orientation. This essay can be read more generally as remarks on the deficiencies of Walrasian methodology. In fact, the next chapter of this book provides just such a reading.

It is impossible to accurately understand Friedman's methodological remarks without relating them to the tenets of Marshallian methodology. With this perspective, we gain a better understanding of Friedman's widely known discussion of the importance of the accuracy of a model's assumptions.

Friedman's basic methodological argument is that the test of the validity of any theory is how well its predictions match reality:

> [T]he only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted ("frequently" or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted; great confidence is attached to it if it has survived many opportunities for contradiction.
>
> (Friedman, 1953b, pp. 8–9)

This argument that theories must be tested by their implications is at the very heart of Marshallian methodology.

Friedman does not stop here. He goes on to argue that it does not matter whether the assumptions of a model are true or not. He argues that the "widely held view" that "the conformity of these 'assumptions' to 'reality' is a test of the validity of the hypothesis *different from* or *additional to* the test by implications . . . is fundamentally wrong and productive of much mischief" (Friedman, 1953b, p. 14).

This statement must be interpreted in a Marshallian framework. The aim of Marshallian analysis is to advance refutable predictions. The test of any theory is whether its predictions are refuted or not. If a theory has predictions which are refuted, it matters not whether the assumptions were true; if the predictions are wrong, then the theory is wrong. Similarly, if the predictions of a theory are not refuted, the theory is of value, even if the assumptions of the theory are incorrect. If we can accurately predict future happenings, we have gained knowledge about the economy. The means by which we obtained this knowledge are irrelevant.

Of course, Friedman goes even further when he argues, "[I]n general the more significant the theory, the more unrealistic the assumptions" (Friedman, 1953b, p. 14). This statement – which Samuelson (1963) dubbed the "F-twist" – has provoked a great deal of controversy. There is no need here to try to interpret this statement or assess its validity. (See Mayer, 1993a, for a good interpretation and a summary of the literature.) Here, we will only note that whatever the statement means, it should be interpreted in a Marshallian framework.

In the end, Friedman reaches Marshallian conclusions about the role of assumptions:

> [T]he relevant question to ask about the "assumptions" of a theory is not whether they are descriptively "realistic," for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions.
>
> (Friedman, 1953b, p. 15)

This is again precisely a statement of Marshallian methodology.

Similarly, much confusion has arisen over Friedman's use of the "as if" argument; e.g., the economy works as if it were populated by profit-maximizing firms. Once again, this argument must be interpreted in a Marshallian framework. This is really the same argument as the one just discussed. Friedman finds the "as if" argument acceptable as long as it provides good predictions. If my theory is that an expert billiard player shoots as if he calculated complicated mathematical formulas, then I would find that the predictions of my model were correct, even if the billiard player made no such calculations.

Thus, the acceptability of "as if" hinges on the argument that it is only the accuracy of the predictions of a model that matters. This does not make any use of the "as if" argument legitimate. Suppose my theory was that an expert billiard player shoots as if he were someone who had never played the game before. Now, the predictions of my theory would be wrong. The "as if" argument was applied incorrectly. Thus, the test of whether the "as if" argument is being used appropriately is the same as the test of the predictions of the theory.

The general point here is that Friedman's methodological statements are only meaningful in a Marshallian framework. The truthfulness or usefulness of these comments in a Marshallian framework has no implications for their use outside of that framework. This is not a remarkable statement; one cannot take any argument out of its context and reliably use it in a completely different context. For example, conclusions about government behavior in twentieth-century America do not necessarily apply to the Roman Empire or even the Soviet Union. Thus, it is not proper for researchers to take Friedman's statements on the nature of assumptions and use them in a different methodological framework. Indeed, the mixing of Friedman's comments and a nonMarshallian methodology is nothing less than the creation of a methodological farrago.

THE REPRESENTATIVE AGENT

Finally, we turn to the question that prompted this inquiry. How does the representative agent fit into all this? As it turns out, the question is easily answered.

In a Marshallian framework, the representative agent hypothesis is no different than any other simplifying hypothesis. There is nothing unique about assuming homogeneity of agents; it involves exactly the same issues as assuming profit-maximization behavior by firms.

Thus, the representative agent hypothesis is appropriate in some conditions but not in others. It cannot be used indiscriminately. For some issues, it does not matter whether agents are different or not, as the same result follows either way. In these cases, the representative agent hypothesis can be a useful simplifying construct. It may be much easier to analyze a particular issue if one can assume away the complications of heterogeneity. Since the aim in Marshallian analysis is the discovery of small truths, a hypothesis which allows a researcher to discover a result which otherwise would be buried under a wealth of complexity is an extremely good thing.

However, there will be cases in which the representative agent hypothesis is not appropriate. For some problems, heterogeneity of agents makes a real difference. In the real world, of course, people and firms are heterogeneous. There are thus likely to be some matters in which heterogeneity plays a crucial role. In these cases, the world would be a very different place if all people were identical.

This is the reason why empirical testing is so important in Marshallian analysis. If the representative agent hypothesis is improperly assumed, i.e., if it is assumed that heterogeneity does not matter when it really does, then the predictions of the model will be refuted. At these times, a researcher must carefully reconsider his model to find what is wrong. During this reconsideration, the representative agent hypothesis should not be considered inviolable. It is always possible that the reason a given model fails to predict well is that homogeneity is improperly assumed.

The representative agent assumption can thus be successfully used in a Marshallian framework. It abstracts from real world complexities so that a researcher can focus on the matter of interest. It can serve exactly the role that Friedman specified for simplifying assumptions in general, namely that of an "economical mode of describing or specifying a theory" (Friedman, 1953b, p. 23).

7

THE NEW CLASSICALS AS WALRASIAN ECONOMISTS

THE METHODOLOGY OF NEW CLASSICAL ECONOMICS

It is now apparent that in order to determine the propriety of the new classical use of representative agent models, we need to establish which methodological tradition is being followed. The purpose of this chapter is to demonstrate that new classical economics follows the Walrasian tradition. The implication immediately follows that since the representative agent is a poor instrument with which to construct Walrasian models in general, it is not of much use in helping the development of new classical Walrasian models.

New classical economics is at its heart a methodological school. It is not a set of policy conclusions or views about the real economy which set these economists apart. Rather, what makes new classical economics distinct is the methodology it uses.

This chapter has two parts, each aimed at demonstrating the similarity between new classical methodology and Walrasian methodology. The first part directly shows this similarity via an inspection of new classical methodological statements. The second section shows that Friedman's criticisms of a Walrasian book by Lange apply with equal force to the new classical writings.

NEW CLASSICAL METHODOLOGICAL STATEMENTS

Let us begin by looking at some general statements made by prominent new classical economists about the proper method of engaging in economic research. Perhaps the most explicit methodological statement is found in Lucas (1980). Note that this quotation could serve very well as a definition of Walrasian methodology:

> One of the functions of theoretical economics is to provide fully articulated, artificial economic systems that can serve as laboratories in which policies that would be

> prohibitively expensive to experiment with in actual economies can be tested out at a much lower cost. To serve this function well, it is essential that the artificial "model" economy be distinguished as sharply as possible in discussion from actual economies. Insofar as there is confusion between statements of opinion as to the way we believe actual economies would react to particular policies and statements of verifiable fact as to how the model will react, the theory is not being effectively used to see which opinions about the behavior of actual economies are accurate and which are not. . . . On this general view of the nature of economic theory then, a "theory" is not a collection of assertions about the behavior of the actual economy but rather an explicit set of instructions for building a parallel or analogue system – a mechanical, imitation economy.
>
> (Lucas, 1980, pp. 696–7)

This emphasis on the use of artificial economies is echoed some years later:

> Dynamic economic theory – I mean theory in the sense of models that one can write down and do something with, not in the sense of "opinion" or "belief" – has simply been reinvented in the last 40 years. It is now entirely routine to analyze economic decision-makers as operating through time in a complex, probabilistic environment, trading in a rich array of contingent-claim securities, and to study agents situated in economies with a wide variety of possible technologies, information structures, and stochastic disturbances. While Keynes and the other founders of what we now call macroeconomics were obliged to rely on Marshallian ingenuity to tease some useful dynamics out of purely static theory, the modern theorist is much better equipped to state *exactly* the problem he wants to study and then to study it.
>
> (Lucas, 1987, p. 2)

The parallel in both substance and style to Walras' description of the nature of pure economics is striking. Model economies are to be sharply distinguished from actual economies; the emphasis is on studying how the model behaves; pure economic theory is nothing more than the construction of model economies; model economies should be general, allowing for wide variety of particular technologies, information structures, and so on; there is no real need for "Marshallian ingenuity" if we simply write down a sufficiently good model.

As a result of this focus, a large amount of the new classical literature is devoted to working out how to solve ever more complicated models. Hansen and Sargent explain their goal in this manner:

> Having tractable expressions for the restrictions across the parameters of stochastic processes that agents face and their decision rules is necessary in order to make rational expectations modeling applicable to problems of even

> moderate dimension. Success in this part of our work will in effect extend the size of rational expectations systems that are manageable.
>
> (Hansen and Sargent, 1980, p. 8)

Even more indicative is the title chosen by Lucas and Sargent for their collection of new classical papers, *Rational Expectations and Econometric Practice* (1981a). The title announces a collection of papers on how to solve models, not a collection of papers about the implications of macroeconomic theory for actual economies.

Let us now turn to several specific issues. Perhaps the most notable feature of Walrasian methodology is its reliance on economic *models* as engines of discovery. This primacy of models is replicated in new classical economics:

> For a long time most of the economics profession has, with some reason, followed Keynes in rejecting classical macroeconomic models because they seemed incapable of explaining some important characteristics of time series measuring important economic aggregates. . . . We now have rigorous theoretical models which illustrate how these correlations can emerge while retaining the classical postulates that markets clear and agents optimize . . .
>
> (Lucas and Sargent, 1979, p. 8)

There is now no longer a reason to reject the classical postulates, *not* because we have empirical evidence that such postulates are better than the Keynesian ones, but simply because we now have new and better *models* of the economy. The key role of models is further seen in the following quotations:

> One exhibits understanding of business cycles by constructing a *model* in the most literal sense: a fully articulated artificial economy which behaves through time so as to imitate closely the time series behavior of actual economies.
>
> (Lucas, 1977, p. 11)

> To understand the implications of long-term contracts for monetary policy, we need a model of the way those contracts are likely to respond to alternative monetary policy regimes.
>
> (Lucas and Sargent, 1979, p. 11)

The important thing in understanding business cycles, labor contracts or anything else is not empirical data, nor is it even finding theories which can be empirically tested, but rather, it is finding a "fully articulated artificial economy."

The simple construction of models does not immediately place a body of work firmly in the Walrasian tradition. It clearly matters what one does with the model. For example, consider the issue of testability. Do we need to test our models against empirical data or do we know they are correct because we know how they were derived?

At times, it is hard to figure out why new classical writers ever bother looking at data:

> The issue of how confident we are in the econometric answer is a subtle one which cannot be resolved by computing some measure of how well the model mimics historical data. The degree of confidence in the answer depends on the confidence that is placed in the economic theory being used.
>
> (Kydland and Prescott, 1991, p. 171)

However, at other times, this would seem to be an area where new classicals are very Marshallian. New classical writings are filled with remarks about the vital importance of testing results against reality. For example: "The research line being pursued by some of us involves the attempt to discover a particular, econometrically testable equilibrium theory of the business cycle, one that can serve as the foundation for quantitative analysis of macroeconomic policy" (Lucas and Sargent, 1979, pp. 7–8). Such quotations seem to be the antithesis of Walrasian methodology.

However, when we turn from the new classicals' statements about the need for testing and look at the actual role of testing in their work, we see a very different picture. Empirical testing is accorded a rather strange status in new classical work. Consider the following statement, which comes from the same paper as the call for a testable theory cited above:

> It is worth reemphasizing that we do not wish our responses to these criticisms to be mistaken for a claim that existing equilibrium models can satisfactorily account for all the main features of the observed business cycle. Rather, we have simply argued that no sound reasons have yet been announced which even suggest that these models are, as a class, *incapable* of providing a satisfactory business cycle theory.
>
> (Lucas and Sargent, 1979, p. 14)

Lucas and Sargent have come a long way in the six pages between these two statements. From calling for a testable theory, they move to arguing that Keynesian economics is dead and has been supplanted by new classical theory, not on the basis of the empirical successes of new classical theory in comparison to Keynesian theory, but rather on the basis that new classical theory has not been demonstrated to be theoretically impossible.

When the new classicals do use empirical tests, they seem to have very different standards of success than those used by traditional economists. For example, Sargent (1976) argues for a model on the basis that his empirical results are "not obscenely at variance with the data" (p. 233) and that "Some evidence for rejecting the model has been turned up, but it is far from being overwhelming and decisive" (pp. 235–6). Or, to take another example, consider a paper by Sargent and Wallace (1975). In their concluding remarks, they give the reasons why they think their results should be taken seriously:

> First, the hypothesis that expectations are rational must be taken seriously, if only because its alternatives, for example, various fixed-weight autoregressive models, are subject to so many objections. Second, the aggregate supply hypothesis is one that has some microeconomic foundations, and it has proved difficult to dispose of empirically. It is precisely these two aspects of our model – rational expectations in conjunction with Lucas's aggregate supply hypothesis – that account for most of our results.
>
> (Sargent and Wallace, 1975, p. 254)

There is no additional empirical evidence presented for their model. Furthermore, they offer no empirical evidence for the propriety of using the rational expectations hypothesis.

In fact, the only mention of empirical results in the above quotation is in reference to the natural rate hypothesis, and here it is only to note that this hypothesis is "difficult to dispose of." Sargent and Wallace footnote this clause to indicate the research on which they are basing this conclusion. It is natural to assume that the authors intend us to believe that the papers cited lend credence to their claim. A brief look at these papers is thus rather illuminating.

There are two papers cited: Lucas (1973) and Sargent (1973). The Lucas paper has two tests of the natural rate hypothesis. Lucas notes that in the first test, the natural rate model "passes the formal tests of significance. On the other hand, the goodness-of-fit statistics are generally considerably poorer than we have come to expect from annual time-series models" (Lucas, 1973, pp. 331–2). The natural rate theory does pass Lucas' second test. Lucas concludes that the natural rate structure "accounts for output and inflation rate movements only moderately well, but well enough to capture the main phenomenon predicted by the natural rate theory" (ibid., p. 334). The evidence from this source is thus, at best, tenuous.

However, when we turn to the second source of support for Sargent and Wallace's empirical claims, the evidence deteriorates rapidly. Sargent (1973) also has two tests of the natural rate hypothesis. One of the tests rejects the hypothesis while the other has mixed results. If anything, the evidence in this paper tends to reject, not support, the natural rate hypothesis. Sargent almost admits as much when he comments, "I imagine that the evidence would not be sufficiently compelling to persuade someone to abandon a strongly held prior belief in the natural rate hypothesis" (Sargent, 1973, p. 462). Exactly! The tests in Sargent (1973) tend toward *rejecting* the natural rate hypothesis, but Sargent is unpersuaded; so unpersuaded, in fact, that he later cites these results as evidence *supporting* the natural rate hypothesis.[1]

Thus, the empirical support cited by Sargent and Wallace for their conclusions is reduced to four empirical tests of only one of the two hypotheses which they claim account for the results of their model. Of these four tests, one supports the hypothesis, two provide mixed results, and one rejects the hypothesis. It is on this empirical basis that Sargent and Wallace seek support for their model.

The foregoing examination of new classical use of empirical tests shows that their standards of a good empirical test are rather loose. More insight is gained into the issue of their views on empirical testing when we look at how new classical methodology evaluates the relative worth of theory and data. Walrasian methodology strongly favors theory over data, as does new classical methodology.

We can see the new classicals' preference for theory over facts in two ways. First, new classicals have generally been more concerned with how the models were structured than with the results that come out of the models. Consider the debate about policy ineffectiveness. The policy neutrality results in early new classical writings were dramatic indicators of the different conclusions that could be obtained by using new classical methodology instead of the Keynesian framework. Yet new classicals seemed almost indifferent to these results; they were far more interested in the uses of theory. Thus, Townsend could state, "[I]t is not right that money doesn't matter. One can write down explicit dynamic models that allow for uncertainty and include rational expectations, but produce the results that money matters" (quoted in Klamer, 1983, p. 85). In other words, the results of the theory are secondary; one cannot argue that money does not matter because it is possible to write down models where money does matter. Similarly, McCallum (1979) writes:

> For the most part, the formal econometric evidence developed to date is not inconsistent with the neutrality proposition. But the power of existing tests is probably not high and, in any event, the evidence is not entirely clearcut. Thus many economists may tend, at least for the present, to maintain adherence to their favorite theoretical model – whichever one offers the combination of features that seems essential.
>
> (McCallum, 1979, p. 244)

We can additionally see in this quotation the lack of reliance on empirical support for theoretical positions.

Another way in which we can assess the relative roles of theory and facts in new classical methodology is to examine what happens when they conflict. If the theory does not match the data, where do new classical sympathies lie? The answer is clearly with the theory. There is no better example of this than Prescott's (1986) defense of real business cycle models, entitled "Theory Ahead of Business Cycle Measurement." Even the title conveys the relative importance of theory versus data. Prescott explains the title thus:

> The match between theory and observation is excellent, but far from perfect. The key deviation is that the empirical labor elasticity of output is less than predicted by theory. An important part of this deviation could very well disappear if the economic variables were measured more in conformity with theory. That is why I argue that theory is now ahead of business cycle measurement and theory should be used to obtain better measures of key economic time series.
>
> (Prescott, 1986, p. 21)

Lucas and Sargent (1979) take this line of reasoning even further:

> There are, of course, legitimate questions about how well equilibrium theories can fit the facts of the business cycle. Indeed, this is the reason for our insistence on the preliminary and tentative character of the particular models we now have. Yet these tentative models share certain features which can be regarded as essential, so it is not unreasonable to speculate as to the likelihood that *any* model of this type can be successful or to ask what equilibrium business cycle theorists will have in 10 years if we get lucky.
>
> (Lucas and Sargent, 1979, p. 10)

These quotations have a common theme. The theory is not quantitatively successful. However, it does have certain theoretical properties that are desirable. It is not "unreasonable" to "speculate" about the "likelihood" that "any" equilibrium business cycle model "can be" successful. Yet Prescott concludes that theory is ahead of measurement and Lucas and Sargent announce that Keynesian economics has been superseded by new classical economics.

Even in new classical statements acknowledging that theory is modified in light of empirical evidence, the relative preference for theory is obvious:

> Like any science, economics has these parts: a body of *theories* (self-contained mathematical models of artificial worlds); methods for collecting or producing *data* (more or less error-ridden and disorganized measurements); statistical methods for comparing a theory with some measurements; and a set of informal procedures for revising theories in the light of discrepancies between them and the data.
>
> (Sargent, 1993a, p. 22)

What is the point of those parenthetical asides?

There is a very good reason for the new classicals' relative lack of interest in empirical results. In exactly the same manner as Walrasian structural assumptions in general, the basic tenets of new classical methodology are completely nontestable. Lucas and Sargent (1979, p. 11) note this fact about the market clearing assumption: "Cleared markets is simply a principle, not verifiable by direct observation, which may or may not be useful in construct ing successful hypotheses about the behavior of these series." Similarly, the rational expectations hypothesis is also not easily tested. As Pesaran (1984, p. 213) has noted, "This unsatisfactory state of affairs is partly due to the inherently non-refutable nature of the REH [rational expectations hypothesis] when direct reliable observations on price expectations are not available."[2]

There is one last similarity between new classical and Walrasian methodology. Both place a very high value on the use of mathematics. There is no need to belabor this similarity. The new classicals' reliance on mathematics is obvious to even a casual reader of their work.

FRIEDMAN'S COMMENTS ON LANGE

The preceding section examined how new classicals describe their own work. These descriptions are very similar to those used by Walras in describing the proper means of studying economics.

This section aims to make the same comparison in a somewhat different manner. Milton Friedman has written a stellar brief detailing the case against Walrasian methodology. His 1946 review of Oscar Lange's *Price Flexibility and Employment* (1944) can be read as a detailed elaboration of the problems that arise in the use of the Walrasian method. We will see here that the problems Friedman details in Lange's book find parallels in new classical work. Such similarities are not proof that new classicals follow a Walrasian line, but they are indicative.

This section is not meant as an independent criticism of Walrasian methodology or new classical economics; its sole function is to show that *Friedman's* criticisms of Walrasian methodology could just as easily be leveled at new classical methodology.[3] Thus, it is quite possible that the criticisms below could be reasonably answered, just as it is quite possible that Lange could have answered Friedman's criticisms. The possibility of an answer to the criticisms is, however, irrelevant to the purpose at hand. It is noteworthy and indicative that Friedman's criticisms of a Walrasian model could just as easily have been made about the new classical models.

Let us begin by looking at Friedman's general descriptions of Lange's work. In the first paragraph of his review, Friedman notes: s

> Here is an obviously first-class intellect at work; yet the analysis seems unreal and artificial. Here is a brilliant display of formal logic, abstract thinking, complicated chains of deduction; yet the analysis seems more nearly a rationalization of policy conclusions previously reached than a basis for them.
>
> (Friedman, 1946, p. 613)

After providing an eloquent description of Marshallian methodology, Friedman goes on to note:

> The approach used by Lange, and all too common in economics, is very different. Lange largely dispenses with the initial step – a full and comprehensive set of observed and related facts to be generalized – and in the main reaches conclusions no observed facts can contradict. His emphasis is on the formal structure of the theory, the logical interrelations of the parts. He considers it largely unnecessary to test the validity of his theoretical structure except for conformity with the canons of formal logic. His categories are selected primarily to facilitate logical analysis, not empirical application or test. For the most part, the crucial question, "what observed facts would contradict the generalization suggested, and what operations could be followed to observe such critical facts?" is never asked; and the theory is set up so that it could seldom be answered if it were asked. The theory provides formal models of imaginary worlds, not generalizations about the real world.
>
> (Ibid., p. 618)

It is almost hard to believe that Friedman is not talking about the new classical economists in those passages. The assorted remarks made about new classicals in the last two decades are all there: first-class intellects and brilliant displays of logic, abstraction, and complicated material; emphasis on the formal structure of the model; no testing of conclusions which are, in any case, largely untestable; formal models of unreal, artificial, imaginary worlds.

Friedman did not stop with these general descriptions of Lange's work. He provided several specific criticisms of Lange's analysis. Every one of Friedman's specific statements about the problems with Lange's methodology could be – indeed, by the end of this section, will be – made about some aspect of the new classical macroeconomics.

Oversimplification

The first weakness Friedman finds with Lange's analysis is the tendency toward abstractness and oversimplification:

> If he [a theorist] is willing, as Lange is, to keep his analysis exceedingly abstract, he can consider an indefinitely large number of variables and functions of each kind, since, on the abstract level on which he has chosen to operate, multiplication of variables and functions of the same kind is likely to mean simply the insertion of appropriate "etc.'s" into the argument; it is not likely to add any essential complication.
>
> (Friedman, 1946, p. 620)

By using such abstract analysis, a theorist "gains the appearance of generality without the substance" (ibid., p. 620).

The exact same tendencies show up in new classical work. Indeed, the very point of using representative agent models is to be able to consider a large number of people on an abstract level. Moreover, the new classicals proclaim their use of abstract analysis as a virtue; from reading their work one gets the impression that concreteness is a vice. Sargent (1982) argues:

> Models at such an explicit level must necessarily be highly abstract and "unrealistic" given our current research technology. . . . It is true that at present these models are so abstract and simple that they cannot be used formally to restrict the rich array of financial variables that appear in say the FMP or DRI models using proper modern rational expectations econometric procedures, for example, *à la* myself and Hansen. This feature of the constraints imposed by our current research technology is unfortunate, but does not seem to argue in favor of models that purchase superficial realism at the cost of making numerous implicit assumptions that violate the principles that emerge from the simple abstract models that we do have.
>
> (Sargent, 1982, p. 384)

Note that this quote could also have been added to the previous section as a further indication of the new classical preference for models over facts, even when the models are admittedly simple. Lucas (1980, p. 700) expresses a similar sentiment about the virtues of abstraction when he argues that "progress in economic thinking means getting better and better abstract, analogue economic models, not better verbal observations about the world."

Now, it is of course true that all economic theorizing – in fact, all theorizing of any sort – must abstract and simplify from the world to some extent. But there are greater and lesser degrees and appropriate and inappropriate levels of abstraction and simplification. Friedman is arguing that Lange is on the extreme side of this simplification spectrum: "The theorist who seeks to devise a generalization from observed facts will also have to simplify and abstract from reality. But it is clear that he need not limit himself to anything like so simple a system as Lange uses" (Friedman, 1946, p. 620). The same can be said of the new classicals. At times they take simplification to an extreme; there are multiple descriptions of the economy which reduce all of economics to the study of unidentified exogenous and endogenous variables with accompanying parameters and random shocks but no discussion of precise empirical counterparts (e.g., Lucas and Sargent, 1979, 1981, pp. 11–40; Lucas, 1980). The new classicals thus do not have the monopoly on simple and abstract models; Lange was using them half a century ago.

The use of classifications that have no direct empirical counterpart

Friedman's second specific complaint about Lange's analysis is that it leads to the use of empirically vacuous constructs:

> The theorist's urge to be realistic therefore almost inevitably conflicts with his urge to be theoretically comprehensive. The result is likely to be a compromise. He uses classifications (and especially names) that appear to have empirical meaning; but, in order to apply them to his entire analysis, he is forced to define them in a way that eliminates their direct empirical content. The end result is likely to be classifications that do not satisfy the initial empirical motivation and yet are not those best suited to the theoretical analysis.
>
> (Friedman, 1946, p. 621)

Exactly the same problem shows up in new classical work. In fact, we saw several examples of this phenomenon in our discussion of the Lucas critique in Chapter 4. Both taste and technology parameters are bandied about as if they represent the truly deep constructs an innocent reader would imagine. However, as we saw earlier, what are defined to be taste and technology parameters are not what most people would think of as such parameters. Taste and technology parameters are defined in new classical models in order to meet the requirements of their model construction; thus, they are not defined to be the empirical counterparts of what most people think of as tastes and technology.

To see this lack of correspondence between reality and the theoretical constructs, recall our earlier discussion (pp. 47–8) of Kydland and Prescott's (1982) real business cycle model. In this model, the authors claim that technology shocks drive aggregate business cycles. However, when we turn to what they define as a technology shock, we find that it has nothing to do with technology. Rather, what Kydland and Prescott call a technology shock is merely a scaling variable which indicates the (differing) levels of output a given worker produces on a *given* machine. All technology variables which have empirical counterparts are held fixed, namely the capital–labor ratio, the elasticity of substitution between capital and inventories, the shares of capital and inventories, the depreciation rate, and the fraction of the resources allocated to an investment project from the jth stage to the last. This same problem is seen in many of the new classical papers: taste and technology parameters in models bear little resemblance to their real world counterparts.

Improperly ruling out theoretical possibilities

The next problem in Lange's analysis which Friedman delineates arises from a desire to simplify the analysis:

> The number of permutations and combinations of even a small number of elements each of which can have several forms or values is so large that there is a strong incentive to limit the number of possibilities considered in detail. One obviously

> attractive method, though one that is really inconsistent with the basic theoretical approach, is to rule out possibilities that on one ground or another can be judged "unrealistic" or "extreme." There is nothing wrong with this procedure if the evidence on which the possibilities are judged unrealistic is convincing. The danger is that the urge to simplify and the preoccupation with abstract logic will lead to the ruling-out of possibilities on grounds that are either unconvincing or wrong.
>
> (Friedman, 1946, p. 623)

The reason Lange would want to restrict consideration of all theoretical possibilities to a more manageable subset is understandable; the problem is the means by which he does it.[4]

Once again, we find the new classicals engaging in the same type of analysis as Lange. As a first example, consider the response to the problem of nonstationary equilibriums. One result of the introduction of rational expectations into economic models induced a great deal of concern in many new classical economists: frequently, rational expectations equilibriums were not stable. Speculative bubbles could arise which would drive prices to either infinity or zero. There was a concerted effort by several new classical economists to rule out these speculative bubbles, leaving only the stable equilibrium point as the model solution.

Friedman notes that Lange faced a similar problem. Lange dealt with the problem by assuming it away: "We disregard, however, the possibility of multiple equilibrium because it seems to be very unlikely in practice" (Lange, 1944, p. 10n). Similarly, Lange ruled out certain theoretical possibilities because they were "special cases." Friedman faults Lange for the "casual empiricism" implicit in this sort of thing:

> If this is good practice for empirical work, it is equally good for theoretical. Lange might as well simply assert his theoretical conclusions without giving the basis for them; and no empirical work need hesitate to assert: "It is obvious on theoretical grounds that. . . . "
>
> (Friedman, 1946, p. 624)

How do the new classicals deal with the problem of multiple equilibriums? Barro (1981, p. 54) summarizes the general thrust of these attempts: "Plausible parameter restrictions can rule out multiple equilibria, but the generality of the uniqueness result is again at issue." Note that these parameter restrictions are said to be "plausible"; they are neither theoretically nor empirically justified. Barro also notes that some new classicals regard these nonstationary equilibriums "as empirically irrelevant intellectual curiosities, which will eventually be disposed of by deeper theoretical arguments" (ibid.). We have here the ultimate in improperly ruling out theoretical possibilities: we have no reason to rule them out now, but we can rule them out because there will be reasons to do so in the future.

Let us consider in detail one of these attempts to rule out nonstationary equilibriums. McCallum (1983, p. 161) argues that they arise only because "unnecessary or 'extraneous' components are permitted to influence expected (and therefore actual) values of endogenous variables." Hoover (1988, pp. 119–20) points out that this means of ruling them out is inherently inconsistent with the model that generates them. The model is one in which agents have rational expectations. Now the whole point of rational expectations is that agents use all the information at their disposal to form their expectations. McCallum's procedure for eliminating nonstationary equilibriums is to restrict the set of information that agents use to only that which is necessary. This violates the assumption that all information is used to form expectations.

Consider a second way in which the new classicals rule out theoretical possibilities: on the grounds of mathematical intractability. This criterion is explicitly advocated in Lucas and Sargent (1981):

> Evidently no progress can be made on this difficult problem at the level of generality at which this discussion has so far been set. It will be necessary to restrict the sets S1, S2, and U and the functions *V, f,* Φ, and *g* in order to bring available mathematical and computational technology to bear on various aspects of this general phenomenon.
>
> (Lucas and Sargent, 1981, p. xiv)

Elsewhere, Lucas and Sargent elaborate on this criterion:

> There are no *theoretical* [sic] reasons that most applied work has used linear models, only compelling technical reasons given today's computer technology. The predominant technical requirement of econometric work which imposes rational expectations is the ability to write down analytical expressions giving agents' decision rules as functions of the parameters of their objective functions and as functions of the parameters governing the exogenous random processes they face. Dynamic stochastic maximum problems with quadratic objectives, which produce linear decision rules, do meet this essential requirement – *that is their virtue.* Only a few other functional forms for agents' objective functions in dynamic stochastic optimum problems have this same necessary analytical tractability. Computer technology in the foreseeable future seems to require working with such a class of functions, and *the class of linear decision rules has just seemed most convenient for most purposes. No issue of principle is involved in selecting one out of the very restricted class of functions available.* Theoretically, we know how to calculate, with expensive recursive methods, the nonlinear decision rules that would stem from a very wide class of objective functions; no new econometric principles would be involved, only a much higher computer bill.
>
> (Lucas and Sargent, 1979, p. 13, emphasis added)

By the end of this passage, Lucas and Sargent have declared that there is only a "very restricted" set of functions that are usable. On what grounds did they rule out all functions not in this "very restricted" set? Purely on the grounds of mathematical tractability. Indeed, mathematical tractability is the only criterion offered for model selection. Note that within this small subset of all possible functions, it is a matter of indifference which is chosen ("No issue of principle is involved"). Lucas and Sargent are here advocating model selection on grounds of "convenience." They are implicitly ruling out all functional forms which are difficult to handle mathematically. Note that they are not simply ruling out functional forms which are *impossible* to handle mathematically, but ones that are possible but difficult.

Now, if the aim of economics is merely to manipulate mathematical models, then the Lucas and Sargent criterion is sensible; in order to perform mathematical tricks we must have models on which these tricks can be done. However, if our goal has anything at all to do with the real world, then the ruling out of models which are difficult to manipulate mathematically is improper, for we have no *a priori* knowledge that the real model is in any way mathematically tractable, let alone "convenient" to use. Lucas and Sargent claim that their aim is to study reality: "The *objectives* of equilibrium business cycle theory are . . . to provide a scientifically based means of assessing, quantitatively, the likely effects of alternative economic policies" (Lucas and Sargent, 1979, p. 15). Unless Lucas and Sargent can provide *empirical or theoretical* evidence that such a goal is achievable within a linear model, their restriction of the set of functions is entirely without basis. Without such evidence, they are, in essence, arguing that because a function is mathematically easy to use, it is empirically likely.

The problem here is not Lucas and Sargent's desire to use only mathematically tractable functions – who wouldn't prefer to use a tractable function rather than an intractable one? The problem is that Lucas and Sargent have elevated the use of mathematical tractability to a standard of good model selection. This is the impropriety. It may be the case that good – good in the sense of being accurate – models are intractable. However, even in this case, the Lucas and Sargent standard asserts that the mathematically tractable models are preferred, even if they are inaccurate. This is exactly the sort of thing about which Friedman warned in the passage above when he wrote, "The danger is that the urge to simplify and the preoccupation with abstract logic will lead to the ruling-out of possibilities on grounds that are either unconvincing or wrong" (Friedman, 1946, p. 623).

Putting primacy on the choice of easily solved models is not simply a matter of convenience; it does in addition have real consequences. Stiglitz (1992) notes:

> Another criterion for choosing assumptions in much recent work has been "solubility" – whether, with the given parameterizations, solutions can actually be

> calculated. . . . The problem is again one of trade-offs: the parameterizations which are soluble have properties which – on theoretical grounds alone, without engaging in much fancy econometric work – can be rejected.
>
> (Stiglitz, 1992, pp. 45–6)

As an example, Stiglitz notes that much recent work has used a constant absolute risk aversion utility function, which has the nice property of being quite mathematically tractable. However, if this utility function were accurate, then we should find a zero wealth elasticity of demand for risky assets and all people holding the same portfolio of risky assets. Stiglitz notes that neither of these implications is even remotely true. In other words, more accurate utility functions are being ruled out on the basis of mathematical tractability.

Introduction of friction

Friedman also noted a pair of errors which arise out of a desire to be realistic. The first of these errors is the improper introduction of "friction" into the analysis:

> Accordingly, to make the possibilities he considers more comprehensive, Lange introduces friction. . . . Lange's "friction" is a *deus ex machina;* it has no place in his theoretical system; he cannot really define it without going outside his system and, indeed, contradicting it. . . . Those of Lange's conclusions that rely on the introduction of friction are therefore different in kind from the rest of his conclusions. They are not the logical implications of a consistent theoretical system but simply *obiter dicta* whose acceptance involves implicit expression of skepticism about the rest of the analysis.
>
> (Friedman, 1946, pp. 626–7)

In essence, Friedman is claiming that Lange is artificially forcing the analysis into certain conclusions out of a recognition that, without such forcing, the analysis would yield results which were not consistent with reality.

We find exactly the same process occurring in new classical macroeconomics. The most notable occurrences arose after Sargent and Wallace (1976) propounded their policy ineffectiveness results. New classicals then scrambled to find means of explaining policy effectiveness.

Lucas and Sargent reprinted one such attempt in their collection of important new classical papers. Fischer (1977) reintroduced policy effectiveness by noting that if labor contracts were set for multiple periods, an active monetary policy would have an influence on output. The introduction of multiperiod contracts acts as a friction on the smooth adjustment of the economy in response to a change in the policy regime. However, such a friction is in direct contradiction to the basic theoretical point that the economy will adjust to policy regime changes. Fischer concludes his paper by noting:

> While the paper argues that an active monetary policy can affect the behavior of output if there are long-term contracts, and is desirable in order to foster long-term contracts, one of the important lessons of rational expectations literature should not be overlooked: the structure of the economy adjusts as policy changes. An attempt by the monetary authority to exploit the existing structure of contracts to produce behavior far different from that envisaged when the contracts were signed would likely lead to the reopening of the contracts and, if the new behavior of the monetary authority were persisted in, a new structure of contracts. But given a structure of contracts, there is some room for maneuver by the monetary authorities – which is to say that their policies can, though will not necessarily, be stabilizing.
>
> (Fischer, 1977, p. 204)

Now, the introduction of this friction is inherently contradictory. On the one hand, Fischer is denying that monetary policy can have an effect in the absence of multiperiod contracts because expectations will adjust to regime changes, while at the same time he is demonstrating the efficacy of monetary policy in a world where contracts do not adjust with policy. But if it is permissible to posit nonadjusting contracts to explain policy effectiveness, why is it not permissible to posit nonadjusting expectations to explain the same thing? We could get policy effectiveness simply by asserting that expectations do not change in the face of policy, thereby introducing friction.

Fischer's explanation for why nonadjusting contracts are acceptable in the model is another example of improperly ruling out a theoretical possibility:

> The paper does not provide a microeconomic basis for the existence of long-term nominal contracts, though the transaction costs of frequent price setting and wage negotiations must be part of the explanation. . . . It is reasonable to conjecture that the costs of wage setting lead to the use of long-term contracts and that the difficulties of contract writing prevent the emergence of contracts that are equivalent to the use of spot markets.
>
> (Ibid., p. 194)

This is an example of what Friedman called "casual empiricism"; Fischer is advancing the argument that his assumption is empirically justified while giving no empirical evidence for the assertion

A second example of introducing frictions into a model to try and give a role to policy is the legal restrictions argument of Bryant and Wallace (1984) and Wallace (1983). Bryant and Wallace are trying to explain why interest bearing assets and noninterest bearing money can coexist; i.e., why don't interest bearing bonds drive noninterest bearing Federal Reserve notes off the market? Their answer is that it is due to restrictions imposed by the government.

This line of argument is functioning exactly like a *deus ex machina;* we don't know why the sun rises, so the god Apollo must drive it across the sky; we can't explain why money exists in the new classical framework, so the god Government must impose it. Just as the chariots of the sun were without an empirical basis, the legal restrictions invoked by Bryant and Wallace cannot be empirically justified.

Treatment of uncertainty

Friedman notes that a second way in which Lange buys realism is the way uncertainty is handled. Lange notes that a person contemplating future prices will be faced with a range of possibilities. It would be enormously complex to include all these possibilities in the analysis. So, Lange (1994, pp. 31–2) argues, "We can substitute for the most probable prices expected with uncertainty equivalent prices expected with certainty. Let us call them *effective* expected prices. . . . By means of this device, uncertain price expectations can be reduced to certain ones."

Exactly the same sort of thing is done in new classical work with respect to policy regimes. In actual economies, governments do not generally announce the exact policies they are following. So actual people face a distribution of possible policy regimes, with probabilities attached to each possibility. Such a problem is very difficult to formulate and solve. The new classical response has been to substitute for the most probable policy regime expected with uncertainty an equivalent policy regime (the one actually used) expected by all agents with certainty.

We should note one way in which new classical work has improved on Lange's analysis. Were Lange not hampered by having written his book before Muth (1961) illuminated the usefulness of rational expectations, he (Lange) could have avoided the cumbersome manner in which he actually dealt with uncertainty and used the much more elegant rational expectations hypothesis.

Friedman concludes his essay on Lange's Walrasian model by noting:

> A man who has a burning interest in issues of public policy, who has a strong desire to learn how the economic system really works in order that that knowledge may be used, is not likely to stay within the bounds of a method of analysis that denies him the knowledge he seeks.
>
> (Friedman, 1946, p. 631)

Half a century later, Hahn and Solow begin their essay on new classical economics by noting:

> Of course that is the economics of Dr. Pangloss, and it bears little relation to the world. In a decade that has seen vast progress in our study of asymmetric information, "missing markets," contracts, strategic interaction, and much else precisely because those aspects are regarded as real phenomena that require analysis, macroeconomics has ignored them all. . . . We found that we could not swallow this way of doing macroeconomics . . .
>
> (Hahn and Solow, 1995, pp. 2–3)

THE NEW CLASSICAL REPRESENTATIVE AGENT IN A WALRASIAN MODEL

As we noted in Chapter 3, the new classicals see themselves as constructing general equilibrium models. This chapter has demonstrated that they are explicitly following the Walrasian tradition. Sargent (1982, p. 383) explains the research agenda: "Since in general one agent's decision rule is another agent's constraint, a logical force is established toward the analysis of dynamic general equilibrium structures." Similarly, Kydland and Prescott (1991, p. 168) argue, "The general equilibrium . . . framework is well-designed for providing quantitative answers to questions of interest to the business cycle student."

If the goal of the new classical methodology is as Sargent and Kydland and Prescott explain it, then attempts can be made in such a direction. However, if that is the goal, it makes no sense to use a representative agent model. Representative agent models do not allow us to meet the standard of having good dynamic general equilibrium models unless we are willing to assume that all people are the same.

Similarly, Kydland and Prescott have argued:

> we exploit the well-known result that, in the absence of externalities, competitive equilibria are Pareto optima. With homogeneous individuals, the relevant Pareto optimum is one which maximizes the welfare of the stand-in consumer subject to the technology constraints and the information structure.
>
> (Kydland and Prescott, 1982, p. 1354)

Again, this is all well and good, but it doesn't explain why we should care about an economy with homogeneous people. Surely the interesting questions are about economies with a heterogeneous population, but the representative agent model does not allow us to study such economies in a Walrasian general equilibrium framework. We can either build good Walrasian general equilibrium models or we can use representative agent models, but we can't do both at the same time.

So, if we take the new classicals seriously when they argue that we should be constructing good dynamic general equilibrium models, we should also insist that the dynamic general equilibrium models constructed actually be good ones. We shouldn't settle for allowing the representative agent to intrude on the models. The representative agent model is not a method of making better Walrasian models at all.

Part IV

MICROFOUNDATIONS

8

MICROFOUNDATIONS: AUSTRIAN SSTYLE

INTRODUCTION

We will now take up the third strand of the argument for representative agent models described in Chapter 3, that of microfoundations. This discussion will begin in a rather unorthodox manner for a book that professed in Chapter 1 to be solidly neoclassical. In this chapter we will explore the microfoundational arguments of the Austrian economists. The new classical and more conventional arguments for microfoundations will be discussed in the next chapter.

The decision to begin the exploration of microfoundations with the methodological statements of the Austrian economists will strike many readers as rather eccentric. However, as Polonius would say, there is a method to this madness. The Austrians take microfoundations *very* seriously. If we are truly interested in establishing microfoundations for all of economics, if we take seriously the proposition that the study of the macroeconomy must begin at the microeconomic level, then we can do no better than use Austrian methodology. This is not to say that only the Austrians can lay claim to the complete explication of the microfoundations of the economy, merely that no other system of study could delve deeper, although it might be able to do as well.

This chapter does two things. It begins by examining the relevant portions of Austrian methodology, particularly the Austrian microfoundational arguments. We then examine whether the new classical representative agent models meet the Austrian criteria for establishing microfoundations. Revealing the punch line early: they do not.

We will not offer here a complete exegesis of Austrian economic thought; that would far exceed the scope of this monograph. Readers interested in Austrian thought are encouraged to read the originals. One of the foremost Austrian tracts – arguably the *magnum opus* – is Mises' *Human Action* (1966), the first edition of which was published in 1949. This work is a philosophical inquiry into the basis and structure of Austrian economics. Readers who prefer a less philosophical and more conventional work –

conventional in its structure, not by any means in its method of analysis – are referred to Rothbard's *Man, Economy, and State* (1962).

Moreover, the purpose of this chapter is merely to demonstrate that representative agent models do not meet the Austrian standards of microfoundations. This is not meant as an evaluation of the arguments themselves; we will defer such an evaluation until later chapters.

AUSTRIAN METHODOLOGY

The starting place or basis of the whole of Austrian economics is a single assumption, namely, that humans act (hence the title of Mises' tome). People engage in purposeful action; or, as Rothbard puts it, the whole of economics rests "on the primordial fact that individuals engage in conscious actions toward chosen goals" (Rothbard, 1976, p. 19).

This statement is not a mere triviality. It has profound implications, the foremost of which arises out of the emphasis on conscious choice. The assumption that people act means that when faced with choices in life, people can actually choose between them, they are not forced down one path or the other. In modern parlance, there are not immutable decision rules by which a person's choices are made; people are not automatons. A choice between two courses of action means exactly that: a choice.[1]

It is on this basic tenet that the Austrian system is built. We have above called this tenet an assumption, but Austrians call it a fact. However, the statement "humans act" seems to be impossible to verify or deny empirically. How would one devise a test?

Whence comes the Austrian assurance that this tenet is true? From introspection. The proposition that humans act is deemed to be self-evident. We need only look inside ourselves, contemplate our existence, if you will, to realize the accuracy of the statement that humans act.[2]

What does the fact of human action imply? We can begin by noting that people must use *means* to attain given *ends.* If a person acts, he must be acting to attain some result, since action makes no sense unless it is directed toward some end. Furthermore, in order to attain any given end, a person must use some means. If no means were necessary, then the desired end could have been attained without action.

All of this leads us to a definition of praxeology. Praxeology is defined as "the formal implications of the fact that men use means to attain various chosen ends" (Rothbard, 1962, p. 64). Praxeology is a general term that encompasses a great number of fields of study, one of which is economics.

The microfoundational nature of Austrian economics is now rather obvious. All of praxeology, and hence all of economics, is merely a study of the implications of human action. Indeed, Mises has noted that praxeological study takes "human action as an ultimate given" (Mises, 1966, p. 17). All of economics is necessarily reducible to people. There is no economic fact which can be derived without starting at a study of people's behavior in a given situation. It is in this sense that we can describe the Austrians as the ultimate in microfoundationalists.[3]

So how is praxeology, or economics, to proceed? How are we to discover the truths of this world? Again, by introspection:

> All that is needed for the deduction of praxeological theorems is knowledge of the essence of human action. It is a knowledge that is our own because we are men; no being of human descent that pathological conditions have not reduced to a merely vegetative existence lacks it. No special experience is needed in order to comprehend these theorems, and no experience, however rich, could disclose them to a being who did not know a priori what human action is. The only way to a cognition of these theorems is logical analysis of our inherent knowledge of the category of action. We must bethink ourselves and reflect upon the structure of human action. Like logic and mathematics, praxeology is in us; it does not come from without.
>
> (Mises, 1966, p. 64)

Economist, know thyself![4]

There is one other thing we need to note about the Austrian methodology. The praxeological method is obviously applicable to any condition in which people may find themselves. We could, for example, study what would happen if people could fly unaided by mechanical contraptions. We could write volumes on such things. However, imaginary worlds are of no interest to Austrian economists. Economics is the study of the real world; it concerns itself with the real constraints that bind real people.

THE AUSTRIAN REJECTION OF MACROECONOMICS

So what does this methodology imply for the field of macroeconomics? The Austrians are quite explicit here: macroeconomics is neither an appropriate nor a feasible field of study. It is not our intention here to evaluate the merits (or lack thereof) of the Austrian rejection of macroeconomic study. Our aim is to gain a greater appreciation for the level of the Austrian commitment to microfoundations by looking at their views on research which does not follow their methods.

The Austrian rejection of macroeconomics follows several routes. Let us begin with the theoretical criticisms. Macroeconomics is the study of social wholes, constructs which encompass multiple individuals. It has seemed to many that these wholes are appropriate constructs for scientific study, that we can independently study the behavior of these wholes. This is exactly the macroeconomist's mission: to explain the behavior of such entities as aggregate consumption or investment without referring to the components which constitute these aggregates.

Hayek has argued that such a study of social wholes is meaningless. Social wholes are not physical entities, such as, say, butterflies, which can be studied in the manner in which they naturally appear. "[T]he wholes as such are never given to our observation but are without exception constructions of our mind" (Hayek, 1952, p. 54). In other words, a social whole is only and exactly what we decide it is:

> The terms for collectives which we all readily use do not designate definite things in the sense of stable collections of sense attributes which we recognize as alike by inspection; they refer to certain structures of relationships between some of the many things which we can observe within given spatial and temporal limits and which we select because we think that we can discern connections between them – connections which may or may not exist in fact.
>
> (Ibid., p. 55)

Thus, the social sciences do not study entities which arise in nature; rather it is precisely the task of the social sciences to construct the wholes. Given this, it makes no sense to turn around and claim that the wholes just constructed have some independent life of their own. The wholes are exactly the sum of those parts that we, as social scientists, have grouped together.

The nonexistence of independent social wholes was approached in a different manner by Mises. Mises begins by noting that all action is performed by individuals; social constructs cannot act, only a person can: "The hangman, not the state, executes a criminal" (Mises, 1966, p. 42). Now, to understand a person's action, we must understand what motivated him to act in that way. The same is true of social wholes: to understand them we must understand the motivations of the people who constitute them, we must understand what these people thought they were doing:

> It is illusory to believe that it is possible to visualize collective wholes. They are never visible; their cognition is always the outcome of the understanding of the meaning which acting men attribute to their acts. We can see a crowd, i.e., a multitude of people. Whether this crowd is a mere gathering or a mass (in the sense in which this term is used in contemporary psychology) or an organized body or any other kind of social entity is a question which can only be answered by understanding the meaning which they themselves attach to their presence. And this meaning is always that of individuals. Not our senses, but understanding, a mental process, makes us recognize social entities.
>
> (Mises, 1966, p. 43)

The notion of a social whole, or a macroeconomic construct, that acts on its own power in its own fashion, independent of the motivations of the people who compose it, is a fallacy. Any attempt to study social wholes is in reality a study of individual action and should be recognized as such. Macroeconomics is not a proper field of study since its subject matter does not exist.

Hayek and Mises are here arguing that as a theoretical matter, the constructs which form the subject matter of macroeconomics are illusory, that they have no independent existence. Let us look at some concrete criticisms of these constructs, beginning with an argument based upon that found in Lachmann (1976).

Consider such macroeconomic entities as national income, national wealth, investment, and the capital stock. In order to get a value for these entities, it is necessary to add up a host of heterogeneous goods. For example, national income encompasses the production of paper, calculators, wheat, and haircuts. How do we add up these disparate goods and services? The traditional answer is to add up the monetary value of the goods, thereby converting all of the units into dollars. But how do we determine the monetary value of these goods? The traditional answer is to look at the price at which they were sold. Therein lies the problem.

In the decision to use the prices at which goods are sold as a measure of the goods' values is the assumption that these prices are equilibrium prices. In other words, the construction of macroeconomic aggregates assumes a consistent set of prices which result in macroeconomic equilibrium. The reason that consistent, equilibrium prices are necessary to construct macroeconomic aggregates is obvious. Take a machine which has an equilibrium value of $10,000. For the purpose of calculating investment, we would like to say that every sale of one of these machines raises the capital stock by $10,000. However, suppose that the machines did not sell at their equilibrium value. Specifically, suppose that one machine sold for $15,000 and a second for $12,000. Now, we have a problem if we use the selling price as the basis for computing investment. We would here say that the first machine raised the capital stock by $15,000 while the second machine only raised the capital stock by $12,000. But they are the same machine! In monetary terms we are saying that the first machine constitutes a greater investment than the second machine, but in physical terms they are exactly the same investment. Our method of converting physical goods into a common monetary denominator thus fails us unless all goods sell at their equilibrium value.

So we must assume that the prices with which we construct our macroeconomic aggregates are equilibrium prices. This is a rather powerful assumption, namely that the market prices which we actually use to construct our aggregates are exactly those of a Walrasian equilibrium. This is to say that every market is in equilibrium every day, that there is never a disequilibrium situation. But there is no reason to assume that this is true. While we can argue that, in the long run, prices will migrate to their equilibrium value, we cannot assume that the long run is always with us and thus we cannot assume that market prices are always equal to their equilibrium value. Macroeconomic aggregates are constructed using an inconsistent, nonequilibrium set of prices. There is no reason to assume that aggregates constructed with inconsistent prices will have any real meaning.

Some may argue that we could rescue the current means of constructing macroeconomic aggregates by finding an alternative justification for using market prices. After all, while market prices do not reflect equilibrium values, don't they equal the value of the good to the parties involved in the trade? Rothbard (1962, p. 728) argues that they do not. The only reason that trade takes place is because the market price is *not* equal to the valuation of the good by either party. The purchaser must value the good more than the amount of money he exchanges for it, while the seller must value the good less than the amount of money he receives for it. Market prices are in no way equal to the value of the good to the parties involved in the trade.

There is another problem with macroeconomic aggregates that is related to this question of pricing. To see this problem let us focus on a single aggregate, namely, the capital stock. Lachmann (1973, 1976) argues that consistent measurement of the aggregate capital stock is not possible.

Everyone familiar with even rudimentary economics knows that the value of a given piece of capital depends on its future income streams. If these income streams are expected to be larger, then the present value of the machine rises. But since we are not omniscient, we do not know the path of the future income streams. So we are forced to form some sort of expectations about the future. Different expectations about the future will lead to different measurements of the present value of capital. Now, if people are heterogeneous in the sense that they do indeed form different expectations, two different people will have two different valuations of a given physical capital stock. Which valuation is correct? There is no way to tell. In this situation it is meaningless to say that the capital stock is measurable. Indeed, the problem here is identical to a situation where a group of people, each of whom has a different idea on how many inches are in a foot, is measuring the length of a table. One person will say the table is three feet, another four feet, and another fifty-nine feet long. There is no way to reconcile these differences unless we reach an agreement on how many inches are in a foot. Similarly, there is no meaningful way to say that we have measured the value of the capital stock unless we agree on the future.

We could try to get out of this impasse by agreeing to value each piece of capital at the value placed on it by the owner of that piece. After all, if our goal is to get an aggregate valuation of capital, it would seem sensible to add up the valuations of those who hold the capital. In this manner, we would be using the division of labor; who knows better than the owner what is the most likely future income stream from a given machine? Now, this procedure makes sense if all we are interested in is the value of a given machine. But, once again, we have problems when we try to get a meaningful aggregate measure of the capital stock. Take the situation where the physical capital stock is fixed, i.e., no new capital is built and no depreciation occurs. Further, make the extreme assumption that no new information is revealed to the economy. This would seem to be an ideal situation in which to measure the capital stock, as nothing is changing. Using our procedure, we can arrive at an aggregate value of this capital stock. But this value is meaningless, for every time a piece of capital changes hands the aggregate value of the capital stock could change. The new owner of the capital stock may well have a different valuation of the future stream of income and thus place a different present value on the machine. Our aggregate measure of the capital stock would change with no change in the fundamentals. The mere change in ownership changes the aggregate capital stock. To assert that at any given time we have a meaningful measure of the aggregate capital stock is thus disingenuous at best.

The Austrian critique of macroeconomics is not limited to matters relating to price. In order to glimpse the breadth of the Austrian critique, we now turn to a couple of other criticisms of macroeconomics.

Lachmann (1973) argues that macroeconomic equilibrium is a problematical concept in and of itself. While it is very easy to bandy about the phrase macroeconomic equilibrium, it is hard to conceive of it referring to anything in the real world. Economists have tended to think of macroeconomic equilibrium in the same way they think of any other equilibrium; i.e., the macroeconomy is simply another market, in this sense no different from the markets for wheat or labor. The wheat market is in equilibrium when wheat demand equals wheat supply; the macroeconomy is in equilibrium when aggregate demand equals aggregate supply. There may be forces or "frictions" (e.g., collusion among wheat growers or wage stickiness) which prevent the equilibrium position from being realized, but the concept of equilibrium is unaffected by these frictions. The *idea* of macroeconomic equilibrium seems to be fraught with no more difficulty than the *idea* of wheat equilibrium.

Lachmann explodes this superficial comparison with a single observation: "Walras's Law teaches us that there can be no equilibrium of the economic system as a whole without equilibrium in every *market.* There can be no market equilibrium without equilibrium of each *individual* trading in it" (Lachmann, 1973, p. 39). It is immediately obvious that macroeconomic equilibrium is a much more problematical idea than wheat market equilibrium. Macroeconomic equilibrium presupposes wheat market equilibrium. Thus, any impediment to equilibrium in the wheat market is an impediment to equilibrium in the macroeconomy. But even if the wheat market is in equilibrium, the macroeconomy may not be; for the macroeconomy to be in equilibrium every other market must also have overcome its own frictions and be in equilibrium. There is thus no simple correspondence between the idea of a single market equilibrium and macroeconomic equilibrium.

The problems of macroeconomic equilibrium are compounded when we consider the results of divergent expectations of the future. In making plans, people must form some idea about future variables. Once formed, these expectations will guide the actions of people; products will be bought or sold as a result of these expectations. The success or failure of a given individual's plans depends upon the correctness of his expectations. While a person may plan to sell his labor at $15 per hour and buy a new car for $15,000 next week, he is not guaranteed to succeed. When next week comes, his labor may be worth $14.75 or $15.25, and the car may cost $15,500. In this case, his plans fail to materialize.

For a market to be in equilibrium, we must have planned expenditure equal to actual expenditure. However, when plans depend on expectations, a failure of the expectations to match reality can cause a failure of an equilibrium to materialize. We thus have very real problems when people have different expectations. It is trivial to say that when two people have different expectations about the realization of a given event, at most only one of them can be right, i.e., at least one of the people must have formed the wrong expectations.

So, unless all people have the same expectations, we know that at least some plans will be unsuccessful. Equilibrium will thus fail to materialize. It is thus extraordinarily improbable that a macroeconomic equilibrium will arise. Lachmann notes, “Walrasian general equilibrium makes sense only in a stationary world in which expectations play no part that could be called economically significant, and in which all plans of households and firms, attuned to the same set of existing prices are consistent” (Lachmann, 1973, p. 43). In other words, macroeconomic equilibrium is only likely in the most uninteresting of worlds.

The problems become even more intensified when we try to add macroeconomic growth. It seems to be virtually impossible to have a situation of equilibrium growth. To get equilibrium growth, the capital stock must be at the appropriate level at every period in time. But with growth comes uncertainty; we have moved out of the static world where everything is the same, period after period. Given this uncertainty, people must form expectations about the future in order to make investment decisions. With a divergence of expectations, it is guaranteed that malinvestment will occur, some people will make investment plans based upon erroneous assumptions. If there is malinvestment, there cannot be equilibrium growth. Lachmann (1973, p. 43) sums it up thus: “We must conclude that the concept of equilibrium growth is a misconception. It would require a world of convergent expectations all of which are invariably fulfilled and, resting upon them, of individual plans all of which are consistent with one another.”

Finally, it is worth considering an argument made by Mises (1966). He argues that the entire notion of getting a monetary value for macroeconomic aggregates like national income or national wealth is “nonsensical”:

> The attempts to determine in money the wealth of a nation or of the whole of mankind are as childish as the mystic efforts to solve the riddles of the universe by worrying about the dimensions of the pyramid of Cheops. If a business calculation values a supply of potatoes at $100, the idea is that it will be possible to sell it or to replace it against this sum. If a whole entrepreneurial unit is estimated [at] $1,000,000, it means

> that one expects to sell it for this amount. But what is the meaning of the items in a statement of a nation's total wealth? What is the meaning of the computation's final result? What must be entered into it and what must be left outside? Is it correct or not to enclose the "value" of the country's climate and the people's innate abilities and acquired skill? The businessman can convert his property into money, but a nation cannot.
>
> (Mises, 1966, p. 217)

In other words, monetary values are merely exchange ratios between money and goods. As no amount of money can be exchanged for national wealth, there is no point in calculating its value – indeed, it is impossible to do so in a meaningful manner.

THE REPRESENTATIVE AGENT

The Austrians have thus forcefully advocated a microfoundational methodology for economic inquiry. We are here interested in how the new classical representative agent models fit into this framework. Do representative agent models fit into the Austrian scheme? Do they establish sufficient microfoundations to avoid the Austrian assault on macroeconomics? Would the Austrian economists see the new classical attempts to provide microfoundations through representative agent models as an acceptable course of economic study? No. New classical representative agent models are every bit as much damned by Austrian microfoundational judgments as the Keynesian macromodels they were intended to replace.

To see the problems with new classical representative agent models from the Austrian perspective, it will be helpful to recall exactly how these models work. The utility (production) function for the representative consumer (firm) is posited. Through some sort of mathematical manipulation, e.g., taking derivatives to get first-order conditions, an equation (or a set of equations) is generated that explicitly shows the relationships between the variables of interest. This equation, which was derived for the individual agent, is then assumed to hold for the aggregate economy. Econometric tests or simulations can be run by inserting the appropriate aggregate variables into the derived equation.

From the Austrian standpoint, this representative agent procedure is in no way an improvement on explicit macromodels. The Austrian claim is that it is the aggregate variables themselves that are meaningless. If a measure of aggregate investment is of no value, then it does not matter how one uses it. There is no way to use macrovariables properly.

Representative agent models are merely disguised attempts to derive macroeconomic equations showing the relationships among macroeconomic variables. What is being sought and derived are not rules governing individual behavior, but rules governing aggregate behavior. The Austrian microfoundational arguments rule out all uses of aggregate variables. Representative agent models cannot be called any more microfoundational than explicit macroeconomic formulations.

The problems with new classical representative agent models go even farther. The root of all economics in the Austrian framework is the actions of real people. It is this focus on human actions that makes it impossible to be more microfoundational than the Austrians; they truly do get to the fundamental units of society. While representative-agent-model advocates claim to be microfoundational, by Austrian standards they are not. The basis for a representative agent model is not the behavior of the people in the real economy. Instead, these models begin with an artificial agent, a collection of assorted mathematically tractable functional forms. From the Austrian perspective, representative agent models are not getting to the heart of the economy at all; instead they are creating an artificial basis for an artificial economy. To the Austrians, this is not microfoundations; it is science fiction.

Representative agent models in no way meet the microfoundational standards of the Austrians. Their claims to establish microfoundations seem to be nothing but boastful talk to Austrians. The Austrians have met such pretenders before and scoffed. Earlier in the century, many Keynesian economists claimed to have provided microfoundations for their macroeconomic relationships. Lachmann (1973) derisively called these claims "lip-service to microfoundations." Although he was speaking of models entirely different from modern-day representative agent models, his remarks apply with equal force today:

> From time to time, though, we find that lip-service is paid to the microfoundations of economic phenomena. . . . But . . . [w]hen it comes to explaining economic processes we are usually told, for example, that "entrepreneurs" make investment decisions, "rentiers" place their wealth in one form or another, while consumers consume what is left of the GNP. Stereotypes play the part of economic agents. Economic events are the result of some kind of collective process of decision-making the *modus operandi* of which is never explained. Imaginary beings take the place of real people.
>
> (Lachmann, 1973, p. 19)

New classical representative agent models thus fail miserably to live up to Austrian standards of microfoundations. As Garrison put it:

> Choice-theoretic roots are necessary but not sufficient. Explaining economic phenomena in terms of choices and actions of individuals is – or should be – the primary business of economics. . . . But having choice-theoretic roots does not, by itself, confer respectability on a macroeconomic theory. A number of modern constructions – I'm thinking of some of the new classical theories and so-called "real-business cycle" theories – involve highly artificial, or deliberately fictitious, environments in which agents make choices. These theories, sometimes apologetically called "parables", are defended on the basis of their involving choice in a mathematically tractable setting. All too often, though, such virtues come at too heavy a cost – losing sight of the economic phenomenon to be explained. The trunk and branches have been traded for roots. It isn't clear to me, for instance, that a one-good choice-theoretic model can shed any light on the problems of inflation and business cycles.
>
> (Quoted in Snowden *et al.,* 1994, p. 384)

In the next chapter we will explore in what way the proponents of representative agent models think they are providing microfoundations. In other words, the advocates of representative agent models must have a very different idea of what constitutes microfoundations than that of the Austrians. Our task will be to elucidate these ideas.

IDEAL TYPES AND REPRESENTATIVE AGENTS

Hoover (1988, pp. 243–4) has noted that there is a superficial similarity between the Austrian use of ideal types and the new classical use of representative agents. Hoover notes that the Austrian method is to analyze the behavior of hypothetical constructs, labeled "ideal types." Analogously, he notes that the new classicals analyze the behavior of a single individual, labeled a "representative agent." Hoover argues that this similarity is only superficial:

> It is important to recognize that representative agents are very different from ideal types. The Austrians understand that the use of ideal types places a severe, although unavoidable, limit on the relevance of their conclusions. They provide answers in principle and not definite predictions about the actions of particular individuals or segments of the economy. In contrast, the new classicals use representative agent models in order to derive supposedly theoretically sound restrictions on admissible empirical observations. They use these models for prediction, and not simply for understanding principles.
>
> (Ibid.)

This is correct. However, it makes the difference between ideal types and representative agents seem simply a matter of the degree of reliance which is placed upon them. One could easily make the argument that representative agent models also only "provide answers in principle." For example, Lucas notes that the representative agent models used in real business cycle research are not meant to be evaluated on the basis of whether or not they are "true":

> Of course the model is not "true": this much is evident from the axioms on which it is constructed. We know from the outset in an enterprise like this (I would say, in *any* effort in positive economics) that what will emerge – *at best* – is a workable approximation that is useful in answering a limited set of questions. . . . Kydland and Prescott [1982] do not say much about which questions they hope their model could simulate accurately, or with what level of accuracy, but the model is set up to focus on the way firms and consumers react to changes in the intertemporal pattern on actual and expected prices. . . . The chances that the model will survive . . . criticism unscathed are negligible, but this seems to me exactly what explicit theory is for, that is, to lay bare the assumptions about behavior on which the model rests, to bring evidence to bear on these assumptions, to revise them when needed, and so on.
>
> (Lucas, 1987, pp. 45–7)

All of which could be read as a loquacious way of saying that the representative agent model used by Kydland and Prescott is simply meant to "provide answers in principle."

In the remarks cited above, Hoover also notes that Austrian ideal types are not meant to provide "definite predictions about the actions of particular individuals or segments of the economy." Again, the same could be said about new classical representative agent models. Most representative agent models are used solely to get aggregate level predictions; few make any attempt to look at any individual agents.[5]

Thus, while Hoover's sentiments are right, he does not draw a sharp distinction between ideal types and representative agents. In fact, there is scarcely even a superficial similarity between the two concepts. Ideal types are much more abstract than Hoover's discussion makes them seem. Mises (1966) defines ideal types as:

> the specific notions employed in historical research and in the representation of its results. They are concepts of understanding. . . . An ideal type cannot be defined; it must be characterized by an enumeration of those features whose presence by and large decides whether in a concrete instance we are or are not faced with a specimen belonging to the ideal type in question.
>
> (Mises, 1966, pp. 59–60)

For example, Napoleon can be described by using the ideal types "commander," "dictator," "revolutionary leader," or the French Revolution can be described using the ideal types "revolution," "disintegration of an established regime," "anarchy." There is literally zero resemblance between the Austrian idea of an ideal type such as "anarchy" and a new classical representative agent. Ideal types are words, and words alone, that are used to convey an image.

There is another large distinction between Austrian ideal types and new classical representative agents. Mises argues that ideal types are not related to statistical averages. First, many of the characteristics that make up an ideal type are not inherently numerical; what is the numerical value associated with "dictator" or "revolution"? Second, even if there is something inherently numerical about an ideal type, the ideal type must be defined before the average can be calculated: "[I]t is logically impossible to make the membership of a class or type depend upon an average" (Mises, 1966, p. 60).

Thus, it is difficult to find even a faint resemblance between the Austrian ideal type and the new classical representative agent. They are entirely different creatures altogether.

ADDENDUM

This discussion may have caused a misconception about the Austrian critique of traditional economics. The unsatisfactory nature of the representative agent models in Austrian methodology is not limited to problems with macroeconomic constructs. In this section, we look briefly at several other aspects of new classical representative agent models that Austrians would find unappealing. While, for our purposes here, it is sufficient to note that representative agent models are not microfoundational by Austrian standards, the further Austrian criticisms of representative agent models are interesting in their own right. This discussion will be brief, but it is important to recognize how fundamental the Austrian challenge really is.

Austrian methodology is solidly rooted in an analysis of the behavior of real people. One of the most basic characteristics of people is their wont to act capriciously. In order for us to be able to say that a person has a choice in how to act, it is necessary that a choice could have been made. If people followed deterministic rules in choosing, it would be meaningless to say they have a choice.

Representative agent models rule out capricious actions by people. They are aimed at generating decision rules which can be used to determine how people will act in any given situation. Such decision rules are the very antithesis of what Austrians mean when they assert that humans act. The Austrians deeply object to such reasoning: "It is characteristic of the formalistic style of thought that those who have imbibed it become incapable of conceiving of spontaneous human action, as distinct from reaction to outside events" (Lachmann, 1973, p. 22).

A related aspect of representative agent models that is unappealing to Austrian aesthetics is their assumption that all people are basically the same. Austrians place a heavy emphasis on the heterogeneity of people. The tendency to abstract from this heterogeneity is contrary to Austrian principles.

The assumption of rational expectations is, of course, not essential to the representative agent framework. However, it is certainly a dominant feature of new classical representative agent models. Austrian methodology categorically rules out the use of rational expectations. The rational expectations argument is that people act as if they knew the true model of the economy. It is irrelevant to new classical methodology that real people neither know how the real economy works nor do they know the values of all of the relevant variables. To the Austrians, this imputing of knowledge not held by real people to agents in our models is anathema. Hayek (1943, p. 60) writes, "[N]o superior knowledge the observer may possess about the object, but which is not possessed by the acting person, can help us in understanding the motives of their actions."

Finally, we should mention the Austrian rejection of mathematics and statistics in economic inquiry. New classical representative agent models are obviously rigorously mathematical in their structure. It is not the specific manner in which mathematics is used in any given representative agent model to which the Austrians would object; rather, it is the use of mathematics at all.

A complete examination of the Austrian rejection of mathematical and statistical methods in economic inquiry is far beyond the scope of this monograph. But to get a flavor for it, consider the following two remarks:

> As far as *precision* is concerned, consider, for example, the statements (2) *To a higher price of a good, there corresponds a lower (or at any rate not a higher) demand.* (2′) *If p denotes the price of, and q the demand for, a good, then*
>
> $q = f(p)$ *and* $dq/dp = f'(p) \leq 0$
>
> Those who regard the formula (2′) as more precise or "more mathematical" than the sentence (2) are under a complete misapprehension. . . . The only difference between (2) and (2′) is this: since (2′) is limited to functions which are differentiable and whose graphs, therefore, have tangents (which from an economic point of view are not more plausible than curvature), the sentence (2) is *more general,* but it is by no means less precise: it is *of the same mathematical precision as* (2′).
>
> (Menger, 1973, p. 41)

> The impracticability of measurement is not due to the lack of technical methods for the establishment of measure. It is due to the absence of constant relations. . . . Economics is not, as ignorant positivists repeat again and again, backward because it is not "quantitative." It is not quantitative and does not measure because there are no constants.
>
> s(Mises, 1966, p. 56)

The Austrian critique of mathematics and statistics is interesting in its own right, even if one finds it ultimately unpersuasive. Readers who wish to follow up on this argument should see Hayek (1952) as well as the treatments in Mises (1966) and Rothbard (1962).

In sum, the Austrian rejection of new classical representative agent models is broadly based. However, all the lines of objection are really an objection to the basic methodology being used. Austrians provide microfoundations by beginning with the individual and building up. The new classicals are working in the opposite direction. They begin with macroeconomics and attempt to build down. Indeed, the difference in the two approaches is seen in the word we have been using to describe them. "Microfoundations" is not a word created because it was needed to explain Austrian economics; in fact, from the Austrian perspective it is rather redundant. The foundations of economics are by definition in the microeconomic agents.

9

THE TRADITIONAL CASE FOR MICROFOUNDATIONS

INTRODUCTION

In the last chapter we examined the Austrian argument for grounding all of economics in microeconomics. It was clear that new classical representative agent models did not live up to Austrian standards. This, however, is really not all that shocking. To say that de Sade did not live up to puritanical standards of virtue would be rather banal; de Sade never intended to do so. Similarly, the new classicals never claimed to be Austrians, nor did they ever make the attempt to meet Austrian objections. Therefore, we cannot fault them for not using this methodology.

Nevertheless, new classicals constantly preach the virtues of microfoundations. In arguing that macroeconomics must be grounded in individual optimization, new classicals must mean something other than that economics should follow the Austrian method. It is the purpose of this chapter to explore exactly what these arguments are. What do the new classicals mean when they argue that we need microfoundations? In fact, our question is even broader. As we shall see, the new classicals claim to be following the path paved by traditional macroeconomists in attempting to provide microfoundations for macroeconomics. So our question really is: Why do the vast majority of economists advocate the establishment of rigorous microfoundations?

THE NEW CLASSICALS

That the new classicals are adamant in their insistence that all of macroeconomics needs to be grounded in individual optimization is easily established. Consider Lucas (1987):

> The most interesting recent developments in macroeconomic theory seem to me describable as the reincorporation of aggregative problems such as inflation and the business cycle within the general framework of "microeconomic" theory. If these developments succeed, the term "macroeconomic" will simply disappear from use

> and the modifier "micro" will become superfluous. We will simply speak, as did Smith, Ricardo, Marshall and Walras, of *economic* theory.
>
> (Lucas, 1987, pp. 107–8)

Or as Hoover (1988, p. 87) memorably put it, "The ultimate goal of the new classical economics is the euthanasia of macroeconomics."

But while we can establish that the new classicals are insistent on providing microfoundations, it is much harder to establish exactly why they think microfoundations are necessary. The passion with which new classicals deride purely macroeconomic exercises is indicative that there must be some deeply held belief that microfoundations are not merely desirable or "aesthetically pleasing" (Sargent, 1981, p. 215), but actually *essential.*

By far the most complete statement of the reasoning behind the microfoundations goal is Lucas and Sargent's "After Keynesian Macroeconomics" (1979). Their reasoning here is primarily historical. Prior to Keynes, the field of macroeconomics did not exist; all of economics was built up from the level of the individual. Keynes broke from this long tradition by arguing for the creation of models which were exclusively macroeconomic in character. The reason he deviated from the classical procedure is not irrelevant. Keynes, writing in the midst of the Great Depression, found the postulates of classical economics completely unable to explain the business cycle. In particular, Keynes saw no way to reconcile fluctuations in the aggregate economy with the propositions that agents always optimize and markets always clear. And thus was born macroeconomics.

Lucas and Sargent go on to note that from the time almost immediately after Keynes wrote, economists such as Hicks (1939) began the process of trying to provide microfoundations for Keynes' ideas. For several decades, these attempts were for the most part unsuccessful. The problem was not the caliber of the economists working on the problem; rather obviously, since they were some of the brightest minds in economics. Their lack of success was due to the same reason Keynes made the detour into macroeconomics in the first place, namely the lack of the appropriate tools. Now, we at long last have the necessary tools to explain aggregate fluctuations while retaining the classical postulates that agents optimize and markets clear. We can now explain macroeconomic relationships in terms of individual optimization. There is thus no longer any need to follow Keynes in his misguided attempt to found a new method of examining the macroeconomy. The Keynesian experiment will be viewed by history as forty years of wandering in the wilderness while technique was catching up to theory.

Upon reflection, this explanation of why microfoundations is necessary is rather unsatisfactory. Lucas and Sargent seem to be saying that their insistence on microfoundations is no different than that of the Keynesian economists who came before them. These Keynesian economists were also seeking microfoundations for Keynes' results; it is merely that the new classicals are better equipped to provide microfoundations than were their predecessors. All sides seem to recognize that Keynes' establishment of macroeconomics was unnecessary.

This argument begs the question. Why did the Keynesian economists try to establish microfoundations? What is wrong with following Keynes in studying the macroeconomy directly? Even if it were possible to explain aggregate fluctuations by starting with individual optimization, it does not necessarily follow that it is desirable to do so. Lucas and Sargent's case for microfoundations is ephemeral; there is no reasoning here which explains why microfoundations are *necessary* in order to study the macroeconomy. Thus, if we want to understand why microfoundations are believed to be necessary, we will have to cast our nets farther out, we will need to see why the new classicals' Keynesian predecessors began the search for microfoundations.

Before we make such a study, we need to look at another line of argument advanced by the new classicals, the seemingly omnipresent Lucas critique:

> But why should anybody want to interpret time-series data as representing the results of interactions of private agents' optimizing choices? . . . The reason for interpreting time series in this way is practical: potentially it offers the analyst the ability to predict how agents' behavior and the random behavior of market-determined variables will each change when there are policy interventions or other changes in the environment that alter some of the agent's dynamic constraints.
>
> (Sargent, 1981, p. 215)

In other words, in light of the Lucas critique, it is only by studying economics at the level of the individual that we can get accurate predictions (cf. Lucas' remarks in Snowden *et al.,* 1994, p. 221).

Now, we really cannot take this argument at face value. It is possible to get exactly the same predictions from a purely aggregate model that we get from a microfoundational model. For example, take the new classicals' most celebrated case of the Phillips curve. New classical microfoundational models predict that there is no permanent trade-off between inflation and unemployment. However, it is not difficult to write down a purely macroeconomic model which yields exactly the same result. Or take the policy ineffectiveness result. This result can also be derived in a purely macroeconomic model; in fact, Sargent and Wallace (1975) did it.

Sargent's statement cannot be interpreted as saying that it is only by beginning at the level of individual optimization that we can study the effects of regime changes; this statement is clearly not true. It is best interpreted as saying that it is more desirable to begin with individual optimization; that for some reason models which are based on individuals are better than those which are not. But this still does not explain why such models are better.

THE MICROFOUNDATIONS TRADITION

The new classicals have not provided a satisfactory rationale for the perceived need for microfoundations. Rather, they seem to rely on the rationale provided by their predecessors. As Mayer (1993c, p. 81) notes, “Long before the rise of new classical economics it was generally agreed that macroeconomic propositions need *some* microeconomic foundations – I remember teaching this in the late 1950s.” If the new classical economists are simply relying on these earlier beliefs, it is no wonder that they do not offer up a new argument for microfoundations. There is no need to reinvent the wheel after all. So we now turn our attention to the broader field. What reasons have other economists offered for the necessity of microfoundations?

There have been several attempts to motivate the microfoundations literature by arguing that it is part of the larger attempt to develop richer general equilibrium models. For example, Weintraub (1979) argues that the modern attempt to provide microfoundations for macroeconomics began with Hicks’ *Value and Capital* (1939), was further developed in Lange’s *Price Flexibility and Employment* (1944) and Klein’s *The Keynesian Revolution* (1947), and finally hit its apogee in Patinkin’s *Money, Interest and Prices* (1956). Weintraub dubs these works the neo-Walrasian model and argues that they are attempts to develop what would later be called the Arrow–Debreu general equilibrium model into something that can explain the macroeconomic phenomena that concerned Keynes. Furthermore, Weintraub traces how Keynes’ concern with macroeconomic phenomena was subsequently downplayed as the neo-Walrasian model was developed. (See also Janssen, 1990, 1993.) While these historical studies have some interest in their own right, they do not answer our question here. The desire to have better general equilibrium models does not necessarily imply that we cannot understand the macroeconomy without providing microfoundations. In other words, why should the development of general equilibrium prohibit a separate study of macroeconomics?

When we turn our attention to the broader literature, we run into the same problem we had when examining the new classical literature. While it is easy to find statements that microfoundations are necessary or desirable, it is difficult to find out why. Consider the following fairly representative justifications for the necessity of microfoundations. Klein argues:

> Many of the newly constructed mathematical models of economic systems, especially the business-cycle theories, are very loosely related to the behavior of individual households or firms which *must* form the basis of all theories of individual behavior.
>
> (Klein, 1946, p. 93, emphasis added)

Similarly, Allen argues:

> Macro-economic models can be set up in their own right. . . . However, this cannot be satisfactory to an economist, conscious that relations between aggregates are the

> resultant of many decisions by consumers and firms. It is a natural wish to go behind the macro-relations, to see how individual decisions lead to stable relations in the aggregate – if indeed they do so at all.
>
> (Allen, 1963, p. 694)

These statements seem to provide a rationale for microfoundations, but they do not. They do not explain why macro *must* be based on individuals and firms. To say that it is not satisfactory to have purely macro models is not to say that it is improper. While Allen may not be content with a purely macroeconomic model, others may be. There is nothing here which should convince a person content with pure macroeconomics that his preferences are irrational or misplaced.

When we get right to the heart of the matter, it seems that most economists would agree with Hahn (1973, p. 36): "The view that macro-economics is in some sense essentially different from other kinds of economics in dealing with relations that are not deducible from the actions of agents I do not deal with, since it is rather obviously false."

It makes sense that all action comes from the activities of individuals. The price level is not some autonomous creature that rises of its own volition; rather, individual people raise individual prices. It thus strikes most economists as rather obvious that all of macroeconomics can and should be provided with rigorous microfoundations. With the idea that microfoundations are unnecessary being "rather obviously false," it hardly seems required to actually defend the proposition.

The fact that one cannot understand macroeconomics without providing microfoundations thus seems to have become one of the unspoken assumptions held by economists. There is no more need to justify the aim of microfoundations than there is a need to justify the utility-maximization hypothesis. Both assumptions are part of the basis of all economic theory. Nowhere does the fact that the microfoundations goal has become one of the basic assumptions of economics show up more clearly than in a conference on *The Microfoundations of Macroeconomics* (Harcourt, 1977). This volume contains eleven papers and accompanying discussions, all of which are devoted to the topic of microfoundations. However, as Nell points out at the end of the conference (pp. 392–3), in all these presentations and responses there is no discussion of why microfoundations are desirable. That they are desirable is merely assumed throughout.

Boland (1982) has also attempted to explain the rationale for insisting on microfoundations for macroeconomic work:

> From the viewpoint of methodology, we need to examine the reasons why methodological individualism is a main item on the neoclassical agenda. Unfortunately, the reasons are difficult to find, as there is little methodological

> discussion of why economics *should* involve only explanations that can be reduced to the decision-making of individuals – except, perhaps, for Hayek's [1937, 1945] arguments for the informational simplicity of methodological individualism.
>
> (Boland, 1982, p. 28)

Boland also argues that an insistence on methodological individualism is one of the two "foundations of neoclassical economic methodology" or "the hidden agenda" of neoclassical economics (ibid., p. 8).

According to Boland, an insistence on the necessity of microfoundations arises out of the problems of having two competing theories to explain the same set of observations. If the two theories are allowed to exist as competitors, there is a fear that a "life-or-death" struggle between the two will ensue, which would be distasteful to most economists. So, to avoid the life-or-death struggle, economists either (a) demonstrate that the two theories are really the same, that the differences are merely superficial; or (b) split the discipline into two distinct compartments.

Turning to the matter of macroeconomics, Boland presents a "rational reconstruction" *à la* Lakatos (1971) of the accommodation of Keynesian macroeconomics. He argues that this accommodation was founded on the premise that macroeconomics "must be not more than an aggregation of microeconomics" (Boland, 1982, p. 90). Thus, the insistence that microfoundations must be provided for all macroeconomics is a means of avoiding the life-or-death struggle between macroeconomics and microeconomics over which theory is the true representation of reality; by showing that they are logically the same theory, that macroeconomics is just microeconomics, conflict is avoided. (Weintraub, 1979, p. 5, makes a similar contention in passing.)

There is, however, no direct evidence that Boland's hypothesis is correct. Indeed, it is hard to imagine what direct evidence on such a proposition could even exist. In fact, as Lakatos himself noted, such a rational reconstruction of history may be a different thing altogether from the actual history. For example, Lakatos (1971, p. 107) argues, "One way to indicate discrepancies between history and its rational reconstruction is to relate the internal history *in the text,* and indicate *in the footnotes* how actual history 'misbehaved' in the light of its rational reconstruction." (Boland does not provide the footnotes for his rational reconstruction, presumably because the actual history in this case seems to be unrecorded.) Thus, at best, Boland has presented an intriguing hypothesis about the inner workings of economists' minds.

However, as an explanation of *why* microfoundations are necessary, Boland's hypothesis is insufficient; even if we accept that there is some desire to avoid a life-or-death struggle between microeconomics and macroeconomics, we do not have an explanation about why the insistence on microfoundations is chosen as the conflict-avoidance measure. Economists could have instead adopted Boland's second conflict-avoidance measure:

compartmentalize the subject. The life-or-death struggle is avoided if economists simply assert that microeconomics is a body of knowledge designed to explain the microeconomy and macroeconomics is a body of knowledge designed to explain the macroeconomy. It could have been left there.

Boland argues that the compartmentalization option is inadequate, that it is only a "temporary measure" (Boland, 1982, p. 89). Even if that were true, it doesn't explain why "temporary" has already passed us by. How long is "temporary" in this context? We can safely drive on a temporary bridge for quite some time; knowing that the bridge is only "temporary" does not mean we must insist that the permanent bridge be built before we go visit the in-laws.

So while Boland may well be accurately describing the underlying motives for many economists' psychological need to see the microfoundations, it does not provide an explicit rationale for insisting on microfoundations beyond "It must be so."

Now even if we can agree that microfoundations are not strictly *necessary* for macroeconomics, that it is conceivably possible to do interesting research in macroeconomics without providing or attempting to provide microfoundations, it does not follow that new classicals are wrong in insisting that microfoundations should be provided. It is possible that while microfoundations are not *necessary,* they are *desirable,* maybe even highly desirable. So, putting aside questions of necessity, why would anyone even want to have microfoundations?

This question has received as little debate as the question of why microfoundations are necessary. Again, the desirability of microfoundations seems to have passed into the collective unconsciousness of economists. Consider this defense of their desirability:

> All the same, microeconomic foundations, while not strictly necessary, are desirable. Not only would firm microfoundations help with respect to the Lucas critique, but they would also enhance the predictive success of economics in general, since it seems plausible that the underlying relationships are stabler than the observed macro regularities (cf. Janssen, 1993, p. 59). Moreover, they can prevent careless errors and deficient analysis.
>
> (Mayer, 1995, p. 30)

The sentiment that microfoundations are desirable here is irrefutable; some people think microfoundations are desirable, so they are desirable to some people. However, the arguments for their desirability are on examination quite weak, which all in all may be the point – if it is self-evident that microfoundations are desirable, then all explanations of their desirability will seem weak. Consider the three arguments offered by Mayer. First, microfoundations help avoid the Lucas critique; but, as we showed in Chapter 4, what are generally considered to be good microfoundations may do little to help with regard to the

Lucas critique. Second, it is plausible that in some cases underlying relationships are more stable than observed macroeconomic regularities; however, it also seems plausible that in some cases the observed macroeconomic regularities are more stable than the relationships we believe to be underlying them. At best then, we have a reason for why microfoundations are desirable *sometimes*. And finally, the argument that microfoundations may help prevent careless errors and deficient analysis is belied by the example Mayer provides in a footnote at the end of the passage quoted. Mayer notes that the government budget constraint has been ignored in both informal macroeconomic models and in formal models (Mayer, 1993c, p. 36; cf. Mayer, 1990, ch. 11). Thus, such explanations would scarcely convince someone skeptical about the desirability of microfoundations.

WHAT ARE MICROFOUNDATIONS ANYWAY?

Neoclassical economists do believe in the need for microfoundations, but their perception of them differs radically from that of the Austrians. In one sense, mainstream microfoundations are not as deep as those of the Austrians. The Austrians are adamant that all knowledge must be derived from the most basic principles, that all economic inquiry must begin with an analysis of the individual. The idea of studying aggregate quantities in an attempt to derive macroeconomic laws is misguided and fruitless. Neoclassical economics is not nearly so adamant; while it asserts that we can never truly understand macroeconomic relationships until we understand the microeconomic behavior which generates them, there is no denial of the idea of aggregate relationships. The neoclassical microfoundations goal is to explain macroeconomic relations in terms of microeconomic behavior; to do so, it is obvious that such macroeconomic relationships must exist.

So what are microfoundations anyway? In this chapter we have been searching in vain for a cohesive, rigorous defense of the proposition that microfoundations are necessary or even desirable. In the absence of an argument for why we want microfoundations, is there at least agreement about what constitutes microfoundations? Oddly, there may not be; in actuality, while there is widespread agreement that microfoundations are at least desirable and may even be a necessity, there is wide range of opinion about what a microfoundational model looks like.

As a quick test, what is the answer to this question? Does Friedman's work on the permanent income hypothesis provide microfoundations for an aggregate consumption function or not?

Friedman (1957) begins his analysis by considering a consumer maximizing utility over two periods; the consumer receives some receipts, R, in each year and must use these receipts to finance consumption in the two periods. The consumer can save or borrow across time periods at the interest rate, i. In this simple world, the consumption decision is based on only two variables: the slope of the intertemporal budget line (which is the interest rate, i) and the position of the budget line (which is determined by the consumer's wealth in period 1:W1 = R1 + R2/(1 + i)). So Friedman argues we can write the consumption function as

$$c_1 = f(\mathrm{W}_1, i) \tag{9.1}$$

Friedman goes on to argue that, on a theoretical level, income can be defined as the amount that a person could consume and still maintain his current level of wealth; Friedman dubs this level of income, permanent income. Consumption, then, is stated to be a function of permanent income, or

$$c_{p1} = g(yp1, i) = g(i\mathrm{W}1, i) \tag{9.2}$$

Thus, the permanent income hypothesis is born.

Now note that we have already hit an incongruity in the development of the hypothesis. In the first part of the story, the consumer consumes all of his wealth in the two periods; in the second part of the story, the consumer consumes only the interest income from his wealth, leaving the wealth extant at the end of period 2. Friedman is not unaware of this change; in fact, he notes that the equation with permanent income "seems somewhat forced" (Friedman, 1957, p. 11) in the two-period case. However, he simply argues that we should consider the permanent income equation as a "generalization" from the two-period case to a longer lifetime. Thus, the permanent income hypothesis is *not* derived from a utility maximization problem even for the individual consumer; rather, it is a rough "generalization" from a simple two-period world.

Of course, Friedman is not simply interested in the individual consumer's decision; the real point of the exercise is to get an aggregate relationship. Friedman notes that the specific relationship between consumption and permanent income will vary; the relationship will depend on the interest rate, i, the ratio of nonhuman wealth to permanent income, *w,* and "utility factors," *u.* So:

$$c_p = k(i, w, u)y_p. \tag{9.3}$$

Since the function $k(\,)$ will vary across people, Friedman argues that we cannot simply assume that the same function holds in the aggregate. However, if the distribution of permanent income across consumers is unrelated to the distribution of *i, w,* and *u* across consumers, then there will be some *other* relationship between aggregate consumption and aggregate permanent income:

$$c^*_p = k^*_p(i,w,u)\, y^*_p \tag{9.4}$$

Finally, Friedman notes that it is unreasonable to assume that the distribution of permanent income across consumers is independent of the distribution of the arguments in $k(\,)$, that such things are obviously correlated with income. Nonetheless, Friedman argues that equation (9.4) is still a useful "approximation."

Thus, by the time Friedman gets to the aggregate consumption function, we have an approximation of the altered aggregate version of a rough generalization of a simple, two-period model. Moreover, the parameter values in the macroeconomic model will not be the same as those from the microeconomic model and must be determined by studying aggregates.

Is this work, then, an example of providing microfoundations for macroeconomics or is it the old-style macroeconomic theorizing for which we need to provide microfoundations? While it would be hard to document, I would guess that the old-style Keynesians who argued that economics needs a microeconomic foundation would argue that this is exactly the sort of thing about which they were talking when they said that macroeconomics needs to be grounded in microeconomics.[1] The simple Keynesian consumption function in which aggregate consumption depends solely on aggregate income is seriously incomplete and needs this sort of microeconomic discussion of the effects of permanent versus temporary changes in income to flesh out the details. On the other hand, I would guess that new classical economists would point to Friedman's work as an example of the old-style macroeconomic work that sorely needed microfoundations.[2] Friedman's work is simply theorizing about aggregates and not grounded in any rigorous utility-maximization problem. We needed Hall (1978) and the work that followed to provide some microeconomic foundations to this literature.

Noting this distinction between the standards of microfoundations clears up much of the confusion that has arisen out of the new classical insistence that rigorous microfoundations must be provided. Consider these remarks from Hahn and Solow:

> [The new classical macroeconomics'] essential characteristic is *not* that it "pays attention to micro foundations." As many people have noticed, macroeconomics has always done that, at least in the sense that aggregative relationships have always been explicated and justified by reference to microeconomic behavior.
>
> (Hahn and Solow, 1995, p. 1)

We seem to have a case of jargon hiding an essential message. The new classicals came along and argued that all the old-style Keynesian stuff was wrong because it didn't provide microfoundations for macroeconomics. The Keynesian response was (and is), "But we do have microfoundations." There was thus no dispute over the need for "microfoundations"; however, there should have been. When the new classicals said that old-style Keynesian macroeconomics had no microfoundations, they meant it – it did not provide what they believed to be acceptable microfoundations. When the Keynesians replied that they do have microfoundations, they also meant it. Nobody seems to have noted that the word "microfoundations" was being used in a fundamentally different way by the participants in the discussions. So when Hahn and Solow assert that macroeconomics has *always* provided microfoundations, old-style macroeconomists nod their heads while new classicals scoff.

This answers a matter that puzzled us earlier in the chapter. Why are all the rationales for the necessity or desirability of microfoundations that are offered up by old-style macroeconomists so fuzzy? Simply because the standards for microfoundations were so fuzzy. Saying that a model had microfoundations in the older sense of the word seems to have meant nothing more than saying that the model was not just some observed statistical correlation between two aggregates. If you can provide an explanation or story, then you have microfoundations.

However, when the new classicals came along and said that macroeconomics needed microfoundations, they meant a very different thing. In the new classical arguments, microfoundations is shorthand for a model in which the starting point is a utility- or profit-maximization problem. This is an entirely different creature. Lots of papers that provided microfoundations in the old sense did not provide microfoundations in the new sense.

This is more than just a discussion of semantics. Recall Lucas and Sargent's explanation of the new microfoundations. Their primary argument for microfoundations was that all the older economists wanted it, but they didn't have the great tools we modern economists have. In essence, the new classical economics is just the fulfillment of the dream. On these grounds we earlier excused the new classicals for not providing a complete defense of microfoundations. However, all the old economists didn't want it. The old economists wanted something completely different. The new classical research program was not the fulfillment of the dream, but the destroyer of it.

By changing the definition of microfoundations, the new classicals made moot the old arguments for its necessity or desirability. If you want all models to start with a utility-maximization problem, it doesn't matter whether or not the old Keynesian economists liked to have some story attached to the model. The new classical macroeconomists needed to provide a defense for the necessity of providing microfoundations in the new sense. This they did not do. What we now have is an advocacy for microfoundations with bite based on fuzzy arguments for toothless microfoundations.

WHERE DOES THE REPRESENTATIVE AGENT FIT IN?

The differences in the possible aims of microfoundations are crucial to an understanding of the role of the representative agent. We saw in the last chapter that if the aim of microfoundations is taken to be that of the Austrians, then representative agent models are not satisfactory. But when new classicals argue for microfoundations, they are arguing for something much less stringent than the Austrian ideal; there is no reason to presume that because representative agent models have no role in Austrian methodology, they are improper in new classical work. On the other hand, when the new classicals argue for microfoundations, they are arguing for something much more stringent than the old-style Keynesian ideal. Examining whether the representative agent model fits into new classical microfoundations is our next task.

The following chapters will examine the microfoundations debate from various angles.

Chapter 10 looks at the problem of aggregation. Suppose we know with certainty what the microeconomic agents look like: under what conditions can we use this information to derive stable, consistent aggregate equations? It turns out that the requirements for such an occurrence are extremely strong.

Chapter 11 asks the question: Are the microeconomic and macroeconomic problems conceptually similar? In other words, if we have a well-formulated microeconomic relationship, do we also have a well-formulated macroeconomic relationship? The answer: Not necessarily.

Chapter 12 is in some sense the heart of this section. The new classicals have argued that we need microfoundations for macroeconomics. Their method of providing microfoundations has been representative agent models. This chapter asks: If we want microfoundations for macroeconomics, can we use representative agent models? We will argue that in general representative agent models do not provide microfoundations.

Chapter 13 probes deeper and asks: Is the whole microfoundations goal really necessary or desirable? We will answer that it is not.

10

THE AGGREGATION PROBLEM

THE GENERAL PROBLEM

The goal of microfoundations is to explain aggregate relationships in terms of individual behavior. In contrast, the traditional macroeconomic model goes about describing the economy by explaining some aggregate variables in terms of other aggregate quantities; e.g., aggregate consumption is described as a function of aggregate income. The macroeconomic approach implicitly assumes that only the aggregate amounts matter, that the distribution of these quantities among the microeconomic agents is irrelevant. If aggregate income is $6 trillion, then aggregate consumption is, say, $4 trillion, regardless of how the $6 trillion is allocated among the population.

The rationale for this macroeconomic modeling is often very simplistic. In microeconomics we see prices and quantities and draw lines through them. Why shouldn't we do the same for the macroeconomy? After all, we have aggregate price and quantity data; why shouldn't we exploit these data by writing down equations that relate them, playing a sort of sophisticated game of connect-the-dots? The answer is that, while we can of course draw such lines, the real question is whether these lines mean anything at all. The problem is that the lines drawn through aggregate points may not represent anything.

The traditional macroeconomic model suffers from a notable problem, namely the aggregation problem. In order to have a well-defined macroeconomic relationship, we need to assume consistency. Consistency exists when the use of more detailed information than the aggregate value makes no difference in the analysis. One immediate implication is that the distribution of the aggregate is irrelevant.

Except in the most perverse and degenerate of worlds, aggregate consistency does not exist. This result is rather old and well established. The literature on the aggregation problem is large but not particularly diverse. Moreover, for those who hope to get around the aggregation problem, this literature is unrelentingly depressing.

The existence of these aggregation problems is undoubtedly one of the primary tangible reasons why some economists profess a need for microfoundations. If stable macroeconomic relations are difficult or impossible to come by, then it would seem that our only recourse is to develop microeconomic models that can explain macroeconomic phenomena.

With their microfoundational aims, representative agent models may seem to get around the aggregation problem. Sargent (1978, p. 1016n) specifically argues for a benefit in terms of aggregation: "Assuming a representative firm is only a convenience, as the model admits a tidy theory of aggregation." Similarly, Snowden *et al.* (1994, p. 265) note, "Real business cycle theorists sidestep the aggregation problems inherent in macroeconomic analysis by using a representative agent whose choices are assumed to coincide with the aggregate choices of millions of individuals." If we understand the activities of every single agent in the economy, the aggregation problem vanishes as there is nothing to aggregate. Representative agent models seem to be a simplified manner of modeling the actions of all agents.

But representative agent models do not bypass the aggregation problem; in fact, they suffer from *exactly* the same problems as traditional macroeconomic models. In general, it is impossible to provide a consistent model of the macroeconomy by using a representative agent model.

Insofar as the goal of microfoundations is to begin at the individual level, derive an accurate representation of the macroeconomy, and thereby bypass the aggregation problem, representative agent models do not provide microfoundations.

What follows is an elaboration of the problem of aggregation. We will try to get a grasp of the depth and breadth of the problem in a fairly nontechnical discussion. Our goal is to describe the aggregation literature, not to re-derive it all. For those who are interested in further detail, there already exist several good technical summaries of the literature; see, for example, Green (1964), Daal and Merkies (1984), and Stoker (1993).

THE BASIC THEORETICAL RESULTS

The gist of the aggregation problem can be seen in a simple example. Suppose we have a world of exactly two people, Pip and Joe, who have the rather simplistic consumption functions: $C_P = 0.8Y_P$ and $C_J = 0.4Y_J$. Our desire is to define a representative agent with consumption function $C = mY$ who will accurately show the relationship between aggregate consumption and aggregate income.

Initially, both Pip and Joe have income of \$100, meaning that aggregate income is \$200 and aggregate consumption is \$120. So our representative agent receives income of \$100 (= \$200/2) and consumes \$60 (= \$120/2), so that $m = 0.6$. If we had two representative agents with the consumption function, C = 0.6Y and income of $100, we would predict an aggregate consumption level of $120, which, of course, is exactly that in the real world of Pip and Joe.

So far, so good – but no further. The representative agent function is not stable. We can see this in two ways. First, take $50 from Pip and give it to Joe. Since aggregate income is unchanged, our representative agent model predicts consumption will still be $120. Aggregate consumption has actually changed to $100. Similarly, consider adding another $100 of income to the economy. The representative agent model predicts that consumption will rise to $180. However, true aggregate consumption can range from $160 (if Joe gets all $100) to $200 (if Pip gets it all). Clearly, there is something wrong with our representative agent.

Obviously, the above model is terribly simple. The problems with the representative agent model in our simple example do not go away as the world gets more complex; rather, they get worse.

Under what conditions will we be able to derive a consistent representative agent (or equivalently a macro model)? The most important conditions are in Gorman (1953).[1] If all agents have parallel, linear Engel curves, or equivalently, if all agents have identical homothetic preferences, consistent aggregation is possible. The requirement that the Engel curves be parallel means that the marginal changes are the same for all agents; e.g., if we take $1 away from Pip and give it to Joe, Pip's consumption falls by exactly the amount that Joe's rises. Unless the agents' Engel curves are parallel, a redistribution of income from one agent to another will alter aggregate consumption while leaving aggregate income unchanged. The requirement that the Engel curves be linear means that the marginal changes are the same for all levels of income. If we did not have linearity, a redistribution of income between poorer and richer individuals would alter consumption while leaving aggregate income unchanged, even if both agents had the same nonlinear Engel curve. Thus, what we might call the Gorman condition for consistent aggregation is that marginal changes must be the same for all agents at all levels of income. The Gorman condition is very stringent and incredibly implausible.

Since Gorman wrote, there have been several papers providing additional conditions which will yield consistent aggregation; see, for example, Green (1964), Stoker (1984), Muellbauer (1975, 1976), Deaton and Muellbauer (1980), Lau (1982), Jerison (1984) and Lewbel (1989). There is no real need for us to explore all of these conditions in detail. It is enough to note that every one of them is thoroughly implausible; it would be remarkable in and of itself if anyone argued that any of these functional forms was in any way realistic. Indeed, Lewbel (1989, p. 631), after an entire paper devoted to developing general forms which allow for aggregation, concludes, "It is a fact that the use of a representative consumer assumption in most macro work is an illegitimate method of ignoring valid aggregation concerns."

In sum, the conditions for consistent aggregation are so severe that we can safely say that they do not hold in reality. This has long been recognized about macroeconomic equations.

What seems to be less recognized, at least in the macroeconomic literature, is that there is nothing in the representative agent framework which in any way alleviates the problems of aggregation. A representative agent model starts out with a single agent and derives assorted relationships among aggregate variables. For these aggregate relationships to be consistent, it is not sufficient for the representative agent to be representative in the sense of being some sort of average individual. Rather, the representative agent must be representative in the sense that every single agent in the economy has identical preferences. If all agents are identical, the representative agent model will give consistent results. However, if all agents were identical, we wouldn't need a representative agent model at all; it would be a simple matter to model each agent separately, since quite literally when you've seen one agent, you've seen them all. If one of the purposes of microfoundations is to bypass the aggregation problem, representative agent models fail to meet the standard.

EXTENSIONS

There are a large number of extensions of the basic aggregation literature. Below, we summarize several of the more interesting. Throughout, our focus is on those aspects that bear on our ability to use representative agent models to study macroeconomics.

Formal measure of aggregation bias

Realizing that the conditions for exact aggregation are so stringent that they cannot be assumed to hold, the inevitable question is: What happens if they don't hold and we use aggregate equations anyway? In other words, how bad is aggregation? In this form, the question is obviously nonsensical; it is like asking: How bad is specification error? The answer depends on the situation being analyzed and the method of analysis. In some cases aggregation error will be large; in others, small.

The largest problem in assessing the size of aggregation error is the inability to measure it. Theil (1954) presents a statistical measure of the aggregation bias from using an aggregate model when the conditions for exact aggregation are not met. The bias comes from correlations among parameters in the microeconomic equations. For example, a relationship between an agent's marginal propensity to consume and his share of aggregate income will cause bias.[2]

It is very difficult to get empirical measures of Theil's aggregation bias since, in order to do so, we must know the true microeconomic model exactly. An error in our specification of the microeconomic model will alter our measure of aggregation bias. There are two noteworthy attempts to gauge aggregation bias empirically by using Theil's measure. The first is an analysis of investment by Boot and de Wit (1960) which finds aggregation bias to be "relatively small." Conversely, Gupta (1969, p. 72) looks at labor market statistics and finds that aggregation bias "can be serious and lead to disturbing results." Neither of these studies should be thought of as decisive since both are hampered by their need to assume that least-squares estimates of the microeconomic relations are, in fact, the true relations. Insofar as this assumption does not perfectly hold, the results of their analyses could be altered.

Simulation exercises are not a way of getting around the specification problems to try to get some idea of the size of aggregation bias. The very method by which the simulated model is constructed will determine the size of the aggregation bias that will be found. We could easily construct simulation exercises that would show that aggregation bias is huge and others that would show it is trivial. The only way to determine which of these simulated exercises is closest to the truth is to compare the microeconomic equations in the model with those of reality. But in order to make this comparison we need to know the true microeconomic model – and if we knew that, there would be no need for the simulation.

Slightly disaggregated aggregates

For now, let us assume that none of the restrictive functional forms needed for perfect aggregation hold in reality – which is not exactly a great leap of faith. In this case, the distribution of aggregates among the population matters. Recall our earlier example of Pip and Joe; changes in the distribution of income between the two caused changes in the aggregate equation. It is exactly the same to say that there are heterogeneities or nonlinearities at the level of the individual as it is to say that there are distributional effects in the aggregate.

We can thus be certain that there are distributional effects in any work involving aggregates. The problem is much worse than this simple fact seems to imply. Stoker (1986b) has convincingly demonstrated that these distributional effects are difficult to capture in empirical work. The presence of distributional effects in macroeconomic equations is statistically indistinguishable from dynamic autocorrelation in macroeconomic variables. In other words, if we measure dynamic effects in a macroeconomic model, we may really be measuring nothing more than moderate changes in distribution in a static world. There is no way to distinguish between these possibilities at a macroeconomic level.

We can use Stoker's result to reach an important conclusion: it is impossible for a representative agent model to meet the standards of the Lucas critique. We know that virtually all government policies have some effect on the distribution of income. (Skeptics, if any, could consult Davidson and Kregel, 1989, for a host of examples.) We further know that changes in the distribution of income will change aggregate equations. The conclusion is obvious. Any regime change that alters the distribution of income will cause a representative agent model formulated in the previous regime to break down. This is true regardless of how deep the parameters in the representative agent model are. Stealing a line from Sargent (1981, p. 216) (who obviously was not applying it to representative agent models, but to Keynesian macro models), "There is a theoretical presumption that historical econometric estimates of such decision rules will provide poor predictions about behavior in a hypothetically new environment."

The Stoker results might lead us to hope that this situation could be remedied by including distributional variables in our models. Stoker creates such a model which works reasonably well; the distributional variables do capture the relevant effects. Buse (1992) finds similar results using different data and model specifications. However, other work in this area has yielded mixed results. For example, Sheffrin (1984) – who wrote before Stoker, but built upon similar work by Lilien (1982) – uses a dispersion measure to capture the effects of differential income growth in different regions. He gets what he calls "economically significant" results. Similarly, Fair and Dominguez (1991) include age distribution variables in several models on the reasonable assumption that different age cohorts cannot be assumed to have identical marginal reactions to changes in economic variables. They get mixed results; the age distribution variables matter in some, but not all, cases.

Differences between the aggregate and the agents

Xu (1991) provides some evidence that the types of aggregation problems we are discussing here could have significant implications in empirical work. The purpose of Xu's paper was to test, via a simulated economy, the rational-expectations/permanent-income hypothesis as formulated in Hall (1978).

The economy in Xu's paper is an overlapping generations model in which each agent lives for 40 periods, and a new generation is born every period for 200 generations. Agents' income is subject to both a common macroeconomic shock and an individual shock. Consumption is determined by a standard rational-expectations/permanent-income rule. Finally, there are different simulations run for the case when consumers are liquidity constrained and when they are not.

As is well known, Hall (1978) predicts that consumption will be a random walk. Xu tests the alternative hypothesis that consumption depends on past income variables versus the null hypothesis that consumption is a random walk. Mathematically, the process

$$c_t = \beta_0 c_{t-1} + \beta_1 y_{t-1} + \beta_2 y_{t-2} + \beta_3 y_{t-3} + \beta_4 y_{t-4} + v_t$$

is tested against the null hypothesis

$$c_t = \beta_0 c_{t-1} + v_t.$$

Xu's results are remarkable. In the case where consumers are not liquidity constrained, Xu finds that the consumption of a majority of agents does follow a random walk, confirming Hall's hypothesis. From this result, it is natural to assume that a representative agent model, using aggregate consumption and income, would also follow a random walk. In fact, the opposite occurs. In the aggregate, consumption depends on lagged income variables.

In the simulations where consumers are liquidity constrained we get equally paradoxical results. At the individual level, consumption depends on lagged income, which is not all that surprising given the liquidity constraints. Again, we might presume that the aggregate results would exhibit similar behavior. In fact, in this case aggregate consumption is a random walk.

We can summarize these results succinctly: In the unconstrained case, individual consumption is a random walk, but aggregate consumption is not; in the constrained case, individual consumption is not a random walk, but aggregate consumption is.

How do Xu's simulations give rise to these paradoxical results? For the unconstrained case, Xu suggests that the difference between the aggregate and individual behavior could be due to distributional effects *à la* Stoker, resulting from changes in wealth as consumers get older. For the constrained case, the acceptance of the null hypothesis could be due to high collinearity between consumption and current income. Regardless of the reasons for Xu's paradoxical results, the very fact that these results arise gives us reason to pause before assuming that individual behavior will aggregate to identical macroeconomic behavior.

Attanasio and Weber (1995) get a similar result. In their simulations of overlapping generation models, the Euler equation does not necessarily hold at the aggregate level. They argue that this result is due to aggregation bias arising from the existence of finite lives and the lack of complete markets.

These results are particularly remarkable when we consider that all agents are identical in every way except their date of birth. Thus, we can run into aggregation problems simply because not all people are born on the same day.

Information aggregation

We noted earlier that the conditions on the objective functions necessary to allow for perfect aggregation are totally implausible. These results are bad enough for anyone wanting to formulate any representative agent model. However, the new classicals do not want to form just any representative agent model; invariably, they formulate models in which the representative agent has rational expectations. For this class of models, the problems are even worse.

Goodfriend (1992) gives a striking example of the effect of aggregating information. Goodfriend's basic framework is Hall's rational-expectations/ permanent-income model. However, he changes the income process; agents receive income:

$$y_t^i = \left(\frac{1}{n}\right)Y_t + v_t^i \tag{10.1}$$

where, y^i is agent i's income, Y is aggregate income, n is the number of agents, and v^i is a relative income component. By definition $\Sigma v^i = 0$. Thus, equation (10.1) says that a given agent receives $(1/n)$ of aggregate income plus or minus some number representing his relative income. There are innovations to v^i each period of magnitude u^i, which allows for changes in agent i's share of aggregate income over time. Furthermore, aggregate income is some ARMA process with random shock ε_t.

An agent's income is thus subject to two shocks each period: first, his income changes by his share of the aggregate shock, $(1/n)\varepsilon_t$; second, his income changes with the shock to his relative income component, u^i_t.

If agents have full knowledge of the magnitude of these shocks every period, then this income process does not alter the Hall result; i.e., consumption is still a random walk. But strange things begin to happen when we alter the information process a little.

Suppose that in each period agents only see the cumulative shock to their individual incomes. They will not know the shock to aggregate income in period t until $t + 1$. In this case, agents face a signal-extraction problem. They will not know how much of the shock to this period's income is due to the aggregate shock and how much is due to their relative income shock. So agents must make their consumption decisions in this period based upon a guess about how much of their current income change is due to the aggregate shock and how much is due to the individual shock.

There are two components to the change in an agent's consumption each period: first, agents change their consumption as a result of the current period shock; second, agents may change their consumption upon the discovery of the size of the aggregate shock last period.

This second change needs elaboration. Suppose that, if an agent knows that his share of aggregate income changes by \$1, then he will change his consumption by α. If agent i knows there is a \$1 change in v^i, he will change his consumption by β. There is no reason that α and β must be identical since the magnitude of each of these variables depends on the permanence of each type of shock.

If α and β are equal, then the signal-extraction problem goes away. A \$1 change in income, regardless of its cause, results in the same change in consumption. However, if α and β are different, then agents will change their consumption by a different amount for the different causes of a change in income.

For concreteness, let us take the case where $\alpha > \beta$. An agent gets a \$1 shock to income this period. He attributes part of this to an aggregate shock, say 60 percent, and the rest to the relative shock, so that he changes his consumption by $0.6\alpha + 0.4\beta$. In the next period, the agent discovers the true size of the aggregate income shock. If the aggregate income shock accounted for more than 60 percent of the total, the agent did not raise his consumption sufficiently last period, and so must raise consumption even more this period. On the other hand, if the aggregate shock was really less than 60 percent of the total, then the agent raised his consumption too much and will have to lower it this period. These changes in consumption are in addition to any changes due to the current period shock.

When Goodfriend looks at this model, he finds a remarkable result. Individual consumption is a random walk, exactly as predicted by the Hall model. However, in the aggregate, consumption is not a random walk, but rather an AR(1). We thus have an aggregate result that is different from the result of every single agent in the economy.

How did this happen? It is solely due to aggregation. For the individual there are two sources of error. Suppose income rises in a given period. If this rise in income is disproportionately due to a rise in aggregate income, then the agent will not increase consumption by enough (still assuming $\alpha > \beta$), and will raise consumption even more next period. Here, next period's consumption is positively related to this period's income. However, if the rise in this period's income is disproportionately due to the relative income shock, the agent will increase consumption this period too much, and will then need to lower consumption next period. Here, next period's consumption is negatively related to this period's income. If the agent is following an optimal signal-extraction process, these two types of error are equally likely, causing no net relation between next period's consumption and this period's income.

In the aggregate we have only one of these cases. The effect of the shock to relative income washes out, leaving only the shock to aggregate income. Large shocks to aggregate income will always be associated with positive increases in consumption next period.

Does this effect matter? Goodfriend shows that this effect provides at least partial explanation for the results in Flavin's (1981) and Hayashi's (1985) tests of Hall's theory.

Problems with information are inherent in representative agent models that incorporate rational expectations. Sargent (1979) and Hansen and Sargent (1980) have used representative agent models as a means of deriving the rational expectations hypothesis from principles of economic optimization. Pesaran (1987, pp. 49–71) demonstrates that this can only be done when heterogeneities of information are ignored. If there is heterogeneous information among agents, it is not generally possible to derive a rational expectations model from the principles of economic optimization within a representative agent model. Hansen and Sargent are only able to derive the rational expectations hypothesis rigorously because they are implicitly assuming that all agents have exactly the same information. The reason for this additional requirement arises from the problem of infinite regress, which was immortalized in Keynes' story of the beauty contest:

> Or, to change the metaphor slightly, professional investment may be likened to those newspaper competitions in which the competitors have to pick out the six prettiest faces from a hundred photographs, the prize being awarded to the competitor whose choice most nearly corresponds to the average preferences of the competitors as a whole; so that each competitor has to pick, not those faces which he himself finds prettiest, but those which he thinks are likeliest to catch the fancy of the other competitors, all of whom are looking at the problem from the same point of view. It is not a case of choosing those which, to the best of one's judgment, are really the prettiest, nor even those which average opinion genuinely thinks the prettiest. We have reached the third degree where we devote our intelligences to anticipating what average opinion expects the average opinion to be. And there are some, I believe, who practise the fourth, fifth and higher degrees.
>
> (Keynes, 1936, p. 156)

With heterogeneous information, any agent, say Harold, is forced to guess how other agents will act; but how other agents will act depends upon how they think Harold will act, which of course depends on how they think Harold thinks they will act, which depends on how – well, you get the point. Unless all agents have the same information, the representative agent will run into the infinite regress problem whenever he wants to act. Representative agent models are inherently suppressing heterogeneities of information among agents in exactly the same manner as they ignore a host of other heterogeneities.

How to aggregate?

Geweke (1985) shows that the way in which we aggregate from microeconomic entities to macroeconomic results can crucially affect the results. He analyzes the process of aggregating from the behavior of an individual firm to a macroeconomic relationship. What is unique is his recognition that a given firm can have its production decision characterized in three different ways: (a) by a supply function; (b) by a demand for inputs function; or (c) by a production function.

The model is designed to make aggregation consistent in each of the three cases; i.e., there is no aggregation bias. Yet, when Geweke considers policy changes, a (perhaps) surprising result emerges. The predicted impact of the policy differs with the different characterizations of the firm. It makes a big difference how the firm is described. This result is very striking and very depressing for those who want to bypass aggregation problems. We are getting different results even though we are analyzing exactly the same firm subject to exactly the same policy. Even if we can characterize firm behavior perfectly and even if the firm's behavior is of a form that allows for perfect aggregation, we cannot be sure that our derived macroeconomic equations are accurate.

This is not a trivial problem. Through numerical examples, Geweke goes on to show that the order of the magnitude of error from ignoring these problems of aggregation can be exactly the same as that of completely ignoring expectations. Thus, while the new classicals have rigorously tried to eliminate the errors associated with ignoring agents' expectations, they have neglected the equally important errors of aggregation.

RESPONSES

What should we make of all this? The best place to begin is to recognize that these problems of aggregation are unsolvable. We should not pretend otherwise. In fact, the problems discussed in this chapter are very similar to another set of problems familiar to most economists: namely, the problem of creating a price index. There are better and worse ways of constructing an index, but none of the methods is perfect; the choice is really the least of several evils.

There are two cogent responses to the aggregation problem. First, we could abandon macroeconomics altogether and insist that economists only write models in which no aggregation takes place. In other words, the whole of economics should be the formulation of purely microeconomic models. In this way, we need not worry that consistent macroeconomic equations do not exist. We can avoid the aggregation problem altogether if we simply never aggregate. Economics would thus become the working out of vast general equilibrium economies from which things like aggregate demand were never derived.

However, there is a problem with this answer to the aggregation problem. Using fully described general equilibrium models is very cumbersome. To be accurate, we need a separate objective function/budget constraint/etc. for every single agent in the economy. A full system will have hundreds of millions, or even billions, of distinct equations. There is no real way to reduce the number of equations we need. Since the goal of this approach to the aggregation problem is to bypass it completely, we cannot simplify matters by lumping together similar, but not identical, agents. Whenever two nonidentical agents are combined, we run into the aggregation problem once again. As the aim of this approach is to bypass the problem completely, no aggregation is permissible.

However, there is another response to the aggregation problem. We could acknowledge that perfect aggregation is not possible and work with aggregates anyway. The aggregation problem will impinge on work in this vein. This is not a recipe for exactness; it is a method of pragmatism.

There have been attempts to split the difference between these two approaches. Stoker (1993) surveys the recent literature on micro–macro models.[3] He advocates the following procedure:

> [A]n individual model is specified together with assumptions that permit an aggregate model to be formulated that is consistent with the individual model. . . . This compromise between the other approaches is typically achieved by using individual level equations that are restricted to accommodate aggregation, together with information on the distributional composition of the population.
>
> (Stoker, 1993, p. 1830)

The literature Stoker surveys is extraordinarily diverse, and it is not entirely clear what specifically he is advocating.

Consider this example. The economy has two types of agents: (a) yuppies who spend all their current income, and (b) stalwarts who have perfectly smooth consumption paths. Stoker shows that a model that ignores the existence of these two distinct types of agents will have false dynamics. So, Stoker argues, the correct model is one in which both types of agents are explicitly recognized. This point seems clear enough until you think about how he set up the problem. The reason that assuming an aggregate function is incorrect is because there are really two types of agents in the economy. But from where did these two types of agents come? Are all "yuppies" really the same? Why not break the "yuppies" into two categories? Why not break those subcategories into sub-subcategories? And so on. There is absolutely no logical stopping place until we reach a model in which every single agent is individually modeled. Stoker is aware of this problem, noting, "[I]t is difficult to argue against the microsimulation approach for modeling aggregates on logical grounds" (ibid., p. 1867). So is he arguing for complete microsimulation? No; he also argues, "Logical correctness, however, does not translate to practical tractability" (ibid.).

Furthermore, "[I]n constructing models that measure aspects of behavior, one must begin 'from the ground up,' or always begin with a model of behavior at the individual level" (ibid., p. 1870). But then Stoker immediately goes on to argue:

> There is no sufficiently broad or realistic scenario in which one can begin with a representative agent's equations without explicitly considering the impact of heterogeneity. Whether a representative agent model fits the data or not, there is no

> realistic paradigm where the parameters of such a model reflect only behavioral effects, uncontaminated by compositional considerations.
>
> (Ibid.)

Now these comments about representative agent models are true enough, but they beg the whole question about how economists are then to build models "from the ground up." Either one starts with a completely disaggregated model in which each individual is modeled separately, or one begins by combining agents into "types." The representative agent approach assumes there is only one type, so is Stoker simply arguing that we should always start with at least two types of agents before aggregating?

In the end, Stoker has not really split the difference between the pure microfoundations models and the aggregate models. A macroeconomic model allowing for two or three or four groups of agents is still an aggregate model. Adding in terms that allow for, say, four different income groups does not create a disaggregate model. Why only four groups? Indeed, why disaggregate only on income? Why not add wealth differences or regional differences or socioeconomic background differences or any and every other difference we can imagine?

The Stokerian solution is nothing more than either an aggregate model with a few disaggregated terms thrown in, as in Stoker (1986b), or an incompletely aggregated model, aggregated to assorted demographic groups but not to a macroeconomic level, as in Stoker (1986a). The method of disaggregating to use is either chosen in a completely *ad hoc* manner or becomes an empirical matter. However, if it is to be the latter, then one can start with an aggregate model and see which terms need to be added to make the aggregate model work better. Either we start with a completely disaggregated model and aggregate up, or we start with an aggregate model and disaggregate in some way. In both cases, the end result is an aggregate model with all of the aggregation problems we have discussed in this chapter. Stoker argues, "There is simply no reason for according the 'aggregation' problem a secondary status relative to other concerns (aside from ill-advised modeling convenience), as in representative agent modeling" (Stoker, 1993, p. 1863). Similarly, there is no reason to accord the aggregation problem primary status, subordinating all other concerns. Whether an aggregate model of, say, consumption needs to be disaggregated in some way is exactly the same sort of decision as whether the function should use current income, permanent income, or wealth as an independent variable.

The idea that we can simply work with aggregates even though we know there may be some aggregation problems is not as irresponsible as it seems at first glance. Throughout this chapter, we have been dealing with three separate entities: microeconomic theory, macroeconomic theory, and methods of aggregation. The method we have been implicitly using is to take micro economic theory and aggregation methods as given and from these derive macroeconomic theory. We have judged any macroeconomic theory that cannot be derived from microeconomic theory via aggregation as unacceptable. This is exactly the approach advanced by the microfoundations proponents.

But there is no reason at all to accept the premises of the above approach. Instead, we could take microeconomic theory and macroeconomic theory as given and try to find aggregation methods that can reconcile the two. In this approach, if we cannot perfectly aggregate from microeconomics to macroeconomics, the problem is not with macroeconomics; rather, the problem is with the method of aggregation. We may know how microeconomic agents act and how the macroeconomy works but not know how to reconcile the two perfectly. From this viewpoint, the entire microfoundations approach is wrong-headed; it is assuming that we have good aggregation procedures when we do not. In fact, Stoker (1993, p. 1830, emphasis added) also acknowledges that it is our limited knowledge of how to aggregate that presents the real problem: "This compromise between the other approaches is typically achieved by using individual level equations that are *restricted to accommodate aggregation,* together with information on the distributional composition of the population."

We know very little about how to aggregate microeconomic entities into macroeconomic relationships. In a world in which the microeconomic agents were all hermits, aggregation could be simply a matter of adding together everyone's demand functions. In more realistic worlds in which agents interact with one another, in which agents' decisions depend on the decisions of other agents or on macroeconomic entities, it becomes very difficult to aggregate. However, these complicated decision rules may result in stable macroeconomic patterns that we can understand. In such a case, we would know a lot about the microeconomic agents and the macroeconomic relationships, but have no idea how to aggregate from the microeconomy to the macroeconomy. This does not necessarily mean that such aggregation is not possible; it simply means we don't know how to do such aggregation.

There is one response to the aggregation problem that is misguided at best and deceitful at worst. Representative agent models are often portrayed as following in the steps of the first response by providing rigorous microfoundations. However, if we have decided to follow the first path and abandon aggregation altogether, then representative agent models are worthless. Representative agent models inherently aggregate nonidentical agents. In fact, representative agent models are exactly the type of model against which the microfoundations case based on the aggregation problem argues.

On the other hand, if we decide to take a pragmatic response to the aggregation problem by just trying to live with it, then representative agent models are superfluous. The representative agent modeling strategy is merely disguising macroeconomic equations in a dress of microeconomic functions. This is substituting one form of aggregation for another. There is no reason whatsoever to assume that aggregating agents into a representative agent is any better (in the sense of having less bias) than aggregating agents directly into macroeconomic entities. It might be far better to recognize explicitly that we are in fact working with aggregates than to try to pretend we are not.

Representative agent models thus do nothing when it comes to the aggregation problem. If we condemn pure macroeconomic models because they suffer from aggregation problems, then in the same breath, representative agent models are condemned. If the rationale behind the microfoundations rhetoric is a belief that aggregation problems are serious and must be avoided, the representative agent models do not provide microfoundations to macroeconomic analysis.

11

INDIVIDUAL AND MARKET EXPERIMENTS

ENDOGENOUS VERSUS EXOGENOUS VARIABLES

Let us recall the basic structure of a representative agent model. The maximization problem for an individual is explicitly stated, from which assorted decision rules and other relationships among variables can be derived. These relationships are assumed to hold in the aggregate. Macroeconomic variables can then be inserted into the model to estimate or simulate macroeconomic relationships.

There is an implicit, and largely unrecognized, assumption underlying this procedure. Consider exactly what is going on. In order to understand aggregate relationships, we are solving the maximization problem of an individual. In order for this procedure to have any meaning at all, we must be assuming that the aggregate problem and the individual problem are conceptually similar.

It is vitally important that the problems be conceptually similar, or the representative agent procedure makes no sense. We cannot gain insights into a problem of interest by solving an entirely unrelated problem. For example, we can gain no insights into the problem of black holes by analyzing consumer preferences for shoes.

So are the individual problems that we solve conceptually similar to the aggregate problems we are interested in? At first glance, it would appear that they are. After all, the variables of interest in macroeconomics are just multiples of the variables of interest in microeconomics. Macroeconomics is interested in aggregate labor supply or consumption; microeconomics is interested in an individual's labor supply or consumption.

But this superficial similarity is deceiving. The problems faced by an individual are conceptually different from those in the macroeconomy. The representative agent approach confuses what Patinkin (1956) has defined as the individual and the market experiments.

Patinkin notes that, in looking at a functional relationship, there is an important distinction between independent and dependent variables. If derivatives are to be examined, it is important that they be set up in such a manner that they show the effect of changes in independent variables on dependent variables. Only such derivatives make any sense; it makes no sense to talk of how dependent variables impinge on independent variables.

The (obvious) distinction between individual and market experiments is the unit of analysis. In the individual experiment, the dependent (or endogenous) variables are those parameters over which the agent has some control; the independent (or exogenous) variables are those over which the agent has no control. The crucial fact is that variables classified as dependent or independent in the individual experiment may change their status in the market experiment. There is no logical (or economic) reason to assume that because a single agent has no control over a variable, the variable is exogenous to the whole economy as well.

Laidler (1982) discusses a very nice example of the confusion that can result when the individual and the market experiments are confused. In the 1970s there were a large number of studies of money demand in which the long-run money demand curve was posited to be something like:

$$m^* = \mathrm{f}(\mathrm{X}) + p$$

where m^* is the log of long-run money demand, p is the log of the price level, and X is a vector of variables determining the demand for real balances. The equation tells us that, given particular values of X and p, individuals want to hold m^* in nominal money. If at time -1 individuals hold less than m^*, they will act to increase their money holdings. However, there may be some portfolio adjustment costs to changing money balances, so individuals may not fully adjust their money holdings to m^* in any given period. The change in money balances is thus:

$$m - m_{-1} = b(m^* - m_{-1}) \qquad 0 < b < 1$$

Combining these two equations yields the short-run demand for money:

$$m = b\{\mathrm{f}(\mathrm{x}) + p\} + (1 - b)m_{-1}$$

In an econometric evaluation of this short-run demand for money equation, the coefficient on lagged money holding represents the portfolio adjustment costs for the individual.

Laidler notes that equations of this form were used to estimate *aggregate* money demand with the coefficient on lagged aggregate money balances being interpreted as the portfolio adjustment costs. He notes that this is not good economics: it is confusing the individual and the market experiments. While an individual may desire to change his nominal money balances slowly to the desired level, the market has no ability to change total nominal money balances slowly. Since the nominal money supply is exogenous to the aggregate economy, the market as a whole does not slowly adjust to a new level of the money supply; rather, it automatically holds the whole nominal money supply. If an individual wants to change his real money balances, he does so by changing his nominal money balances and thereby may incur some portfolio adjustment costs. The whole economy, however, adjusts real balances by changing the price level, thereby possibly incurring some costs from changing prices, but not incurring anything like a portfolio adjustment cost. Laidler thus shows that the interpretation of the individual short-run money demand curve makes absolutely no sense for a market short-run money demand curve.

These sorts of problems extend well beyond the particular specification of the money demand function discussed by Laidler. Representative agent models inherently confuse this distinction between individual and market experiments. Note that the methodology assumes that the market exhibits the same functional relationships as the individual. Below, examples of new classical representative agent models are examined to see how the individual and the market experiments are confused. The implications of this problem are then discussed.

AN EXAMPLE: HANSEN AND SARGENT (1980)

The first example is from Hansen and Sargent (1980). This model is chosen for several reasons. First, and most importantly, it illustrates beautifully the point being made here. Moreover, this model is technically complex. As a result, it further illustrates why the individual and the market experiments get confused. In what follows, we will deal with a simplified version of the model. However, it will not be as simple as possible; furthermore, we will make no attempt to derive all of the relevant relationships. Interested readers should consult Hansen and Sargent (1980). Throughout, an attempt has been made to provide an accurate picture of the core of the model while avoiding burdening the reader with a surfeit of computations. There is no need to try and decipher the model below line by line. The important equations are explicitly noted.

The model here is of a firm choosing a single input to maximize profits. We will consider the input to be labor (although it would make no difference if the input were capital). The variables in the model are as follows (listed in alphabetical order, English alphabet preceding Greek alphabet):

a_t = random shock to technology

c_t = defined in equation (11.9)

d_t = defined in equation (11.10)

e_t = defined in equation (11.6)

E_t = the expectations operator

I = the identity matrix

L = the lag operator

nt = employment of labor

q = number of lags in $\alpha(L)$

r = number of lags in $\zeta(L)$

t = time index

U = $(1 \times p)$ unit row vector: 1 in the first place, 0 elsewhere (n.b., p is size of x)

v^i_t = *innovation in the i process: i = a,b,c,d,e,x,*

w_t = real wage

$x_t = (1 \times p)$ vector with w_t in the first position

$\alpha(L) = 1 - \alpha_1 L - \ldots - \alpha_q L^q$; see equation (11.2)

β = constant discount factor

γ_i = positive parameter in firm's maximization function (equation 11.1): $i = 0, 1$

δ = parameter governing cost of changing employment size

$\zeta(L) = 1 - \zeta_r L^r$; see equation (11.3)

$\theta(L)$ = defined in equation (11.8)

$\lambda = \rho^{-1}_2 = \beta\rho_1$; see definition of ρ

$\mu(L)$ = defined in equation (11.5)

$\pi(L)$ = defined in equation (11.5)

ρ_i = roots of the characteristic equation, $i = 1, 2$

ν = defined in equation (11.9)

Φ = defined in equation (11.13)

Ψ = defined in equation (11.13)

Ω = defined in equation (11.14)

We now proceed with a description of the model. The model is of a firm choosing the size of its labor force, n_t, to maximize the intertemporal objective function:

$$\lim_{N\to\infty} E_t \sum_{j=0}^{N} \beta^j \left[(\gamma_0 + a_{t+j} - w_{t+j}) n_{t+j} - \left(\frac{\gamma_1}{2}\right) n^2_{t+j} - \left(\frac{\delta}{2}\right) (n_{t+j} - n_{t+j-1})^2 \right] \tag{11.1}$$

The technology shock, a_t, is a stochastic process with

$$\alpha(L) a_t = v^a_t \tag{11.2}$$

The real wage, w_t, is the first term in the autoregressive process x_t which satisfies

$$\zeta(L)x_t = v_t^x \tag{11.3}$$

The above equations (11.1)–(11.3) are solved to obtain the decision rule:

$$\begin{aligned} n_t =& \rho_1 n_{t-1} - \left(\frac{\rho_1}{\delta}\right) U\zeta(\lambda)^{-1}\left[I + \sum_{j=1}^{r-1}\left(\sum_{k=j+1}^{r}(\lambda^{k-j}\zeta_k)\right)L^j\right]x_t \\ &+ \left(\frac{\rho_1}{\delta}\right)\alpha(\lambda)^{-1}\left[1 + \sum_{j=1}^{q-1}\left(\sum_{k=j+1}^{q}(\lambda^{k-j}\alpha_k)\right)L^j\right]a_t \\ &+ \left(\frac{\rho_1\gamma_0}{\delta}\right)\left(\frac{1}{1-\lambda}\right) \end{aligned} \tag{11.4}$$

For simplicity we condense (11.4) into

$$nt = \rho_1 nt_{-1} + \mu(L)xt + \pi(L)at \tag{11.5}$$

where $\mu(L)$ and $\pi(L)$ are defined appropriately and the constant term is neglected.

Consider equation (11.5). This says that the firm's employment decision of how many workers to hire depends upon two things: (a) the state of technology, a_t; and (b) the real wage, w_t. (Note that U in equation (11.4) causes only the w terms in x to appear in the equation.) So far, so good. In a large population, any individual firm can reasonably consider both technology and the real wage to be exogenous. In essence, this is nothing more than the common price-taking assumption of firm behavior. A firm merely looks out, sees the state of technology and how much it costs to hire labor, and acts accordingly.

Hansen and Sargent go on to examine issues of Granger causality. A new variable e_t is defined as the third term on the right-hand side of equation (11.4), or

$$e_t = \left(\frac{\rho_1}{\delta}\right)\alpha(\lambda)^{-1}\left[1 + \sum_{j=1}^{q-1}\left(\sum_{k=j+1}^{q}(\lambda^{k-j}\alpha_k)\right)L^j\right]a_t \tag{11.6}$$

Using this definition and equation (11.2), we can write:

$$\alpha(L)e_t = \pi(L)v_t^a \tag{11.7}$$

But, since $\pi(L)$ may not be invertible, a new variable, $\theta(L)$ is designed such that:

$$\alpha(L)e_t = \theta(L)v_t^e \tag{11.8}$$

We then define two new variables c_t and d_t as:

$$c_t = v_t^a - \upsilon v_t^e \tag{11.9}$$

$$d_t = (1 - \rho_1 \mathrm{L})^{-1} \pi(\mathrm{L}) \alpha(\mathrm{L})^{-1} c_t \tag{11.10}$$

Finally, (11.10) can be rewritten as:

$$d_t = (1 - \rho_1 \mathrm{L})^{-1} \theta(\mathrm{L}) \alpha(\mathrm{L})^{-1} v_t^d \tag{11.11}$$

We have now reached the relevant portion. Using the above relations, we can write out the joint (n_t, x_t) process as:

$$\begin{bmatrix} n_t \\ x_t \end{bmatrix} = \begin{bmatrix} (1 - \rho_1 \mathrm{L})^{-1} \theta(\mathrm{L}) \alpha(\mathrm{L})^{-1} & (1 - \rho_1 \mathrm{L})^{-1} \{\mu(\mathrm{L}) \zeta(\mathrm{L})^{-1} + \pi(\mathrm{L}) \alpha(\mathrm{L}) \nu\} \\ 0 & \zeta(\mathrm{L})^{-1} \end{bmatrix} \times \begin{bmatrix} v_t^d \\ v_t^x \end{bmatrix} \tag{11.12}$$

or

$$n_t = \Phi v_t^d + \Psi v_t^x \tag{11.13}$$

$$x_t = \Omega v_t^x \tag{11.14}$$

with Φ, Ψ, and Ω defined appropriately. (*Note:* (11.14) is identical to (11.3).) The authors then note, “The triangular character of this moving average representation together with Sims’s theorem 1 (1972) imply that n_t fails to Granger cause x_t” (Hansen and Sargent 1980, p. 23). In other words, the authors purport to have shown that current and past values of n have no independent influence on x.

However, the authors are interested in more than issues of Granger causality. What is of real interest are issues of exogeneity. In short, Hansen and Sargent wish to establish under what conditions x_t is an exogenous factor. They establish the following result:

> Sufficient conditions are both that (a) there exists a triangular moving average representation, i.e., n_t does not Granger cause x_t, and (b) the vector of regression parameters $\nu = 0$, i.e., *Evxt vat* = 0. Thus the conditions under which xt is exogenous in the labor demand schedule are more stringent than the conditions under which n_t fails to Granger cause x_t.
>
> (Hansen and Sargent 1980, p. 24)

Condition (a) has been shown to be true in all cases. Hansen and Sargent proceed to describe two different means of testing the hypothesis that $\nu = 0$.

The requirements for exogeneity are not the subject of interest here. What is interesting is that Hansen and Sargent are elaborating these requirements at all. The authors spend a large amount of time carefully demonstrating the fact that n_t does not Granger cause x_t and means by which economic exogeneity can be proven. But we have lost sight of exactly what we are doing. Since we are comparing the abstract entities x and n, we may very well wonder if one or the other is exogenous. However, this entire discussion is based upon a confusion of individual and market experiments.

The above model was developed for an individual firm. Hansen and Sargent are seeking to model aggregate relations. To them, the connection is simple: use the above equations as aggregate relations; for example, instead of a single firm's labor demand, use aggregate labor demand.

This model is *mathematically* correct. However, when applied to aggregate variables, it is *economically* flawed. Consider what the model is saying: an individual firm's decision to hire labor depends on current and past values of the wage and technology; at the same time, knowledge of the firm's hiring decision provides no information about the real wage.

For an individual firm, this is sensible. If there are 10,000 firms in an economy, and we know the employment decision of only one of them, we can infer nothing about real wages. However, if we know the current and past employment decisions of all the firms in the economy, we can infer a great deal about the real wage. While it is reasonable to assume that any one firm has no effect on real wages, it is unreasonable to expect the aggregate decisions of all firms to have no effect on the real wage.

Hansen and Sargent have carefully examined the relationship between nt and x_t. When n is an individual firm's labor demand, their discussion is fine. But when they make the switch to having n as aggregate labor demand, their model loses its meaning. For while an individual firm may face an exogenous wage, it is clear that wages are not exogenous to the *aggregate* economy.

In fact, even the Granger relations are suspect; it would be remarkable if aggregate employment failed to Granger cause wages. Hansen and Sargent's decision to set up a model in which employment does not Granger cause wages while wages do Granger cause employment builds on one of Sargent's earlier papers (1978). In that paper, Sargent looks at Granger causality between employment and wages and finds "much stronger evidence of Granger causality extending from real wages to employment than in the other direction" (Sargent, 1978, p. 1011). This evidence, however, is quite weak; if we use the standard 5 percent significance level, Sargent's results show no Granger causality in either direction. So why does Sargent then set up a model in which Granger causality runs in one direction but not the other? "This assumption, which will be imposed below, substantially simplifies the modeling task" (ibid., p. 1012). Thus, from one paper (Sargent, 1978) that shows very weak empirical evidence for a one-directional Granger causality we move to another paper

(Hansen and Sargent, 1980), in which such a Granger causal relationship is imposed by assumption. Moreover, one should be skeptical of relying too heavily on any single Granger causality test. As Hamilton (1994, p. 305) notes, “The results of any empirical test for Granger causality can be surprisingly sensitive to the choice of lag length (p) or the methods used to deal with potential nonstationarity of the series.”

The problem with these assumptions shows up fully in equation (11.4) (or equation (11.5)). If we were to use this equation as a regression equation, we would regress n_t on x_t and a_t.[1] This regression implies that x_t and a_t are exogenous processes in determining n_t. In the individual experiment, this is sensible. However, if we use aggregate data in equation (11.4), we run into problems. The real wage is not exogenous with respect to the current level of aggregate labor demand. Changes in aggregate labor demand will change the real wage. Thus, when using aggregate data, we have a simultaneous equations problem; the estimates in such a regression will be biased.

The point here is not that Hansen and Sargent’s derivation is flawed; it is not. The argument is that while the derivation is technically impressive, it is not macroeconomics. Using the present model as a macroeconomic relationship is improper and misleading. The problem stems from the confusion of the individual and the market experiments. In the individual experiment, it is entirely proper to conceive of real wages as being exogenous. However, in the market experiment, it is not. We cannot merely take relationships that hold for individuals and apply them to the economy as a whole.

The example above regarding wages is of a variable that is exogenous in the individual experiment but endogenous in the market experiment. The problem can (and does) run the other way. In other words, when attempting to apply an individual’s equations to the aggregate economy, there may be variables that are endogenous to the individual but exogenous in the aggregate.

Hansen and Sargent’s model contains such a variable, namely labor demand. When setting up the individual firm’s equations, it was implicitly assumed that at the prevailing real wage rate the firm could hire as much labor as it wished. This is sensible at the individual level. However, in the aggregate there is only a certain amount of labor supply at any given wage rate. Thus, the total amount of labor employed in the aggregate is bounded by the forthcoming labor supply.

For example, suppose the model says that at prevailing wages and technology, each of 100 firms should hire ten workers. However, suppose there are only 800 workers in the economy. We have a problem: the aggregate labor supply is fixed below the amount at which the model says firms will hire. There is nothing in this model that can deal with this eventuality. Note that this problem cannot be avoided by arguing that if labor market conditions are one of the variables in x that determine w, then tight labor market conditions will influence the wage and hence employment decisions. If this were true, then n would influence x, contradicting the individual firm’s pricetaking behavior.

This situation could be a problem in the following manner. Suppose that real wages are constant, but a series of technology shocks induces firms to want to hire more labor. Any individual firm may be able to get the additional workers. However, in the aggregate there are no more workers to be had. Thus, Hansen and Sargent's model would show an increasing labor force when such a situation is impossible.

In sum, when equations from the individual experiment are used with macroeconomic data, the result can be economic nonsense. The relationships between independent and dependent variables can change between the two cases. To assume that the exogeneity or endogeneity of a variable in the individual case is identical to that in the market case is wrong. However, this is exactly what representative agent models do.

A SIMPLER EXAMPLE: HALL'S PERMANENT INCOME MODEL

The complexity of the Hansen and Sargent model helped to illuminate the intricacies of the argument presented here. Moreover, the amount of space devoted to discussions of Granger causality and exogeneity in their paper helped to illuminate how easy it is to make fundamental errors when we lose sight of just what it is we are saying.

However, the complexity of the model may also have detracted from the argument itself. So another example is presented here based on a model familiar to most economists.

In Hall (1978), the famous Euler equation relationship between consumption this period and consumption next period is derived. The paper uses a representative agent model to generate the equation and then tests the results using aggregate data. We thus have a perfect example of the point being made here. The variables are:

E_t = the expectations operator

δ= rate of subjective time preference

r = real rate of interest

T = length of economic life

$U(\cdot)$ = one-period utility function

c_t = consumption

w_t = earnings

A_t = assets apart from human capital

The representative agent maximizes

$$E_t \sum_{\tau=0}^{T-t} (1+\delta)^{-\tau} U(c_{t+\tau}) \tag{11.15}$$

subject to the budget constraint

$$\sum_{\tau=0}^{T-t} (1+r)^{-\tau}(c_{t+\tau} - w_{t+\tau}) = A_t \tag{11.16}$$

From this, the Euler equation can be derived as

$$E_t U'(c_{t+1}) = \left[\frac{(1+\delta)}{(1+r)}\right] U'(c_t) \tag{11.17}$$

We thus have a relationship telling us how a consumer decides to allocate his consumption between this period and next.

Hall uses this relationship and a particular form for the utility function to derive the regression equation:

$$c_t^{-1/\delta} = \gamma c_{t-1}^{-1/\delta} + \varepsilon_t \tag{11.18}$$

Aggregate consumption levels are then used to test the model. However, in using aggregate consumption levels in the regression, the individual and market experiments are being confused.

Consider the set-up of the model. An individual is assumed to take the interest rate as given in deciding how to allocate his income between consumption and savings. This is reasonable. If I save a little more (or less) of my paltry earnings, I certainly won't affect the real interest rate. However, for the economy as a whole this is not true. If the economy as a whole saves a little more (or less) interest rates will move.

While in the individual experiment interest rates can safely be assumed to be exogenous, it is wrong to assume that this is true in the market experiment. If all *n* individuals in an economy decide to save a little more, interest rates will fall.

The problem here shows up in the γ in equation (11.18). The parameter γ is composed of the term $(1 + \delta)/(1 + r)$. In running a regression on equation (11.18), it is implicitly assumed that γ is a constant. However, when using aggregate data, this is wrong. γ depends on c_t; i.e., as c_t moves, γ will change via the change in interest rates. Equation (11.18) is thus nonlinear. The parameter estimates have no meaning.

There have been extensions of Hall's model, e.g., Michener (1984), which recognize the endogeneity of interest rates in such cases. But the general problem remains: the individual and the market experiments are fundamentally different. While some variables are exogenous to both the individual and the market and others are endogenous to both, there is no reason to assume that the individual's optimization problem looks anything like the market optimization problem.

CONCLUDING NOTE

The new classicals have thus greatly oversold the ease with which decision rules from representative agents can be used as macroeconomic relationships. The simple fact is that individual relationships are not the same as aggregate relationships. We must be very careful when taking equations generated in individual experiments and attempting to apply market data to them. Often, the results will be meaningless.

12

THE REPRESENTATIVE AGENT VERSUS MICROFOUNDATIONS

THE ARGUMENT

In the preceding two chapters we indirectly examined the ability of a representative agent model to fit into the microfoundations framework. Chapter 10 showed how the representative agent framework was unable to bypass one of the problems that the microfoundations approach is intended to solve, namely the aggregation problem. Chapter 11 demonstrated that the representative agent model inherently neglects the fact that the exogeneity or endogeneity of a given variable may be different at the microeconomic and macroeconomic level. In this chapter we want to answer the question directly: do representative agent models establish microfoundations for macroeconomics?

If we desire to provide microfoundations for macroeconomics, we cannot use a representative agent model. There is simply no plausible way to argue that the microeconomic foundations provided by a representative agent in any way resemble the microeconomic foundations provided by modern microeconomic theory.

We begin our exploration by noting the incompatibility of calling for both microfoundational models and representative agent models. From this general discussion, we explore the microfoundational nature (or, as it turns out, the lack thereof) of actual new classical representative agent models. From showing that these models do not provide microfoundations, we move on to a discussion of whether it is even possible for a representative agent model to provide a foundation in modern microeconomics. And we end up with a discussion of a well-known compositional fallacy.

THE MONKEY MODEL

To get at the question of whether representative agent models can provide microfoundations, consider the allegory Lucas (1980) constructs, which we will dub the Lucas monkey model. Lucas wants to answer the question: How will a macroeconomy react to exogenous changes in the environment? To simplify the matter, Lucas reflects on how five monkeys will respond to having a banana thrown into their cage. A knowledge of the monkeys' preferences and the technology do not suffice to answer the question. It is conceivable that the whole banana could go to the strongest or the fastest or the shrewdest monkey. Instead, the banana could be shared equally among all the monkeys or a subset of the monkeys. There is simply no way to answer this question without knowing something about how the monkeys interact.

Lucas proceeds to explain how we can determine what will happen without needing to run the experiment. He argues that the question is unanswerable at this point because it lacks one necessary ingredient, namely competition. So Lucas suggests that we start the experiment by cutting the banana into five equal pieces and giving each monkey one piece and then let them compete. But simply imposing competition does not tell us anything about the result. It is still possible that the strongest or shrewdest could appropriate all the banana or that the banana could be evenly divided in the end. Since introducing competition is not enough, Lucas imposes a particular rule: the monkeys can only interact by exchanging bananas for back-scratching at a fixed rate.

If we can impose the exchange rule on the society as a whole and we have detailed information about the individual monkeys' preferences, we can now predict the effect of introducing the banana to the cage. The image conjured up is one in which monkeys with a high relative preference for backscratching trade with monkeys with a high relative preference for bananas. Some monkeys are thus able to consume more banana by being willing to scratch other monkeys' backs; some monkeys get back-scratches by giving up their banana.

Now obviously Lucas is not particularly concerned with monkeys but wants us to draw a larger moral from this tale. Simply stated, the moral is that providing microfoundations for the macroeconomy is essential. If all we know is that there are five monkeys and one banana, we cannot know the result of introducing the banana; we cannot determine how the monkeys will react to the change in their environment. Without more detailed information, we can do nothing more than offer up the banal result: the banana will be eaten. However, if we introduce detailed information about both the preference structure of the individual monkeys and the rules of exchange, we can accurately predict the result without needing to conduct the experiment. Lucas proceeds to argue that exactly this problem occurs when we contemplate examples involving humans. Without providing microfoundations, macroeconomic study can do very little. However, after providing microfoundations, we can study complex interactions among humans without needing to actually conduct the experiments.

Leaving the world of monkeys, how can we study human interactions? Lucas argues that the hypothesis of competitive equilibrium gives us the ability to predict aggregate outcomes from knowledge of individual preferences and technology. To this point it seems as if Lucas is suggesting a mammoth general equilibrium study. If we gain knowledge about the preference structure over all goods of every single person in the economy and we impose fixed, definite rules of exchange on all interactions among these people, then we, economists, can run simulations of the economy on our computers. If the monkey allegory means anything, it means exactly this. Nothing short of total knowledge of both preferences and the rules of exchange will suffice; nothing could be determined in the monkey model until both things were known.

The monkey model can thus be read as a striking call for microfoundations. However, it is also proposing a formidable project. It is inconceivable that we could ever collect this sort of information, let alone generate a computer model which could make use of it. So Lucas provides us with a way out: "It is possible, we know, to mimic the aggregate outcome of this interaction fairly well in a competitive equilibrium way, in which wages and manhours are generated by the interaction of 'representative' households and firms" (Lucas, 1980, p. 711).

How do we know that it is possible to mimic the economy with a representative agent model? Lucas provides just one citation for this claim: Lucas and Rapping (1969). Now, as we saw in our discussion in Chapter 3, this is a very weak hook on which to hang an argument. Lucas and Rapping is a modern representative agent model in name only; it lacks the rigorous derivation of macroeconomic equations from a utility-maximization problem complete with testable cross-equation restrictions that mark the state of representative agent models at the time Lucas was writing. In fact, in a footnote Lucas notes that, to be convincing, parts of the Lucas and Rapping paper need to be updated using the ideas found in Sargent (1978) and Hansen and Sargent (1980). Furthermore, as we saw in Chapter 3, the empirical test of Lucas and Rapping was less than compelling.

Setting aside the lack of support for the statement that "we know" a representative agent model can mimic the macroeconomy, how does it perform in cases we can consider directly? Determining whether a representative agent economy mimics behavior in the actual economy is a complex task. However, Lucas has just provided us with a marvelous opportunity to run the representative agent model through its paces. Let us think about a representative agent version of the monkey model.

With five monkeys, the representative monkey, by definition, starts with one-fifth of the banana. After all is said and done, the representative monkey, by definition, ends up with one-fifth of the banana. Thus our remarkably shrewd representative monkey was able to somehow end up with exactly what he was given. There is simply no way that the representative monkey can ever end up with more or less than one-fifth of the banana. So, after introducing our elaborate rules of competition, what does our representative monkey do? He eats his endowment. We hardly need elaborate rules of competition to tells us that would occur; all we need to know is that monkeys like bananas.

Lucas suggests that we study the "interaction" of representative households and firms, so let us begin by studying the interaction of a group of representative monkeys. They all start with one-fifth of a banana; they all end with one-fifth of a banana. Maybe they traded banana pieces?

But what about the back-scratching? If Lucas will let them, the monkeys might be able to work out a deal of "if you scratch my back, I'll scratch yours." However, the exchange here does not depend on the exchange ratio between bananas and back-scratches; it depends solely on the marginal disutility of scratching another monkey's back versus the marginal utility from having your own back scratched. The bananas are irrelevant to this exchange.

Rather than the hubbub of activity conjured up by the monkey model, where monkeys compete with one another to exchange bananas for back-scratches, the representative agent model gives us a world in which we see each monkey quietly sit down and eat the fifth of a banana which he was given and, if we are lucky, we also see some monkeys sitting in a circle afterwards, scratching each other's backs. There are no exchanges of bananas for back-scratches at all. There is no competition among monkeys. The interactions among monkeys are dull or nonexistent.

Now, if we know that this is the way monkeys behave, there is very little need for a model at all, let alone a complex microfoundational model. In fact, we need to know little about the individual monkey's preferences beyond the fact that monkeys get positive utility from eating a banana. We need to know nothing about the rules of exchange or the price at which exchanges take place, because there is no exchange in the model. Providing microfoundations for the macroeconomy is a trivial matter.

So why do we worry about providing microfoundations at all? It is precisely because we do not know that the monkeys will behave in this way. We have a reason to wonder whether the monkeys will really just eat their endowment or not. The uncertainty about the result is what prompts us to search further.

The very reason Lucas set forth the monkey model was to convince us that providing microfoundations is necessary in order to understand the macroeconomy. But then, in the very next paragraph, he abrogates the very necessity he has just decreed. If the monkey model can be simulated by a representative agent model after all, then we don't need all those specific and artificial rules about competition and exchange rates; we don't need specific knowledge about individual monkeys' preferences.

So what after all is the point of the monkey model? If the monkey model is meant to convince us that microfoundations are important, then the very same model demonstrates that a representative agent model cannot provide microfoundations. We cannot study the complex interactions of the monkeys with a representative agent model at all. If we try to escape this conclusion by arguing that the complex interactions don't matter, that all we care about is the aggregate result, then what is the point of providing microfoundations? Even without microfoundations we could predict that in the aggregate exactly one banana would be eaten; neither providing complicated microfoundational rules about exchange rates nor writing down a specific representative agent model adds anything to our knowledge about how many bananas would be eaten.

Thus, even in this very simple model of monkeys eating bananas in a cage, we run into a serious problem in using a representative agent model to provide microfoundations. The only hypothesis of monkey behavior in which a representative agent model provides microfoundations is one in which all monkeys just quietly eat their endowment. However, it is exactly when this hypothesis is accurate that microfoundations are least interesting and important; if all monkeys just eat their endowments, for what purpose do we need microfoundations? When the microeconomic behavior is interesting enough to warrant study, the representative agent model fails to provide microfoundations.

MARKET CLEARING AND RATIONAL EXPECTATIONS

Let us set aside the inherent problems in using a representative agent model to provide microfoundations and look at the related question: Are the representative agent models used in macroeconomics actually grounded in utility-maximization problems? At first glance, this question seems absurd. A quick look at the literature shows that the models start with something saying "maximize utility subject to some constraints," so it seems trivially obvious that these models are grounded in microeconomic utility-maximization problems.

There is more to the story, however. The utility-maximization problem that starts the representative agent model is not sufficient to generate aggregate outcomes. In order to use a representative agent model as the new classicals have done, two additional assumptions must be imposed. First, we need to know the rules of the game; what is the economic environment in which the agent operates? Second, we need to know the process by which the agent forms expectations about the future. In most new classical models, these questions are answered by imposing competitive equilibrium (as we saw in the monkey model above) and rational expectations.

How do these additional assumptions relate to the aim of proving microfoundations? New classical theorists have long insisted that models need to be rigorously derived from an individual optimization problem, that they need to start with "the objective functions that agents are maximizing and the constraints they are facing, and which lead them to choose the decision rules that they do" (Sargent, 1982, p. 383). In order to claim that a representative agent model actually meets this microfoundational standard, we need to be able to claim that all the important assumptions are generated by microeconomic considerations. Consider a representative agent model in which the individual's utility maximization problem is solved subject to the constraint that an aggregate Phillips curve exists. Is this a microfoundational model? Of course not; the constraint is coming from nowhere and is not the result of the optimization of individual agents.

Thus, representative agent models can only be called microfoundational models if the assumptions of competitive equilibrium and rational expectations can be generated by a theory of individual behavior. Janssen (1993) shows that neither of these assumptions can be so generated.[1]

Consider first the assumption of competitive equilibrium. New classical representative agent models explicitly invoke the Arrow–Debreu general equilibrium model as the justification for assuming competitive equilibrium. (See, for example, Prescott, 1986; Cooley and Prescott, 1995; and Hansen and Sargent, 1990, forthcoming.) The Arrow–Debreu model characterizes an equilibrium state as one in which aggregate excess demand is less than or equal to zero. The model starts with given taste and technology parameters and finds a price vector for which market supply equals market demand. Thus, the starting point of the Arrow–Debreu model is microfoundational.

However, Janssen notes that starting out as a microfoundational model is not the same as ending up as a microfoundational model:

> What one would like to have is a theory specifying how the market outcome (prices) depends on the decisions taken by individual agents. [The Arrow–Debreu model], however, only says what individual agents do at given prices; it is not about how prices result from individual actions. . . . Merely saying that prices have to be such that *aggregate* demand equals *aggregate* supply begs the questions from a methodological individualistic point of view.
>
> (Janssen, 1993, p. 111)

Janssen notes that others have noticed the same problem, and that the conventional explanations of how prices come to be set at market clearing levels are not based on microfoundational reasoning. For example, the law of supply and demand is often invoked: prices rise if demand exceeds supply and fall if supply exceeds demand. Even Arrow has noted the unsatisfactory nature of this explanation:

> It is not explained whose decision it is to change prices. . . . Each individual participant in the economy is supposed to take prices as given and determine his choices as to purchases and sales accordingly; there is no one left over whose job it is to make a decision on price.
>
> (Arrow, 1959, p. 43)

Sometimes, the task of setting prices is assigned to some sort of auctioneer or social planner. However, such an individual does not exist; assuming the existence of such an individual is not microfoundational; one might as well assume the existence of a simplistic Keynesian consumption function. At other times, the law of supply and demand is interpreted as the result of the omnipresent "invisible hand." Janssen (1993, p. 112) notes, "Giving the market mechanism a name such as the Invisible Hand cannot substitute, however, for the fact that a process description in terms of market institutions and individual behavior is lacking."

A final protest against the argument that there are no microfoundations for the setting of market-clearing prices in the Arrow–Debreu model is that we do have existence proofs. Given certain regularity conditions, it can be shown that the price vector necessary to induce a competitive equilibrium does exist. However, a proof of existence is not the same as a proof of attainment:

> In other words, existence proofs show that an economy populated with self-interested agents does *not necessarily* end up in a state of chaos. They do not show, however, that the general equilibrium allocation is the only allocation that may result in the economy. They only point at the *possibility* of a coherent disposition of economic resources. However, they neither show that this particular possibility will materialize, nor do they show how it results from the behavior of individuals if it materializes at all.
>
> (Janssen, 1993, p. 112)

Janssen's argument here is strong; if we want our models to be built up from utility-maximization problems, we cannot blithely assume that a competitive equilibrium exists.[2]

When we turn to the new classical literature, then, we find a remarkable anomaly. Lucas and Sargent offer up this defense of using models with competitive equilibria:

> Cleared markets is simply a principle, not verifiable by direct observation, which may or may not be useful in constructing successful hypotheses about the behavior of these series. Alternative principles, such as the postulate of the existence of a third-party auctioneer inducing wage rigidity and uncleared markets, are similarly "unrealistic," in the not especially important sense of not offering a good description of observed labor market institutions.
>
> (Lucas and Sargent, 1979, p. 11)

Renaming assumptions as "principles" does not change the inherent *ad hoc* nature of the assumption. The clearing of markets is not derived from taste and technology parameters; instead it is simply assumed. Thus, Sargent's call to go "beyond demand and supply curves in macroeconomics" is seen in a whole new light; the models he was advocating do not go beyond aggregate supply and demand curves at all; instead, the models assume that prices move to the point of intersection between these curves. Despite all the derivation of equations from a utility-maximization problem, there is lurking in the background an old-fashioned intersection of aggregate supply and aggregate demand determining the market clearing prices.

Janssen points to a similar problem with the assumption of rational expectations; it also cannot be derived from a microfoundational model of utility maximization. The fundamental question here is: Would a utility maximizing individual choose to employ rational expectations?

Janssen demonstrates that in a multi-agent economy, assuming that agents are individually rational and know the true model is not sufficient to justify assuming that agents form expectations according to the rational expectations hypothesis. The agents must additionally assume that the average expectation of other agents will equal the expectation generated by the rational expectations hypothesis. Thus, agents must form expectations of other agents' expectations – in other words, we are faced with Keynes' beauty contest problem again (Keynes, 1936, p. 156).

This problem is even more serious than it appears. Can we show that if individual agents are rational and that this rationality is common knowledge (i.e., agents know that the other agents are also rational), then the rational expectations hypothesis is optimal? Janssen demonstrates that even these conditions are not sufficient. It is possible that in this case the individual's rational expectation will be equal to that assumed by the rational expectations hypothesis, but we would only hit such a world by chance; the above assumptions about human behavior do not necessarily or even probably result in such a situation. Janssen thus concludes:

> The term "rational expectations" is thus rather misleading. It *suggests* that the [rational expectations hypothesis] is an extension of the rationality principle to the domain of expectations. This chapter has argued that in general this is *not* what the [rational expectations hypothesis] does. Thus, [the rational expectations hypothesis] is an aggregate hypothesis that cannot unconditionally be regarded as being based on [methodological individualism].
>
> (Janssen, 1993, p. 142)

Where does this leave new classical representative agent models? Again, we can turn to Lucas and Sargent's (1979) criticism of Keynesian macroeconomic models:

> The casual treatment of expectations is not a peripheral problem in these [Keynesian macroeconomic] models, for the role of expectations is pervasive in them and exerts a massive influence on their dynamic properties (a point Keynes himself insisted on).

> The failure of existing models to derive restrictions on expectations from any first principles grounded in economic theory is a symptom of a deeper and more general failure to derive behavioral relationships from any consistently posed dynamic optimization problems.
>
> (Lucas and Sargent, 1979, p. 5)

Now this is a seemingly strong indictment of Keynesian models. Yet the new classical representative agent models suffer from exactly the same problem. Using expectations formulated according to the rational expectations hypothesis is not derived from any "first principles grounded in economic theory." In fact, "rational expectations" are no more or no less derived from behavioral principles than adaptive expectations. Both are simply *ad hoc* assumptions about how agents form expectations. If we take Lucas and Sargent seriously and indict all models which fail rigorously to derive rules about expectation formation from behavioral relationships, then we must indict new classical representative agent models for failing to do so.

Thus, if the goal of providing microfoundations to macroeconomics is to write down models in which macroeconomics is explained as the result of the decisions of individual people, then new classical representative agent models fail to provide microfoundations. These models may look as though they are based on the utility maximization of individual agents, but in building up the model two completely *ad hoc* assumptions are made. Neither the assumption of competitive equilibrium nor the assumption of rational expectations is based on a microfoundational model. If the microfoundations goal is, as Sargent (1981, p. 215) explained, "to interpret time-series data as representing the results of interactions of private agents' optimizing choices", then new classical representative agent models do not provide microfoundations.

MICRO THEORY

The previous section examined whether new classical representative agent models *do* provide microfoundations. The present section examines a more fundamental question: *Can* representative agent models provide microfoundations? Setting aside the problems of motivating the assumptions of competitive equilibriums and rational expectations, is it possible to provide a microfoundational model of the macroeconomy using a representative agent? The answer to this question hinges on the answer to a related question: What is the goal of a microfoundational model? What is a microfoundational model supposed to accomplish? If the goal of a microfoundational model is to ground macroeconomics in contemporaneous microeconomic theory, then a representative agent model is fundamentally incompatible with the goal of providing microfoundations.

The idea of microfoundations seems to assume implicitly that there is some monolithic entity we can call "micro theory." Explaining macroeconomics merely in terms of microeconomic agents is insufficient; it must be explained in terms of microeconomic agents who follow acceptable behavior. The role of micro theory is to tell us what constitutes "acceptable behavior."

Starting with the assumption that such a micro theory of the individual exists, that we can explain individual behavior at the microeconomic level quite well, the ability of a representative agent model to provide microfoundations does not follow. The fact that the whole of macroeconomics is explainable in terms of the actions of microeconomic agents in no way implies that the whole of macroeconomics is explainable in terms of the actions of a *representative* microeconomic agent.

Consider what would be necessary for representative agent models to provide microfoundations. A representative agent model might be sufficient if all agents were independent of one another, if their behavior were explainable without reference to agents different from themselves. We would need a micro theory in which interactions among heterogeneous agents played no role in explaining behavior. On the other hand, if micro theory asserts the importance of interactions and heterogeneous agents, then a representative agent model would be unable to mimic the diverse activities which actually generate macroeconomic behavior.

Representative agent models can provide microfoundations in a micro theory in which nothing really matters. However, current micro theory looks nothing like this. In fact, the whole notion of micro theory as a monolithic entity is wrong. Rather, there are lots of micro theories. At best, representative agent models can provide microfoundations of a *certain* type. It isn't at all clear that the type of microfoundations that representative agent models can provide is the most accurate or even the most interesting.

Microeconomic models with asymmetric information or strategic interactions cannot be provided by a representative agent model. Microeconomic models with interesting cooperative or noncooperative games between agents cannot be provided by a representative agent model. Becker (1974) provides a laundry list of papers in which social interactions between different people matter:

> Pigou (1903), Fisher (1926, 102), and Panteleoni (1898) included attributes of others in utility functions (but did nothing with them). In recent literature, "demonstration" and "relative income" effects on savings and consumption [Brady and Friedman, 1947; Duesenbery, 1949; Johnson, 1952], "bandwagon" and "snob" influences on ordinary consumption theory [Leibenstein, 1950], and the economics of philanthropic contributions [Vickery, 1962; Schwartz, 1970; Alchian and Allen,

> 1967, 135–42; Boulding, 1973] have been discussed. . . . Further reflection gradually convinced me that the emphasis of earlier economists deserved to be taken much more seriously because social interactions had significance far transcending the special cases discussed by myself [Becker, 1971, 1961, 1968] and others.
>
> (Becker, 1974, p. 1065)

Indeed, a whole host of possible microeconomic models is impossible to incorporate in a representative agent model. There is furthermore no need to decide which micro theory is correct to evaluate the use of a representative agent model. If *any* of these microeconomic theories in which interactions matter is correct – not all, but any one of them – then a representative agent model is not providing microfoundations. A representative agent model would still be microeconomic, but it wouldn't be accurate microeconomics.

So what does contemporary microeconomics look like? Consider Porter's (1991, p. 553) *Journal of Economic Literature* review of the *Handbook of Industrial Organization* (Schmalensee and Willig, 1989): "Research in Industrial Organization has undergone a dramatic change in the last 20 years. Neo-classical decision-theoretic analysis and competitive general equilibrium theory have been supplanted almost completely by non-cooperative game theory." Representative agent models are thus at best using the microeconomics of a quarter-century ago; if representative agent models provide microfoundations, a host of *microeconomic* theorists in the past twenty years have done nothing to enhance our understanding of the *microeconomy.*

Or consider Stoker's *Journal of Economic Literature* review of the aggregation literature:

> In broader terms, to the author's knowledge there are no studies of disaggregated, micro level data that fail to find strong systematic evidence of individual differences in economic behavior, whether one is concerned with demographic differences of families or industry effects in production.
>
> (Stoker, 1993, pp. 1827–8)

Representative agent models cannot incorporate this sort of microeconomic research. Or consider Kirman's *Journal of Economic Perspectives* discussion of the representative agent literature:

> As the complexity of economic models increases – with the addition of uncertainty, infinite horizons, infinite commodity spaces, and so on – the plausibility of the single representative agent, acting optimally in all markets and at all times, diminishes. An alternative and attractive approach is offered by game theory, where the interaction between heterogeneous individuals with conflicting interests is seriously taken into account.
>
> (Kirman, 1992, p. 131)

Similarly, consider these remarks from Hahn:

> Thus we have seen economists abandoning attempts to understand the central question of our subject, namely: how do decentralized choices interact and perhaps get coordinated in favour of a theory according to which an economy is to be understood as the outcome of the maximisation of a representative agent's utility over an infinite future? Apart from purely theoretical objections it is clear that this sort of thing heralds the decadence of endeavour just as clearly as Trajan's column heralded the decadence of Rome. It is the last twitch and gasp of a dying method. It rescues rational choice by ignoring every one of the questions pressing for attention. Moreover, those who pursue this line defend it on the grounds that it "fits the data". Nothing could illustrate better than this that the habits of proof and argument are gone.
> (Hahn, 1991, p. 49)

Et cetera, et cetera, et cetera. An entire volume could be devoted to a discussion of microeconomic theories that are fundamentally inconsistent with a representative agent. One could go model by model, explicating each and every step, but what after all would be the point of such a volume? Does anyone really doubt it could be done?

The sort of microeconomics we see in a representative agent model looks nothing like the sort of microeconomics we see in many modern microeconomic models. It seems almost trivial to note that the simple representative agent maximizing utility subject to a production function is hardly the state of microeconomic theory – if it were, that first-year course in graduate microeconomics would not be so long. Yet, for some reason, the perception persists among macroeconomists that the representative agent framework is essentially a modern microeconomic framework.

Can we rescue the representative agent model by arguing that, while modern microeconomic theory is richer than the representative agent framework would suggest, using a representative agent is a useful approximation? Can we get around this problem by simply noting that in order to gain tractability we must give up some elements of realism or complexity? We could of course argue exactly this. However, if the argument for representative agent models is that they are a useful approximation, then we cannot simultaneously argue that representative agent models provide solid microfoundations. Arguing that representative agent models are simply a good approximation is exactly the same as arguing that they are not microfoundational models, that they are not rigorously derived from actual microeconomic theory. In fact, if the argument for representative agent models becomes simply that they are approximations for true microfoundational models, then it is hard to see why we need to provide the representative agent at all – why not just start with the aggregate equations which are also an approximation of the microfoundational model?

So representative agent models can only be considered microfoundational models if microeconomics is extremely simple. If, on the other hand, microeconomic theory is rich, involving the complex interactions of heterogeneous agents, representative agent models are no more or less microfoundational than the old-fashioned Keynesian consumption function.

THE FALLACY OF COMPOSITION

There is a further conceptual problem with the use of the representative agent methodology. The whole premise of using a representative agent model to provide microfoundations is "the simple principle, already familiar to us in statics, that the behavior of a group of individuals, or group of firms, obeys the same laws as the behavior of a single unit" (Hicks, 1946, p. 245). Representative agent models are premised on the belief that the activity of the aggregate economy looks like the activity of a single giant entity which acts exactly like the micro entities which comprise the aggregate. Thus, if we understand the micro agents, we understand the aggregate.

This description of representative agent models should look very familiar. It is precisely a restatement of the fallacy of composition: "A fallacy in which what is true of a part is, on that account alone, alleged to be true of the whole" (Samuelson, 1951, p. 10).

There is, however, absolutely no reason for the aggregate to look like an individual agent; indeed, there is no reason to expect such a similarity with a high degree of probability. Microfoundations is not the issue here. Even if all of the aggregate is explainable in terms of micro entities, there is no logical reason to assume that the aggregate will resemble the micro entities in any manner. The general point was made quite eloquently by Ernest van den Haag in a study of the criminal justice system:

> Philosophers have lately taken to calling "rational reconstruction" the kind of analysis to which I mean to subject the whole problem of punishing lawbreakers. Indeed my analysis will be as rational as I can make it. But I shall bear in mind (and I hope the readers will) that neither society itself nor its institutions (such as punishment) are cemented merely by reason, or rationally designed, or motivated. The social order can be rationally analyzed, but it is historically accumulated and actively held together, made to cohere, by affective bonds, by traditional institutions, and by fundamental needs. To analyze the regularities displayed by bees, whether in a hive or swarming, scientists may construct a rational theory to explain, to predict, and even to influence their behavior. Scientists can do so without attributing the rationality of their analysis

> to its subject and without imitating (or being infected by) its nonrationality. So with the analysis of the social institution of punishment.
>
> (van den Haag, 1975, pp. 6–7)

And so with the study of the macroeconomy.

In macroeconomics, this general point cuts both ways. First, there is no reason to assume that the macroeconomy will exhibit the same rationality that is found among the microeconomic agents. This point is well known among economists in other contexts. For example, the Condorcet paradox tells us that even though every agent in the economy has transitive preferences, aggregate preferences derived by pairwise majority voting may not be transitive. Similarly, as we saw in our discussion of Jerison (1990) in Chapter 5, the representative agent may have a very different preference structure from every single agent in the economy.

The general problem also runs in the opposite direction: the macroeconomy may exhibit a rationality that is not found in the microeconomic agents. Again, economists recognize this point in other contexts but seem to forget it when their attention turns to representative agent models. Consider the establishment of societal institutions, e.g., markets or political systems. As Hayek (1946, p. 8) put it, "[T]he spontaneous collaboration of free men often creates things which are greater than their individual minds can ever fully comprehend."

The fallacy of composition is an inherent part of using a representative agent model. In fact, quite remarkably, the representative agent model is exactly the embodiment of the fallacy of composition. Coleman notes:

> Social theory has too often taken the easy path of creating, conceptually, exactly that kind of creature at the micro level that by simple aggregation will produce the observed systemic behavior – whether that systemic behavior is the orderly and mundane functioning of a bureaucracy or the spontaneous and emotional outbursts of a crowd.
>
> (Coleman, 1990, p. 197)

Theorizing of this sort is ignoring the fact that individuals of a particular type may interact in such a manner that a very different type of aggregate behavior is observed.

Schelling (1978) provides a host of examples of how microbehavior of one type can result in macrobehavior of a different type. As a simple example, consider Schelling's (1978, p. 187) tale of two lunch rooms. Suppose there are a hundred people and every one of them would like to eat in the lunch room which has closest to fifty-five people in it. How will people divide themselves between the two lunch rooms? The result will not be fifty-five in one room and forty-five in the other, nor will it even be fifty in each room. Rather, all one hundred will end up eating in the same lunch room. Observing this aggregate behavior a representative agent model would posit that the representative agent preferred to eat in the same room as everyone else. However, in fact, every single person would be happier if the people were divided equally between the rooms than be in the situation that actually resulted, with everyone in the same room. The problem is that this equal division of people is not stable. The representative agent model is incapable of capturing these sorts of interaction effects.

A couple of papers suffice to demonstrate the sorts of errors to which ignoring the fallacy of composition leads. Mankiw (1985) shows that if firms face small menu costs, money may be non-neutral. Suppose the central bank increases the money supply, thereby raising aggregate demand. In the absence of menu costs, it is now optimal for firms to increase the prices they charge for their goods; if they do not raise their prices, they will not be maximizing profits. However, if there are menu costs and if the menu costs are larger than the loss in profits from not adjusting prices, then firms will optimally choose not to change prices. This price stickiness means that the change in the money supply is not neutral; aggregate output is increased.

However, Caplin and Spulber (1987) examine whether price stickiness at the level of an individual firm carries over to price stickiness in the aggregate. They make a relatively simple change in the basic story: they assume that not all firms start out at the same price. In particular, they assume firms follow an (*s,* S) pricing model; S is the optimal real price to charge; if it were costless to set prices at any level, firms would choose S. However, due to menu costs, firms do not always set prices equal to the optimal level. If there is inflation in the economy, then the real price received by the firm falls. When the real price falls below s, the firm readjusts its price to S. Thus, at the level of the individual firm, there is price stickiness when the money supply increases; small increases in the money supply do not cause the individual firm to change its prices.

Caplin and Spulber, however, do not assume that there is only one firm. Instead, they assume there are lots of firms and that the prices being charged by the assorted firms are evenly spread out over the interval (*s,* S). Now when the money supply increases, the real price received by all firms falls. Some of these firms hit the s threshold and immediately increase their price to S; others do not adjust their prices. However, in the aggregate there is exactly the same distribution of firms over possible real prices as there was at the start; firms are still evenly spread out over the (*s,* S) interval. Although there is price stickiness at the level of the individual firm, there is no price stickiness in the aggregate; money is neutral.[3]

So if we use a representative agent model to examine the aggregate effect of price stickiness, we find that menu costs can cause aggregate price stickiness and thus enable money to have real effects. However, this is simply the fallacy of composition. If there is more than one firm and the firms start out at different real prices, then even though each one of the firms individually has sticky prices, in the aggregate there is no price stickiness.

A more recent paper, by Caplin and Leahy (1991), shows that the money neutrality in the aggregate arises only in the special case when prices are uniformly increasing. If prices can both rise and fall, then the sticky prices at the firm level can lead to sticky prices in the aggregate.[4] The non-neutrality in this case arises from the fact that firms get bunched up around the optimal price. Sutherland (1995) provides a more general demonstration that if the shocks are always in the same direction, money is neutral, but if the shocks vary in sign, then there is aggregate price rigidity.

Does the fact that we can get aggregate price rigidity in some cases validate the representative agent approach? Can we say that we have found a way around the fallacy of composition? Not at all. First, we can note that the aggregate price rigidity arises in cases when the dispersion of firms across possible prices is reduced; in other words, we can get aggregate price rigidity by reducing the heterogeneity of firms. This should not be very surprising. In the extreme case, if we could get all firms charging exactly the same price, then the aggregate economy will have exactly the same price rigidity as the representative firm. Second, and more fundamentally, unless all firms charge exactly the same price, the aggregate will exhibit less price stickiness than the representative agent model indicates. In the representative agent model, all firms keep the same price until a threshold is hit, then all firms change prices simultaneously. Prices are thus completely rigid for a long time and then suddenly move. However, when we allow for firms to be at different real prices, then it is no longer true that all firms experience stickiness at the same time. Instead, there will be times when some firms change their prices and others do not. The aggregate price will not move sufficiently to maintain constant real prices, but the real price will not be as stable as predicted by the representative agent model. We can thus see a range of possibilities, from the complete price flexibility of Caplin and Spulber (1987) when firms are very heterogeneous to the high inflexibility of the representative agent model when firms are completely homogeneous. Far from being some sort of "average" or "representative" possibility, then, the representative agent model turns out to be the extreme case of price inflexibility. To assume that the aggregate will look like the "representative" firm is thus still a fallacy of composition.

A similar result is found in Caballero (1992). Briefly, he examines whether or not heterogeneities in hiring and firing workers at the individual firm level show up as heterogeneities in job-creation and -destruction at the aggregate level. In the model, firms hire workers in good times and fire them in bad times. However, the number of workers hired in good times is smaller than the number of workers fired in bad times. In other words, when firms grow they hire only a small number of people, but when they shrink they fire a large number of people. There is thus an asymmetry in the volatility of hiring and firing at the firm level. However, this asymmetry does not automatically carry over to the aggregate level. When there is sufficient heterogeneity among firms, the individual-level asymmetries do not appear at the aggregate level.

These examples are not isolated incidents. The representative agent model is literally the very embodiment of the fallacy of composition; it purports to derive rigorously how an individual agent behaves and then applies those rules of behavior to the entire economy. But as Samuelson wrote in his introductory textbook a half a century ago:

> What is true for each is not necessarily true for all; and conversely, what is true for all may be quite false for each individual. Especially where his own interests are at stake, an individual tends to look only at the immediate effects upon himself of an economic event. A worker thrown out of employment in the buggy industry cannot be expected to reflect that new jobs may have been created in the automobile industry. But we must be prepared to do so. The reader should try to give other examples of this fallacy; *e.g.,* standing on tiptoes at a parade, counterfeiting meat-ration coupons, cutting production in order to raise one's prices, etc.
>
> (Samuelson, 1948, p. 9)

Indeed, when we think about the sorts of results in Caplin and Spulber (1987) and Caballero (1992), Samuelson's comment seems strikingly true: "Once explained, each is so obvious that you will wonder how anyone could have failed to notice it" (Samuelson, 1948, p. 8).

Now, none of this is in any way a refutation of an argument that we need or desire microfoundations. The sole point is that, even if we can explain aggregates in terms of microeconomic agents, there is no reason to assume that the aggregates will look exactly like the microeconomic agents from which they are built. We can study the behavior of the beehive by studying the behavior of bees, but that does not mean that we can assume that the hive works like one giant bee.

CONCLUDING NOTE

Can we get around this problem by arguing that it doesn't really matter what the microeconomic foundations look like as long as we get the right aggregate implications? Can we argue that while microeconomic theory is actually complex, the representative agent model gives us something that is a good approximation at the aggregate level? Well, we can obviously make such an argument, but we cannot simultaneously argue that we need to provide microfoundations. If all the representative agent model is doing is to serve as an aggregate proxy for the true microeconomic foundations, why bother with the representative agent at all? Why not just write down a macroeconomic model? If the model isn't using actual microeconomic theory, it isn't providing microfoundations.

At best, representative agent models can give us foundations in a degenerate, simplistic, and rather banal pseudo-microeconomics. But what is the good of that? If we really want microfoundations, we should make modern microeconomic theory the foundation. If we aren't going to do that, why pretend to be providing microfoundations? We might as well ground our models in any old assertion about human behavior. In fact, we might as well ground macroeconomics in a theory of the behavior of monkeys.

13

THE MYTH OF MICROFOUNDATIONS

THE MYTH

There is an ancient myth passed on from one generation of economists to the next. In the story, economists set out like a brave band of Argonauts in quest of that golden fleece called "A Microfoundational Model of the Macroeconomy." Our hearty band must face many hardships along the way but, in the end, the golden fleece is attained.

Like most myths, this one serves a purpose. It teaches the young how to live a good and virtuous life. It teaches them the aim of life and how to find a ship that will get them there. But there is always that great heretic lurking in the background. In 1937, Keynes wrote:

> My next difference from traditional theory concerns its apparent conviction that there is no necessity to work out a theory of the demand and supply of output *as a whole.* Will a fluctuation in investment, arising for the reasons just described, have any effect on the demand for output as a whole, and consequently on the scale of output and employment? What answer can the traditional theory make to this question? I believe it makes no answer at all, never having given the matter a single thought; the theory of effective demand, that is the demand for output as a whole, having been entirely neglected for more than a hundred years.
>
> (Keynes, 1937, p. 219)

If Keynes is right, if it is *necessary* to work out theories of *aggregate* demand and supply, then the microfoundations story is a myth. Lucas and Sargent (1979) assure us that Keynes is wrong, that if only Keynes had just had the tools we have today, that if only Keynes had had better sailing ships, he would not have made such preposterous claims. Lucas and Sargent assure us that a microfoundational, equilibrium model of the economy is within our grasp, that we should see some success within a decade. Don't listen to that raving old fool over there; he knows not of what he speaks.

Scientific progress has not been kind to the myth of microfoundations. As we learn more and more, it becomes ever more apparent that Keynes was right, that it is not possible to provide microfoundations now nor will it be at any foreseeable point in the future. The realization is spreading slowly that the golden fleece is not really at the end of the ocean, that we need once again to take up the task Keynes set for us of building up theories of aggregate economics. As the evidence mounts, it seems ever more unlikely that in 2037 a future Keynes will declare that "the theory of effective demand, that is the demand for output as a whole, [has] been entirely neglected for more than a hundred years."

To avoid confusion, let us be very clear at the outset exactly what is being discussed. The new classicals have long argued that we must "explicitly formulat[e] dynamic general equilibrium models at the level of objective functions, constraint sets, and market clearing conditions of their counterparts" (Sargent, 1982, p. 383). That it is possible to study the macroeconomy in this manner is the myth. It is this sort of microfoundations, the sort advocated by new classicals, that this chapter evaluates.

Let us also be clear what is not being directly discussed in this chapter. It is not a myth that only people act, that all aggregate activity must be the result of the actions of micro agents. Of course only people act; of course there is no monster out there called aggregate output with a life of its own; of course explanations of aggregate activity *may* refer to the behavior of people. Even Keynes (1936) motivated his aggregate consumption function with a discussion of the objective and subjective factors influencing the propensity to consume (chs 8 and 9) and his aggregate investment function with a discussion of the expectations of entrepreneurs (ch. 12). However, this is a very different thing from an insistence that any model of aggregate activity must be rigorously derived from an individual's optimization problem. As we saw in Chapters 9 and 11, there is a world of difference between the kind of "microfoundations" used by Friedman in deriving the permanent income hypothesis and the "microfoundations" used by Hall in deriving the same thing.

WHAT FOUNDATION?

The new classical assault on macroeconomic theorizing is a quite serious affair. It is not simply a matter of using microeconomic models to inform macroeconomic models; rather, the very legitimacy of macroeconomic models is questionable. All macroeconomic reasoning should be carried on using microeconomic models; the entire notion of macroeconomics is suspect:

> The most interesting recent developments in macroeconomic theory seem to me describable as the reincorporation of aggregative problems such as inflation and the business cycle within the general framework of "microeconomic" theory. If these developments succeed, the term "macroeconomic" will simply disappear from use

> and the modifier "micro" will become superfluous. We will simply speak, as did Smith, Ricardo, Marshall and Walras, of *economic* theory. If we are honest, we will have to face the fact that at any given time there will be phenomena that are well-understood from the point of view of the economic theory we have, and other phenomena that are not. We will be tempted, I am sure, to relieve the discomfort induced by discrepancies between theory and facts by saying that the ill-understood facts are the province of some other, different kind of economic theory. Keynesian "macroeconomics" was, I think, a surrender (under great duress) to this temptation. It led to the abandonment, for a class of problems of great importance, of the use of the only "engine for the discovery of truth" that we have in economics.
>
> (Lucas, 1987, pp. 107–8)

Now there is a lot in that passage and it will take us some time to explore all the nuances, but let us first note the breathtaking, wholesale rejection of macroeconomics contained therein. If ever there was a call for microfoundations, this is it.

Lucas is arguing that we do not understand macroeconomic relations until we can motivate them from microeconomic theory. But one could equally well argue that we do not understand microeconomic relations until we can motivate macroeconomic theory from them. Both claims are true in a sense and equally worthless.

When proponents of microfoundations claim that macroeconomic theory must be derived from microeconomic theory, they are in effect claiming that we understand the microeconomy and we need to figure out what macroeconomic relations come out of this well-defined theory. In this view, microeconomic theory is our *terra firma* on which we can stand while trying to figure out the nebulous world of macroeconomic theory. On the other hand, by reversing the emphasis, we can note that we do see macroeconomic regularities, and until microeconomic theory can explain these regularities, we do not understand the microeconomy. In this view, macroeconomic theory is our *terra firma* on which we stand while trying to figure out microeconomic theory. We do not know how to reconcile macroeconomic theory and microeconomic theory. Given that, how do we choose which theory is correct and which theory needs to be explained in light of the correct theory?

To microfoundations' proponents, the question just posed may well seem absurd. As Lucas argues, the answer is obvious: microeconomic theory is "the only 'engine for the discovery of truth' that we have in economics." Microeconomic theory is our rock, our foundation on which all else is to be built.

But whence comes this faith in the foundational stability of microeconomics? Is microeconomic theory really so solid? Not necessarily. In fact, not even the standard models of profit and utility maximization are undeniably true; both have been widely examined and modified even by research firmly in the neoclassical tradition. Microeconomists are constantly re-evaluating these maximization theories, looking for better theories to explain both firm and individual behavior. The uncertainty surrounding firm behavior is vast:

> It has been argued that business firms actually maximize a multivariate utility function that includes profits, leisure, prestige, liquidity, control etcetera; that they maximize total sales subject to a minimum level of profits rather than profits themselves; that they do not maximize at all but "satisfice" by adjusting their profit targets in the light of experience so as to reach satisfactory levels; that they cannot maximize because of prevailing uncertainty and, therefore, adopt rules-of-thumb like full-cost pricing; and that they do not want to maximize but instead to survive and hence operate in terms of administrative rules that serve to keep them one step ahead of their rivals. Such criticisms and their associated proposals for reconstructions of the theory of business behavior have greatly multiplied in the last thirty years, virtually amounting to what some commentators have described as the breakup of the traditional theory of the firm (Nordquist, 1967).
>
> (Blaug, 1980, pp. 175–6)

After reviewing the assorted theories of the firm in their *Journal of Economic Literature* survey, Cyert and Hendrick (1972, p. 409) concluded, "[W]e wonder whether economics can remain an empirical science and continue to ignore the actual decision-making processes of real firms." With this sort of uncertainty around the theory of profit maximization, it seems entirely legitimate to wonder how solid a firm-based microfoundational model really is.

The problems are no different for the theories of utility maximization. While we can always define the general idea of utility maximization so as to be tautological, we still have to write down a specific utility-maximization problem at some point. How certain are we that the way we write down these models is true? Once again, this foundation is far from firm. For example:

> Standard economic analyses often rely on the modern conception of the utility function that resembles a black box. People reveal their utility function through their choices. However, this modern view of utility is not the only possible conception. An earlier view, that associated utility with the pleasures of consumption, prevailed in economics from the days of Daniel Bernoulli and Jeremy Bentham and through the nineteenth century (George Loewenstein, 1991; George Stigler, 1950). We shall refer to this older notion as *experienced utility,* in contrast to the revealed preference notion we call *decision utility.* Since experienced utility is presumably what people try to

> maximize and what welfare policies are all about, the use of decision utility in economic analyses can only be justified if experience and decision utility coincide. Recent psychological research, however, suggests several reasons why the concepts might in fact diverge (Kahneman and Carol Varey, 1991; Kahneman and Jackie Snell, 1990).
>
> (Kahneman and Thaler, 1991, p. 341)

As a specific example of the debate among microeconomists about the standard utility function, consider the rate of time preference. The standard theory assumes a positive rate of time preference; however, Loewenstein and Prelec (1991) present a solid case that the rate of time preference among real people may in fact be negative. The foundation of standard models of utility maximization seems less than solid.

We can additionally note the conclusions reached in the summaries of the current state of the field for *The New Palgrave* (Eatwell *et al.,* 1987). In the article on the theory of the firm, we read, "[W]hat is the scope and purpose of the theory of the firm? Indeed, is there *a* theory of the firm at all? Perhaps not. There is a file of optimizing models. . . . there is clearly no such thing as *a* theory of the firm" (Archibald, 1987, pp. 361–2). Similarly, in the article on consumers, we read:

> In this essay, the major themes will be the interplay between theory and evidence in the study of consumers' expenditure and its composition. If economists have any serious claim to being scientists, it should be clearly visible here. The best minds in the profession have worked on the theory of consumption and on its empirical implementation, and there have always been more data available than could possibly be examined. I hope to show that there have been some stunning successes, where elegant models have yielded far from obvious predictions that have been well vindicated by the evidence. But there is much that remains to be done, and much that needs to be put right. Many of the standard presumptions of economics remain just that, assumptions unsupported by evidence, and while modern price theory is logically consistent and theoretically well developed, it is far from having that solid body of empirical support and proven usefulness that characterizes similar theories in the natural sciences.
>
> (Deaton, 1987, p. 592)

We thus have reason to wonder how far our "engine for the discovery of truth" will get before it needs to be rebuilt. As Thaler notes at the end of a collection of papers exploring assorted economic anomalies:

> The primary lesson here is admittedly a depressing one for economic theorists. The lesson is that their job is much harder than we may have previously thought. Writing down a model of rational behavior and turning the crank may not be enough, and

> writing down a model of less than fully rational behavior is difficult for two reasons. First, it is not generally possible to build good descriptive models without collecting data, and many theorists claim to have a strong allergic reaction to data. Second, rational models tend to be simple and elegant with precise predictions, while behavioral models tend to be complicated, and messy, with much vaguer predictions. But look at it this way. Would you rather be elegant and precisely wrong, or messy and vaguely right?
>
> (Thaler, 1992, p. 198)

Even within the realm of microeconomic theories using profit and utility maximization, we are still forced to answer the question: *Which* microeconomic theory is supposed to serve as the foundation? Moreover, if there are two competing microeconomic theories, what does it mean to have a micro *foundation*? So, for example, when Hahn and Solow (1995, p. 3) write, "In the modern spirit, however, resistance has to begin with alternative micro foundations," in what sense are the microeconomics providing a foundation? Isn't such a foundation inherently on sand?

Let us be very clear about the point of this discussion. This is not meant as an endorsement of these papers in whole or in part. Moreover, nothing in this discussion is in any way a suggestion that economists should abandon the idea of rational agents maximizing utility. The only thing being questioned is whether our current *simplified models* of utility maximization and our current *simplified models* of profit maximization are *necessarily* accurate. We are simply noting that there is considerable debate and discussion about our current set of *models,* that microeconomic theory is not a settled body of fact or opinion. The whole point is that when microfoundations proponents argue that macroeconomics must have a microeconomic foundation, we are entitled to ask, "What foundation?"

One could thus argue with every bit as much vehemence as Lucas that the real problem here is that microeconomic theorists have been unable to figure out how the *micro* economy works. We can look forward to the day when microeconomics advances to the point where aggregative problems such as inflation and the business cycle can be explained by microeconomic theory. At that time, the term "microeconomic" will disappear and the modifier "macro" will become superfluous.

In short, we have here a real muddle. It is just as easy to say that macroeconomics must be brought down to the level of microeconomic theory as it is to say that microeconomics must be brought up to the level of macroeconomic theory. In fact, we understand neither the microeconomy nor the macroeconomy. Now (to rewrite Lucas again), if we are honest, we will have to face the fact that at any given time there will be phenomena that are well understood from the point of view of macroeconomic theory that we cannot yet explain using microeconomic theory, and there will be phenomena for which the reverse is true. We will be tempted, I am sure, to relieve the discomfort induced by discrepancies between macroeconomic theory and microeconomic theory by denying that there are two theories and trying to subsume everything into one ill-understood economic theory. Representative agent models were, I think, a surrender to this temptation. They led to an abandonment, for a class of problems of great importance, of the use of the only "engine for the discovery of truth" that we have in macroeconomics.

Now, all this sort of thing is fine to a point, but such verbal convolutions don't get us very far. Lucas' bold argument for microfoundations is no more or less rigorous than this parallel argument against microfoundations. And so let us turn our attention to more concrete matters.

AGGREGATION GAIN

A fundamental premise of the microfoundations argument is that, if we have both an aggregate model and a disaggregate model, the disaggregate model is preferred. Part of the reason for preferring the disaggregate model is the existence of aggregation bias in the aggregate model; the disaggregate model suffers from no such bias. Similarly, it is individual people who act, not abstract aggregate constructs; disaggregate models thus directly model the entities that act and might be preferred on those grounds. Thus, to many people there seems to be a *prima facie* case for preferring disaggregate models. However, in the less abstract world of actual models that economists can (or do) write down and use, such arguments in favor of disaggregate models are not the only, or possibly not even particularly important, criteria.

When comparing aggregate and disaggregate models, aggregation bias is not the only concern. Specification bias is extremely important. There is no theoretical reason to assume that a disaggregate model suffers from less specification bias than an aggregate model, and in fact disaggregate models may suffer from greater specification bias. This point was first made by Grunfeld and Griliches:

> Our main argument will be that *in practice* we do not know enough about micro behavior to be able to specify micro equations perfectly. Hence, empirically estimated micro relations, whether those of individual consumers or of individual producers, should not be assumed to be perfectly specified either in an economic sense or in a statistical sense. Aggregation of economic variables can, and in fact frequently does, reduce those specification errors. Hence, aggregation does not only produce an aggregation error, but may also produce an aggregation gain.
>
> (Grunfeld and Griliches, 1960, p. 1)

Now this is a remarkable claim. Grunfeld and Griliches note that aggregation will be good when the aggregate equations are more accurately specified than the disaggregate equations. Aigner and Goldfeld (1974) extend the realm of aggregation gain to cases where the aggregate data are better than the corresponding disaggregate data. Since the argument for aggregation gain was made, there have been a large number of efforts at explaining when, why, or even if aggregation is good. (See, for example, Orcutt *et al.,* 1968; Edwards and Orcutt, 1969; Gupta, 1969; and Green, 1977.) Not surprisingly, the theoretical literature has mixed results. Theoretically, you can find the absence of aggregation gain by assuming extensive knowledge of the true microeconomic relationships; theoretically, you can find the presence of aggregation gain by assuming limited knowledge of the true microeconomic relationships.

Thus, the existence of aggregation gain is actually an empirical matter. Such empirical tests can also be expected to give mixed results since they depend on how accurately the microeconomic relationships are estimated. For example, Pesaran *et al.* (1989) examine the employment demand functions of forty industries. They compare the predictions for the disaggregate equations to the predictions from an aggregate equation for both the set of all forty industries and the subset of twenty-three manufacturing industries. They get quite mixed results. For the set of all forty industries, the disaggregate equations are preferable to an aggregate equation; for the set of twenty-three manufacturing industries, the aggregate equation is preferable to the disaggregate equations. The authors attribute the result from the set of twenty-three manufacturing firms to specification errors at the microeconomic level. The results in Pesaran *et al.* are interesting, not only because they demonstrate that there is no unique answer to the question of whether aggregation produces gains, but more importantly because it removes the question of whether the results are driven by the predisposition of the researcher. There are no theoretical absolutes in the matter of aggregation gain.

The aggregation gain literature is important because it demonstrates that there is no *a priori* theoretical imperative to assume that *actual* microeconomic models are better than *actual* macroeconomic models. There is no reason to assume that there is a smaller specification bias at the microeconomic level. The whole case for insisting on microfoundations thus turns out to be an empirical matter. If new classical economists want to insist that their microfoundational models are superior to other less microfoundational models, then the case must be made empirically and not via theoretical absolutism.

The benefits of aggregation gain can possibly be eliminated by better microeconomic models and better microeconomic data. However, even if we improve our microeconomic models, there may still be large gains to using aggregate models. The true microeconomy has a vast number of individual people; a true representation of the microeconomy would thus need hundreds of millions of complex microeconomic equations. A one-to-one mapping of reality into our model would solve the problem of inadequate microeconomic equations. However, such a one-to-one mapping is not practical. So in practice our microeconomic models must be simplifications, thereby admitting at least some specification bias.

Even if we grant that there is *some* set of disaggregate equations that will be better than an aggregate equation, there is no *a priori* reason to assume that there is a *simple* set of disaggregate equations that will dominate an aggregate equation. The microfoundations argument implicitly assumes, without evidence, that the aggregate economy can be well represented by a simple, tractable model of the microeconomy. In the extreme case, the microfoundations proponents argue that a model with a single agent will suffice. Yet whence comes this assurance? How do we know that a simplified microeconomic model dominates a simplified macroeconomic model? We do not know this and, moreover, we cannot know this as a theoretical matter. It is, as we have said before, an empirical matter.

The case for microfoundations thus loses much of its punch. In this less than perfect world where we do not have a perfect (or even an extremely good) understanding of the appropriate specification of microeconomic agents, where we do not have perfectly measured microeconomic variables, where we do not have the ability to handle large, complicated sets of microeconomic equations, there should be no theoretical predisposition toward microeconomic models. A comparatively simple macroeconomic model could well do better.

If new classical economists want to argue that their microeconomic representative agent models are superior to less disaggregated models, they cannot do so simply by asserting that good models go "beyond supply and demand curves." They should go further and show that their models are better at whatever it is they think models should be doing. If the criterion is ability to predict the effects of regime changes (cf. Lucas' comments in Snowden *et al.,* 1995, p. 221), then they should provide models that are superior at such predictions. Ironically, however, the most famous new classical work demonstrating the importance of modeling the effects of regime changes, Sargent and Wallace's (1975, 1976) policy ineffectiveness papers, uses a pure macroeconomic model.

THEORY OF THE FIRM

The foregoing argument, that at times aggregate models may be preferable to disaggregate models, may strike some as fundamentally odd. For some, it may almost be an article of faith that aggregate models are inferior to disaggregated models. However, it should not seem odd at all; in fact, it should seem perfectly normal. While they generally do not think about it in these terms, economists are quite accustomed to using aggregate entities even in work commonly labeled microfoundational or microeconomic.

Consider the firm. What is a firm if not an aggregation of individual people? What do we mean when we say that firms maximize profits? Firms themselves can do nothing of the sort. We mean that there is a group of people who act in such a manner that this aggregate entity can reasonably be modeled as if it were an entity maximizing profits. All models in which there is a firm are in this sense aggregate models.

So why do economists use firms in microeconomic work? Can't we levy a host of criticisms against the very construct? Using a firm is completely *ad hoc;* we need to find the microfoundations of the firm. We cannot simply assume that firms maximize profits; we need to set up explicit optimization problems at the level of individual people that will generate the aggregate implication of a firm maximizing profits.

In fact, we find a true oddity in the new classical microfoundations literature. Both Sargent (1981) and Hansen and Sargent (1980) are arguments for and examples of the use of microfoundations in macroeconomic work. Yet both papers set up models using a representative *firm.* Now, as we know, firms are not structural. So what Sargent (1981) said of "structural" consumption functions would seem to apply to the firm's decision rules in Sargent's paper: "In dynamic settings, regarding the parameters of these rules of choice as structural or invariant under interventions violates our simple principle from economic theory" (Sargent, 1981, p. 214). There is thus a theoretical presumption that the parameters in the firm's decision rule will change whenever there is a change in policy. All historical work using firms is flawed. We need models that will generate firm behavior at the level of individual people's optimization and the constraints they face.

A consistent application of the logic of the new classical microfoundations arguments compels us to reject much new classical work as insufficiently microfoundational. Models with firms are purely *ad hoc;* they are not grounded in individual optimization. One could easily write a paper entitled "Beyond Firm Supply Curves in Macroeconomics."

Moreover, it is not a trivial matter to write down an individual optimization problem that yields the behavior of a firm. As Coleman (1990, p. 145) notes, the behavior of subordinates in a firm is not the sort of behavior we typically model: "The subordinate's actions seem to violate the principle of rational action in that they are directed toward maximizing realization of the superordinate's interests rather than his own." Simply assuming that the workers in a firm always act so as to maximize the firm's profits is very poor economics.

So why do economists who advocate microfoundations include such aggregate constructs as firms in their models? There have been attempts to explain firm behavior by looking at the actions of individual people. The principal– agent problems are good examples. However, for most purposes, economists tend to stick with the traditional theory of the firm maximizing profits. Why? Presumably for the simple reason that to use a firm makes for a better model than to start with the utility-maximization problem of all of the workers in the firm. Using a firm could be defended on the grounds that the predictions from models using firms are superior to those from models involving individual utility maximization. Firms can also be defended on the grounds of simplicity; it is simpler to start with firms than to go all the way down to individual people.

However, the most basic reason for using the construct of the firm in economic work is the belief that firms are more than simply a loose confederation of individual people. Somehow, a group of people acting in concert in an organization we call a firm is very different from that same group of people acting in concert in an organization we call a mob or a neighborhood or in no organization at all. As Coleman (1990, p. 427) notes, a firm is really a set of job titles with specific responsibilities toward other job titles: "The social structure exists independently of the persons occupying positions within it, like a city whose buildings exist independently of the persons who occupy them." One term for such a phenomenon is "emergence." When people combine into a firm, a new entity emerges that is more than simply the sum of its parts. It acts in ways different from those that a simple examination of the utility functions of its employees might predict. If so, there is legitimate reason to study this entity, the firm, independently of a study of its constituent parts, the people who work there.

Economists are thus very used to the idea that it is quite legitimate to study aggregate entities without always starting at the level of individual optimizers. A firm is a *system* that organizes the actions of multiple individuals so that the resulting behavior is very different from what would be the behavior of a single individual.

So, when we turn to the matter of macroeconomics, it does not seem such an oddity that we might think that the whole (the macroeconomy) is more than the sum of its parts (people and firms). It does not seem such an oddity that a study of the aggregate economy is legitimate even if we do not start with firms or people. Yet economists have been arguing that a study of the aggregation of the activities related to the demand of a group of people, the aggregate demand curve, is illegitimate, while a study of the aggregation of the activities related to the supply of a group of people, the firm's supply curve, is entirely legitimate.

THE MACROECONOMY AS A SOCIAL SYSTEM

A large part of the reason that aggregate entities may behave in a very different manner than individuals in isolation is that there are large interdependencies between people. Coleman (1990) extensively discusses a broad set of these interdependencies, noting that actual people exhibit such behavior as trust and the transfer of authority. A simple, intriguing example is provided in his discussion of the origins of money. Coleman argues that money emerges in a world where people are trusted to varying degrees. While I may be willing to give you something now in exchange for something else later, I may not trust you to pay your obligation. However, there may be a third party (a respected merchant perhaps) whom I trust and who in turn trusts you. I may be willing to accept a claim on that third party in exchange for goods now because I trust that third party to pay on the obligation. In such a world, claims on widely trusted agents will start to circulate widely, thereby functioning as money.

What makes this story of the origins of money intriguing is that it explains the notorious inability of general equilibrium theory to incorporate money meaningfully into the model (see, for example, Hahn, 1965). In the Arrow– Debreu world, there is no real interdependency between agents; there is never a need to wonder if agent i actually trusts agent j. If in the real world this interrelationship (the existence or lack of trust) generates the need for money, then it is little wonder that the general equilibrium model is incompatible with money.

So, if Coleman is correct, money is a purely aggregate phenomenon. There is no need for money if I am Robinson Crusoe. There is no need for money even when Friday shows up on my island. However, as the economy expands, things like trust become important, giving rise to things like money. If you want to understand money, there is necessarily a study of aggregate phenomena.

Knowing about individual behavior is thus not enough to understand aggregate behavior. We must also know how individuals interact with one another:

> There is a broadly perpetrated fiction in modern society. . . . This fiction is that society consists of a set of independent individuals, each of whom acts to achieve goals that are independently arrived at, and that the functioning of the social system consists of the combination of these actions of independent individuals. . . [T]he fiction is just that – for individuals do not act independently, goals are not independently arrived at, and interests are not wholly selfish.
>
> (Coleman, 1990, pp. 300–1)

When we recognize that society is not actually composed of identical automatons, it becomes apparent that system-level activity can be quite complicated. How can we gain greater understanding of the aggregate system? Coleman suggests:

> With this conceptual structure the only *action* takes place at the level of individual actors, and the "system level" exists solely as emergent properties characterizing the system of action as a whole. It is only in this sense that there is behavior of the system. Nevertheless, system-level properties will result, so propositions may be generated at the level of the system.
>
> (Ibid., p. 28)

It is the study of these system-level economic properties that constitutes macroeconomics. In thinking about macroeconomics in this manner, there is no denying that it is individuals who do the consuming and saving. However, simply understanding how a single individual makes his consumption choice is not sufficient for understanding how the level of aggregate consumption is determined. When we go from the microeconomic level to the macroeconomic level, there is something else emerging.

Since going from the micro level to the macro level involves understanding how micro agents interact with one another, finding micro level explanations for aggregate behavior is a very complicated business. We cannot simply assume that the micro behavior is identical to the aggregate result. Working out how individuals interact to produce particular system-level behavior is a very tricky business. Coleman (1990) is clearly a praiseworthy example of how to make micro-to-macro transitions, but all his demonstrations are of relatively simple phenomena. There is nothing even approximately as complicated as an explanation of how changes in the aggregate money stock work their way through the system to change income and prices. However, even in the relatively simple phenomena that Coleman discusses, it is not at all clear that he has correctly analyzed how micro behavior actually generates the system-level behavior (see Frank, 1992, for a number of examples). When we start to examine a vastly complex social system like the macroeconomy, the difficulty of decomposing aggregate behavior into micro-level actions and interactions becomes quite severe.

Moreover, in order to even begin studying how aggregate behavior arises out of individual-level behavior, we need first to determine what aggregate behavior we are trying to explain. In order to understand how otherwise quiet people turn into a mob, it is first necessary to figure out what exactly a mob is. As Le Bon notes, a crowd is not a simple extension of the individual:[1]

> Contrary to an opinion which one is astonished to find coming from the pen of so acute a social philosopher as Herbert Spencer, in the aggregate which constitutes a crowd there is no sort of summing up of or an average struck between its elements. What really takes place is a combination followed by the creation of new characteristics just as in chemistry certain elements when brought into contact – bases and acids, for example – combine to form a new body possessing properties quite different from those of the bodies that have served to form it.
>
> (Le Bon, 1895 [1960], p. 27)

Moreover, causality does not run just one way. Not only do the actions of microeconomic agents combine to create macroeconomic phenomena, but these macroeconomic phenomena influence microeconomic agents. Coleman (1990) calls these relationships the micro-to-macro and the macro-to-micro transitions and notes that good social science makes both transitions correctly.

Macroeconomics is thus a difficult and rewarding field of study. If we were all-knowing, we could see how the interactions of 260 million people result in aggregate money demand or consumption; we could decompose the aggregate into its 260 million constituent parts. Short of such omniscience, finding perfect microfoundations for macroeconomics is impossible. The best we can hope for is small understandings of parts of the macroeconomy. Over time, these small understandings will hopefully build into bigger understandings; we certainly know a lot more about the aggregate economy today than was known when Keynes penned *The General Theory.*

A study of aggregate phenomena without reducing everything to the most basic building blocks is not some failing. It is not unscientific to study aggregates without instantly decomposing them into their parts. Weiss (1967), for example, provides a plethora of examples in the natural sciences of phenomena that are witnessed at an aggregate level but not understood at the level of their parts. There is no way to know if these phenomena can be explained with better research into the "microfoundations" of life; however, the lack of microfoundations does not prohibit scientists from studying the aggregate phenomena. Another striking example is the study of life itself: we know that man is simply a combination of chemical processes but, as one wag put it, "The only known way to reduce biology to chemistry is murder." Should the study of living organisms cease until we figure out how to resurrect the dead?

GENERAL EQUILIBRIUM THEORY

We have argued that it is a mistaken goal to insist that all study of the macroeconomy begin with a utility-maximization problem. There may be phenomena that are at present understandable only by studying the macroeconomy directly; there may even be phenomena for which we will never understand the process by which individual behavior aggregates into the aggregate phenomena. To insist that macroeconomics in its present state must be derived from microeconomics in its present state is to ignore the host of macroeconomic phenomena which we cannot so explain but which we might be able to explain in aggregate terms.

Some might argue that this is not what is meant by a call for microfoundations. It could be argued that the goal of microfoundations is nothing more than the incorporation of macroeconomics into the microeconomic Arrow– Debreu general equilibrium framework. Construing the microfoundations argument in this manner is in some ways less restrictive than construing it as a call to base everything on individual maximization problems. For example, we need not worry that firms are aggregate constructs because firms are an integral part of the general equilibrium framework. (Such an argument would, of course, beg the whole question.) However, construing the microfoundations argument in this manner is in other ways more restrictive. For example, the desirability of microfoundations now lives or dies with the ability of the Arrow–Debreu framework to explain aggregate phenomena.

As we shall see, if we construe the call for microfoundations as an insistence that macroeconomics should be based on Arrow–Debreu general equilibrium theory, then the microfoundations goal is already dead and its persistence in the literature is nothing more than evidence that macroeconomists do not take microeconomic theory very seriously.

It is not difficult to understand why general equilibrium theory has such allure for economists in general and macroeconomists in particular. The theory provides for an extensive model of the economy with individual consumers maximizing utility and individual firms maximizing profits, all interacting in competitive markets. Yet, despite this complexity, it can be shown that an equilibrium exists. A host of existence proofs were meticulously worked out. The simplicity of the notion and the general nature of the result captured the imagination of economists:

> [T]he notion that a social system moved by independent actions in pursuit of different values is consistent with a final coherent state of balance, and one in which the outcomes may be quite different from those intended by the agents, is surely the most important intellectual contribution that economic thought has made to the general understanding of social processes.
>
> (Arrow and Hahn, 1971, p. 1)

It isn't hard to see why economists like Arrow and Hahn were excited; the Arrow–Debreu model is simply an embodied version of Smith's invisible hand.

However, the party was not to last long. Having established the existence of an equilibrium point, economists began turning their attention to related matters. Knowing that an equilibrium point exists is all well and fine, but it doesn't get you very far. What else can we tell about the economy from the general equilibrium framework?

The answer to that question turned out to be quite depressing: very little can be inferred about the aggregate economy from a general equilibrium model. As Kirman (1989) subtitled a paper about this state of affairs, "The Emperor Has No Clothes."

The research began innocently enough. After showing that an equilibrium existed, people became interested in the question of whether it could be shown that the equilibrium was either unique or stable. In order to answer this question, the shape of the aggregate excess demand curve had to be determined. In a remarkable series of papers, Sonnenschein (1972, 1973, 1974), Mantel (1974, 1976), Debreu (1974) and Mas-Colell (1977) showed that in an economy in which every individual has a well-behaved excess demand function, there are only three restrictions on the aggregate excess demand function: (a) it satisfies Walras' law; (b) it is continuous; and (c) it is homogeneous of degree zero in prices.

That is it. *Nothing* else can be inferred. Any completely arbitrary function satisfying these three properties can be an aggregate excess demand function for an economy of well-behaved individuals. Having an economy in which every single agent obeys standard microeconomic rules of behavior tells us virtually nothing about the aggregate economy. For example,, not even something as basic as the Weak Axiom of Revealed Preference carries over from the microeconomic level to the macroeconomic level. (See Shafer and Sonnenschein (1982) for a complete, technical discussion of this literature and Kirman (1989) or Ingrao and Israel (1990) for good non-technical discussions.)

The implication can be stated in two ways. Even if we know that the microeconomy is well behaved, we know very little about the aggregate excess demand function. Or, given a completely arbitrary aggregate excess demand function that satisfies the three characteristics above, we can find a well-behaved microeconomy that generates that aggregate function.

These results are quite general, placing no restrictions on the distribution of agents' preferences or income distribution, and were initially met with skepticism. Deaton (1975, p. 237) argued that the results required "arbitrary manipulation of the income distribution and of preferences." (See also Grandmont, 1987, and Kirman's, 1989, discussion of it.) However, Kirman and Koch (1986) show that restricting the distribution of agents' preferences or income distribution does not solve the matter. As Kirman summarizes:

> Put another way, given an arbitrary excess demand function, no matter how ill-behaved and difficult to work with, I can give you an economy in which people are as close as you like to being identical, i.e., they have the same preferences and almost the same income, which will generate this ugly aggregate excess demand function.[2]
>
> (Kirman, 1992, p. 128)

The Sonnenschein–Mantel–Debreu result (as it has come to be known) is an incredibly fundamental challenge to the notion that general equilibrium theory provides a basis for the microfoundations of macroeconomics. Rather than giving us the ability to rigorously generate macroeconomic theories from first principles, general equilibrium theory tells us that all things are possible at the macroeconomic level. It provides no rigorous guidance to macroeconomics at all.

Kirman's (1992) *Journal of Economic Perspectives* article was largely centered on showing how these results invalidated the use of a representative agent model. There is simply no theoretical justification for assuming that the excess demand function of a representative agent bears any resemblance to the excess demand function for an aggregate economy. If we want to justify the notion that macroeconomics needs microfoundations by pointing to general equilibrium theory, then these results derived by general equilibrium theorists unambiguously demonstrate that the representative agent is flawed. Oddly, we seem to be simultaneously seeing a situation in which macroeconomists point to general equilibrium theory as a justification for representative agent models at the same time that general equilibrium theorists are prominently noting that the representative agent has no home in the theory.

However, the Sonnenschein–Mantel–Debreu results yield an even more striking result. The goal of proving microfoundations for macroeconomics using general equilibrium theory is dead. Starting with just the idea that individuals maximize their utility, that "engine for the discovery of truth" that Lucas (1987) mentions, we can derive nothing about the macroeconomy. In order to get macroeconomic results, we must impose some sort of aggregate restrictions. As Kirman (1989) notes, "[D]emand and expenditure functions if they are to be set against reality must be defined at some reasonably high level of aggregation. The idea that we should start at the level of the isolated individual is one which we may have to abandon." Rizvi (1994) goes even further in declaring the end of the microfoundations project:

> [T]he fact that [aggregate excess demand] is essentially arbitrary, and that no reasonable assumptions at the level of individuals remove this indeterminacy, means that the era of strict microfoundations has come to an end. This arbitrariness specifically means that attempts to give strict microfoundations to macroeconomics, especially ones that strive for the same level of generality and completeness found in the treatment of competitive Walrasian general equilibrium, are not generally valid.
>
> (Rizvi, 1994, p. 373)

As Rizvi documents, general equilibrium theory has never shown, and can never show, that macroeconomic regularities can be generated solely by starting at the level of individuals.

The first results in this vein are over twenty years old now, but very little attention has been paid to them in the macroeconomic literature. Oddly, Kirman (1992, pp. 122–3) attributes the fashionability of representative agent models among macroeconomists to a realization of the implications of the Sonnenschein–Mantel–Debreu results, arguing that these results "have driven those wishing to reconcile rigor, individual maximization, uniqueness and stability into the straight-jacket of the representative agent model." Kirman provides no citations for this belief; in fact, it is hard to figure out to what body of work he is referring. There is very little evidence that macroeconomists are even aware of the Sonnenschein–Mantel–Debreu results. Rizvi (1994) finds just one reference in the work of a macroeconomist, that of Fitoussi (1983, p. 2); moreover, he notes that both Harcourt (1977) and Weintraub (1979), both of whom are devoted to a study of the microfoundations of macroeconomics, have no mention of the Sonnenschein–Mantel–Debreu results. More recently, Stiglitz (1992, pp. 44–5) makes brief mention of the results, and Grodal (1991) mentions them in a comment on Summers (1991). There are undoubtedly other brief mentions of these results in assorted stray paragraphs cast hither and thither in the macroeconomics literature. But it is a far different thing to find references to these results in rather obscure corners than to argue that these results have had a major impact on the development of macroeconomic thought. More to the point, to the best of my knowledge no macroeconomist arguing for the necessity of providing rigorous microfoundations has either used the Sonnenschein–Mantel–Debreu results as a justification for representative agent work or defended the practice against these results. (Furthermore, neither Rizvi, 1994, nor Kirman, 1989, 1992, cite such a defense or justification.)[3]

The silence in the macroeconomics literature on this point is deafening. Not even Kirman's (1992) prominent *Journal of Economic Perspectives* paper outlining these results seems to have sent the barest ripple through the practice of macroeconomists. For example, the recent book *Frontiers of Business Cycle Research* (Cooley, 1995), full of microfoundational, general equilibrium, representative agent models, has nary a citation of *any* of these papers. Why this silence? Is it possible that macroeconomists are sure that the results are incorrect but just have not bothered to refute them? That hardly seems likely given the tremendous influence the results have had among theoretical microeconomists. It seems far more likely that macroeconomists have been so busy constructing ever more elaborate microfoundational models that they haven't noticed that this entire practice has been seriously undermined for nearly twenty years.

There is one other extension of the Sonnenschein–Mantel–Debreu literature that is worth mentioning here. After the implications of these results became clear, microeconomists began casting about for new methods of deriving restrictions on aggregate behavior. Hildenbrand (1994) and Grandmont (1992) have both shown that by increasing the amount of diversity at the microeconomic level, downward-sloping aggregate demand curves can be generated. However, in these cases, the downward-sloping aggregate curve is not the same as the curves governing individual behavior. In fact, Grandmont finds the aggregate regularity in a model in which individual agents are not even assumed to be rational; the goal of Grandmont's (1992, p. 33) paper is "to actually *reverse* the traditional Neoclassical research programme, and to try and obtain some form of *aggregate rationality*. . . by relying more on particular features of the distribution of behavioral characteristics among the members of the system under consideration." (Compare van den Haag's, 1975, comment about bees cited on p. 170.) We thus have a true irony here; the quest for better microfoundations has led microeconomic theorists to a realization that aggregate regularities may exist which do not exist at the microeconomic level.

CONCLUDING NOTE

Microfoundations in the sense of starting with a rigorous optimization problem and deriving aggregate behavior is thus a myth. Quite simply, there may be no correspondence between any aggregate regularities and any micro regularities. It is certainly true that the aggregate regularities may look nothing like the micro regularities. If we knew everything, then it might well be possible to demonstrate precisely how individual behavior aggregates up to macroeconomic behavior, but in the real world with our limited information and understanding, we are simply incapable of this task.

One could respond to this argument by asserting that since we do not understand aggregate behavior, there is little point in studying it. One could simply deny that there are any macroeconomic regularities, that there are really just statistical anomalies. Given the wealth of data that is collected, it is inevitable that some things will historically have been correlated with other things. To erect a theory that tries to explain such correlations is futile and foolish.

Another response is to argue that aggregate regularities do exist and arise from individual behavior. However, we do not fully understand how such individual behavior aggregates to the macroeconomic level. Thus, the proper response is to study both the individual behavior and the aggregate behavior. Discoveries in one field of study may well illuminate conundrums in the other theory. The end goal, the goal we hope to hit at the end of time, may well be the complete reconciliation of micro and macro economic theory, but until such a state of bliss is reached, we may have to struggle along with two separate, but not mutually exclusive, fields of study.

However, it is clearly illegitimate to assert that macroeconomics can be and must be derived from microeconomic theory. There is no logical reason for this insistence. There is no theoretical reason for insisting that all of macroeconomics is reducible to microeconomics, that microeconomic theory yields explanations for macroeconomic regularities, or that the direction of causality is solely from microeconomics to macroeconomics. There is no empirical justification for this insistence. In fact, there is every reason to believe that macroeconomic regularities are not derivable from standard microeconomic theory. There is in sum no reality in the myth of microfoundations.

Part V

WHITHER MACROECONOMICS?

14

AFTER REPRESENTATIVE AGENT MODELS

THE END OF THE REPRESENTATIVE AGENT

The last two decades have certainly been an exciting time to be a macroeconomist. It is hard not to be swept up in the excitement of the new classical methodological revolution. Reading Lucas and Sargent's (1979) "After Keynesian Macroeconomics" stirred the soul, captured the imagination, and made one eager to start hammering away at these models. The world seemed about to yield up its secrets. The models were becoming ever more rigorous, the mathematics ever more challenging and intriguing. The world suddenly looked different; few things in macroeconomics are as provocative as the new classical discussion of unemployment as an equilibrium phenomenon, neither inherently good nor bad (see, for example, Lucas, 1978, and his remarks in Klamer, 1983).

Moreover, we have seen a spate of intellectually stimulating models; the Lucas monetary misperceptions model, the Sargent and Wallace policy ineffectiveness and unpleasant monetarist arithmetic models; the Kydland and Prescott real business cycle models; the new Keynesian imposition of frictions into the models. Whether one finds these models persuasive or not, it would be hard to deny that they have made us think hard and seriously.

But then the program started to crash. Along the way too many compromises had to be made. Deriving a complete general equilibrium model that explained macroeconomic phenomena is a fascinating idea, but it is intractable. So, to gain tractability, to gain the ability to "write down and *do* something" with the models, compromises had to be made.[1] The representative agent model was enshrined; heterogeneity was necessarily abandoned. People hoped for the best.

Instead, the representative agent model failed as an analytical device for the study of macroeconomics. It was argued that the representative agent model would help us solve the Lucas critique. It was apparent that the oldstyle Keynesian macroeconomic models failed to incorporate the effects of changing expectations about policy; in a dynamic world, this is a serious failure. Representative agent models were supposed to fix that problem. By getting down to the level of an individual, by rigorously modeling how the average person would respond to policy changes, we Could get a real picture of how people's decision rules changed in the face of a new environment. We could study policy not in some static sense, pretending the world never changes, but really get to the heart of the effect of policy changes, calculating not just the change in government action but, more importantly, the change in how people respond.

As time went by, however, the representative agent proved to be too fixed in its ways, too stubborn to change. In order to make this program work, we have to get down to the deep parameters of taste and technology, but we just cannot get that far. We simply do not know enough about utility to write down utility functions that will not change with policy; we simply' do not have a good enough handle on the workings of a firm or the nature of technology to write down production functions that are invariant to regime changes. We may wish that it were otherwise; we may wish that we could base our models on deep parameters, but "if wishes Were horses, then beggars would ride" and all that.

The representative agent model was also supposed to help us get better Walrasian models. Who could object to a desire to get a complete *working* model of the economy? The Arrow–Debreu model is widely admired; why should not macroeconomists make use of it too? But solving out a model with 260 million distinct utility functions is a bit beyond our computational capacity, so to make this thing work, we need to simplify. It seemed easy enough to shrink the problem down to a single agent. It seemed unremarkable to exploit the fundamental theorems and just look for the Pareto optimum. The representative agent model seemed perfect for the task.

As time went by, however, the representative agent had a harder and harder time living in its new Walrasian home. The Walrasian model, complex though it seemed, turned out to be far too simple. Economists began studying the effects of heterogeneity and gradually realized that the world acts differently when there are differences among people. Economists looked seriously at the world and concluded that it was unrealistic to assume that it actually was always at a Pareto optimum. *Candide* joined *Robinson Crusoe* on the economist's bookshelf, as Dr Pangloss found his way on to our island; this is *not* the best of all possible worlds, so why pretend otherwise?

Ultimately the representative agent was supposed to help us achieve microfoundations. Why do we need to separate macroeconomics from microeconomics, anyway? Why not simply build up macroeconomics from microeconomic theory? To start with curves, to start with aggregates seems odd. These aggregates do not exist; it is the people who act. If we know how people act, then we can figure out how the economy behaves. It suddenly seemed obvious that the way to figure out how the macroeconomy works is simply to study intensively how the people who make up the economy behave. And who better to study than the "representative" agent?

However, as time went by, the representative agent turned out to be a macroeconomic model in disguise. It did not solve the aggregation problem; it merely hid it. It did not solve the problem of keeping exogenous and endogenous variables straight; it obfuscated it. And ultimately, the final indignity came when the representative agent turned out not to be a microeconomic model at all. If you look closely enough at microeconomics and the representative agent side by side, it becomes glaringly obvious that they do not look all that much alike after all. The disguise turned out to be simple wishful thinking.

Moreover, as the representative agent model was slowly self-destructing, it became more apparent that economists were chasing a dream. The Lucas critique was obviously true but unfortunately unsolvable. We have no idea how to write down models that are invariant to all policy changes. The critique became a bludgeon in theoretical work, but the bludgeon had no weight in empirical work. It became increasingly obvious that thinking about policy had to be done in a less abstract manner, that policies had to be considered individually, that it was far too simplistic to write down mechanical rules dictating what would and would not change with some undefined policy.

Simultaneously, the Walrasian goal of a rigorous, usable general equilibrium model crashed. These models tried to do too much and ended up doing too little. The Sonnenschein–Mantel–Debreu results painfully showed that we could derive no aggregate implications starting with the general equilibrium model. It is not just that it is hard to get aggregate results from a general equilibrium model: it is impossible to do so. The only way to get definite results in the aggregate is to impose the representative agent model, but the representative agent model is only meaningful in a world with zero heterogeneity. Perhaps someday we will find a planet populated by clones where these models will be useful, but until then we have little reason to expect them to be of much use in studying any economies about which we know.

And finally, the whole microfoundations project was revealed to be unworkable. The idea that we can start with nothing other than individuals maximizing their own utility and build up a model that explains the macroeconomy is nothing but a myth. At present, we simply do not know enough about either the microeconomy or aggregation to even get close to the goal. Given heterogeneities and the fact that people interact, the microfoundations project is unworkable. While we can certainly use microeconomic study to inform our aggregate models, it simply is not possible to rigorously derive good models of the macroeconomy from microeconomic principles.

So, for one last time, we can modify the remarks of Lucas and Sargent:

> The task now facing contemporary students of the business cycle is to sort through the wreckage, determining which features of that remarkable intellectual event called the

> [new classical] revolution can be salvaged and put to good use and which others must be discarded.
>
> (Lucas and Sargent, 1979, p. 1)

We turn in the next section to a brief discussion of the work emerging in the wake of the death of representative agent models.

WHITHER MACROECONOMICS?

At this early date, it is impossible to tell exactly what macroeconomic theory will look like in a decade, but the general tenor of the emerging theory is apparent. There is a growing body of macroeconomic literature which abandons new classical representative agent models in whole or in part. While there is little point in elevating any particular models to canonical status at this point since, after all, none of today's models will be state-of-the-art in a decade, we can point to bodies of research that seem particularly promising as macroeconomists look to the future.

The new macroeconomics will largely abandon new classical style microfoundations but is likely to resurrect the older meaning of microfoundations. With the passing of the representative agent model, the problem of aggregating from microeconomic agents to macroeconomic agents will once again become severe. Since we do not know how to rigorously model the transition from heterogeneous agents to an aggregate model, macroeconomists will have to adopt the older approach of thinking seriously about microeconomic agents as a means of formulating interesting macroeconomic hypotheses. It seems unlikely that macroeconomists will completely abandon the rhetoric of microfoundations, but microfoundations will once again mean nothing more than that there is a story about individuals underlying the model.

The new macroeconomics will increasingly abandon the attempt to generate comprehensive Walrasian general equilibrium models. Work on general equilibrium models with heterogeneous agents will undoubtedly continue; the mathematical niceties and intellectual satisfaction from working out such complicated models are too great to ignore. However, the bulk of the macroeconomics profession seems to be turning increasingly to limited models intended to explain small parts of the economy. We will see models trying to explain how credit markets work, why prices and wages might be sticky, how coordination failures can lead to nonoptimal equilibriums, and so on. The only connecting thread among all these models will be that they try to explain just a part of the macroeconomy.

This sort of research strategy is firmly in what we called the Marshallian tradition. By working out the implications of, say, credit rationing, we think seriously about how credit markets work, what can go wrong, and how we can fix it. More importantly, we generate testable predictions about the market, giving us reasons to accept or reject particular stories. Knowing more about how credit markets work will lend itself to better understanding of the effects of monetary policy, which may feed into interest rate studies and so on. While individual models study only a part of the economy, the profession as a whole will be engaged in a study of the economy as a whole.

However, one aspect of the Marshallian tradition is unlikely to be resurrected. Marshall's vision of the limited role for mathematics will not be realized. Mathematics has proven to be far too powerful a tool in modern macroeconomics to be abandoned or even pushed into the background. There is little doubt that the increasing mathematization of the profession has produced much good; it has crystallized thought in a manner impossible when bogged down in elaborate verbal reasoning. The new macroeconomic models will be less ambitious models than the new classical representative agent models, but there is no reason to assume they will be less mathematical. This is not to say that Walras' vision of economics as nothing but the rigorous derivation of mathematical models will constitute macroeconomic study. As the appreciation of the complexities of the real economy becomes once again widespread, so will an appreciation of the limits of our current mathematical aptitude relative to the problem at hand. Marshall was right to express skepticism about the ability to mathematize all economic problems of interest even if he was wrong to want to relegate all of the mathematics to an appendix.[2]

The effects of policy will be taken more seriously in the new macroeconomics. Instead of policies which do nothing other than change agents' expectations of future aggregate money supplies or abstract fiscal policy, we are likely to see more focus on specific, tangible policies. If one is trying simply to understand the credit market without ambitious goals of incorporating the result into a general equilibrium model, then mathematical tractability no longer is such a dominant concern. It becomes easier to think about specific policies or regulations and to imagine what they will do in the working of a particular market. The Lucas critique as it is currently understood will become a thing of the past as we begin to study how people, not an abstract agent, respond in the face of changes in policy regimes.

So what is the body of work that is emerging along these lines? Most notable is the resurgence of bounded rationality in macroeconomics, work on which is becoming more interesting all the time. Sargent (1993a) and Conlisk (1996) provide good introductions to these sorts of models.

Conlisk (1996, pp. 685–6) notes that the rational expectations hypothesis almost forces macroeconomists into using representative agent models. Rational expectations present tremendous problems of mathematical tractability in models with heterogeneous agents. However, as bounded rationality models become more prominent, the need to use representative agent models diminishes; at the same time, as the problems of representative agent models become more apparent, the ability to incorporate rational expectations into a meaningful model diminishes. The stage thus seems set for an increasing reliance on bounded rationality models.

Moreover, an increasing use of bounded rationality models means that the effects of policy regime changes will be studied more appropriately. Rational expectations forced economists to assume superhuman ability to recognize and adapt to changes in the policy regime. With bounded rationality models, researchers are forced to think through the effects of regime changes much more clearly; no longer do we simply assume that all agents instantly know that the policy regime has changed but expect that the new regime will last forever. The Lucas critique will necessarily be abandoned in favor of serious thinking about policy.

The use of bounded rationality models thus forces economists to think more seriously about the role of information in the economy. Informational considerations are also increasingly important in the emerging macroeconomic theory. Brunner. and Meltzer (for example, 1971, 1993) have been arguing for the importance of informational considerations in modeling the macroeconomy for years. In fact, one of the aspects of monetarism that seems to have been left by the wayside in the last two decades is a real appreciation of the complexity of the economy, which the monetarist "black box" was meant to capture. However, in the emerging theory the economics of information is increasingly coming to the forefront, led perhaps by the work of Stiglitz (1979; Stiglitz and Weiss, 1981; Greenwald and Stiglitz, 1986).

As bounded rationality and the role of information play increasingly important roles in the new macroeconomics, there is a sharpening focus on the role of relationships between people. When macroeconomics was dominated by homogeneous agents all of whom had all the information in the world, there was no place for interdependencies among agents. On the other hand, the recognition of informational constraints and limits to rationality combine nicely with the literature on how agents interact. Game theory, which to be interesting requires thinking about more than a single representative agent, is playing an increasingly important role in the new macroeconomics (see, for example, Shubik, 1990). Similarly, much of the coordination failure literature focuses on how agents work together or fail to do so (see, for example, Cooper and John, 1988).

The emerging macroeconomic theory carries with it a strong sense of *déjà vu.* As we noted earlier, the methodology, but not necessarily the content, of the old monetarist literature had many of these same characteristics. Throughout this book we have noted how Friedman's work is an example of work that contains many of the features of the new macroeconomics that is emerging. However, these similarities are not limited to just Friedman. Brunner noted that in the monetarist literature, something very much like bounded rationality was used, though it wasn't called by that name:

> The second thesis [the monetarist story of expectation formation] recognizes rational expectations in Muth's sense as a longer run phenomenon. It also recognizes that expectations are rationally formed on the basis of available information in the context of some beliefs about the nature of the process generating the expected magnitudes.

> The conditioning beliefs barely coincide however with the structure of the hypothesis incorporating the expectations. . . . The second thesis emphasizes thus a learning process with systematic revision of information and beliefs.
>
> (Brunner, 1978, p. 68)

Similarly, Brunner explicitly rejects new classical style microfoundations:

> The Cartesian tradition insisted that all statements be derived from a small set of "first principles." "Cogito ergo sum" and everything else follows. This idea has had a strong influence on philosophy but also on the program of the new classical economics best represented by Nell Wallace. Anything not derived from "first principles" does not count as knowledge. . . . This methodological position is quite untenable and conflicts with the reality of our cognitive progress over history. . . . Adherence to the Cartesian principle would condemn science to stagnation.
>
> (Quoted in Klamer, 1983, p. 195)

However, the similarities to older literature do not end with the monetarist literature. Even more strikingly, Simon, the father of bounded rationality, wrote almost four decades ago:

> But an understanding of Robinson Crusoe, however important as a first step, is only a preliminary to an understanding of modern, urbanized man. The characteristic environment of man is constituted not of nature but of his fellows. His rational decision making – at least during most of his waking hours – takes place in social groups including organizations.
>
> (Simon, 1957, p. 196)

The time has come for economists to take that second step. Simon's work is a remarkable forerunner to the incorporation of limited information, bounded rationality, and interdependencies into economic models.

The modern incarnation of the literature being described is currently disjointed and spread about all over the place. It seems likely that it ever will be so. It is hard to conceive, for example, of a general game theory model that can explain the whole of the macroeconomy. It is more likely that these models will continue to fall into the Marshallian tradition of being small explanations of parts of the economy.

At present, this literature is scattered amongst what is generally called new Keynesian economics. The new Keynesian literature is spawning incredible numbers of different explanations of the same phenomenon. The efficiency wage literature, for example has models explaining the wage rate as a means of reducing shirking, a means of reducing labor turnover, a method of dealing with adverse selection and the result of sociological factors

(Yellen, 1984). Individually, this body of literature has very little coherence; collectively, it is painting a general picture of the macroeconomy. Attempts to incorporate all the aspects of the new Keynesian literature into one all-encompassing model seem doomed to failure; the literature is simply far too diverse to capture all the intricacies of the stories.

Similarly, there is an emerging body of literature which has been dubbed post-Walrasian and is nicely summarized in Colander (1996).[3] This literature explicitly examines how macroeconomic factors influence microeconomic decision makers and how microeconomic agents interact to produce aggregate outcomes. Much of this literature overlaps with the new Keynesian literature and other parts of it are simply vague admonitions. However, some of the work is relatively distinct and rather intriguing.

Thus, we need not mourn the passing of the new classical representative agent nor wonder if it is possible to do work in some other way. The return to macroeconomics *qua* macroeconomics is already taking place. The work left to be done is hard but promises to be rewarding.

NOTES

1 INTRODUCTION

1 As most readers will recognize, the structure and phrasing of that sentence are a direct lift from Lucas (1987, p. 35).

2 THE ORIGINS OF THE REPRESENTATIVE AGENT

1 This is a bit of an overstatement in two ways. First, while Robbins' article is the most comprehensive assault on the representative firm, it was not the only one. Second, the banishment of the representative firm from economics was not total; it still lurked here and there. However, Wolfe's essential point is correct; the opponents of the representative firm removed its intellectual respectability.

2 The phrase "representative firm" was first used in the second edition of *Principles,* published in 1891, though there were intimations of the concept in the first edition (1890). See, for example, Guillebaud's discussion in Marshall (1920 [1961], vol. 2, pp. 18, 346–7) which provides the relevant quotations from the first edition. All page references here are from the variorum edition, published in 1961, using the text of the eighth edition, published in 1920.

3 Mayer (1993b) examines a similar idea. That paper shows that indexed bonds do not provide an estimate of inflationary expectations in a world of heterogeneous agents.

4 It is not necessary to assume that all people hold the same amount of bonds. All we need in this case is that at least 10 percent of the bonds are held by risk neutral individuals. Furthermore, this assumption is only necessary for the extreme case of no measured risk premiums. As is discussed below, the general problem does not require this assumption.

5 The risk premium will not become negative for the same reason. If it were negative, then nobody would want to buy corporate bonds, driving their price down and their return up.

3 ARGUMENT FOR THE NEW CLASSICAL USE OF REPRE SENTATIVE AGENT MODELS

1 Little discussion is not the same as no discussion. See, for example, the discussion of new classical representative agents compared to Austrian ideal types (Hoover, 1988, pp. 242–4); this section is discussed more fully in Chapter 8 of the present work.

2 The Appendix has two tables reporting regressions with an included interest rate term. In one, only the current interest rate is included and its sign is negative. (Lucas and Rapping's

theoretical section predicts a positive coefficient on interest rates.) In the other, both the current and one-year lagged interest rates are included; the coefficient on the contemporaneous interest rate has a positive sign, but the coefficient on the lagged interest rate is negative and larger in absolute value. Thus, the overall effect of introducing interest rates is negative.

4 BEYOND TASTE AND TECHNOLOGY PARAMETERS IN MACROECONOMICS

1 This is not precisely correct. Lucas argues that the problem with equation (4.1) is that the set (F,*?*) is held constant. However, in Lucas' own specification of a proper model, F is still held constant across regimes. I have followed Lucas in his deeds rather than his words. Nothing in the analysis is affected if F is also assumed variable.
2 That such empirical testing should be the rule is the argument presented at the end of this chapter.
3 It is not clear, however, that everyone recognizes the small influence of the Lucas critique on empirical work. When asked, "How important has the 'Lucas critique' actually been in practice?" Lucas tersely replied, "It's had an enormous influence, I think, and all for the good" (quoted in Snowden *et al.,* 1994, p. 225).

5 WALRASIAN METHODOLOGY

1 We should note that what we are here calling Walrasian methodology did not spring *sui generis* into the mind of Walras. This methodology has a long intellectual tradition. For example, Mill's methodological views are very similar to Walras'. Readers interested in knowing more about Mill's methodological views should see the excellent synopsis in Hirsch and de Marchi (1990, ch. 5). This chapter elaborates on Mill's statements in *A Statement of Logic Ratiocinative and Inductive* (1881) and "On the Definition of Political Economy" (in Mill, 1967).

6 MARSHALLIAN METHODOLOGY

1 Readers interested in a fuller discussion of Marshall's views on induction and deduction should see the extensive discussion in Reisman (1987, pp. 321–38).
2 Recently in Hirsch and de Marchi (1990) and Mayer (1993a). Readers are referred to these works both for their surveys of the relevant literature and for their analyses of Friedman's writings.

7 THE NEW CLASSICALS AS WALRASIAN ECONOMISTS

1 Sargent's remarks also serve as further evidence of the new classicals' use of a Walrasian methodology in demonstrating where sympathies lie when fact and theory are in contradiction.
2 Pesaran (1987) provides a detailed argument for this conclusion.
3 This comparison of Friedman's complaints against Lunge and new classical work serves an additional purpose. There are few comparisons that are as uninformed as those which argue

that new classical work is following in Friedman's footsteps. As we can see here, Friedman provided an extensive critique of what is in essence new classical methodology twenty-five years before the new classicals came along. Therefore, this demonstration may help put yet another nail in the coffin of spurious comparisons between new classical macroeconomics and monetarism. See also Hoover (1988, ch. 9).

4 In his analysis, Friedman actually breaks this section into two parts, both of which are examples of this general phenomenon. The two parts are "Casual Empiricism" and "Invalid Use of Inverse Probability."

8 MICROFOUNDATIONS: AUSTRIAN STYLE

1 The fact that humans act does not mean that passivity is never a viable choice. To remain passive is every bit as much an action as to engage in physical exertion. urthermore,to defer a choice, or to decide not to decide, is also an action. The statement that humans act is not a statement about the types of choices they make; it is a statement that these choices can and must be made by an individual, himself.

Moreover, nothing in this statement denies that people's choices are limited, that they are not omnipotent. The fact that a person who decided to evaporate the oceans with a magnifying glass would be unable to do so is not relevant. There are technological limitations on people's ability to convert their wishes into desires. These limitations in no way negate the proposition that humans act.

2 Is this right? Is the proposition that humans act truly self-evident to all? The appeal to a set of basic beliefs is not without rationale. All people must hold some set of basic beliefs which are immune from scientific inquiry. Even nihilists believe in something, namely that they believe in nothing. What the Austrians are asserting here is that the proposition that humans act is in everyone's set of basic beliefs, that no one can honestly deny belief in his own purposeful action.

3 There may be some detractors who will claim that it is possible to delve even deeper in the search for microfoundations than even the Austrians have done. One could argue that a person's mind is nothing but a series of physicochemical processes. If so, we can never understand human action until we delve even deeper and understand the physicochemical processes which constitute humans.

Mises (1966, pp. 17–18) dealt with this line of reasoning by pleading scientific ignorance. At present, we do not know how or if these physicochemical processes constitute the minds of people. There is no scientific foundation for the statement that people's actions can be reduced further. In the face of this scientific ignorance, we can do no other than start with a person as heis.

We could get around these detractors in a simpler fashion than Mises if we were willing to posit the existence of soul. If a person has a soul which is not reducible to physicochemical processes, if people are something more than a mere lump of chemical compounds, then the study of humans is truly as far as we can delve. A person is an ultimate given.

4 There are undoubtedly skeptics who doubt the ability to derive anything meaningful from a single basic proposition coupled with introspection. I would refer such skeptics to either of the texts mentioned in the introduction, both of which deal with a panoply of issues.

5 A notable exception is Hansen and Sargent (1991), which provides methods to disaggregate the economy and get time series paths for each agent in the economy. However, even this is a limited attempt at individual-level prediction; the solution path for the aggregate economy is unaffected by individual-level allocations.

9 THE TRADITIONAL CASE FOR MICROFOUNDATIONS

1 See, for example, Hahn and Solow (1995, p. 1).

2 See, for example, Lucas and Sargent (1979, pp. 4–5n).

10 THE AGGREGATION PROBLEM

1 We should note that the basis for much of the aggregation literature is Leontief (1947a, 1947b).

2 Some readers may desire to know the exact measure of aggregation bias offered by Theil. It is easiest to see in a concrete example with two parameters. Consider a relationship among the variables *x,y* and *z*. Lower case will denote the micro parameters; upper case, the aggregate. For simplicity, omit weighting schemes and let

$$X_t = \sum_i x_{it};\ Y_t = \sum_i y_{it};\ Z_t = \sum_i z_{it}.$$

We know that the micro equation is

$$x_{it} = a_i + b_i y_{it} + c_i z_{it}.$$

We want to get the aggregate equation

$$X_t = A + BY_t + CZ_t.$$

Furthermore, there is a relationship between the micro parameters and the aggregate parameters as follows:

$$\begin{aligned} y_{it} &= a_{1i} + \beta_{1i} Y_t + G1_i Z_t + \mu 1_{it} \\ Z_{it} &= a_{2i} + \beta_{2i} Y_t + G2_i Z_t + \mu 2_{it}. \end{aligned} \tag{1}$$

The μs are error terms and can be set to zero without affecting the results below, but they make the equations more plausible. The equations in (1) are general; for example, if *y* were income, and $a1i = G1i = 0$, $\mu 1i = 0$, then $\beta 1i$ is just agent *i*'s share of aggregate income. Note, however, that we need not attach any economic meaning to the equations in (1).

Given the above specification, we can show that the parameters in the aggregate equation are as follows:

$$\begin{aligned} B &= \left(\frac{1}{n}\right) \sum_i b_i + n[\mathrm{cov}(b_i, \beta_{1i}) + \mathrm{cov}(c_i, \beta_{2i})] \\ C &= \left(\frac{1}{n}\right) \sum_i c_i + n[\mathrm{cov}(c_i, \Gamma_{2i}) + \mathrm{cov}(b_i, \Gamma_{1i})] \end{aligned} \tag{2}$$

The terms in the brackets in equation (2) are Theil's measure of aggregation bias. It is simple to generalize from the two-variable case by adding covariance terms or by using matrix notation.

The bias in the example given in the text is easily seen. Let *x* be consumption and *y* be income. If an agent's mpc varies with his share of aggregate income, the term cov(bi, $\beta 1i$) is non-zero.

3 Also see the papers in Barker and Pesaran (1990).

11 INDIVIDUAL AND MARKET EXPERIMENTS

1 This is not precisely what the authors advocate. Actually, Hansen and Sargent use a different equation employing c_t rather than a_t as their regression equation. The difference is irrelevant for the argument here, so (11.4) is used for convenience.

12 THE REPRESENTATIVE AGENT VERSUS MICROFOUNDATIONS

1 For an earlier treatment of the same issues, see Janssen (1990).
2 Janssen (1993) goes on to show that assorted game theoretic attempts to provide individualistic foundations to competitive equilibriums are also not completely successful.
3 The result can be illustrated with a simple example using discrete prices. Let the optimal price be 0. With inflation the real price received by a firm falls by 1. When the firm's real price gets lower than -2, the firm changes its price to 0. So (*s,* S) = (-2, 0). There are three possible prices a firm could charge: (0, -1, -2). Suppose that a firm starts out at price 0. With inflation, the firm's real price falls to -1, but the firm does not adjust its price. This is the price stickiness result explained by Mankiw. However, instead of having just one firm, assume that there are lots of firms and that initially one-third of all firms are at each of the three possible prices. Now, when there is inflation, the firms starting at price 0 see their price fall to -1; the firms starting at price -1 see their price fall to -2; the firms starting at price -2, see their price fall to -3, and so they immediately raise their price to 0. In the end, we still have one-third of all firms at each of the possible prices; the monetary expansion had no effect on the distribution of real prices.
4 We can see this in another simple example. Again, let the optimal price be 0, but now have firms adjust their prices whenever the real price falls below -1 or rises above 1. There are three possible real prices that a firm can charge: (1, 0, -1). Again let one-third of all firms start out at each price. Now, when the money supply increases, the firms that started at 1 end up at 0; the firms that started at 0 end up at -1; the firms that started at -1 fall to -2 and so immediately raise their price to 0. In the end, we find no firms at a price of 1; two-thirds of the firms at a price of 0; and one-third of the firms at a price of -1. Since the distribution of real prices has changed, money is nonneutral.

13 THE MYTH OF MICROFOUNDATIONS

1 Coleman (1990, p. 200) provides a more extensive discussion of this passage from Le Bon.
2 Note the "almost the same income" in Kirman's summary. The qualification is important. If all agents have identical preferences and identical income, then the aggregate economy does look like the individual. In fact, this is simply the representative agent model. We can once again see how much of a special case the representative agent model is. Surely, many macroeconomists would justify the use of a representative agent model on the grounds that, while it is not precisely true that all agents have the same preferences and income, it is a close enough approximation. However, in this case, being close does not count. In order to argue that the general equilibrium framework provides us with microfoundations for macroeconomics, we must believe that it is precisely true that all agents have the same income and preferences. As Kirman and Koch (1986) show, if we know that they are simply close, then the aggregate excess demand function is arbitrary. There is one further caveat to this conclusion: as we noted in Chapter 10 on aggregation, if individual preferences are both identical and homothetic, then the representative agent is justified regardless of the income distribution. However, this is no more plausible than assuming that agents have identical incomes.
3 Lucas and Sargent (1979) do mention Sonnenschein (1973). They note that when Keynes was writing, the term "equilibrium" carried with it the connotation of "ideal." Lucas and Sargent argue that in the wake of Arrow and Debreu, the term "equilibrium" has lost its normative connotations:

> This development, which stemmed mainly from work by Arrow (1964) and Debreu (1959), implies that simply to look at any economic time series and conclude that it is

a disequilibrium phenomenon is a meaningless observation. Indeed, a more likely conjecture, on the basis of recent work by Sonnenschein (1973) is that the general hypothesis that a collection of time series describes an economy in competitive equilibrium is *without content.*

(Lucas and Sargent, 1979, p. 7)

The reference to Sonnenschein here is thus not an acknowledgment of what later became known as the Sonnenschein–Mantel–Debreu results, but rather an argument that there is no longer any real normative content in saying that one is developing "equilibrium" as opposed to "disequilibrium" models.

14 AFTER REPRESENTATIVE AGENT MODELS

1 The quoted text is from Lucas (1987, p. 2).

2 It is not entirely certain that Marshall would still dispute the good of increasing mathematization in economics. Schumpeter gives a nice perspective on the role of mathematics in Marshall's work:

> the point is – not merely that his [Marshall's] mathematical turn of mind was favorable to his achievement in the field of economic theory, but – that the actual use of the methods of mathematical analysis *produced* that achievement and that the transformation of the Smith–Ricardo–Mill material into a modern engine of research could hardly have been accomplished without it. . . . performance of the Marshallian kind *practically* presupposes a mathematical schema. And this Marshall always refused to admit. He never gave full credit to the faithful ally. He hid the tool that had done the work.
>
> Of course there were excellent reasons for this. He did not want to frighten the layman, he wanted – strange ambition! – to be "read by businessmen" . . . . As a matter of fact, Marshall himself cannot be fully understood by readers who have no grasp at all of the elements of the calculus. No good purpose is served by making them think that he can. Much good could have been accomplished if Marshall had resolutely stood for the line of advance which he had done more than anyone else to open.
>
> (Schumpeter, 1951, pp. 97–8)

3 Whatever happened to the good old days when schools of thought had nice simple names like Keynesian and Monetarist? These days we are faced with a host of news and neos and posts: new Keynesian, post-Keynesian, neoKeynesian, new classical, post-Walrasian. Colander (1993) discusses the relationship between new neo-Keynesians and new Keynesians who have become post-Walrasians. Can the day be far off when the post-new neoKeynesians are debating the post-new classicals and the new post-Walrasians?

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