

'Michel De Vroey does not simply record what he finds. He has a vision of the kind of macroeconomics he would like to see, perhaps one he has developed gradually over the years he has worked on this book. What makes this book enjoyable is that he has high hopes for economics, he flatters us that we are important, and he praises the progress we have achieved. In the end, he has the integrity not to hide his disappointments, his conviction that, while there is no turning back, there is still a long way to go.'

Robert E. Lucas, Jr., John Dewey Distinguished Service Professor in Economics, University of Chicago

'No branch of economics has witnessed as many revolutions and counter-revolutions as macroeconomics, starting from Keynes' General Theory 80 years ago. Michel De Vroey's book provides a thorough but highly readable account of the main developments in the field over that period. It conveys a sense of the excitement generated by the advent of every new paradigm, as well as of the growing tension between the requirements of theoretical purity and those of empirical and policy relevance that macroeconomists keep facing these days.'

Jordi Galí, CREI, Universitat Pompeu Fabra and Barcelona GSE

'Macroeconomics research has largely proceeded through "revolutions." One wishes for a more linear and evolutionary process, one where most new contributions would naturally fit, and the common core become steadily stronger. But the immense complexity of modern economies, the difficult methodological choices, may be such that "revolutions" will keep happening, with their share of destruction, confusion and eventual reconstruction. Understanding the nature of these revolutions is essential to understanding where we are today, and Michel De Vroey's book does a masterful job of doing just that. A thoroughly illuminating and enjoying read.'

Olivier J. Blanchard, Robert M. Solow Professor of Economics, Massachusetts Institute of Technology

'Macroeconomics is a complex evolving system of thinking. Michel De Vroey's latest book dives into that evolving complexity and, by distinguishing between Marshallian and Walrasian macroeconomics, helps make the history of macroeconomics a bit more understandable. It's sad that more economists don't make that distinction.'

David Colander, Distinguished College Professor, Middlebury College

Michel De Vroey is a professor emeritus at the Université catholique de Louvain and visiting professor at the Université Saint Louis in Brussels. He has published several books, including *Involuntary Unemployment: The Elusive Quest for a Theory* (2007) and *Keynes, Lucas: D'une macroéconomie à l'autre* (2009). He has also published extensively in scholarly journals.

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De Vroey

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Keynes to Lucas and Beyond

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Michel De Vroey

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This book retraces the history of macroeconomics from Keynes's *General Theory* to the present. Central to it is the contrast between a Keynesian era and a Lucasian – or dynamic stochastic general equilibrium (DSGE) – era, each ruled by distinct methodological standards. In the Keynesian era, the book studies the following theories: Keynesian macroeconomics, monetarism, disequilibrium macroeconomics (Patinkin, Leijonhufvud, and Clower) non-Walrasian equilibrium models, and first-generation new Keynesian models. Three stages are identified in the DSGE era: new classical macroeconomics (Lucas), RBC modelling, and second-generation new Keynesian modelling. The book also examines a few selected works aimed at presenting alternatives to the Lucasian macroeconomics. While not eschewing analytical content, Michel De Vroey focuses on substantive assessments, and the models studied are presented in a pedagogical and vivid yet critical way.

Michel De Vroey is a professor emeritus at the Université catholique de Louvain and a visiting professor at the Université Saint Louis in Brussels. He held visiting positions at the Sorbonne University, Duke University, the University of British Columbia, Vancouver, and Clemson University. He has published several books, including *Involuntary Unemployment: The Elusive Quest for a Theory* (2007) and *Keynes, Lucas: D'une macroéconomie à l'autre* (2009). He has also published extensively in scholarly journals.

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MICHEL DE VROEY

Université catholique de Louvain, Belgium



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*To Jean Cartelier, Marie-Paule Donsimoni, Franco Donzelli, and
Laurent d'Ursel, who helped me shape my vision of economic theory*

Contents

<i>List of Figures</i>	page ix
<i>List of Tables</i>	xi
<i>Boxes</i>	xii
<i>Preface</i>	xiii
<i>Acknowledgements</i>	xix

PART I: KEYNES AND KEYNESIAN MACROECONOMICS

1 Keynes's <i>General Theory</i> and the Emergence of Modern Macroeconomics	3
2 Keynesian Macroeconomics: The IS-LM Model	27
3 The Neoclassical Synthesis Program: Klein and Patinkin	50
4 Milton Friedman and the Monetarist Debate	65
5 Phelps and Friedman: The Natural Rate of Unemployment	95
6 Leijonhufvud and Clower	112
7 Non-Walrasian Equilibrium Modeling	123
8 Assessment	143

PART II: DSGE MACROECONOMICS

9 Lucas and the Emergence of DSGE Macroeconomics	151
10 A Methodological Breach	174
11 Assessing Lucas	191
12 Early Reactions to Lucas	204
13 Reacting to Lucas: First-Generation New Keynesians	225

14	Reacting to Lucas: Alternative Research Lines	247
15	Real Business Cycle Modeling: Kydland and Prescott's Contribution	261
16	Real Business Cycle Modeling: Critical Reactions and Further Developments	282
17	Real Business Cycle Modeling: My Assessment	299
18	Second-Generation New Keynesian Modeling	307
PART III: A BROADER PERSPECTIVE		
19	The History of Macroeconomics through the Lens of the Marshall-Walras Divide	339
20	Standing up to DSGE Macroeconomics	358
21	Looking Back, Looking Ahead	378
	<i>Bibliography</i>	389
	<i>Index</i>	418

Figures

1.1	Temporary and normal equilibrium: Marshall's fish market	<i>page</i> 11
1.2	The firm's output decision	17
1.3	The determination of effective demand	19
2.1	Equilibrium in the IS-LM model	28
2.2	The Keynesian LM curve according to Hicks	29
2.3	Contrasting two definitions of rigidity	31
2.4	The lack of equilibrium between saving and investment at full-employment income, according to Klein	35
2.5	The labor market outcome	36
2.6	The Phillips relationship	42
2.7	The discrepancy between the Phillips relation and the Phillips curve	43
2.8	Integrating involuntary unemployment and frictional unemployment	44
2.9	The Phillips curve	45
3.1	The commodity and labor markets in equilibrium	60
3.2	The commodity and the labor markets in disequilibrium	61
4.1	Velocity in the US Economy	91
5.1	Relations between vacancy and unemployment rates	99
5.2	Phelps's expectations-augmented Phillips curve	101
5.3	The accelerationist view of the Phillips curve	104
6.1	Involuntary unemployment as resulting from a signaling defect	121
7.1	The Keynesian regime	126
8.1	A decision-tree representation of the early years of macroeconomics	147
13.1	Involuntary unemployment in the shirking model	230
13.2	The profit function of a monopolistic firm	237
13.3	The labor market short-period normal equilibrium	241

13.4	Inconsistent claims	242
13.5	Labor market disequilibrium and inflationary dynamics	243
14.1	Different levels of activity in Diamond's search model	250
14.2	The trade structure in Roberts's model	257
15.1	TFP and real GDP	270
18.1	The Dixit-Stiglitz monopolistic competition model	312
19.1	Marshall's breakdown of the economy into separate elements	342
19.2	Marshall's fishing industry example	342
19.3	Walras's strategy for dealing with complexity	343
20.1	Effective demand <i>à la</i> Keynes and <i>à la</i> Farmer	366
20.2	The determination of output	367

Tables

0.1	The main episodes in the history of macroeconomics	<i>page xv</i>
4.1	The evolution from Friedman's expectations-augmented Phillips Curve model to Lucas's and Kydland and Prescott's models	93
6.1	The commonalities and differences between Patinkin, Clower, and Leijonhufvud	113
7.1	A typology of non-Walrasian equilibrium states	137
8.1	Alternative views of the Keynesian program	144
10.1	Comparing Keynesian and new classical macroeconomics	187
14.1	Comparing the Lucas and the Diamond approaches	252
15.1	Kydland and Prescott's main results	265
18.1	The differences between first- and second-generation new Keynesian modeling strategies	309
18.2	The new Keynesian-RBC synthesis	326
19.1	The Marshallian and the Walrasian approaches: contrasts and commonalities	340
19.2	A comparison of the Keynesian and the DSGE programs	348
19.3	Lucas's neutrality of money model as an amended Walrasian two-good exchange model	352
19.4	Classifying macroeconomic models against the Marshall-Walras divide	356
21.1	Claims made about labor market outcomes	381
21.2	Classifying models against the requirement for full general equilibrium analysis	384
21.3	A typology of macroeconomic models in the 1970s	386
21.4	The policy conclusions of the models	386

Boxes

0.1	A snapshot account of the history of macroeconomics	<i>page xvii</i>
1.1	Marshall's equilibrium triad versus the post-Marshallian equilibrium dyad	13
9.1	The notion of intertemporal elasticity of substitution	156
12.1	The VAR approach	205
15.1	The Solow residual	269
18.1	The Dixit-Stiglitz monopolistic competition model	310

Preface

To know who we are, we need to know where we come from.

The aim of this book is to trace the evolution of modern macroeconomics from its inception to the present. A sufficient justification for this enterprise is that modern macroeconomics, which originated with the publication of John Maynard Keynes's *The General Theory of Employment, Interest, and Money* (Keynes 1936), has now existed long enough to make it worth assessing what has happened over the past seventy years. An additional justification is that during this period macroeconomics has undergone a radical change with the dethroning of Keynesian macroeconomics and its replacement by dynamic stochastic macroeconomics initiated by Robert Lucas. This revolution begs to be assessed. In Axel Leijonhufvud's words:

The main task for the history of economic thought of the second half of the twentieth century must surely be to explain this 180-degree turn in the worldview of the representative [macro] economist. (Leijonhufvud 2006a: 35)

My book is primarily addressed to those macroeconomists, be they teachers or students, who feel the need to go beyond the technicalities that provide their daily bread and butter, and wish to ponder the origin of the kind of modeling with which they are familiar. My wish is that it might be especially useful to graduate students and young academics. Their training is often purely technical and centered on the models of the day, as if there had been no useful past, and as if no conceptual or methodological problems inherited from the past still had an influence today. Of course, this book is also addressed to historians of economics, be they doing internal history, as I do, or working from an external history perspective. Finally, I hope that it will be useful to economists working in other branches of the discipline who are curious to learn what has happened in their neighbors' yards.

My essay originated from a teaching experience. For more than twenty years, I have been teaching a graduate course on the history of macroeconomics mainly at my own university (the University of Louvain in Belgium) but also, in the past, at the Sorbonne in Paris and at Duke University. In this seminar-like course, I require students to read a series of seminal works spanning the history of macroeconomics from Keynes to the present.¹ Usually students enjoy reading these texts for the simple reason that they represent a refreshing change from their normal curriculum. At each reading, they tend to be convinced by the author's arguments before often realizing these authors' shortcomings and blind spots at the next reading. I genuinely relish helping them discover that macroeconomics is full of disagreements bearing both on argumentation and policy conclusions. This approach is more congenial to me than the conventional view of monotonic progress. Of course, students change every year, but I repeat the course – so I have read some of these texts about twenty times! Surprisingly enough, I have not become bored with them, a sure testimony to their profundity. Actually, for some authors, in particular Lucas and his followers, returning to them frequently has been a good thing: due to my own prejudices and the counterintuitive nature of their thinking, it took me a long time to fully appreciate their contributions.

The history of modern macroeconomics has been witness to two important breaches. The first relates to the transition from what Keynes wrote in *The General Theory* to what it became in the hands of Keynesian economists – in Leijonhufvud's terms, the transition from the 'Economics of Keynes' into 'Keynesian Economics' (1968).² The second one was the 'Lucasian revolution,' which swept away Keynesian macroeconomics. Thus, leaving aside Keynes's *General Theory* proper, the history of macroeconomics can be divided into two eras, the first one, roughly extending from the 1940s to the 1970s, during which "Keynesian macroeconomics" held sway, and the era of "DSGE macroeconomics" – DSGE standing for dynamic-stochastic general equilibrium – that started in the mid-1970s and is still the dominant paradigm.

To give a more detailed account, Keynes's aim in *The General Theory* was to demonstrate the existence of involuntary unemployment under the assumption that wage rigidity was not responsible for it. The first generation of Keynesian economists, led by John Hicks, Franco Modigliani, and Lawrence Klein, admitted to all intents and purposes that Keynes had failed in his enterprise and

¹ These include a few chapters of the *General Theory*; Hicks's IS-LM paper; chapters XIII and XIV of Patinkin's *Money, Interest and Prices*; Clower's 1965 article; Barro and Grossman's 1971 article; Friedman's 1968 Presidential Address; Lucas's "Understanding Business Cycle" and "Problems and Methods in Business Cycle Theory" articles; one or two new Keynesian pieces; Prescott's Nobel Prize lecture; and a few more recent pieces.

² Leijonhufvud contended that there was a significant discrepancy between the content of the *General Theory* (the Economics of Keynes) and what it became in the hands of Keynesian economists.

TABLE 0.1 *The Main Episodes in the History of Macroeconomics*

Episodes	Main Characters
Keynes's <i>General Theory</i>	Keynes
Keynesian (or IS-LM) macroeconomics	Hicks, Modigliani, Klein
Monetarism	Friedman
The invention of the natural rate of unemployment	Phelps, Friedman
Disequilibrium theory	Patinkin, Clower, and Leijonhufvud
Non-Walrasian equilibrium models	Barro and Grossman, Benassy, Drèze, Malinvaud
DSGE I: Lucasian macroeconomics (or 'new classical macroeconomics' or 'rational expectations revolution')	Lucas, Sargent, Wallace, Barro
First generation of new Keynesian modeling	Akerlof, Azariadis, Ball, Blanchard, Fischer, Mankiw, Romer, Shapiro and Stiglitz, Solow, Taylor
Alternative research lines	Carlin and Soskice, Diamond, Hart, Roberts,
DSGE II: RBC modeling	Kydland and Prescott
DSGE III: second generation of new Keynesian modeling	Blanchard, Christiano, Eichenbaum and Evans, Gali, Taylor, Rotemberg, Smets and Wouters, Woodford

argued that involuntary unemployment was due to wage rigidity. This proposition is the cornerstone of Keynesian macroeconomics, centered on the IS-LM model. In the late 1960s and 1970s, Leijonhufvud and non-Walrasian equilibrium economists (following Don Patinkin's footsteps), on the one hand, and Edmund Phelps and Milton Friedman, on the other, started to question Keynesian macroeconomics in different ways and for different reasons. Treading in Friedman's and Phelps's footsteps, Robert Lucas launched a more radical attack against Keynesian macroeconomics. It led to a new approach, DSGE macroeconomics. An occurrence that had all the trappings of a Kuhnian scientific revolution, it sealed the fate of Keynesian macroeconomics. However, the ascent of the DSGE program did not occur without resistance. Defenders of Keynesian macroeconomics dismissed it on the grounds that it amounted to replacing "messy truth by precise error" (Lipsey 2000: 76). Other economists, who rallied under the 'new Keynesian' banner, tried to rescue some Keynesian insights, while espousing the new equilibrium standard Lucas had imposed. The new research program inaugurated by Lucas came to fruition with the emergence of real business cycle (RBC) modeling initiated by Finn Kydland and Edward Prescott. Bringing macroeconomics to the computer, it became the be-all and end-all of the young researchers entering the profession in the mid-1980s. Successive modifications of the inaugural RBC model brought

about the realization that a breach had imperceptibly occurred and that a distinct new way of pursuing the Lucasian program had emerged. For reasons that will become clear in the course of the book, I call it ‘second-generation new Keynesian’ modeling. Although they are built on the methodological principles of the DSGE program, these models depart from RBC modeling by bringing back a few central Keynesian assumptions and resorting to other empirical techniques than those of RBC modelers. Second-generation new Keynesian modeling was the state of the art in macroeconomics at the onset of the 2008 recession. Table 0.1 and Box 0.1 complement this summary.

Box 0.1 hints at what will be a guiding thread in my analysis, Leijonhufvud’s decision-tree metaphor for the history of macroeconomics:

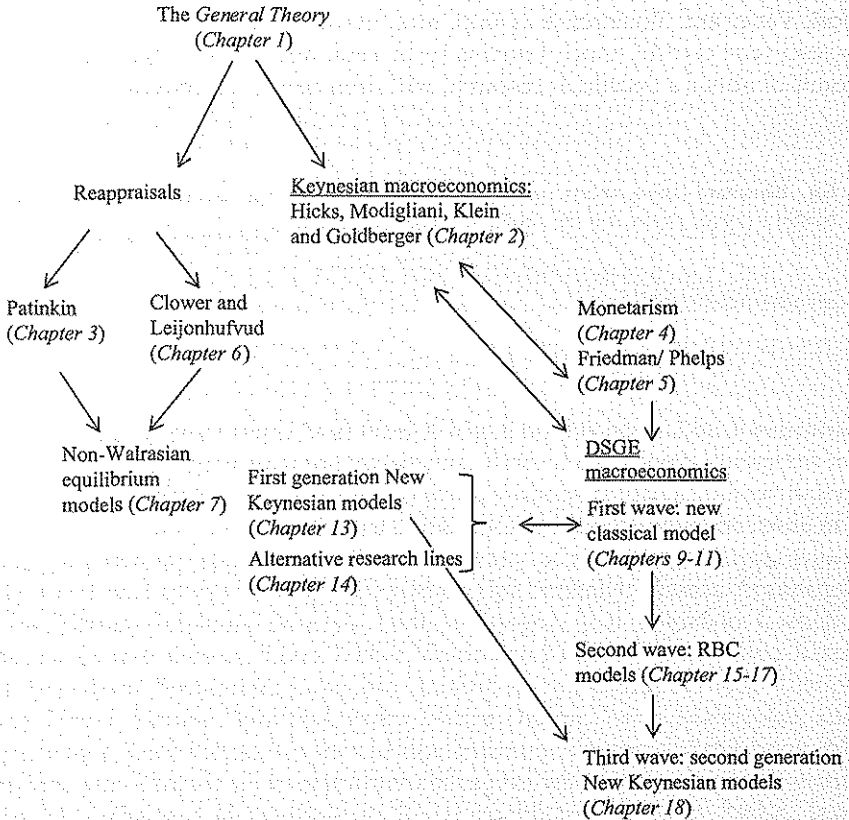
Major economists force their contemporaries to face choices – the choice of what to ask, what to assume, what to regard as evidence and what methods and models to employ – and persuade the profession or some fraction of it to follow the choice they make. The path that any particular school has followed traces a sequence of such decisions. Many of the choices faced in such a sequence were not anticipated by the founder to which we trace the development in question but were created by subsequent contributors; some of the decisions made we may judge to have been wrong in hindsight. (Leijonhufvud 1994: 148)

Any major bifurcation on the tree, that is, a new research line, starts as an original contribution, which in the beginning is like a thin new branch on a tree. Its success hinges on the attention it receives. The original work must be considered sufficiently interesting to be elaborated on, and the ensuing chain of contributions building on each other is what makes the branch sturdy. Once mature, a research track may gradually lose its momentum: puzzles arise, objections are leveled, and doubt about its validity sets in. Leijonhufvud calls what occurs then ‘backtracking,’ that is, traveling back down the decision tree to an earlier bifurcation that at the time was neglected but now seems a viable and appealing alternative. When this backtracking process goes back all the way to a distant decisional node, as happened with Lucas, it is tantamount to a scientific revolution.

My study focuses on what I view as the most salient episodes in the history of macroeconomics. I do not claim that it is exhaustive. I have chosen to give more emphasis to theoretical aspects than to empirical ones. My work is internal history and leaves aside most of the contextual dimension. I deal neither with pre-Keynesian macroeconomics nor with heterodox theory.³ Moreover, although macroeconomics as it is understood at present encompasses both growth and business fluctuations, I will say nothing about the former for the simple reason that writing its history would require a book of

³ For a study of pre-Keynesian macroeconomics, see Laidler (1999) and Dimand (2008b) For a study of heterodox macroeconomics, see King (2002) and Fine and Milonakis (2008).

BOX 0.1 A snapshot account of the history of macroeconomics



A single-headed arrow indicates a relation of continuity, a double-headed arrow a relation of opposition.

its own. I apologize to growth theorists for using the macroeconomics generic term for designating only one of its components, namely the study of business fluctuations.

It is not the role of a historian of economics to decree what the research agenda should be. Nonetheless, I will not refrain from expressing my personal critical judgments about the various economists I study. This should not be taken as a sign of arrogance. It is merely that the history of economic analysis is a *via negativa* – engaging in it amounts to a large extent to critiquing past authors. I am, of course, aware that the older a theory, the easier it is to detect its blind spots. It bears repeating that that my paramount feeling about the authors I study is one of admiration, even when I am critiquing them.

Several excellent surveys of the history of macroeconomics are available, but they are mainly articles, with the ensuing limitations.⁴ There are also a few books: Hoover (1988), Snowden, and Vane (2005) and its precursor, Snowden, Vane, and Wynarczyk (1994), and, more recently, Backhouse and Boianovski (2013). Still, much remains to be said, and I hope that this book, the result of about a decade of research, will open new perspectives.

⁴ E.g., Blanchard (1990, 2000a, 2008), Colander (2006), Hoover (2003), Leijonhufvud (2006a) Mankiw (1990, 1992a, 1992b, 2006), Woodford (1999, 2009).

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Figure 6.1 "Involuntary unemployment as resulting from a signaling defect" is drawn from Clower. "The Keynesian Counterrevolution: A Theoretical

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Figure 14. 1 “Different levels of activity in Diamond’s search model” is drawn from Diamond, P. “Aggregate Demand Management in Search Equilibrium.” *Journal of Political Economy*, vol. 90, 1992 with the permission of the University of Chicago Press.

Figures 20.1 “Effective demand *à la* Keynes and *à la* Farmer” and 20.2, “The determination of output” are drawn from *Expectations, Employment and Prices* by Roger Farmer (2010) with the permission of Oxford University Press.

PART I

KEYNES AND KEYNESIAN MACROECONOMICS

Keynes's *General Theory* and the Emergence of Modern Macroeconomics

When a modern economist reads *The General Theory*, the experience is both exhilarating and frustrating. On the one hand, the book is the work of a great mind being applied to a social problem whose currency and enormity cannot be questioned. On the other hand, although the book is extensive in its analysis, it somehow seems incomplete as a matter of logic. Too many threads are left hanging. The reader keeps asking, what, precisely, is the economic model that ties together all the pieces? (Mankiw 2006: 31)

My study must start with John Maynard Keynes's book, *The General Theory of Employment, Interest, and Money* (1936). Before writing it, Keynes was already internationally famous, a towering figure in the economics profession, as well as in policy decision making in the United Kingdom, but this book definitively placed him in the pantheon of great economists.¹ Although he had a solid reputation among academics, for a long time Keynes's main activity was providing expertise on monetary matters to the British government and international organizations. *The Treatise on Money* (Keynes 1930) was his first important foray into high theory. Sadly, Keynes's great hopes for this book were not fulfilled. Soon recognizing its flaws, he started working on what was to become *The General Theory*.

Keynes's aim in writing this book was to identify the causes of the mass unemployment that affected all developed economies in the Great Depression years. The 1930s were also a time during which Russia was witnessing strong economic results to the effect that a possible electoral victory of parties leaning toward communism (or their taking power in more unorthodox ways) was a

¹ Two renowned biographies of Keynes are Moggridge's (1992) and Skidelsky's three-volume work (1983, 1992, and 2000).

possibility that could not be discarded. In short, capitalism was in peril, both economically and politically, and Keynes realized that its survival implied important changes in its functioning. As noted by Robert Skidelsky, the task ahead intertwined theory and persuasion:

Keynes understood that his theory had to be usable for politicians and administrators: easily applied, offering political dividends. But he also understood that, before he could win the political argument, he had to win the intellectual argument. (Skidelsky 1992: 344)

The main diagnosis about the crisis available to economists at the time was of “Austrian” inspiration. The crisis, the story ran, signaled a situation of over-investment and misallocation of resources, a state of affairs that required for its solution a process of ‘liquidation,’ a real wage deflation, on the one hand, and some sanctioning of the firms that had engaged in wrong investment decisions, on the other. Flexibility was thus the motto. The more flexible prices and wages were, the faster the liquidation process would come to an end and conditions for prosperity would be reestablished. However, when the depression kept its course without wages deflation exerting its proclaimed effect, economists started to waver about the virtues of laissez-faire and to wonder whether, this doctrine to the contrary notwithstanding, governments should engage more actively in the economy. Thus, economists were torn between the policy conclusions following from accepted theory and their gut feeling that another path should be taken. Keynes’s project was to remove this contradiction by providing a theoretical argument in favor of the gut feeling. *The General Theory* ensued.

It was received enthusiastically – greeted as a “liberating revelation” in Leijonhufvud’s words (1968: 31) – especially by young economists.² There were a few dissenting voices, focusing on the shortcomings of Keynes’s reasoning, but the pressure to produce a new theoretical framework that might account for the obvious dysfunctions in the market system was such that they did not gain much traction. Keynesian theory took off rapidly. As a paradigm, it held sway until the 1970s when it came under strong attack, first by Friedman and Phelps and then by Lucas.

Today, Keynes’s theory is divisive. In the wake of the 2008 recession, after more than two decades during which Lucasian macroeconomics held sway, many economists have claimed the need to return to the master (Skidelsky 2009). In terms of Leijonhufvud’s decision-tree image, this implies a long, drawn-out backtracking process, a return either to square one (*The General*

² “One of the exciting things, of course, for a nineteen-year old was the sense of intellectual revolution, overturning the obsolete wisdom encrusted in the past, especially when the new theory was on the side of promising to do something constructive about the main problems that concerned me and people of my generation” (Tobin’s interview with Snowden and Vane [1993] 2005: 149).

Theory) or to the first subsequent node, the IS-LM bifurcation. For their part, mainstream macroeconomists reacted with outrage to this suggestion.

This is the type of divide that can hardly be settled, with the two camps talking at cross purposes and digging in their heels. There is, however, one aspect of this debate on which I have reached a firm conclusion. From the outset, 'the economics of Keynes' as well as 'Keynesian economics' (to borrow Leijonhufvud's terminology [1968]) were plagued with conceptual issues which, for the sake of pragmatism, were swept under the rug and have almost never been addressed since.³ Patinkin once observed that the fundamental problem facing the reader of *The General Theory* is that Keynes "never pulled together its various analytical components into an explicit and complete model: this task was left for its contemporary interpreters" (Patinkin 1990: 234). In itself, this would not have been dramatic, but things were actually worse. Even if Keynes had decided to take up the task of constructing the "complete model" to which Patinkin was referring, he would have been bound to fail. Many of Keynes's admirers will find this judgment too harsh. Once things are put into perspective, I think that it is not the case. Keynes could simply accomplish no more than what was possible given the state of economic theory at the time. The program he pursued was extremely ambitious, more than he realized, and he lacked the means to achieve it.

This observation explains the way in which I have chosen to deal with Keynes in this inaugural chapter, that is, by focusing on the difficulties which he encountered. I start by presenting my reconstruction of Keynes's project when he was writing *The General Theory*. Next, I bring out the obstacles to his program. I argue in particular that there was no room for a rationing outcome (and hence unemployment) in the theoretical framework Keynes wanted to use, Marshallian theory, except for the trivial wage floor assumption. I also show that economists writing after Alfred Marshall and before Keynes made scant progress on the front of unemployment theory. The chapter continues first with a presentation and next with a critique of Keynes's effective demand model, *The General Theory*'s core model. Finally, in the last section of the chapter, I briefly sketch out how Keynes's theory was transformed into Keynesian macroeconomics.

THE RESEARCH PROGRAM IN *THE GENERAL THEORY*

Since the publication of *The General Theory*, a seemingly unending flow of books have been written with the purpose of deciphering its central message. Significantly enough, after all these years no consensus has been reached, and the chances are high that there will never be one. My own reconstruction

³ For the meaning of Leijonhufvud's Economics of Keynes/Keynesian economics distinction, the reader is referred to Note 2 in the Preface.

of the research program underpinning *The General Theory* can be summarized as follows:

- a) Keynes aimed at demonstrating the theoretical existence of involuntary unemployment. The latter, he recognized, was a phenomenon whose real-world existence was compelling, especially in the years of the Great Depression, yet for which the economic theory of the time had no room. In view of the peaks to which unemployment rose in the wake of the Great Depression, Keynes decided to split unemployment into frictional and involuntary unemployment, the former considered normal and the latter abnormal. Taking for granted that the former was well understood, he zeroed in on elucidating the latter. Keynes regarded involuntary unemployment as a violation of the second classical “postulate,” referring to a state of equality between the marginal utility of consumption and the marginal disutility of labor. In modern terms, taking the standard derivation of labor supply as a reference, the criterion for the existence of involuntary is that at the closure of a given period of exchange some agents find themselves excluded from participating in the labor market in spite of the fact the market wage is higher than their reservation wage. This means that the involuntary unemployed agents, unlike the employed ones, are unable to make their optimizing plan come through, a state that can be characterized as ‘individual disequilibrium.’ Such an outcome implies that agents are heterogeneous: the unemployed enjoy less immediate utility than the employed. Looking at the matter from the market level, the situation is one in which the labor features an excess labor market supply or, in other words, a case of labor rationing.
- b) The received view of the time was that unemployment was caused by wages rigidity. Keynes was eager to dismiss this view. That is, he wanted to exonerate wage rigidity from being responsible for the presence of involuntary unemployment.
- c) Keynes’s interest in involuntary unemployment followed from the presumption that it expressed some system failure, a systemic problem affecting the working of decentralized economies. More specifically, he wanted to link involuntary unemployment with a deficiency in aggregate demand for the output as a whole, which was itself associated with some leakage from the productive towards the financial sector. The result of such a state of affairs was that the optimistic interpretation of the market economy put forward by economists since Adam Smith needed to be tempered.
- d) Keynes wrote in the Preface to the French edition of *The General Theory*, “I have called my theory a *general* theory. I mean by this that I am chiefly concerned with the behavior of the economic system as a whole” (Keynes 1939). In other words, he perceived that involuntary unemployment should be accounted for in general equilibrium terms (although he did

not use this expression): but its origin had to be sought in other parts of the economy than the labor market.⁴ Yet, Keynes's decision to adopt an interdependency perspective should not be interpreted as an adhesion to the Walrasian general equilibrium approach.⁵ To him, the route to be taken was to generalize Marshallian analysis.

- e) Instead of joining the imperfect competition line of argumentation which was emerging at the time in Cambridge, Keynes wanted to use the perfect competition framework – presumably because he associated imperfect competition with collusion, unions, and so on, whereas he wanted to bring something deeper to the fore.
- f) The remedy for involuntary unemployment which Keynes proposed was a state-induced demand activation, combined with a policy of low interest rates as well as some dose of income redistribution. To Keynes, all these measures hardly amounted to introducing socialism. On the contrary, their aim was to prevent it from arising and to preserve democratic capitalism. Hence, his characterization of his theory as “moderate conservative” (Keynes 1936: 377).
- g) After some wavering, Keynes decided to develop his argumentation within the canons of existing theory, that is, Marshallian theory.⁶ That is, his aim was to sustain his contentions with as minimal as possible changes in this theory.

ANIMAL SPIRITS

This analysis is my personal rational reconstruction of *The General Theory*. Keynes himself did not spell out his project in these terms. Likewise, none of the many accounts of what Keynes might have had in mind that can be found in the literature is exactly like mine. Still, I am of the opinion that my presentation of Keynes's project can easily be reconciled with most of them.

I readily admit that it is incomplete. Indeed, it leaves aside what, in an article reacting to some critics and published one year after his book, Keynes declared to be its central message, namely, the radical uncertainty surrounding investment decisions (Keynes 1937). Keynes's declaration is somewhat surprising as

⁴ For example, in his Appendix to chapter 19, where he criticized Pigou, Keynes wrote the following: “I maintain that the real wage ... is not primarily determined by ‘wage adjustment’ ... but by other forces of the system ... in particular the relation between the schedule of the marginal efficiency of capital and the rate of interest” (1936: 278).

⁵ At the time, Walras's views were hardly appreciated in Cambridge and, for better or worse, Keynes did not think that Walras's theory could be of any help for his own project. Clower quotes an extract of a letter from Keynes to Georgescu-Rodan, dated December 1934: “All the same, I shall hope to convince you some day that Walras's theory and all the others along those lines are little better than nonsense!” (Clower 1975, reprinted in Walker 1984: 190).

⁶ On Keynes's Marshallian roots, see Clower ([1979] 1984), Leijonhufvud (1968, 1999) and De Vroey (2011b).

uncertainty is only present in one chapter, Chapter 12, which deals with long-term expectations. Herein Keynes used the felicitous 'animal spirits' expression to refer to "a spontaneous urge to action rather than inaction, and not as the outcome of a weighted average of quantitative beliefs multiplied by quantitative probabilities" (Keynes 1936: 161). Chapter 12 is a fascinating read, yet its content is nonetheless extraneous to the rest of the book. In the latter, Keynes separated the short- and the long-period working of the economy, zeroing in on the more tractable issue, that is, the short-period determination of the level of employment, and basing his analysis on the perfect information assumption – the very opposite of animal spirits. My reconstruction of Keynes's program relates to this analytical core. As for the 1937 article, I regard it as expressing Keynes's regret about what he would have liked to analyze in his book yet was unable to.

Others have a different opinion. For example, in several papers and books, G. L. S. Shackle heralded that the idea of radical uncertainty is what should be retained from Keynes's book, much more than his analytical developments.

Keynes in *The General Theory* attempted a rational theory of a field of conduct which by the nature of its terms could be only semi-rational. But sober economists gravely upholding a faith in the calculability of human affairs could not bring themselves to acknowledge that this could be his purpose. They sought to interpret *The General Theory* as just one more manual of political arithmetic. In so far as it failed the test, they found it wrong, or obscure. (Shackle 1967: 129)

Shackle's point is appealing. The problem, however, is what to do once his conclusion has been attained, except repeating the same idea in different ways. Expanding the animal spirit idea has proven to be a hard nut to crack. There have only been a few interesting attempts, and then only decades after the publication of Keynes's book, and they have not yet gained much ascendancy.

THE OBSTACLES TO KEYNES'S PROJECT

The problem with Keynes's research program is that it was overambitious, in particular with respect to the state of economic theory at the time. Three difficulties seem paramount to me.

A first one relates to Keynes's project of generalizing Marshall's partial equilibrium analysis. At the time, Marshallian general equilibrium was non-existent and deemed unnecessary. As Joseph Schumpeter put it in his semi-centennial appraisal of Marshall's *Principles*, "A full elaboration of the theory of general equilibrium [by Marshall] could only have duplicated the work of Walras" (Schumpeter [1941] 1952: 100). I disagree with Schumpeter's judgment. As for Keynes, my view is that achieving his generalizing goal in a rigorous way was beyond his capabilities and time constraints.

A second difficulty is that at the beginning of his inquiry Keynes wanted to highlight a malfunction of the equilibration mechanism by displaying an impediment to the adjustment process. Later, Leijonhufvud labeled this process

the “laws of motions” of markets, these motions following from agents’ reactions to market signals (Leijonhufvud 2006a). In Marshall’s theory, the two distinct issues of the static determination of equilibrium and that of the equilibration dynamics were unequally addressed; whereas the former came close to receiving a mathematical treatment, the second one remained unaddressed. “Individual adaptive learning and market equilibrating processes were loosely sketched at best” (Leijonhufvud 2006a: 29–30). Marshall was hardly bothered by this defect as he took it for granted that these laws of motion worked well in reality. The contrary was true for Keynes as the economic situation he observed seemed a testimony to their malfunctioning. However, he lacked the means to make progress on the matter. This explains that he ended up setting aside the “laws of motions” research theme to content himself with static analysis. As stated by Leijonhufvud:

To find a manageable static model that would capture the essence of his theory, he [Keynes] had to reason through the dynamics ‘verbally’ while dealing with this system that was mathematically intractable! ... He was really operating beyond the limits of what Marshall’s method could accomplish. (Leijonhufvud 2006b: 70)

A third obstacle facing Keynes, the existence of which he actually was unaware, was that his project of improving on existing theory of unemployment by adding a theory of involuntary unemployment to the supposedly existing theory of frictional unemployment, all this within a Marshallian framework, was more daunting than he imagined. The reason is that Marshallian theory has no room for any kind of unemployment, being it involuntary or frictional unemployment, except for the trivial exogenous wage or price floor assumption. This point deserves a more in-depth analysis.

No Room for Unemployment in Marshall’s Principles

Let me begin with recalling the main tenets of Marshall’s value theory. The latter is based on the assumption that trade is confined to well-defined periods of exchange with production taking place before trade. Take his corn market model in Chapter 2 of Book V of the *Principles* (Marshall 1920) or his fishing industry model (Marshall 1920: 307), the two markets that Marshall considered exemplary. In these markets, at the end of a given period of exchange, the market finds itself in a state that he called “temporary equilibrium.” This result is what we now understand by market clearing. Put negatively, rationing is absent.⁷ Turning to the issue of how this outcome is reached,

⁷ Rationing is a case of short-side trading. Take a standard supply and demand graph and draw a horizontal line from the ordinate at the given price. If this line crosses the supply and demand functions at their intersection, rationing is absent. Otherwise, the first function the line intersects is the short side. Although the agents on the short side of the market achieve their desired trade, those on the long side do not and are called rationed.

Marshall assumed that all agents have a perfect knowledge of market conditions. In his words:

Though everyone acts for himself, his knowledge of what others are doing is supposed to be generally sufficient to prevent him from taking a lower or paying a higher price than others are doing. This is assumed provisionally to be true both of finished goods and of their factors of production, of the hire of labor and of the borrowing of capital. . . . I assume that there is only one price in the market at one and the same time. (Marshall 1920: 341; my emphasis)⁸

Thus, agents are supposedly able to mentally reconstruct the exact equilibrium allocation. In such a case, neither a supplier nor a demander will ever find an agent from the opposite side of the market wanting to trade at a price either higher or lower than the equilibrium price. As a result, exchanges will take place only at the market equilibrium price and quantity mix.

This analysis bears on a single period of exchange (hence the 'temporary' qualifier). It needs to be extended to a broader time range covering several such periods and their intervals. This extension can be visualized by referring to the week device put forward by Hicks in *Value and Capital* (1946: 122–23). The period of analysis is now a given succession of weeks. In this scheme, every week, production takes place from Tuesday to say Saturday (if Sunday is a holiday), with trading occurring exclusively on the next Mondays.

Once this broader perspective is adopted, a second, more fundamental, equilibrium concept enters the picture, equilibrium as a state of rest. Marshall called it 'normal equilibrium.' Two new distinctions must be considered. First a distinction must be drawn between the market and the normal supply and demand functions. Second, two types of normal equilibrium have to be separated: 'short-period normal equilibrium' acting as a center of gravity in a fixed-capital stock context, and 'long-period equilibrium' acting thusly when the capital stock is variable. Hicks ([1957] 1967:149) re-baptized Marshall's notion as 'full equilibrium.' It is achieved whenever the market-day allocation (temporary equilibrium or the matching of *market* supply and demand) coincides with the normal allocation (the matching of *normal* supply and demand). Only then do agents lack any incentive to change their behavior. The same proposition can also be expressed in reference to agents' expectations by stating that normal equilibrium is a state in which agents' expectations have been fulfilled.

Marshall's analysis perfectly admits that, at the closure of the period of exchange (or market period) producers have an incentive to change their behavior. In other words, a combination of market clearing and disequilibrium,

⁸ In a Marshallian framework, the variable that agents bargain over is the nominal price. The nominal price (and not the real one) is the variable operative in ensuring the matching of supply and demand. This principle holds for the labor market even if workers care about the real wage. Cf. Lipsey (2000: 70), Branson (1972: ch. 6) and De Vroey (2004: 63).

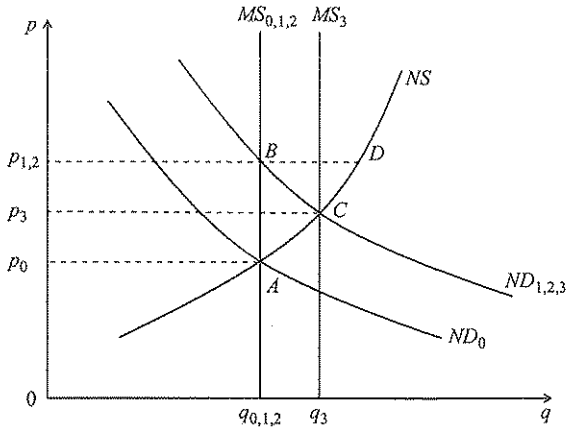


FIGURE 1.1 Temporary and normal equilibrium: Marshall's fish market

understood as a lack of full equilibrium, is a possible occurrence.⁹ Figure 1.1 illustrates this point with respect to Marshall's fish market example.

Starting from a state of full equilibrium at t_0 (point A), a change in normal demand (ND) of a moderate length occurs at t_x (I suppose that market and normal demand coincide). As for supply, a distinction has to be drawn between market supply (MS), which is vertical due to the perishable nature of fish, and short-period normal supply (NS) expressing firms' optimal plan when they can change their output by using more variable capital. The initial result of the change in demand is that at t_x the market equilibrium price rises to the distance $o-p_x$. At B, the market is in disequilibrium because the short-period normal equilibrium is not attained. Note, however, that market clearing prevails: *normal* supply and demand do not match, but *market* supply and demand do. Assume that it takes two weeks for firms to adapt and produce the new optimal quantity of fish; as a result, the market remains in the state of disequilibrium at t_2 . The short-period normal equilibrium is reached on the third week at point C.

Sluggishness is thus present in Marshall's theory, yet it concerns only the formation of normal equilibrium. One cannot assume that it also affects the formation of temporary equilibrium. Actually, it does not matter whether temporary equilibrium is reached quickly or slowly. Think of an auction market: whether the sale is conducted in five minutes or in an hour is of anecdotal importance. The same applies to the bargaining process across the perfectly informed agents participating in any given Marshallian market.

⁹ This view stands in contrast to the present-day widespread view according to which disequilibrium and market non-clearing are considered identical (a view that originates from the Walrasian vision of equilibrium).

Applying Occam's razor, we must consider that the formation of market equilibrium occurs in logical time, that is, instantaneously. Therefore, the possibility that sluggishness explains rationing must be excluded. Clearly, this result hinges on the perfect information assumption; the latter is thus as much a *deus ex machina* as the Walrasian auctioneer. Why not dispense with it then? Again, the reason lies in a matter of tractability: without the perfect information assumption, indeterminacy about the market outcome arises, and if some form of rationing result were to emerge, economists would be at a loss to devise the law of motion from rationing to market clearing.¹⁰

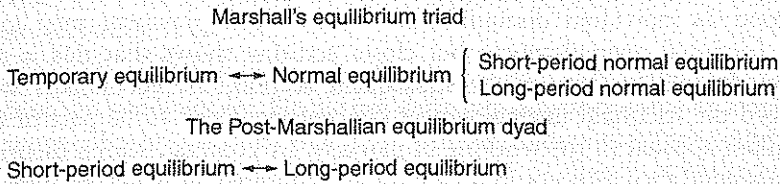
Is this conclusion to be extended to the labor market? Although no systematic analysis of the labor market is to be found in the *Principles*, Marshall made scattered remarks about it. These pertained to the particularities of the demand for and supply of labor. But he never took the further step of studying how these particularities impinge on the working of the labor market. According to Matthews (1990), Marshall's limited interest in unemployment could be explained by social context.¹¹ An additional explanation was his adhesion to the classical dichotomy. The latter consists of dividing economics into two broad subfields: value theory, dominated by equilibrium principles, where market clearing always obtains; and business cycle theory, in which monetary disturbances are supposed to play a central role and that is divorced from those principles. The notions of equilibrium and disequilibrium may still be evoked, but their use is more metaphorical than analytical. The problem is of course how to reconcile these two strands. As long as it is adopted, economists cannot but exhibit split personalities. When wearing their value theory hat, they need to exclude rationing in general (and unemployment in particular) from their discourse, whereas they have no qualms about such outcomes when speaking as business cycle theorists.

To conclude on this point, in Marshall's *Principles*, the labor market is regarded as working like the fish market. In the same way as there can be a disequilibrium output of fish, over- or underproduction with respect to the normal equilibrium quantity, cases of over- or underemployment are a likely occurrence. Yet, as Figure 1.1 illustrates, this is not rationing. The standard Marshallian theoretical apparatus is unable to tackle unemployment of any kind, save for one trivial possibility.

¹⁰ In his corn model, Marshall showed that the equilibrium of his perfect information model differed only thinly when some information imperfection was instilled into it as the result of assuming that the marginal utility of money is constant. This, he argued, vindicated his more heroic perfect information assumption.

¹¹ In Matthews's words, "Unemployment, particularly in combination with inflation, has made the functioning of the labor market a central topic in present-day economics. Unemployment has been judged as both intellectually anomalous and a social challenge. This emphasis is absent in Marshall. The social problem that disturbed *his* conscience was poverty; and poverty might have a number of causes, of which unemployment was only one" (1990: 33-34).

BOX 1.1 Marshall's equilibrium triad versus the post-Marshallian equilibrium dyad



This possibility consists in assuming the presence of a price or wage floor, set exogenously.¹² That is, taking the case of the labor market, it is assumed that some authority sets a minimal nominal wage at the beginning of the market period. If the market equilibrium wage coincides with the wage floor, market clearing is present otherwise it is absent. Wages are said to be rigid in that they cannot be lower than the floor. Although this assumption leads to a rationing result, it is trivial, and certainly not revolutionary. Moreover, either this wage floor exists for a good reason (in which case rationing is the 'price' to be paid for the advantage provided), or there is no good reason for it (in which case it should be abolished, and involuntary unemployment would disappear).

A last question to be answered is why so many economists have erred in believing that the Marshallian framework is amenable to disequilibrium in the rationing sense. I suggest the following explanation. At some point in time, a semantic evolution occurred by which Marshall's complex triadic equilibrium configuration became replaced with a dyadic relationship comprising the short and the long period. Box 1.1 illustrates. Thereby the distinction I have made in reference to the fish market between the matching between market supply and demand, on the one hand, and short-period normal equilibrium, on the other, vanishes. I invite the reader to return to Figure 1.1 and make the thought experiment of deleting the market supply of fish, the *MS* lines, from the graph. As a result, she may be tempted to interpret the excess of *normal* fish supply over *normal* demand for fish (the distance *BD*) as meaning that the supply of fish is rationed (i.e., as a case of market non-clearing). This is wrong because market non-clearing is an excess of *market-day* fish supply over *market-day* demand for fish.

It took about four decades after the publication of *The General Theory* for economists to realize that there was a deadlock and that the way out of it was to depart from the Marshallian trade technology and information assumptions.

¹² One might think that money illusion is another possibility. It will be seen in my discussion of Friedman's expectations-augmented Phillips curve model in Chapter 5 that this is not the case.

Such a departure occurred with the ascent of the search paradigm, as documented in Batyra and De Vroey (2012). In the setup adopted in search models the unemployment result merges quasi naturally. It takes the form of search unemployment, a concept that is different from involuntary unemployment understood as individual disequilibrium. Search unemployment may look akin to frictional unemployment but closer scrutiny shows that this is not the case. As will be seen in the subsequent section, the economists who introduced the frictional unemployment concept wanted it to be part of the Marshallian account of the working of markets. By contrast, search theorists have embedded their concept in a new trade technology in which features of real-world labor markets that have no place in the Marshallian framework – such as the notions of a job, of an employment relationship, of vacancies, or of wage differences and of workers visiting firms sequentially – get pride of place.

It makes no sense to blame generations of economists for having failed to address the need to go beyond standard supply and demand analysis. When Keynes was writing his *General Theory*, the times were not ripe yet. It remains that the route he chose, trying to introduce an unemployment result within this framework, led to an impasse as I will argue presently. Before that, I want to show that economists who addressed unemployment in between Marshall and Keynes made little progress.

UNEMPLOYMENT THEORY BETWEEN MARSHALL AND KEYNES

More than four decades elapsed between 1890, the date of the first edition of Marshall's *Principles*, and the publication of *The General Theory*. During this period, the issue of unemployment became crucial. It is thus a small wonder that it received more attention. However, with hindsight, the progress between these two books turned out to be limited. Let me briefly sketch out the reason why, in reference to the main works on the subject due to W. H. Beveridge (1908), Hicks (1932), and A. C. Pigou (1933).¹³

Beveridge's book, *Unemployment: A Problem of Industry* (first edition 1908; third edition 1912), was a pioneering piece because it provided its readers with a wealth of data at a time when statistics were scarce.¹⁴ Beveridge took it for granted that unemployment was frictional and attributed its existence to three types of adjustment imperfections: changes in the industrial structure, fluctuations of industrial activities, and the need for a reserve of labor in trades experiencing a high volatility of activity. The common underlying factor, Beveridge argued, was the plurality of labor markets. According to him, the

¹³ For a more detailed analysis, see De Vroey (2011b).

¹⁴ For a broader account of Beveridge's intellectual itinerary, see Dimand (1999a). For a study of the complex relations between Beveridge and Keynes, see Dimand (1999b).

solution to the problem was as straightforward as its diagnosis: the labor market needs to be better organized – that is, to become more centralized:

There shall be known centers or offices or exchanges, to which employers shall send or go when they want workpeople, to which workpeople shall go when they want employment. (Beveridge 1912: 198)

However, Beveridge did not go further than naming three causes likely to cause frictional unemployment. He did not delve into how they could be integrated in economic theory; I surmise that he was of the opinion that they could not.

Not surprisingly, Hicks's book, *The Theory of Wages*, published in 1932, had a more theoretical tone. It addressed issues such as the equality between wages and the marginal product and the simultaneous occurrence of increases in wages and in unemployment. However, on the topic of unemployment *per se*, Hicks took a rather subdued standpoint as if he had limited faith in the ability of economic theory to come to grips with it. His reasoning can be summarized as follows: (a) pure theory has little room for unemployment; (b) unemployment is nonetheless an undeniable fact of life; and (c) because there are discrepancies between the pure theory model and reality, explaining unemployment involves resorting to factors relating to the interstices between them. For example, theory states that wages must decrease in the presence of unemployment. Labor economists' task is then to explain why this has not happened. For his part, Hicks mentioned three reasons. First, an irrepressible level of unemployment always exists because of the presence of 'unemployable' workers whose efficiency is subnormal and who are long-term unemployed. A second reason lies in the existence of a non-competitive labor market in which trade unions play a central role. Third, even when the economy is in a stationary state, frictional unemployment is present.¹⁵

To conclude on Hicks, he was right in stating that contemporary economic theory had no room for unemployment. Unfortunately, this conclusion had no positive counterpart. Small wonder then that an explanation for it came to be looked for on the outskirts of pure theory, in institutional aspects – a line that several generations of labor economists were to take up.¹⁶

The third author I want to comment on is Pigou, who served as Keynes's foil in *The General Theory*. Keynes may well have been too harsh on his Cambridge

¹⁵ To Hicks, frictional unemployment was an equilibrium phenomenon, as he argued that firms have no interest in profiting from the existence of unemployment to cut wages. "By reducing wages he [the employer] has reduced his chances of getting good workmen; and sooner or later he will find that he suffers" (Hicks, 1932: 46).

¹⁶ A second edition was published in 1963. In the latter, Hicks admitted that 1932, the blackest year of the Great Depression, was not a lucky date for the publication of his book. Operating at a high level of abstraction, it had nothing to say about the situation of the time, and this was certainly shocking. Moreover, the book was published on the eve of the release of Robinson's book on imperfect competition and Keynes's *General Theory*, which were to radically change economists' vision.

colleague, but it is understandable that he took him as a scapegoat as he was the emblematic representative of traditional Marshallian theory. I personally find Pigou's book, *Theory of Unemployment* (1933), a frustrating read. It comprises about three hundred pages. Two hundred forty of them are devoted to the study of the short-period elasticity of the real demand for labor, which to Pigou seemed the only factor determining the level of employment, and related matters. It is only in Part V of the book, starting on page 247, that the subject of the causation of employment and unemployment is broached, and what Pigou had to say then was hardly original:

In stable conditions everybody is employed. The implication is that such unemployment as exists at any time is due wholly to the fact that changes in demand conditions are continually taking place and that frictional resistances prevent the appropriate wage adjustment from being made instantaneously. (Pigou 1933: 252)

Wage policy was regarded by Pigou as an aggravating factor in so far as it usually tended to impose a wage rate substantially higher than the full employment level (Pigou, 1933: 253). Pigou's overall conclusion was that, to ensure an appropriate distribution of labor, wage deflation was necessary. He could not have made a more orthodox statement.

The conclusion I draw from this brief examination is that during the period considered little progress in explaining unemployment took place. The notion of frictional unemployment may well have been invented but is content was shallow. In view of my analysis in the previous section, this is hardly surprising.

KEYNES'S EXPLANATION OF INVOLUNTARY UNEMPLOYMENT: THE EFFECTIVE DEMAND MODEL

In *The General Theory*, Keynes mistakenly took the existence of frictional unemployment as granted and accounted for. This allowed him to focus his attention on an additional kind of unemployment, which was involuntary unemployment.¹⁷

Chapter 3 of *The General Theory* summarizes his main argument with the subsequent chapters developing it in a more detailed way. As stated, Keynes's aim was to generalize Marshall's partial equilibrium analysis. Yet according to Clower's blunt assessment, what he did was nothing more than a "straight-forward concoction" (Clower 1997: 42). The first feature worth noticing about it is that it constitutes an extrapolation of Marshall's analysis of firms' optimal production decisions in a given industry in the short-period competitive equilibrium framework (Marshall 1920, Book V, chapter 5). The condition of

¹⁷ He could have taken another bifurcation: keeping a single notion yet considering distinct causal factors.

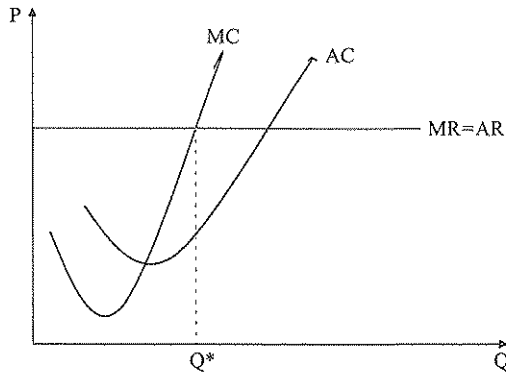


FIGURE 1.2 The firm's output decision¹⁸

optimality is the equalization of marginal revenue and marginal cost. Figure 1.2 illustrates this. To get this result, firms must decide jointly on the supply of their output and their demand for inputs. When they establish their supply curve (their marginal cost function), they need to establish a conjecture about the price of their inputs, in accordance with the possible levels of demand for their product. To simplify by considering only one of them, the wage rate, firms need to anticipate the labor market outcome associated with this rate. Note the sequential dimension of the process. The determination of equilibrium in a given branch first occurs as a thought experiment in the minds of firms' managers. It becomes an objective observable market experiment at a later date. Marshall's jumped from firms' optimizing planning to market equilibrium, which means that he implicitly assumed that these expectations are correct. This implies that firms have perfect foresight.¹⁹ The fact that all firms are in a state of individual equilibrium also implies that the market for the goods they produce is in equilibrium. The same diagnosis must be made about the different input markets for this good. As a result, it must be concluded that the wage rate, which firms have integrated in the calculation of their optimal planning, is the market-clearing wage rate.

¹⁸ MC is marginal cost, AC average cost, MR marginal revenue, and AR average revenue or price.

¹⁹ According to Kregel (1976), Keynes's program consisted of three interlocked models. The first is based on the assumptions that short-period expectations are always realized and are independent from long-term expectations. The former are assumed to be relevant for output decisions, and hence for employment, the latter for investment decisions. In this model, it is assumed that both types of expectations are fulfilled. The second model departs from the first by dropping the assumption that short-period expectations are realized. However, the fulfillment of long-period expectations remains assumed. In the last model, short-term expectations and long-term expectations are no longer assumed to be independent from one another. If we follow Kregel's reconstruction, Keynes planned to drop this assumption in a later model. Be that as it may, it remains that Keynes did not go beyond the construction of the effective demand model, the latter having the perfect information assumption as its cornerstone.

Keynes was hardly explicit about the way he extended Marshall's reasoning. For my purpose, it is necessary to identify the modifications that he introduced more precisely. First, he considered proceeds (PQ , i.e., price times quantity) as the signaling variable rather than the price alone. Second, he took employment instead of the quantity produced as the reacting variable, these two variables being linked through the production function. He also assumed indivisible labor time. Third, instead of considering a single industry, Keynes took the manufacturing sector, encompassing both final and intermediate goods, as his object of study. More precisely, he implicitly assumed that the entire economy could be divided into three subgroups: first, the labor-intensive branches (the manufacturing sector), second, those branches in which labor intervenes minimally (money and assets markets, secondhand markets) and that for the sake of argument can be supposed to employ no labor, and, third, the labor market. Any modification in demand across the first two sub-groups has an impact on employment. For example, a shift away from the demand for manufactured goods toward that for the goods of non-labor branch reduces wages and the level of activity. The effective demand model analyses what goes on in the manufacturing sector. This sector is treated as if it were a single branch. Fourth, Keynes replaced Marshall's infinitely elastic demand with an upward-sloping aggregate demand curve, its slope being determined by the propensity to consume. He also assumed that aggregate demand is driving the economy while regarding aggregate supply as passive. Fifth, Keynes introduced a notion which was absent from Marshallian theory, full employment, which he defined as "a situation in which aggregate unemployment is inelastic in response to an increase in the effective demand for its output" (Keynes 1936: 26). In his model, full employment means zero involuntary unemployment. Finally, Keynes did not follow Marshall in separating the exchange period from the short period (see Box 1. 1). He presented his model as a short-period model in the Marshallian sense (i.e., pertaining to a succession of exchange periods over which fixed capital cannot be changed), while to all intents and purposes it should be interpreted as having a single given exchange period for object of study (i.e., Marshall's market day). Indeed, a model purporting to explain the existence of involuntary unemployment must be concerned with the market day on which it arises.

Keynes accounted for the formation of effective demand along the same lines as the formation of the representative firm's equilibrium values in Marshall's analysis. As in Marshall, Keynes's reasoning requires that entrepreneurs have perfect information. In other words, expected proceeds and proceeds coincide. Entrepreneurs make their production decisions on the basis of their conjectures about the aggregate supply price of input and the aggregate demand for output functions. The nominal wage is one of the ingredients of aggregate supply (wages as a cost) and of aggregate demand (wages as income). Keynes assumed a fixed nominal wage but promised to remove this

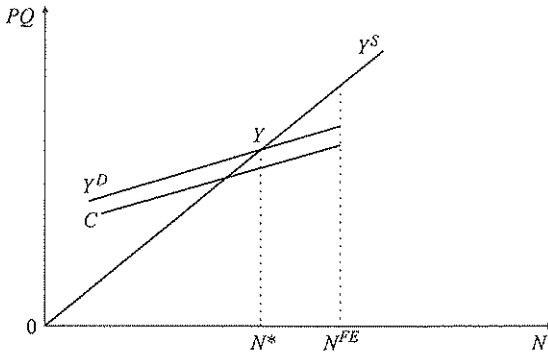


FIGURE 1.3 The determination of effective demand

assumption later in the book without imperiling his earlier results.²⁰ He coined the expression 'effective demand' to refer to the intersection of aggregate demand and aggregate supply. The determination of employment is declared coterminous to that of effective demand. Involuntary unemployment exists as soon as employment falls short of full employment. It results from a deficiency in aggregate demand, the demand for investment goods by firms being lower than the quantity that would ensure full employment. The remedy is a state-activated increase in aggregate demand through autonomous investment. This enables the aggregate demand line to shift up to the point where it intersects with aggregate supply at its kink. When this point is reached, full employment is achieved. Any further pressure on aggregate demand acts only on the price level, leaving output unchanged.

The so-called Keynesian cross or income-expenditure graph is the standard way of capturing the above reasoning. It is represented in Figure 1.3, where N is employment, N^* the level of employment as determined by effective demand, and N^{FE} full employment.

A standard representation of Keynes's argumentation is as follows. Aggregate demand (Y^D) is defined as comprising two elements, private consumption (C) and investment (I), all nominal magnitudes:

$$C = a + bY, \text{ with } b, \text{ the marginal propensity to consume smaller than } 1,$$

$$Y^D = C(Y) + I(r)$$

²⁰ In view of the importance of the nominal wage rigidity assumption, it is worth quoting the passage where Keynes justifies introducing it: "In this summary we shall assume that the money-wage and other factor costs are constant per unit of labor employed. But this simplification, with which we shall dispense later, is introduced solely to facilitate the exposition. The essential character of the argument is precisely the same whether or not money-wages, etc. are liable to change" (Keynes 1936: 27).

where r is the interest rate and $dI/dr < 0$. As seen earlier, effective demand (Y) designates the intersection of aggregate demand and aggregate supply (Y^S):

$$Y^D = Y^S$$

Through simple manipulation, Y can be expressed as:

$$Y = \left(\frac{1}{1-b} \right) [a + I(r)]$$

Firms' decisions hinge on a comparison between the marginal efficiency of capital, the relation between the expected return of one additional unit of a given capital good and its cost of production, and the interest rate (r). Firms are led to invest up to the point where these two rates are equalized. For given expectations, investment stands thus in an inverse relation to the interest rate.

Two concepts are central in this construction. The first one is the propensity to consume ($0 < dC/dY < 1$), which Keynes characterized as a "fundamental psychological law":

upon which we are entitled to depend with great confidence both *a priori* from our knowledge of human nature and from the detailed facts of experience, is that men are disposed and on average to increase their consumption as their income increases, but not by as much as the increase in their income. (Keynes 1936: 96)

The second is the famous 'multiplier,' $1/(1-b)$ explaining how an increase in autonomous expenditure exerts a multiplying effect on income.

This analysis must be completed with the determination of the equilibrium interest rate. It is obtained when the supply and the demand for money are equalized. According to Keynes, the demand for money has three components, which are usually simplified in two, the transaction (L_1) and the speculative demand (L_2) for money:

$$M^S = L_1(Y) + L_2(r)$$

Keynes assumed that the former is a stable positive function of income. As for the second, he regarded it as expressing the opportunity cost of holding money, agents confronting the zero rate of return of money holdings with the expected rate of return of holding long-term bonds, that is, the summation of their current yield and their expected gain or loss in value. Expectations play an important role here. One way of simplifying the issue is to assume agents make their decision of holding money rather than bonds by comparing the current rate with some normal value they attribute to it. The speculative demand for money can then be expressed as standing in inverse relation to the current interest rate.

Keynes insisted on two features of his theory: first, that only one market, the labor market, experiences market clearing; and, second, that this state of affairs qualifies as an equilibrium state in the classical sense of the term, that is, it

constitutes a state of rest. Moreover, to him, the deficiency of aggregate demand and the violation of Say's Law (that supply creates its own demand) were two sides of the same coin. Say's Law, which was later to become Walras's Law, states that situations of generalized excess supply cannot arise. Put differently, no single market can be in disequilibrium. Keynes wanted to challenge this view. He admitted that leakages out of the circuit of exchange were inconceivable in a barter economy, but claimed that they could arise in a monetary economy.

The above reasoning is based on the assumption of a rigid nominal wage. When introducing this assumption, Keynes announced that he would remove it in a second stage of his analysis without this removal impairing his aggregate-demand-deficiency claim. He undertook this task in chapter 19.²¹ The line of reasoning adopted by Keynes in this chapter was to study the conditions under which a decrease in nominal wages fails to cause an increase in employment. His argumentation ran as follows. The favorable effect on employment of this decrease depends on whether such a reduction will improve investment. This, in turn, depends on whether it will either increase the marginal efficiency of capital or decrease the rate of interest. Keynes argued that there is no reason to believe that either of these factors will automatically move in the direction favorable to employment. Hence no guarantee exists that a decrease in wages will reduce unemployment:

There is therefore no ground for the belief that a flexible wage policy is capable of maintaining a state of continuous full employment; – any more than for the belief that an open-market policy is capable, unaided, of achieving this result. The economic system cannot be made self-adjusting along these lines. (Keynes 1936: 267)

Keynes took this conclusion as signifying that too high wages are not the cause of involuntary unemployment. An additional conclusion he put forward was that, at least in the context of widespread involuntary unemployment, wage rigidity is a good thing, "the most advisable policy for a closed system" (Keynes 1936: 270).

A CRITIQUE

The question to address is what explains that Keynes could introduce an involuntary unemployment (rationing) result in a model that was a mere

²¹ Many commentators have praised this chapter. To Patinkin, it constitutes the apex of *The General Theory*, proving "that, the many contentions to the contrary notwithstanding, the analysis of this book does not depend on the assumption of absolutely rigid money wages" (Patinkin 1987: 28). Other interpreters of *The General Theory* treading Patinkin's footsteps are Lawlor, Darity and Horn (1987: 321), Howitt (1990: 72), and Trevithick (1992). But they all take Keynes at his word without investigating the validity of his reasoning. As will be seen presently, I am skeptical about Keynes's argumentation in it.

extension of Marshallian without resorting to the wage floor assumption. The answer to this question is that he actually resorted to this assumption.

First of all, let me observe how odd it was for Keynes to decide, allegedly for the sake of simplicity, to base his demonstration of effective demand deficiency on the assumption that nominal wages are rigid. Why not discard this rival explanatory factor at once, albeit at the price of a more complicated demonstration? Moreover, if this assumption is introduced just as a matter of simplification, it must be possible to demonstrate that deficient effective demand causes involuntary unemployment under the wage flexibility assumption. For all its greater complication, pursuing this line of argumentation would have been more apposite than taking the rival explanation as a starting point. Keynes did not do this, nor did 'old' Keynesian macroeconomists. In my view, this is a sign that the introduction of this assumption is less benign than has been acknowledged.

It is therefore important to probe into Keynes's reasoning in chapter 19, in which he claimed to have removed this troublesome assumption. Let me begin with a warning. The issue under discussion is not whether further wage deflation would have worsened the economic situation during the Great Depression. The precise question to be addressed is whether in Keynes's effective demand model the involuntary unemployment result can still be obtained when substituting market-day flexibility to market-day rigidity.

As explained above, two separate adjustment processes must be disentangled, the adjustment within the market day and the adjustment across market days. In his effective demand model, Keynes studies involuntary unemployment on a given market day. Hence the wage rigidity assumption to be removed and replaced with the flexibility assumption relates to market-day analysis. However, this is not what was removed in chapter 19. Herein the issue tackled is the adjustment process across market days, the issue of whether employment increases if wages decrease from one Monday to the next. Thus, it must be presumed that the assumption made earlier about the formation of market-day equilibrium still prevails: at each trading round, an exogenous wage floor remains. It is just that now the wage floor may be different across these rounds, and the question addressed is: Would employment increase if on market day t_2 , the wage floor was fixed below that of the t_1 floor? Keynes may well have clinched a point by stating that this result can fail to happen. Still, the wage rigidity assumption as adopted in Keynes's effective demand model argumentation is not removed.

The same dismissive conclusion can be reached differently by confronting 'effective demand *à la* Keynes' with 'effective demand *à la* Marshall,' an alternative, more classical, way of extrapolating Marshall's theory of firms' individual equilibrium behavior. One element that they have in common is the perfect foresight assumption. It is here that a clue to the difference between them is to be found. When the strict Marshallian viewpoint is adopted,

everything is simple: it is assumed that the aggregate supply price function incorporates wages at their market-clearing magnitude. Instead, when taking Keynes's line, it must be assumed that the wage rate that firms consider when constructing their supply price function is a 'false' (i.e., non-market-clearing) wage. Now, if we want to keep firms' perfect foresight assumption (and, let me repeat, we need to lest we fall into a theoretical wilderness), it must be concluded that firms' incorporation of a false wage into their supply function follows from their correct expectation that this is indeed what will happen in the labor market. That is, firms' managers are aware that in this market something impairs market clearing. No other explanation than the wage floor assumption is available as long as one remains in the canonical Marshallian framework. Therefore, all Keynes's claims to the contrary notwithstanding, it is difficult to escape the conclusion that his effective demand reasoning is based on the fixed-wage hypothesis. The reason for unemployment lies in the labor market, and no fuss should be made about effective demand being than the other way around.

A GENERAL ASSESSMENT

My assessment of *The General Theory* follows from this analysis. In this book, Keynes developed a series of views about the functioning of the market economy that had a strong *prima facie* validity. However, he was unable to translate them into a rigorous demonstration. Items (a) and (b) of Keynes's program are incompatible. More important, his program faced the insuperable deadlock of aiming at introducing an unemployment outcome in a framework that altogether has no room for it. This failure should not be taken to mean that his diagnosis was wrong; it is simply that his argumentation was wanting. This was an inevitable occurrence at the time. The aim Keynes pursued was too ambitious; the concepts and tools to achieve it were lacking as, to some extent, they still are today. As stated by Colander:

Keynes's revolution failed not because its vision was wrong, but because the tools were inadequate to the task at hand. (Colander 2006: 69)

Thus, Keynes's failure was a foregone conclusion. However, in no way must this failure hide the extraordinary breakthrough he achieved. His work changed the course of economic theory by setting the scene for a new subdiscipline, macroeconomics, that is, simplified, applied, and policy-oriented general equilibrium analysis.

A diagnosis like mine could not have been made at the time. It is thus no surprise that when *The General Theory* was published, no such criticisms were leveled against it. The relevance of the notion of involuntary unemployment was not questioned. Microeconomics was insufficiently developed to tackle the difficulties involved in trying to introduce it into neoclassical theory. Moreover, the fact that unemployment was massive was taken as an indication that it could not

be voluntary. Finally, in the context of the Great Depression, many British economists had come to think that wage cuts were to be opposed and public works were needed to mop up unemployment. Keynes's contribution was to provide a justification for this viewpoint. Although with hindsight his argumentation must be considered wanting, at the time it was sufficiently sensible and conceptually rich to win the hearts and minds of the majority of economists.

THE EMERGENCE OF KEYNESIAN MACROECONOMICS

For all the enthusiastic reception of *The General Theory*, in the wake of its publication confusion over its central message was great, even among its admirers. Progress occurred when a session of the Econometric Society Conference was devoted to the book. During this session, James Meade ([1937] 1947), Roy Harrod (1937) and Hicks (1937) gave three separate papers discussing it.²² All three set out reconstructing the classical model in order to assess whether Keynes's claim that his model was more general than the classical one was right. They all concluded that it was not. Their interpretations were also rather similar. Although they admitted that, once the wage rigidity assumption was accepted, Keynes's contribution lost much of its theoretical cutting edge, they however claimed that it maintained its policy relevance.

Out of these three papers, Hicks's was to have an extraordinary future, containing as it did the first version of what was to become the IS-LM model. In order to compare Keynes's views with those of the "classics," Hicks transformed them into a simple system of simultaneous equations. He also conceived an ingenious graph allowing the joint outcome of three different markets to be represented. Hicks's model became the organizing theoretical apparatus of the emerging discipline of macroeconomics. One may wonder what *The General Theory* would have become had Hicks's interpretation never seen the light of day.²³

The IS-LM model marked a real split between "the economics of Keynes" and "Keynesian economics." Ironically enough, from then on Keynesian economists came to declare that the hallmark of Keynesian macroeconomics was the wage rigidity assumption, the very claim that Keynesians wanted to dismiss. "Mr. Keynes goes as far as to make the rigidity of wage-rates the corner-stone of his system" (Hicks 1939:266). This move hardly resulted from a conceptual criticism as that I expressed above. Rather, it was grounded on an argument of realism. In Paul Samuelson's words:

Had Keynes begun his first few chapters with the simple statement that he found it realistic to assume that modern capitalist societies had money wage rates that were sticky

²² Young's book, *Interpreting Mr. Keynes* (1987) is a detailed historical account of the reception of the IS-LM model based on a series of interviews with economists of the time.

²³ Cf. Clower ([1975] 1984: 192-93), Laidler (1999: 324), and Backhouse and Laidler (2004).

and resistant to downwards movements, most of his insight would have remained just as valid. (Samuelson 1964: 332)

Not that the wage floor idea itself was defended. The story usually told was that in reality wages adjusted sluggishly. The wage rigidity assumption was rather viewed as an extreme but convenient representation of this fact of life.

Other, less numerous, economists underlined that taking the rigidity line amounts to reverting to the very position that Keynes attacked, a viewpoint expressed by Leijonhufvud in the following words:

The rigid wage hypothesis was *not* a novel idea in Keynes' day. That the explanation of why labor fails to sell must start from the presumption that wages are too high and won't come down is a notion that is in all probability older than is economics as a discipline. The idea that Keynes sought to differentiate himself from the "Classics" and start a "revolution" by reasserting this old platitude is not necessarily the "most plausible reading of *The General Theory*." (Leijonhufvud 1988: 210)²⁴

Leijonhufvud and the other economists who joined him in his diagnosis were right when it comes to describing Keynes's intentions. Yet too often, many of them, for example, Post-Keynesian economists, instead of admitting that Keynes had failed in implementing his program, argued that he provided a globally consistent analytical framework, an interpretation that I have tried to debunk.

The final step in the emergence of macroeconomics consisted in transforming qualitative models into empirically testable ones. Jan Tinbergen played an important role in this respect. Like Keynes, he was a reformer, motivated by the desire to understand the Great Depression and to develop policies that would prevent it from happening again. In 1936, Tinbergen constructed a macroeconometric model of the Dutch economy with a special emphasis on unemployment. In 1937 the League of Nations published Gottfried Haberler *Prosperity and Depressions*, a systematic survey of business cycle theories (Haberler 1937). To complement Haberler's theoretical essay, the League invited Tinbergen to engage in a statistical investigation of business fluctuations in the United States for the post-1918 period. It came out as a two-volume book, *Statistical Testing of Business Cycle Theories* (1939).²⁵ The second

²⁴ See also Chick (1983: 132) and Laidler ([1991] 2004: 263).

²⁵ Keynes was asked by the authorities of the League of Nations to referee Tinbergen's work (Moggridge 1973: 277-320). This led him to correspond with people at the League, as well as with Harrod and Tinbergen, and culminated in a review article in the September 1939 issue of the *Economic Journal*, to which Tinbergen wrote a reply, followed by a last comment by Keynes in the March 1940 issue. All in all, Keynes was dismissive of Tinbergen's work, being of the opinion that little was to be gained from trying to test theoretical models empirically. Too much arbitrariness was involved in such an exercise, he argued. However, Keynes's reservations towards the construction of macroeconometric models were to no avail. It was soon to become an industry that was growing too fast to be stopped by epistemological considerations such as Keynes's. See Bateman (1990) and Garrone and Marchiotti (2004).

pioneering piece of macroeconomic econometrics was Larry Klein's *Economic Fluctuations in the United States 1921-41*, published in 1950 for the Cowles Commission. In his celebrated book, *The Keynesian Revolution*, Klein (1948) wrote that Keynes's concepts were crying out for a comparison with the data. He and his co-author Arthur Goldberger played a decisive role in implementing this insight (Klein and Goldberger 1955).

In this way, the three constitutive elements of macroeconomics came into existence. Very soon macroeconomics became a new and thriving subdiscipline of economics. A product of the Great Depression, its overarching aim was to highlight market failures that could be remedied by state action. So, from the onset, it had a reformist flavor. Unemployment – and in particular involuntary unemployment – was its defining element.

Keynesian Macroeconomics: The IS-LM Model

Centered on the IS-LM model, Keynesian macroeconomics was the predominant paradigm in macroeconomics from the 1950s until the 1970s, when it came under fire without fully disappearing from the scene. My aim in this chapter is twofold. In its first three sections, I study the different stages through which Keynesian macroeconomics was put on track. The first is Modigliani's transformation of Hicks's inaugural model (the IS-LL) into what was to become the canonical IS-LM model. The second is the ascent of Keynesian macroeconomic modeling following the Klein-Goldberger model. The third is the transformation of William Phillips's empirical work on the relationship between wages and unemployment into the 'Phillips Curve.' The last section is of a more meta-theoretical nature as my concern in it is the relationship between the Keynesian and the classical approaches, a topic that came to have a life of its own under the 'the neoclassical synthesis' label.

HICKS'S "MR. KEYNES AND THE CLASSICS"

Hicks's (1904–1989) celebrated "Mr. Keynes and the Classics" paper aimed at gauging the generality of Keynes's theory. The problem, Hicks argued, was that, to the reader's perplexity, Keynes castigated classical theory without defining it precisely.¹ Therefore, Hicks believed that it was necessary to reconstruct classical theory. A comparison of the 'classical' and the 'Keynesian' systems would then allow to assess the robustness of Keynes's claim that his theory was more general. Hicks's skill lay in his ability to capture what he

¹ "Even if they [the readers of *The General Theory*] are convinced by Mr. Keynes's arguments and humbly acknowledge themselves to have been 'classical economists' in the past, they find it hard to remember that they believed in their unregenerated days the things Mr. Keynes said they believed" (Hicks 1937: 147).

declared to be the central features of the classical doctrine in a set of three equations, which made it relatively easy to reconstruct the Keynesian system along the same lines. As can be seen, the two systems differ only slightly:

The classical system	The Keynesian system
$M = kY$	$M = L(Y, r)$
$I = f(r)$	$I = f(r)$
$I = S(r, Y)$	$I = S(Y),$

M is the money supply, Y nominal income, k the 'Cambridge coefficient' (the proportion of agents' income which they keep in the form of cash), I investment, r the nominal interest rate, and S saving. The first equation expresses the money market equilibrium, the second states that investment is a function of the interest rate, and the third one is the good market equilibrium condition.

Having posited these two systems of equations, Hicks continued by presenting the graph that made him famous. It is reproduced in Figure 2.1.

The IS curve represents the combination of the nominal income and nominal interest rates for which investment and savings are equal. It is assumed that investment varies negatively with the interest rate and that saving is a positive function of income. The LM curve, which Hicks called the LL curve, indicates the combinations of income and interest rates that represent equality between the supply of and the demand for money. The latter has two components. As in *The General Theory*, the first one, the transaction demand for money is a positive function of income, the second one, the speculative demand for money, an inverse function of the interest rate. The upwards slope of the LM curve is explained as follows. If nominal income increases, the transaction demand for money also increases. In order for money supply and demand to remain balanced, the rate of interest must increase, thereby allowing the speculative

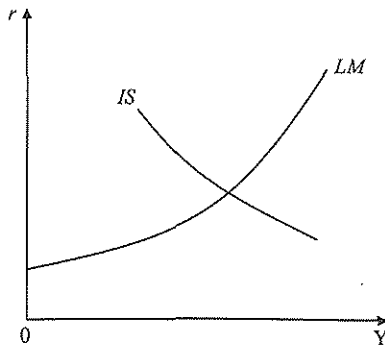


FIGURE 2.1 Equilibrium in the IS-LM model

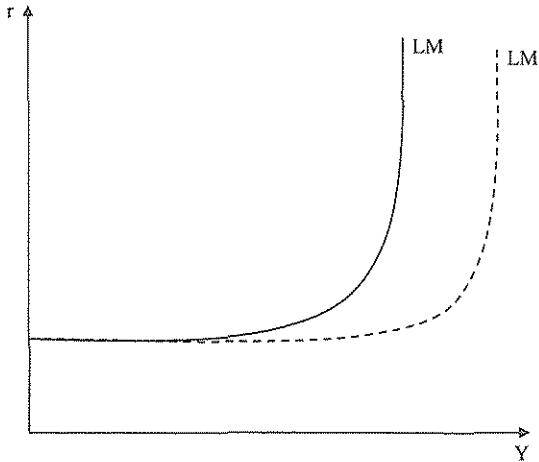


FIGURE 2.2 The Keynesian LM curve according to Hicks

demand for money to decrease. The IS-LM graph describes the interaction between the goods and the money markets leaving what happens in the two other markets, the bonds and the labor market, implicit.

As far as the labor market is concerned, Hicks's aim was not to explain why it features unemployment – its existence being taken for granted – but rather its persistence, that is, which policy is efficient in eliminating it. In his article, he merely stated that the nominal wage “can be taken as given” (Hicks 1937: 148); it cannot be given at the equilibrium level because in this case there would be no persistence of unemployment issue. It must be underlined that Hicks assumed nominal-wage rigidity in both the classical and the Keynesian subsystem. This followed from his pragmatic viewpoint on the rigidity versus flexibility issue. According to him, the choice between these two assumptions needed to reflect the reality of the moment. His opinion was that, at the time, the rigidity assumption was more relevant than the flexibility assumption. Hence it would make no sense to have different assumptions for the two subsystems.² Hicks had little interest in demonstrating the existence of involuntary unemployment. Although he did not like the term, he took its cause, wage rigidity, as a fact of life, at least in some circumstances.

The only difference between the Keynesian and the classical subsystems bears on the slope of the LM curve. In the classical subsystem, it has a positive

² As Hicks put it in his subsequent article, “The Classics’ Again”: “[Rigidity] is a special assumption that can be incorporated into any theory. Certainly the economists of the past cannot be criticized for not making it, for in their time, it would quite clearly, not have been true. This is not a matter on which there can be any theoretical contradiction; it is the kind of change in the exposition of the theory which we ought to be making, all the time, in response to changing facts” (Hicks [1957] 1967: 147).

slope. By contrast, in the Keynesian case, it comprises three sections. Starting from the origin, there is a first section that is horizontal, a second one that is upward sloping, and a third one that is vertical. The effect of a money injection is to stretch the initial part of the curve, as illustrated in Figure 2.2. Hicks's main point is that this injection exerts an impact on income only when the last two sections are relevant. Whenever the IS curve intersects the LM curve on its horizontal section – that is, when the interest-elasticity of the demand for money is infinite, the famous ‘liquidity trap’ – the new money injected into the economy is hoarded rather than spent.

Hicks's conclusion is straightforward: Keynes's claim to the contrary notwithstanding, his theory is a special case rather than a generalization of classical theory. Moreover, the Keynesian model is at odds with the classical model only when the liquidity trap prevails. According to Hicks, this happens during depressions. Then, and only then, is the Keynesian system “completely out of touch with the classical world” (Hicks 1937: 154). The classical remedy to depressions, he stated, is to increase the money supply. In so far as this leads to an increase in the price level, the real wage will decrease and an increase in employment will ensue. According to Hicks, Keynes's contribution was to show that in depressions this remedy fails to work while fiscal policy succeeds. This is exactly what Hicks's model does, displaying both a stumbling block for the traditional monetary recommendation (the liquidity trap) and an alternative remedy (acting on the IS through fiscal policy).

MODIGLIANI'S TRANSFORMATION OF HICKS'S MODEL

The IS-LM model as it stands in macroeconomics textbooks is widely believed to be a direct transcription of Hicks's own model. However, this is not the case. The transition from Keynes's economics to Keynesian economics was a two-step process, of which Hicks's models constituted the first stage. The second one was a shift from Hicks's use of the IS-LM framework to its modern version. As argued in De Vroey (2000), Modigliani's article, “Liquidity Preference and the Theory of Interest and Money” (1944), played a decisive role in this transformation. It is Modigliani's and not Hicks's version that underlay the first generation of IS-LM models. Surprisingly enough, this discontinuity has hardly been underlined.

To Modigliani (1918–2003), the hallmark of the Keynesian system consisted in the special character of the supply of labor. In his words:

Unless there is ‘full employment,’ the wage rate is not really a variable of the system but a datum, a result of ‘history’ or of ‘economic policy’ or of both. (Modigliani 1944: 47)

Modigliani's model comprises nine equations. Eight of them are standard, including the production function and the demand for labor. The production function, ($y = y(n)$), where y is output and n aggregate employment, is subject to the standard conditions. The demand for labor satisfies the condition

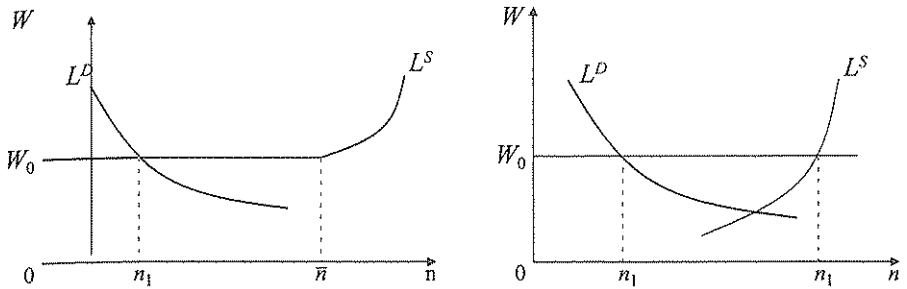


FIGURE 2.3 Contrasting two definitions of rigidity

of equality between the real wage and the marginal productivity of labor: $W=y'(n)p$, where W is the nominal wage and p the price of the good produced. Only the ninth equation, the supply of labor, is special. Modigliani assumed that the supply of labor is a function of the nominal wage: $L^S = n(W)$, thereby making money illusion a feature of Keynesian theory.³ But his main originality lay in the form he gave the labor supply. It is better to express it in inverse form:

$$W = W_0 + W(n); \quad W(n) = 0 \text{ if } 0 < n \leq \bar{n} \text{ and } W'(n) > 0 \text{ for } n > \bar{n},$$

where \bar{n} is full employment.

The left panel of Figure 2.3, referring to supply of and demand for labor, illustrates this. The distance between \bar{n} and n_1 , the intersection between supply and demand, is meant to express the size of involuntary unemployment.

In Modigliani's model, nominal wage rigidity is the only explanatory factor for the lack of full employment. However, the quantity of money plays a central role in his argumentation since it may cause a discrepancy between the possibility of a maladjustment between the quantity of money and the real wage rate. Whenever the nominal wage is too high relative to the money supply it will be profitable to increase the money supply in order to bring the level of employment closer to full employment.

Systems with rigid wage share the common property that the equilibrium value of the 'real' variables is determined essentially by monetary conditions rather than by 'real' factors. ... The monetary conditions are sufficient to determine money income and, under fixed wages and technical conditions, to each money income there corresponds a definite equilibrium level of employment. This equilibrium level does not tend to coincide with full employment except by mere chance, since there is no economic mechanism that ensures this coincidence. (Modigliani 1944: 66)

Modigliani's model has two sources of inspiration, Hicks's 1937 article and a 1938 paper by Oskar Lange, "The Rate of Interest and the Optimum

³ Nowadays, nobody dares to make this assumption, but at the time economists had no such qualms. This was, for example, the viewpoint defended by Leontief (1946) and Tobin (1947).

Propensity to Consume.” As for Hicks, there is a clear connection between his IS-LL model and Modigliani’s model. Nonetheless, the differences between them are important. To begin with, the two economists differed on the main point they wanted to bring out. While Hicks emphasized the role of liquidity preference, in Modigliani’s eyes it was unnecessary to resort to it to explain unemployment. The two models also diverge substantially beyond that. A first difference is that in Hicks’s paper, the labor market features rationing both in the classical and the Keynesian systems, as the result of the existence of a false nominal wage. In Modigliani’s model, the picture is different. In the classical subsystem, the labor market has flexible wages and exhibits market clearing; only the Keynesian subsystem exhibits wage rigidity. The two papers also differ in their policy conclusions. In Hicks’s model, monetary expansion has real effects in the classical regime but not in the Keynesian one. By contrast, in Modigliani’s model, monetary expansion is the proper remedy for involuntary unemployment. In other words, what to Hicks was ‘classical’ policy in Modigliani’s hands became ‘Keynesian’ policy!

Lange was Modigliani’s second source of inspiration.⁴ In his 1938 article, Lange presented a system of equation which was close to Hicks’s, adding the observation that the relative prices underpinning the aggregate values “may be thought of as determined by the Walrasian or Paretian system of equation of general equilibrium” (Lange 1938: 13). Lange also stated, yet with scant justification, that both the Keynesian and the classical variants of the IS-LL model were limiting cases of a more general theory of the interest rate, which was for the most part contained in Walras’s theory (Lange 1938: 20). Congruently with this interpretation, Lange considered that Keynes’s theory displayed market clearing. This led Lange to depart from Keynes on the definition of involuntary unemployment. The following extract from his 1944 book, *Price Flexibility and Employment*, confirms the similarity between Modigliani’s and Lange’s views:

‘Involuntary unemployment’ is not an excess supply of labor but an *equilibrium position* obtained by intersection of a demand and a supply curve, the supply of labor curve, however, being infinitely elastic over a wide range with respect to money wages, the point of intersection being to the left of the region where elasticity of supply of labor with respect to money wages becomes finite. (Lange 1944: 6, his emphasis)

After having characterized Modigliani’s model thusly, I want to explain why I find it to be a strange construct:

- (a) Modigliani’s ‘historically ruling wage’ idea is vague. A more choice-theoretical interpretation of his inverse-L supply curve is to reconstruct it in reference to the real rather than the nominal wage; such a re-scaling is possible as long as P is given ($w = W/P$). w_0 ; the re-scaled value of W_0 ,

⁴ Cf. Rubin (2011).

may then be thought of as the reservation wage. Still referring to the left panel of Figure 2.3, replacing W with w , at any lower wage than w_0 , agents prefer to devote their entire time endowment to leisure. Hence, the origin is also part of the labor supply. When n is supposed to refer to a representative agent, it means hours worked (intensive margin). In this case, the perfectly elastic section of the supply curve represents a situation in which consumption and leisure are perfect substitutes. The representative agent's indifference curves are thus straight lines, to the effect that any movement on this section does not change the agent's utility. When n is supposed to designate the number of (supposedly identical) participants in the labor market (extensive margin), the on_t distance refers to the employed agents, the $n_t - \bar{n}$ distance to the unemployed. However, this difference in employment status does not impinge on utility since at w_0 both the employed and the unemployed are indifferent towards working or not working. The conclusion to be drawn is that situations in which the level of employment is lower than full employment (defined as the maximum level of activity) are not necessarily suboptimal.

- (b) Modigliani's analysis brings out the need for a distinction between the notions of underemployment and involuntary unemployment. Unemployment *tout court* implies the existence of a split within the labor force between those who are employed and those who are not. This must also be true for involuntary unemployment in Keynes's sense (i.e., individual disequilibrium). Underemployment refers to a situation in which the total hours worked are equally distributed among the labor force. Furthermore, two subcategories of underemployment should be distinguished. In the first one, the maximum and the optimal levels of employment are one and the same thing while in the second the possibility that the optimal is lower than the maximum level of employment is admitted. In light of these distinctions, Modigliani's model pertains to underemployment rather than to unemployment.⁵ Moreover, when asking ourselves whether Modigliani's underemployment is 'underemployment as non-maximum employment' or 'underemployment as suboptimal employment,' the first answer turns out to be right. This means that Modigliani's outcome is efficient, so that the demand activation policy will have no impact in terms of welfare.

⁵ Modigliani used the terms involuntary unemployment, unemployment and underemployment interchangeably. The first of these is used only on one occasion, when he wrote that "one the most important achievements of the Keynesian theory [has been] that it explains the consistency of economic equilibrium with the presence of involuntary unemployment" (1944: 65). However, when defining unemployment, the definition given by Modigliani is that of involuntary unemployment: "there may be unemployment in the sense that more people would be willing to work at the current real wage rate than are actually employed" (1944: 67). This is Keynes's definition. The term that comes most frequently under Modigliani's pen (four times: pp. 65, 66, 74 and 76) is "underemployment equilibrium." However, he provides no definition for it!

- (c) Modigliani defines wage rigidity as “the infinite elasticity of the supply curve of labor when the level of employment is below ‘full’” (Modigliani 1944: 65, note 23). This is different from the notion of rigidity encountered in the previous chapter.⁶ Here, rigidity is a feature of the supply function, whereas previously it was characterized as an impediment to the functioning of the market. As seen before, rigidity can generate market rationing and involuntary unemployment (the right panel of Figure 2.3), but this is not what happens with Modigliani’s definition (the left panel). When his definition of rigidity is used, no rationing is involved.

Should we conclude in light of these criticisms that Modigliani’s model is a failure with respect to implementing the Keynesian program? The answer to this question must be subtle. Had Modigliani’s objective really been to demonstrate involuntary unemployment, understood as labor market rationing, the answer would be yes. He did claim that this was his aim. But it is also possible that, more or less unwittingly, he had two irons in the fire. When thinking about reality, he held the strong belief that a large share of existing unemployment was involuntary. However, it may be surmised that, when it came to model building, Modigliani strived to produce a Keynesian conclusion while adopting what Lucas was to call the equilibrium discipline (postulating market clearing). Thereby, he significantly departed from Keynes’s program, an attitude that one may be tempted to reproach him with. But then, if Keynes’s program has the flaws I brought out in the previous chapter, taking the more amenable ‘underemployment route’ instead of the awkward ‘involuntary unemployment’ one in order to preserve the demand activation policy conclusion may well have been the right thing to do. From this perspective, Modigliani’s strategy is commendable. His own underemployment model can hardly win the day, as it comprises neither a foundation for rigidity nor any suboptimality feature, but this is not a sufficient reason to conclude that the road opened was a dead end; subsequent developments have shown the contrary. Still, the drawback of Modigliani’s attitude is that, while taking the underemployment bifurcation, he gave the impression that he had taken the other one.

KLEIN AND THE EMERGENCE OF MACROECONOMETRIC MODELING⁷

Klein (1920–2013) was Samuelson’s first doctoral student. On Samuelson’s suggestion, he wrote his dissertation on Keynes’s theory, which was published

⁶ Cf. Rubin (2002: 213).

⁷ This section is based on De Vroey and Malgrange (2012). Visco (2014) is a general introduction to Klein’s work.

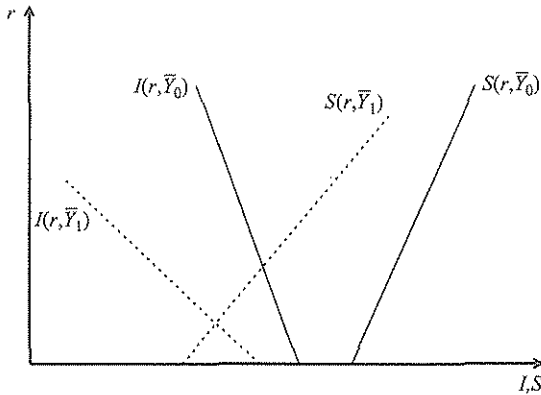


FIGURE 2.4 The lack of equilibrium between saving and investment at full-employment income, according to Klein⁸

as *The Keynesian Revolution* (Klein 1948). His work deserves attention for three reasons. The first one is that he authored *The Keynesian Revolution* (1948). In this book, Klein adopted the IS-LM framework while nevertheless trying to distance himself from its standard interpretation, which viewed either the liquidity trap or rigid money wages as the central contribution of *The General Theory*. The second reason is that, during his stay at the Cowles Commission in Chicago from 1947 to 1950, he worked on a project that can be regarded as the first attempt at achieving the ‘neoclassical synthesis program.’ The third reason relates to his role in putting macroeconometrics on track in his joint work with Arthur Goldberger. In this section, I am concerned only with the first and the third point, my examination of the second being postponed until the next chapter.

Klein’s Keynesian Revolution Book

Klein’s book was more than just of presentation of the ideas to be found in *The General Theory* (as was for example Hansen’s *A Guide to Keynes* [1953]). With respect to Hicks and Modigliani, Klein’s distinct take was to link the occurrence of involuntary unemployment with a state where, at the full employment level of activity, there is no positive rate of interest able to equalize savings and investment. Figure 2.4 illustrates this point.

At the full-employment level of output (\bar{Y}_0), the two functions fail to intersect in the positive quadrant.⁹ This only becomes possible if output is trimmed (\bar{Y}_1), with the investment and saving functions shifting to the position

⁸ Drawn from Klein (1948: 82).

⁹ Klein mentions in his Mathematical Appendix that it ought to be assumed that, when the interest rate is zero, $S(0, \bar{Y}_0) > I(0, \bar{Y}_0)$ and that $\frac{\partial I}{\partial Y} < \frac{\partial S}{\partial Y}$ (Klein 1948: 203).

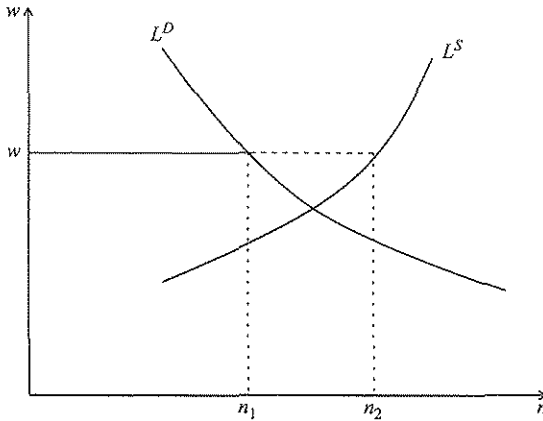


FIGURE 2.5 The labor market outcome

described by the dotted lines. This decrease in income will in turn exert an impact on the labor market, generating an excess of labor supply over labor demand at an increased real wage. Trading then takes place at a point which is off the supply curve. Figure 2.5 illustrates this: involuntary unemployment is the $n_1 - n_2$ distance. This is a case of market rationing or off the supply curve trading; thus, some agents must be experiencing individual disequilibrium.

In modern parlance, we would speak of short-side trading but Klein told another, more ideologically laden, story by declaring that this outcome resulted from an asymmetrical power relationship between employers and employees.

If there is ever any conflict between the demand and supply of labor in the perfectly competitive case like the one we are considering (e.g. one of no trade-union influence), we can be certain that a short demand will dominate a long supply (Klein 1948: 203).¹⁰

In certain respects, Klein's explanation is more appealing than Hicks's or Modigliani's. Indeed, it conveys the idea of a spillover, that is, the idea that the origin of unemployment should be sought elsewhere in the economy. Less convincing is the fact that everything in Klein's theory hinges on the investment and savings function lacking interest-elasticity. The Keynesian nature of this hypothesis is open to debate. Many passages in *The General Theory* state exactly the opposite. For example, in the final chapter of the book Keynes's urge to keep the interest rate low is based on the assumption that investment has a high interest-elasticity. While admitting his departure from Keynes's standpoint, Klein defended his own view on empirical grounds by referring to two studies based on questionnaires submitted to

¹⁰ See also Klein (1948: 86-87).

business men (Klein 1948: 65–66). Of course at the time, data were scarce, but nonetheless I am tempted to conclude that Klein's position here was as much *a priori* as empirical. So, at the end of the day, Klein's argumentation looks contrived. As with Hicks, there is a sharp contrast between Klein's claim that Keynes's theory represents a revolution with respect to traditional theory and the fact that the only difference which really matters concerns the shape of two functions. Finally, as far as his argument that workers are powerless and have to give in to firms, the question is whether there is room for this within supply and demand analysis. It amounts to stating that the labor market cannot be conceived of on the same pattern as normal markets, having employment determined unilaterally by firms rather than by the interaction between supply and demand.

The Klein-Goldberger Model

While Klein was developing his interpretation of *The General Theory*, the idea dawned on him that the conceptual apparatus set up by Keynes "cried out for empirical verification (or refutation)" (Bodkin, Klein, and Marwah 1991: 19). Undertaking this empirical extension became his life's work. Success came when his joint work with Goldberger, *An Econometric Model of the United States* (1955) blazed the way for a new field of research, macroeconomic modeling. In this book, Klein and Goldberger presented a model of the US economy which comprised fifteen structural equations and five identities. It started as a project of the Research Seminar in Quantitative Economics at the University of Michigan. The objective was, first, to make predictions about economic activity, and, second, to simulate the effects of alternative policy measures. Klein always insisted that the inspiration for it came from the IS-LM model. But significant transformations were needed. Above all, the static character of the initial model had to be replaced with a dynamic framework. Capital accumulation and technical progress had to be introduced. Some price and wage adjustments were also introduced, although only on a limited scale so that states of general excess supply were always present. These important modifications granted, it remains that the structure of the model mimics that of the standard Keynesian model with a series of additions: consumption function, investment function, corporate savings function, relationship between corporate profits and nonwage nonfarm income, depreciation function, demand for labor, production function, wage market adjustment, import demand function, agricultural income determination, household liquidity-preference function, relation between short- and long-term interest rates, definition of gross product, definition of national income, definition of wage rate, definition of stock of capital, and definition of accumulated corporate savings (Klein 1955: 314–316).

Klein and Goldberger worked in a pragmatic spirit. To them, modeling was more data- than theory-constrained. Their overarching principle was to increase

the fit between the model and reality. As a result, they had no qualms about engaging in a back-and-forth process between the specification and the estimation of parameters, a practice that was later to be vilified as 'data mining,' with the consequence that the theory supporting the model was obscured. Moreover, their model was in no way a once-and-for-all construction. Rather they viewed it as the first stage in a broader program around which other economists' might rally.

The implementation of Klein and Goldberger's project involved various steps. The first one was deciding on the mathematical structure of the model. Klein and Goldberger opted for a system of time-recursive difference equations, most of which were linear approximations of the structural theoretical relations. The fully specified system of equations they constructed had to be numerically solved for each period. The next stage, to which Klein and Goldberger devoted a lot of attention, consisted in estimating the model's parameters. To this end, they used the latest econometric techniques that were being developed at the Cowles Commission at the time. For example, they were the first to apply the limited-information maximum-likelihood technique to real data. Once the estimation task was completed, the model could be run either for predictive purposes or for comparing the effects of alternative economic policies.

The Klein-Goldberger model ensued from a Keynesian motivation, at least in the case of Klein, who started working on it in the wake of his *Keynesian Revolution* book. This is also clear when reading his contribution (Klein 1955) to the *Post-Keynesian Economics* volume, edited by Kurihara, in which he expounded the motivation underlying the building of the Klein-Goldberger model.¹¹ In this paper, Klein made it clear that the motivation for creating the model was to assert which of the two variants of the general IS-LM model, the classical and the Keynesian ones, fitted the facts best and could thus be considered right. Crucial in this respect was the way in which the workings of the labor market was accounted for. Klein encapsulated this in two equations: the classical and the Keynesian wage adjustment equations.¹²

¹¹ In this article, Klein expressed his gratitude to Goldberger for having done the calculations for his model ("Mr. Arthur Goldberger of the staff of the Research Seminar in Quantitative Economics, University of Michigan, has prepared the basic data and carried out the computation" [Klein 1955: 314, note 48]). This suggests that Goldberger played a secondary role in the development of the model, and that most of the methodological choices underpinning it can be attributed to Klein. Therefore, in the rest of this section, I shall refer only to Klein, rather than to Klein and Goldberger.

¹² Of all the equations in the Klein-Goldberger system of equations, the wage adjustment equation is the only one that is not written in terms of real (i.e., deflated) variables. The justification is as follows: "Without claiming that workers are blinded by 'money illusion,' we merely build into this equation the institutional observation that wage negotiations are usually in terms of money and not real wages" (Klein and Goldberger 1955: 18).

$$\frac{dW}{dt} = f(N^S - N^D); 0 = f(0) \quad \text{the classical wage adjustment equation}$$

$$\frac{dW}{dt} = f(N^S - N^D); 0 \neq f(0) \quad \text{the Keynesian wage adjustment equation.}$$

According to these equations, classical theory states that in equilibrium (i.e., a zero rate of change in wages), the supply of and demand for labor are equal while, by contrast, in Keynesian theory an excess supply of labor is still present. Couching the problem in these terms allowed Klein to declare that he could confront classical and Keynesian theory by assessing which of these two equations held empirically. This "testing of the association of zero unemployment with zero wage changes in the bargaining equation of the labor market" (Klein 1955: 289), was the ultimate purpose of the Klein-Goldberger model. Klein also made it clear that, in this exercise, he stuck to the Keynesian definition of involuntary unemployment as the contrary of full employment, the latter being defined as a "situation in which all who are willing to work at going real wage rates can find employment" (Klein 1955: 283).

Thus, to assess the success of Klein's project of making such an assessment, we need to focus the attention on the wage-adjustment equation of the Klein-Goldberger model. They started to express it as $dW = F(U, dP_{-1})$. Next, they transformed it into a first-difference equation where changes in wages over time are a function of the excess supply of labor, inflation and productivity (Klein and Goldberger 1955: 19).

$$W_t - W_{t-1} = \varepsilon_0 - \varepsilon_1(N - N^D - N^{SE}) + \varepsilon_2(p_{t-1} - p_{t-2}) + \varepsilon_3 t,$$

where W measures the nominal index of hourly wages, N the labor force, N^D the number of wage earners, and N^{SE} the number of self-employed workers.¹³ The last term, $\varepsilon_3 t$, can be interpreted as a proxy for the effect of increases in productivity. The bold type indicates that the variable is exogenous. The statistical estimation of this equation, calculated on the years 1929-41 and 1946-50, is:

$$W_t - W_{t-1} = 4.11 - 0.75(U) + 0.56(p_{t-1} - p_{t-2}) + 0.56t$$

(4.83) (0.63) (0.30) (0.26)

where U is unemployment and t is a time trend ($t = 1$ in 1929) (Klein and Golberger 1955: 52).

This result is hardly convincing. In Klein and Goldberger's book, it is not commented on. It may be surmised that econometricians soon discovered that, of all the results of the model, this one was one of the weakest and the most in

¹³ The selfsameness of labor supply and labor force is justified by a pragmatic reason: "it is difficult to assess individuals' economic motives beyond demographic forces and other factors in deciding whether or not to offer their services on the labor market" (Klein 1955: 307).

need of being further worked on.¹⁴ No conclusion about Klein's initial aim can be drawn from it. Surprisingly, in his 1955 article, Klein took the very opposite standpoint. For my purpose, it is worth delving into his argumentation.

To begin with, Klein admitted that labor is more difficult to assess than labor demand. He also granted that the homogeneity of labor supply in wages and prices cannot be known. Finally, he also recognized that an equation making wage movements a function of unemployment is not entirely satisfactory for the study of the "delicate question" of settling which of the Keynesian or the classical wage equation is right (Klein 1955: 309). However, although in his description of the results of the Klein-Goldberger model he wrote the above equation, when coming to assess this question, instead of turning to these results, he used an estimation of the wage adjustment equation he had made earlier (Klein 1950).

This estimated equation has the property that if the change in wage is set equal to zero, unemployment is greater than 3 million for average values of the lagged wage level. (Klein 1955: 308)

Klein added that Christ (1951) obtained a similar result. He then returned to the Klein-Goldberger model, making the following observation:

For equilibrium, we set the rate of change in prices equal to zero. We then find a zero rate of change of wages in his [Christ's] equation associated with substantial unemployment (6-7 million persons) for the average level of the lagged wage. (Klein 1955: 308)

On this basis, Klein declared that the contest between the classical and Keynesian approaches had been won by the Keynesian one.

This conclusion does not stand up to scrutiny. In Chapter 1, I remarked that Keynes's theoretical reasoning in *The General Theory* (as distinct from his remarks about reality) left aside frictional unemployment, envisaging only two possible outcomes, involuntary unemployment and full employment. The same is true for the standard IS-LM model as well as for Klein's modified IS-LM model: the only possible type of unemployment is involuntary unemployment. This standpoint is fine for theory but will not do for empirical work. In order to compare the classical and the Keynesian system, Klein and Goldberger should have addressed the question of what fraction of unemployment is involuntary, and what is not. Their mistake was to believe that real-world unemployment was necessarily the empirical counterpart of the theoretical category of market non-clearing. Or, to put it differently, they took the existence of what was later to be called the natural rate of unemployment as an indicator of the existence of involuntary unemployment. At present, this mistake may look gross, but at the time it went totally unnoticed. While we should not blame him for it, the fact

¹⁴ On another aspect, the Klein-Goldberger model proved to be more cutting-edge as it implied a tradeoff between the goals of full employment and inflation, thus anticipating the Phillips curve. Cf. Bodkin, L. Klein and K. Marwah (1991: 61).

remains that Klein's statement that he had demonstrated the empirical existence of involuntary unemployment is unwarranted.

This criticism bears on the motivation behind the model. Motivations matter less than end results. In this last respect, the construction of the Klein-Goldberger model was an impressive step forward. Several factors concurred to make this new development possible: the ascent of the IS-LM model, new and more rigorous statistical estimation methods, the systematic construction of national databases, and the invention of new calculation methods eventually leading to the emergence of computers. For the first time, governments had a quantitative macrodynamic model of the economy as a whole at their disposal.

The Klein-Goldberger model paved the way for several generations of macroeconometric models. A milestone in this development was the Brookings model. It originated at a conference on the stability of the American economy organized by the Social Science Research Council and held at Ann Arbor in 1959. Unlike the Klein-Goldberger model, it was a large disaggregated model. A further step was the MPS model (MIT-Penn-Social Science Research Council) constructed under the stewardship of Modigliani and Albert Ando. Over time, thanks to the expansion of the computer industry, models became bigger and bigger, eventually including hundreds of variables and equations. In the process, Klein's initial theoretical purpose was lost. Applied mathematicians, who did not share Klein's theoretical interest, took hold of macroeconometric modeling. They developed it in a pragmatic way as a short-period model, while quasi-automatically understanding it as implying excess labor supply.

THE BIRTH OF THE PHILLIPS CURVE¹⁵

It all started with a study of the relationship between wages and unemployment in the UK from 1861 to 1913 by William Phillips, a New Zealand economist, who taught at the London School of Economics (Phillips 1958). In order to eliminate cyclical aspects, Phillips divided this period into six subperiods. This led him to propose the following long-term relationship between the rate of nominal wage variation and the unemployment rate:

$$\dot{W} = \alpha U^{-1} - b$$

where \dot{W} is the proportionate rate of change of the nominal wage, α a positive constant, U unemployment and b the asymptote, a small negative percentage. When the unemployment rate falls toward zero, the wage rate increases, and \dot{W} tends toward infinity; conversely, when the unemployment rate rises toward 100% the wage rate decreases tending towards $-b$.

¹⁵ On the history of the Phillips curve, see Gordon (2011) and Humphrey (1985).

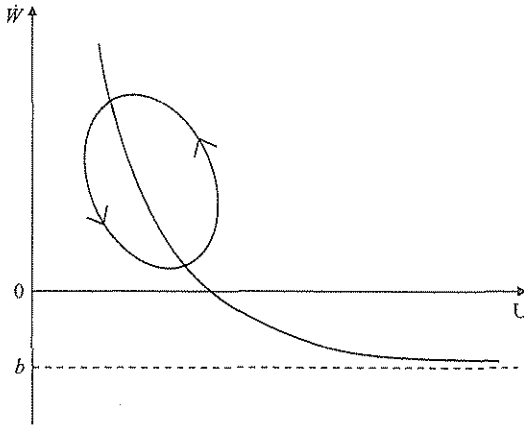


FIGURE 2.6 The Phillips relationship

As for the short-period relationship, Phillips observed that it took the shape of an ellipse revolving counterclockwise around the trend as illustrated in Figure 2.6. An increase in unemployment indicated that short-period wages were above the long-period equilibrium, and vice versa. In a further, more superficial examination of the subsequent periods, 1914–47 and 1948–57, Phillips concluded that they showed the same general pattern.

Phillips's aim was mainly empirical. Nonetheless, a vision of the workings of the labor market – and for that matter a standard vision – underpinned his study. It stressed the existence of an inverse relationship between the rate of unemployment and the change in wages. Whenever the labor market is tight, the story ran, unemployment is low and wages tend to rise, the reverse being true in the case of a slack labor market. Moreover, it was common at the time to adhere to a cost-push explanation of inflation. This led some economists to take the further step of declaring that any positive acceleration of the wage change rate was conducive to inflation, and brought the view that the Phillips relationship represented a stable relationship between unemployment and inflation. However, it is doubtful that Phillips himself shared these views.¹⁶

In the 1950s and 1960s, most macroeconomists were Keynesian. They regarded the integration of the Phillips curve into macroeconomics as a good thing, but they did not want it to happen at the expense of its Keynesian features. In particular, they did not want to abandon the involuntary

¹⁶ Leeson (2000) and Sleeman (2011) explain the context of the publication of Phillips's paper and his own reserved attitude toward it.

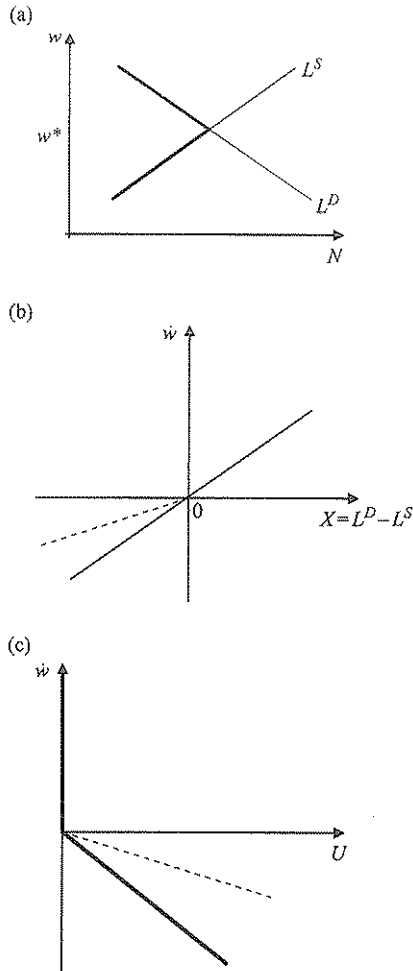


FIGURE 2.7 The discrepancy between the Phillips relation and the Phillips curve

unemployment result (unaware as they were of its fragile foundations). To this end, some theoretical reconstruction was needed. Richard Lipsey deserves the credit for having undertaken this task.

His starting point was the realization that no direct relationship exists between the Phillips curve and the standard supply and demand account of the labor market. Figure 2.7 illustrates this point. Panel (a) represents the Marshallian labor market except for the fact that it (unduly) allows out-of-equilibrium trade. The dark sections of the supply and demand schedules describe out-of-equilibrium exchanges on the assumption that trade occurs on the short side. Above the equilibrium wage, suppliers are rationed, while below the equilibrium wage rationing is applied to demanders. Involuntary

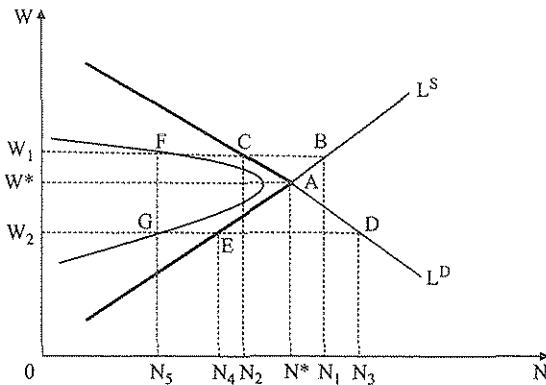


FIGURE 2.8 Integrating involuntary unemployment and frictional unemployment

unemployment is present as soon as the wage is higher than w^* .¹⁷ Panel (b) describes Lipsey's assumption about the speed of adjustment – the higher the excess demand, the faster the adjustment. The hatched section allows for the downward resistance of wages. Panel (c) combines Panels (a) and (b). It shows the relationship between the wage change rate and the unemployment rate. As can be seen, it does not resemble the Phillips relation.

Lipsey's solution to the problem was to add the frictional unemployment to the picture. As a result, full employment, defined as an absence of involuntary unemployment, can accompany a positive unemployment rate. Such a state qualifies as an equilibrium in so far as the number of unemployed people is equal to the number of vacancies. It is just that job seekers are unable to locate these vacancies (Lipsey 1960: 14).

Figure 2.8 shows that, at equilibrium, when the wage is W^* , employment is ON^* , involuntary unemployment is nil, and frictional unemployment equals BA . If the wage is $W_1 > W^*$, employment is ON_5 , and total unemployment ($N_1 - N_5$) is the sum of the two types of unemployment, (the distance $N_2 - N_5$) being frictional unemployment and the distance ($N_1 - N_2$) involuntary unemployment. If the wage is $W_2 < W^*$, involuntary unemployment is absent and frictional unemployment ($N_2 - N_3$) coexists with firms being rationed. After an additional manipulation, Lipsey reaches what will become known as the Phillips curve (see Figure 2.9).

This is how Lipsey reconciled Phillips's empirical relationship with Keynesian theory. The Phillips curve quickly found its place in the macroeconomic corpus. The first generation of IS-LM models had no explanation for the formation of the price level as they assumed a fixed price level. This state of

¹⁷ Lipsey explained its occurrence as a matter of sluggishness rather than mandatory wage floor, the explanation that I have debunked in the previous chapter.

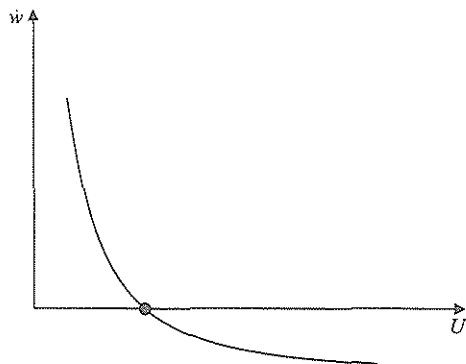


FIGURE 2.9 The Phillips curve

affairs became untenable in the 1960s, when inflation became an important phenomenon. Thus, the Phillips curve came at the right moment: serving as a law of motion for the nominal wage rate, it filled the gap of what has often been called the “missing equation” in the system.

With hindsight, two criticisms to Lipsey’s construct come to mind. The first bears on the relationship between frictional unemployment and involuntary unemployment. It is a mistake to attempt to combine these two notions in a single model because they are underpinned by irreconcilable trade-technology assumptions, the supply and demand framework in one case, the decentralized market organization in the other.¹⁸ A graph can certainly be drawn which combines the two types of unemployment, but this is because graphs do not take all what matters into account. My second criticism is that at the end of the day, it turns out that in his attempt to defend the Keynesian cause Lipsey played the sorcerer’s apprentice. To him, the reformulated Phillips curve had to combine the notions of involuntary unemployment and frictional unemployment. However, it would gradually become apparent that the Phillips curve could very well exist based only on frictional unemployment. In other words, a situation was created in which, for better or worse, involuntary unemployment could be dispensed with. This means that the Phillips curve has nothing specifically Keynesian about it.

When the Phillips curve was integrated into macroeconomic theory, a further shift occurred with the transformation of the object of study from the relationship between the wage change rate and the unemployment rate, to the relationship between the rate of change in the price level and the rate of unemployment. This prompted a further step to be taken by Samuelson and Robert Solow (1960), which consisted in declaring that the Phillips curve offered a menu for policy making. They claimed that, once armed with the “Phillips curve,” economists can provide the government with a menu of

¹⁸ Cf. Batyra and De Vroey (2012).

options: an increase in employment is achievable at the “cost” of a positive inflation rate.¹⁹ With this last development, all the elements that would allow Friedman to launch his anti-Keynesian offensive were in place.

THE NEOCLASSICAL SYNTHESIS

The period during which IS-LM macroeconomics held sway has recurrently been associated with the ‘neoclassical synthesis’ name. The latter was introduced by Samuelson in the third edition of his *Economics* textbook in 1955 to refer to the consensus that he claimed was emerging in the economics profession as to the complementarity of microeconomics and macroeconomics.

In recent years 90 percent of American economists have stopped being ‘Keynesian economists’ or ‘anti-Keynesian economists.’ Instead they have worked towards a synthesis of whatever is valuable in older economics and in modern theories of income determination. The result might be called neo-classical synthesis and is accepted in its broad outlines by all but about 5 per cent of extreme left wing and right wing writers. (Samuelson 1955: 212)

In this edition of his textbook, Samuelson used the expression time and again, yet in a loose, almost metaphorical way, without clarifying its content.²⁰ Strictly speaking, the word synthesis must be understood as designating the outcome of a process by which two theoretical analytical frameworks, which at a certain stage are viewed as unrelated, are made compatible or integrated. Nothing of this is to be found under Samuelson’s pen.

The meaning of the neoclassical synthesis term became narrowed down to refer to the relationship between the Keynesian and the ‘classical’ approaches. Hicks is the reference here, but not in his “Mr. Keynes and the Classics” article. In the latter, he used a single short-period model to confront the two approaches, which led him to regard Keynesian theory as a particular case of classical theory. Many economists casually call this result the ‘neoclassical synthesis,’ yet to me this is an abuse of language: making one model a sub-case of another is not a synthesis in the strict sense of the term. Hicks’s paper to which we must refer was written two decades later. Therein, reflecting on the nature of Keynes’s main contribution, he expressed the view that his main originality was to have shifted economic analysis from the long to the short period.

[Classical economists] did work far too much in terms of this full-equilibrium model; the qualifications due to the relative rigidity of wages (and some other prices), which should have been evident even from their experience did not receive enough attention. ... the full-equilibrium theory always received more attention than the short-period theory. (Hicks [1957] 1967: 149–150)

¹⁹ Forder (2010) and Hoover (2014) discuss the reception and subsequent interpretations of the Samuelson and Solow paper.

²⁰ See also Samuelson (1955: VII, 360, 709).

Hicks found this move praiseworthy. Not that he thought that economists should get rid of the long period; he just believed that the time had come for a change in emphasis. A division of theory between Keynesian and classical theory was thereby drawn, the former 'owning' the short period and proceeding with disequilibrium analysis, the latter the long period to be analyzed in equilibrium terms.²¹

Though enlightening, Hicks's viewpoint could not have been more general. It comprised no definition of the short and the long period. Their delineation and the nature of their relationship remained untouched. Yet, at least the ground was set for reflecting in terms of a possible synthesis. Two questions arose. The first related to a more precise identification of that classical long-period analysis. The obvious candidate was Walrasian theory. The second concerned the relationship between Keynesian and Walrasian theory. Here there was also a bifurcation. The first alternative, the only one which deserves to be called a synthesis, may be labeled the 'neoclassical synthesis program.' Here the aim is to show how Keynesian and Walrasian theory can be brought together. The second alternative, which I label the 'neoclassical synthesis viewpoint', consists in walking a tightrope between admitting that no synthesis is possible and refusing that either the Walrasian or the Keynesian approach becomes hegemonic. Thus, it amounts to defending an eclectic view of the macroeconomic discipline. According to it, macroeconomics ought to comprise different types of, possibly incompatible, models, each fit for a particular purpose. Here is how Solow, an emblematic defender of the neoclassical synthesis viewpoint, described the two standpoints in presence. There are macroeconomists

... who want to establish a canonical model, and then answer whatever question they are interested in by using that model. ... [The alternative vision is that] macroeconomics ought to be a collection of models each of which focuses on one or two of the macroeconomics mechanisms that might be operating out there. ... Now I am definitely in that second group of tinkerers and eclectics. (Solow interviewed by Snowdon and Vane 1999: 282)

My own preferred stance in macroeconomics is to be a sort of eclectic Keynesian in the short run and a sort of eclectic neoclassical economists in the long run. (Solow 2001: 27)²²

To wrap up, leaving aside Samuelson's understanding of the neoclassical synthesis expression, three possible interpretations coexist attesting to a terrible semantic muddle. First, this expression has often been taken as meaning the IS-LM methodology, in which case it is useless, having no specific content. Second, as will be seen in the next chapter, a few economists

²¹ See Blanchard (1987: 634–635) and Woodford (1999: 9).

²² Mankiw has been another staunch defender of this viewpoint (see, e.g. Mankiw (2006). For a more detailed analysis, see De Vroey and Duarte (2012).

tried to come to grips with the neoclassical synthesis program, but they happened not to put their work under this label! Third, when wondering in what way the notion has been used the most recurrently, it turns out that it is Solow's. This amounts to a full reversal of meaning: the synthesis term is now applied to a methodological standpoint admitting that no synthesis has been achieved yet hardly bothering at such a state of affairs. We will see subsequently that the Lucasian revolution involved the repudiation of neoclassical synthesis so understood.

CONCLUDING REMARKS

The IS-LM model rapidly became the cornerstone of the new discipline of macroeconomics. In as far as this model allowed two alternative sub-systems, the Keynesian and the classical ones to exist side by side, it should not, strictly speaking, be considered Keynesian. Nevertheless, at the time of its dominance, most economists were convinced that the Keynesian variant corresponded to reality while the classical system was taken as a foil.²³

Keynesian macroeconomics, which was identified with the IS-LM model, underwent tremendous developments. The initial model was extended in several directions by giving a more explicit role to the state, or integrating the international dimension among other things. Each of its components (consumption, investment, portfolio choice, and the labor market) became the object of extended investigation, bearing, for example, on testing their initial propositions, exploring their microfoundations or transforming their static formulation into a dynamic one.

Such success cannot be due to mere luck. The IS-LM model has two main virtues. The first is its ability to model economic interdependence in a simple and intuitive way. In this respect, the IS-LM model has been unrivalled. Even in its most elementary form, it lends itself to drawing cogent real-world inferences. Think, for example, of the impact on a country's economy of the victory of its national team in an important sporting event, such as the soccer World Cup. The new lease of optimism that such a victory can trigger can be translated in the terms of the IS-LM model into a rightward shift of the IS-LM curve. The second main virtue of the IS-LM model is its versatility. In Stanley Fischer's words:

The versatility of the [IS-LM] model is responsible for its survival: it can be used to analyze both monetary and fiscal policy, in both full employment and unemployment modes; it can generate quantity theory or pure Keynesian results with only minor modifications. The model is capable of accommodating monetarist and Keynesian views, as Friedman's (1970) theoretical framework shows. In my view, it can also

²³ For a general assessment of the IS-LM methodology, see Darity and Young (1995) and De Vroey and Hoover (2004).

accommodate a basic rational expectations-market clearing view, though I am not sure adherents of that approach would agree (Fischer 1987: 6 note 7).²⁴

Macroeconomic textbooks exemplify the ability of the IS-LM model to rebound. As mentioned above, the early IS-LM studied the goods and the money markets in an integrated way, dealing with the labor market and the bonds market in a separate way. The way the labor market was tackled was most rudimentary; that is, rigidity was invoked in one way or another. Present-day incarnations of the IS-LM model go beyond such an explanation. For example, Blanchard's macroeconomics textbook (2000, second edition) keeps the general IS-LM framework, while getting rid of the earlier account of the labor market, explaining wage determination by collective bargaining and efficiency wages.

Thus, the IS-LM framework provides a framework which is general enough to allow a quasi-unlimited diversity of specifications. A corollary of this plasticity is pragmatism. Whenever new specifications needed to be added, they originated in the observation of reality rather than in theoretical considerations. This plasticity also extended to policy implications, since friends and foes alike of Keynesian policy could use it to promote or refute policy prescriptions.

But the IS-LM model also has noteworthy shortcomings. Its conceptual sloppiness has been documented earlier. The lack of attention given to micro-foundations is one of the consequences of this state of affairs. Its static character was improved upon but insufficiently, for a lack of adequate tools, and also because conceptual thinking was neglected. Last but not least, the IS-LM model has been unable to achieve what it claimed to be doing, namely, explaining involuntary unemployment as a systemic market failure.

For some twenty-five years after the end of World War II, the IS-LM model dominated macroeconomics. With the advent of the new classical macroeconomics in the early 1970s, this dominance was at first challenged and then broken. Yet, the IS-LM model still lived on. Although no longer central to the graduate training of most macroeconomists or to cutting-edge macroeconomic research, it remained a mainstay of undergraduate textbooks. It also found wide use in areas of applied macroeconomics away from the front lines of macroeconomic theory, and until recently it was still at the core of most governments' and central banks' macroeconomic models.

²⁴ Alan Coddington made the same point. "Hicks's IS-LM apparatus has provided an analytical receptacle of quite astonishing versatility and resilience within which even the antagonists in protracted controversies have been able to find a common framework for their disputes. As is hardly surprising, however, this apparatus fails to capture the inspirational qualities and the feeling of boundless intellectual possibilities that many found in Keynes's work of the 1930" (Coddington 1983: 66-67).

The Neoclassical Synthesis Program: Klein and Patinkin

My aim in this chapter is to study two attempts at implementing the neoclassical synthesis program, that is, purporting to bring together Keynesian and Walrasian theory. The first is by Klein. In Chapter 2, I explained his interpretation of *The General Theory* and his pioneering role in the development of econometrics. Here I want to draw the reader's attention on the work he did earlier in his career during his stay at the Cowles Commission in Chicago from 1947 to 1950. It led to a Cowles Commission Monograph entitled *Economic Fluctuations in the United States, 1921-1941* (Klein 1950). Klein attempted to mix Walras and Keynes by resorting to the former for the theoretical foundations of the work and to the latter for the structure to organize the data. I will devote the bulk of the chapter to the second economist, Don Patinkin, the author of *Money, Interest and Prices* (Patinkin 1956, first edition), a hugely influential book. Unlike Klein, Patinkin's attempted synthesis was purely theoretical. His version of the neoclassical synthesis program assigns the study of the economy in the short period (i.e., in disequilibrium) to Keynesian theory, and that of the economy in the long period to Walrasian theory, with Walrasian equilibrium supposedly acting as the center of gravity for Keynesian disequilibrium states.

Klein's and Patinkin's projects were thus quite different. Although neither of them referred to the neoclassical synthesis terminology to characterize his project, it is what they were actually doing. One important experience they had in common was to have spent their formative years at the Cowles Commission in Chicago, pre-doc for Patinkin, post-doc for Klein. Much has been written about the Cowles Commission, so a few words of presentation will suffice.¹ Created in 1932, it moved from Colorado to Chicago in 1939 to

¹ Cf. Epstein (1987), Morgan (1989), Louçã (2007) and Qin (2013).

become the place for both high abstract theory and innovative statistical work (and trying to mix the two). The reigning attitude was one of social engineering. Dütte and Weintraub described it as a gathering of “European socialists and social democrats, and home-grown left-liberals” (2014: 79). Its senior members were eminent economists, such as Jacob Marschak, Oskar Lange, and Tjalling Koopmans, but with hindsight its list of junior members was even more impressive reading, a veritable who’s who of the economist profession with for instance names such as Kenneth Arrow, Gérard Debreu, Leonid Hurwicz, Franco Modigliani, and Edmond Malinvaud, in addition to Klein and Patinkin.

The chapter comprises three sections. The first one deals with Klein. In the second one, I give a brief reminder of some basic tenets of Walras’s *Elements of Pure Economics*, a prerequisite for my discussion of Patinkin, whose work I study in the third section.

KLEIN 1950, *ECONOMIC FLUCTUATIONS IN THE UNITED STATES*,
1921–41

The task assigned to Klein by Marschak, the Cowles director, for his three-year stay was to revive Tinbergen’s early attempts at econometric modeling. That is, he was to use modern probabilistically-based econometrics to construct structural models (systems of difference equations) under general equilibrium. In Klein’s words:

Jacob Marschak, after inquiring about professor Samuelson and his latest professional activities said to me: “What this country needs is a new Tinbergen model to forecast the performance of the American economy after the War.” This remark excited me, and I was more than pleased to consider his offer of my coming to the Cowles Commission to take up the task. (Klein, 2006: 173–174)²

The result was a monograph entitled *Economic Fluctuations in the United States, 1921–41* (Klein 1950). In retrospect, it can be regarded as the first step of the Klein-Golberger model studied in Chapter 2. Counting 168 pages, Klein’s book started with a short first chapter on the principles of model building. It was followed by a forty-five-page theoretical chapter. If present-day macroeconomists came across it, they could not but be impressed by Klein’s sophisticated coverage of households’ and firms’ decision problems. For

² In his book, *Voies de la recherche macroéconomique*, Malinvaud has a chapter on Klein’s 1950 monograph. According to his first-hand testimony, the most influential members of the Cowles Commission were not all convinced of the interest of applying the econometric method to macroeconomics. As for the econometricians themselves, Malinvaud wrote that they did not believe in Klein’s project, but did not want to discourage him. Yet, he added, “a combination of intellectual audacity, obstinacy and hard word ended up building a belief that indeed this new line of research was useful” (Malinvaud 1991: 523; my translation).

example, he considered both aggregation issues and dynamics. As for the latter, he postulated that firms maximize the present value of future profits. Their technology constraint had substitutable factors and integrated different degrees of utilization. Firms were also supposed to hold expectations about prices and wages, a weighted average of their past and present values. As far as households were concerned, Klein considered only two time periods, the present and the future, the latter being compacted in a single period. Savings were defined as the value of future goods. Price formation was supposed to occur through a Walrasian tâtonnement process. This theoretical task over, Klein constructed three increasingly complex models of the U.S. economy. The first one dealt with a simple three-equation system, the third one comprised twelve behavioral equations, four identities and eleven variables. Klein's claim was that this last model could explain the behavior of the U.S. economy during the interwar period and predict future national income. The book ended with a short chapter discussing the availability of statistical material and a thirty-three-page appendix presenting the data and time-series results and graphs.

My reason for drawing the reader's attention to Klein's monograph is that I regard it as an attempt at synthesizing Walras's and Keynes's contributions to economic theory. His theoretical chapter was state-of-the-art Walrasian theory, while the empirical model was structured around the categories proposed by Keynes in *The General Theory*, which had just started being used for national accounting series. The high level of abstractness of Walrasian theory combined with the poverty of the statistics available and the fact that econometrics was still in its infancy made a clear-cut transition from theory to measurement unachievable at the time. Klein tried to compromise as much as he could, but at the end of the day, the gulf between the theoretical model and the tested model had become so big that one might wonder why he wanted to start the analysis with Walrasian principles if they eventually had to be swept under the rug.

Klein's contribution to econometrics described in the previous chapter stemmed from his 1950 essay, yet it attested to his abandonment of the neoclassical synthesis program and of its Walrasian component.

In Chapter 2, I recounted that Klein posited the Keynesian wage adjustment equation as follows:

$$\frac{dW}{dt} = f(N^S - N^D); 0 \neq f(0)$$

Thus, in the Klein-Golberger model, even in equilibrium the labor market features a mismatch between the supply of labor and the demand for it. The fact that the Klein-Golberger model incorporated a convergence towards long-period equilibrium shows that it achieved a synthesis of some kind: not the neoclassical synthesis, understood as bringing together Keynesian and Walrasian theory, but rather a Keynes-Keynes synthesis, that is, the gravitational process from Keynesian short-period disequilibrium (i.e., state of unrest) to

Keynesian long-period equilibrium (state of rest). Such a connection had not been made either in *The General Theory* or in the theoretical IS-LM model.

WALRAS ON TÂTONNEMENT

Walras's aim in his *Elements of Pure Economics* (English translation 1954) was to study equilibrium in a competitive economy, and to demonstrate its efficiency. He believed that such a study needed to be led at a high level of abstraction and to adopt the mathematical language. By equilibrium, he meant a situation in which all agents' optimizing plans have been made compatible. In this enterprise two tasks needed to be addressed. The first one was determining the conditions under which the logical existence of such an equilibrium can be established. The second one was studying how this state can arise as the endogenous result of agents' interactions. Walras used the word 'tâtonnement' (a French word for trial and error) to refer to this second point. In each of his successive models, Walras proceeded in the same way, first determining the conditions for equilibrium, next explaining its formation through the tâtonnement process. One definite feature of this process is that agents are price-takers. Somewhat oddly, Walras came short of stating explicitly who set prices, using the French pronoun "on," which in English would be translated as "someone." Walrasian economists ended up making the assumption that this someone is an auctioneer. An outsider to the economy, her role is to elicit the equilibrium price vector by announcing prices and collecting agents' reactions in terms of proposed trading plans. The criterion for equilibrium is that every excess demand is zero. As long as this is not the case, the auctioneer changes prices in the direction of aggregate excess demand.

The idea behind the tâtonnement term was hardly new. It can be found in Chapter VII of Adam Smith's *Wealth of Nations*, in which he discussed the gravitation from market to natural prices. For Smith, the realization of natural prices results from the correction of a state of disequilibrium in which effective prices depart from natural prices. That trade takes place at disequilibrium prices was obvious to Smith. What mattered was the existence of a re-equilibration mechanism. Walras wanted his tâtonnement notion to capture Smith's insight. This implies that trading at 'false prices' is a central feature of his theoretical construct.³ In Walras's eyes, the tâtonnement hypothesis was "a natural, almost self-evident, and basically irreplaceable formalization of the way in which any real-world competitive market actually operates in the establishment of a competitive equilibrium" (Donzelli 2007: 101). So, he was taken aback when, after the publication of the first edition of the *Elements*, Bertrand (1883) and Edgeworth (1989) strongly attacked this assumption. Their criticism pointed to a path-dependency effect: out-of-equilibrium trade

³ In *Value and Capital* (1939: 128), Hicks called these non-equilibrium prices "false prices," a terminology that I borrow.

generates changes in agents' wealth after each round of exchanges. This impinges on the formation of aggregate supply and demand in the next rounds. As a result, even if the adjustment process converges toward equilibrium, the attained allocation will be different from that obtained without false trading.⁴

It took Walras about two decades to stomach the consequences of this criticism and to admit that the only way out of the gridlock was to base his analysis on the 'no trade out-of-equilibrium' assumption.⁵ After much wavering, he eventually did this in the fourth edition of the *Elements*. Thereby, in its final acceptance, the notion of tâtonnement came to refer to a virtual process occurring in logical time, that is, instantaneously, all actual disequilibrium behavior being eliminated. This amounted "to remorselessly suppress[ing] any sort of realistic pretense in the analysis of the tâtonnement process" (Donzelli 2007: 128). Thereby, the tâtonnement hypothesis became a *deus ex machina*.

Walras did not make this change wholeheartedly. He felt compelled to do it because of the contradiction between the practical solution and the theoretical solution that arose otherwise. He nonetheless kept the original tâtonnement term, which was now a misnomer. As noticed by William Jaffé, "he left us finally with a theory of market groping without any groping in it" (Jaffé [1981] in Walker [1983: 244]).

The transformation achieved by Walras led to representing the functioning of the economy as a huge auction market embedding all goods and services and all agents in a single contract. Representing a separate market as if it were an auction market is already a heroic move; extending this to the economy as a whole is an even more audacious methodological leap. Moreover, in contrast to what happens in real-world auctions, here trade does not bear on existing goods but on goods and services that will be produced or made available after the equilibrium allocation has been found. This makes the Walrasian economy a hybrid construction, a planning and private economy at the same time, which obviously runs counter to Walras's initial desire of accounting for the working of market forces. The only justification for the change in assumption is that when a problem looks intractable, it may be a good strategy to set it aside and resort to some admittedly contrived alternative device.

During the first half of the twentieth century, Walras's theory underwent a period of latency that came to an end for a variety of reasons, among which the work done at the Cowles Commission, the ascent of neo-Walrasian theory

⁴ Here is how Hicks depicted this wealth effect in *Value and Capital*: "If there is a change of price in the midst of trading, the situation appears to elude the ordinary apparatus of demand-and-supply analysis; for, strictly speaking, demand curves and supply curves give us the amount which buyers and sellers will demand and supply respectively at any particular price, if that price is fixed at the start and adhered to throughout" (Hicks 1939: 128).

⁵ For his part, Edgeworth had proposed "recontracting" to refer to the same effect in a set up without an auctioneer. That is, as long as the price is false and equilibrium is not yet reached, agents are allowed to change the contracts made earlier.

under the lead of Arrow, Debreu, and Lionel McKensie, not mentioning Jaffé's translation of Walras's *Elements of Pure Economics* (Walras 1954). This revival has an impact on my inquiry. As seen in the previous chapter, Hicks characterized Keynesian theory as concerned with the short-period leg of the short-/long-period dyad. In the wake of this revival and in particular after the publication of Samuelson's *Foundations of Economic Analysis* (1947), the long-period category became associated with Walrasian general equilibrium analysis. This brings me to Patinkin. The distinct aim he pursued was to explain how Keynesian short-period theory and Walrasian long-period theory could be pieced together – in other words, he strived at implementing the neoclassical synthesis program.

PATINKIN'S DISEQUILIBRIUM INTERPRETATION OF KEYNES

The man and his work

Patinkin (1922–1995), a student of Marschak and Lange, obtained his PhD from the University of Chicago in 1947. After spending some time at The Cowles Commission, he moved to the Hebrew University in Jerusalem, where he spent his whole career, not only building the economics department of the Hebrew University but also becoming the “the father of the economic profession in Israel” (Liviatan 2008).

Patinkin's book, *Money, Interest and Prices*, the first edition of which dates from 1956, has been a milestone in monetary theory. A perfect mix between rigor and pedagogy, it is still a fascinating read to this day. In this book, Patinkin pursued two aims. The first was addressing the issue of the integration of money in Walrasian theory. The second one was returning to the topic of his 1947 doctoral dissertation, involuntary unemployment, and trying to integrate it into the Walrasian conceptual apparatus.

As for monetary theory, Patinkin's objective was to overcome the traditional dichotomy between the determination of equilibrium relative prices analyzed in terms of supply and demand, on the one hand, and the determination of the general price level based on the quantity theory of money, on the other. Their integration was made possible by introducing the notion of ‘real balances,’ the real quantity of an agent's money holdings, and making it an argument of utility functions. Thus, in equilibrium, there existed an optimal quantity of real balances. In this new framework, the traditional view that agents who are free from money illusion will not react to a proportionate change in all money prices no longer holds.

In the second part of the book, entitled *Macroeconomics*, Patinkin studied a simplified Walrasian economy. After three chapters describing the equilibrium allocation in this economy, Patinkin set about accommodating Keynes's insights in this framework. This task was undertaken in the two Keynesian chapters of the book, chapters 13 and 14. They were written before the neoclassical

synthesis term became popular, but what Patinkin purported to do in them was nothing else than implementing the programmatic version of the synthesis.⁶

Attempting a Keynes-Walras synthesis

Patinkin conceived *Money, Interest and Prices* as a contribution to both Walrasian and Keynesian theory. Indeed, Walras and Keynes were his two sources of inspiration. He saw no incompatibility between them and hence no reason to have to choose one over the other. By thinking in this way, he treaded the footsteps of his teacher and mentor, Lange, whose views I briefly evoked in the previous chapter. However, on one point Patinkin distinctly parted ways with his mentor. Lange did not take the ‘involuntary unemployment’ expression literally; his definition of it amounted to making it a case of equilibrium underemployment. Patinkin was totally opposed to such an interpretation. As documented by Rubin (2002 and 2012), in his 1947 dissertation he already wanted involuntary unemployment to refer to a state of individual disequilibrium – that is, depicting agents as being unable to make their optimizing plan come through. For the same reason, Patinkin strongly disagreed with Modigliani, accusing him (not falsely, as seen) of sweeping involuntary unemployment under the rug (Patinkin 1965: 347).⁷ To Patinkin, if just based on the rigidity assumption, *The General Theory* lacked any originality:

If the whole purpose of Keynes is to say that with rigid wages we can have unemployment “equilibrium,” I really do not see his contribution. (Patinkin. Various. Box 29)

Yet, Patinkin was keenly aware of the fact that Keynes had been unable to fully vindicate his involuntary unemployment claim. Mending this flaw by reconstructing Keynesian theory was the mission he assigned himself, already in his dissertation and later in the Keynesian chapters of *Money, Interest and Prices*. His hunch was that involuntariness meant trading ‘off the supply curve.’

As long as workers are “on their supply curve” – that is, as long as they succeed in selling all the labor they want to at the prevailing real wage rate — a state of full employment will be said to exist in the economy. . . . Conversely, if workers are not on this curve, they are acting involuntarily. (Patinkin 1965: 315)

The real issue was to explain why off the labor supply trade could occur. Drawing from the Patinkin Archives at Duke University, Boianovsky (2002) and Rubin (2002, 2012) have convincingly highlighted that Patinkin’s views about unemployment hardly evolved linearly and cumulatively. In his dissertation in particular, he put forward the view that the root of involuntary

⁶ In his “Methods and Problems in Business Cycle Theory,” Lucas characterized Patinkin’s work as “perhaps the more refined and influential version of what I mean by the term ‘neoclassical synthesis’” (Lucas [1980] 1981: 278).

⁷ Henceforth, I will quote from Patinkin’s book second edition (1965).

unemployment lies in the existence of an "additional restraint", the presence of an extra variable in agents' budget constraint.⁸ Realizing that his argumentation failed to convince the members of his Thesis Committee (among whom Marschak, Gregg Lewis, and Paul Douglas), Patinkin backpedaled from it to the effect that in *Money, Interest and Prices* the idea of an additional restraint vanished, while the spillover effect received most of the attention.⁹

As for Patinkin's allegiance to Walrasian theory which at the time had just gained new momentum, the following reasons can be put forward. First, to him, general equilibrium was superior to partial equilibrium analysis. As he did not think it possible to do general equilibrium analysis differently from Walras, Walras's way thus looked compelling. He also favored rigor and internal consistency and therefore strongly preferred Walras to Marshall. Finally, Patinkin appreciated Walras's choice-theoretical approach. To him, this approach hardly impeded getting an individual disequilibrium result.¹⁰

On the issue of how Keynesian and Walrasian theory were linked, Patinkin had a simple answer: the model analyzed by Keynes in *The General Theory* was a simplified Walrasian model.

A basic contribution of *The General Theory* is that it is in effect the first practical application of the Walrasian theory of general equilibrium: "practical," not in the sense of empirical (though *The General Theory* did provide a major impetus to empirical work), but in the sense of reducing Walras's formal model of n simultaneous equations in n unknowns to a manageable model from which implications from the real world could be drawn. (Patinkin 1987: 27)

The analysis of this book is essentially that of general equilibrium. The voice is that of Marshall, but the hands are those of Walras. And in his IS-LM interpretation of *The General Theory*, Hicks quite rightly and quite effectively concentrated on the hands. (Patinkin, 1987: 35)

⁸ According to Rubin, when working on his dissertation, Patinkin faced the following dilemma: "How could involuntary unemployment, so defined, be consistent with choice theory?" "Involuntary" in Patinkin's "off the curve" sense meant "not chosen." But economic theory only dealt with chosen outcomes. Economic action and voluntary action were one and the same thing. To escape this contradiction, Patinkin insisted on the relative nature of 'involuntary action' and developed the concept of "additional restraint." The fact that the unemployed were "off their curve" did not mean that they were on no curve. Although they did not achieve their Walrasian plans, they were still guided by a plan but one including additional restraints. Their behavior was then "involuntary" as compared to the behavior defined by Walrasian plans, plans without additional restraints, reflecting what agents "truly" desired. The unemployed were "coerced 'in a relative sense only'" (Rubin 2012: 241-242).

⁹ Rubin goes as far as to put forward that "chapter 13 marked a crucial step forward in the elaboration of disequilibrium macroeconomics, but it may have contained simultaneously a step backward, announcing Lucas's notion of 'equilibrium discipline'. What Clower (1965) and Barro and Grossman (1971) did was to reinvent the concept of choice under additional restraints that Patinkin had put aside along the way" (Rubin 2012: 272).

¹⁰ His justification for this standpoint rested on what he called the distinction between "individual experiments" and "market experiments" ([1956] 1965: 11-12, 387-392). See also Yeager (1960: 59).

Nonetheless, Patinkin's twofold allegiance was hard to sustain. In order to construct his Keynes-Walras synthesis, he ended up having to both sacrifice two central tenets of Keynes's theory, and betray Walras's modified conception of *tâtonnement*.

The first lapse from Keynes occurs in the money part of Patinkin's book. As seen in Chapter 1, Keynes's aim was to demonstrate a state where the economy was at rest with the labor market experimenting involuntary unemployment. However, it did not take long for Keynes's contention to be questioned. Pigou opposed to it what became the 'Pigou effect,' a mechanism triggering a re-equilibration process toward full equilibrium (Pigou 1943).¹¹ Patinkin borrowed Pigou's idea and made it a central component of his project of introducing money into the utility function, renaming it the 'real balance effect.' Patinkin's argument starts from a situation of generalized individual equilibrium, when the quantity of money held by agents is optimal, and assumes a temporary shock that decreases the price level. Consequently, the quantity of balances held by households exceeds its optimal size, which drives them to devoting at least a fraction of the excess balances to purchasing commodities. This will result in reversing the declining course of the price level and bringing the economy back to equilibrium. The acceptance of this mechanism has a far-reaching consequence for Keynesian theory as it amounts to a rebuttal of Keynes's claim that involuntary unemployment is a state of rest. Thus, Patinkin's bringing the real balance effect to the forefront was to the detriment of Keynesian theory. To save the cause, he was compelled to walk a tightrope. On the one hand, he admitted that on the theoretical level the proposition "there exist no automatic forces for restoring full employment" was false. On the other hand, he backed off declaring that, though effective in theory, the real balance effect worked only slowly in reality and was likely to be thwarted by other factors. Therefore, he still felt entitled to defending demand activation as a way of speeding up the adjustment process.

The second lapse resulted from Patinkin's dogged aim to embed involuntary unemployment in Walrasian theory. His good knowledge of this theory compelled him to admit that there was no room for this notion in it as long as the analysis was confined to the study of the existence of equilibrium. This route being blocked, he laid claim to an alternative one – that involuntary unemployment existed as a disequilibrium phenomenon, limited to the re-equilibration process separating two successive equilibrium positions. Its occurrence then hinged on the slowness of the adjustment process. So, any involuntary unemployment the economy might experience is transitory because it calls the real-balance effect into operation. Confining involuntary unemployment to

¹¹ To get such an effect, Pigou introduced real cash balances as an argument of the saving function. As a result, saving varied inversely with the real stock of cash balances. If competition cuts wages and hence prices while the supply of money remains constant, an increase in M/p will allow saving and investment to be equal at full employment.

temporary disequilibrium states, Patinkin declared, is the price to be paid for making involuntary unemployment theoretically acceptable. Here again, we have a serious breach from what Keynes argued in *The General Theory*.¹²

Patinkin's argumentation

From the first part of *Money, Interest and Prices*, only one point needs to be kept in mind for my purpose, namely that chapter 3 makes it clear that Patinkin was aware of the problem Walras encountered with the tâtonnement hypothesis. Therefore, he decided to adopt Edgeworth's recontracting assumption: none of the offers to buy or to sell are "binding unless the proclaimed of prices at which they were made turns out to the equilibrium set" (Patinkin 1965: 40). Thus, out-of-equilibrium trading is excluded.

I now turn to Patinkin's macroeconomics chapters. The first task he undertook is to study the economy in equilibrium. He did it in chapter X, "The working of the model: full employment" in which full employment means generalized individual equilibrium. The economy is inhabited by households, firms and government. It comprises four markets: the market for labor services, the market for commodities, the market for bonds and the market for money. Patinkin's attention was mainly directed toward the interactions between the first two. The main elements of his model are as follows.

– Production function:

$$Y = \varphi(N, K_0) \quad (3.1)$$

Where Y is the real gross national product, N the total input of labor services and K_0 the total fixed capital equipment.

– Labor demand, at any real wage, must satisfy the relationship

$$w = \varphi_N(N, K_0), \text{ with } w = W/P \quad (3.2)$$

where w is the real wage, φ_N the marginal productivity of labor, W the nominal wage and P the price level (i.e., the price of the commodities bundle). Labor demand is expressed as

$$N^D = N^D(w, K_0)$$

– Labor supply is:

$$N^S = N^S(w) \quad (3.3)$$

– Labor market equilibrium:

$$N^D = N^S \quad (3.4)$$

¹² In further writings (e.g., Patinkin 1987), Patinkin took much pain in endeavoring to demonstrate that his reinterpretation of Keynes's theory actually coincided with what Keynes really meant.

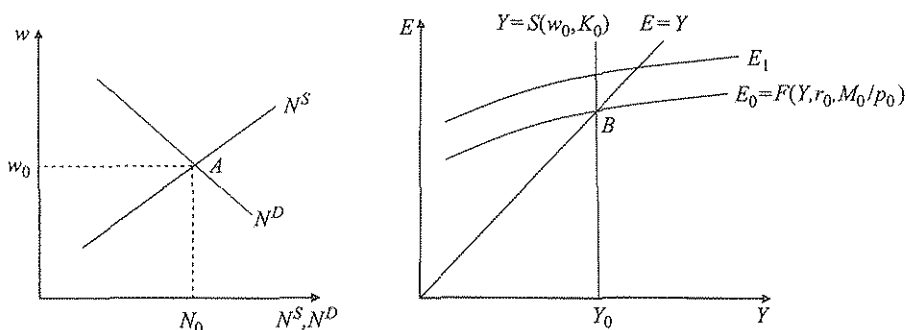


FIGURE 3.1 The commodity and labor markets in equilibrium

- The aggregate demand (E) for commodities is the sum of the real demand by households for consumption commodities, by firms for investment commodities and by the government for consumption commodities:

$E = F(Y, r, M_o/P)$, where r is the nominal interest rate and M_o the total fixed quantity of money in the economy.

- The aggregate supply of commodities is:

$Y = S(w, K_o)$; Patinkin insisted that Y is the gross national product and not income. From the previous equations, it can be drawn that

$$\phi[N^D(w, K_o), K_o] \equiv S(w, K_o)$$

That is, the magnitude of output as determined by the aggregate demand for labor, the latter based on a given w , must be equal to output as determined by the supply function. For any wage, the aggregate supply must be expressed graphically as a vertical line which shifts with variations in the real wage (see Figure 3.1, right panel).

The equilibrium condition for the commodity market is written as

$$E = Y \tag{3.5}$$

Patinkin also describes the variables and equilibrium conditions of the bonds and the money market, but they are unnecessary for my purpose. Figure 3.1, in which o suffixes indicate equilibrium, illustrates the joint equilibrium in the commodity and the labor market.

To understand the workings of Patinkin's economy, a little exercise in comparative statics may be useful. Assume that the nominal wage and the price level fall in the same proportion, the interest rate remaining unchanged. As a result, production does not change. However, through the real balance effect, the decrease in the price level causes an increase in the demand for goods (a shift from E_o to E_x in the right panel of Figure 3.1). As a result the demand for goods exceeds its supply (the vertical intercept between E_x and E_o). An upward pressure on prices is thereby triggered, which brings E_x back to E_o .

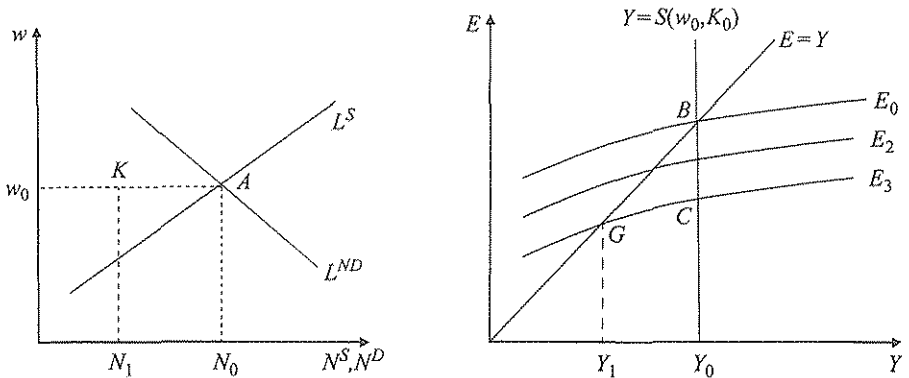


FIGURE 3.2 The commodity and the labor market in disequilibrium

Against this background, I can now expound the content of Patinkin's Keynesian chapters. In these, unlike the previous chapters, Patinkin provides no model. It may be surmised that the reason why he acted so was an inability to construct one.¹³ Let me remind the reader that his purpose consisted in integrating involuntary unemployment, understood as individual disequilibrium, into Walrasian theory. His explanation was that, after a shock, the economy's return to equilibrium is a slow process, what generates labor exchanges off the supply curve.

Starting from a situation of equilibrium, Patinkin assumes that a shock occurs, the effect of which is to increase the demand for bonds; a decrease in the demand for commodities ensues. Although he does not further comment on this occurrence, it must be assumed that it is a reversible move since in his reasoning the economy returns to the equilibrium allocation that existed before the shock. Patinkin proceeds by comparing how Walrasian and Keynesian theory come to grips with such an occurrence. According to the Walrasian story, the adjustment process in the goods market operates quickly. As a result, a new equilibrium is rapidly established so that no departure from full employment has the time to appear. By contrast, in the Keynesian story, "the adjustment process becomes a long, drawn-out one" (p. 318). Firms face a lower demand for their output (a shift from E_1 to E_3 on the right panel of Figure 3.2). Their sales fall short of their earlier equilibrium output, Y_0 . This situation presses them to decrease both the commodity price and the nominal wage. Let us assume that these two moves are proportionate, so that the real wage remains the same. In the beginning, firms pile up inventories. After a while, however, such a behavior cannot be sustained, and they have no choice but to decrease production. Production shifts to the left, from Y_0 to Y_1 . As production decreases, less labor force is needed. The result from this spillover from the

¹³ For an examination of Patinkin's failure in this respect, see Rubin (2012: 261, seq.)

goods to the labor market is that, on the labor market, firms trade off their demand for labor curve.

For the input of labor N_o they will now offer a real wage rate below that indicated by the demand curve; or, alternatively, at the real wage w_o they now demand a smaller input. (p. 320)

This is illustrated in the left panel of Figure 3.2. Assume that only N_x workers are hired. K is then the real wage/employment mix. At K , the Walrasian real wage prevails. This follows from the assumption that the nominal wage and the price level vary proportionally. Although this assumption is not needed for Patinkin's argumentation (and he will soon remove it), it is appealing because it backs up Keynes's exoneration of wages as the cause of involuntary unemployment.

The reader may have noticed that the displacement that I have just described runs counter to what Patinkin specified when characterizing his model in chapter 10, writing that the supply curve shifts only with changes in the real wage. Anticipating such criticism, Patinkin argues that point G on the right panel is not on the supply curve, no more than point K on the left panel is on the demand curve. At G , there is no excess output but an excess supply: firms would produce more "should the market be willing to absorb this output" (p. 321). Thus, an excess supply of labor ($N_o - N_x$) and an excess supply of good ($Y_o - Y_x$) exist simultaneously. Both firms and workers find themselves in a state of involuntariness, being off their supply curve.

Just as the latter [the workers] are then not receiving as much employment as they would normally like at the prevailing real wage rate, so the former [the firms] are not providing as much as much as they would normally like. Both firms and workers are being coerced by the same *force majeure* of insufficient demand in the commodity market. (Patinkin 1965: 322)

Notice how Keynesian this passage looks. Involuntariness is present, the wage rate cannot be the culprit since it has not departed from its Walrasian level, and the cause of the problem is an aggregate demand deficiency. One could not be more Keynesian, except for one point. The twofold out-of-equilibrium allocation is not a state of rest, as Keynes wanted it to be.¹⁴

This position is *not* one of equilibrium: for at point K , there is an excess supply of labor, $N_o - N_x$, which continues to press down on the money wage rate, and at point G there is an excess supply of commodities, $Y_o - Y_x$, which continues to press down on the price level. (Patinkin 1965: 320-321)

Patinkin was adamant that, in his account, prices are sluggish rather than rigid. Unlike rigidity, sluggishness implies a return to equilibrium. In Patinkin's

¹⁴ "The possibility of an automatic decrease in the extent of involuntary unemployment is what is denied by the usual oversimplified statement of Keynesian position" (Patinkin 1956: 324).

framework, this occurs through the real balance effect. The latter results in a shift upward of aggregate demand, from E_3 to E_2 . It will come to an end only when aggregate demand returns to E_1 . A parallel movement takes place in the labor market with trading moving back from N_1 to N_0 .

This is how Patinkin claimed to have improved on Keynes' objective of demonstrating the possibility of involuntary unemployment, be it at the price of rounding out Keynes' overly sharp edges. All in all, the difference between the Walrasian and the Keynesian story is limited. It is just that in the former, equilibration is instantaneous while in the latter it requires a lot of time. From the theoretical viewpoint, this difference may look thin yet, Patinkin argues, the contrary is true as far as policy is concerned. In his words, the economy may endure an "intolerably long period of dynamic adjustment":

... during which varying numbers of workers would continue to suffer from involuntary unemployment. Though I am not aware that he expressed himself in this way, this is the essence of Keynes' position. This is all that need be established in order to justify his fundamental policy conclusion that the "self-adjusting quality of the economic system" – even when reinforced by central-bank policy – is not enough. (Patinkin 1965: 339)

An assessment

Patinkin's analysis suffers from three flaws, which all follow from the fact that in the Keynesian chapters of his book he departed from the principles he adopted in the Walrasian chapters. The first is that, in his two scenarios, he took stability for granted, supposing that after a shock the economy returns to the same equilibrium. It is true that he demonstrated stability in chapter 10, but this demonstration was based on assuming fixed output and instantaneously adjusting labor market as well as recontracting. None of these two assumptions hold in the Keynesian chapters. Thus, there is no guarantee for stability in the Keynesian case.

Patinkin's second flaw is that he missed the fact that the Walrasian adjustment process must be interpreted as occurring in logical time, that is, instantaneously. Therefore drawing a contrast between quick and slow adjustment is irrelevant. He also surreptitiously departed from the assumptions he made in his Walrasian chapters. In the latter, Patinkin adopted the recontracting assumption, which is equivalent to Walras's 'no disequilibrium trade' rule. Without warning, he stopped doing so in the Keynesian chapters. Moreover, he took for granted that the Keynesian story reaches the same equilibrium allocation as the Walrasian story. This amounts to neglecting the existence of wealth effects, modifying the demand functions and hence the final equilibrium allocation. Thus, Patinkin fell prey to the very mistake that Bertrand and Edgeworth detected in the first edition of Walras's *Elements* (yet which Walras corrected in the fourth edition).

The third criticism relates to the speed of adjustment concept. The latter is a 'free parameter' the size of which is arbitrarily determined by the model builder.

Suppose that the latter is keen to get the classical result. It then suffices for her to assume a quasi-instantaneous adjustment. By contrast a Keynesian economist will desire a situation in which, as in the last quotation earlier, some workers suffer an intolerable state of affairs; to this end it suffices to assume, as Patinkin does, that the adjustment is a "long, drawn-out process."

I am aware that criticisms like mine are easy to make with the benefit of hindsight. Still, they need to be expressed. None of these defects were perceived at the time, and Patinkin's views were highly influential. Most macroeconomists adopted his sluggishness explanation of involuntary unemployment even if, when it came to translating it into their models, they adopted the wage floor or the underemployment assumption. Modigliani is a good example. He never recanted his 1944 model; yet, in discussions, when he had to pinpoint the essence of Keynesian theory, he always referred to the sluggish adjustment of wages.

A broader conclusion ought to be drawn from my study in this chapter. Patinkin was different from most Keynesian macroeconomists. They were pragmatists. They had no qualms about taking the wage floor assumption as a proxy for sluggishness or about using the 'involuntary unemployment' label for outcomes that, strictly speaking, should be called equilibrium underemployment. Patinkin was more alert to conceptual consistency. He was also aware that a problem of compatibility between Keynesian and neoclassical theory, taken as Walrasian theory, existed, and he took solving it at heart. But this was to no avail. My inquiry in this book has led me to believe that any attempt at a Keynes-Walras synthesis is doomed to fail at least in so far as its Keynesian component has involuntary unemployment, defined as individual disequilibrium, at its core. Once more, this is the kind of statement that can only be made after such a project has been undertaken. This conclusion will be further supported when I study non-Walrasian equilibrium modeling in Chapter 7.

Milton Friedman and the Monetarist Debate

Chapter 2 closed with the observation that, from the 1950s to the 1970s, Keynesian macroeconomics held sway over the field. In the present one I want to show that things were more complicated as, in the 1960s, the Keynesian consensus came to be the object of a fierce attack, the ‘monetarist counter-revolution.’ Milton Friedman was its main champion. Intense methodological brawls between Keynesians and monetarists ensued. In this chapter, I discuss the main tenets of monetarism and the disputes between monetarists and Keynesians. In view of the overarching role played by Friedman, I will focus on his work, thereby sidestepping the contributions of other monetarists such as Karl Brunner and Alan Meltzer. The next chapter will also be concerned with Friedman, this time in his quality of co-inventor, jointly with Phelps, of the natural rate of unemployment concept.

MILTON FRIEDMAN: THE MAN AND HIS WORK¹

Milton Friedman (1912–2006) played a decisive role in the history of twentieth-century macroeconomics. As Snowdon and Vane (2006) put it in the title of their obituary, he was a “Polemicist, Scholar, and Giant of Twentieth-century Economics.” After having first worked as an applied statistician and taught at a few universities, in 1946 Friedman got a position at the University of Chicago where he stayed for thirty-one years before retiring and moving to the Hoover Institute at Stanford. With George Stigler and Gary Becker, he became one of the best known professors at Chicago’s department of economics. At the beginning of his career, Friedman was an outlier,

¹ Friedman’s work has been much discussed. In writing this chapter, I have drawn from de Marchi and Hirsch (1990), Laidler (1990, 2007, 2012), Hetzel (2007), Hammond (1996), Cagan (2008), Johnson (1971), Mayer (1978), and Thygesen (1977).

criticizing Keynesian theory at a time when it was the uncontested orthodoxy. For many years, his views were met with condescension or hostility.² In the 1970s, however, things changed; Friedman's work gained recognition and his views started to exert a strong influence on both theory and policy.

Treading Keynes's footsteps, Friedman was both a theorist, writing for fellow academics, and a public intellectual, addressing himself to large audiences and acting as an expert for the government. He saw it as his mission to persuade his compatriots and people all around the world of the virtues of capitalism and economic liberalism. His list of publications is record breaking, the result of both high productivity and longevity. Over the seventy years separating his first academic publication (Friedman 1935) from his last one (Friedman 2005), he published 18 technical and 14 general public books, 104 technical economics articles, 192 articles, lectures, and addresses on public policy and intended for the general public, not counting hundreds of newspaper columns.³ However, these impressive numbers should not be regarded as meaning that, like Samuelson, Friedman was a versatile economist who wrote on many different subjects. On the contrary, Friedman was a single-idea person, repeated his single idea time and again and fighting for it unremittingly – “money matters,” meaning that money can be malfeasant when badly managed by central banks.

In an interview with Taylor, Friedman indicated that his interest in monetary economics started when he was serving in the Treasury Department from 1941 to 1943 at a time when inflation had become a nagging issue with price control being advocated by many (Taylor 2001: 118).⁴ It may be presumed that he got his basic theoretical insights in his late thirties.⁵ According to Hetzel, he became a quantity theorist when realizing “that he could endow the quantity theory with predictive content by assuming that velocity was a stable variable” (Hetzel 2007: 10). As for his public policy and political philosophy views, they dated from his marriage and the influence of his brother-in-law, Aaron Director, as well as his discussion with colleagues and friends at Chicago and his attendance of the first meeting of the Mont Pelerin society in 1947 (Friedman and Friedman 1998: 333).

To give a more detailed view of Friedman's academic contributions, I propose to separate his career into three periods, the first two somewhat overlapping: period I runs from 1935 to the mid-1950s – the formative

² “In writing this note, I feel at one and the same time as if I were preaching in the wilderness and belaboring the obvious. For the major conclusions of this paper are important and widely neglected; yet they seem distressingly obvious” (Friedman [1951] 1953: 131).

³ Source: http://hoohila.stanford.edu/friedman/pdfs/116_6.pdf (accessed on October 20, 2014).

⁴ Other interesting interviews of Friedman are by Hammond (1992) and Snowdon and Vane (2005).

⁵ His first writings on inflation (e.g., Friedman 1943) were indirectly inspired by Keynes (1940); his earliest work on the macro-stabilization policy (1948) paid at least as much attention to fiscal as to monetary measures.

years – period II, from 1948 to the end of the 1960s – the creative years – and period III, from the 1970s to the first decade of the twenty-first century – a time when he devoted himself to spread the word about the monetarist approach and the virtues of the free market.

Period I

During the first period of his research career, Friedman wrote papers on statistics and microeconomics. He forged his ‘positivist’ methodology, exposed in his famous 1953 article as well as in several papers he wrote in the 1940s and 1950s. He also started working on monetary matters, as the six papers of Part II in his *Essays in Positive Economics* (Friedman 1953) attest.

I also consider that Friedman’s *A Theory of the Consumption Function* (1957) belongs to this period, a book which exerted a lasting influence. Its gist was that the Keynesian consumption function, in which consumption was a function of current income, needed to be replaced by a different function with agents’ lifetime resources, that is, wealth or permanent income, as its argument. Friedman’s novelty with respect to the existing literature was to draw a distinction between recorded income and permanent income. He argued that the latter, a non-directly observable magnitude, must be regarded as households’ effective reference when adapting their behavior. Any study based on recorded income is necessarily off the mark in as far as the transitory components of income have no impact on consumption. The book struck a first blow against Keynesian orthodoxy as it amounted to questioning the strength of the income multiplier effect. In Laidler’s words:

Friedman’s permanent income hypothesis implied that Keynes’s marginal propensity to consume and therefore the multiplier were anything but stable, and thereby provided a shaky foundation indeed for any theory that sought to explain the behavior of the macro-economy or purported to be a reliable guide to policy. The full destructiveness of this analysis was not at first widely understood, however. (Laidler 2005: 6)⁶

Period II

The second period, going from 1948 to the turn of the 1970s, was the most creative years of Friedman’s career. The triggering factor was Arthur Burns, the research director of the National Bureau of Economic Research (in short the National Bureau), who teamed Friedman up with Anna Schwartz to study monetary factors in business cycles.⁷ From 1941 onward, Schwartz had been

⁶ For a study of how Friedman’s theory of the consumption function fared from his days to the present, see Carroll (2001).

⁷ The National Bureau, founded in 1920, was dedicated to creating measurement concepts and their related time series. A landmark book in its third decade of existence was Arthur Burns and Wesley Mitchell’s, *Measuring Business Cycles* (1946).

working at the National Bureau on a series of monthly estimates of demand and time deposits and currency. Her expertise lay in her in-depth acquaintance with the available data and her knowledge of the history of American banking institutions. As for Friedman, in spite of his applied background, he was the theorist of the duo. Their research project was supposed to be implemented in three years, but it took fifteen years to complete. Its highpoint was Friedman and Schwartz's 1963 book, *A Monetary History of the United States, 1867-1960* (1963a).⁸ As underlined by Harry Johnson, it was a monumental accomplishment in every sense of the term:

... in its sheer bulk, monumental in the definitiveness of its treatment of innumerable issues, large and small, in U.S. monetary history, monumental in the consistency and coherence of its analysis of nearly a century of drastic institutional change, monumental, above all, in the theoretical and statistical effort and ingenuity that have been brought to bear on the solution of complex and subtle economic issues. (Johnson 1965: 388)

A Monetary History comprises almost no theory. After a short historical introduction, the book goes at once into data analysis starting with the Greenback period. Only at the end of the book does the reader find a definition of the notion of money stock, which is so central to their investigation, and a summary of the results obtained. However, this lack of explicit theory is misleading. As Daniel Hammond's study (1996: 53, seq.) makes it clear, Friedman had already spelled out its main hypotheses in a memento written at the onset of their research: the need to do dynamic analysis and hence give pride of place to expectations; the idea of leads, lags, and reverse causation; the decision to regard money as an asset and velocity as an element of the demand for money equation rather than as a technically determined factor; the idea that the opportunity cost of money involves a wide range of assets; and the view that business fluctuations need to be controlled and offset, yet certainly not through discretionary policy. The fact that Friedman was able to enunciate these insights before any data gathering indicates that his theoretical propositions did not spring from empirical observation but rather guided it. Moreover, the policy conclusions for which Friedman fought throughout his life pre-existed the empirical research.⁹

Friedman, alone or with a co-author, developed these insights in articles drawing from the research for *Monetary History*, several of which were published before it. The first of these papers was "The Quantity Theory of

⁸ It was followed in 1970 by a second volume providing additional data, *Monetary Statistics of the United States* (Friedman and Schwartz 1970). A further installment was published twelve years later in a new book wherein Friedman and Schwartz updated their study of the U.S. economy and extended their investigation to the United Kingdom (Friedman and Schwartz 1982).

⁹ Testifying before the Joint Committee on the Economic Report of the United States of the U.S. Congress in 1952, Friedman said that "The primary task of our monetary authorities is to promote economic stability by controlling the stock of money. ... Monetary policy should be directed exclusively toward the maintenance of a stable level of prices" (Friedman U.S. Cong. 1952; 689, quoted by Hetzel 2007: 11).

Money – A Restatement.” It was the seminal paper in a volume edited by Friedman which acted as a manifesto for monetarism, *Studies in the Quantity of Money* (1956).¹⁰ Next, came “The Supply of Money and Changes in Prices and Output” (1958) “The Demand for Money: Some Theoretical and Empirical Results” (1959), “The Lags in Effect on Monetary Policy” (1961), “Money and Business Cycles” (1963b), co-authored with Schwartz, “The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897–1958” (1963), co-authored with David Meiselman, and “Interest Rates and the Demand for Money” (1966).¹¹

Returning to Friedman and Schwartz’s *Monetary History*, by all means, its most famous part of was its Chapter 7, entitled “The Great Contraction 1929–33” and totaling about 120 pages. Friedman and Schwartz’s aim in this chapter was to dismiss the Keynesian claim that the Great Depression revealed a large-scale failure of the market system due to insufficient investment and lack of confidence. They had an alternative story to tell which they substantiated in a detailed and precise way. Its gist was that The Federal Reserve System’s restrictive monetary policy in the 1930s was dramatically wrong, precipitating rather than easing an ongoing recession. In Friedman’s words:

It [the FED] failed to exercise the responsibilities assigned to it in the Federal Reserve Act to provide liquidity to the banking system. The Great Contraction is tragic testimony to the power of monetary policy— not, as Keynes and so many of his contemporaries believed, evidence of impotence. (Friedman 1968: 166)

Such was the wealth of statistics and examination of institutional and human factors provided in the *Monetary History* that the Keynesian explanation, with its general and little substantiated character, could only pale in comparison.

The 1960s were thus highly productive years. In addition to the above publications, Friedman also wrote two important theoretical papers. The first one, “The Optimum Quantity of Money” (1969), was a contribution to the pure theory of money. In this paper, Friedman claimed that, because of an externality, agents hold a less than optimal quantity of real money balances. This led him to argue that the proper rule for the optimum quantity of money must be so that this quantity “will be attained by a rate of price deflation that makes the nominal rate of interest equal to zero” (1969: 34). This rule has been taken up in modern monetary theory under the name of ‘Friedman rule.’ To have it proposed by Friedman is odd as it runs counter to the type of policy he steadfastly recommended, namely a 3 to 5 percent annual increase in the money stock. In the final section of the article, Friedman acknowledged the contradiction without wanting to delve into it. The second paper is Friedman’s

¹⁰ The other papers in the volume were written by graduate students in his workshop, the most influential of them being Philip Cagan’s paper on the monetary dynamics of hyperinflation.

¹¹ Most of these articles were republished in Friedman’s 1969 *The Optimum Quantity of Money and Other Essays*, which thus contains the core of his theoretical contributions.

expectations augmented restatement of the Phillips curve paper (Friedman 1968), to be studied in the next chapter.

Period III

The 1970s marked the end of Friedman's most creative scientific years. For the rest of his long career, he kept publishing abundantly, but most of his production consisted in defending the monetarist vision he had developed. This was also the period when he became more active as a public intellectual. Among his many publications in defense of the free market, *Capitalism and Freedom* (1962) has probably been the most influential.

FRIEDMAN ON METHOD

Throughout his career, Friedman remained faithful to a few methodological principles which he put in writing around the turn of the 1950s. An important first piece in this respect is his essay, "The Methodology of Positive Economics," which was the first chapter in the *Essays in Positive Economics* volume (1953). Also relevant are the different articles in which he opposed the Marshallian and the Walrasian research strategies and argued in favor of the former.

The methodology of positive economics

In this hugely influential essay, Friedman developed three themes.¹² First, he expressed his basic creed that theoretical and empirical work must be interwoven. In his mind, the purpose of economics is to address concrete well-defined questions that can be settled empirically and yield valid and meaningful predictions while resisting falsification; theoretical propositions that are empirically refuted must be rejected. Friedman's standpoint thus implies a balanced blend of theory and empirics, similar to what can be found in Marshall's *Principles*.¹³ In this perspective, any 'pure theory' approach, that is, deprived of any empirical assessment, is bound to be useless. Likewise, a single theoretical framework is not something that economists should strive for, a point he made later in his response to his critics in the volume edited by Gordon, *Milton Friedman's Monetary Framework* (Gordon 1974):

¹² For an in-depth discussion of Friedman's essay, see Hirsch and de Marchi (1990). A more recent assessment is Mäki (2009).

¹³ "Two of the things that Friedman valued in Marshall were categories molded to the data of the problem at hand, and analysis that represents an appropriate level of abstraction – simple enough to manage, yet detailed enough to yield precise answers to concrete problems. Quantification is very important here. Theoretical categories must be related to measurable entities; and the ultimate aim is to get quantitative estimates of the effects of causal factors so that the analysis can be used to shape policy" (de Marchi and Hirsch 1990: 188).

On this view, there is no such thing as “the” theory, there are theories for different problems or purposes; there is nothing inconsistent or wrong about using a theory that treats the real interest rate as a constant in analyzing fluctuations in nominal income but using a theory that treats the real interest as variable in analyzing fluctuations in real income; the one theory may be most useful for one purpose, the other theory for the other. We lose generality but his procedure but gain simplicity and precision. (Friedman 1974b: 146)

The second theme is that positive and normative judgments need to be kept separate. Positive economics must be concerned with “what is” and not with “what ought to be” (Friedman 1953: 4). It must be “independent of any particular ethical position or normative judgments” (ditto). This does not mean, however, that positive economics has no policy implications. For these to prevail, the scholars involved must simply agree about the results of the positive work, whatever their personal ideological preferences.

The third theme, which became famous in its own right, is Friedman’s defense of the view that theoretical assumptions do not need to be realistic.

The relevant question to be asked about the “assumptions” of a theory, is not whether they re descriptively “realistic,” for they never are, but whether they are sufficiently good approximations for the purpose in hand. (Friedman 1953: 14–15)

One of Friedman’s examples is the assumption that firms maximize profits. Friedman readily admitted that this assumption is descriptively false, but argued that this does not matter as far as theory is concerned since competition compels firms to act in a way which is close to what is assumed in theory. Hence, theorists may reason ‘as if’ firms acted in this way. However, Friedman’s standpoint should not be misunderstood. The notion of realism can be understood in a broader way, meaning that theoretical propositions need to pertain to the real world rather than to a fictitious theoretical model. If realism is understood this way, Friedman is definitely a realistic economist. It will be seen later in the book that Lucas will defend the opposite standpoint, namely, that theoretical propositions pertain only to fictitious model economies.

Friedman on the Marshall-Walras divide

In a series of articles published in the 1940s and 1950s, Friedman argued that the Marshallian and the Walrasian approaches were opposed, claiming that the former is superior to the latter.¹⁴ Marshall was what an economist should be in Friedman’s eyes – his methodological principles described above were all put under Marshall’s patronage. In his 1949 paper, “The Marshallian Demand Curve,” Friedman devoted a full section to the Marshall-Walras divide.

¹⁴ Cf. Friedman’s reviews of Triffin’s *Monopolistic Competition and General Equilibrium* (Friedman 1941) and of Lange’s *Price Flexibility and Employment* (Friedman ([1946] 1953), his “Marshallian Demand” article (1949), his comment on Christ’s assessment of Lawrence Klein’s econometric model (1951), and, finally, his review of Jaffé’s translation of Walras’s *Éléments d’économie pure* (Friedman [1955] 1993). For a more detailed analysis of Friedman’s claim, see De Vroey (2009b).

He complained that an alteration of the role assigned to economic theory was occurring, the result of which was that “we curtsy to Marshall but walk with Walras” ([1949] 1953: 89). Thereby, “abstractness, generality, and mathematical elegance have in some measure become ends in themselves, criteria by which to judge economic theory” (Friedman [1949] 1953: 91).¹⁵ This was a tide against which he decided to fight.

Considering the time when he was writing, Friedman’s fierce attack on the Walrasian approach is surprising. Indeed, it is baffling to read that Walrasian theory dominated the field at the time.¹⁶ It may be surmised that the reason for Friedman’s hostility towards Walras’s theory was that his knowledge of it was based on the work done at the Cowles Commission located at the time at the University of Chicago, two floors from the Department of Economics. Methodologically speaking, economists at the National Bureau and at the Cowles Commission did not see eye to eye. Friedman worked for the National Bureau and supported its research strategy focusing on measurement concepts. To him, the National Bureau method was the perfect mix between theory and measurement. He surely must have strongly disagreed with Koopmans’s famous review article of Burns and Mitchell’s *Measuring Business Cycles*, with his scathing “Measurement Without Theory” title (Koopmans 1947). Cowles economists’ advocacy of a general equilibrium framework was the very line he opposed, as was their project of pursuing Tinbergen’s work and constructing structural models.¹⁷ In addition to method, ideology was also at work, since several Cowles economists had a socialist inclination. So, when studying what was going on in Chicago in the 1940s and 1950s at the level of ideology and politics, a possible shorthand answer to the question “What did Friedman mean by ‘Walrasian’? is ‘Socialism’.”¹⁸

Lurking behind the Marshall-Walras opposition was another divide, ‘small versus big models’ or ‘partial versus general equilibrium.’ This appeared clearly in Friedman’s comments on an article by Christ presented at an NBER Conference on Business Cycles and where Christ set out to test Klein’s first econometric model (Christ 1951). Friedman’s assessment was quite negative, his main

¹⁵ See also Friedman ([1946] 1953: 283) and Friedman [1949] 1953: 91–92.

¹⁶ At the time, Walras’s *Elements* had not been translated into English. The seminal neo-Walrasian works by Arrow, Debreu and McKensie had not seen the light of day yet. Only the works of Cassel and Wicksell – as well, of course, as Hicks’s *Value and Capital* – were available to those economists who could not read French yet wished to become acquainted with Walras’s work.

¹⁷ Friedman wrote a negative review of Tinbergen’s book (Friedman 1950).

¹⁸ Cf. Mirowski and Hands (1998: 268). Later, when Walras’s book became available in English, Friedman was asked to review it (Friedman [1955] 1993). He then realized that Walrasian theory did not carry the associations which Cowles Commission economists had imposed on it. Friedman found himself ready to give it due credit for having given economists a “bird’s eye-view of the economic system as a whole” and “a framework for organizing their ideas.” Nonetheless, it was time, he claimed, to return to the more serious business of “meaningful theory,” that is, adopting the Marshallian approach.

objection bearing on the mere purpose of constructing simultaneous equation models in the spirit of the Cowles Commission, pertaining to an entire economy and geared at making short-term predictions. In Friedman's eyes, Christ had shown that Klein's model was a failure. This was music to his ears. He then raised the question of what to do next:

Does it then follow that despite the unsatisfactory results to date, the appropriate procedure is to continue trying one after another of such systems [of general equilibrium] until one that works is discovered? (Friedman 1951: 112)

No, was his answer, for

... the probability that such a process [of constructing a model for the economy as a whole] will yield a meaningful result seems to me almost negligible. (Friedman 1951: 113)

Instead, his proposed line was to return to the study of industries.¹⁹

FRIEDMAN ON KEYNES

As will be seen in the next sections, Friedman's work constantly consisted in confronting monetarist propositions to Keynesian ones and resulted in asserting the superiority of the monetarist ones and hence of their policy conclusions. However, though a fierce opponent of Keynesianism, time and again Friedman expressed high praise for Keynes's work.²⁰ The following quotation illustrates the two facets of Friedman's judgment:

Keynes's heritage was twofold – to technical economics and to politics. I have no doubt that Keynes's bequest to technical economics was extremely beneficial, and that historians of economic thought will continue to regard him as one of the great economists of all time, in the direct line of succession to his famous British predecessors, Adam Smith, David Ricardo, J. S. Mill, Alfred Marshall, and W. Stanley Jevons. The situation is very different with respect to Keynes's bequest to politics, which has had far more influence on the shape of today's world than his bequest to technical economics. In particular, it has contributed substantially to the proliferation of overgrown governments, increasingly concerned with every aspect of the daily lives of their citizens. (1986: 47)

Friedman's attitude can in part be explained in terms of tactics and style of discussion but, in my view, there was more to it than that.²¹ What he declared

¹⁹ "The direction of work that seems to me to offer most hope for laying a foundation for a workable theory of change is the analysis of parts of the economy in the hope that we can find bits of order here and there and gradually combine these bits into a systematic picture of the whole. In the language of model builders, I believe our chief hope is to study the sections covered by individual structural equations separately and independently of the economy" (Friedman 1951: 114).

²⁰ Leeson (2003) is a three-volume book collecting papers on the relationship between Keynes and Friedman and their interpreters.

²¹ Much fuss has been made about Friedman's statement, which made the cover of the *Time* magazine (December 31, 1965, issue), "We are all Keynesians now," but this was a

to like above all in Keynes was that he was Marshallian. “Keynes was a true Marshallian in method” (Friedman and Schwartz 1982: 45) is a compliment coming from someone like Friedman. He had no complaints about Keynes’s way of thinking and modeling. To him, this was the right way to proceed. It was simply, he claimed, that at the end of the day, Keynes’s model was dismissed empirically, a normal occurrence in scientific work.

I believe that Keynes’ theory is the right kind of theory in its simplicity, its concentration on a few key magnitudes, its potential fruitfulness. I have been led to reject it, not on these grounds, but because I believe that it has been contradicted by evidence: its predictions have not been confirmed by experience. The failure suggests that it has not isolated what are “really” the key factors in short-run economic change. (1974b: 134)²²

Friedman’s standpoint raises two issues. First, it is sure that both Keynes and Friedman were Marshallians, yet this does not mean that they fully agreed. One striking difference is that Keynes wanted to do what I call ‘Marshallian general equilibrium’ analysis, while Friedman wanted to stick to partial equilibrium analysis. Another important difference is that, as his opposition to Tinbergen’s work makes it clear, Keynes was hardly convinced that much progress would come from empirical testing. Thus, one may wonder whether Keynes would have subscribed to Friedman’s positivist methodology. The second issue is whether we should take Friedman on his word when he asserted bluntly that Keynesian theory had been invalidated empirically. Later in the chapter, it will be seen that this statement is questionable.

THE MAIN TENETS OF MONETARISM

The many commentators of monetarism have often listed its main features. While the length of these lists differs from one author to another, there seems to be a consensus that the following four traits capture its quintessence:

- (a) “The quantity theory is in the first instance a theory of the demand for money” (Friedman 1956: 4).
 - (b) “Substantial changes in prices or nominal incomes are almost always the result of changes in the nominal supply of money” (Friedman 1987: 4).
- Two implications of this proposition are that for a large part business

misunderstanding and truncated quotation. The full passage of the quote, re-published in a later volume, runs as follows: “In one sense, we are all Keynesians now; in another, no one is a Keynesian any longer. We all use the Keynesian language and apparatus, none of us any longer accepts the initial Keynesian conclusions” (Friedman 1968: 15).

²² See also Friedman ([1989] 1997) as well as Friedman’s interview with Snowdon and Vane (Snowdon and Vane 2005).

fluctuations have a monetary origin and that inflation is always a monetary phenomenon.

(c) The private economy is stable.

(d) Money creation should be guided by a monetary growth rule.

Clearly, propositions (a) and (b), on the one hand, and (c) and (d), on the other, differ in status. Friedman proved able to cast (a) and (b) in a way which allowed them to be confronted with alternative Keynesian propositions. In each case, Friedman concluded that the winner was the monetarist proposition.²³

A rehabilitation of the quantity theory of money

Friedman's hunch was that it was possible to transform the tautological quantity of money equation into a substantive demand for money equation.²⁴ Let us use Fisher's equation of exchange expressed in terms of income as a starting point:

$$M^S V = PY,$$

where M^S is the money supply (equal to its stock), V the income velocity, P the price level and Y the real aggregate output (income). PY can be viewed as nominal income or GDP. The income velocity of money ($V = \frac{PY}{M^S}$) is the fraction of nominal income purchased by one monetary unit over a given time span. Fisher assumed that velocity and output were constant in the short period. This leads to the quantity theory of money equation, stating that changes in the quantity of money result in changes in the price level.

$$M\bar{V} = P\bar{Y}$$

The same relation can be expressed using the Cambridge cash balance equation, which has a stronger behavioral foundation:

$$M^D = kPY,$$

where k is a proportionality factor between the demand for money and nominal income.

To make sense of the notion of money market equilibrium, the supply of money and the demand for money functions must be independent. According to Friedman, the supply of money in a given country refers to a *nominal* stock of money. Friedman and Schwartz identified three determinants to this stock: high-powered money (i.e., the monetary base), the reserve ratio of commercial banks

²³ Another proposition that is often presented as a main tenet of monetarism is that no durable tradeoff exists between inflation and unemployment, my object of study in the next chapter. Friedman must be hailed, jointly with Edmund Phelps, for having enunciated it. However, in my eyes, this proposition stands up independently from monetarist theory as summarized above.

²⁴ Laidler (1997) is a classical study of the demand for money. See also Serletis (2007).

and the ratio of deposits to currency held by the public. Friedman and Schwartz in their *Monetary History* and Friedman in his theoretical work took up the long-standing premise that the supply of money is exogenous. Friedman's main interest was in giving a foundation for the demand of money, referring to the *real* quantity of money (or 'real balances') that optimizing economic agents decide to hold. In the introductory chapter of the *Studies in the Quantity of Money* (1956), Friedman posited that it is a function of a small set of variables related to the workings of the real economy. It can be expressed as follows:

$$\frac{M^D}{P} = \Phi(Y_P, k(R_B - R_M, R_E - R_M, \pi^e - R_M)),$$

where Y_P is the real permanent income, R_B (R_E) the expected nominal rate of return on bonds (equities), R_M is the expected nominal rate of return on money, and π^e the expected inflation rate.

Notice worthy, to Friedman the opportunity cost of holding money depends on a wide range of assets, including not only equities and bonds but also physical assets such as real property and durable goods (π^e acting as a proxy for the value of physical assets). He considered velocity (i.e., $1/k$) determined by these opportunity costs. The demand for real money balances is positively related to permanent income and negatively related to the yield on assets. The introduction of expectations in the demand for money equation gave it a dynamic dimension. That is, in Cagan's words, money is regarded "as a capital asset yielding a stream of particular services and dependent on permanent values of wealth income and on the interest rates" (Cagan 2008: 6).

As seen in Chapter 1, a demand for money equation is to be found in Keynes's *General Theory*. It is also a component of the IS-LM model. For Keynes, the total demand for money depended on income and a single interest rate. To him, the speculative demand for money (one of the components of the total demand for money) stood in an inverse relationship to a single variable, the rate of return of long-term bonds. When such a standpoint is taken, many factors, including expectations and rumors, come into play to determine it. As a result, the interest-elasticity of speculative demand for money can be expected to be high. In the special case of the liquidity trap, evoked by Hicks in his 1937 article, it is infinite. This makes for a high velocity of money and demand for money.

Friedman saw things differently. He deemed the rates of returns of financial assets to evolve in parallel to interest rates, which makes the interest-elasticity of the demand of money very low. With both the permanent income and volatility being stable, the conclusion to be drawn is that the demand for money is stable as well. There is thus a sharp contrast between the Keynesian and the monetarist propositions. According to Friedman, it was up to empirical work to assess which of the two theoretical propositions about the demand for money equation is correct. His contention was that empirical testing revealed the superiority of the monetarist proposition over the Keynesian one.

There is an extraordinary empirical stability and regularity to such magnitudes as the income velocity that cannot but impress anyone who works extensively with monetary data. (Friedman 1956: 21)²⁵

Much was at stake in this confrontation. If the monetarist claim is confirmed, the quantity equation according to which a change in the nominal money supply results in a predictable change in the price level is also vindicated. A stable velocity has also a straightforward policy implication. In De Long's words:

The money stock became a sufficient statistic for forecasting nominal demand, and central bankers could close their eyes to all economic statistics save monetary aggregates alone. (De Long 2000: 91)

Finally, a stable demand for money permits the rehabilitation of the classical dichotomy – a watertight separation between price theory and monetary theory. By contrast, were this stability invalidated, the classical dichotomy would no longer hold.

The causal role of the supply of money

Central to Friedman's vision is the existence of a causal link between changes in money supply and changes in nominal income both for secular changes and business cycle-type changes (Friedman and Schwartz 1963b: 53). To Friedman, this causal link from money to nominal income was one of the best established economic relationships:

There is perhaps no empirical regularity among economic phenomena that is based on so much evidence for so wide a range of circumstances as the connection between substantial changes in the stock of money and in the level of prices . . . Instances in which prices and the stock of money have moved together are recorded for many centuries of history, for countries in every part of the globe, and for a wide diversity of monetary arrangements. (Friedman ([1958] 1969: 172–173)

In terms of the Cambridge equation, this causal link implies, first, that k (or its inverse, velocity) is stable – the point made earlier – and, second, that real output is determined autonomously from money supply. Somewhat oddly in view of Friedman's staunch opposition to Walrasian theory, he had no qualms stating that real output was determined through the Walrasian system of equations, without giving any further explanation.

²⁵ In his "Demand for Money" paper (Friedman [1959] 1969), Friedman declared that the money demand was totally interest-inelastic. Later, he stepped back from this conclusion, writing that he saw "no fundamental issue in either monetary theory or monetary policy hinge on whether the estimated elasticity can for most purposes be approximated by zero or is better approximated by -1 or -1.5 or -2.0 , provided it is seldom approximated by $-\infty$ " (Friedman [1966] 1959: 155).

The most contentious part of this causality claim concerns the short period. Here, two points must be noticed. The first is that, when Friedman discussed short-period monetary changes, he implicitly assumed that the changes in money supply were of a disturbing nature with respect to the functioning of the real economy. Second, from the empirical side, the study of their impact is compounded by the need of integrating the duration factor. According to Friedman, the impact exerted by money on nominal income was deemed to be lagged rather than immediate with the length of the lags varying according to circumstances.

The first paper where Friedman tried to show the existence of a long and variable time lag in a series for changes in M_2 (currency plus time and saving deposits of commercial banks; M_1 consists of currency and checkable deposits) and business conditions was written for the U.S. Congress's Joint Economic Committee (Friedman [1958] 1969). Though admitting that causation was less clear-cut and more complex for the short period than for long movements, Friedman nonetheless concluded in favor of a "strong though not conclusive evidence for the independent influence of monetary changes" ([1958] 1969: 180). His main argument was that, when comparing the timing of the monetary change and the occurrence of peaks or troughs in income and prices, the former turned out to precede the latter by a long interval:

On the average, the rate of change of the money supply has reached its peak nearly 16 months before the peak in general business and has reached its trough over 12 months before the trough in general business. (Friedman [1958] 1969: 186)

A monetary explanation of business fluctuations

This observation led Friedman and Schwartz to argue in their "Money and Business Cycles" article (1963b) that the regularities observed in their historical study of the U.S. economy allowed the rehabilitation of a monetary explanation of business fluctuations, the view that prevailed before the ascent of Keynesian theory. In their *Monetary History*, they indeed established that an absolute decline in the money stock accompanied all the strong depressions that had occurred in U.S. history, that is, 1875–1878, 1892–1894, 1907–1908, 1920–1921, 1929–1933, and 1937–1938. Although the lags differed from one cycle to the other, there were nonetheless limits to their variability. Hence the possibility of drawing a general picture. It runs as follows. After a change in the growth rate of the money stock, it takes on average between six to nine months for an effect to manifest itself. The first effect concerns output rather than prices. Six to nine months later, an effect on prices appears. However, the predominance of the output effect may last as long as five to ten years. Over decades, the effect is mainly on prices. According to Friedman, the variability of the lags had the policy implication that fine tuning – frequent discretionary

changes in money supply aiming at keeping the economy on course – would be ineffective, exacerbating recessions instead of offsetting them.

As was the case for the demand for money, Friedman did not content himself with documenting that the monetarist claim fitted the data. He wanted to do more than that, namely engage in a confrontation with Keynesian theory. This led to a joint paper with David Meiselman, “The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897–1958” (Friedman and Meiselman 1963). Using simple linear regressions, they compared two simplified visions of the economy, the monetarist demand for money equation and the Keynesian income-expenditure model, for the 1897–1958 period. The monetarist model consisted in regressing consumption on the money supply, the Keynesian model of regressing it on autonomous investment. Friedman and Meiselman concluded that for every decade studied, the money supply had a better fit and hence provided a more convincing explanation. This paper, “the single most influential study among Friedman’s many publications,” (Thygesen (1977: 75), created a stir. It triggered strong reactions and valid refutations, yet all in all it did more to forward the monetarist cause than to discredit it.

A theory of inflation

For a large part, monetarism made its way both in the profession and in the wider public because of its concern for inflation. Johnson has a nice explanation for this:

The General Theory was successful, precisely because, by providing an alternative theory to the prevailing orthodoxy, it rationalized a sensible policy that had hitherto been resisted on purely dogmatic grounds. Similarly, the monetarist counter-revolution has ultimately been successful because it has encountered a policy problem – inflation – for which the prevailing orthodoxy has been able to prescribe only policies of proven or presumptive incompetence, in the form of income or guideline policy, but for which the monetarist-counter-revolution has both a theory and a policy conclusion. (Johnson 1971: 12)

“Inflation, Friedman wrote, is always and everywhere a monetary phenomenon that can be produced only by a more rapid increase in the quantity of money than in output” (Friedman, 1970a: 24). This was hardly the viewpoint that prevailed in the 1950s and 1960s.²⁶

²⁶ In their 1963 survey of inflation, Bronfenbrenner and Holzman put cost-push, demand-pull and monetary explanations on the same footing (while expressing more sympathy for the first two ones). One example, among many others, of the pre-monetarist viewpoint on inflation is a paper by Ackley (Ackley 1959). Laidler and Parkin (1975) is an authoritative survey on inflation and monetarism.

The defense of a monetary growth rule

Throughout his career, Friedman fought against giving discretionary power to monetary authorities. Instead, he advocated the constitutional decision of setting up a constant growth of the money stock rule which central banks would have to comply with. He estimated that it should be something like 4 to 5 percent per year for the United States for M₂.

A belief in the stability of the economy

It was a shared belief among monetarists that "the dynamics of the private sector is basically very stable" (Brunner 1970: 6).²⁷ In Mayer's words:

In general monetarists appear to be much more satisfied with outcome of market processes than the Keynesians are. There is, of course, no way of proving that this attitude is should be considered a component of monetarism rather than a characteristic which those economists who are monetarists happen to have for extraneous reasons. However, a dislike of government regulations fits very well with most of the previously discussed components of monetarism. Thus, a belief in the quantity theory implies that there should be no counter-cyclical fiscal policy. . . . If the private sector is inherently stable no countercyclical policy may be needed or be desirable. (Mayer 1978: 38)

Although Mayer's statement has the merit of being candid, I cannot but agree with Frisch's comment on Mayer declaring that it is a presupposition rather than a positivist statement (Frisch 1978: 122).

THE KEYNESIAN-MONETARIST DEBATE

My aim in this section is to focus on the bones of contention between monetarists and Keynesians. One way of implementing such a project is to survey the different papers written by Keynesian economists to debunk the monetarist contentions. They were numerous; if I had I to mention two of them that stand out, they would be Albert Ando and Modigliani (1965) and James Tobin (1970). Another important piece was Temin's book-length criticism of Friedman and Schwartz's interpretation of the Great Depression (Temin 1976). However, instead of taking this line, I have opted for basing my analysis on a single confrontation, opposing Modigliani and Friedman in an exchange that took place in January 1977 at the Federal Reserve Bank of San Francisco. Modigliani had been invited to present a seminar on the themes he had developed in his December 1976 Presidential Address to the American Economic Association entitled "The Monetarist Controversy or, Should We Forsake Stabilization Policies"? (Modigliani 1977a). Friedman, who was visiting the Bank, was invited to be Modigliani's discussant. The transcripts of his comments and Modigliani's

²⁷ See also Laidler (1991: 639).

responses have been published as a Supplement to the Bank's *Economic Review* (Federal Reserve Bank of San Francisco 1977; henceforth FRB SF 1977).²⁸ It was a lively and instructive debate in which neither of the speakers pulled his punches. Friedman is known to have been a fierce debater. The discussion showed that this was also true for Modigliani.

Studying this exchange allows me to bring out what I regard as the three main bones of contention between the two approaches. They bear on stability, the short-period money non-neutrality, and the discretion-versus-rules divergence. To set the scene, I begin with summarizing the views Modigliani expressed in his Presidential Address.

Stability

Modigliani's 1965 paper, co-authored with Ando, was strongly dismissive. Ten years later, in his Presidential Address, Modigliani adopted a more conciliatory tone:

Milton Friedman was once quoted as saying, "We are all Keynesians, now," and I am quite prepared to reciprocate that "we are all monetarists" – if by monetarism is meant assigning to the stock of money a major role in determining output and prices. (Modigliani 1977a: 1)

A possible explanation for this change in accent is that, between the two dates, the landscape had significantly changed. The 1970s were the heyday of the monetarist counter-revolution (De Long 2000: 84). Despite Keynesians' responses to Friedman and his co-authors' claims, monetarism was on the move, not only in the profession but also in the political sphere. In 1979 the FED started what was called its 'monetarist experience' and, under the Thatcher government, the United Kingdom followed suit. On a more theoretical front, Friedman's American Economic Association Presidential Address paper published in 1968, which I will study in the next chapter, had struck a blow against the Phillips curve. Moreover, it must be remembered that the monetary dimension was an important element of Modigliani's version of the IS-LM model. So, he was closer to monetarism than most other Keynesian economists.²⁹

In his Address, Modigliani took stock of these changes. He acknowledged that several of Friedman's criticisms of "early, simple minded Keynesianism" were to a large extent valid, namely that (a) the interest elasticity of the demand for money is modest, (b) the consumption function should be based on the

²⁸ No transcript of Modigliani's presentation seems to be available. However, the Supplement includes his Presidential Address. In view of the closeness between the Address and the Seminar, we can safely assume that no dissimilarities in content must have been present. This is also visible when looking at Friedman's comments; they were congruent to the content of the Address.

²⁹ What Friedman gladly recognized in his San Francisco comment on Modigliani: "I may say that I've always thought that Franco, insofar as you use these terms, has always been a monetarist, in very important ways. His famous 1944 paper certainly qualifies as a major element in the so-called monetarist structure" (FRB SF 1977: 12).

permanent income hypothesis (which was close to his own Grunberg-Modigliani hypothesis), and (c) interest rates exert pervasive effects not only on investment but also on the demand for goods. Nor did Modigliani object to the view that the quantity theory of money was verified in the long period. Nonetheless, in his view, there remained an essential difference:

Non-Monetarists accept what I regard to be the fundamental practical message of *The General Theory*: that a private enterprise economy using an intangible money *needs* to be stabilized, *can* be stabilized, and therefore *should* be stabilized by appropriate monetary and fiscal policies. Monetarists by contrast take the view that there is no serious need to stabilize the economy. (Modigliani 1977a: 1)

Stabilization, Modigliani argued, was a protracted process, having “more nearly the character of a crawl than of a gallop” (Modigliani 1977a: 8), particularly for what concerned employment. For Modigliani, the basic project of Keynesian theory was to make the “case for counteracting lasting demand disturbances through stabilization policies rather than by relying on the slow process of wage adjustment to do the job, at the cost of protracted unemployment and instability of prices” (Modigliani 1977a: 3). By taking this standpoint, it must be noticed that Modigliani departed from the contention made in his 1944 article, namely, that the economy experienced a state of underemployment equilibrium, adhering instead to Patinkin’s disequilibrium line – what shows how pervasive Patinkin’s influence has been.

According to Modigliani, the monetarist standpoint would be valid if it were shown that phases of stable monetary growth would translate into a stability in real income, and vice-versa. To test this hypothesis, Modigliani looked at two periods during which monetary growth was fairly smooth, the first one extending from the beginning of 1953 to the first half of 1957, the second one going from the first quarter of 1971 to the first quarter of 1975. The first exhibited a rather low rate of money growth, the second a high one, about 7 percent. According to monetarist theory and taking into account a one-year lag, one should expect a similar steady evolution of output. Modigliani argued that this was not the case. Two conclusions ensued. The first is that the instability observed in the United States in the last three decades could not be ascribed to monetary instability. The second is that the stability of the money supply is no guarantee for a stable economy (Modigliani 1977a: 13).

This sets the scene. Let me now turn to the San Francisco exchange and Friedman’s reactions to Modigliani’s views. As one may expect, they were strong. He expressed his surprise at Modigliani’s assertion that 1953–1957 and 1971–75 were periods of stable monetary growth. Providing charts about M1 and M2 in support of his view, he argued that they manifested to no stability at all; if there was one period that did, it was the 1961–66 one. So, in no way did he feel that the monetarist contention had been invalidated.

When facing clashing propositions, both supposedly empirically grounded, the most plausible explanation is that they are based on different measurement

choices. This was indeed the case: Modigliani chose to measure M_1 , Friedman M_2 ; Modigliani looked at the growth of money supply, Friedman at the rate of change of this growth; Modigliani took a one-year lag, Friedman a two-year one. The following excerpt gives a flavor of the liveliness of their exchange:

MODIGLIANI: Is that 1961–65 a stable period? Do you realize that M_1 went from 1½ percent up to 7 percent?

FRIEDMAN: Of course; but they were proceeding along a steady path.

MODIGLIANI: But what's the difference? You are talking about the second derivative now.

FRIEDMAN: Of course I am.

MODIGLIANI: Ah, the second derivative counts. Well, that's a new theory.

FRIEDMAN: Excuse me, it is not a new theory. We both agree that what matters is the difference between anticipations and realizations. And what we are talking about are the deviations from anticipated rates of growth.

MODIGLIANI: And you mean, in this period everybody anticipated that there would be acceleration, acceleration, acceleration? (FRB SF: 15).

The short-run real effects on changes in money supply

Later in the discussion, the topic of interpreting the 1972–75 period came up again. Answering Friedman, Modigliani insisted that the point he wanted to make about this period was that it featured a concomitant stable money growth and strong real instability. In other words, what one should look at is the correlation between money supply and real income rather than that between money supply and nominal income as Friedman did. As this point is important, it is worth quoting their exchange at length:

MODIGLIANI: In the period 1972–75 there was great instability, which is disguised when you plot money income. Prices were rising like mad. Then the whole problem was that the Federal Reserve wasn't providing enough money, and so, naturally, money income didn't change in the face of prices rising by 12 percent. So if, instead of taking money income, you take real income – which is what I measured – you will see the great instability. And I'm surprised that we need to discuss it, because you've all lived it. So do you believe everything was rosy and stable between 1972 and 1975? If you believe it, then I think you'd better go back to school.

FRIEDMAN: But one of the things, Franco, that I thought you and I agreed on, and that I have written on extensively, is that we know much more about the nexus between money and money income, or nominal income, than we do about the forces that cause the division between prices and output.

MODIGLIANI: This happens to be a point of complete disagreement. I believe we know equally much about both issues. . . . But we do know that wages respond to unemployment, and past inflation, with fair regularity – with the coefficient of past inflation not very far from one. There is an extensive literature that explains why that would be the case. This is perhaps the difference between a monetarist and a non-monetarist, in the sense that, if you start from LM and IS, a non-monetarist will

stress real output and will derive money output as a result. Monetarists, instead, tend to go directly to money income – and I think that is misleading. Although in many cases it would be all right, 1973–74 is not one of those periods. (FRB SF 1977: 20–21).

We touch here on what I believe is the central difference between Keynesians and monetarists. It concerns the short period effect of monetary changes, that is, whether their impact is exclusively exerted on the price level or whether it also affects the level of activity. As seen in the quote, Friedman made an admission of ignorance. In other papers, he mentioned “unfinished business” (Friedman 1974a: 40; Friedman and Schwartz (1982: 26). Here and elsewhere, he applied this judgment to both the monetarist and the Keynesian approaches, presenting this unfinished business as a defect which they had in common (Friedman 1974a: 44–5). Friedman was right in asserting that monetarists had skipped the issue, although there is one exception, which I would have expected him to mention, namely, the argumentation he developed in his Phillips curve paper.³⁰ But as Modigliani’s reaction attests, Keynesians could not accept this judgment as far as their program was concerned. From the beginning, they (especially those who followed Modigliani’s line) had strived to study the conditions under which monetary changes are non-neutral – on the theoretical level, using the Keynesian sub-variant of the IS-LM model, on the empirical level, using their structural econometric models. Thus, Friedman’s standpoint amounted to judging that all of the work bestowed by Keynes on the issue was of no more weight than the monetarists’ scant attention to it. It was a small wonder then that Modigliani was outraged.

Rules versus discretion

For all its importance, this last difference calls for fewer comments. The Friedman-Modigliani exchange is interesting because it brings out that, at least in the case of these two protagonists, the vindication of their respective standpoints had little to do with theory. Friedman claimed that the reason why he favored rules was his mistrust of political interferences with economic decisions: whenever individual discretionary power is given to individuals (central bankers), even if economists could agree that the policy choice made is commendable, political forces are likely to come into play and impede the result aimed at to occur (Friedman FRB SF 1977: 19). Hence the need, he claimed, to have some constitutional provision allowing the

³⁰ Another exception is the work done at the Saint Louis Federal Reserve Bank (Andersen and Jordan 1968), but it is usually admitted that their research was inconclusive.

avoidance of political obstruction.³¹ To this, Modigliani retorted that there were plenty of qualified people in government (and in the Council of Economic Advisers!). As for the FED, he complained that it had no explicit targets or did not say what they were. This needed to be changed so that its behavior could be judged on the grounds of their achievement. But this was quite different from imposing a monetary-growth rule of the type advocated by Friedman.³²

THE LIMITATION OF THE MONETARIST CHALLENGE

Keynesians' reactions to Friedman's work were not confined to retorting to his empirical contentions. They also pressed Friedman to make the wider theoretical framework to which monetarist propositions belonged explicit. He yielded to their demand in two *Journal of Political Economy* articles (Friedman 1970b, 1971). They were published in a slightly enriched version in a volume edited by Robert Gordon and entitled *Milton Friedman's Monetary Framework* (Friedman 1974a). Friedman's paper was followed by reactions written by Brunner and Meltzer, Tobin, Paul Davidson, and Patinkin. The book ends with Friedman's rejoinder (Friedman 1974b).³³

In the first twenty-eight pages of his paper, Friedman gave a systematic exposition of the rehabilitated quantity theory of money. He also presented his interpretation of Keynes's contribution to monetary theory, an exercise that I find at the same time penetrating and somewhat biased. After these first skirmishes, he finally came to the task expected from him setting out to construct a "highly simplified aggregate model of an economy that encompasses both simplified quantity theory and a simplified income-expenditure theory as special cases" (Friedman 1974a: 29). As his model comprised seven variables (nominal income, consumption, investment, the nominal interest rate, the price level, the demand for money, and the supply of money) and only six equations, one variable needed to be determined outside the system.³⁴

³¹ "Friedman: I have increasingly moved to the position that the real argument for a steady rate of monetary growth is at least as much political as it is economic; that it is a way of having a constitutional provision to set monetary policy which is not open to this kind of political objection" (FRB SF 1977: 18).

³² "Modigliani: I complain that the Federal Reserve does not tell us its target. Why do I complain? Because I have no way of telling if it is doing a good job or not. That's why I want them to tell us what their targets are – and not necessarily money targets. I don't care about money targets. They can do anything with money, as long as they tell us what their real targets are – and as long as they take the blame when they do not hit the real targets. Now that seems to me to be the fundamental issue" (FRB SF 1977: 21–22).

³³ I will not comment on the discussion between Friedman and the other participants because I find it disappointing (except that between Friedman and Brunner and Meltzer) and certainly less interesting than the Friedman-Modigliani exchange.

³⁴ Friedman did not consider it necessary to introduce labor market variables, the benchmark differentiating the classical and the Keynesian cases in the IS-LM model.

Friedman argued that this was where the difference between Keynesians and monetarist lay. The 'quantity theory specialization,' as he called it, consists in positing that real income is determined outside the system:

It appends to the system the Walrasian equations of general equilibrium, regards them as independent of these equations defining the aggregates and as giving the value of Y/P [real income]. (Friedman 1974a: 31-32)

The 'income-expenditure specialization' considers the level of prices as an institutional datum.³⁵ Friedman showed that the quantity equation ($MV = PY$) can easily be derived from the first specialization. Likewise, once the income-expenditure specialization is adopted, the system of equations can easily be recast in terms of the *IS-LM* model.

Constructing an abstract model of the working of the economy was not Friedman's preferred topic – actually, as seen earlier, it went against his methodological principles. He addressed it just to meet other economists' request. It remains that when doing it, he fell back on a construct akin to the classical sub-variant of the *IS-LM* model. Thereby, he implicitly admitted that, as far as the analysis of the economy as a whole is concerned, monetarism is embedded in no specific framework different from the Keynesian one.³⁶

This observation allows me to address the further question of whether monetarism could have succeeded in overthrowing the Keynesian paradigm. I do not believe so. The problem goes back to Friedman's positivist methodology. Macroeconomics requires a general equilibrium framework – a simplified one, definitely, but a general equilibrium one nonetheless. This was the case for Keynesian macroeconomics even if it was hardly rock-solid general equilibrium analysis.³⁷ For his part, Friedman refused to engage in this type of analysis. To him, constructing small-scale modeling, limited to a few propositions, was what needed to be done. Whatever the intrinsic validity of such a standpoint, adopting it impeded any all-out attack against Keynesian theory for it takes a general equilibrium theory to dismiss another general equilibrium theory. Global theories, are dismissed because of a methodological criticism of their foundations rather than the invalidation of some of their particular theoretical propositions.

³⁵ In note 20, p. 32, Friedman made it clear that he was aware that this assumption of taking prices rather than wages as rigid departed from Keynesian theory but he considered it a useful simplification, which he added has been widely used.

³⁶ Other monetarist, in particular Brunner and Meltzer strongly disagreed with Friedman on this point. See, for example, Brunner and Meltzer (1993).

³⁷ Keynesians did not regard their theory this way because they took it for granted that 'Walrasian general equilibrium' was the only way of doing general equilibrium analysis, not realizing that what they were doing was 'Marshallian general equilibrium.' Time and again, the *IS-LM* has been described as Walrasian because of this automatic association. I will show in Chapter 18 that this view does not stand up to closer scrutiny.

These considerations cast doubt on Friedman's bold appraisal of Keynes's work mentioned above and according to which Keynes's theory, praised as a potentially great one, happened to be disqualified by empirical evidence. This was true just for particular Keynesian propositions, not for the whole construct.

An economist who rightly perceived this point was Johnson. In his *Monetarist Counter-Revolution* paper, he declared that monetarism suffered from two main flaws: its "abnegation from the responsibility of providing a theory of the determination of prices" (i.e., the allocation of the effects of monetary changes between output and prices) and its continuous reliance on the methodology of positive economics (Johnson 1971: 10).³⁸ Hence, in his eyes, the best outcome that monetarist could have hoped for was a Keynesian-monetarist synthesis – a far cry from a radical dismissal of the Keynesian paradigm.

FRIEDMAN ON THE RELATIONSHIP BETWEEN THEORY AND IDEOLOGY

Central to the positivistic method is the idea that ideological vision and positive work can and must undergo a watertight separation. My aim in this section is twofold. First, I want to take the opportunity offered by such a standpoint to reflect on the place of ideology in a field like macroeconomics. Second, I will show that doubts can be cast on whether Friedman's work attests to his declared principle.

General observations

The term 'ideology' often carries a pejorative meaning, referring to arguments imbued with bad faith or based on the defense of particular interests. I prefer to regard it as pertaining to the vision held about the ideal way of organizing a society in its economic dimension, and thus, for our discussion, about capitalism. The ideological issue then boils down to having a specific vision about the respective role of government and market forces in production and distribution. At stake here is a matter of degree rather a dual division. Yet, for the present discussion, I will content myself with a polar opposition between Friedman's free trade point of view and the Keynesian vision.

Friedman did not want this dimension to interfere with scientific work, a commendable wish. The problem when it comes to macroeconomics is that it is

³⁸ "The methodology of positive economics was an ideal methodology for justifying work that produced apparently surprising results without feeling obliged to explain just why they occurred, and in so doing mystifying and exciting the curiosity of noncommitted economists and wavering Keynesians. But the general equilibrium and empirical revolutions of the recent past have taught economists to ask for explicit specification of the full general equilibrium systems with which the theorist or empiricist is working and to distrust results that appear like rabbits out of a conjurer's hat – and an old-fashioned hat at that" (Johnson 1971: 13).

geared towards producing policy conclusions, and such conclusions necessarily support a particular ideological vision, to simplify either the free market solution or the Keynesian one.

In this perspective, if the ideological dimension cannot be erased, what must be done is to circumscribe its impact. That is, the profession should strive for a situation where it would not matter whether a given argument is underpinned by an ideological motivation because methodological criteria exist to gauge its validity whatever this motivation (assuming there is one which is not necessarily the case). The use of mathematical modeling as opposed to modeling in prose is a first step in this direction. Yet, more is needed, namely, making the view adopted on the matter of the relationship of models and reality explicit, defining guidelines for econometric work, and, finally, enunciating the standards to be respected for sound theory construction. All these choices are definitely a matter of convention and may always be impugned through a scientific revolution. At the time of the debates between Keynesians and monetarists, these standards were hardly rigorously set. This explains the impression one gets when returning to them with hindsight and seeing Keynesians and anti-monetarists hurling alternative measurement alternatives (M_1 , M_2 , and lags of different lengths) at one another in a bid to prove the other side wrong. In this perspective, once standards get more explicitly defined, as will be the case after the Lucasian revolution, having some economists who drive their theoretical work in such a way that the conclusions of their models support their ideological vision becomes hardly dramatic since what counts is abiding by the standards.

While I find nothing to blame in the possibility that economists carry an ideology that may impinge on their theoretical work, it would be better to have them admitting to such a motivation; admittedly, in a profession dominated by the positivistic creed, it is hard to take such a stance. If nobody comes out with such a motivation, can an outside observer nonetheless detect signs of it? As it is impossible to read people's minds, the presence of a political agenda is hard to establish. However, its absence is easier to detect. One sign of it is a contradiction between an economist's explicitly espoused ideological vision and the conclusion of her model. Patinkin is a case in point. It is hard to argue that his endorsement of the real-balance effect was politically motivated since it led to an important trimming of the scope of the paradigm he defended, Keynesian theory.³⁹ A similar conclusion arises when looking at the range of models constructed by a given economist. If it turns out that some of them lean toward a Keynesian conclusion and others toward a laissez-faire one, one may conclude that this economist is not pursuing a political agenda.

A final issue to be addressed is whether there is an inherent link between a given conceptual apparatus and the support of either free market or

³⁹ Cf. Rubin (2005).

Keynesianism. My answer is no. Friedman's argumentation in his Monetary Framework paper leads to the conclusion that he was Keynesian on the methodological score! Later in the book, other examples will surface. For example, so-called non-Walrasian equilibrium models, to be studied in Chapter 7, are Keynesian as far as the vision is concerned, yet non-Keynesian for what concerns the conceptual apparatus.

Friedman

Few economists were more open about their ideological commitment than Friedman. To him, markets do the best possible job of allocating resources efficiently.⁴⁰ However, he claimed that his defense of free markets did not run counter to the positivist method. When arguing that economics should evolve into a positive discipline, this is exactly what he meant: ideology and any normative judgment should be left aside. In the early 1990s, he was invited to write a postscript for a book on monetarism, the main tone of which was critical. In these afterthoughts, he flatly denied that his theoretical work was underpinned by a strategy of "encirclement" of Keynesian theory as his critics asserted. Instead, he presented himself as having a schizophrenic mind-set:

During my whole career, I have considered myself somewhat of a schizophrenic, which might be a universal characteristic. On the one hand, I was interested in science *qua* science, and I have tried – successfully I hope – not to let my ideological viewpoints contaminate my scientific work. On the other, I feel deeply concerned with the course of events and I wanted to influence them so as to enhance human freedom. Luckily, these two aspects of my interests appeared to me as perfectly compatible. (Friedman 1993: 1; own translation)⁴¹

I find this quotation as revealing as a slip of the tongue sometimes is. In my opinion, Friedman was anything but schizophrenic. Schizophrenia would have required that his theoretical conclusions be poles apart from his ideological views, which in his case never happened. From the 1950s onward, all his work consisted in proposing alternatives to established Keynesian policies, trying to substantiate them empirically and to draw policy conclusions that always went against Keynesian recommendations. This exceptionally strong coincidence between vision and positive work is in my view a good reason for being skeptical toward Friedman's declarations of success in keeping ideology away from his positive work.⁴² Let me repeat that I am of the opinion that no fuss

⁴⁰ Friedman admitted that they can fail but argued that any attempts by governments to remedy on these failures would only aggravate them. See his interview by Snowdon and Vane (2005).

⁴¹ He reiterated this claim in an interview by Taylor: "I believe that I can honestly say that I never reached a judgment about monetary or fiscal policy because of my belief in the markets. I believe that the empirical work is independent and honest in this sense" (Friedman interviewed by Taylor, (2001: 120).

⁴² This claim is more systematically substantiated in Cherrier (2011).

should be made about these interferences. There is no reason for stigma. Still, the record must be set straight: in my view, Friedman, his refusal to admit it notwithstanding, pursued a political agenda when engaged in theoretical work. No better witness than Milton's wife, Rosa Friedman, can be invoked in defense of my viewpoint. Indeed, she wrote:

[I find it possible] to predict an economist's positive view from my knowledge of his political orientation, and I have never been able to persuade myself that the political orientation was the consequence of the positive views. (Friedman Rose 1976: 30)

THE FALL OF MONETARISM

Although the 1970s were the heyday of monetarism, the next decade saw its dismissal. Two reasons explain this. The first is factual: from 1982 onward, the stability of the demand for money, the cornerstone of the monetarist approach, ceased to be empirically verified. The second was the ascent of DSGE macroeconomics that led to the joint dismissal of the two earlier rival approaches, Keynesian macroeconomics and monetarism. I will deal with them in turn.

Earlier on, I gave quotes from Friedman in which he stated that he knew of no better empirically established propositions than those pertaining, firstly, to the stability of the demand for money proposition, itself dependent on a stable velocity of money, and, secondly, to the connection between the quantity of money and nominal income. These two propositions were the pillars of monetarism. We have also seen that Friedman disliked grand theoretical constructs. Instead, he favored small-scale theories consisting of a few theoretical propositions drawing their strength from their resistance to refutation and their empirical superiority with respect to rival ones. Such a methodological standpoint may be alluring, yet it is also risky because of its win-or-lose character. If, for whatever reason, the core propositions happen to be refuted, the theory they sustained collapses altogether. This happened to the monetarist approach in the 1980s.

The collapse occurred three years after an event that, by contrast, sounded like one more victory for the monetarist doctrine. In October 1979, the Federal Reserve System decided to adopt a new policy line consisting in reducing the growth of the money stock and placing less emphasis on interest rates. The underlying motivation was to act on the high, increasing rate of inflation.⁴³ It was hard not to regard the Federal Reserve's decision as a test of the validity of Friedman's ideas. The new policy led to some success as the money growth rate was indeed reduced, though it was accompanied by an increase in its volatility. The flip side, however, was a deepening recession, high unemployment and a strained financial system. These factors led the FED to abruptly stop the new policy in the summer of 1992. Such an experience cried out for assessment. At the

⁴³ Over the year 1979, the consumer price index increased by close to 8 percent.

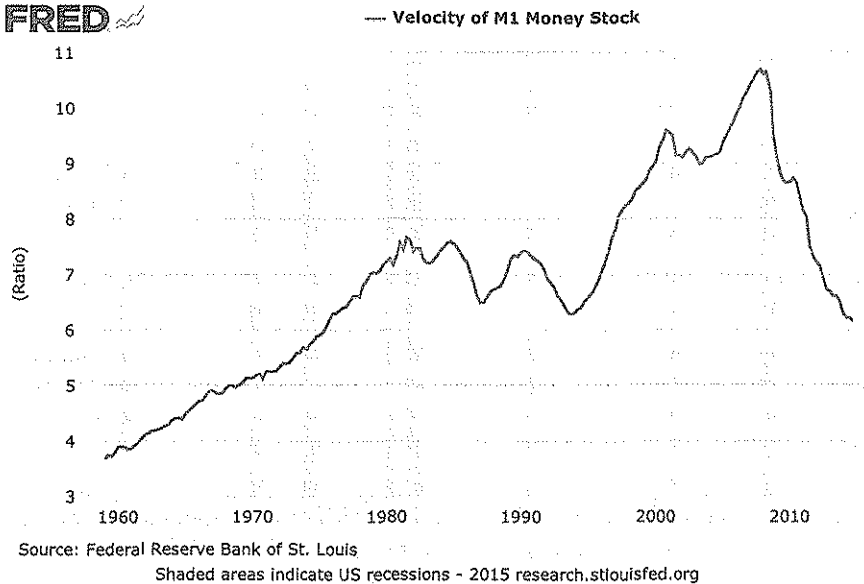


FIGURE 4.1 Velocity in the US Economy.⁴⁵

AEA 1983 meeting, a session was organized aiming at addressing the issue. Two of its participants, Milton Friedman (Friedman M. 1984) and Bennet McCallum (1984) took it at heart to protest against any claim that the policy had been genuine monetarism. Without endorsing this claim explicitly, a third participant, Benjamin Friedman, took the opportunity to point out a series of ambiguities of monetarism (Friedman B. 1984).⁴⁴ For my purpose here it is unnecessary to try to separate the wheat from the chaff. My viewpoint is that, though the Federal Reserve experience did no good for monetarist theory, the outcome of the experience was hardly clear-cut enough to strike a lethal blow against the theory. Yet it was already nearing its end at the time of the AEA session. The death knell came from the surprising occurrence that, after decades of stability, in the second quarter of 1982, the statistics revealed sharp swings in the velocity of money (or its reverse, the ratio of money to GNP). Figure 4.1 illustrates this.

A few years after the AEA session, a new factual configuration was indeed widely acknowledged. Here is how two eminent monetary economists – William Poole, a monetarist and the aforementioned Benjamin Friedman, a non-monetarist – assessed the situation in a 1988 issue of *The Journal of Economic Perspectives*.

⁴⁴ A fourth participant, James Pierce, examined the role that financial innovations exerted on the outcome of the FED's experiment.

⁴⁵ Federal Reserve Bank of St. Louis, *Velocity of M1 Money Stock [M1V]*, retrieved from FRED, Federal Reserve Bank of St. Louis <https://research.stlouisfed.org/fred2/series/M1V/>, May 29, 2015.

By 1975, most economists agreed that the money demand function in the United States was reasonably stable and could serve as a reliable basis for the formulation of monetary policy. Those suspicious of monetarism were on the defensive in the light of the apparently inexorable increase of M1 velocity of about three percent per year with deviations of only a few tenths of a percent. Experience with rising money growth and rising inflation through 1980 only confirmed monetarist views. All this had changed by 1986. With disinflation in the 1980s, M1 velocity departed convincingly from its 1953–79 trend. The money demand function seems to have fallen apart and is apparently not a reliable basis for monetary policy after all. (Poole 1988: 73)

Bad news never travels alone. A similar negative result arose for the connection between money and prices. Here is how B. Friedman put it:

The double-digit average growth rate [of M1] maintained for five years following mid-1982 represents the most rapid sustained money growth the United States has experienced since World War II, yet these same years also saw the strongest sustained deceleration of prices in the postwar period. (Friedman, B. 1990: 59)

In the beginning, the departure from the monetarist propositions might have been thought of to be temporary and accidental. For example, in his American Economic Association Meetings paper, Friedman (Milton) judged that the decline in inflation from the 1979–81 period to the 1981–83 period was significantly greater than what one would have expected from the decline in monetary growth (Friedman 1984: 399). While he safely forecasted that inflation in 1983–85 would be higher than in 1981–83, the contrary happened.

It did not take long for economists to realize that the empirical basis of monetarism that Friedman characterized as rock solid simply no longer was. The result is that monetarism was led to stumble on the very principle that Friedman had decreed, theoretical propositions hold as long as they are not refuted. There were of course reasons why the monetarists' propositions ceased to be verified, such as financial innovations or the conjecture that after all the demand for money is highly sensitive to interest rates, a feature which was more visible once controlling the money supply became the aim of central banks' policy. But in Friedman's methodology, facts are judge and jury to the effect that new causal factors are no excuse.⁴⁶

⁴⁶ In a 2000 *Journal of Economics Perspectives* article, De Long published an article entitled "The Triumph of Monetarism?" wherein he wrote that "the influence of monetarism on how we all think about macroeconomics today has been deep, pervasive, and subtle" (DeLong 2000: 85). In particular, he underlined that many of Friedman's insights could be found in second-generation new Keynesian models (to use my terminology). DeLong's remarks are less opposed to mine than it may seem at first. It is just that he divided monetarism into distinct streams, Friedman's monetarism being one of them and the only one to meet with his approval. Still, his judgment about it was mixed. He admitted that stable velocity and stable demand for money had been disqualified. He also judged that "Political Monetarism' crashed and burned in the 1980s" (DeLong 2000: 22) by this he meant the view that money just matters because of the possibility of central banks' potential for mischief. These are the items on which my downfall judgment is based.

TABLE 4.1 *The evolution from Friedman's expectations-augmented Phillips Curve model to Lucas's and Kydland and Prescott's models*

	Friedman (1968)	Lucas (1972)	Lucas (1975, 1976)	Kydland and Prescott (1982)
<i>Purpose</i>	Demonstrating the inefficiency of monetary policy	Demonstrating the inefficiency of monetary policy	Constructing an equilibrium theory of the business cycle	Constructing an equilibrium theory of the business cycle
<i>General approach</i>	Marshallian partial equilibrium	Walrasian general equilibrium	Walrasian general equilibrium	Walrasian general equilibrium
<i>Attitude w.r.t. the neoclassical synthesis</i>	support	rejection	rejection	rejection
<i>Main assumptions the nature of the shock</i>	money creation mistake	monetary and real shock	monetary and real shock	technology shock
<i>Expectations Allocation</i>	adaptive intra-temporal	rational inter- and inter-temporal	rational inter- and inter-temporal	rational inter- and inter-temporal
<i>Information</i>	imperfect	imperfect	imperfect	perfect

Friedman disarray was visible in his 2001 interview with Taylor. Before starting the formal interview, Taylor and Friedman engaged in an informal chat about charts on the volatility of real GDP and real M2 and on velocity, a part of which was recorded and included in the published transcripts. There, Friedman can be seen expressed his bafflement at the decrease variance in real GDP and real M2 from the early 1990s onwards “as if the Fed has installed a new and improved thermostatic controller in the 1990” (Taylor 2001: 103). For his part, Taylor tried to make Friedman admit that the improved economic performance of the United States could be explained by the FED’s return to a policy of checking interest rates and use of the rule that he himself had formulated in those years. Refusing the bait, Friedman had no other interpretation for the fact that a “good thermostat had replaced a bad one” than saying it was all due to Alan Greenspan’s talent!

What I’m puzzled about is whether, and if so how, they suddenly learned how to regulate the economy. Does Alan Greenspan have an insight into the movements of the economy and the shocks that other people don’t have? (Friedman in Taylor 2001: 105)

Let me now turn to the second aspect of the fall of monetarism. In the beginning, Keynesians believed that Lucas’s seminal money-supply surprise model, to be studied in Chapter 9, was a mere extension of Friedman’s work, which led some commentators to label it ‘monetarism mark II.’⁴⁷ Had this been true, concluding to the final victory of monetarism over Keynesian macroeconomics would have been warranted. Lucas’s contribution would have then been regarded as the mere transformation of Friedman’s model into a general equilibrium model. However, after the dust settled, it turned out that this is an incorrect judgment. In spite of Friedman and Lucas’s personal links and mutual esteem, a methodological gulf separates Friedman’s monetarism and DSGE macroeconomics for which Lucas blazed the trail. In Chapter 10, I will compare the main methodological tenets of Keynesian and Lucasian macroeconomics bringing out how deeply they differ. It will then be clear that on most of the benchmarks considered, Friedman stands on the Keynesian rather than on the Lucasian side. Anticipating on my analysis in further chapters, Table 4.1 summarizes the methodological distance separating Friedman from new classical and RBC modeling on a few selected benchmarks.

So, Keynesian macroeconomics and monetarism were companions in misfortune. As noticed by Sargent, the Lucasian revolution was “impartial in the rough treatment it handed out to participants on both sides of the monetarist-Keynesian controversies” (Sargent 1996: 5). A collateral result of this downfall was that, facing a new common adversary, the old foes discovered that, beyond their differences, they had much in common.

⁴⁷ Cf. Tobin (1980) and Hoover (1984).

Phelps and Friedman: The Natural Rate of Unemployment

In Chapter 2, I explained why Keynesian economists were glad to include the Phillips relation within their paradigm: it filled the earlier gap with regards to price formation. Moreover, the Phillips relation provided macroeconomists with the possibility of making clear policy recommendations to the government about the different mixes of unemployment and inflation. The late 1960s were thus a good time for Keynesian theory. In Edmund Phelps's words, "This seemingly rock-solid Phillips curve put the American Keynesians into a sort of euphoria, as if they had discovered atomic power" (Phelps 2006: 8). However, the euphoria would not last long.

It might be thought that the late 1960s modeling of the Phillips curve and, as a by-product, the natural rate grew out of great agitation and yearning for light. In fact, the Keynes-Phillips orthodoxy was sailing on smooth waters, the object of much congratulation, rather like the liner *Titanic* prior to its collision with the fateful iceberg. In the present case the iceberg was the neutrality axiom of Lerner and Fellner. What made a collision of the Phillips curve with neutrality inevitable was that sooner or later someone – or some *two* – would provide one or more micro-models of the Phillips curve, and with that development the difficulty of maintaining that inflation was non-neutral, which the Keynes-Phillips position implies, would be exposed. (Phelps 1995: 17)

The aim of this chapter is to recount the change in the intellectual scene that occurred after two economists – Phelps in a 1967 and a 1968 paper and Friedman in his Presidential Address to the 1967 meetings of the American Economic Association (Friedman 1968) – concomitantly but independently, dismissed the Keynesian Phillips curve, with its trade-off correlate, by introducing the notion of a 'natural rate of unemployment.'

The chapter begins with an exposition of Phelps's contribution. Next, I move on to the study of Friedman's paper. The last section is a comparison between Phelps's and Friedman's papers. These, I show, are less similar than they are usually believed to be.

PHELPS

The man and his work

Over his long research career, from the early 1960s to the present, Phelps has pursued a variety of intellectual queries.¹ It started with an important contribution to growth theory (Phelps 1961) in which Phelps set out the ‘golden rule of accumulation’ – that the accumulation of capital ought to be equal to the profit rate. Next, he tackled the issue of employment and inflation (Phelps 1967, 1968), my object in this chapter. He was also the leading character behind the celebrated Phelps volume, a collection of articles about the microeconomic foundations of employment and inflation theory (Phelps 1971). Two decades later, discussing unemployment in Europe, he wrote a book proposing a ‘structuralist’ model of macroeconomics in which slumps and booms are analyzed as fluctuations of the natural rate of employment (Phelps 1994). Finally, time and again Phelps has reflected on the nature of a capitalist economy with a concern for justice and inclusiveness (Phelps 1997).

Most of the economists I study are easy to qualify, old or new Keynesian, neoclassical synthesis economist, monetarist, new classicist, or RBC economist, and so on. Not so for Phelps. He has been too eclectic to be pigeonholed. He was a forerunner of search theory but soon lost interest in it. Although close to Lucas, who borrowed the islands parable from him, Phelps has nonetheless been an adamant critic of the rational expectations school (Phelps 1990: 46, Frydman and Phelps 1983, 2013). Nor has he demonstrated any sympathy for RBC macroeconomics (Phelps 1990: 90–91). As for Keynes, as will be seen, the fact that Phelps contributed to demolishing a totemic ingredient of Keynesian theory, did not stop him from praising Keynes as a great modernist character.

Phelps’s two main studies on equilibrium unemployment are his “Phillips Curves, Inflation, Expectations and Optimal Employment over Time” (Phelps 1967) and “Money-Wage Dynamics and Labor-Market Equilibrium” (Phelps 1968) articles. The aim of the first of these was to provide an account of the optimal inflation policy in an economy where the traditional Phillips curve is augmented by agents’ expectations about inflation. Phelps argued that the equilibrium rate of unemployment is independent from the rate of inflation as the Phillips curve shifts on a par with agents’ expectations about the price level. The second paper, my object of study in this section, was similarly motivated by Phelps’s dissatisfaction with the Phillips curve as it was incorporated into standard macroeconomics. “Is the Phillips curve trade-off real, serious, and not misleading?”, Phelps asked (1968: 681), and his answer was dismissive.

¹ See Dimand (2008a), Howitt (2007), as well as Phelps’s own comments on his work, in particular Phelps (2006) and Phelps (2007), and Vane and Mulhearn’s (2009) interview with Phelps.

The reasons for his dissatisfaction were manifold. One of them was that the labor market does not function like an auction market. Trade technology is different. Unemployment may well be a possibility for the supply side of the market, but as far as the demand side is concerned, there is a counterpart to it that standard theory neglects, which is vacancies. Any account stating that excess demand for labor (be it positive or negative) depends on the arithmetic sum of demand and supply cannot encompass this aspect. The consequence of bringing vacancies into the picture is that unemployment finds its place within it almost naturally. Another point that Phelps wanted to stress was the need to give attention to the strategy pursued by agents – in modern terms, to microfoundations.² Any theory starting from market supply and demand functions misses this dimension. Phelps's attention in this respect was less on workers than on firms. The one point he decided to focus on was that there were good reasons for firms to offer higher wages than their competitors. Another element which was missing was the intertemporal perspective and the consideration of expectations. The existing Phillips curve consisted in a succession of temporary equilibria without raising the question as to whether they gravitated towards steady state equilibrium. Finally, Phelps believed that the existing Phillips curve ran counter to the stylized fact stemming from econometric works that, as far as the impact of wages on unemployment was concerned, changes in rates were more important than changes in levels (Phelps 1968: 681). Therefore, he also felt the need to show that his new theoretical categories allowed for a good econometric fit, which he did in a statistical appendix to the paper.

Phelps's model

Phelps's analysis is based on two related premises. The first one, which I have just evoked, is that vacancies must receive a central place in the analysis. He argued that, contrary to what is usually assumed, there is no one-to-one relationship between vacancies and unemployment. That is, an increase in vacancies, resulting for example from an increase in aggregate demand, does not automatically lead to a proportionate decrease in unemployment. Phelps also wanted a model in which the economy could be in disequilibrium, approaching the steady state only gradually. In other words, he wanted employment to react to increases in vacancies with inertia, with firms smoothing their recruitment efforts over time. To this end, he assumed that marginal recruitment costs were rising. The second premise of his analysis is

² In the context of the 1960s, he puts himself in the camp of the "revolutionaries" of the 1960s vintage, that is, "those economists who wanted macroeconomic models to have lifelike actors whose expectations and beliefs were causal forces" (2006: 1) against those who did not.

that a departure from the Marshallian supply and demand trade technology is needed. Markets – and, in particular the labor market, Phelps’s specific object of study – ought to be viewed as decentralized. Firms are supposed to set prices, and in doing this to be attentive to their competitors’ wage setting. As they lack full information on the subject, they need to develop expectations about their competitors’ wage behavior. Moreover, Phelps assumed that hiring is costly. Hence firms have an incentive to avoid a high turn-over in the labor force. To keep their workers, they set the wage at a higher level than that of their competitors. As a result, the unemployment rate and the vacancy rate are always positive.³ All firms being identical, they will all behave in this way, but this equalization of wage differentials is assumedly not instantaneous.

I now turn to presenting the main lines of Phelps’s model. Excess demand can be defined as follows:

$$N_D - L = (N + V) - (N + U) = V - U \quad (5.1)$$

where N_D is the demand for labor, L the supply of labor, supposedly equal to the labor force, N the number of employed workers, V the number of vacant positions, U the number of unemployed workers.

To capture the idea that an individual firm’s optimizing plan implies a desired wage differential with other firms, Phelps introduced the notion of an ‘average desired wage differential’ (Δ^*), which he posited as a function of the unemployment rate and the vacancy rate:

$$\Delta^* = m(u, v) \quad (5.2)$$

where $u \equiv U/L$, $v = V/L$ and m is a function that is monotonically increasing with respect to v and monotonically decreasing with respect to u . The wage-setting dynamic equation follows:

$$\frac{\dot{W}}{W} = \Delta^*, \quad (5.3)$$

where W is the money wage.

Equilibrium exists whenever $\Delta^* = 0$. Phelps began by assuming that each firm expects the wage paid elsewhere to be constant. Later in the article, he removed this assumption and replaced it with the assumption that firms form expectations about the changes in wages made by their competitors.

³ “Firms must incur ‘search costs’ to find round pegs to fill round holes, and unemployed workers must also expend money and energy to find suitable employment. As a consequence, positive unemployment and positive job vacancies tend to persist in a growing labor market and even under stationary labor supply because of the turnover or attrition of firms’ employment rolls” (Phelps 1968: 683).

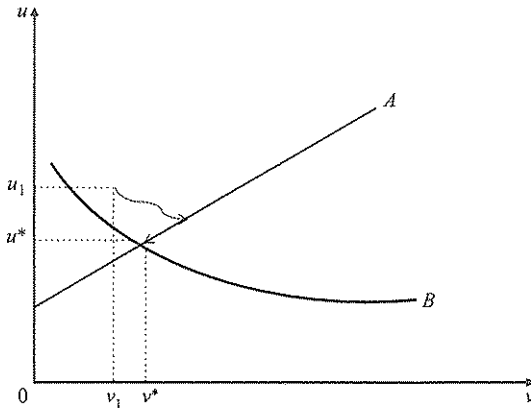


FIGURE 5.1 Relations between vacancy and unemployment rates

The dynamics of the labor market is described by

$$\dot{N} = R - Q$$

where $\dot{N} = dN/dt$, R is the recruitment flow and Q the quitting flow. Phelps posited that $(R - Q)/L$ is a function of u and v :

$$z = \dot{N}/L = z(u, v) \tag{5.4}$$

z , the rate of employment change per capita, is a convex function which is monotonically increasing with respect to its two arguments. Its dynamics is related to the unemployment dynamics by the relation

$$\dot{N} = \dot{L} - L\dot{u} - u\dot{L} \tag{5.5}^4$$

or, per capita:

$$\dot{N}/L = \gamma(1 - u) - \dot{u} \tag{5.6}$$

where $\gamma \equiv \dot{L}/L$ is the growth rate of labor supply.

A two-equation system ensues:

$$\dot{W} = Wm(u, v) \tag{5.7}$$

$$\dot{u} = \gamma(1 - u) - z(u, v) \tag{5.8}$$

Equation (5.7) is identical to equation (5.3), equation (5.8) is obtained by equalizing equations (5.4) and (5.6).

The elements above are combined in Figure 5.1, which has u and v for coordinates. The A curve is an example of the locus of (u, v) pairs such that

⁴ (5.5) follows from $N = L - uL$.

$\Delta^* = m(u, v) = 0$. The B curve is an example of the locus of (u, v) pairs such that $z(u, v) = \gamma(1 - u)$.

By definition, a steady-state equilibrium exists if u, v are constant over time, which implies that w is also constant. The steady state (u^*, v^*) corresponds to the intersection of A and B . Under plausible assumptions, it can be shown that curve B is attractive. For any (u, v) point lying above curve B , \dot{u} is negative, and u decreases whatever the value of v . Therefore it can be assumed that (u, v) will be close to curve B . Hence system (5.7–5.8) can be approximated as follows:

$$\dot{W} = Wm(u, v) \quad (5.9)$$

$$z(u, v) - \gamma(1 - u) = 0 \quad (5.10)$$

Since $z(u, v)$ is monotonic with respect to v , equation (5.8) can be solved for v . Thus, v function of u exists.

$$v = \psi(u, z) = \psi(u, \gamma(1 - u)) \quad (5.11)$$

By substituting (5.11) in (5.9), one obtains what Phelps calls the 'steady-state Phillips curve' (Phelps 1968: 693):

$$\frac{\dot{W}}{W} = m(u, \psi(u, \gamma(1 - u))) = f(u, \gamma(1 - u)) \quad (5.12)$$

As such, the steady-state Phillips curve hardly imperils the standard conception of the Phillips curve. The tradeoff conclusion is still valid. This result changes, however, when expectations about wages and inflation (since Phelps adhered to the cost-push explanation of inflation) enter the picture. Instead of assuming as before that firms expect their competitors to hold wages fixed, Phelps now assumed that firms have to form expectations about the changes in wages made by their competitors. He further assumed that these changes are not made instantaneously.⁵ The wage dynamics equation then becomes

$$\frac{\dot{W}}{W} = \Delta^* + \frac{\dot{W}^e}{W} = f(u, \gamma(1 - u)) + \frac{\dot{W}^e}{W} \quad (5.13)$$

where \dot{W}^e is the firms' expectations about the changes in the wage rate in the next period of exchange. In equilibrium $f(u, \gamma(1 - u))$ equals zero. Thus, firms' expected and actual rates of change of wages are equal:

$$\frac{\dot{W}}{W} = \frac{\dot{W}^e}{W}$$

Figure 5.2 displays the steady-state Phillips curve and its possible positions along the \dot{W}/W axis. u^* is the same unemployment rate as in Figure 5.1. It

⁵ "Each firm expects with certainty that the average wage paid elsewhere will change at a certain proportionate rate over the life of the firm's wage contract" (Phelps 1968: 697).

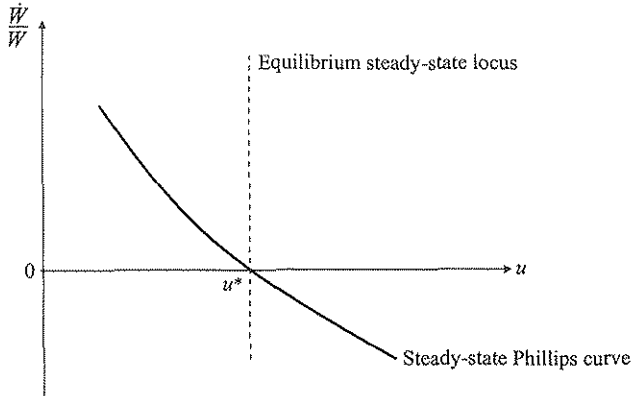


FIGURE 5.2 Phelps's expectations-augmented Phillips curve

constitutes the point toward which any (u, v) pair converges. The labor market equilibrium is invariant to whatever inflation rate is expected. That is, when expectations about the wage rate change, the steady-state Phillips curve moves upward or downward vertically along the dashed line, which Phelps calls the 'locus of steady-state equilibrium points.'⁶ In steady-state equilibrium an economy experiencing and anticipating a given money-wage growth and a corresponding inflation rate must have an unemployment rate similar to that which would prevail if it were experiencing and anticipating any different rate of wage increase (Phelps 1968: 703).

Phelps's paper is a difficult read; his argumentation is convoluted. The main technical flaw in his reasoning concerns the adjustment process toward u^* . That there is a tendency for u and v to join the B curve in Figure 5.1 raises no problem. However, in order to reach the intersection between the two curves, an additional specification is needed. Phelps implicitly assumed that starting, for example, from coordinates u_t and v_t , the trajectory toward equilibrium is described by the hatchet arrow, but he gave no indication about the underlying adjustment mechanism along curve A, merely calling it the 'equilibrium path.' Clearly, this impinges on the formation of the vertical Phillips curve. The process through which $\dot{W}/W = \dot{W}^e/W$ is reached is not explained. In the same vein, Phelps does not explain how firms' expectations are formed.

These shortcomings follow from the fact that Phelps was blazing a new trail, and for that matter a difficult one. They should not hide the important step forward which this article achieved. Its main contribution is to have devised the notion of an equilibrium unemployment rate that was lacking in the Keynesian conception of the Phillips curve. Moreover, Phelps was able to break the

⁶ This conclusion hinges on a few conditions that I have not mentioned such as a perfectly inelastic labor supply. See Phelps (1968: 702).

deadlock that I highlighted in Chapter 1, namely, the impossibility for early neoclassical theory to conceptualize unemployment – not only involuntary unemployment but also frictional unemployment. Phelps’s focus on equilibrium also marks the beginning of a radical move within macroeconomics from a time when it was considered normal to depict agents and markets as being in a state of disequilibrium to an era when it has become the rule to take agents’ optimizing behavior as the starting point for the analysis.⁷ In short, the importance of Phelps’s theory of unemployment lies mainly in the doors that it opened rather than in the intricacies of his argumentation: the attention to microfoundations, equilibrium reasoning, a new way of tackling the functioning of markets, expectations, are all features that were to become central in the following years.

FRIEDMAN

In Chapter 4, I studied monetarism in general. In this chapter, my object of study is one of Friedman’s papers, “The Role of Monetary Policy.”

A theory of the natural rate of unemployment

Friedman’s American Economic Association Presidential Address attacked two central policy tenets of Keynesianism. The first was the view that governments should push central banks to keep the interest rate as low as possible, a prescription that Keynes made in chapter 24 of *The General Theory*. In Friedman’s eyes, such a policy could not be sustained in the long run. His second target, my object of study, is the view that the Phillips curve is a stable relation enabling a tradeoff between inflation and unemployment. Friedman claimed that it is no longer true as soon as expectations are part of the equation.

Fiscal policy and monetary policy are the two standard policy tools recommended by Keynesian theory to deal with underemployment. Friedman barely mentioned fiscal policy, merely noting that the delay between the fiscal policy decision and the effects it has is usually so long that by the time these effects appear, the need for them may have vanished. He mainly focused his attention on monetary policy. As seen in the previous chapter, Friedman believed that money matters because, under the inspiration of Keynesian theory, central bankers are tempted to engage in mischievous money creation in the hope of durably increasing employment. His stroke of genius was to realize that the Phillips curve framework, which was a central piece of Keynesian theory, could be a vehicle for making his point. In other words, Friedman’s strategy was to alter the standard Phillips curve apparatus rather than to discard it. To this end,

⁷ The fact that agents behave in an optimizing way does not preclude that markets can experience disequilibrium, i.e., a departure from the steady state.

he pointed out that this apparatus suffered from two related flaws, its failure to take expectations into account and its focus on the money wage instead of the real wage. He also had a narrower target in mind, Samuelson and Solow's claim that the Phillips curve offered the government a menu for policy making, that is, the possibility of trading less unemployment for more inflation. The model embedded in the Presidential Address tackles this very issue of what happens when a central bank increases the money supply in order to push unemployment down. It shows that indeed this policy is non-neutral, it exerts an impact on the level of activity, yet no Keynesian conclusion should be inferred from this result. As for the reason explaining money non-neutrality, Friedman put forward an asymmetry in expectations about real wages between firms and workers. Firms are supposed to hold perfect foresight about the future prices of goods in contrast to workers whose expectations are supposed to be adaptive; they expect future prices to be a weighted average of past prices. Applying this assumption to his model leads to a result that Friedman summarized as follows:

Because selling prices of products typically respond to an unanticipated rise in nominal demand faster than prices of factors of production, real wages received have gone down – though real wages anticipated by employees went up, since the employees implicitly evaluated the wages offered at the earlier price level. Indeed, the simultaneous fall *ex post* in real wages to employers and rise *ex ante* in real wages to employees is what enabled employment to increase. (Friedman 1968: 10)

This somewhat enigmatic statement can be summarized in the following set of equations:

$$p_t > p_{t-1}, \quad W_t > W_{t-1} \quad (5.14)$$

$$p_t/p_{t-1} > W_t/W_{t-1} \quad (5.15)$$

$$p_t^{ew} = p_{t-1} \quad \text{and} \quad p_t^{ef} = p_t \quad (5.16)$$

$$\frac{W_t}{p_t^{ew}} > \frac{W_{t-1}}{p_{t-1}} > \frac{W_t}{p_t} = \frac{W_t}{p_t^{ef}} \quad (5.17)$$

where p_t is the price of goods at time t , W_t the nominal wage, p_t^{ew} (p_t^{ef}) workers' (firms') expectations of the goods price at t as formed at the opening of the labor market.

(5.14) states that the inflationary impact of money creation has reached both markets. (5.15) indicates that this impact is stronger in the goods market than in the labor market, while (5.16) indicates that workers' expectations about the goods prices, unlike those of firms, are backward-looking. Equation (5.17) summarizes the situation at the end of the period of exchange. It is assumed that during this period, the labor market and the goods market open sequentially. In the labor market, workers and firms bargain over the nominal wage, but their supply and demand function are parametrized with their expectations

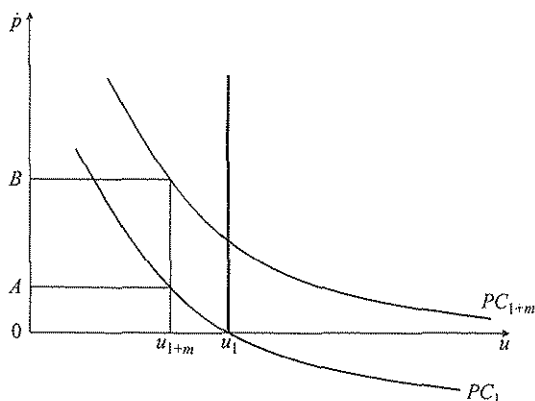


FIGURE 5.3 The accelerationist view of the Phillips curve

about the price that will prevail in the goods market. In this bargain, workers expect the price of the good to remain unchanged. Hence, they are willing to trade for a higher wage/hour mix as they expect it to provide them with a higher real wage (5.16). Yet they will be proven wrong; when the goods market closes, it will turn out that the real wage has declined (5.17). Firms, for their part, expect this decrease, which makes them willing to accept a higher nominal wage-hour mix, as they have correctly anticipated the decline in the real wage.

This is how Friedman is able to show that monetary expansion effectively results in real effects. However, he warns us that this is only half of the story:

But this situation is temporary: let the higher rate of growth of aggregate nominal demand and of prices continue, and perceptions will adjust to reality. When they do, the initial effect will disappear, and then even be reversed for a time as workers and employers find themselves locked into inappropriate contracts. Ultimately, employment will be back at the level that prevailed before the assumed unanticipated acceleration in aggregate nominal demand. (Friedman 1977: 14)

When the goods market closes, workers realize that their expectations about the real wage were wrong. Were the monetary expansion a one-shot move, the labor market would quickly return to its normal equilibrium. To keep the higher level of employment, monetary expansion must continue expanding the money supply at an increased rate (the so-called accelerationist view). It follows from equation (5.16) that at the next trading round, workers will expect the past increase in prices to persist. This results in a shift upward of the Phillips curve. Figure 5.3 illustrates this. It has unemployment on its horizontal axis and the rate of change of the price level on its vertical axis. It is assumed that at t_1 the natural rate of unemployment prevails and inflation is absent (the Phillips curve is PC_0 , with the u_0 unemployment rate). However, the government wishes to achieve a lower level of unemployment for which it is ready to tolerate an increase in the

inflation rate corresponding to OA on the vertical axis, and therefore engages in money creation. Effectively, at t_1 , ($t_1 - t_0$ indicating the time span needed for the effects of monetary changes to manifest themselves), a decrease in unemployment surfaces with unemployment equal to u_1 . This goes along with a decrease in the real wage. Because of (5.16), workers now anticipate an inflation rate equal to OA , which translates into the upward shift of the Phillips curve (PC_1). If the government wants to keep unemployment at its lower level, u_1 , it will need to surprise workers again with a higher level of inflation. The result is an increased inflation rate. To keep unemployment below the natural rate, money creation must go on at an increased rate. At some point, inflation will be transformed into hyperinflation and the monetary system will be imperiled. The central bank will then be compelled to engage in a deflationary policy, which will bring unemployment back to its natural rate. This explains why the Phillips curve, although featuring a downward slope in the short period, becomes a vertical line in the long period.

The lesson is clear: a departure from the natural rate of unemployment is possible only if inflation is unexpected. Maintaining a lower unemployment level requires an unsustainable permanent acceleration of the inflation rate. In other words, the labor market cannot permanently depart from the natural rate of unemployment.

An assessment

Few papers have been as influential as Friedman's Presidential Address for a number of valid reasons that I will touch on presently. Still the paradox is that, while Friedman's basic idea was powerful, his argumentation was hardly rock solid. The list of critical remarks that can be leveled against is surprisingly long.

- (a) The most obvious limitation of Friedman's reasoning concerns workers' misperceptions. The result of the adaptive-expectations assumption is that the workers are constantly wrong. Not only are they systematically mistaken, they also fail to draw any lessons from this experience. Thus, Friedman's analysis cries out for a new concept of expectations.
- (b) Friedman defines the natural rate of unemployment as follows:

[The] natural rate of unemployment is the level that would be ground out by the Walrasian system of general equilibrium equations, provided there is embedded in them the actual structural characters of the labor and commodity markets including market imperfections, stochastic variability in demands and supplies, the cost of gathering information about job vacancies and labor availabilities, the cost of mobility and so on. (Friedman 1968: 170)

This definition is more of a shopping list than a demonstration. The reference to Walras is also odd in view of Friedman's rejection of Walrasian general equilibrium analysis. More importantly, it turns out that unemployment is absent from the paper, which is rather ironical for a paper that is

considered to have introduced the notion of natural rate of unemployment!⁸ As observed time and again by Phelps, whose model features unemployment, Friedman's model is about over-employment rather than unemployment (Phelps 2006: 8). In reference to my Chapter 1 analysis, this makes Friedman's result a standard case of Marshallian disequilibrium, arising when the market-day allocation departs from its full equilibrium allocation, although nonetheless exhibiting market clearing.

- (c) Most of Friedman's discussion bears on the expansionary side of the natural rate. He states almost nothing about how backward gravitation, that is, the disinflationary process, might occur.⁹
- (d) As is often the case with Marshallian economists, Friedman has no qualms when it comes to switching from propositions pertaining to a model to propositions pertaining to reality. In the framework of the model, the natural rate of activity (a more correct appellation than natural rate of unemployment) must be understood as a center of gravity. This requires that the full equilibrium allocation, and the data of the market underpinning it, be assumed to be unchanging over the period analyzed. On at least two occasions, Friedman does not respect this requirement. The first one comes when he discards the possibility that monetary activation should be engaged in when the market rate of unemployment is higher than the natural one. At this juncture, he states that the size of the natural rate cannot be observed because it changes from time to time (Friedman 1968: 172). The second one is when he declares the re-equilibration process can take "a couple of decades" (Friedman 1968: 172). Here, the problem is that it is a stretch to think that the same center of gravity could hold for so long.¹⁰
- (e) Finally, another blind spot in Friedman's reasoning worth pointing out is that his dismissal of Keynesian demand activation is constructed with reference to a case in which no stimulation policy is really needed. "Let us assume that the monetary authority tries to peg the 'market rate' of unemployment at a level below the 'natural rate' (Friedman 1968: 9). As soon as this hypothesis is made, Friedman's conclusion becomes compelling. Friedman's argument consists in asserting that demand stimulation policies will have no lasting effects whenever the natural rate of employment is realized. In other words, he declares that over-employment cannot persist. But who would oppose such a statement? To undertake demand stimulation in this context is absurd, since all the issues that such

⁸ Cf. De Vroey (2007). ⁹ Cf. Leeson (2000, chapter 5).

¹⁰ This is a point on which Modigliani jumped in his 1977 exchange with Friedman, discussed in the previous chapter: "Now it seems to me that if indeed it takes five years to dispose of unemployment, then it is hard to believe that a policy-maker can be so stupid that one would believe he cannot do something to improve the situation" (Modigliani in FRB SF 1977: 19).

a policy might correct are absent. The success of such a policy should only be assessed in a context in which there is a reason to implement it.¹¹

Why, in view of all these flaws, must Friedman's Presidential Address nonetheless be considered as a milestone in the history of macroeconomics? A first reason is that none of the shortcomings mentioned above is a sufficient condition to reject Friedman's claim. They just indicate that his argumentation is far from tight and that the right implications have not yet been fully drawn. For example, adopting a search framework allows for the natural rate of unemployment notion to make sense.¹²

A second reason is that, assuming Friedman's target was indeed the Samuelson-Solow argument, his paper succeeds in demolishing it, which is a significant victory. Friedman's argumentation also points to a deep methodological flaw in theoretical IS-LM modeling, namely its tendency to isolate the short period from the long period analysis. In the terminology used in Chapter 1, Phillips curves account for a succession of temporary equilibrium allocations without connecting these to the full equilibrium allocation which acts as a center of gravity. More broadly, until Friedman (and Phelps) entered the fray, Keynesians 'owned' the Phillips curve. Keynesians had taken advantage of the fact that it was hard to deny fact that monetary changes have short-term effects. This led them to make a 'proof of the pudding is in eating it' type of argument, claiming that the very existence of such an effect vindicates the pre-existence of involuntary unemployment or underemployment. To Keynesians economists, monetary expansion was deemed to correct a disequilibrium state. According to Friedman, it created one!¹³

A third reason concerns originality. Taking the counterpoint of the standard view that Friedman's paper was a milestone, Forder (2010a, 2010b) has argued that the expectations argument was commonplace before Friedman and Phelps expressed them.¹⁴ It is true that the expectations theme was all around before Friedman wrote his paper. However, in my eyes, this does not make Friedman's paper trivial. By making the assumption of an asymmetry in expectations between firms and workers, in his own inchoative way, he devised a way of introducing mistaken expectations in the theoretical language. My surmise is that no theoretical model of misperception is to be found in earlier writings.

¹¹ Oddly enough, this criticism had to wait until the 1980s to be voiced. Cf. Hahn, (1982: 74-75) and Modigliani's interview with Feiwel (Feiwel 1989: 570).

¹² See Rogerson (1997).

¹³ It may be wondered why Friedman did not mention the claims made in his Presidential Address in his exchange with Modigliani discussed in the previous chapter. My surmise is that that to Friedman these claims had not reached the empirical level, which to him was the *sine qua non* of a valid theoretical proposition. This was also Thygesen opinion: "The theory of the natural rate of unemployment is a complicated hypothesis which is sketched suggestively, but not subjected to empirical testing by Friedman in either of his two main writings on the subject" (Thygesen 1977: 68).

¹⁴ In 2014, Forder published a book expanding on these views (Forder 2014), which I was unable to consult before the delivery of my manuscript.

Contributions have also to be gauged by their posterity, and Friedman's model paved the way for Lucas's "Expectations and Neutrality of Money" which was the fountainhead of the DSGE program.

Finally, Friedman's paper came at the right moment. It implicitly comprised a prediction: if governments keep creating money in order to decrease unemployment, this will just result in an acceleration of inflation and even an increase in unemployment. A few years later, when stagflation surfaced, Friedman's followers could trumpet that his model had won the day over the Keynesian one through a quasi real-life experiment: the stagflation episode, they claimed, was a real-world confirmation of Friedman's theory.¹⁵

These remarks suggest that Friedman's paper was a terrible blow to Keynesian theory. Again, the truth is more complex. As stated, it was definitely a blow as far as the policy-menu aspect was concerned. However, from another point of view, Friedman's analysis can be considered as a contribution to Keynesian macroeconomics. Keynes took it that full employment existed when frictional unemployment was the only kind of unemployment present. This means that Keynes's notion of full employment and Friedman's notion of a natural rate of unemployment are akin. Keynesians eventually realized this kinship and adopted the natural rate of unemployment idea; in the beginning, they preferred to brand it afresh, less naturalistically, as the 'non-accelerating inflation rate of unemployment.'¹⁶

CONFRONTING FRIEDMAN'S AND PHELPS'S APPROACHES

Phelps's and Friedman's papers are based on the same insight that expectations and a long-period or steady-state perspective need to enter the study of the relationship between inflation and unemployment. They also reach the same conclusion, that in the long period no stable tradeoff exists between inflation and unemployment. It is a small wonder that the image of simultaneous

¹⁵ As noted by Gordon, "The timing of Friedman's address was impeccable and even uncanny. The Kennedy-Johnson fiscal expansion, including both the tax cuts and Vietnam war spending, accompanied by monetary accommodation, had pushed the unemployment rate down from 5.5% to 3.5% and each year between 1963 and 1969 the inflation rate accelerated, just as Friedman's verbal model would have predicted. The acceleration of inflation bewildered the large-scale econometricians, who had previously estimated a 'full employment' unemployment rate of 4% and whose forecasts of inflation had been exceeded by the actual outcome year after year" (Gordon 2009:7).

¹⁶ I will return to the topic of how Keynesian economists responded to Friedman's argumentation in Chapter 12. At this juncture, I just want to mention one additional reaction to the Phillips curve discussion – and for that matter, a more radical one. It was expressed by Lawrence Summers in a paper entitled "Should Keynesian Economics Dispense with the Phillips Curve" (Summers 1988). To this question, Summers answered positively, returning to the animal spirits theme. In his words, "I believe that models allowing for hysteresis effects – models in which equilibria are fragile and history dependent – offer the best prospect for redeeming the promise of Keynesian macroeconomics" (Summers 1988:11). Summers pursued this hysteresis line in two stimulating papers co-authored with Olivier Blanchard (Blanchard and Summers 1986 and 1987).

independent discoveries comes to the mind. I nonetheless believe that significant differences are present behind these basic similarities.

Style and method

Friedman's and Phelps's papers are poles apart in style, aim and method. As far as style is concerned, Phelps's paper is a complicated, technical piece, hermetic to non-technical readers. Friedman's article is in plain English. On reading it, the non-specialist will easily understand the message. Thus, as far as persuasion is concerned, Friedman's paper is more effective. On another score, they did not pursue the same objective. Friedman's overarching aim was to uphold a policy conclusion. The Presidential Address was one of the many manifestations of his recurrent aim of dismissing Keynesian activation policy. While introducing the notion of a natural rate of unemployment, the central message he wanted to convey was that monetary policy should consist of rules rather than discretion. By contrast, Phelps wrote for the sake of the advancement of analytical theory.¹⁷ As he said in an interview with Vane and Mulhearn:

In this respect, it [the Phelps's model] was a significantly more advanced thing than Milton Friedman was talking about, and naturally I felt good about the added feature. On the other hand, it made it harder to discuss my model. Everything went so simply for Friedman because he had such a straightforward framework, but I was stuck with one that was inherently a bit more complicated. (Vane and Mulhearn 2009: 113)

Another difference is that Friedman and Phelps took a different approach of the working of markets. Friedman had not qualms about the Marshallian supply and demand analysis, thereby failing to realize that it had no room for the notion of unemployment that he wanted to bring to the forefront. In contrast, Phelps's intuition was that the traditional Marshallian framework constituted an impediment to the construction of a theory of unemployment, and hence needed be removed. On this basis, he proved able to construct an alternative framework wherein the existence unemployment would come out almost spontaneously. For this reason, Phelps's work is more innovative. Friedman and Phelps also diverged about the inflation mechanism: Friedman assumed that it was caused by undue monetary activation, Phelps had a cost-push explanation. Finally, a last difference between Friedman and Phelps concerns their treatment of the natural rate notion. Friedman was keen to argue that it is exogenously given while in his further work Phelps developed a theory of its endogenous determination. This move results in different viewpoints about governmental interventions.

¹⁷ Friedman would of course have argued that he was pursuing the same aim. Nonetheless, I cannot but think that, like Keynes, he was more preoccupied with persuading people that his diagnosis was accurate than with polishing its supporting arguments.

Relation to Keynes

Friedman's relation to Keynes was discussed in the previous chapter. Hence I will comment only on Phelps's. In his meta-theoretical writings, Phelps presented himself as a protagonist of a modernist revolution that struck the arts and many of the sciences in this century and of which economics was not left out. He claimed that in economics incomplete information was one of the most critical elements of this modernist surge.¹⁸ And to him, Keynes was the first modernist in economics, the precursor of imperfect information.

All of Keynes is about the difficulties of coordination that beset a market economy in which production decisions are left to decentralized enterprises and effort decisions left to decentralized households. (Phelps 1990: 17)

As for what concerns us more directly, Phelps recurrently presented his theory as an attempt at demonstrating the existence of involuntary unemployment, thereby following in Keynes's footsteps. Yet he did not understand this notion in Keynes's way. Keynes defined involuntary unemployment as a case of individual disequilibrium meaning by this that agents are unable to achieve their optimizing plan (what I call the 'involuntary unemployment concept in the individual disequilibrium sense'). Phelps pursued the more modest aim of developing a theoretical line in which involuntary unemployment co-existed with individual equilibrium. This implied a different understanding of the involuntary unemployment notion, taking now its common language meaning. It refers to people in a disagreeable position that they choose for lack of a better option yet with the idea of quitting it as soon as possible (the concept of 'involuntary unemployment in the casual sense'). Whenever the term is understood like this, most job-seekers qualify for it. Moreover, Phelps did not follow Keynes in wanting to have involuntary unemployment as a new category of unemployment to be added to frictional unemployment. To him, involuntariness in the casual sense was a trait of frictional unemployment, the only needed type of unemployment.

Fame and influence

From a theoretical point of view, Phelps's contribution has been more innovative and rigorous than Friedman's. Nonetheless, it is Friedman who received most of the credit for having introduced the natural rate of unemployment notion. Several reasons explain. Friedman was a master in communication. His

¹⁸ See Phelps (1990: 94). Giving free rein to a lyrical mood, Phelps also wrote: "Keynes was the bearer in economics of the intellectual revolution of his time. His outlook paralleled what was turning up in much art and philosophy – in the cubism of Picasso and Braque, the atonalism of Schoenberg and Berg, the fragmentary poetry of Eliot and Pound, and various writings from Nietzsche to Sartre. Keynes brought to economics the outlook generally called modernism: the consciousness of the distance between self and others, the multiplicity of perspectives, the end of objective truth, the vertiginous sense of disorder" (Phelps 1990: 5). See also Phelps (1993: 20; 2006: 7).

prose was convincing enough to hide the flaws in his reasoning. On the contrary, Phelps took the harder way of building a full-fledged model, and the one he ended up with was rather forbidding. Another factor is that in the late 1960s, Friedman had become a well-known, even if controversial, scholar, while Phelps was just a promising young economist. Moreover, Friedman's Presidential Address was part of a long-standing strategy, a further step in a line that he had started years before while Phelps was still in the process of building his wider theoretical vision. It is also small surprise that a Presidential Address written by a towering figure in the profession gets more resonance than a technical article written by an upcoming young economist, even if it is published in a prestigious journal. Finally, a last possible reason for the prevalence of Friedman's paper (and more broadly for the impact of the natural rate hypothesis) is that, as already mentioned, it implicitly proposed a predictive test of the validity of the theory. As Friedman stated in the Taylor interview, "I never had any expectation that it would have the impact it did. It only had that impact because of the accidental factor that you had a test right after" (Taylor 2001: 124).

For all these reasons, Friedman's name came to overshadow that of Phelps as the inventor of the natural rate of unemployment notion. While Friedman received the Nobel Prize in 1976, Phelps had to wait thirty more years to receive the same honor. Not that Phelps exerted no influence. The contrary is true. His islands parable was taken up by Lucas in his path breaking 1972 article to be studied later. The thriving literature about search unemployment can also be viewed as an indirect offspring of his 1968 article. Moreover, Phelps was also a precursor of the many new Keynesian economics who developed models aiming at producing equilibrium market non-clearing results. But his influence was more subterranean than Friedman. Fortunately enough for him, things changed belatedly, as the following two quotations attest:

Friedman (1968) is often cited in connection with the natural rate hypothesis, but in fact Phelps deserves at least as much credit, and I would argue even more, although it was Friedman who coined the term "natural rate of unemployment," deliberately paraphrasing Wicksell's "natural rate of interest." Phelps's analysis was more precise, deeper and more operational than Friedman's. Instead of writing out a formal model of the determination of the natural rate, Friedman was content to say that it was the rate "that would be ground out by the Walrasian system" if appropriate account were taken of imperfections, costs of information, structural characteristics of the labor market, etc. Phelps actually provided such an account, and it involved going far beyond the Walrasian system. (Howitt 2007: 208)

Friedman had the most influence at the time, but Phelps was the one to provide a micro-founded model with the natural rate property. (Leijonhufvud 2004: 811)

Leijonhufvud and Clower

After two chapters on Friedman and the monetarist challenge to Keynesian macroeconomics, in this chapter I return to the study of economists who reappraised *The General Theory*, a thread opened by Patinkin. I will focus on a 1968 book by Axel Leijonhufvud and a 1965 article by Robert Clower which both exerted an important influence.

Patinkin's shadow loomed large over these two economists. All three found that the gist of Keynesian theory lay in the existence of some malfunctioning of the adjustment toward the equilibrium process. However, they strongly diverged as far as the Walras/Marshall divide was concerned. Whereas Patinkin argued that Keynes's and Walras's theories could and needed to be integrated, Clower and Leijonhufvud found such a synthesis unacceptable (Clower came to this view after having first attempted to recast Keynesian theory in Walrasian language in his 1965 article). Table 6.1 summarizes this mix of commonalities and differences.

LEIJONHUFVUD

On Keynesian Economics and the Economics of Keynes

In 1968, a young economist, Axel Leijonhufvud, published a book entitled *On Keynesian Economics and the Economics of Keynes*, which caused a great sensation. It questioned the IS-LM interpretation of Keynes's *The General Theory* that prevailed at the time. Leijonhufvud's book was a book about a book, and its central message was that Keynes's book had been misread and its central message lost. "The Keynesian Revolution got off on the wrong track and continued on it" (Leijonhufvud 1968: 388). The lost message was that a decentralized economy faces information and signaling problems which make the coordination of economic agents' activities sub-optimal. "Keynes rejected

TABLE 6.1 *The commonalities and differences between Patinkin, Clower, and Leijonhufvud*

	Marshallian approach	Walrasian approach
Involuntary unemployment as existing only during the adjustment toward equilibrium process		Patinkin 1956
Involuntary unemployment as an end-state occurrence arising instantaneously		Clower 1965
Involuntary unemployment as a phenomenon of blocked adjustment	Leijonhufvud (1968) Clower (after 1965)	

the neoclassical notion that the price mechanism would efficiently perform the information function in the short run" (Leijonhufvud 1968: 394). Moreover, while most interpreters of *The General Theory* have viewed it as mingling incompatible theoretical claims – a point of view that I share – Leijonhufvud tried to show that the different components of *The General Theory* were all pieces of a unified project.

According to his interpretation, Keynes's argumentation rests on three tenets. The first is that what Keynes's theory aims at is explaining why market economies can remain in a state where the adjustment process is blocked:

The subject of his work [Keynes's *General Theory*] is not "unemployment equilibrium" but the nature of the macroeconomic process of *adjustment* to a disequilibrating disturbance. (Leijonhufvud 1968: 50)

The real question is why, in the Keynesian unemployment state, the forces tending to bring the system back to full employment are so weak. (Leijonhufvud, 1969: 22, note 1)

The second tenet also concerns the adjustment process. Unlike Patinkin, Leijonhufvud did not argue that all there was in Keynes's theory was sluggishness. Still, he believed that it was part of the picture. In his view, Keynes's specificity was to have reversed Marshall's ranking of the price and output adjustment speeds. According to standard Marshallian theory, in the short period quantities are fixed and prices flexible. Reversing this process means having prices sluggish accompanied with quantities that change quickly. Thus, Leijonhufvud's reversal captures the idea that it is easier for firms to modify the size of their labor force than to change wages.¹

¹ "In the Keynesian macrosystem the Marshallian ranking of price- and quantity-adjustment speeds is reversed: in the shortest period, flow quantities are freely variable, but one or more prices are given, and the admissible range of variations for the rest of the prices is thereby limited. The

However, for Leijonhufvud, sluggishness hardly encapsulated the whole of Keynes's construct. Insufficiency of investment was also crucial, which brings me to the third tenet. It concerns the long-period interest rate. Here, two elements interact in Leijonhufvud's interpretation. The first is the prevalence of a false (i.e., non-equilibrium) interest rate, the market rate being higher than the natural rate or, in other words, the price of bonds too low (Leijonhufvud 1968: 335). This means that the interest rate is unable to do the job it should do, coordinating saving and investment. The second element is the absence of any signal allowing this state of affairs to be perceived.

Intertemporal prices are wrong. Saving in itself does not constitute an effective demand for future consumption goods. And of all prices in Keynes's theory, the (long-term) real rate of interest is the slowest adjusting: "it may fluctuate for decades about a level which is chronically too high." (Keynes 1936, p. 204). (Leijonhufvud 1998: 229)²

This view has the important policy implication that "the burden of adjustment should not be thrown on this market [the labor market]. Asset prices are 'wrong' and it is to asset markets that the cure should, if possible, be applied" (Leijonhufvud 1968: 336).

These are according to Leijonhufvud the main themes running through *The General Theory*, and his complaint was that IS-LM macroeconomics failed to come to grips with them. Finally, another recurrent theme in Leijonhufvud's book is that it is impossible to achieve Keynes's aim within the framework of Walrasian theory. In this respect, Leijonhufvud strongly departed from Patinkin. For all their common insistence on disequilibrium and sluggishness, they were poles apart on whether Keynes's theory belongs to the Walrasian or the Marshallian branch of neoclassical theory. According to Leijonhufvud, Keynes was a Marshallian economist *par excellence*, which in his eyes meant being non-Walrasian in the strong sense of the term.

Keynes's model is characterized by the absence of a 'Walrasian auctioneer' assumed to furnish, without charges and without delay, all the information need to obtain the perfect co-ordination of the activities (both spot and future) of all traders. (Leijonhufvud 1968: 48)

To make the transition from Walras's world to Keynes's world, it is thus sufficient to dispense with the assumed *tâtonnement* mechanism. The removal of the auctioneer simply means that the generation of information needed to co-ordinate economic activities in a large system where decision making is decentralized will take time and will involve economic cost. (Leijonhufvud 1967: 404)³

'revolutionary' element in *The General Theory* can perhaps not be stated in simpler terms." (Leijonhufvud 1968: 52)

² For a more detailed account, see (Leijonhufvud 1969: 37).

³ See also Leijonhufvud (1968: 85).

An assessment

The aim of Leijonhufvud's book was to offer a fresh interpretation of *The General Theory* that would bring out the richness and originality of Keynes's theory, while at the same time acknowledging the obstacles Keynes encountered in achieving his program. In this respect, it was a full success. Leijonhufvud's depth of insights, as well as his mastery of Keynes's arcane argumentation, is still impressive today. A rare occurrence for a book that was mainly critical, Leijonhufvud prompted macroeconomists, especially young ones, to reassess the road taken by the first generation of Keynesian economists and to realize that alternative routes could be envisaged.

One criticism that was levelled at his book was that it was based on questionable textual evidence:

I was struck by how much of [Clower and Leijonhufvud's] work is a positive contribution and how little of it is an exposition of what Keynes himself said or can reasonably be interpreted to have meant. (Yeager 1973: 156)⁴

Yeager may be right but it does not mean that his criticism is relevant. It may be true that Leijonhufvud was trying to read Keynes's mind, but so what? It matters little whether Leijonhufvud's views can be found in Keynes's *The General Theory*, or whether they constitute a bold extension of Keynes's writings. What counts is whether they are interesting. With the passing of time, texts can take on a life of their own, to the extent that what readers draw from them may change over time. There is also the possibility of a discrepancy between an author's intentions and the book's meaning as perceived by readers. Keynes wanted to emphasize coordination problems but was unable to translate this into an adequate theoretical corpus. It is therefore understandable that commentators feel it is important to return to Keynes's initial intentions, and to try to re-express these in a stronger way.

Other criticisms can be levelled at Leijonhufvud's work. As already stated in Chapter 1, if my interpretation of Marshall's temporary equilibrium theory is correct, sluggishness cannot cause market rationing. Thus, Leijonhufvud's idea of a reversal of adjustment speed seems spurious. It boils down to arguing that it takes longer to reach full equilibrium rather than temporary equilibrium. I find this proposition trivial. Because reaching full equilibrium happens over a succession of market periods while reaching temporary equilibrium arises within one such period, it is a small wonder that the former process takes more time. A second critical remark pertains to Leijonhufvud's insistence on describing Keynes's theory as trying to get rid of the auctioneer assumption. In terms of substance, I fully agree that adopting the auctioneer hypothesis precludes Keynesian conclusions. But how could Keynes, who was writing in

⁴ See also Coddington (1983: 107).

1936 at a time when Walrasian theory was close to oblivion, have wanted to get rid of the Walrasian auctioneer?⁵ As for Leijonhufvud's considering the IS-LM model as part of the Walrasian approach, I believe that it does not stand up to scrutiny, but I will postpone vindicating this view until Chapter 18.⁶

However, these criticisms are of secondary importance. The main problem with Leijonhufvud's interpretation of *The General Theory* and his ensuing plea for redirecting macroeconomics toward the coordination or signalling failures theme is of another order. Many economists agreed that he had indeed put the finger on a theme that ought to be put at the top of the research agenda. Yet what to do next was less clear. Leijonhufvud's book was mainly critical, so that no tractable research program ensued from his analysis. This explains why, although it was highly praised, Leijonhufvud's book generated relatively little following. More exactly, as will be seen in the next chapter, those economists who declared treading Leijonhufvud's footsteps did no really do so.

One prominent economist who decided to join forces with Leijonhufvud was Robert Clower. He had authored two influential articles published in 1965 and 1967, respectively, "The Keynesian Counter-Revolution: A Theoretical Appraisal" (to be discussed in the next section) and "A Reconsideration of the Microfoundations of Monetary Theory." With a few companions, among whom Peter Howitt, he and Leijonhufvud set out to define what could be called the 'Clower-Leijonhufvud program.'

The task ahead, as they viewed it, was to reconstruct macroeconomics on a Marshallian basis – that is, to construct a simplified Marshallian general equilibrium theory into which Keynesian theory could be embedded. Moreover, unlike Walrasian general equilibrium analysis, Marshallian general equilibrium analysis, as they meant it, had to pay more attention to the issue of adjustment towards equilibrium than to that of existence. Hence, they labelled it "general process analysis." In this perspective, a different, more realistic, trade technology needed to be devised. In a joint article, Clower and Leijonhufvud made the following list of main constitutive features:

[The new trade technology] (1) lacks a central information-processing and bill-collecting agency; (2) has, instead, middlemen trying to coordinate production and consumption activities in each output market separately; (3) makes the management of stocks of inventories essential to the coordination of these activities; and (4) has the system potentially subject to the commercial crises associated with expansions and contractions

⁵ In the quotations given earlier, Leijonhufvud characterizes Keynes's model as devoid of any auctioneer without stating explicitly that Keynes's aim was to get rid of it. But he did go that far at least once: "The only thing which Keynes *removed* from the foundations of classical theory was the *deus ex machina* – the auctioneer which is assumed to furnish, without charges, all the information need to obtain the perfect co-ordination of the activities of all traders in the present and through the future" (Leijonhufvud 1967: 309).

⁶ Later, Leijonhufvud (1984: 33) took a more subdued view, admitting that the IS-LM model was not a pure Walrasian construct but, rather, a hybrid.

of the volume of bank and nonbank credit. All this might be J.S. Mill or Alfred Marshall. (Clower and Leijonhufvud 1975: 187)

The Marshall to whom Clower and Leijonhufvud recommended returning was not the Marshall whose views were taken up in standard microeconomic analysis (i.e., the value theorist) and to whom I referred in Chapter 1, but Marshall the behavioral theorist. In other words, to them, what was important in Marshall's *Principles* was less his Book V, which received most of the attention, but his scattered and admittedly less systematically developed descriptive insights into the working of the economic system.⁷

Unfortunately, it proved extremely difficult for Clower and Leijonhufvud to take their views beyond the blueprint stage, that is, to transform their scenario of a decentralized economy with money as a social link, in which private merchants substitute for the auctioneer and in which multiple disequilibria could be the norm, into a full-fledged research program.

CLOWER'S "KEYNESIAN COUNTER-REVOLUTION" ARTICLE

The man and his work

Most of the economists I have studied hitherto have been examples of constancy, having defended the views of their youth throughout their lives. Modigliani and Patinkin are good examples of such an attitude. Keynes was not! Clower (1926–2011) was also of a different breed. He excelled in the opposite virtue, intellectual suspicion, exerted both on himself and others. Time and again, he tried to uncover the logic at work in *The General Theory* without ever producing a definitive answer (Clower [1960]1984, [1965] 1984, [1975] 1984, 1997) – except for one point, already made in his 1960 paper, namely, the view that “Keynes dealt with disequilibrium states,” the existence of which classical economists recognized without systematically analyzing them (Clower [1960] 1984: 25). From this, he inferred that the uppermost task to be undertaken was to study the “dynamical assumptions underlying the market adjustment process” (Clower [1960] 1984: 23). As for his own position on the chessboard of theoretical positions, he moved from admitting that he was a non-fanatic neo-Walrasian economist (Clower [1960] 1984: 25), to striving to formulate Keynesian insights in Walrasian language, before eventually becoming fiercely opposed to Walrasian theory joining forces with Leijonhufvud. The subject-matter of this section is his second phase to be found in his paper, “The Keynesian Counter-revolution: A Theoretical Appraisal.” It was presented at an International Economic Association Conference held in 1962 at Royaumont in France and published in 1965 in a volume edited by Frank Hahn and Frank Brechling. I have chosen to discuss it, first, because of its panache and its provocative character, and, second, because it paved the way for non-Walrasian equilibrium modeling, which will be studied in the next chapter.

⁷ See Clower [1975] 1984: 193–194) and Leijonhufvud (2006b).

Clower's article was one more contribution to the 'Mr. Keynes and the Classics' branch of literature. Although he failed to make clear what he meant by classical theory, using the "traditional theory" terminology, it must be interpreted as Walrasian theory. While the adjustment failure dimension was part of the paper, Clower's distinctive take in it was slightly different. He argued that what was central in *The General Theory* was the idea that households' demand for consumption goods is a function of their income. That is, income acts as a quantity constraint on consumption. This feature is absent from classical theory: "income magnitudes do not appear as independent variables in the demand or supply functions of a general equilibrium model" (Clower [1965] 1984: 42). Hence the task that Clower set out for himself was to study the consequences of introducing income into Walrasian demand functions. In Walrasian theory, it is assumed that households' purchasing and selling decisions are made simultaneously. Clower called this the 'unified decision' process. He proposed the 'dual decision' notion to designate the alternative case of a separate operation of these decisions. In his eyes, this notion was a good way of capturing Keynes's basic insight. Clower took the example of an economic consultant who has a strong appetite for champagne but faces a weak demand for her services. In this context, will she gratify her desire for her preferred drink? His answer was 'No' if this desire imperils the household's finances. Champagne consumption will be postponed to more prosperous times. In short, consumption is income constrained.

Clower drew two implications from the adoption of the dual decision hypothesis. The first is that it leads to an allocation in which the labor market displays market non-clearing and the other markets display market clearing (yet with an allocation which is different from the Walrasian one). Thereby, he claimed to have established the invalidation of Say's Law, now re-formulated as Walras's Law – a claim that Keynes had made, but that most macroeconomists had stopped supporting. The second one is that the prevalence of a non-Walrasian allocation can be regarded as a case of coordination failure, a blocking of the adjustment process due to a signalling problem.

Looking at the functioning of the economy through the prism of the dual decision hypothesis prompted Clower to split the hitherto unified notion of a demand function into two ones, 'notional' and 'effective' demand. The difference between them is related to the nature of the household's budget constraint. In Walrasian theory, everything works with the notional aspect. According to Clower, the hallmark of Keynesian theory is that effective demand comes to replace notional demand.⁸

Clower's 1965 model

Clower's model studies an economy comprising n goods and the market for labor services. They are subdivided into two types: m output commodities (in

⁸ Clower uses the same terminology as Keynes, yet with a different meaning.

number $i = 1, \dots, m$) demanded by households and supplied by firms, and $n-m$ inputs (in number $j = m+1, \dots, n$) supplied by households and demanded by firms. P is the price vector with input n , labor, serving as the numéraire. The model describes the end result of an adjustment process, which is described in no more precise terms than as resulting from the working of market forces (Clower [1965] 1984: 37). No mention is made of Walrasian theory. Nonetheless, the assumptions usually associated with it – decisions taken in a unified way, the presence of the numéraire, a general equilibrium approach, Walras’s Law – are present. Above all, the price-making assumption requires the presence of an auctioneer. Thus, I take it that the economy under study is a Walrasian economy.

In the standard Walrasian framework, households’ notional aggregate demand and supply functions, $\bar{d}_i(P, \pi), \bar{s}_j(P, \pi)$, are the solutions to the following problem:

$$\begin{aligned} &\text{Maximize} && U(d_1, \dots, d_m; s_{m+1}, \dots, s_n), \\ &\text{Subject to the budget constraint,} && \sum_i p_i d_i - \sum_j p_j s_j - \pi = 0 \end{aligned}$$

where d and s are quantities of commodities, and π profits. The bold characters are parameters, the others are decisional variables. p_i and p_j are numéraire prices.

Whenever the standard Walrasian theory is modified by adopting the dual decision hypothesis, effective aggregate demand functions, as distinct from the notional ones, come into play. Clower’s analysis postulated that workers are rationed, that is, $\bar{s}_n > \bar{d}_n$. Behind this occurrence, there must be a real wage that is higher than the equilibrium wage. Like Hicks, Clower postulated wage rigidity. Labor supply is rationed, which can be expressed by the equation $s_n = \bar{d}_n$. This equation means that the quantity of labor traded is no longer the result of a choice made by households. It has become a parameter. A transformation of the households, budget constraint ensues:

$$\sum_i p_i d_i - \sum_j p_j s_j - \pi = 0 \text{ with } \sum_i p_i s_i < \sum_j p_j s_j$$

The difference between the two budget constraints is that the former comprises no quantity limits on trade except endowments, while the latter is based on a transformed budget constraint, including such limits, namely s_n . In other words, effective demand is more strongly constrained than notional demand.⁹

⁹ This is the very idea that Patinkin had toyed with in his dissertation (cf. Rubín 2012). It will be translated into the somewhat ambiguous phrase “constrained demand”, leading in turn to the phrase ‘constrained equilibrium’.

By spill-over from the labor market to the goods markets, the notional demand function comes to be replaced by the effective demand (\bar{d}_i) with $\bar{d}_i < \tilde{d}_i$, where \tilde{d} designates the effective demand. Households constrained demand function are expressed as $\bar{d}_i(P, Y)$, where

$$Y = \sum_j p_j s_j - \pi = 0$$

A second claim made by Clower is that Keynes was right in arguing that the existence of involuntary unemployment and the invalidation of Say's Law were one and the same thing. Unafraid of being provocative, Clower decided to take up Keynes's position by boldly arguing that there was room for only one of the two 'sacred cows,' Keynesian theory and Walras's Law.

Either Walras's law is incompatible with Keynesian economics or Keynes had nothing fundamentally new to add to orthodox economic theory. (Clower [1965] 1984: 41)

Equation (6.1) is Walras's Law, valid both for equilibrium and disequilibrium states of the economy:

$$\sum_i^m p_i [\bar{d}_i(P) - \bar{s}_i(P)] + \sum_j^n p_j [\bar{d}_j(P) - \bar{s}_j(P)] \equiv 0. \quad (6.1)$$

In this equation, only notional demand and supply functions appear ($\bar{d}_i, \bar{d}_j, \bar{s}_i, \bar{s}_j$). Its first term aggregates excess demand functions for all commodities times their prices expressed in terms of the numéraire good; the second term does the same for all inputs. The equation backs up the standard meaning of Walras's Law: these two sums cancel each other out.

Introducing the labor market rationing and its spill-over effect in the goods market leads to equation (6.2). Because $\bar{s}_n > \bar{d}_n$ and $\bar{d}_i = \bar{s}_i$, Walras's Law is invalidated, as the first term will be zero and the second one will be negative:

$$\sum_i^m p_i [\tilde{d}_i(P, Y) - \bar{s}_i(P)] + \sum_j^n p_j [\bar{d}_j(P) - \bar{s}_j(P)] \leq 0 \quad (6.2)$$

A third theme developed in Clower's article is coordination failures. Returning to the champagne example, the consultant is not the only frustrated agent in this story; the champagne firm also is. Consultants wish to purchase more champagne, but in order to do so they must be able to sell more of their consultant services. Firms wish to sell more champagne and so they should buy more factor services, including those of consultants. The two parties have a common interest. The problem is how to communicate their mutual readiness. The standard answer of economic theory is that prices, acting as signals to which agents react by adjusting quantities, serve the purpose of making agents' plans compatible. Clower's claim is that this no longer works once income enters the utility maximization problem:

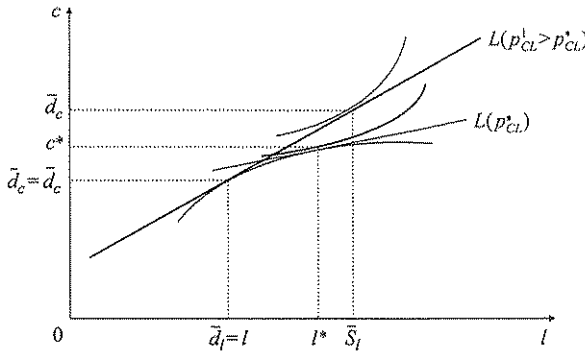


FIGURE 6.1 Involuntary unemployment as resulting from a signaling defect

When income appears as an independent variable in the market excess demand functions more generally when transaction quantities enter into the definition of these functions – traditional price theory ceases to shed any light on the dynamic stability of a market economy. (Clower [1965] 1984: 55)

Clower developed this argument in the last part of his paper, referring now to a simplified economy comprising only two commodities, one consumption good and labor. He kept the assumption that labor serves as the numéraire. This means that the quantities the auctioneer watches in order to reach equilibrium are the consumption good quantities. In the Walrasian case, the observation that the quantity of the good supplied is higher than the quantity demanded leads her to decrease its relative price in terms of labor until the quantities balance out. Clower argued that this will not happen in the Keynesian case. In his story, it turns out that the auctioneer declares that equilibrium has been reached after observing a balance between the supply of the good and the demand for it without realizing that this balance concerns the *notional* supply and the *effective* demand. As a result, the Walrasian allocation fails to be reached, the result of a defect in the signaling process. Figure 6.1 illustrates this.

The coordinates of the graph are quantities of goods (c) and labor (l). $L(p_{CL})$ simultaneously represents the firms' profit function and the households' budget constraint. p_{CL} is the price of the goods with labor as the numéraire. p_{CL}^* is the Walrasian price, l^* and c^* the Walrasian quantities. The convex curves are the households' indifference curves. The concave function is the production function. The auctioneer announces $p_{CL}^l > p_{CL}^*$. Observing what to her looks like a balance between the supply of c and the demand for it, she closes the auction, declaring that equilibrium has been reached.

An assessment

Clower's paper is a little gem of panache and originality. However, several criticisms can be leveled at it. To begin with, one must realize that Walrasian

theory is uncongenial to the dual decision hypothesis. This hypothesis makes sense in a Marshallian framework in which a sequential operation of markets is assumed, input markets taking place before goods markets, as in the latter supply consists of already produced goods. The Walrasian setup is different. All trades take place simultaneously and production starts only after equilibrium has been reached.

A second criticism is that Clower was probably too bold when he attacked such a sacred cow as Walras's Law. Two decades after the publication of Clower's article (at a time when nobody was interested any longer), in his entry on Walras's Law in the *New Palgrave Dictionary*, Patinkin found the chink in Clower's armor (Patinkin 1987). He argued that Walras's Law holds as long as it is related to excess-demand functions of the same type, be they notional or effective. Clower's mistake then was to have mixed the two types in the same equation.¹⁰

A third criticism concerns the signaling defect argument. Here, Clower's reasoning is *ad hoc* on two scores. First, his story implies that economic agents do not understand the working of the auction. Normally, they express quantities based on the Walrasian budget constraint, the only way which allows the auctioneer to do her job properly. Second, had Clower taken the commodity as numéraire, the Walrasian result would have been reached.

To his credit, Clower was the first person to perceive the limits of his model. Indeed, he soon recanted – according to his own testimony, even before the publication of the paper (Clower 1984: 266).¹¹ As touched on earlier, abandonin the Walrasian line, he decided to rally to Leijonhufvud. However like works of art, once models are made public, they take on a life of their own, independent from what their creators might come to think about them. Divergence about their usefulness may ensue. Clower's article has been a case in point. Regardless of Clower's own dismissal of his paper, some economists found that it blazed a trail worth exploring. This is the subject matter of my next chapter.

¹⁰ Cf. Rubin (2005).

¹¹ Clower related the reason for its abandonment more to the analytical difficulties arising when wanting to specify out-of-equilibrium behavior than to the matter of principle I just evoked. See Clower (1984: 266).

Non-Walrasian Equilibrium Modeling

Patinkin's book (with some delay), Clower's 1965 article and Leijonhufvud's book triggered a flow of new papers forming what was initially known as the 'disequilibrium macro' school – a label that was later questioned. The 1970s and the early 1980s were its heyday. Its main contributors included, in alphabetic order, Jean-Pascal Benassy, Robert Barro and Herschel Grossman, Jacques Drèze, Jean-Michel Grandmont, Guy Laroque, Edmond Malinvaud, John Muellbauer and Richard Portes, Takashi Negishi, Hal Varian, and Yves Younes.¹ The disequilibrium approach was mainly a European phenomenon, with French and Belgian economists taking the lead. They all shared the exhilarating feeling of participating in an innovative intellectual experience which, they believed, might change the course of macroeconomics. As Muellbauer and Portes put it:

The structure of this class of models is in some respects more complex than that of IS-LM. If it can be made equally familiar and easy to manipulate, however, it might be more likely to serve as an alternative framework. Once the basic principles of the interactions of markets in non-Walrasian equilibria are understood, one is on more secure ground when turning to the fascinating but difficult problems raised by the recent reappraisal of Keynes. (1978: 788)

This approach displayed three main features. The first was wanting to construct a microfounded (i.e., choice-theoretically based) analysis. Next, the task non-Walrasian equilibrium economists set out for themselves was to construct

¹ See Barro and Grossman (1971, 1976), Benassy (1975), Drèze (1975), Grandmont and Laroque (1976), Malinvaud (1977), Muellbauer and Portes (1978), (Negishi 1979). Benassy (1982, 1990, 1993), Grandmont (1977) and Picard (1993) provide good technical introductions. Weintraub (1979) remains a compelling methodological assessment. Backhouse and Boianovsky (2013) put non-Walrasian equilibrium models in a wider perspective.

general equilibrium models based on the assumption of rigidity and displaying involuntary unemployment as viewed by Patinkin, that is, with trade occurring off the labor supply curve. The justification for this endeavor was the belief that rigidity – or, in a lesser form, sluggishness – and involuntary unemployment were facts of life that could not be set aside. Remember that in standard Keynesian macroeconomics, rigidity is what distinguishes Keynesian theory from classical theory. The project that these economists pursued can be seen as an attempt at importing this Keynesian insight into the Walrasian language. Semantics is thus confusing: non-Walrasian equilibrium models fully belong to the Walrasian research program! The third feature is that, while non-Walrasian equilibrium economists found their inspiration in reading Patinkin, Clower and Leijonhufvud, the line they took departed from what these economists had in mind, namely to provide a *disequilibrium* explanation of involuntary unemployment. By contrast, non-Walrasian economists were striving to produce an *equilibrium* result, a radically different project. Hence their dissatisfaction with the early ‘disequilibrium’ label and shift to the non-Walrasian equilibrium label.

The chapter comprises two parts. In the first one, I study seminal contributions. The first is the Barro-Grossman model (1971), which aimed at bringing Patinkin’s and Clower’s theories together. This paper, which still used the disequilibrium terminology, spurred subsequent developments. Oddly enough, Barro and Grossman themselves soon became disillusioned with the lines of research they had pioneered. Next, I examine the contributions of Drèze and Benassy. These did not belong to the macroeconomics community. Rather, they were general equilibrium economists who decided to evoke macroeconomics themes. Finally, I turn to Malinvaud’s work. The role he played was to translate the results of previous abstract general equilibrium models in a way which was more familiar to macroeconomists, and to bring out policy implications. The second part of the chapter offers my assessment of non-Walrasian equilibrium modeling. Among other issues, I ponder the reasons explaining why, after a strong start, this approach subsided.

THE BARRO-GROSSMAN MODEL (1971)

Barro and Grossman’s aim in their 1971 paper, “A General Disequilibrium Model of Income and Employment” was to construct a synthesis of Patinkin and Clower’s contributions.² There was a good rationale for such an attempt: they were complementary. While Patinkin’s concern was profit maximization under an output constraint, Clower’s model was utility maximization subject to an employment constraint (Barro and Grossman 1971: 88). Unfazed by the

² They further elaborated their views in their 1976 book *Money, Employment and Inflation*, introducing, among other things, a dynamic perspective. As the book is mainly an expansion of the article, the study of the latter suffices for my purposes.

feud between Patinkin and Clower, Barro and Grossman worked in a pragmatic way. They abandoned Patinkin's governing idea that involuntary unemployment can only exist during the process of price formation, replacing it by an end-state analysis (thus replacing Patinkin's sluggishness with Clower's rigidity assumption). They also made much use of Clower's distinction between effective demand and notional demand, extending it to the supply aspect.

To make their Patinkin-Clower synthesis, Barro and Grossman constructed a simplified general equilibrium model, comprising three goods: labor, a commodity that is produced with labor, and a non-produced good. I will use the symbols l for labor, c for the commodity, for instance corn, and g for the non-produced good (g for gold, it being understood that the quantity of gold which exists cannot be changed).³ In this economy, three exchanges take place: the labor/corn exchange, the corn/gold exchange and the labor/gold exchange. By Walras's Law, the analysis can be led considering only two exchanges. Barro and Grossman decided to focus on the first two. They called the labor/corn exchange the 'labor market' (an ambiguous appellation as it will be seen presently) and used the variable w to designate the quantity of corn exchanged for one unit of labor. They called the corn/gold exchange the 'commodity market,' using the variable p to designate the quantity of gold exchanged for one unit of corn.

The aim of their paper, the main tenet of which is the spill-over effect present in both Clower's and Patinkin's papers, was to study what happens when prices differ from the Walrasian equilibrium price vector (i.e., are 'false prices' in Hicks's terminology). This led them to propose a typology of the different regimes in which the economy could end up: a Keynesian unemployment regime, a classical unemployment regime, and a repressed inflation regime. The Keynesian regime arises when $p > p^*$ and $w > w^*$ (where asterisks designate the equilibrium value): the economy features excess supply in the commodity and in the labor market. The classical regime has $p < p^*$ and $w > w^*$: the economy features excess demand on the commodity market and excess supply on the labor market. In the repressed inflation regime, with $p < p^*$ and $w < w^*$, there is excess demand on both the commodity and the labor market. In what follows, I limit myself to describing Barro and Grossman's analysis of the Keynesian regime.

In a flexible price and wage framework, agents' objective functions are described in notional terms. A firm's objective function is: $\pi = \bar{c} - w\bar{l}$, where π is profit, \bar{c} the notional supply of goods, and \bar{l} the notional demand for labor. Corn acts as the numéraire, profit is thus measured in quantities of corn. The production function is $c = F(l)$ with the standard properties. The notional demand for labor is:

$$\bar{l} = l(w) \quad \text{with} \quad \delta F / \delta l = w$$

³ They call the last good money, but this is misleading because their model refers to a barter economy.

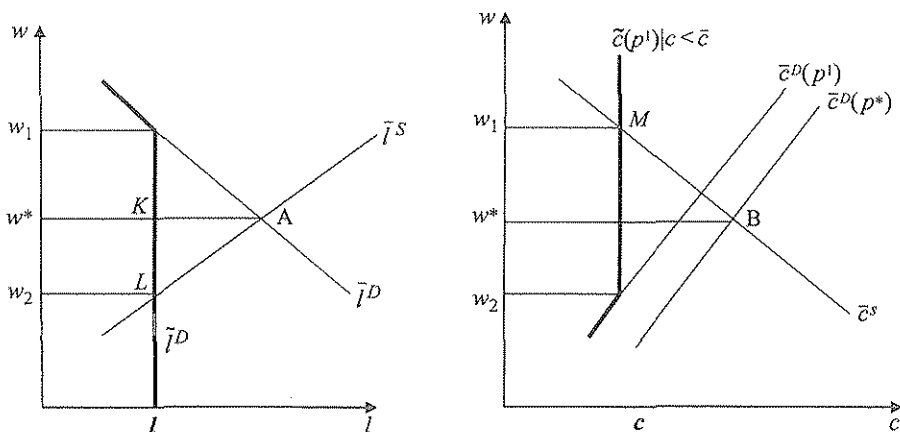


FIGURE 7.1 The Keynesian regime

Households' utility function is $U = (l, c, G + g)$, where G is the stock of gold held by households, and g their excess demand for it. Their budget constraint is:

$$\bar{c}^D + \frac{1}{p} \bar{g}^D = \pi + w\bar{l}$$

Things are different when assuming $p_I > p^*$. The consequences of this change in assumption can be studied with the help of Figure 7.1, close to one of Barro and Grossman's figure.

Barro and Grossman's analysis started with the Patinkin spill-over. The non-Walrasian allocation follows from assuming that one price is false ($p_I > p^*$). As a result, firms' sales of corn are constrained: $c < \bar{c}$, where c is the quantity of corn that is traded (as in Clower's paper, bold characters refer to constrained quantities). This result spills over to the labor market, the effective demand for labor (\tilde{l}^D) partially substituting itself to notional demand, (\bar{l}^D), as shown in the left panel of figure 7.1.⁴ Thus, firms are still implementing an optimizing program yet under a more constrained objective function ($\pi = c - w\tilde{l}^D$). Two outcomes are possible. The first one is that the labor market also features rationing, that is, short-side trading. This is what occurred in Patinkin's reasoning (I have transposed his Figure 3.2 allocation K into Figure 7.1). The second possible outcome is that the market closes with a matching between effective demand and notional labor supply, $\tilde{l}^D = \bar{l}^S$, as in point L on the left panel.

⁴ \tilde{l}^D starts from point $l = F^{-1}(c)$. It crosses \bar{l}^D at the height of w_1 , above which firms want to produce less than c to the effect that, from w_1 onward, notional and effective demand for labor coincide.

The next step is to incorporate the Clowerian spill-over in Patinkin's result. Following Clower, Barro and Grossman assume excess supply on the labor market. That is, the notional labor supply exceeds the notional demand for labor, so that short-side trade takes place ($l < \bar{l}^s$). This state of affairs impinges on households' budget function, which must now be expressed as:

$$\bar{c}^D + \frac{1}{p} \bar{g}^D = \pi + w_l.$$

In other words, households have to make optimizing decisions under a more stringent budget constraint. As a result, their notional demands are replaced by effective demands.

The right panel of Figure 7.1 describes the effect of labor rationing on the corn market. This is not the standard representation of a commodity market because the graph's coordinates are c and w instead of c and p . Yet p is present as a parameter of the notional demand functions. Two such functions are represented in the graph, one having p^* as its parameter, the other p_x . Since it is assumed that $p_x > p^*$, only the second one must be considered. These demand functions are upward sloping because of a combination of substitution and income effects.⁵ Consequently to labor rationing on the labor market, the effective demand for corn, a function of the wage, departs from the p_x -notional demand and is independent from the wage if the latter is above w_2 . When it is below w_2 , the notional and the effective demand coincide. The notional supply of corn is a decreasing function of the wage. Again, two outcomes are possible: either households end up being rationed a second time, or the effective demand for corn and its notional supply happen to balance, point M in the right panel.

Figure 7.1 also illustrates the range over which the spill-over of a false price on wages can extend. Its upper limit is w_x , the threshold from which the effective demand for labor departs from the notional demand for it. This wage marks the start of the divergence between the effective demand and the notional demand. w_2 is the lower limit: here, the labor market features a matching between the effective demand and the notional supply. Considering the two markets together, two occurrences are conceivable: either a twofold rationing (for p_x , at any wage between w_x and w_2), or a combination of market rationing on one market and a notional/effective matching (point L or point M) on the other one. In any case, the final allocation displays no generalized market clearing, even if this notion is broadened enough to allow a balance between a notional and an effective function.

This is Barro and Grossman's argumentation. As for the conclusion they draw from it, it runs as follows:

⁵ As the wage increases, leisure becomes more expensive and workers work and consume more, this increase in consumption resulting from a positive income effect.

The conclusion is that too high a real wage was not the cause of the lower employment and a reduction in the real wage is only a superficial cure. The real cause of the problem was the fall in commodity demand, and only a reflation of commodity demand can restore employment to the proper level. (Barro and Grossman 1971:86–87)

Beyond doubt, this result looks Keynesian. It should, however, not be forgotten that the Keynesian regime, the framework in which it arises, is only one of the possible regimes in which the economy can find itself.

An assessment

Barro and Grossman's paper is definitely ingenious.⁶ The main criticism against it is that the rigidity assumption has no foundation. In their article, Barro and Grossman assumed that there are price-taking agents, but they gave no hint as to how prices are formed and why they are rigid. This shortcoming was clarified in their book, and it is worth looking at the line they take there. As far as the flexible wage and price model is concerned, they admitted that prices are formed under the aegis of the auctioneer hypothesis. However, they claimed that, instead of being a convenient analytical device, this hypothesis dodges the issue of equilibrium formation (Barro and Grossman 1976: 38) – a point with which I fully agree. For someone like Leijonhufvud, this observation was sufficient to reject the Walrasian approach, but not for them. Instead of really getting rid of the auctioneer, they just assumed that she is unable to do her job fully. In other words, what led Leijonhufvud to condemn the Walrasian approach was transformed by Barro and Grossman into assuming that the *tâtonnement* process is blocked.

An additional criticism relates to their conclusion quoted above. A first claim Barro and Grossman made there was that wages are not responsible for involuntary unemployment since it is possible for it to coexist with the Walrasian real wage. A second one was that the policy conclusion to be drawn from the examination of the Keynesian regime is that demand activation is the proper action to be taken. I disagree with them on both scores. Regarding their first claim, the problem lies in their describing the economy in terms of markets, the 'labor market' and the 'corn market,' rather than in terms of exchanges. Using a single name to refer to a market makes sense in a partial equilibrium framework where the economy is monetary – the market for corn then relates to the corn-money exchange – but it is misleading in a general equilibrium framework. In the latter, one cannot conclude that, when w^* prevails (cf. the left panel of Figure 7.1), it means that the Walrasian wage exists. The reason is that there is no single Walrasian wage in their framework. What is shown in this left panel should be called p_{LC} , the relative price of labor in terms of corn. However, labor also has another relative price, p_{LG} , the price of labor in terms of gold.

⁶ A few years later, Mulbauer and Portes (1978) presented the same ideas in a more rigorous manner.

To have the fully Walrasian wage, the price achieved in this last exchange should be $p_{LG} = p_{LG}^*$. Since, by construction, the Walrasian allocation is not realized, this cannot be the case. Walras's Law admits a situation in which the Walrasian relative price exists in one of the three markets, while the other two exhibit false prices. Associated with $p_{CG} > p_{CG}^*$, there must be $p_{LG} < p_{LG}^*$. Thus, Barro and Grossman's statement that no false wage is required to have involuntary unemployment is incorrect.

It is also misleading to insist, as they do, on the existence of a deficiency in the demand for the good. The ultimate cause for their suboptimal result is the false price vector rather than a fall in commodity demand, as they claim. What needs to be done in order to change this result is to unlock the tâtonnement process, not demand activation as they declared in the quotation above.

DREZE'S 1975 MODEL

The aim of Drèze's paper "Existence of an Equilibrium under Price Rigidity and Quantity Rationing" (Drèze 1975) is to show that an economy in which prices are rigid still possesses an equilibrium solution making agents' optimizing plans compatible. To this end, Drèze constructed a model of an exchange economy in which the price vector is blocked at magnitudes known by the agents. Drèze's paper is a pure general equilibrium work. When writing it, his aim was not to contribute to macroeconomics.⁷ However, economists of the nascent non-Walrasian equilibrium research line soon claimed it as their own, possibly because it gave their approach serious general equilibrium credentials.

To describe this model, I borrow Picard's presentation (1993: 13-21), recasting it as a two-period overlapping generation model, with agents living for two periods. The economy comprises m agents, indexed by i , $i = 1, \dots, m$ and n goods indexed by h , $h = 1, \dots, n$. The numéraire good performs the monetary function of money. My purpose is to compare the *modus operandi* and outcome of Drèze's model with that of the canonical neo-Walrasian model. When prices are flexible, the equilibrium allocation is Walrasian, when they are rigid the allocation is non-Walrasian. The comparison bears on the temporary equilibrium state of the first period. It can be described using the indirect utility of money held at the end of the period, V_i , as an expected utility index.

In the Walrasian context, agents' maximization problem is as follows:

Max $V_i(z_i^1, M_i, p^1)$ with respect to z_i^1 and M_i subject to:

$$p_i^1 z_i^1 + M_i = M_{i0}$$

$$z_i^1 + \omega_i^1 \geq 0 \quad M_i \geq 0$$

⁷ Cf. Backhouse and Boianovsky (2013: 109-111).

where $z_i^t = z_{hi}^t(p^t)$ is agent i 's net demand vector in period $t = I$ for given endowments and money holdings. $p^t = (p_b^t)$ is the price vector in period $t = I$. M_{i0} is agent i 's money balances at the beginning of $t = I$ and M_i her money balances as present at the end of $t = I$ and to be used for consumption in period $t = 2$. $\omega_i^1 = \omega_{hi}^1$ is agent's initial endowment in period $t = I$.

A Walrasian temporary equilibrium is defined by the price vector, p^* , making the excess demand equal for all goods to zero:

$$\sum_{i=1}^m z_{hi}^t(p^*) = 0 \text{ for all } h.$$

The economy studied by Drèze is different. Its particularity is that agents face a personalized quantity constraint:

$$\bar{z}_i^t = (\bar{z}_{hi}^t) \in R_+^n \quad \text{and} \quad z_i^t = (z_{hi}^t) \in R^n,$$

where \bar{z}_{hi}^t is an upper bound limiting agent i 's purchases on market h , and $-z_{hi}^t$ is an upper bound limiting agent i 's sales on market h . These constraints on excess demand functions can also be expressed as $z_{hi}^t \leq z_{hi}^t \leq \bar{z}_{hi}^t$, where $z_{hi}^t \in R_+^n$, $\bar{z}_{hi}^t \in R_+^n$ and $z_{hi}^t \in R_-^n$ for $t = 1, 2$ and for all h . An agent is rationed on market h when her utility increases if the constraint limiting her trade is lifted. Agent i 's 'constrained excess demand' is noted $\hat{\xi}_i(z_{hi}^t, \bar{z}_{hi}^t)$.

In this fix-price context, agents' maximization problem is expressed as follows:

Max $V_i(z_i^1, M_i, \bar{z}_i^1, z_i^2)$ with respect to z_i^1, M_i , subject to:

$$p_b^1 z_b^1 + M_i = M_{i0}$$

$$z_{hi}^t \leq z_{hi}^t \leq \bar{z}_{hi}^t$$

$$z_i^1 + \omega_i^1 \geq 0 \quad M_i \geq 0$$

The equilibrium of this economy is defined as an array of net trades satisfying three conditions: (a) net trades maximize utility under quantity constraints, (b) they are mutually compatible, and (c) supply and demand for a given good are not simultaneously rationed. More formally, it consists of transactions z_i^* and quantity constraints \bar{z}_i^*, z_i^* for $i = 1, \dots, m$, such that:

$$(a) \quad z_i^* = \hat{\xi}_i(\bar{z}_i^*, z_i^*)$$

$$(b) \quad \sum_{i=1}^m z_i^*(p^*) = 0$$

(c) For all h , $z_{hi}^* = \bar{z}_{hi}^*$ for all least one i implies $z_{hj}^* \geq z_{bj}^*$ for all j ; and, $z_{hi}^* \geq z_{bi}^*$ for at least one i implies $z_{hj}^* < \bar{z}_{bj}^*$ for all j .

This result is a non-Walrasian equilibrium – an *equilibrium* in that it makes agents' plans compatible, a *non-Walrasian* equilibrium in that it differs from the Walrasian allocation that would have emerged had the price system worked.⁸

Drèze's paper demonstrates the existence of equilibrium without examining price formation. However, when this issue is addressed, the auctioneer figure becomes compelling.⁹ Tâtonnement now occurs in quantity instead of prices. That is, the auctioneer sends agents individual quantity signals. Agents react by sending back their constrained excess demands. Whenever the auctioneer registers a non-zero total excess demand for a given commodity, she either lowers the upper bound or increases the lower bound. This goes on until the excess demand for every good is zero.¹⁰

BENASSY

Like Drèze, Benassy had a general equilibrium background (Debreu was the supervisor for his dissertation). After having written his PhD on disequilibrium theory, Benassy became one of the main protagonists in the development of non-Walrasian equilibrium theory (Benassy 1975, 1976, 1977). Like Barro and Grossman, his motivation was to give stronger foundations to the Patinkin-Clower spillover idea:

An involuntarily unemployed worker will not maintain his Walrasian consumption demand, nor will a firm that is experiencing sales difficulties continue to employ the Walrasian profit-maximizing quantity of labor. (Benassy 1982: 41)

Benassy's distinctive feature compared to the other economists studied in this chapter is to have been Clower's closest follower. From Clower's celebrated article, "A Reconsideration of the Microfoundations of Monetary Theory" (Clower [1967] 1984), he drew the lesson that a barter economy (in which goods buy goods) and a monetary economy (in which goods buy money and money buys goods) should not be confused. For reasons of realism, he wanted the economy studied to be both monetary and decentralized, "a system of independently organized and decentralized markets in each of which goods are exchanged for money" (Benassy 1982: 17). He also took Clower's

⁸ A distinction must be drawn between Drèze's model and Jacques Drèze the model-building person. Although the 1975 model is non-Walrasian in a slight way, Drèze's subsequent work moved more radically away from Walras. In Drèze's eyes, his model was the first step of such a program. The later developments of his research program are expounded in Drèze (1993a, 1993b).

⁹ Drèze himself was silent about the presence of the auctioneer in his model. I, for one, like most commentators – see, for example, Grandmont (1977: 175) or Picard (1993: 21) – can see no other way of attaining equilibrium.

¹⁰ Cf. Grandmont (1977: 175).

dual-decision hypothesis in earnest, taking it as the starting point of his reasoning. In his 1965 article, Clower did not mention the auctioneer but, as I have argued in Chapter 6, his model requires it.¹¹ For his part, Benassy firmly wanted a model from which the auctioneer was absent. All this made for an ambitious program attempting, in Benassy's words, nothing less than constructing a synthesis between three paradigms, the Walrasian one, the Keynesian one, and imperfect competition (Benassy 1993: 732).

In various works, Benassy (1982, 1990, 1993) introduced the effective demand idea in a partial equilibrium framework, studying an isolated market where a final commodity is exchanged for money. In these models, a sequential approach is adopted: households first sell their labor, and the income so obtained is then spent on purchasing goods. Benassy assumed that labor suppliers are rationed, the result of a fixed nominal wage. As a result, workers' demand for goods is 'constrained.' All this is 'pure Clower.'¹²

However, Benassy's aim was to have a general equilibrium analysis. More precisely, he wanted to demonstrate the existence of a non-Walrasian equilibrium in an economy in which the price vector is fixed. As a result, his simplified models had to be modified substantially. First, the sequential approach had to be abandoned. Benassy assumed instead that trading of all goods takes place simultaneously (Benassy 1975: 504). He also confined his study to an exchange economy. Second, Benassy kept the dual-decision hypothesis. However, the context in which it works is quite different. In the sequential framework, the constraint refers to an exchange trade that is over, while in the simultaneous approach it pertains to agents' conjectures, their perception in Benassy's terminology, of market outcomes. In this new context, agent i 's effective demand on market b is a function of the constraints she perceives in all the other markets.

Although his model is a general equilibrium model based on the simultaneity assumption, Benassy studied the formation of equilibrium market by market. Let us consider what happens on market b . Agent i enters the market with an effective excess demand in the back of her mind, the result of her conjecture about what might happen on the other markets. At the end of the market process and assuming an equilibrium solution, market b supposedly closes with agent i having traded an optimal quantity of good b . This is true for all agents. The question is, how does one go from effective exceeds demand offers to equilibrium trading in market b ? Benassy uses the notation \tilde{z}_{bi} for agent i 's

¹¹ Later, Clower ([1975] 1984) lambasted the auctioneer hypothesis.

¹² "Effective demand (or supply) on one market is the trade which maximizes the agent's criterion subject to the usual constraints *and* to the quantity constraints on *other* markets. This definition thus naturally includes the well-known *spill-over effects*: We say indeed there is a spillover when an agent who is constrained to exchange less than he wants in a market because rationing modifies his demands or supplies in other markets" (Benassy 1993: 739; his emphasis).

effective demand for h , and z_{hi}^* for her ex-post equilibrium trading. Equilibrium requires the aggregate excess demand to be nil:

$$\sum_{i=1}^m z_{hi}^* = 0$$

There is no reason for this to be the case after all agents have expressed their effective demand. A specific trade technology, different from the auctioneer, must be used. Benassy's story is as follows.

At the beginning of market h , every household quotes its effective excess demand quantity. That is, agents make their effective demand public by sending signals to fellow traders (Benassy 1990: 118). Benassy assumed that agents are able to add up these individual trading offers, thereby mentally reconstructing the aggregate excess demand function. Chances are that it will be different from zero. Agents are able to realize that this is the case. They must also be aware of which side of the market will be rationed and to what extent. Benassy further assumed that agents have agreed in advance on a 'rationing scheme,' the aim of which is to transform inconsistent excess demands into consistent transactions.¹³ After completing the scheme, each agent is assigned her equilibrium trading quantity, z_{hi}^* . The rationing scheme can be expressed as a function $F_{hi} : R^m \rightarrow R$ such that:

$$z_{hi}^* = F_{hi}(\bar{z}_h, \dots, \bar{z}_{hm}) \text{ for all } i.$$

Every agent is then able to know the constraint affecting her on market h by comparing her effective demand with the quantity assigned to her by the rationing scheme. This constraint can be expressed in the same way as it is in Drèze's model, that is, $\underline{z}_{hi} \leq z_{hi} \leq \bar{z}_{hi}$. These bounds can be viewed as a function of total effective demand on market h :

$$\bar{z}_{hi} = \bar{G}_{hi}(\bar{z}_h) \text{ for all } i.$$

$$\underline{z}_{hi} = \underline{G}_{hi}(\bar{z}_h) \text{ for all } i.$$

The transition from the initial signal exchange to equilibrium occurs through agents revising their effective demand according to their perceived constraint. Equilibrium – a state in which the constrained optimizing plans have been made compatible – is achieved as soon as nobody finds it necessary any longer to modify their effective demand. Trade remains suspended until then.

This reasoning concerns the formation of equilibrium on one market. Getting to the general picture is a matter of defining \bar{z}_{hi} as the h -th component of vector $\bar{z}_i = (\bar{z}_{i1}, \dots, \bar{z}_{im})$, and solving for agent i 's following program:

¹³ I skip the conditions which the rationing scheme should satisfy (cf. Picard 1993: 22). In principle, a rationing scheme can either be 'manipulable' or 'non-manipulable,' but to all intents and purposes Benassy only works with non-manipulable schemes.

Max $V_i(z_i, M_i, \bar{z}_i, \underline{z}_i)$ with respect to z_i, M_i , subject to:

$$p_i^1 z_i^1 + M_i = M_{i0}$$

$$\underline{z}_{ki} \leq z_{ki} \leq \bar{z}_{ki} \text{ for all } k \neq i$$

$$z_i + \omega_i \geq 0 \quad M_i \geq 0$$

Turning to the economy as a whole, the vector of agent i 's effective demand can be expressed as a function of perceived constraints and written as:

$$\tilde{z}_i = \tilde{\zeta}(\bar{z}_i, \underline{z}_i)$$

The equilibrium of the economy can then be defined as follows:

A K -equilibrium (K for Keynes) is an array of trades z_i^* , effective demands \tilde{z}_i , and quantity constraints $\bar{z}_i, \underline{z}_i$, such that for all $i = 1, \dots, m$ and all $h = 1, \dots, n$:

$$(a) \quad z_{hi}^* = F_{hi}(\tilde{z}_h)$$

$$(b) \quad \bar{z}_{hi} = \bar{G}_{hi}(\tilde{z}_h)$$

$$\underline{z}_{hi} = \underline{G}_{hi}(\tilde{z}_h)$$

$$(c) \quad \tilde{z}_i = \tilde{\zeta}(\bar{z}_i, \underline{z}_i)$$

The equilibrium allocation is quasi the same as in Drèze's model.¹⁴ Benassy contended that this equilibrium has a Keynesian flavor. It is assumed that the fixed-price vector defines a Keynesian regime, one of the possible regimes as in Barro and Grossman. Like the latter, Benassy argued that, if some exogenous demand activation is undertaken, firms will be ready to sell more and an increase in social welfare will ensue.

Benassy also claimed that his sub-optimal equilibrium allocation result remains valid without the fix-price assumption. To this end, one needs to move away from perfect competition and adopt an imperfectly competitive framework. This is the third pillar of Benassy's synthesis. He assumed that agents on one side of the market are price-makers, those on the other side being price-takers. On the ground that goods ought to be distinguished not only by their physical characteristics, but also by the agent who sets their price, Benassy posited that each price-maker stands alone on her side of the market. As a result, monopoly theory can be used. It is assumed that firms are the quantity/price-making agents in goods markets. They will choose a point on the demand curve such that marginal revenue equals marginal cost. The standard result of sub-optimal output ensues. "There is thus 'excess supply' of the good, even though this excess supply is fully voluntary on the part of the price maker" (Benassy 1993: 741).

¹⁴ For further comments on this similarity, see Silvestre (1982) among others.

Let me conclude this presentation of Benassy's theory with a quotation where he summarizes the result of his theoretical endeavor:

As compared with the Walrasian approach, we have described in this article a more general and realistic view of the market process, where the exchange of information consists not only of price signals, but also of quantity signals generated in a decentralized fashion in each market by the agents' demands and supplies. All agents, including some explicitly modeled price makers, take these signals into account for their price-quantity decisions. The result is a more general formulation of demand and supply theory, as well as of price making, that takes full account of the intermarket "spillovers" generated by the quantity signals. (Benassy, 1993: 757)

Assessment

One thing that makes Benassy's contribution appealing is that he was unique among his fellows in taking Clower's views in earnest. The catch, however, is that Clower disowned him, quite harshly.¹⁵ As we have seen, Clower's post-recantation viewpoint is that Walrasian theory is radically unable to support a Keynesian vision. Yet this is also Benassy's stance. What then is the rationale behind Clower's complaint? A clue to understand it, I guess, is that Clower and Leijonhufvud must have felt that, while admittedly departing from the auctioneer assumption, Benassy remained nonetheless Walrasian in style and method because of his microfoundations approach, his high-brow mathematical reasoning, and his abstract, reductionist way of modeling. These are all dimensions of which Clower and Leijonhufvud were extremely critical.

My second comment is that Benassy's conception of the formation of equilibrium is reminiscent of Marshall's theory discussed in Chapter 1. In my eyes, he was unwittingly engaged in conceiving a Marshallian general equilibrium model. Of course, nobody would complain about a shift from Walrasian trade technology, in which the auctioneer makes prices, towards Marshallian trade technology, in which agents are price/quantity makers. However, if the auctioneer is a blatant *deus ex machina*, Marshallian trade technology hardly fares much better as it is based on the heroic assumption that agents have the data

¹⁵ "As for the dual-decision hypothesis, I gave it up, for the reasons indicated, before the 'Reconsideration' appeared (and also before the 'Counter-Revolution' was published). Imagine my astonishment when a virtually distinct branch of economic theory began to develop from the dual-decision hypothesis and from the surprisingly similar (but, to my mind, even less coherent) Patinkin model of constrained supply. I refer, of course, to the fix-price models of Barro and Grossman, Drèze, Negishi, Grandmont, Benassy, Malinvaud, Varian and other writers. Although I am an acknowledged 'grandfather' of all these 'babies,' I disowned them at the 1980 Aix-en-Provence World Conference of the Econometric Society as 'monsters' begotten by a father (the dual-decision process) whose paternity I admitted but whose character I deplored. I then gave my blessing to other babies – a motley lot, excepts for their distinctively Marshallian grins – describing them as well-formed off-springs of a fraternal twin of the father whose babies I just disowned" (Clower in Walker, 1984: 266).

and means allowing them to reconstruct mentally the equilibrium allocation of the entire economy they live in.¹⁶

The moral of my examination is two-pronged. On the one hand, Benassy ought to be credited, for having been able to conceive a trade technology different from the auctioneer story. On the other hand, his indirect way of reaching equilibrium leads to the same equilibrium allocation as Drèze's model. Hence one may wonder whether the detour was necessary.¹⁷

MALINVAUD

Malinvaud (1923–2015) was already a known scholar before becoming a contributor to non-Walrasian equilibrium modeling. His first source of fame was a 1953 article aiming at consolidating the bridge between capital theory and general equilibrium analysis (Malinvaud 1953). Moreover, he authored two influential books, *Statistical Methods of Econometrics* (1966) and *Lectures of Microeconomic Theory* (1972). As he explained in his interview with Alan Kruger, he adhered to the line opened by Barro, Grossman and the others because it provided what he believed was urgently needed, a framework allowing to explain “the respective roles of wage push shocks and aggregated demand shocks on changes in employment” (Krueger 2003: 191). His *Theory of Unemployment Reconsidered* (1977), based on his Yrjö Jahnsson lectures resulted from this awareness. It was one of the most discussed books in the second half of the 1970s. On the back cover of its 1985 paperback edition, one can read a statement that aptly summarizes Malinvaud's basic message: “the microeconomic theory of fixed-price general equilibrium has direct significance for the macroeconomic theory of involuntary unemployment.”

The difference between the economists studied until now in this chapter and Malinvaud bears on the level of abstraction at which they decided to proceed. The early ones created abstract neo-Walrasian models which generate outcomes some of which resemble the results that Keynesian economists wanted to highlight. In spite of this resemblance, their models were too rooted in neo-Walrasian general equilibrium to appeal to macroeconomists. For his part, like standard Keynesians, Malinvaud was more preoccupied with real-world issues and policy-making. To this end, it was necessary to make the non-Walrasian equilibrium conceptual apparatus more amenable to macroeconomists. This was his distinct contribution: acting as a middleman between highbrow

¹⁶ In an auctioneer economy, agents do not need to be knowledgeable about excess demand functions.

¹⁷ “In other words, under the above assumptions, the equilibrium trades obtained in an indirect manner by following the entangled procedure suggested by Benassy would precisely be the same equilibrium trades as would be obtained in a direct manner by following Drèze's procedure” (Donzelli 1989: 296).

TABLE 7.1 A typology of non-Walrasian equilibrium states

		Goods market	
		Excess supply	Excess demand
Labor market	Excess supply	Keynesian unemployment	Classical unemployment
	Excess demand	—	Repressed inflation

mathematical economists and leading macroeconomics figures such as Tobin and Solow.¹⁸

Malinvaud realized the explanatory potential of the typology of regimes alluded to in Barro and Grossman’s paper – classical unemployment, Keynesian unemployment, and repressed inflation. In his 1977 book, he investigated the characteristics of these categories, with special attention to their real-world relevance and policy implications. He showed that their combination could be neatly captured by the box diagram in Table 7.1.

According to Malinvaud, the Keynesian revolution was characterized by “a shift of emphasis from one type of short-run equilibrium [classical unemployment] to another type [Keynesian unemployment] as providing the appropriate theory for actual unemployment situations” (1977: 29). Malinvaud also claimed that this contrast has an immediate practical relevance since the policy to be undertaken radically changes depending on whether unemployment is classical or Keynesian. In a state of Keynesian unemployment, the best policy is to increase aggregate demand. This will lift firms’ constraints on their sales, which will in turn benefit employment. In a state of classical unemployment, the source of unemployment is firms’ low profitability. Real wages are too high. Decreasing them will restore profitability and employment.

To cure Keynesian unemployment, one should lower prices or raise wages. To cure classical unemployment, one should do precisely the reverse. This explains why debates on economic policy were so heated in the thirties, when most economists were more or less consciously thinking in classical terms, whereas a few others were already “Keynesians” without knowing it. (Malinvaud 1977: 66)

Diagnosing the nature of unemployment situations then became a central task for Malinvaud. He asserted that “Keynesian unemployment is much more frequent than classical unemployment. Casual observation shows this to be a fact” (1977: 77). Nonetheless, he admitted in 1976 that “such a situation [classical unemployment] built up progressively in the Western world. It may be

¹⁸ When Solow attended the NBER 1978 Bald Peak Colony Conference, he was invited to assess the U.S. macroeconomic policy in 1974–1975. The macroeconomic model he conceived of to do the job was directly inspired by Malinvaud (Solow 1980).

considered as partly responsible for the rather substantial level of unemployment that remained during the 1972–3 boom” (Malinvaud 1977: 108). However, he continued, “there is no doubt that the main features of the 1975 unemployment are again Keynesian” (Malinvaud 1977: 108).¹⁹

Malinvaud also played the role of spokesman for non-Walrasian equilibrium economists, and he was never shy to defend them methodologically, which led him to sparring with Lucas. His prevalent methodological attitude was one of realism and pragmatism. Many neo-Walrasian economists, for example, Hahn or Cass, believed that their paradigm provided a benchmark, a way of organizing one’s thoughts and of having counterexamples, but could hardly be considered a realistic representation of the market economy. This was not Malinvaud’s opinion. Though belonging to an approach that claimed a Walrasian heritage, he did not hesitate to follow suit with Marshall and Keynes in going back and forth between the model economy and reality. This happened, for example, when raising the question of whether real-world unemployment was classical or Keynesian (what implies that he had no qualms about thinking that theoretical categories had a direct observational counterpart). He also pulled non-Walrasian equilibrium modeling towards the neoclassical synthesis vision, writing for example:

It is clear that the labor market does not operate in this way [the market clearing ‘idealization’]. Wages are not flexible in the short term in the way assumed by this form of the law of supply and demand. They are not completely insensitive to pressure on the labor market, but they adjust much less than would be required for permanent market clearing. (Malinvaud 1984: 18)

FROM DISEQUILIBRIUM TO NON-WALRASIAN EQUILIBRIUM THEORY

The models studied in this chapter were launched under the name of disequilibrium theory. The seminal papers in which they were published referred to the works of Patinkin, Clower, and Leijonhufvud as their sources of inspiration. Still, after a few years, these authors realized that it was misleading to put their models under the disequilibrium label and decided for the ‘non-Walrasian equilibrium model’ terminology.²⁰ While the equilibrium characterization is apposite, the ‘non-Walrasian’ qualifier is infelicitous as it is true for every negative definitions. Defining oneself as non-Christian covers many possible standpoints on religion. All the models studied in this chapter and in the previous one, except Patinkin, can be called ‘non-Walrasian,’ but then one

¹⁹ See also Laroque (1986).

²⁰ For example, Benassy’s 1982 book, synthesizing the basic ideas of the approach was entitled *The Economics of Market Disequilibrium*, while eight years later an essay aiming at the same objective was entitled “Non-Walrasian Equilibria, Money, and Macroeconomics” (Benassy 1990).

must realize that there are degrees of 'non-Walrasianism.' The Clower-Leijonhufvud program is radically non-Walrasian as it is anti-Walrasian. The Barro-Grossman and Drèze models are moderately non-Walrasian. Benassy's model is more non-Walrasian than them, but less than Leijonhufvud and Clower's.

Be that as it may, the change in label was warranted because significant important differences exist between the disequilibrium and the non-Walrasian approaches. Both disequilibrium theorists (i.e., Patinkin, Clower, and Leijonhufvud) and non-Walrasian equilibrium economists (i.e., Barro and Grossman, Drèze, Benassy, and Malinvaud) claimed to have built a theory of involuntary unemployment but they conceived their claims in different ways. Disequilibrium theorists followed in Keynes's footsteps by taking the involuntary unemployment notion as two-pronged. From the point of view of the agents involved, it designates a state of individual disequilibrium – agents are unable to make their optimizing plan come through. From the point of view of what is going on in the labor market, it is characterized as trading off the supply curve or excess labor supply. To disequilibrium theorists, these were two faces of the same coin. This correspondence between individual disequilibrium and off-the-labor-supply trading is absent from non-Walrasian equilibrium models as their ambition was to demonstrate the coexistence of individual equilibrium and trading off the (notional) labor-supply curve. Indeed, the distinctive contribution of non-Walrasian equilibrium models is to have shown that the existence of fixprices does not rule out the possibility of a general equilibrium state. In other words, non-Walrasian equilibrium economists abided by what Lucas was to call the equilibrium discipline.

INVOLUNTARY UNEMPLOYMENT? MARKET NON-CLEARING?

In the previous section, I wrote that the aim of non-Walrasian equilibrium economists was to demonstrate the coexistence of generalized individual equilibrium and trading off the labor-supply curve. Phrased as it is, the last sentence will raise no highbrows. But replace 'trading off the labor-supply curve' with 'involuntary unemployment,' and the pairing of involuntary unemployment and individual equilibrium looks like an oxymoron. It definitely runs counter to Keynes's definition of involuntary unemployment (forced leisure). Hence, it must be that non-Walrasian equilibrium economists have implicitly shifted towards another understanding of involuntary unemployment. This is the case: in their models, involuntary unemployment is said to exist whenever individual equilibrium is attained subject to a non-standard, quantity-augmented, budget constraint. For my part, I see no reason to characterize such an occurrence as involuntary unemployment.

A similar ambiguity surrounds the market clearing/non-clearing distinction. To me, market clearing must understood as synonymous with generalized individual equilibrium, whatever the precise constraint under which the

optimizing plan is formed.²¹ Market non-clearing must then be viewed as a lack of generalized individual equilibrium (i.e., when some agents are unable to achieve their optimizing plan). However, this is not the line taken by non-Walrasian economists. According to them, it suffices that the content of the budget constraint changes for giving the resulting new allocation the market non-clearing label. Again, I find this definitional standpoint contrived as it has not no other rationale than to give the theory a Keynesian flavor.

AN ABORTED TAKEOFF

At the beginning of this chapter, I quoted a passage from Muelbauer and Portes expressing the wish that non-Walrasian equilibrium modeling would become the prevailing approach in macroeconomics. This did not happen. After a quick and promising start, especially in Europe, the new approach lost its momentum. Many of the young researchers who started their career in this line moved on to other areas of research. What explains this aborted takeoff? Was it due to some intrinsic flaws or to more extraneous reasons? This is the last question that I wish to tackle in this chapter.

Of course, criticisms were leveled at the new approach. Actually, a rather unusual event took place as Barro and Grossman, the two economists who had taken the lead in launching it, soon recanted. Barro (1979) explained his reasons in an article entitled "Second Thoughts on Keynesian Economics." He stated that the virtue of the private market system lies in its ability to exhaust trades mutually advantageous to the exchanging parties. He claimed that the flaw in non-Walrasian equilibrium models is that they "mechanically" leave "opportunities for mutually desirable trades" and, additionally, make "government policy activism much too easy to justify" (Barro 1979: 56). Although he did not use the famous metaphor of the \$500 notes lying on the sidewalk, it was all about that. To me, this argument is hardly convincing because it fails to separate propositions pertaining to reality and to the fictitious model economy constructed by economists. In a fix-price auctioneer-led economy (as in Drèze's model), or a fix-price agent-led economy (as in Benassy's model), at the close of the exchange, all mutually advantageous trade opportunities have been exploited, taking the features ascribed to the model economy, including the rigidity hypothesis, into account. For his part, Grossman came to think that implicit contract models did a better job than non-Walrasian equilibrium models in explaining unemployment without falling prey to what he saw as the flaw of these models, namely their inability to provide a convincing rationale for the persistent restriction on transactions they involved (Grossman 1979).

²¹ In McKenzie's words: "General equilibrium implies that all subsets of agents are in equilibrium and in particular that all individual agents are in equilibrium" (McKenzie 1987: 498).

Another criticism was voiced by Lucas in his book *Models of the Business Cycle* (1987: 52–53). He pointed out the oddity of resorting to the theoretical artifact of the auctioneer while arbitrarily preventing her from doing the job for which she was created. Shrewdly enough, the strategy behind Lucas's criticism was to underline the limitations of his own research line, the neo-Walrasian approach, namely, its inability to come to grips with unemployment. Although to the point, Lucas's criticism holds only with respect to models that conform to the auctioneer hypothesis (or use it with only a modicum of change). This is the case for Patinkin's theory, for the Barro-Grossman model and for Drèze's 1975 model, but not for Benassy's model. Moreover, Drèze quickly moved away from this hypothesis. Thus, Clower and Leijonhufvud, on the one hand, and Drèze and Benassy, on the other, are not concerned by Lucas's criticism because their models comprise no auctioneer.

The reason for the demise of non-Walrasian equilibrium models might lie elsewhere. I believe that three elements played a role. The first is that while the pioneering contributions studied above succeeded in setting out a new framework, no precise view emerged about what to do next: falling back on standard Keynesian macroeconomics, or making new original advances? The second element relates to the motivation of these economists, trying to make Keynesian theory more rigorous by anchoring it in Walrasian theory. Their endeavor was the same as Patinkin's, establishing the neoclassical synthesis *program*.²² Unfortunately for them, if this endeavor is understood as wanting to introduce involuntary unemployment into Walrasian theory, whether the attempt was Patinkin's, or that of non-Walrasian equilibrium economists, it failed. This failure is not an accident, it reveals something deeper, namely that there is no room for this notion in Walrasian theory. The ultimate lesson to be learned from both attempts is that Leijonhufvud was right, Keynes and Walras are irretrievably incompatible bedfellows. The possibility of such a deadlock may have dawned on them with a demoralizing effect.

The third, and in my eyes most important, factor is that the 1970s were years of high theory. Non-Walrasian equilibrium modeling was hardly the only new theoretical development that arose in macroeconomics. Another breakthrough surfaced under Lucas's lead, new classical macroeconomics. Lucas and his followers proved to be daunting rivals, all the more so because both approaches were very close. In this battle, non-Walrasian equilibrium lost out to DSGE macroeconomics. It may be conjectured that it was because the Lucasian program was more attractive, in particular because it allowed engaging in dynamic analysis in a more serious way than before while non-Walrasian equilibrium models remained basically static. This is a better explanation for Barro's change

²² Like Patinkin, they did not use this expression, leaving it to those who held the Keynes-Walras juxtaposition viewpoint.

of mind than what he wrote in his 1979 article.²³ My guess is that he was not the only one to have found Lucas's new line more appealing. Thus, the demise of non-Walrasian equilibrium models ought to be understood in the light of the ascent of Lucasian macroeconomics. However, the real game changer was RBC modeling, spearheaded by Kydland and Prescott. It stabilized the Lucasian revolution into a well-defined research program with a strong applied component, opening up new horizons for score of young macroeconomists. The non-Walrasian equilibrium economists were left with two options: jump on this bandwagon, or be left behind.

All things considered, it is nonetheless misleading to speak of a defeat, or a failure, of the non-Walrasian equilibrium approach. In one way or another, any 'victorious paradigm' will always be, if not dethroned, at least endogenously transformed. What is true is that one road has been traveled while the other has not. However, this hardly means that the non-Walrasian equilibrium economists' motivation of anchoring Keynesian theory onto microfoundations principles would find no other outlets somewhere down the road. An unexpected supporter of this revival viewpoint was the late Grossman, who on November 11, 2001, sent me the following email in answer to my query about his opinion on the fate of the "disequilibrium approach":

I do not know that what you call the disequilibrium approach was ever abandoned. But, even if it was abandoned at one time, it has now [become] the universally accepted paradigm. Everybody is a Keynesian now even those, like Lucas and Barro, who at one time seemed sure that they had a good neoclassical alternative to Keynesian models. Let me be clear how I use the term Keynesian. Keynesian economics has two distinctive features: (1) The factual observation that monetary policy, and other factors that affect aggregate demand, also affect real economic activity, not only prices and wages. In short, Keynesian economics starts with the observation that money is not neutral at the business-cycle frequency. (2) The theoretical hypothesis that the observed non-neutrality of money results from the stickiness of nominal wage and/or prices and manifests itself in the failure of markets to clear in response to purely nominal disturbances.

²³ This interpretation is drawn from Barro's 2005 interview with *The Region*, the magazine of the Federal Reserve Bank of Minnesota.

Assessment

In this brief chapter, I draw a few lessons from my analysis of Keynesian macroeconomics.

THE LIMITS OF MY STUDY

The time span studied in the previous chapters, from Keynes's *General Theory* to the beginning of the 1970s, witnessed the birth and impressive development of macroeconomics as a new sub-discipline of economics. It was centered on a new modeling strategy and anchored in a vibrant scientific community with its own particularities and style. I readily admit that the picture given in the previous chapters is too sketchy a representation of what was going on. Indeed, I have devoted as much space, that is, a chapter, to individual economists – Patinkin, Leijonhufvud, and Clower – as to Keynesian macroeconomics, to which hundreds of economists were contributing. Moreover, the result of my focus on theoretical developments, I have also somewhat left aside the empirical side of the field in spite of its central role in the development of the discipline.

CONTRASTS

The title I gave to Part I, Keynes and Keynesian Macroeconomics, may be misleading, because the models I have studied are far from homogeneous. *The General Theory*, Keynesian macroeconomics, the works of Patinkin, Clower and Leijonhufvud, and non-Walrasian equilibrium models all pursued the same aim: demonstrating the existence of involuntary unemployment and supporting a demand activation policy. Therefore, they all deserve the Keynesian label. Yet they sharply differed in their methods. A first benchmark against which they can be contrasted is the Marshall-Walras divide. *The General Theory*, IS-LM macroeconomics, and the Clower-Leijonhufvud program belonged to the

TABLE 8.1 *Alternative views of the Keynesian program*

	Marshallian approach	Walrasian approach
Involuntary unemployment as an end-state occurrence	<i>The General Theory</i> IS-LM macroeconomics	Clower 1965 Barro and Grossman, Drèze, Malinvaud
Involuntary unemployment as existing only during the adjustment toward equilibrium process		Benassy Patinkin
Involuntary unemployment as a phenomenon of blocked adjustment	Leijonhufvud Clower (after 1965)	

Marshallian approach; Patinkin, Clower's 1965 article, and non-Walrasian equilibrium models to the Walrasian approach. Moreover, among non-Walrasian equilibrium economists, Benassy stands apart: his trade technology is Marshallian but his methodological principles are Walrasian. Another useful benchmark is whether involuntary unemployment is regarded (a) as an end-state occurrence (i.e., a state of rest) the arising of which is deemed instantaneous, or (b) as existing only during the equilibration process (disappearing when equilibrium is reached), or (c) as a phenomenon of blocked adjustment. Table 8.1, which completes Table 6.1, summarizes how the models studied fare with respect to this benchmark.

What about the two other economists studied in this part of the book, Friedman and Phelps? They have in common to have blazed the trail for the natural rate of unemployment concept. However, their views differed significantly. For sure, Friedman had no Keynesian inclination; he was interested neither in demonstrating involuntary unemployment, nor in commending aggregate demand activation. Nonetheless, he shared a Marshallian affiliation with traditional Keynesian macroeconomists. Also, he had no qualms using the IS-LM framework. Phelps was less anti-Keynesian than Friedman. What made him different from all the other economists studied, including Friedman, was his perception that a different representation of the functioning of markets was a prerequisite to introduce the unemployment concept in the theoretical lexicon.

A CONCEPTUAL MESS

Let me now address Keynesian macroeconomics specifically. One trait of Keynesian economists that must be underlined is their pragmatism, non-Walrasian equilibrium models being the exception. They gave little thought to definitions

and did not bother with drawing neat distinctions. Nor did they feel a need to state clearly whether a given proposition pertained to the real world or to a fictitious model economy. Over time, such an attitude led to a series of ambiguities.

Keynes's aim when writing *The General Theory* was to use the Marshallian theoretical apparatus to demonstrate the existence of a suboptimal labor market outcome taking the specific form of involuntary unemployment. At first sight, such a project did not look particularly daunting, but on closer scrutiny it was.

To begin with, a sub-optimal outcome can take two forms, unemployment or underemployment. Unemployment refers to agents who are searching for a job. They want to participate in the labor market under existing conditions, yet are observed non-participating. Heterogeneity thus exists between the employed and the unemployed. Underemployment refers to a sub-optimal number of hours worked, with the assumption that these hours are equally distributed across the labor force. No heterogeneity is involved. Assuming a representative agent is possible when addressing underemployment; it is not when studying unemployment. Unemployment and underemployment, thus, cannot be taken as synonyms. To put it more technically, unemployment exists along the extensive margin of labor supply (the number of individuals employed), underemployment along its intensive margin (the average number of working hours).

A second ambiguity is that Keynes was interested in involuntary unemployment as distinct from other types of unemployment, in particular frictional unemployment. The passage of time has shown is that involuntary unemployment concept is a tricky concept. Keynes regarded it as an occurrence wherein are agents unable to achieve their optimizing plans (what I have called 'involuntary unemployment in the individual disequilibrium sense'). Keynes and Keynesian macroeconomists (Hicks included as will be argued later in the book) took it for granted that the demonstration of its existence had to be made using the Marshallian supply and demand framework. I have argued that this research line is a dead-end. The only way in which involuntary unemployment can be produced in this framework is by assuming an exogenously given wage floor, a far cry from Keynes's initial intuition. An alternative understanding of the involuntary unemployment exists, which I have dubbed as 'involuntary unemployment in the casual sense.' It refers to agents who, though experimenting an optimizing position, want to move away from it as soon as possible. Such an unemployment experience is at the same voluntary and involuntary.³ This is the line that search theory has taken thereby departing from the standard Marshallian trade technology.

³ Aristotle's testimony can be invoked in support of this view. In a passage of *The Nichomachean Ethics* he discussed whether given actions are involuntary or involuntary using the example of sailors taken in a storm and compelled to throw goods overboard in order to avoid the wreckage of their boat. "In the abstract, Aristotle wrote, no one throws goods away voluntarily, but on

A third ambiguity concerns the notion of full employment to which Keynes gave pride of place in his *General Theory*. 'Full employment' is more a descriptive than a theoretical notion, and importing it into the theoretical language proved problematic. The simplest way of doing this is to assume that it is just another name for an optimal labor market outcome (in present-day terminology, the natural rate of unemployment). But why using it then? Moreover, the full employment concept is equivocal because its opposite, the lack of full employment, can mean either involuntary unemployment or underemployment.

A fourth problem is that Keynesian macroeconomists, with the exception of Patinkin, hardly reflected on the meaning of the concepts of flexibility, rigidity, and sluggishness and on the differences between them. They regarded these differences as a matter of degree and defended their use of the wage rigidity assumption on the grounds that wage rigidity was just an extreme case of sluggishness, the latter deemed to be a compelling fact of life. The following statement by Tobin illustrates this:

Keynesian macroeconomics neither asserts nor requires nominal wage and/or price rigidity. It does assert and require that markets not be instantaneously and continuously cleared by prices. That is a much less restrictive assumption, and much less controversial. (Tobin 1993: 46)

To me, the matter is more complicated. To begin with, we must ask ourselves whether the discussion about the notions of rigidity, sluggishness, and flexibility relate to reality or the theoretical universe. As far as reality is concerned, it is plausible to consider that the difference between the three notions is a matter of degree. At one extreme, there are goods the price of which changes daily (equity), from one month to another (vegetables), or from one year to the next (labor). The first can be called flexible, the second sluggish, and the third rigid. However, these observations cannot be transposed to the theoretical universe. In the latter, flexibility means the instantaneous adjustment of equilibrium, sluggishness a time-taking attainment of equilibrium, and rigidity an exogenous impediment to this attainment. We have seen in the previous chapters that in both Marshallian and Walrasian theory, flexibility is assumed as premise as far as period-of-exchange price formation is concerned. Hence, sluggishness is automatically excluded as an explanation for rationing in general and for unemployment in particular. Underemployment is then the only way of reaching a Keynesian outcome. Against, this background, Tobin's view that the rigidity assumption can be taken as a proxy for sluggishness, which I surmise

condition of its securing the security of himself and his crew any sensible man does so. Such actions, then, are mixed, but are more like voluntary actions; for they are worthy of choice at the time they are done, and the end of an action is relative to the occasion. . . . Such actions, therefore, are voluntary, but in the abstract perhaps involuntary; for no one would choose any such act in itself" (Aristotle 1980: 48-49).

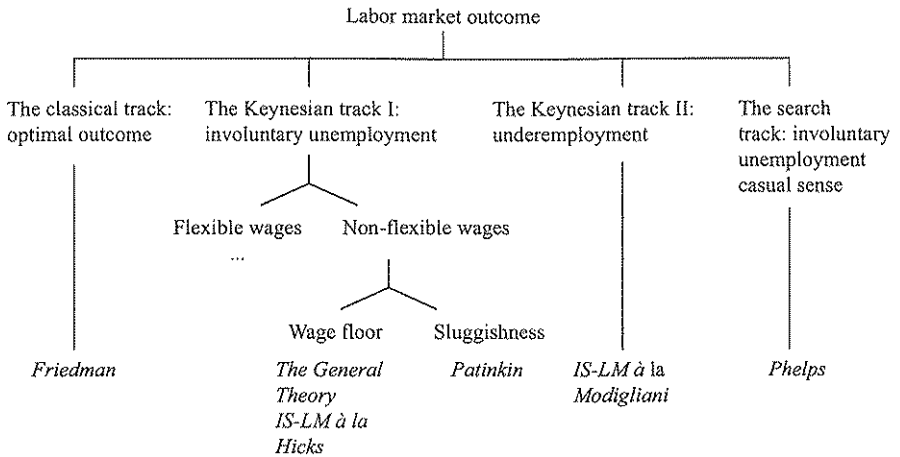


FIGURE 8.1 A decision-tree representation of the early years of macroeconomics

most Keynesian macroeconomists were ready to endorse, looks botched: rigidity cannot be a proxy for sluggishness because the relation of degree between the two notions which is valid for the real world cannot be extended to the theoretical world. In the latter, the choice is either between rigidity (the wage floor) or flexibility.

WHAT HAS KEYNESIAN MACROECONOMICS ACHIEVED?

Figure 8.1 is a decision-tree summary of the achievements of Keynes in *The General Theory* and of the economist studied above leaving non-Walrasian economists aside.

The tree describes what I think economists achieved, not necessarily what they claimed they achieved. As argued in Chapter 1, Keynes claimed he demonstrated the existence of involuntary unemployment under flexible wages, but in fact his analysis rested on the wage floor assumption. The same is true for Keynesian IS-LM models *à la* Hicks. Here, the wage rigidity assumption was explicitly endorsed as the theoretical translation of real-world sluggishness. As for the IS-LM model *à la* Modigliani, I have argued that its object is underemployment. I have also concluded that, while the underemployment track proved to be promising, its formulation as found in Modigliani's paper is wanting, full employment being defined as the maximum rather than the optimal number of hours worked. In Chapter 3, I showed that Patinkin's disequilibrium explanation of involuntary unemployment stumbled on the wealth effect. When it comes to explaining unemployment in its standard understanding of joblessness, among the

different authors studied in the previous chapters, the only one who really delivered was Phelps.

CONCLUDING REMARKS

At the turn of the 1970s, Keynesian macroeconomics seemed to be a sturdy theoretical construction, a powerful engine for the analysis of the aggregate economy and a performing instrument of prediction. And it was. Yet, behind the appearance of strength, it was fragile. Phelps's Titanic metaphor quoted in Chapter 5 was as relevant for the whole of Keynesian macroeconomics as for the Phillips curve topic in particular. On the theoretical level, it was hampered by the pervasive conceptual ambiguities I have pointed out. On the empirical side, Keynesian macroeconomic models, though they did a fairly good predictive job, were too big and badly identified, and the benign neglect that their lack of microfoundations was deemed to be turned out to the source of a more dramatic defect, as Lucas and Sims were to bring out.

PART II

DSGE MACROECONOMICS

Lucas and the Emergence of DSGE Macroeconomics

In the early 1970s Robert Lucas launched the rational expectations revolution with a series of papers. ... Ever since then macroeconomics has never been the same (Mishkin 1995: 1).

I will devote three chapters to the study of Lucas's contribution to business cycle theory.¹ In the present one, I give an in-depth account of the theoretical journey through which Lucas evolved "from an attempted contributor to Keynesian macroeconomics to that of severe critic ..." (Lucas, 1981a: 2).² In Chapter 10, I bring out the different dimensions in which his approach to macroeconomics differs from the Keynesian one. In Chapter 11, I present my personal assessment of Lucas's contribution to macroeconomics.

The transformation of macroeconomics that Lucas initiated had all the trappings of a Kuhnian scientific revolution: a shift in the type of issues addressed, a new conceptual toolbox, new mathematical methods, the rise to power of a new generation of scholars. Such a shift in paradigm is always collective. Among its main protagonists and in addition to Lucas's name, those of Barro, Thomas Sargent, and Neil Wallace definitely need to be mentioned. However, I will focus my attention on Lucas, not only because he was the

¹ Later Lucas turned his attention to growth theory, but I will not discuss this part of his work.

² To address these tasks, I have drawn from several scattered sources: (a) the numerous articles in which Lucas explained his intellectual journey: his introduction to the *Studies in Business Cycle Theory* volume (Lucas 1981a) and the various methodological papers contained in that volume; (b) his Professional Memoir (Lucas 2001), his "My Keynesian Years" (2004) lecture; (c) the numerous interviews in which he commented on his own work (Klamer 1984; *The Margin*, 1989; *The Region*, 1993; Snowden and Vane 1998; Usubiaga Ibanez 1999; McCallum 1999); and last, but not least, the Lucas archives held at Duke University Rare Book, Manuscript, and Special Collections Library (Lucas. Various).

leading character in the plot but also because he commandingly assumed the role of methodological spokesperson.³

Lucas's work has a substantive and methodological component. Substantively, it is geared toward explaining business fluctuations in their generality, thus excluding more dramatic episodes such as great depressions. The purpose underlying his study is to allow better-founded discussions of economic policy. As for methodology, in their review of Lucas's *Models of Business Cycles*, Rodolfo Manuelli and Sargent aptly captured the gist of Lucas's contribution by noting that it consisted in setting up "particular sets of rules and techniques to model aggregative economic observations" (Manuelli and Sargent 1988: 523). These rules, which amounted to reviving basic neoclassical principles, acted as standard setters, discriminating between up-to-the-standard and sub-standard models. Prominent among them are a general equilibrium perspective, dynamic analysis, the rational expectations assumption, the microfoundations requirement, market clearing, stochastic shocks, and a procedure for empirical assessment. Finally, another requirement is that macroeconomics should evolve under a single set of principles, which amounted to a rejection of the neoclassical synthesis. More generally, Manuelli and Sargent also underlined "a desire to be intellectually conservative in the sense of preserving as much contact as possible with a quantity theory tradition that emerged from pre-Lucas modeling rules" (Manuelli and Sargent 1988: 526).

TERMINOLOGY

A preliminary terminological clarification bearing on the whole research program spearheaded by Lucas is necessary. For a while, Lucas's papers on business cycle were branded as marking the birth of "monetarism mark II." This terminology made sense in that it underlined the similarity in purpose between Lucas and Friedman, yet it hid the deep methodological breach between them (see Hoover 1984). It was soon abandoned in favor of two others, referring to the 'rational expectations school' and the 'new classical school' (or 'revolution'). The first of these two labels aptly brings to the fore a central modification that took place. The second evokes the rehabilitation of the classical approach trail-blazed by Lucas – as Samuelson wrote, "The new classical economics of rational expectationists is a return with a vengeance to the pre-Keynesian verities" (1983: 212). However, all in all, the best way of describing the new approach is 'dynamic stochastic general equilibrium' (DSGE) modeling, a label that Narayana Kocherlakota commented as follows:

Dynamic refers to the forward-looking behavior of households and firms. Stochastic refers to the inclusion of shocks. General refers to the inclusion of the entire economy.

³ An early assessment of Lucas's contribution, which is still worth consulting is Hoover (1988).

Finally, equilibrium refers to the inclusion of explicit constraints and objectives for the households and firms. (Kocherlakota 2010: 9)

These four elements are all landmarks of the revolution that Lucas initiated. Hence it is appropriate to call the new program that it generated the ‘DSGE program.’ There is however a semantic problem. To date, this program has evolved in three successive stages: ‘new classical macro,’ RBC modeling, and a third stage usually labeled either ‘new Keynesian macro’ or ‘DSGE macro.’ Both labels are troublesome. The DSGE name cannot at the same time designate the research program in general and one particular form that it has taken. My choice is to use it for the general program. However, I will not give its last installment the mere ‘new Keynesian’ name. Later, I will argue that two generations of new Keynesian models must be distinguished. Hence, the name I will resort to for the last installment is ‘second-generation new Keynesian’ modeling.

THE FORMATION OF LUCAS’S THEORETICAL FRAMEWORK

Lucas’s pre-macroeconomist years

Having majored in history at the University of Chicago, Lucas started his graduate studies in history at Berkeley. In his *Professional Memoir* (Lucas 2001), he recounted that, while writing an essay on nineteenth-century British business cycles for a course in economic history, he came to realize that he needed some economic background (which he totally lacked) to understand the subject fully. This led him to abandon history and shift to economics, not at Berkeley, “where the economics department was not encouraging,” but back in Chicago (2001: 6). To make up for his lack of knowledge in economics, he taught himself by reading Samuelson’s *Foundations of Economic Analysis*.⁴ This put him on the track on which he would remain for the rest of his life, but it also led him away from the Chicago tradition because, in Lucas’s terms, Marshall was “the god of Chicago economics” (2001: 8). By contrast, Lucas’s fascination with mathematical theory *à la* Samuelson tilted him toward Walras, an economist who was not popular in Chicago, castigated as he was by Friedman and Stigler.

Nonetheless although he was drawn to abstract theory, in his early years as an economist Lucas engaged mainly in applied work. The subject of his dissertation was capital-labor substitution in U.S. manufacturing. After getting a position at the Graduate School of Industrial Administration of the

⁴ “I loved the *Foundations*. Like so many others in my cohort, I internalized its view that if I couldn’t formulate a problem in economic theory mathematically, I didn’t know what I was doing. I came to the position that mathematical analysis is not one of the many ways of doing economic theory: it is the only way. Economic theory is mathematical analysis. Everything else is just pictures and talk” (Lucas, 2001: 9).

Carnegie Institute of Technology in 1963, he worked on the modeling of firm and industry dynamics following Jorgenson's approach to investment decisions.

In his memoir, Lucas emphasized that, when he was studying at the University of Chicago (as well as at the beginning of his time at Carnegie), he invested little time in studying macroeconomics. However, he stated that he had no antagonism toward Keynesian economics:

Everyone from Chicago is a Friedman student in some very basic sense, but in terms of macro, I claim that the credentials I'm describing are true-blue Keynesian. When I was done with my graduate education, how did I think of Keynesian economics? I didn't think about it very deeply, to tell you the truth. It wasn't my field. I didn't picture myself as doing research in the area. But I certainly thought of myself as a Keynesian. (2004: 19)

What Lucas adhered to in Keynesianism was its applied part, econometric modeling: "So, when I think of Keynesian economics or at least the Keynesian economics I signed on for, it was part of this econometric model-building tradition" (2004: 22).⁵ A similar testimony of the revolutionary-to-be's state of mind can be found in Sargent's reminiscences. In a 1996 article written for the twenty-fifth anniversary of Lucas's "Expectations and the Neutrality of Money," he wrote that "the 1960s were a good time to be a young macroeconomist because the air was so charged with new ideas, mentioning amongst other things distributed lags, cost of adjustment, rational expectations, portfolio theory, the natural rate of unemployment, and the optimal quantity of money" (Sargent 1996). Important progress in making macroeconomics models dynamic was appearing. Economists' conversations were dominated by the controversies between Keynesians, led by Tobin, and monetarists, under Friedman's lead. In a 1998 interview with Sent, Sargent opposed the state of mind of those of his kind with that of monetarists. The latter, he stated, were "people trying to knock off and destroy the Keynesian tradition from the outside, who weren't sympathetic enough to it to learn it." The topics on which Sargent and others, in contrast, were working "were all pieces of a Keynesian model, a high-tech Keynesian style." "The paradox, he continued, is that what's ended up being perceived as the most destructive in Keynesian tradition is from its own children" (Sargent interviewed by Sent 1998: 165-166).

⁵ Here is how he later described his state of mind in an interview with Snowdon and Vane: "Lucas: Keynesian models in the 1960s, and this is what excited people like Sargent and me, were operational in the sense that you could quantify the effects of various policy changes by simulating these models. You could find out what would happen if you balanced the budget every year, or if you increased the money supply, or changed fiscal policy. That was what was exciting. They were operational, quantitative models that addressed important policy questions" (Lucas's interview by Snowdon and Vane 1998: 131).

Lucas and Rapping's 1969 paper, "Real Wages, Employment, and Inflation"

The turning point in Lucas's research interests came when, triggered by the intense discussions about the Phillips relationship that were taking place in the 1960s, Leonard Rapping and him wrote a joint paper that became the trail-blazing "Real Wages, Employment, and Inflation" article.⁶

Its aim was to do for the wage-employment sector what Baumol and Tobin had done for the demand of money, Friedman and Modigliani for consumption, and Eisner and Jorgenson for investment decisions. "We viewed ourselves as constructing a model of the 'wage-price sector,' potentially suitable for combining with other models of other 'sectors' to provide a model of the entire economy" (Lucas 1981a: 6). At the time, Lucas and Rapping were unaware that this paper would be the starting point of a process that was to put macroeconomics on a radically new path. In Lucas's retrospective papers, he recurrently claims that their aim at the time was to enrich the foundations of Keynesian theory rather than to challenge them.⁷ As far as expectations were concerned, the paper was conventional since it rested on the notion of adaptive expectations.

However, the paper was original on two counts. First, it broke with traditional macroeconomics by adopting a Walrasian microfoundations perspective and by introducing the Lucas-Rapping supply function (to be detailed below). Lucas and Rapping depicted labor suppliers as rational optimizing agents engaged in intertemporal leisure substitution – for example, working more when current wages are high relative to expected future wages (see Box 9.1). The second striking feature of Lucas and Rapping's model was its depiction of the labor market as always being in a state of market clearing for a given period of exchange. This move caused a big stir, not only because it dispelled the possibility of involuntary unemployment, but also and more broadly because they focused on labor supply instead of demand for it, thereby departing from traditional thinking in macroeconomics.

Whether or not Lucas and Rapping were aware of it when they started working on their paper, the fact is that it set them (actually only Lucas, because Rapping soon lost interest in the subject) on a collision course with Keynesian macroeconomics:

We thought of ourselves as engaged in a collective project which engaged efforts of many during 50's and 60's – providing 'microeconomic foundations' for Keynesian macroeconomic models. Many viewed 'wage-price' sector as last frontier in this effort. ...

⁶ Ramalho (2013) is a fine account of the formation of Lucas ideas.

⁷ See Lucas (1981a: 3) and Lucas (2004: 20).

BOX 9.1 The notion of intertemporal elasticity of substitution

The notion of intertemporal elasticity of leisure substitution is a cornerstone of DSGE macroeconomics.⁸ It originated in the Arrow-Pratt measure of relative risk aversion explaining how optimizing agents react to changes in the real interest rate. A risk-averse household dislikes big intertemporal variations in wealth. The same is assumed to be true for consumption. Therefore, under certain conditions, households decide to smooth their consumption over time. Their choice will depend on the relation between the real interest rate, r , and their utility discount rate, δ . Were $r = \delta$, households would decide for a falling consumption plan. Whenever $r > \delta$, the consumption plan will either fall less or increase over time. Although the intertemporal elasticity of substitution notion had already gained pride of place in general equilibrium theory and in capital theory, Lucas and Rapping were probably the first to apply it to labor supply. Subsequent developments have shown that this change had far-reaching consequences.

Of special interest is the case when the relative risk aversion is constant. This is the case with the constant elasticity of substitution (CES) utility function:

$$\begin{aligned} u(c) &= \frac{1}{1-\theta} c^{1-\theta} \quad \text{for } 0 < \theta < 1 \text{ and} \\ u(c) &= \ln c \quad \text{for } \theta = 1 \\ u'(c) &= c^{-\theta} \end{aligned}$$

Call c_1 good c at date t_1 and c_2 its consumption at date t_2 . We then obtain:

$$\frac{u'(c_1)}{u'(c_2)} = \left(\frac{c_2}{c_1} \right)^\theta$$

Solving for c_2/c_1 , one finds:

$$\frac{c_2}{c_1} = \left(\frac{u'(c_1)}{u'(c_2)} \right)^{1/\theta}$$

Assume that c is leisure, $\sigma = 1/\theta$ is the constant intertemporal elasticity of leisure substitution. When σ is high, the agent will accept big variations in leisure.

⁸ See Bliss's entry in *The New Palgrave Dictionary, Second Edition* (2008).

Turned out that this work, in conjunction with similar efforts of others, was deeply subversive of Keynesian macroeconomics. [We] were led to complete rejection of this line and its policy implications, obliged to search for quite different ways of thinking about business cycle. Surprisingly, search led back to old-fashioned pre-Keynesian theories – but without rejection of modern analytical methods. (extract from a talk given by Lucas at Princeton in 1979, Lucas. Various. Box 22)

“Expectations and the Neutrality of Money” (1972)

Lucas’s seminal article is a development of the Lucas-Rapping 1969 paper, retaining the market clearing premise as well as the need to think intertemporally, and hence to use the notion of intertemporal substitution as the model’s cornerstone. Its motivation was twofold. First, Lucas wanted to give a stronger vindication of the claim made by Friedman in his Presidential address. To him, the Friedman paper was, simultaneously, worth supporting and in need of improvement. Second, under Phelps’s impulsion, Lucas realized that partial equilibrium could not do and that a general equilibrium framework had to be adopted.⁹ The “Expectations . . .” article was the outcome of such an endeavor.

Lucas’s model is an overlapping generation model in a Phelps island economy set-up and with agents living for two periods. There is one perishable good, produced by the young generation yet consumed both by young and old agents. The young are self-employed. They acquire fiat money, which does not enter the utility function, by selling the good to the members of the old generation, and spend it to purchase goods when old. The overall size of the population is fixed with an equal proportion of young and old people. Young agents are stochastically distributed across the islands, whereas old ones are equally distributed across them. Production decisions depend on the relative price of the good. Two types of uncorrelated stochastic shocks are introduced – a nominal and a real shock. The nominal shock arises because the members of the older generation receive a variable beginning-of-period money transfer proportional to their pretransfer holdings of money. The real shock results from the fact that trade is supposed to take place in separate places, Phelps’s, ‘islands,’ each organized under the auspices of an auctioneer. As a result, when young agents happen to be on an island with a proportionally low young population (thus facing a higher per capita demand), they will produce more and consume less (and vice versa), it being assumed that the substitution effect dominates the income effect. The concomitant occurrence of the two shocks results in young agents’ inability to distinguish between them, since the available information bears on their joint effect. Therefore, they need to enter into a ‘signal extracting’ process. Its solution implies that they will always

⁹ See Lucas’s interview with Snowdon and Vane (1998: 126).

alter their production, possibly in a minimal way. Thus, in this framework there is, as argued by Friedman and Phelps, a positive relationship between the rate of inflation and the rate of employment yet, as will be explained below, no possibility of exploiting this relation policy-wise.

One of the paper's features that drew most of the attention was the rational expectations assumption. The notion had been introduced by Muth in a partial equilibrium framework in an article published in 1961 (Muth 1961). In this article, Muth challenged the informational asymmetry in forecasting capacity between the model-builder and the agents in the model, as it existed in the cobweb model. With Muth working at Carnegie, Lucas was acquainted with the rational-expectations assumption at an early date, but, initially, he saw no way of exploiting it. But this changed gradually as it became a cornerstone of the Lucas program, to the point of triggering the 'rational expectations school' label.¹⁰ This assumption declares that agents' expectations with respect to a given variable ought to be consistent with the predictions of the theoretical model: "the forecasts made by the agent within the model are no worse than the forecasts that can be made by the economist who has the model" (Sargent 1987: 76).

A simplified version of Lucas's model is as follows.¹¹ The economy comprises a number of islands, each identified by z . Producers' supply function is defined as:

$$y^S(z)_t = \beta_0 + \beta_1 [p(z)_t - E_t(z)p_t] \quad (9.1)$$

where all variables are defined in natural logarithms. $y^S(z)_t$ is the quantity of output supplied by young agents on market z in period t , β_0 and β_1 ($\beta_1 > 0$) are coefficients; β_0 is the economy's steady-growth output path. $p(z)_t$ is the price of output on market z in period t . $E_t(z)p_t$ is market z producers' beginning of period expectations about the economy-wide average price of output, p_t .

In every market, the equilibrium price is reached through the services of an auctioneer. In a static model, agents can act automatically on the price signal. Here, agents need to interpret its meaning. To grasp the content of equation (9.1), consider the case of an island where young agents, observing the price prevailing on their market, realize that it is higher than their expectation about the economy-wide price. In a perfect foresight context (that is, were the combined shocks absent), they could easily conclude that this means that

¹⁰ Lucas and Prescott had already used the rational expectations assumption in their joint paper, "Investment Under Uncertainty" (Lucas and Prescott 1971). Sargent and Wallace also toyed with it. Sargent used it in a paper published the same year as Lucas's (Sargent 1972). Sargent (1987) is a fine presentation of rational expectations.

¹¹ In what follows, I take up Attfield, Demery, and Duck's pedagogical presentation of Lucas's model (1991, chapter 4).

they face a favorable relative demand; hence, increasing their output is optimizing behavior. However, in the case at hand, the matter is more complicated.

Next, I turn to the demand side equations.

$$y^D(z)_t = \alpha_0 + m_t(z) - E_t(z)p_t \quad (9.2)$$

$$m_t(z) = m_t + \varepsilon_t(z) \quad (9.3)$$

$$y^D(z)_t = \alpha_0 + m_t - E_t(z)p_t + \varepsilon_t(z) \quad (9.4)$$

$$m_t = m_{t-1} + g + v_t \quad (9.5)$$

Equation (9.2) is the demand on market z . α_0 is a coefficient. $m_t(z)$ refers to the quantity of money held by consumers on the z -th market. Equation (9.3) indicates that changes in nominal money holdings on market z depend on variations in the economy-wide money stock and on a stochastic element, ε_t , with mean zero and constant variance σ_ε^2 . Equation (9.4) combines (9.2) and (9.3). Equation (9.5) describes the average money variations as decided by the government (or the central bank). It has a fixed component, g , and a stochastic one, v_t . The latter is a random, serially uncorrelated, error term with zero mean and constant variance σ_v^2 .

Equation (9.6) expresses the equilibrium condition on every island.

$$y^S(z)_t = y^D(z)_t \quad (9.6)$$

The equilibrium price on the z -th market is obtained by combining equations (9.1) and (9.4–9.6):

$$p(z)_t = \frac{1}{\beta_1} [(\alpha_0 - \beta_0) + m_{t-1} + g(\beta_1 - 1)E_t(z)p_t + v_t + \varepsilon(z)_t] \quad (9.7)$$

As rational expectations imply that agents know the structure of the economy, the solution of $p(z)_t$ must be elicited before being able to write an expression for the expectations term. On the basis of a few assumptions that I will not describe, this solution can be expressed as in equation (9.8):

$$p(z)_t = \pi_0 + \pi_1 m_{t-1} + \pi_2 g + \pi_3 v_t + \pi_4 \varepsilon(z)_t \quad (9.8)$$

Equation (9.8) shows that the size of $p(z)_t$ depends on the coefficient assigned to five variables. These are known to producers, except for two of them, the aggregate nominal shock ($\pi_3 v_t$) and the local relative price, $\pi_4 \varepsilon(z)_t$. Producers' problem is that they are only able to observe their combined effect, $(\pi_3 v_t + \pi_4 \varepsilon(z)_t)$. If they knew that ε_t is equal to zero, they could interpret any change in nominal demand as resulting from a nominal shock and they would stay put, having no money illusion. If, on the contrary, they were sure that v_t is equal to zero, they would fully react to a change in nominal demand. This is the signal extracting problem they need to solve. To this end, they may base themselves on their knowledge of past occurrences and calculate the ratio of each parametrized

variance to their combined variance. This ratio will be called γ when the nominal shock is of concern:

$$\gamma = \frac{\pi_3^2 \sigma_v^2}{\pi_3^2 \sigma_v^2 + \pi_4^2 \sigma_\varepsilon^2}.$$

The following two expressions can be derived:

$$\begin{aligned} E_t(z)\pi_3 v_t &= \gamma[\pi_3 v_t + \pi_4 \varepsilon(z)_t] \\ E_t(z)\pi_4 \varepsilon(z)_t &= (1-\gamma)[\pi_3 v_t + \pi_4 \varepsilon(z)_t] \end{aligned}$$

Thus, if in the past the variance of the nominal shock represented a high proportion of the combined nominal and real shocks, producers will assign more of the period disturbance to the nominal shock and act in consequence. Further developments permit to express aggregate output as in equation (9.9):

$$y_t = \beta_0 + \frac{\beta_1 \sigma_\varepsilon^2}{(\beta_1 \sigma_\varepsilon^2 + \sigma_v^2)} v_t \quad (9.9)$$

This equation indicates that only the surprise component of monetary growth affects output, the very claim made by Friedman. It also indicates that the higher the volatility of money growth, the lower its impact on real output.

Lucas's model permits to confirm Friedman's intuition about the inefficiency of Keynesian policy. To this end, equation (9.9) can be rewritten as:

$$y_t = \beta_0 + \phi v_t \quad (9.10)$$

where $\phi = \frac{\beta_1 \sigma_\varepsilon^2}{\beta_1 \sigma_\varepsilon^2 + \sigma_v^2}$, $\phi > 0$.

Drawing from (9.5), (9.10) can be rewritten as:

$$y_t = \beta_0 + \phi(m_t - m_{t-1}) - \phi v_t$$

Defining $\lambda_x = \phi$ and $\lambda_0 = \beta_0 + \phi g$

we get:

$$y_t = \lambda_0 + \lambda_x(m_t - m_{t-1}) \quad (9.11).$$

Equation (9.11), a reduced-form equation, could be interpreted as meaning that a change in money supply causes a change in output. This interpretation will be made if λ_0 and λ_x are (mistakenly) taken as fixed behavioral parameters. Assume that on the basis of this belief, the government engages in monetary expansion in order to increase the level of activity by the rate h . Thus (9.5) can be transformed into the following equation:

$$m = m_{t-1} + g + h + v_t$$

To grasp agents' behavior, the λ_0 parameter must be deconstructed. As a result of an increased rate of money creation, its value is now $= \beta_0 - \phi(g+h)$ to the effect that the output will remain unchanged, all this under the condition that

agents have rational expectations and that h becomes common knowledge at once. Hence the government will fail in its aim of increasing the level of activity.

“An Equilibrium Model of the Business Cycle” (1975) and
“Understanding Business Cycles” (1977)

Lucas’s “Expectations and the Neutrality of Money” had two follow-ups. The first one was his 1973 article “Some International Evidence of Output-Inflation Tradeoffs” (Lucas [1973] 1981a). After offering a simplified version of his 1972 model, Lucas proceeds with testing one of its possible predictions, namely that the real effect of variations in money supply will vary with the variance of monetary shocks. The greater this variance, the smaller the real effect of money supply shocks. To this end, Lucas compared annual time series from eighteen countries over the years 1951–1967. Sixteen of these countries had rather stable prices paths, two of them (Argentina and Paraguay) volatile prices. The difference in the effect of unanticipated demand shifts on real output, Lucas argues, is startling. Thus, he claimed, the results of his 1972 paper were verified.

The second and decisive follow-up was the extension of his 1972 model into an explicit equilibrium analysis of the business cycle (Lucas [1975] 1981a). This involved less innovation because the 1972 expectations model was already about an economy undergoing “what is in some sense a business cycle” (Lucas 1981a: 8). To Lucas, the possibility of such an easy (conceptually but not technically) extension of the earlier model was a testimony to its potential.¹² Lucas’s basic general insight was that business fluctuations – auto-correlated variations in output, hours worked, consumption, investment, prices, profits, and interest rates – are movements around a trend that can be described by a stochastically disturbed difference equation of very low order (Lucas [1977] 1981a: 217). His specific model claimed that unanticipated monetary shocks were the cause of these movements. As in the “Expectations...” paper, the condition for their occurrence was that production and trade take place in physically separated markets, and that agents have imperfect information.

Lucas’s 1977 article “Understanding Business Cycles” (Lucas [1977] 1981a) is a pedagogical exposition of his 1975 technical article. The story goes as follows. A self-employed producer has to make intertemporal production choices while confronted with a complex signal-extracting problem. On a given market day, she observes an increase in the demand for her product. The puzzle that she must solve in order to optimize her intertemporal consumption path is whether this shock is transient or permanent, it being assumed that capital is fixed. If the shock is deemed transitory, she will engage in intertemporal substitution of leisure; if permanent, she will stay put. Whenever changes in

¹² “Our intention is ... to extend the equilibrium methods which have been applied in many economic problems to cover a phenomenon which has so far resisted their application: the business cycle” Lucas and Sargent ([1979] 1994: 28).

capital equipment enter the picture, the opposite rule surfaces: increase the stock of capital goods if the assumedly positive shock is permanent, do not move if it is transitory. The matter is further compounded by the fact that investment decisions have a delayed effect. An investment decision may turn out to have been erroneous, but the additional capacities generated cannot be scrapped. Their presence will delay the needed adjustment. The second signal processing entering the picture bears on sorting out whether the change faced is real (a change in relative prices) or nominal (a change in the general price level). As in the 1972 model, if agents had perfect information about the states in the world, their decision would be easy. With imperfect information, the solution they choose will be a mix between two extremes (changing their behavior or staying put), their respective weight depending on the values which prevailed in earlier exchanges.

The mere fact that Lucas was able to construct an equilibrium model of business fluctuations was a feat.¹³ Before that, business fluctuations had surely been a central object of study but formal modeling was deemed inadequate. Lucas demonstrated the contrary. Moreover, his model featured surprising implications. First, disequilibrium is totally absent from it. Second, unemployment is also absent from it. What is investigated is the fluctuations in the level of activity, that is, the total number of hours worked rather than their allocation across agents. A third and last feature is that the earlier judgments made about the necessarily harmful character of business cycles vanish from the picture. No malfunctioning of the system is associated with fluctuations. Hence the earlier received view that the state should intervene in order to mitigate fluctuations vanishes as well.

LUCAS'S APPRAISAL OF KEYNES'S CONTRIBUTION TO ECONOMIC THEORY

Parallel to the shaping his own research program, Lucas grew increasingly critical of Keynesian macroeconomics and by extension of Keynes's *General Theory*. In this section, I expose his grievances toward Keynes, in the next one the reason for his dissatisfaction with Keynesian macroeconomics.

Lucas has been abundantly interviewed. Each time he was asked about his opinion on Keynes, the same negative assessment came out. To Klammer (1984), he freely admitted that *General Theory* was a book he disliked (1984: 50). Over

¹³ Even economists who cannot be suspected of an inclination towards Lucas's views, such as Leijonhufvud, acknowledged it: "By the early thirties, business cycle theorists had come to realize that use of the equilibrium toolbox could be strictly justified only for stationary and perfect foresight processes. This pretty much excluded business cycles – and there was no other toolbox. Keynes's new method successfully *evaded* this dilemma. Lucas's new method attempts to *solve* it" (Leijonhufvud 1983: 184).

the years, he did not change his mind, as shown by the following extract from his lecture, "My Keynesian Education":

[Keynes's real contribution] is not Einstein-level theory, new paradigm, all this. . . . I think that in writing *The General Theory*, Keynes was viewing himself as a spokesman for a discredited profession. . . . He is writing in a situation where people are ready to throw in the towel on capitalism and liberal democracy and go with fascism or corporatism, protectionism, socialist planning. Keynes's first objective is to say, "Look, there's got to be a way to respond to depressions that's consistent with capitalist democracy." What he hits on is that the government should take some new responsibilities, but the responsibilities are for stabilizing overall spending flows. You don't have to plan the economy in detail to meet this objective. . . . So, I think this was a great political achievement. . . . [Keynes] was a political activist from beginning to end. What he was concerned about when he wrote *The General Theory* was convincing people that there was a way to deal with the Depression that was forceful and effective but didn't involve scrapping the capitalist system. Maybe we could have done without him but I'm glad we didn't have to try. (2004: 24)¹⁴

Lucas's criticism goes beyond Keynes as a person, his feeling being that at the time economics in general was still underdeveloped. Although he did not use this terminology, he might have argued that economics was still evolving at a pre-scientific level, since it mainly consisted of verbal discussions, as opposed to the hard scientific status it would gain when it started to be equated with mathematical modeling. To Lucas, Keynes's *General Theory* was definitely on the side of the pre-scientific *modus operandi*. He regarded it as a rambling verbal exposition, eliciting endless hermeneutic discussions – "disconnected qualitative talk" as stated in Lucas and Sargent ([1979] 1994: 6).

Concerning Lucas's more specific criticisms of *The General Theory*, the following points are worth mentioning. First, Lucas criticized Keynes for abandoning his initial aim of constructing a theory of the business cycle by redirecting his attention to the apparently simpler issue of explaining the existence of involuntary unemployment at one point in time (Lucas [1977] 1981a: 215, [1980] 1981a: 275). Lucas's second indictment of Keynes bore on his abandonment of the market-clearing assumption and its underpinning by what he calls the 'equilibrium discipline,' the decision to depict agents as behaving in an optimizing way.

After freeing himself of the straightjacket (or discipline) imposed by the classical postulates, Keynes described a model in which rules of thumb, such as the consumption function and liquidity preference schedule, took the place of decision functions that a classical economist would insist be derived from the theory of choice. (Lucas and Sargent [1979] 1994: 15)

¹⁴ See also Lucas (1995: 916–917) and his interview with Usabiaga Ibanez (Usabiaga Ibanez 1999: 180).

According to Lucas, Keynes's departure from the equilibrium discipline was an example of "bad social science: an attempt to explain important aspects of human behavior without reference either to what people like or what they are capable of doing" (Lucas, 1981a: 4). Although understandable in view of the apparent contradiction between cyclical phenomena and economic equilibrium, this lapse caused a long detour in the progress of economic theory.

A third conceptual criticism addressed by Lucas to Keynes, is that his analysis was based on ill-defined and basically useless concepts, in particular those of involuntary unemployment and full employment. In his eyes, it would have been better had these concepts never been introduced in the theoretical lexicon.

What is the excuse for letting his [Keynes's] carelessly drawn distinction between voluntary and involuntary unemployment dominate aggregative thinking on labor markets for the forty years following? ([1978] 1981: 242)

LUCAS ON KEYNESIAN MACROECONOMICS

While Lucas was strongly dismissive of Keynes's work as well as of those economists who defend the consistency of *The General Theory*, his attitude towards Keynesian economics, as different from the economics of Keynes, was more complex. Let me remind the reader that Keynesian economics is the IS-LM interpretation of *The General Theory* as inaugurated by Hicks and fixed by Modigliani, as far as the theoretical model is concerned, and made empirical by Klein and Goldberger. Lucas praised this last extension, judging that it was a huge breakthrough, the transformation of economic theory from verbal modeling into modern science. To him, this was progress:

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 times series behavior of actual economies. The Keynesian macroeconomic models were the first to attain this level of explicitness and empirical accuracy; by doing so, they altered the meaning of the term "theory" to such an extent that the older business cycle theories could not be viewed as "theories" at all. (Lucas [1977] 1981a: 219)

In his most articulated methodological paper, "Methods and Problems in Business Cycle Theory" (Lucas [1980] 1981a), Lucas pondered the factors that may explain the rise of equilibrium business-cycle models. He deemed three to be of importance: technical developments, outside events and the internal development of the discipline. In his opinion, the first of these, which historians of economics tend to neglect, plays a crucial role. Progress in macroeconomics, in Lucas's view, has mainly been a matter of discovering or applying new tools, new techniques for treating old issues.

I see the progressive development in economics as entirely technical: better mathematics, better mathematical formulation, better data, better data-processing methods, better

statistical methods, better computational methods. I think of all progress in economic thinking, in the kind of basic core of economic theory, as developing entirely as learning how to do what Hume and Smith and Ricardo wanted to do, only better: more empirically founded, more powerful solution methods, and so on. (Lucas, 2004: 22)¹⁵

Two technical innovations were particularly important: the use of new mathematical tools, borrowed from engineering sciences; and the increased computational ability associated with the tremendous progress that had taken place in computer science, paving the way for large-scale simulation work. It is their absence that explains earlier theoretical dead ends. Taking Keynes's *Treatise on Money*, the aim of which was to construct a theory of the business cycle, as an example, Lucas noted that:

The difficulty is that Keynes has no apparatus for dealing with these problems. Though he discusses them verbally as well as his contemporaries, neither he nor anyone else was well enough equipped technically to move the discussion to a sharper or more productive level. (Lucas [1980]1981a: 275)

As for new developments "thrown at us by the real world" (Lucas [1980] 1981a: 272), Lucas disliked giving them too much importance because this would go against his premise that all business cycles are basically alike.¹⁶ There was nonetheless one external influence to which he referred time and again, the stagflation episode in the 1970s. Lucas considered it as a quasi-laboratory experience that allowed sorting out which of the two competing interpretations was more successful, the stable Phillips curve trade-off model, defended by the Keynesians, and the natural-rate-of-unemployment model put forward by Phelps and Friedman. The latter, he claimed, won the contest hands down:

Keynesian theory is in deep trouble, the deepest kind of trouble in which an applied body of theory can find itself: It appears to be giving serious wrong answers to the most basic questions of macroeconomic policy. Proponents of a class of models which promised 3½ to 4½ percent unemployment to a society willing to tolerate annual inflation rates of 4 to 5 percent have some explaining to do after a decade such as we have just come through. A forecast error of this magnitude and central importance to policy has consequences, as well it should. (Lucas, 1981b: 559–560; Lucas's emphasis)

Lucas believed that the stagflation episode condemned Keynesian theory on an empirical basis. But he also wanted to highlight its methodological and theoretical flaws. I propose to organize this aspect of his criticism into four themes: (a) the 'Lucas critique', (b) a critique of Keynesian macroeconomics as being policy without theory, (c) the dismissal of the neoclassical synthesis viewpoint, and (d) the fall of the 'Keynesian consensus.'

¹⁵ See also Lucas ([1980a] 1981: 275), Lucas (1987: 2), and Lucas (1996: 669).

¹⁶ Lucas admitted that the Great Depression remains a "formidable barrier to a completely unbending application of the view that business cycles are all alike" (Lucas [1980] 1981a: 273).

The 'Lucas critique'

The implementation of any economic policy consists in using instruments, such as government expenditures, the interest rate, tax policy, and so on, to attain the objectives of the chosen policy. To this end, public authorities must have an idea of the magnitudes involved – for example, knowing at which level the interest rate should be set to stabilize the economy. Macroeconometric models are necessary to make these decisions. In his landmark article, "Econometric Policy Evaluation: A Critique" (Lucas [1976] 1981a), Lucas claimed that, while Keynesian models did a fairly good forecasting job, they were a total failure as far as the assessment of alternative policies was concerned.¹⁷

All these models had a Keynesian inspiration, formalizing the major Keynesian macroeconomic functions, consumption, investment, the demand for labor, the demand for money, and so on. While initially small, Keynesian macroeconometric models had become more and more sophisticated, ending up with hundreds of equations. This made it difficult to give consideration to general equilibrium issues. Nor did these models bother to base the evolutions observed on economic agents' optimizing behavior. Taking a pragmatic approach – that is, a more data- than theory-constrained approach – the modelers were mainly aiming at accounting for the observed facts as best as possible. Theoretical specifications were transformed and simplified in order to obtain relations, in general linear or log-linear, whose parameters had been the object of econometric estimation.

This state of affairs stirred Lucas to make what came to be called the 'Lucas Critique.' The best summary of it can be drawn from the conclusion of the article in which he expressed it:

This essay has been devoted to an exposition and elaboration of a single syllogism: given that the structure of an econometric model consists of optimal decision rules of economic agents, and that optimal decision rules vary systematically with changes in the structure of series relevant to the decision maker, it follows that any changes in policy will systematically alter the structure of econometric models. (Lucas [1976] 1981a: 126)

Lucas's critique stated that the econometric practice of his time was of no help for confronting the results of alternative policies, in as far as models regarded the estimated coefficients of the model as independent from the policy regime. Their reduced-form status leads to transforming endogenous variables, sensitive to variations in economic policy, into exogenous variables. This was due to their limiting themselves to recording the past decisions made by agents without going deeper and explore the agents' objective functions. Thereby, they missed

¹⁷ Sargent's unpublished paper, "Is Keynesian Economics a Dead End"? (Sargent 1977), is a little known yet excellent discussion of the Lucas Critique.

the fact that agents change their decisions when faced with a change in the policy regime. As a result, a model of the economy estimated at a period during which a particular regime holds sway can only provide inadequate information for assessing what might occur under a different one. Only ‘deeply-structural models,’ derived from the fundamentals of the economy, are able to provide a robust grounding for the evaluation of alternative policies.¹⁸

The Lucas critique had a tremendous impact on the profession.¹⁹ As noted by Duarte and Hoover, it was a turning point: “each macroeconomic methodology after the mid-1970s has been forced to confront the central issue that it raises” (Duarte and Hoover, 2011: 19).

Policy without theory

Old-style business cycle studies *à la* Burns and Mitchell were criticized by Koopmans for being ‘measurement without theory.’ According to Lucas, a analogous criticism needed to be addressed to Keynes theory: it was ‘policy without theory’ (or with so thin a theoretical backbone that it amounted to none). Since the diagnosis of the malfunctioning of the economy is shallow, it is a small wonder that policies undertaken to cure it is ineffective. According to Lucas, what was needed was an in-depth understanding of aggregate fluctuations grounded on a choice-theoretical framework. Unlike the Keynesian strategy, this one leads to no quick policy fix, but it has the merit of drawing the attention to institutional design.

The abandonment of the effort to explain business cycles accompanied a belief that policy could effect immediate, or very short-term, movement of the economy from an undesirable current state, *however arrived at*, to a better state. The belief that the latter objective is attainable, and the attempt to come closer to achieving it is the only legitimate task of research in aggregate economics, is so widespread that argument to the contrary is viewed as ‘destructive,’ a willful attempt to make life more difficult for one’s colleagues who are only trying to improve the lot of mankind. Yet the situation is symmetric. If the business cycle theorists were correct, the short-term manipulation on which much of aggregative economics is now focused only diverts attention from discussion of stabilization policies which might actually be effective. (Lucas [1976] 1981a: 216–217)²⁰

¹⁸ According to Sims (1982: 116), the term ‘structural’ was introduced by Hurwicz apropos models *à la* Cowles Commission which served as a springboard for Keynesian macroeconomic models.

¹⁹ By contrast, its impact on policymakers and central banks was limited. For lack of tractable alternatives, until recently, they kept relying on Keynesian macroeconomic models.

²⁰ Lucas made the same point in a more casual way in an interview with Parker: “The Keynesian view was that a capitalist economy was a pretty shaky piece of machinery, it is pretty prone to instabilities. It can break down for all kinds of reasons. So what we want from economics is to be like a mechanic. You come in with your car and something is wrong, your mechanic can diagnose what is wrong and fix it up. How did it go wrong in the first place? He doesn’t know

The dismissal of the neoclassical synthesis viewpoint

I have argued in Chapter 2 that the idea of a neoclassical synthesis may refer to (a) the theoretical program aiming at integrating Keynes and Walras (b) the judgment that macroeconomics should be pluralistic with the right to exist for both Keynesian theory and classical theory, the former holding sway over the short-period disequilibrium field and the latter relevant for long-period analysis. Adopting the Lucas program amounts to dispelling the neoclassical synthesis idea in its two meanings. There is no longer any need for trying to build a synthesis between Keynesian disequilibrium theory and Walrasian equilibrium theory because the latter, now understood in reference to neo-Walrasian theory, can address that part of the *explanandum* which had previously been assigned to Keynesian theory. Thus, analytically, nothing is lost with the dismissal of neoclassical synthesis. Lucas viewed neoclassical synthesis as a second-best solution, valid as long as the tools for doing serious dynamics work remained unavailable to economists. Once this was no longer the case, it lost its *raison d'être*.

The end of the Keynesian consensus

In macroeconomics, controversies are never purely theoretical. They also have policymaking implications and often involve the political divide evoked in Chapter 4 between the Keynesian and the laissez-faire visions. So the heyday of theoretical Keynesian macroeconomics percolated into a societal consensus about the role of governments close to the Keynesian vision. In a 1980 lecture, provocatively entitled “The Death of Keynesian Economics” (Lucas [1980c] 2013), Lucas argued that the emergence of the Keynesian consensus was an unsurprising consequence of the Great Depression. This crisis led to a widespread loss of faith in market forces, which gave rise to the belief that the task of governments was to manage the economy on a year-to-year basis.

The central message of Keynes was that there existed a middle ground between the extremes of socialism and laissez faire capitalism. . . . True that economy cannot be left to its own device, but *all* that we need to do to manage it is to manipulate the general level of fiscal and monetary policy. If this is done right, all that elegant 19th century economics will be valid and individual markets can be left to take care of themselves. In effect, Samuelson told his colleagues: “Face it – you live in a world where virtually

and doesn't care. All he knows is this tire is flat, it hit a nail. I don't know what happened to it; but I know how to fix tires and I will fit it for you. That was the kind of Keynesian attitude, not so much telling us how we got in trouble, but telling us how to get out. Government projects, tax stimulus, interest rate reductions, these were all ways to get things going again. That was kind of the Keynesian attitude, a kind of can-do attitude” (Lucas's interview by Parker, Parker 2007: 96).

nobody has any faith in this laissez faire religion of yours. I am offering a substitute ideology which concedes the inability of a competitive economy to take care of itself, but which also offers a management system which is, say, 95% consistent with laissez faire." These were hard times, and this was too good a deal to pass up. (Lucas [1980c] 2013: 502)

In another article, written more or less in the same period, "Rules, discretion and the role of economic advisor" (Lucas [1980b] 1981a), Lucas emphasized a related aspect of the same phenomenon, the belief that Keynesian economists could act as impartial experts on economic policy, advising governments on how to manage the economy. This, he claimed, was the core of the Employment Act of 1946. As a result:

... within the existing institutional framework, the role of the economic expert as a day-to-day manager expanded rapidly, and the role of the academic macroeconomist became that of equipping these experts with ideas, principles, formulas which gave, or appeared to give, operational guidance on the task with which these economic managers happened to be faced. (Lucas [1980b]1981a: 251)

However, Lucas's reasoning went on, this social consensus could only last as long as the theoretical construction on which it was based was deemed robust. Once contrary events, stagflation, methodological criticisms, and the ascent of an alternative approach to the field came forth, this could no longer be believed. As a result, the consensus broke down, as uncomfortable as it may have been to admit it:

What I meant by saying that Keynesian economics is dead at the beginning of my talk is just that this middle ground is dead. Not because people do not *like* the middle ground anymore but because its intellectual rationale has eroded to the point where it is no longer serviceable. (Lucas [1980c] 2013: 502)

Lucas saw this outcome as good news, a good riddance of a mistaken line of research, although he acknowledged the feeling of disarray and painful loss felt by some.

THE RATIONAL EXPECTATIONS REVOLUTION

The turning point

Lucas's 1972 article was a highly technical piece published in a journal that was not standard macroeconomists' preferred read. Somebody like Friedman presumably would have little to say about it except that he liked its conclusion! The same was true for his 1975 paper, which used the 1972 apparatus to construct an equilibrium model of the business cycle. What made the fuss was the rational expectations assumption. The latter soon became the talk of the town, especially after Lucas drew its implications for econometric modeling in his "Econometric Policy Evaluation" 1976 paper (which started to circulate in

1973). According to Sargent's first hand account (Sargent 1996), Wallace and him had already written papers about rational expectations at the turn of the 1970s and read drafts of Lucas's *Journal of Economic Theory* without really understanding its implications. It was only on reading a draft of "Econometric Policy Evaluation" that they had an epiphany, suddenly realizing that optimal control theory was an inadequate technique.

We were stunned into terminating our long-standing Minneapolis FED research project to design, estimate and optimally control a Keynesian macroeconomic model. We realized then that Kareken, Muench and Wallace's (1973) defense of the "look-at-everything" feedback rule for monetary policy – which was thoroughly based on "best responses" for the monetary policy exploiting a "no-response" private sector – could not be the foundation of a sensible research program, but was better viewed as a memorial plaque to the Keynesian tradition in which we had been trained to work. (Sargent 1996:539)

This was the beginning of Keynesian macroeconomics losing its dominant position in academics. A similar testimony comes from Wallace. I want to evoke it, not only because it is telling but also because it is reminiscent of the earlier event, noticed in Chapter 1, that at the time Keynes's *General Theory* came as a liberation for young economists. The same was true for young economists in the 1970s with respect to Lucas's innovative push. In a 2013 interview with Altig and Nosal (2013), Wallace vividly recounted his first contact with macroeconomics when taking a class with Harry Johnson in the early 1960s. Most of the course consisted in Johnson teaching a kind of history of macroeconomics class presenting ideas about which Wallace declared that he hardly knew what to do with. Then at last the class read a paper that he understood, Modigliani's 1944 article.

After looking at this early unrecognizable macro stuff, in Modigliani you see equations whose number is equal to that of the unknowns, and the equations seem to allow you to talk about things the way people still do today. So it was a real eye opener. (Altig and Nosal 2013: 4)

However, later, when working on the Phillips curve with Sargent, his colleague at the University of Minnesota trying to endogenize people's expectations of inflation, a new disappointment cropped up. Wallace realized the IS-LM approach was also methodologically flawed because of its basically static character.

Then I saw this paper by Lucas [Lucas 1972]; I don't know why I read it, or tried to read it. In part, I did because Bob had been a classmate at Chicago and I had a high opinion of him. I picked up the paper, and it's talking about people, two-period lived people. What's that? There are no people in macroeconomics! I pretty much saw that the Phillips curve ideas that Sargent and I had been working on were a dead end. That paper raised the standard, and there was no turning back. (Altig and Nosal 2013: 4)

Implications of the rational expectations hypothesis

Some changes in assumptions do not change the theory concerned much. This was not the case for rational expectations. Its introduction made a huge difference. It compelled the analysis to be fully general equilibrium by bringing in cross-equations and cross-frequency restrictions, which was not the case for Keynesian econometric models. As already mentioned, it made the earlier brawls between Keynesians and monetarists look obsolete. Finally and crucially, it imposed important changes in modeling strategy, the eventual result of which was to disrupt standard Keynesian policy conclusions.

Two examples are worth considering. The first one is Sargent and Wallace's 'policy ineffectiveness proposition' (Sargent and Wallace 1976). It asserts that real variables are not affected in the least by systematic monetary policy.²¹ Having no impact on production, it has none on employment either. Any systematic policy – that is, that economic agents are able to anticipate – aiming at increasing unemployment through an increase in the money supply and prices, must be ineffective. In other words, Sargent and Wallace's paper argued that the Phillips curve is as vertical in the short period as it is in the long period.

In this system there is no sense in which the authority has the option to conduct counter-cyclical policy. To exploit the Phillips curve, it must somehow trick the public. By virtue of the assumption that expectations are rational, there is no feedback rule that the authority can employ and expect to be able to systematically to fool the public. This means that the authority cannot exploit the Phillips curve even for one period. (Sargent and Wallace 1976: 177)

The second example, on which I will elaborate a bit more, is Kydland and Prescott's time inconsistency argument. It concerns the issue of the credibility of commitments made by political authorities. Kydland and Prescott addressed it in a famous article on time inconsistency, entitled "Rules rather discretion: the inconsistency of optimal plans" (Kydland and Prescott 1997).²²

Friedman pleaded against discretionary monetary policy by arguing that people can be fooled once but not indefinitely. Taking up the same argument more rigorously, Kydland and Prescott demonstrated that this conclusion is strengthened once the rational expectations assumption is adopted. What is at stake is the problem of time inconsistency. The basic idea of the paper is that a benevolent government will often be tempted to renege on its promises. This occurs when a commitment that is optimal at one point in time no longer is at a subsequent point in time. As a result, when this point arrives, the authority is prompted to forego its commitment. The standard example is natural disasters. Assume an area that is prone to floods. Households might take the risk of

²¹ For discussions of Sargent and Wallace's argumentation, see McCallum (1980), Hoover (1988: 66 seq.), and Heijdra and van der Ploeg (2002: 67-70).

²² Cf. Tabellini (2005).

settling in such an area under the presumption that in case of a flood, the government will provide assistance. This is a case of moral hazard. To avoid it, the government may announce that in case of a disaster it will provide no help. However, if the government has a free hand, this announcement will not be credible because once a flood has occurred, it will be hard for it to honor its announcement. Agents endowed with rational expectations will be aware of this propensity and therefore they will take the risk of settling in the risky area. A more strictly economic example is that of a government aiming at boosting investment and therefore announcing that an increase in the interest rate is going to occur in a year's time, thereby leading to firms hastening their investment plans. The snag is that, a year later, it may be in the government's interest to forego this increase because of its deflationary effects. This is a general feature: whenever some optimal policy is devised on one matter or another, after it has been implemented and the desired result has been obtained, a better alternative emerges. In both examples, credibility is at stake. If the government reneges on its commitment, its credibility will be harmed, and its future announcements may no longer be taken seriously. Thus, once rational expectations are introduced into the picture, the government's leeway is eventually drastically reduced.

Kydland and Prescott's argument has a direct impact on the rules versus discretion debate, as it implies drastic narrowing of governmental discretion. In the case of discretionary policy, decisions are left to the discretion of the authority, which is supposedly well informed and benevolent. The opposite case is when the authority is bound by state-contingent rules set once and for all. Kydland and Prescott demonstrate that if time inconsistency problems are present and agents hold rational expectations, the second set-up is superior.

CONCLUDING REMARKS

I conclude this chapter by making four remarks. The first is that I am aware that my account of Lucas's intellectual journey is largely based on Lucas's own account of what happened. Narratives by successful scholars recounting what led them to their inventions must be scrutinized heuristically. This is a task that I leave for further research. A second limitation is that my investigation has focused on contents rather than on the sociological dimension of the Lucasian revolution. A more detailed history of the way in which a small number of individuals, concentrated in a few universities, were able to change the landscape of macroeconomics over a few years is called for. A third remark is that my account may have suggested that Lucas's contribution was sufficient to set the revolution in motion. It is true that it directly impacted the works of people like Sargent and Wallace, but the DSGE revolution was broader than that. In subsequent chapters, I will underline the crucial role played by Kydland and Prescott in this respect; indeed, they redirected and stabilized Lucas's initial insights.

My final remark is that Lucas's theoretical journey can be interpreted through the prism of Leijonhufvud's decision tree metaphor, mentioned in the Preface. When a research line leads to a dead end, a possible solution is to backtrack – “to identify, as it were, the forks in the road where the major decisions on what conceptual experiment to pursue were made” as he wrote in a letter to Patinkin quoted by Backhouse and Boianovsky (Backhouse and Boianovsky (2012: 56) – and take another bifurcation, neglected at first, but which now seems viable and appealing. This is what Lucas did, engaging in a long-haul backtracking process from the state of macroeconomics in the 1960s, which he considered an impasse, to some earlier methodological fork in the road which he believed was at the root of the present stalemate.

The apparent novelty of the model of “Expectations and the Neutrality of Money” ... renewed my interest in the vast pre-Keynesian literature on business-cycle theory. There I found ... a sophisticated literature, however unaided by modern theoretical technology, emphasizing the recurrent character of business cycles, the necessity of viewing these recurrences as mistakes, and attempts to rationalize these mistakes as intelligent responses to movements in nominal ‘signals’ of movements in the underlying ‘real’ events we care about and want to react to. (Lucas, 1981a: 9)

A Methodological Breach

The aim of this chapter is to substantiate my claim that Lucas's work marked a turning point in the history of macroeconomics. Any scientific revolution always comprises an important methodological component. I will argue that, in the case at hand, three important shifts stand out: a change in the *quaesitum* of macroeconomics, a change in the conception of the relation between theory and model and, finally, a change in the notion of equilibrium upon which the analysis is based. The overall picture surfacing from my study is that these differences are linked with Lucas's decision to make macroeconomics neo-Walrasian, while traditional Keynesian macroeconomists followed Keynes by adopting a Marshallian methodology.

Lucas had a long-standing interest in methodological problems; in an interview with Snowdon and Vane, Prescott hailed him as "the master of methodology, as well as defining problems" (Snowdon and Vane 2005: 351). While he never wrote a systematic exposition of his methodological standpoint – his "Methods and Problems in Business Cycle Theory" ([1980a] 1981a) comes closest to such an enterprise – it nonetheless worth noticing that about half the essays in his book, *Studies in Business Cycle Theory*, are of a methodological nature. Moreover, these are only the tip of the iceberg. Exploring the Lucas Archives at Duke University's Special Archives division (Lucas. Various) reveals the existence of many drafts and notes by Lucas pertaining to methodological issues. They are invaluable for my inquiry for several reasons: they may complement the published works, they may shed light on the genesis of Lucas's mature vision, and they may reveal Lucas's deep-seated views, what he really believed but preferred not to include in published articles. I will abundantly draw from this little known material.¹

¹ I am aware that unpublished pieces found in archives also raise difficulties: they must be interpreted cautiously because the reason they have remained unpublished may well be that the author abandoned the views expressed in them. Be that as it may, the greater the congruency of

A CHANGE IN THE RESEARCH AGENDA

Macroeconomics arose in the wake of the Great Depression from the desire to bring to the fore the existence of market failures on which the state should act. It is a small wonder that the field was almost naturally skewed towards social reformers. Unemployment was considered the main of these market failures. Therefore, it became the main research topic of macroeconomics.² After the Lucasian revolution, things changed sweepingly, as business fluctuations became the defining object of study of macroeconomics. The unemployment topic was no longer regarded as a priority and disappeared from the radar. Research was redirected towards the level of activity, that is, the total numbers of hours worked. Here is how Lucas justified this move in his book *Models of Business Cycles*:

In most such models [of the business-cycle] unemployment as a distinct activity plays no role whatever. For many other economists, explaining business cycle is taken to *mean* accounting for recurrent episodes of widespread unemployment. From this alternative viewpoint, a model with cleared markets seems necessarily to miss the main point, however successful it may be accounting for other phenomena, and the work of "equilibrium" macroeconomists is often criticized as though it were a failed attempt to explain unemployment (which it surely does fail to do) instead of as an attempt to explain something else. (Lucas 1987: 48)³

Several factors came into play for explaining this sweeping change of agenda. Awareness of the shortcomings of Keynesian macroeconomics was one of them. The availability of new tools allowing a more rigorous study of dynamics, was a second factor. A third one was the emergence of search modeling to the effect that the study of unemployment could be sent back to labor economics. Broader historical events also played a part. The memory of the Great Depression faded away. The contrasted evolution of socialist and capitalist economies strengthened the belief in the superiority of the market system. In many countries, unemployment ceased to be the haunting theme that it used to be.

While earlier the view of business cycle theorists centered more on turning points than on fluctuations, often regarded as caused by idiosyncratic events, Lucas shifted the emphasis toward fluctuations, further arguing

drafts with published pieces, the higher the likelihood that they are more than tentative ideas committed to paper. This is often true for Lucas, a typical situation being one in which ideas that are expressed succinctly, if not inadvertently, in a published article are based on a fuller treatment in earlier notes which remained unpublished.

² A remnant of the earlier view is Azariadis's statement in an entry in the *New Palgrave Dictionary* that "involuntary unemployment is for many economists the *sine qua non* of modern macroeconomics" (Azariadis 1987: 734).

³ As will be seen, the unemployment theme made a return in later developments of the DSGE program.

that they exhibited enough regularity to make the construction of a general theory possible.⁴

Lucas's project rested on the premise that neoclassical theory, understood as neo-Walrasian theory, should be taken more in earnest than the macroeconomists believed at the time. It cannot be stated that the historical context was particularly suited to this view. At the turn of the 1970s, neoclassical theory was rather on the defensive. On the one hand, a radical political economy stream was emerging with people who wanted to return to Marxian intuitions.⁵ On the other hand, Herbert Simon, also at Carnegie like Muth and Lucas, had suggested that optimizing rationality should be replaced with bounded rationality, a view that at the time was compelling for many.⁶ The distinctive feature of Lucas and the likes of him was to have taken the exact counterpoint. They thought, in Sargent's words, that "Keynes and his followers were wrong to give up on the possibility that an equilibrium theory could be account for the business cycle" (Sargent 1977: 14). Positively, they believed that the future of macroeconomics lay in becoming more neoclassical rather than less ('more' is an understatement here, as they meant that macroeconomics should be fully absorbed into neoclassical theory).

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: 107–108)

LUCAS ON METHOD⁷

Standards

Lucas held a narrow view of what macroeconomic theory ought to be. To him what mattered when doing theory was respecting a series of methodological standards.

- (a) There should be no split between the principles underpinning microeconomics and those underpinning macroeconomics (Lucas 1987: 107–108). That is, macroeconomics without (choice-theoretical) micro-foundations is sub-standard.

⁴ "With respect to the qualitative behavior of co-movements among series, *business cycles are alike*. To theoretically incline economists, this conclusion should be attractive and challenging, for it suggests the possibility of a unified explanation of business cycles, grounded on the general laws governing market economies, rather than in political or institutional characteristics specific to the particular countries or periods" (Lucas [1977] 1981a: 218, Lucas's emphasis).

⁵ Cf. Piore (2013). ⁶ Cf. Sent (2002).

⁷ This section is drawn from De Vroey (2011a). Other studies of Lucas's epistemology are Vercelli (1991) and Boumans (1999, 2005).

- (b) Macroeconomics is part of general equilibrium analysis. Its concern is the working of an entire economy, and it must account for the interactions between the component parts of the economy. It must be dynamic, that is, dealing with the economy over time. Uncertainty is accounted for by ascribing probabilities to future states of the world and to the occurrences of shocks. Thus, the DSGE acronym, which had not yet been invented, fits what Lucas had in mind.
- (c) A macroeconomic theory and a mathematical model are one and the same thing. This conception, which can be traced back to Walras, runs counter to another, more widespread, understanding of the relationship between theory and model. According to the latter, they are distinct entities: a theory is a set of propositions about reality while a model, be it mathematical or in prose, is an attempt at rigorously setting out the implications of some part of the theory.⁸
- (d) A theory is concerned with imaginary constructions; it is avowedly non-realistic.

Insistence on the 'realism' of an economic model subverts its potential usefulness in thinking about reality. . . . 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. A 'good' model, from this point of view, will not be exactly more 'real' than a poor one, but will provide better imitations. (Lucas [1980a] 1981a: 271–272)

The central assumptions of macroeconomic models, such as rational expectations, ought thus to be viewed as modeling devices, model-building principles rather than propositions about reality.

One can ask, for example, whether expectations are rational in the Klein-Goldberger model of the United States economy; one cannot ask whether people in the United States have rational expectations (Lucas. Various. Box 23, Barro Folder).

By prescribing these standards, Lucas was treading Walras's footsteps. However, on two other scores, he adhered to the earlier practice of macroeconomics. First, he held the strong conviction that macroeconomic models are of no interest if they fail to reach policy conclusions.

'The central question that macroeconomists need to resolve: Which necessarily abstract models can help us to answer which practical questions of economic policy? (Lucas. Various. Box 26, Reflections on contemporary economics folder).

⁸ Thus, in the traditional view, the model is subservient to the theory. The following quote from Leijonhufvud is a fine depiction of this viewpoint: "I propose to conceive of economic 'theories' as a set of beliefs about the economy and how it functions. They refer to the 'real world'. . . . 'Models' are formal but partial representations of theories. A model never encompasses the entire theory to which it refers" (Leijonhufvud 1997). A different approach of the theory-model relationship can be found in Boumans (2005) and Morgan (2012).

Second, he regarded macroeconomics as an applied rather than a purely abstract field. A confrontation between theory and reality needed to be at its heart. What was to be done was to construct “a fully articulate artificial economy which behaves through time so as to imitate closely the time series behavior of actual economies” (Lucas [1977] 1981a, p. 219), what I call ‘Lucas’s FORTRAN injunction.’

Our task as I see it is to write a FORTRAN program that will accept specific economic policy rules as ‘inputs’ and will generate as ‘output’ statistics describing the operating characteristics of times series we care about, which are predicted to result from these policies (Lucas [1980a] 1981a: 288).

A theory/model ought to be assessed on its ability to make correct predictions. The better its ability to reproduce past events, the more trustworthy the model is for assessing new policy measures.

Model economies as analogous systems

There is a tension in Lucas’s methodological standpoint bearing on how to bring together the disparate sets of requirements I just exposed. Lucas’s solution to this riddle was to regard models as analogous systems. Time and again, one finds Lucas writing that economic models should be viewed this way. In Lucas’s “Methods and Problems” article alone, I counted seven occurrences of the term ‘analogy.’ For example:

Progress in economic thinking means getting better and better abstract, analogue economic models, not better verbal observations about the world. (Lucas [1980a] 1981a: 276)

However, in his published papers, Lucas gave no clue as to what he meant exactly.⁹ This gap can be filled by searching through his archives. They contain a series of drafts – some handwritten, some typed – in which he expanded at length on the notion of an analogy, on the role of models, and on the relationship between modeling and economic policy.¹⁰ The subsequent discussion is based on one such fragments.

⁹ According to Boumans, Lucas’s analogical vision of modeling came interpreted as a Turing test (Boumans 2005: 92–96).

¹⁰ My guess is that these drafts were written at the end of the 1970s, a period when Lucas gave many seminars on rational expectations and business cycle theory. Unfortunately, the drafts are collected in a disorderly manner in the archives: a series of fragments, often four or five pages long, all related to the same themes, but without any indication as to how they might be combined. To the best of my knowledge, they have never appeared in a finalized form in a published piece. In view of their interest and the lack of easy access to them, I shall quote from them extensively.

Lucas's argumentation starts from two premises, both of which may come as a surprise. The first is that a model, though a fiction, is nonetheless an observable reality:

We speak of modeling phenomena or models of phenomena, suggesting that observed phenomena are one kind of thing and models of them another thing, but I want to define a model to be itself a phenomenon: something the behavior of which can be observed. Then what is the relationship between a set of phenomena and the second set that we call a model of the first set? I will call this relationship *analogy*. (Lucas. Various. Box 27, adaptive behavior folder).

This still does not clarify what Lucas meant by the term 'analogy.' An answer is provided later in the same draft, where Lucas wrote that he took an analogy to "mean a symmetric relationship between two things." These things may either be a thing in the usual sense and a theory, which to Lucas is just another thing, or two distinct procedures for generating observations. In another set of notes, Lucas added that "we must make liberal use of analogies: judgments that one situation is similar enough to another to call for the same reaction."

Lucas's second premise was that economic theory (or at least general equilibrium theory) is utopian in nature, a proposition that he drew from a (bold) comparison between economics and anthropology:

Economic theory, like anthropology, 'works' by studying societies which are in some relevant sense simpler or more primitive than our own, in the hope either that relations that are important but hidden in our society will be laid bare in simpler ones, or that concrete evidence can be discovered for possibilities which are open to us which are without precedent in our own history. Unlike anthropologists, however, economists simply invent the primitive societies we study, a practice which frees us from limiting ourselves to societies which can be physically visited as sparing us the discomforts of long stays among savages. This method of society-invention is the source of the *utopian* character of economics; and of the mix of distrust and envy with which we are viewed by our fellow social scientists. The point of studying wholly fictional, rather than actual societies, is that it is relatively inexpensive to subject them to external forces of various types and observe the way they react. If, subjected to forces similar to those acting on actual societies, the artificial society reacts in a similar way, we gain confidence that there are useable connections between the invented society and the one we really care about. (Lucas. Various. Box 13, Directions of macroeconomics 1979 folder).

Surely, anthropologists will fail to recognize themselves in Lucas's account but, leaving that aside, the comparison successfully brings out Lucas's message: models are fictitious economies and by manipulating them, we can learn about the functioning of real economies. Lucas's reason for resorting to the use of analogies followed from his view that macroeconomics should be used to assess the impact of policy measures. Normally, these cannot be assessed experimentally, the stagflation episode of the 1970s being a possible exception. So, second-best solutions need to be found. One of these is to look for analogous real-world experiences – has there been another, not too different, country

where the policy under consideration has been tried? If yes, this experience may constitute a valuable benchmark. Unfortunately, such real-world analogies are scarce. Again, a way out exists. The reference for such comparisons does not need to be a real-world experience; a model economy can do the job. Actually, it can do it better, the advantage of the model economy over the real-world economy being that it can be controlled to improve on the similarities it offers. This is how Lucas justified model building.¹¹ He readily admitted that an analogy which one person finds persuasive will look ridiculous to another. Is there an escape? Here is his answer:

Well, that is why honest people can disagree. I don't know what one can do about it, except keep trying telling better and better stories, to provide the raw material for better and more instructive analogies. How else can we free ourselves from the limits of historical experiences so as to discover ways in which our society can operate better than it has in the past? (Lucas 1988: 5)

Thus, no matter how arcane mathematical models may seem, in the end they are always a story. At the beginning, it may be vague. Progress consists in 'making it better' in terms both of the concepts introduced and of the logical steps involved in the argumentation.

I started my presentation of Lucas's view of a theory by arguing that, to him, a theoretical proposition is a statement about a fictitious economy rather than about an actual economy. As a result, the right question to ask about such propositions is not whether they are true or false (because the answer is always 'false'). The right way to look at it is: "All we can say about an analogy is that it is good or bad, useful or useless, and such subjective terms only raise further questions: Good for what? Useful for what purpose?" (Lucas. Various. Box 23, Barro folder).

Do we value this theory (if one can discuss valuing the theory of value!) because we agree that it implies a set of verbal propositions about observations that can be refuted by keeping our eyes open for black swans [because seeing a black swan would indicate that the proposition that all swans are white is refuted (MDV)]? If so, what are these sentences that express – more accurately than the theorems or formulas themselves – what this theory really means, really implies? Shall we test the theory by checking sentences like: "in all economies, production possibility sets have nonempty interiors?" or "People tend to act in their own self-interest"? against what we see, the way we are supposed to check swan colors? Shall we dismiss Arrow and Debreu's theory as vacuous,

¹¹ "I like to think of theories – economic and psychological, both – as simulatable systems, *analogues* to the actual system we are trying to study. From this point of view, the Wharton model, say, bears the same kind of logical relationship to the United States economy as France, say, does: it is just a different economy, or system, but one that is similar enough to the U.S. economy that we might hope to learn about the properties of one through the study of the other. If our objective is to learn what the consequences of introducing a value added tax in the U.S. might be, we might study its consequences in France or simulate the Wharton system under such a tax or, better still, do both." (Lucas. Various. Box 27, adaptive behavior folder; Lucas's emphasis)

and Kydland and Prescott's application of it as wrong? (Yes, I think, except for 'dismiss'). An alternative point of view toward things and theory is this: we observe things and events, and we perceive analogies among them. (Lucas. Various. Box 27, adaptive behavior folder).

A NEW EQUILIBRIUM CONCEPT

From Adam Smith onward, equilibrium has played a central role in economic theory, one of the main features differentiating economics from the other social sciences. The traditional view of equilibrium was that it means a state of rest. This was also how it was understood in Keynesian macroeconomics until the Lucasian revolution. The latter introduced a new equilibrium concept in macroeconomics. It could be named 'dynamic equilibrium,' but this label is equivocal since the state of rest view also comprises a dynamic dimension (yet an unsatisfactory one). I will therefore use the 'intertemporal equilibrium' terminology. Lucas did not invent the new concept; he imported it into macroeconomics from Arrow and Debreu's theory of competitive equilibrium. My aim in this section is to contrast the two concepts.

The traditional understanding of equilibrium: equilibrium as a state of rest

First of all, it must be noticed that traditional equilibrium theory is a set of propositions about reality. It is taken for granted that the functioning of real economies can be interpreted as manifesting that equilibration forces, the so-called 'market forces,' are at work in them. Among many others, here are three quotations, drawn from economists of different allegiances, written in support of the state of rest concept.

Such is the continuous market, which is perpetually tending towards equilibrium without ever actually attaining it, because the market has no other way of approaching equilibrium except by groping. . . . Viewed in this way, the market is like a lake agitated by the wind, where the water is incessantly seeking its level without ever reaching it. (Walras 1954: 380)¹²

The ordinary economic situation is one of disequilibrium moving in the direction of equilibrium rather than of realised equilibrium. (Viner [1931] 1953: 206)

If we are to make empirically interesting statements about disequilibrium and equilibrium, statements that have potential empirical content, we must define these terms so that both are meaningful and both can be observed – in order to say that in fact we do not observe one of them. (Lipsey 2000: 72)

¹² It seems odd to state that the intertemporal equilibrium concept was formulated by Debreu, the emblematic neo-Walrasian theorist, and give a quotation from Walras in support of the old concept. As argued aptly by Donzelli (1989), the fact is that Walras wavered between the old and the new concept.

In an article entitled, "Alfred Marshall's Theory of Value," Frisch (1950) compared the state of rest conception of equilibrium to a pendulum. Like a pendulum, the industry studied is supposed to have a single standstill position. When we observe that the pendulum is moving, we can infer that disequilibrium exists. The same holds for the industry. If prices and/or quantities change over time, it means that the industry is in disequilibrium. This view can be extended to the economy as a whole. In short, in Friedman's words, "an equilibrium position is one that, if attained, will be maintained" (Friedman 1976: 19).

The central feature of the state of rest conception of equilibrium is its static character: its object of analysis is what happens in an industry or an economy over a given period of time during which a single equilibrium allocation acts as a center of gravity. Here is how Marshall, who reflected a lot on the issues of time and adjustment, posited the framework in which this concept must be analyzed:

The unit of time may be chosen according to the circumstances of each particular problem: it may be a day, a month, a year, or even a generation: but in every case it must be short relative to the *period of the market* under discussion. It is to be assumed that the general circumstances of the market remain unchanged throughout this period; that there is, for instance, no change in fashion or taste, no new substitute which might affect the demand, no new invention to disturb the supply. (Marshall 1920: 342, my emphasis)

Marshall proposed the 'period of the market' terminology. I prefer to call it the 'period of analysis.' I also find it misleading to call the center of gravity allocation a 'long period equilibrium,' as is often done. In my eyes, a better terminology is 'normal equilibrium' or 'period-of-analysis equilibrium.'

When the state of rest equilibrium concept is adopted, it must be assumed that the same equilibrium allocation exists at the beginning and at the end of the period of analysis. In other words, the initial configuration of data characterizing the economy or the market studied (technology, preferences, endowments, population, and states of the world) ought to still be found at the end of the period.

Another basic trait of this concept is that the notions of equilibrium and disequilibrium are organically linked. When the question is asked of whether equilibrium exists at a given date, the answer will usually be 'No': the economy generally is out of equilibrium, changes in prices or quantities being the indicators of such a state of affairs. There is nothing dramatic about such a situation, however. What matters is the existence of efficient re-equilibrating forces.

A final characterization is that, though basically static, this equilibrium notion comprises a dynamic dimension. It is related to stability, that is, the issue of how, after a shock, the unchanged equilibrium allocation is restored. In his *Foundations of Economic Analysis* (1947), discussing the economy as a whole rather than an industry as Marshall did, Samuelson encapsulated this re-equilibration process in a set of differential equations:

$$\frac{dp_i}{dt} = a_i E_i(p_1, \dots, p_{m-1})$$

These equations refer to an exchange economy comprising m goods, with good m acting as numéraire; p_i is the numéraire price of good i , every a_i is a positive constant expressing the speed of adjustment in the i -th market, E_i is the excess demand function for good i .

This traditional conception of equilibrium may well receive some vindication from elementary physics, but the main factor explaining its adoption is that it corresponds to our common-sense understanding of equilibrium. It is thus no surprise that the founding fathers of economic theory adopted it. However, on reflection, its appositeness is far from obvious. The state of rest equilibrium concept captures one aspect of time, namely duration – it takes time for the effects of decisions to be realized. Unfortunately, it cannot come to grips with another of its dimensions, namely that the passage of time is accompanied by incessant irreversible changes, large or small. Therefore, as argued by Donzelli, it turns out to be a-temporal. As a result, any models based on it

... are structurally incapable of providing the slightest explanation of any economic phenomenon whose occurrence essentially depends on economic activities taking place in time. (Donzelli 1989: 158)

Changes in these data can occur during the period of analysis, but they must be reversible and temporary. Irreversible changes can only enter the picture in the interstices between periods of analysis, which means that they cannot be part of price theory.

A related problem is that the length of the period of analysis cannot be inferred from observation but is decided by economists (this explains why I dislike Marshall's expression 'period of the market'). Yet, they face an impossible dilemma as they need to find a compromise between two opposite criteria: on the one hand, the period of analysis must be long enough to allow adjustment processes to take place, on the other hand, the longer it is, the more contrived the assumption that only reversible shocks can be considered. No satisfying solution to this dilemma can be found.

A last problem is that the speed of the adjustment process is a 'free parameter'; assigning a value to it is a decision left to the economist. If the latter is eager to argue that the economy evolves in disequilibrium, she just has to assign the speed of adjustment a low value, the opposite being true is she likes the idea of a quick return to equilibrium.

Lucas's equilibrium conception

In their "After Keynesian Macroeconomics" paper, Lucas and Sargent insisted that the new paradigm they advocated was based on a different equilibrium concept, intertemporal equilibrium, arguing as follows:

When Keynes wrote, the terms *equilibrium* and *classical* carried certain positive and normative connotations which seemed to rule out either modifier being applied to business cycle theory. The term *equilibrium* was thought to refer to a system at rest, and some used both *equilibrium* and *classical* interchangeably with *ideal*. Thus an economy in classical equilibrium would be both unchanging and unimprovable by policy interventions. With terms used in this way, it is no wonder that few economists regarded equilibrium theory as a promising starting point to understand business cycles and design policy to mitigate or eliminate them. In recent years, the meaning of the term *equilibrium* has changed so dramatically that a theorist of the 1930s would not recognize it. An economy following a multivariate stochastic process is now routinely described as being in equilibrium, by which it is meant nothing more than at each point in time, postulates (a) [that markets clear, MDV] and (b) [that agents display optimizing behavior, MDV] above are satisfied. This development, which stemmed mainly from work by K. J. Arrow and G. Debreu, implies that simply to look at any economic time series and conclude that it is a disequilibrium phenomenon is a meaningless observation. (Lucas and Sargent [1979a] 1994: 15)

Before discussing Lucas's equilibrium concept, I have one remark to make about the two postulates Lucas and Sargent mention. These postulates would have been better captured by writing that their approach rests on (a) optimizing *planning* and (b) a trade technology allowing for a generalized transformation of optimizing planning into optimizing *behavior*. Since in their framework (b) is taken care of by the auctioneer assumption, these two postulates can be combined into a single one, namely that all agents experience optimal behavior, in the spirit of McKenzie's observation, mentioned in Chapter 7 that general equilibrium is generalized individual equilibrium.

In Lucas and Sargent's new approach, time is framed as a succession of points in time which need to be dated. It is assumed that exchanges are confined to some of these. Decisions are made before trade. As a result, the analysis can be considered as an intertemporal planning problem. Equilibrium is defined as a state where all the agents (of the fictitious model) follow an optimizing consumption/leisure intertemporal path. It is no longer equated with 'being at rest,' and the impossible task of defining an adequate period of analysis vanishes. Observing that quantities and prices change across time no longer needs to be interpreted as meaning that the economy displays disequilibrium.

It is possible to construct systems in competitive equilibrium in a contingent-claim sense which exhibit a vast variety of dynamic behavior. The idea that an economic system in equilibrium is in any sense 'at rest' is simply an anachronism. (Lucas [1980] 1981a: 287)

Two traits of the new equilibrium concept must be underlined. The first one is that equilibrium is declared to exist as a postulate. This is well encapsulated in Lucas's 'equilibrium discipline' expression. The term 'discipline' must be understood as conveying the view that the equilibrium postulate is a rule that economists impose upon themselves when constructing their models. What matters for judging the appositeness of adopting such a stringent postulate is what can be done with models based on it (and what are the drawbacks of not

adopting it). The second trait pertains to another important breach from the traditional conception of equilibrium. Supporters of stationary equilibrium take it for granted that equilibrium and disequilibrium are features of reality. Here, Lucas also embraces the opposite viewpoint. His view is that equilibrium ought to be understood as a characteristic of the way in which economists look at reality rather than as a characteristic of reality.

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 [1978] 1994: 21)

I think general discussions, especially by non-economists, of whether the system is in equilibrium or not are almost entirely nonsense. You can't look out of this window and ask whether New Orleans is in equilibrium. What does that mean? Equilibrium is a property of the way we look at things, not a property of reality. (Lucas's interview with Snowdon and Vane, 1998: 127)¹³

This change of equilibrium concept proposed by Lucas amounted to a Copernican revolution. The transformation involved comprises several dimensions: (a) from the state of rest to the intertemporal concept of equilibrium, (b) from an approach wherein equilibrium and disequilibrium are organically linked to an approach using only the equilibrium category, and (c) from an approach wherein the assessment of the existence of equilibrium or disequilibrium is a matter of characterizing reality to an approach where instead the equilibrium concept pertains only to the fictitious model economy. Keynesian economists had a hard time accepting this threefold modification. For example, as will be seen in Chapter 12, time and again they criticized Lucas on the grounds that it was obvious that in reality markets, and in particular the labor market, are in a state of disequilibrium.¹⁴

In light of these remarks, it proves difficult to assess whether the new conception of equilibrium, with its exclusion of the disequilibrium notion, amounts to attributing a higher or a lower role to the notion of equilibrium. Getting disequilibrium out of the picture may suggest a higher role for equilibrium. But there is another side to the picture: the fact that equilibrium has

¹³ In other words, to use Weintraub's apt formulation, equilibrium is imposed upon the world: "This symposium provided additional examples of such argumentations: the discussions generated by McCallum's paper, and Grandmont's, contained various appeals to the 'Principle' that the world either was or was not in equilibrium. The commentators in this audience seemed to think that they had a way of discussing the truth of the idea that observed states were equilibria without committing themselves to any particular theory of macroeconomics. This is, of course, an illusion: equilibrium states, or disequilibria are characteristics of our theories, and are thus imposed on the world" (Weintraub 1990: 273).

¹⁴ As stated by Farmer: "When economists before Lucas saw unemployment in the labor market, they thought that they were observing a market in disequilibrium" (Farmer 2010b: 70).

become a postulate and that equilibrium or disequilibrium are no longer claimed to be characteristics of reality amounts to shrinking the pretense of equilibrium theory. The assessment to make about the equilibrium discipline then does not consist in pondering whether it is realistic but whether it is an efficient way of constructing economic theory. Moreover, when every outcome is by construction an equilibrium outcome, the normative connotation that was associated earlier with equilibrium vanishes. Welfare considerations now need to focus on the comparison of alternative equilibrium positions.

According to Lucas, there were at least two reasons, both related to 'theoretical efficiency,' why macroeconomics must shift towards the intertemporal equilibrium approach. The first is that the traditional conception is wanting as explained above. The second reason is that dropping the disequilibrium notion is good riddance. It must be banned because it refers to 'unintelligent behavior' (Lucas [1977] 1981a: 225) or, in other words, it lacks microfoundations. Now that the new concept is available (thanks to him), Lucas stated, not using it would make no sense:

To ask why the monetary theorists of the 1940s did not make use of the contingent-claim view of equilibrium is, it seems to me, like asking why Hannibal did not use tanks against the Romans instead of elephants. ([1980] 1981a: 286)

COMPARING KEYNESIAN AND NEW CLASSICAL MACROECONOMICS

In the above sections, I have dwelled on what are in my eyes the two central programmatic bifurcations made by Lucas. In this last section, I want to integrate these in a broader comparison of Keynesian macroeconomics and DSGE macroeconomics in its first installment, that is, new classical macroeconomics. Table 10.1 summarizes the contrast between them.

Several of these benchmarks have already been discussed earlier. Therefore, I will content myself with commenting the remaining ones.

The starting point

Keynes's *General Theory* lay the foundations for Keynesian macroeconomics, while Lucas's "Expectations and the Neutrality of Money" article did so for Lucasian macroeconomics. As noticed by Sargent, it is difficult to imagine two works that are more opposed, a complex kaleidoscopic 384-page book and a 21-page mathematical article:

Many of us regard Lucas's 1972 *Journal of Economic Theory* paper as the flagship of the Revolution; it is different than the flagship of that earlier revolution, Keynes's *General Theory of Employment Interest and Money*, which was ambitious, wide ranging, imprecise and vague enough to induce twenty-five years of controversy about

TABLE 10.1 *Comparing Keynesian and new classical macroeconomics*

	Keynesian macroeconomics	New classical macroeconomics
1. Starting point	Keynes's <i>General Theory</i> (1936)	Lucas's "Expectations and the Neutrality of Money" (1972)
2. Overarching aim of macro	Explaining unemployment or underemployment	Explaining the business cycle
3. Main explanatory factor	Wage rigidity or sluggishness	Agents' response to monetary shocks in a signal extracting context
4. Equilibrium concept	State of rest equilibrium	Intertemporal equilibrium
5. General equilibrium	'Incomplete' general equilibrium	General equilibrium
6. Type of economy	Monetary economy	Monetary economy
7. Microfoundations	Not strictly required	Strictly required
8. Expectations	Adaptive expectations	Rational expectations
9. Driving factor, demand or supply	Demand	Supply
10. Real effects of monetary changes	Monetary activation works	Monetary activation does not work
11. Neoclassical synthesis	Support	Rejection
12. Empirical method	Cowles Commission method	Lucas critique
13. Methodological priority	External consistency	Internal consistency
14. Standard policy conclusion	Demand activation	Laissez faire
15. Intuitiveness and accessibility	High	Low

what the book really meant. Lucas's paper was a narrow, technical study. ... There was never any confusion about what Lucas's paper meant. (Sargent 1996: 537)

A general equilibrium approach?

Two criteria must be fulfilled for models to qualify as general equilibrium models. First, they must be concerned with the economy in its entirety rather than with particular sections of it. Second, the analysis must encompass the study of the interactions between the composing branches of the economy. A general equilibrium intention was already present in Keynes's *General Theory*. But a full implementation did not follow up. Keynesian econometric models took entire economies as objects of analysis, but dealt with across

equations relations in an offhand way.¹⁵ As for the DSGE program, it is fully general equilibrium.¹⁶

Money

Both modeling strategies are concerned with a monetary economy. Contrary to the other items, there is a similarity here.

Microfoundations

Microfoundations, as understood by Lucas, relate to Walras's method of starting the analysis by analyzing how agents make optimal choices. With a few exceptions, Keynesian models usually followed the Marshallian principle that analysis can start at the level of market supply and demand functions rather than at the individual decision-making level. It is not that agents are assumed to behave in a non-optimizing way, but rather that this stage of the reasoning is skipped. On the contrary Lucas regarded microfoundations *à la* Walras as a *sine qua non* for writing sound theory. To Lucas, microfoundations were more than the choice-theoretical apparatus. They also involved bringing people into the picture, a perspective that he claimed was absent from Keynesian macroeconomics.

I think a lot of the work in Keynesian economics has gotten too far away from thinking about individuals and their decisions at all. Keynesians don't often worry about what actual individuals are doing. They look at mechanical statistical relationships that have no connection with what real individuals are actually doing. (Lucas. Various. The Margin's interview. Box 7, Correspondence 1989 folder).

The driving factor, supply or demand?

According to the Keynesian approach, variations in output and employment result from changes in aggregate demand. The underlying picture is that labor suppliers are passive, employment decisions being made unilaterally by firms. Moreover, this approach tends to consider the supply of labor and the labor force as the same thing, a fixed magnitude. By contrast, in the DSGE program the driving force of economic activity is the supply of labor. This change in emphasis, forward-looking behavior

¹⁵ "Sargent: The earlier literature proceeded as if you could build an optimizing consumption function, an optimizing investment schedule, an optimizing portfolio schedule, in isolation from one another. They are essentially partial equilibrium exercises which were then put together at the end. The Brookings model, built in [19]65, is a good example of this practice. They handed out these various schedules to different people and put them together at the end. The force of rational expectations is that it imposes a general equilibrium discipline. In order to figure out people's expectations you had to assume consistency" (Sargent's interview by Klammer 1984: 66).

¹⁶ As seen, non-Walrasian equilibrium models anticipated this move.

being combined with the idea of intertemporal leisure substitution, is a main ingredient in Lucas's view of macroeconomics. An implication of this move from demand to supply is a shift of emphasis from firms' to households' decision-making process.

Real effects of monetary changes

While Hicks favored demand activation under the form of fiscal policy, Modigliani argued in favor of monetary activation. From then on, Keynesians held the conviction that monetary expansion can increase the level of employment in a sustained way. Friedman and Lucas shared the goal of demonstrating that Keynesians were wrong.

Empirical method

Keynesian macroeconomics embraced the Cowles Commission's simultaneous equation method, witnessing to impressive developments. The Lucas critique brought out its flaws.

Methodological priority

The central methodological principle of Keynesian macroeconomics was external consistency. In this view, models are only as good as they are realistic. The prevailing intellectual mood was pragmatic. That several of the basic notions – involuntary unemployment, full employment, rigidity, and sluggishness – were defined in a loose way, or that the analysis focused on the short period with no attention being given to the linkage between the short and the long period, were by no means considered harmful methodological practices. Empirical models, the construction of which was often left to engineers rather than economists, were more data-than theory-constrained. Lucas wanted macroeconomics to abide by the Walrasian methodological principles. Accordingly, internal consistency became the alpha and the omega of theoretical construction.

Natural policy conclusions

The vision of the economy closest to Keynesian macroeconomics can be branded 'mitigated liberalism.' It defends the market system as being superior to a planning system without fully advocating *laissez-faire*. Keynesian models tend to support demand activation by the state. The opposite conclusion, a defense of *laissez-faire*, emerges from new classical macroeconomics. Fluctuations in employment reflect rational and optimal reactions by economic agents to changing conditions. If there is no market failure, there is also no need for the state to intervene.

Intuitiveness of the approach taken and accessibility to the layman

Keynesian macroeconomics theory is simple to understand even by non-economists. Its level of technicality is low. Many of its basic notions have made their way into newspapers and political discourse. Lucasian macroeconomics resorts to new, complex mathematical techniques, such as dynamic programming. Consequently, the technical barrier to entry is much higher than for Keynesian macroeconomics. The Lucasian conception of theory and model is counter-intuitive and of little appeal to the layman.

* * *

These are, in my view, the most salient traits on which the two approaches of macroeconomics can be compared. They stand in sharp contrast. However, the breach is not total. They still have a monetary economy as their object of analysis in common. This makes for one precise object of disputation between them, the issue of the real effects of monetary policy.

Three final remarks must be made about this comparison. First, one clue to understand what underpins this contrast and at which I have hinted on several occasions is that the change that occurred can be interpreted as a transition from Marshallian to neo-Walrasian macroeconomics. Second, I need to underline that my comparison only serves a pedagogical purpose. Its main drawback is probably that it is static. This does a disservice to the presentation of Keynesian macroeconomics. It extended over more than two decades, and many improvements took place over this period, so that a snapshot such as mine is necessarily a simplification. My last remark is that the comparison above concerns only the first wave of modeling in the DSGE program. When I will come to the study of the subsequent waves, it will be seen that most of the specificities of the first one remained valid, but that there were significant shifts nonetheless.

Assessing Lucas

In this chapter I assess Lucas's contribution to macroeconomics. In the first section, I express my disagreement with his judgment about Keynes. In the second, I briefly comment on intertemporal substitution, the cornerstone of DSGE macroeconomics. In the third, I discuss whether Lucas's attempt of making macroeconomics Walrasian may be regarded a valid move. In the fourth, I probe into Lucas's methodological standpoint by displaying what I consider to be two ambiguities in his approach. Lucas's opponents have often argued that his work was ideologically motivated. This is a point that should not be swept under the rug. Therefore, in the penultimate section I consider whether it is a valid contention. Finally, the chapter ends with a few concluding remarks.

LUCAS ON KEYNES

Lucas's claim, that *The General Theory* is a minor contribution to economic theory – that “Keynes was not a very good technical economist” (Usabiaga Ibanez 1999: 180) – has the merit of being original. Lucas may have been led to make such a judgment because of his conviction that economic theory ought to be mathematical. Still, I find it difficult to agree with him. By passing such an a-historical judgment, Lucas went against what he wrote in his “Problems and Methods” article, namely, that economists of the past, as clever as they may have been, could not do much better than what the contemporary level of development of the discipline allowed. With respect to the standards of his time, there is no reason to state that Keynes was a bad technical economist. On the contrary, gauged against the writings on unemployment at the time— say, Hicks's *Theory of Wages* (1932) and Pigou's *Theory of Unemployment* (1933) – Keynes's work seems to me much superior as far as breadth of inspiration and conceptual innovations are concerned.

Let me give just one example of Keynes's sharp mind. That the notion of rational expectations could have been conceived of in Keynes's time is

unimaginable. Still, Keynes hinted at the gist of it when criticizing Tinbergen's *Statistical Testing of Business Cycle Theories* (1939).¹ One of Keynes's complaints concerned the absence of expectations in Tinbergen's estimations: "Is it assumed that the future is a determinate function of past statistics? What place is left for expectations and the state of confidence relating to the future?" (Moggridge 1973: 287). Amazingly enough, Keynes's criticism of Tinbergen's model makes one think of the Lucas Critique.

I also want to emphasize strongly the point about economics being a moral science. I mentioned before that it deals with introspection and with values. I might have added that it deals with motives, expectations, psychological uncertainties. One has to be constantly on guard against treating the material as constant and homogeneous. It is as though the fall of the apple to the ground depended on the apple's motives, on whether the ground wanted the apple to fall, and on mistaken calculations on the part of the apple as to how far it was from the center of the earth. (A letter from Keynes to Harrod, dated July 16, 1928, quoted in Moggridge 1973: 300)

In light of this quotation, the difference between Keynes's and Lucas's results largely from the state of development of economics they each faced. Observing that there was no way to integrate expectations in Tinbergen's econometric model, Keynes declared that one should dispense with econometrics. He was not followed however. Decades later, Lucas found that what needed to be done was to drive econometrics in a direction enabling it to come to grips with Keynes's preoccupation.

INTERTEMPORAL SUBSTITUTION

No economist denies the existence of the phenomenon of intertemporal substitution. Incorporating it into macroeconomics was long overdue. The problem lies elsewhere, in that DSGE macroeconomics needs a strong intertemporal leisure elasticity of substitution and hence a high instantaneous elasticity of labor supply. Lucas had initially dealt with the issue in an offhand manner.² Soon, however, the validity of this assumption became the subject of hot debates. Several econometric studies argued that no elasticity of labor supply of the size needed for Lucas's claim to hold could be found in the data.³ Although this criticism was crucial as it touched on the cornerstone of the

¹ The context of Keynes's criticism has been described in Note 24, chapter 1.

² "What we do know indicates that leisure in one period is an excellent substitute for leisure in other, nearby periods. . . . The small premium required to induce workers to shift holidays and vacations (take Monday off instead of Sunday, two weeks in March rather than in August) point to the same conclusion, and this 'causal' evidence is somewhat more impressive because of its probabilistic simplicity: holidays are *known* to be transitory. On the basis of this evidence, one would predict a *highly elastic* response to transitory price changes" ([1977] 1981a, p. 224).

³ Ashenfelter (1984) surveys this literature.

new classical paradigm, it hardly stopped the momentum of the Lucasian revolution. The reason is that intertemporal substitution is such a crucial ingredient of the whole program that it cannot do without it, while alternative mechanisms are scarce. In Lucas's words:

I see no way to account for observed employment patterns that does not rest on an understanding of the intertemporal substitutability of labor. The literature contains innumerable examples of possible additional, supplementary considerations, but to my knowledge no alternatives. (Lucas 1981a: 4)

The debate about the empirical validity of the assumption of a high leisure intertemporal substitutability has persisted up to the present (although interesting progress has taken place; more about this in subsequent chapters). For the sake of the argumentation, let us assume that the defenders of low elasticity are right. Would this suffice to condemn the DSGE program? My answer, based on the conclusion of my discussion of monetarism, is 'No': demonstrating the empirical invalidity of a theoretical proposition that is a part of a wider theoretical paradigm (however central it may be) is not a sufficient condition for dismissing the whole paradigm.

THE PROS AND CONS OF WALRASIAN MACROECONOMICS

Lucas's methodological standpoint can be summarized in the statement that macroeconomics should be based on neo-Walrasian principles. He once wrote "I am a hopeless 'neo-Walrasian'" (letter to Driscoll, dated November 23, 1977 Lucas. Various. Box 30). He also set himself apart from Friedman on the grounds that the latter was Marshallian whereas he was Walrasian.⁴ When thinking of neo-Walrasian theory, the names which come to mind are those of Kenneth Arrow, Gérard Debreu, Lionel McKensie, Frank Hahn, David Cass, and Karl Shell, among others. These economists are also those Lucas seemed to refer to in his "Method and Problem in Business Cycle Theory" article. Yet it is unsure whether neo-Walrasians were ready to consider him as one of their own. At least, somebody like Cass was not:

Bob [Lucas] was in the Chicago tradition and was very concerned about empirical testing – whatever the hell that means – something that I have little sympathy for and very little interest in, to be perfectly honest. So there was quite a difference in viewpoints about why you did theory and what the relevance of theory is (Cass's interview with Spear and Wright 1988: 546).

⁴ "Snowdon and Vane: You acknowledge that Friedman has had a great influence on you, yet his methodological approach is completely different to your own approach to macroeconomics. Why did his methodological approach not appeal to you? Lucas: I like mathematics and general equilibrium theory. Friedman didn't." (Lucas's interview by Snowdon and Vane 1998: 132).

The problem did not lie in Lucas's "Expectations and the Neutrality of Money" paper in itself. It was acceptable to general equilibrium theorists as it resembled the incursions into neighboring territories in which they sometimes indulged. What matters in Cass's quotation is the last sentence, that is, his perception that neo-Walrasian theory and macroeconomics pursue different purposes and hence must not be mingled. Contemporary neo-Walrasian economists regarded general equilibrium theory as an abstract construction, the strength of which lay in its ability to posit issues in a rigorous way. They were also aware of its limits. In their view, the best it could provide was to be a negative benchmark.

In a nutshell, behind the veil of their mathematical language, neo-Walrasians do political philosophy *à la* Rawls, while macroeconomists engage in applied work. Lucas may well have wanted macroeconomics to become Walrasian, but he also wanted it to keep its long-standing traits of empirical verification and policy conclusions. This was not to neo-Walrasian economists' liking.⁵ To them, testing their models empirically made little sense. As noticed by Roy Weintraub, "empirical work, ideas of fact and falsifications, played no role at all" in Walrasian theory (Weintraub 1983: 37). Or, in Hahn's words:

It is for all these reasons that I have always held the view that the Walrasian theory in all of its manifestations is an important theoretical benchmark but that a vast and unruly terrain had to be traversed before one understood (let alone predicted) the behavior of an actual economy. No economist and certainly no theorist should be ignorant of the Walrasian theory, and no economist and certainly no theorist should pronounce on actual economies and policies on its basis alone (Hahn 1983: 224).

In neo-Walrasian theory agents are price-makers. Hence there must be someone else announcing prices, the auctioneer. The auctioneer hypothesis, though necessary to explain the formation of equilibrium in neo-Walrasian theory, has little to commend it.⁶ Indeed, it amounts to pre-empting the main issue that should be addressed, that is, whether market forces are able to bring the economy to a state of efficiency.

As for policy conclusions, the problem is not just that neo-Walrasian theory evolves at a stratospheric level of abstraction. It is also that Keynes's indictment of classical theory because it relied on a 'Panglossian' vision of the economy (Pangloss is Candide's tutor in Voltaire's eponymous book) fully applies to neo-Walrasian theory:

The celebrated optimism of traditional economic theory, which as led to economists being looked upon as Cándides, who, having left this world for the cultivation of their

⁵ In this respect, Lucas was more in the line of the vision held at the Cowles Commission.

⁶ Solow declared it a 'swindle' in his review of the English translation of Walras's *Elements* (Solow 1956: 88).

gardens, teach that all is for the best in the best of all possible worlds provided we will let well alone ... (Keynes 1936: 53).⁷

Leaving the imaginary, best-of-all-possible worlds behind in order to study the malfunctions likely to arise in real-world economies is certainly not what occurs when taking the Walrasian track. Thus, with respect to Keynes's call to forgo the Panglossian vision, Lucasian macroeconomics is clearly a step backward. Economists such as Hahn and Solow, and many others, found this move outrageous:

The irony is that macroeconomics began as the study of large-scale economic pathologies: prolonged depressions, mass unemployment, persistent inflation, etc. This focus was not invented by Keynes (although the depression of the 1930s did not pass without notice). After all, most of Haberler's classic *Prosperity and Depression* is about ideas that were in circulation before *The General Theory*. Now, at last, macroeconomic theory has as its central conception a model in which such pathologies are, strictly speaking, unmentionable. There is no legal way to talk about them (Hahn and Solow 1995: 2-3).

Though I can understand Hahn and Solow's viewpoint, I am less categorical than they are. At stake is the internal/external consistency dilemma, the worthiness of engaging in theoretical detours and ... patience – in one of his *notes d'humeur* (casual annotations scribbled on pieces of paper) Walras wrote:

One must know what one is doing. If one wants to harvest promptly, one should plant carrots and salads; if one has the ambition to plant oak trees, one must be wise enough to say: [posterity] will owe me this shade (Baranzini and Allison (2014: 1).

A possible justification of Lucas's standpoint can be found in a 1989 *Journal of Economic Perspectives* by Charles Plosser. According to Plosser, the results of Keynesian economists' attempts to theorize malfunctions were disappointing because they were introduced in an *ad hoc* way, instead of being systematically derived from a theoretical core model. The lesson he drew was that it is wiser to start the analysis with the study of an idealized state of the economy. In his words:

Progress towards understanding this idealized state is essential because it is logically impossible to attribute an important portion of fluctuations to market failure without an understanding of the sorts of fluctuations that would be observed in the absence of the hypothesized market failure. Keynesian models started out asserting market failures (like unexplained and unexploited gains from trade) and thus could offer no such understanding. (Plosser 1989: 53)

Taking such a position is admissible at a condition that I suggest to call the 'non-exploitation principle.' Models *à la* Lucas may well have policy

⁷ Willhem Buitter dubbed his criticism of new classical macroeconomics, "The Macroeconomics of Dr Pangloss" (1980).

conclusions as their normal outlet. Yet, their builders should refrain from recommending these to decision makers. Clearly, this implies a strong dose of stoicism. To his credit, Lucas expressed awareness of this precept as the following passage from the concluding section of his “Understanding Business Cycles” article testifies:

By seeking an equilibrium account of business cycles, one accepts *in advance* rather severe limitations on the scope of governmental countercyclical policy which might be rationalized by the theory. (Lucas [1977] 1981a: 234, Lucas’s emphasis)

The important element of this quote is the term ‘in advance,’ italicized by Lucas. By stressing this, he admitted that the limitation on countercyclical policy, the policy conclusion of his model, followed from its premises. Again, while this point is only hinted at in Lucas’s published papers, more is to be found in the archives:

One now reads of rational expectations not in *Econometrica* but in *Time and Business Week*, where it appears as a ‘school’ or ‘theory’ with apparently sweeping implications for important issues of economic policy. These implications seem primarily of a ‘conservative’ cast, favoring a reducing role for government, balanced budget fiscal policy, and tight and ‘unaccommodating’ monetary policy. Now the idea of limited government, budget balance and tight money are not unimportant to me; they are high on the list of values I carry into the voting booth every year, and for reasons I am willing to defend in some detail. These developments are not, then, ones which I find unwelcome or displeasing, nor do I find the journalistic treatment of rational expectations any less accurate than similar treatments of other developments in economics. . . . There can be no simple connection between what appears on the scratch pads of professional economists, however original, and important conclusions about the way our society ought to operate. (Lucas, Various. Box 13, Directions of macroeconomics 1979 folder).

Interpreted in a loose way, this passage states that economists should be cautious when extending the policy conclusions of their models into direct advice to governments. Taken strictly, it means that economists should totally refrain from politically exploiting the results of their models.

LUCAS’S AMBIGUITIES

Wavering between monetarism and DSGE reasoning

Like Friedman’s 1968 article, Lucas’s 1972 paper aimed at demonstrating that monetary shocks can have real effects only in as far as they cannot be perfectly unanticipated. When Lucas expanded this into a model of the business cycle, he kept money shocks as the causal factor of fluctuations. However, a few years later, he admitted that RBC modeling, wherefrom money is absent, was the right line to follow. Lucas dated the ‘beginning of the end’ of his viewing the explanation of business fluctuations in terms of monetary shocks to the 1978 Bald Peak Conference where Kydland and Prescott presented an early

version of their “Time to Build and Aggregate Fluctuations” paper. Nonetheless, in an October 1982 correspondence with Leijonhufvud, who wrote in a letter to him that he did not understand why he was such a monetarist, Lucas kept claiming his monetarist allegiance.

‘My’ monetarism is simply Friedman’s. I’ve tried over and over again to make this clear, but I don’t think people take it seriously. I suppose this is because Friedman and I have such different styles of doing economics, but really, style has nothing to do with it. (Lucas. Various. Box 3. Letter to Leijonhufvud, October 28, 1982. Correspondence 1982 folder).⁸

True to himself, Lucas expressed the same viewpoint three decades later in the Introduction of the volume collecting his papers on monetary theory (Lucas 2013):

Now toward the end of my career as at the beginning, I see myself as a monetarist, a student of Milton Friedman and Allan Meltzer. My contributions to monetary theory have been in incorporating the quantity theory of money into modern, explicitly dynamic modeling. . . . It is understandable that in the leading operational macroeconomics models today – real business cycle models and new Keynesian models – money as a measurable magnitude plays no role at all, but I hope we can do better than that in the models of the future. (Lucas 2013: XXVI–XXVII)

These two statements must be related to an intriguing observation made by Sargent in an article commemorating the twenty-fifth anniversary of Lucas’s “Expectations . . .” article (Sargent 1996). Sargent’s point was that this article, which played such a seminal role, was actually “the first and last paper [Lucas] would write in this line” – a line that Sargent defined as “an unrelentlessly ‘deep’ approach to modeling monetary and macroeconomic phenomena in terms of explicitly spelled out phenomena” (Sargent 1996: 544), the very program that he set for himself.

The link to monetarism in Lucas’s *Journal of Economic Theory* paper was incidental to the methodology of the rational expectations program, but integral to the substance of Lucas’s own research program. (Sargent 1996: 544)

The first part of the quotation was confirmed by what happened afterwards, Kydland and Prescott’s transformation of Lucas’s model into RBC modeling from which the monetary dimension was absent, while nonetheless abiding by the DSGE program as set out by Lucas. As for Lucas’s own and distinct research program, Sargent characterized it as mere monetarism, that is, (a)

⁸ While nobody could be further from Lucas than Leijonhufvud, the latter nonetheless wrote in the letter to which the excerpt above was Lucas’s answer: “I haven’t read a better book (in economics) for years” referring to *Studies in Business Cycle Theory* (Lucas 1981a). The moral to be drawn is that economic theory is also a gentleman’s sport.

trying to integrate money in price theory while keeping the latter intact, (b) using quantity theory to explain inflation and (c) regarding business fluctuation as resulting from monetary disturbances. According to Sargent, the 1972 article was hardly the best vehicle to carry this monetarist project forward. This, he claimed, explains why afterward Lucas returned to “a more superficial and workable approach using arbitrary cash in advance restrictions” (Sargent 1996: 544).

Replacing the neoclassical synthesis dichotomy with another one

One of the criticisms of the Lucas-Rapping 1969 paper, voiced by Albert Rees (1970), was that Lucas and Rapping’s analysis implicitly assumed that all unemployment during the Great Depression was voluntary:

Though scientific discussion is supposed to be dispassionate, it is hard for one old enough to remember the Great Depression not to regard as monstrous the implication that the unemployment of that period could have been eliminated if only all the unemployed had been more willing to sell apples or to shine shoes. (Rees 1970: 308)

In their answer, Lucas and Rapping (1972) avoided discussing the meaning of involuntary unemployment, but admitted that their model was inadequate for tackling unemployment during the Great Depression.⁹ Lucas returned to the issue of the Great Depression on several occasions, mainly in interviews or book reviews, although never in much detail. Referring to RBC models, he reiterated the same view that such models were unable to explain the Great Depression and that one needed to return to Friedman and Schwartz’s analysis for such an explanation.¹⁰ That is, RBC models are apt to study periods of plain sailing (what later became known as ‘periods of moderation’) but ill-suited when it comes to more dramatic events such as the Great Depression:

In Kydland and Prescott’s original model, and in many (though not all) of its descendants, the equilibrium allocation coincides with the optimal allocation: fluctuations generated by the model represent an efficient response to unavoidable shocks to productivity. One may thus think of the model not as a positive theory suited to all historical time periods but as a normative benchmark providing a good approximation to events when monetary policy is conducted well and a bad approximation when it is not. Viewed in this way, the theory’s relative success in accounting for post-war experience can be interpreted as evidence that post-war monetary policy has resulted in near-efficient behaviour, not as evidence that money does not matter. (Lucas, 1994: 13)

⁹ Actually, Lucas wrote this response alone because Rapping had lost interest in the subject.

¹⁰ Other references are Lucas [1980a] 1981: 273, 284), Lucas (1987: 87), Klammer (1984: 41–2), Snowdon and Vane (1998: 125) and McCallum (1999: 284).

I personally agree with Lucas's standpoint. However, there is a flip side to it, which Obstfeld and Rogoff aptly pointed out:

A theory of business cycles that has nothing to say about the Great Depression is like a theory of earthquakes that explains only small tremors. (Obstfeld and Rogoff 1996: 627)

So, Lucas ended up supporting a divide between types of explanations according to the subject dealt with, the very idea that he strongly rejected *apropos* of the neoclassical synthesis!¹¹ This is another ambiguity in Lucas's thinking.

A POLITICAL AGENDA?

In an article entitled "The Fall and Rise of Keynesian Economics," Alan Blinder, an eminent Keynesian economist from Princeton University who also held important political appointments at the Council of Economic Advisers and the Board of Governors of the Federal Reserve Bank, assessed new classical macroeconomics with the following words:

I argue . . . that the ascendancy of new classicism in academia was instead a triumph of *a priori* theorizing over empiricism, of intellectual aesthetics over observation and, in some measure, of conservative ideology over liberalism. (Blinder [1988] 2001: 110)¹²

In this quotation Blinder made two points. First, he expressed his regret about the 'Walrasation' of macroeconomics spearheaded by Lucas; Blinder's words are close to those which Friedman used to dismiss Walrasian theory. His second point may be interpreted in two ways – either as meaning that the conclusions of new classical models were more conservative than those of Keynesian models, or as meaning that the ascent of new classical models resulted from a political motivation. The first of these statements is obviously right. The second amounts to stating that Lucas pursued a political agenda. In Chapter 4, I concluded that this was the case for Friedman, his declarations to the contrary notwithstanding. Can the same conclusion be reached about Lucas?

When trying to answer this question, the first observation to be made is that Lucas departed from Friedman's assertion that theory and ideology could be radically split. In Lucas's mind, macroeconomics is geared towards producing policy conclusions, and such conclusions necessarily support a particular ideological vision, to simplify either the free market solution or the Keynesian one. The two following quotations the first drawn from an interview with *The*

¹¹ This dichotomy led Prescott to depart from the Lucasian standpoint, which he had initially endorsed, by claiming that the RBC conceptual apparatus, if slightly changed, can account for great depressions – now without capital letters! Cf. De Vroey and Pensieroso (2006).

¹² The title of Blinder is not a typo. A few sentences after those I quote, Blinder writes: "macroeconomics is already in the midst of another revolution which amounts to a return to Keynesianism – but with a much more rigorous theoretical flavor" (Blinder 1988: 110).

Region, the second from a draft of his review of Tobin's Yrjö Jahnson lectures (Tobin 1982) – make the point:

Lucas: In economic policy, the frontier never changes. The issue is always mercantilism and government intervention vs. laissez faire and free market. (Lucas interviewed in *The Region* 1993: 3)

There are, it seems to me, two schools of macroeconomic (and perhaps all) social policies: one which keeps the power of government to injure in the front of its mind, and stresses policies which take the form of institutional constraints on government action, and another which focuses on the power of government to improve welfare, and seeks methods by which this power may be exercised more effectively. (Lucas. Various. Box 26, Directions of macroeconomics 1979 folder).

Admitting that there is inevitably an ideological dimension to economic discussions is one thing, liking it is another, and Lucas surely did not.¹³ Thus, since the ideological dimension could not be dispensed with, it had to be tamed.

Using the mathematical language, which allowed keeping hermeneutic and ideological discussions at bay, was according to Lucas a first way of doing so.¹⁴ A second way was to draw a sharp separation between those propositions that pertain to the fictitious model economy and those pertaining to the real world. This is supposedly true for the premises of a model, for example, rational expectations, but also for its policy conclusions. Making the methodological standards ruling model construction explicit was a third barrier. Under these conditions, Lucas argued, fruitful conversations can take place even if they will be unable to settle ideological disputes. The following two quotations, the first drawn from a draft fragment, the second from Lucas's correspondence, make the point:

The classical issue of the proper role of government in a democratic society, of 'laws versus men' or 'rules versus authority,' are not going to be settled by technical advances in economics. It follows that no one's position on such basic questions needs to be threatened by such new technologies as may come to be at our disposal [Lucas had in mind here rational expectations]. (Lucas. Various. Box 26, Directions of macroeconomics 1979 folder).

¹³ In a deleted passage from the introduction of his *Models of the Business Cycle* book, Lucas wrote: "I think it is fair to say that ideology plays an important role in contemporary discussion. There is probably no point in getting angry about this, but I do." (Lucas. Various. Box 13, Models of business cycles 1985-87 folder).

¹⁴ Lucas, quite bluntly, considers that economists such as Coase and Hayek, who broadly speaking share his vision of the economy, are more engaged in ideological rather than theoretical activities. "What I want from economics is a set of principles I can use to evaluate proposed government interventions, case by case, on their individual merits. I agree that explicit modeling can give a spurious sense of omniscience that one has to guard against, ... *But if we give up explicit modeling, what have we got left except ideology?* I don't think either Hayek or Coase have faced up to this question" (Lucas. Various. Box 8. Letter to K. Matsuyama, March 29, 1995. Correspondence 1995 folder; my emphasis).

Really, there is never going to be such a thing as an uncontroversial way to settle disputes over economic policy, nor do I see why one would hope for such a state of affairs. It seems to that our job is to try to make controversy *useful*, by focusing on discussable, analyzeable issues. In Taylor's first paper, for example, contract length was selected arbitrarily (hence 'controversially') and was central to the operating characteristics of the model. But labor contracts are something we can [?] independent evidence on (as John did) or theorize about (as many people are not doing). Work like this is productive not because it settles policy issues in a way that honest people can't disagree over but because it channels controversy onto potentially productive tracks, because it gets us talking and thinking about issues that our equipment may let us make some progress on. (Lucas. Various. Box 5. Letter to Sims, July 15, 1982. Correspondence 1983 folder).¹⁵

These different elements point to the conclusion that the criticism I address to Friedman of mixing ideology and theory must not be extended to Lucas. An additional factor in favor of this judgment is the observation made earlier about Lucas's awareness that the policy conclusions of models are embedded in their premises and must not be peddled to policymakers. This is a statement that Friedman would not have uttered.

CONCLUDING REMARKS

Let me begin with a general impression about the present-day perception of Lucas's role in the history of macroeconomics. As mentioned in the Preface, last year I taught a graduate course on the subject matter of this book to third- or fourth-year doctoral students. It made me realize how unfamiliar they were with Lucas's work. They knew his name, they were aware that he had played a pioneering role in macroeconomics, but that was it. I see this as a testimony to how fast one may become *passé*. Leaving nostalgia aside, I find that a loss in knowledge ensues, which I regret. Of course, this is not the students' fault, but the result of how graduate programs are conceived with too much focus on technique. For their part, economists working at the cutting edge of the profession tread in Lucas's footsteps, but feel no need to refer to his views. They just look forward. The result is that most of what is written about Lucas comes from opponents to his views. On the one hand, there are those who dislike Lucas's work for ideological reasons. They take it for granted that it is part of the weaponry of 'ultra-liberalism,' which they regard as a sufficient condition for dismissing it without delving into its intricacies. On the other hand, there is also a dissension within the macroeconomics profession. It comes from defenders of the neoclassical synthesis viewpoint (meaning a defense of a pluralistic macroeconomics field). The objection here is against the Walrasian hegemony that Lucas wants to impose on the field. My surmise is thus that many of the critical positions taken against Lucas within the macroeconomics profession

¹⁵ The work by Taylor to which Lucas refers is staggering contract modeling; it will be studied in Chapter 13.

have to do with the Marshall-Walras divide. I would not be surprised if a high correlation between a disinclination for Walrasian theory and one for Lucas's approach existed.

My own viewpoint about this divide is relativistic as behoves a historian of economics. Where I depart from the critics of Lucas is that I do not share their view that following the Walrasian paradigm is per se a flaw, while monetarists and Keynesians see eye to eye in this respect. Therefore, I have no objections of principle against Lucas's methodological canons, which have a Walrasian affiliation (except for what concerns empirical verification). What I want to insist on is the need to acknowledge the limitations of adopting the Walrasian approach. This is often not the case, Lucas himself being an exception here.

My general standpoint is that programs must be judged on their results. More generally, what I find important when assessing theoretical lines is less their value at the time of their rise than their posterity, that is, whether they have led to cumulative developments – in terms of Leijonhufvud's metaphor, whether a frail branch developed into a sturdy one.¹⁶ Sargent put this nicely in an interview with Klamer:

When we do research, the idea is that you don't produce a finished produce. You produce an input. You write the paper with the hope that it will be superseded. It's not a success unless it's superseded. Research is a living process involving other people. (Klamer 1984: 74)

The subsequent chapters will amply attest that this has been the case for the DSGE program.

I have also shown that Lucas, in spite of his strong desire for consistency, has been unable to avoid ambiguity. This is of course a flaw, but it is an understandable one – after all economics is a social science and the quest for epistemological purity may be fruitless if pushed too far. Lucas's main ambiguity was his twofold allegiance to neo-Walrasian theory and to empirical verification. Did this amount to wanting to have the best of both worlds, or was it a bold move that would later prove to have been productive? As Lucas himself did not engage in implementing his empirical injunction, again the answer to this question must await my examination of the works of those who set this task out for themselves. Still, I may already ponder on his view of models as analogous systems. I surmise that thereby Lucas was hoping to break the deadlock of the non-exploitation principle. If analogous models can be proven to be empirically robust, they can be used to compare the outcomes of alternative policy measures. As a result, policy recommendations would receive some 'scientific'

¹⁶ Compare Hayek's papers on knowledge and its role in the functioning of market economies (Hayek 1948) with Walras's *Elements of Pure Economics*. The immediate value of the former to understand the economy is incomparably superior to that of Walras's *Elements*. Yet, as far as cumulative development is concerned, at the risk of upsetting my Austrian economist friends, I find that the latter has fared much better than the former.

vindication allowing to soften the non-exploitation principle. It remains that, to me, this ambition to straddle external and internal consistency is utopian. The replication discipline is surely useful, yet not to the point of lifting the non-exploitation requirement. As for the other ambiguities, let me just say that I find Sargent's remark about Lucas's 1972 paper, stating that it was almost an accident with regard to his perennial monetarist vision, enlightening. I like what it suggests, namely that models have a life of their own that may evolve independently from the motivation and vision of those who created them.

Early Reactions to Lucas

To many Keynesians, the new classical program replaced messy truth by precise error. (Lipsey 2000: 76)

The bulk of this chapter will consist in my examination of how traditional or 'old' Keynesians, with such leading figures as Tobin, Modigliani, and Solow, reacted to the transformation of macroeconomics advocated by Lucas.¹ First, however, I will briefly discuss a reaction of a different nature, namely Christopher Sims's dismissal of the Lucas critique on the grounds that a substantive gain in economists' knowledge of the economy would accrue from making less rather than more use of economic theory. As for the reactions of old Keynesians, I will consider several aspects: their general attitude toward rational expectations, Robert Gordon's attempt at salvaging the Phillips relation, and Arthur Okun's proposal to integrate search elements into the Keynesian framework. Special attention will be given to one special bone of contention between Lucas and traditional Keynesians, their disagreement over the notions of market clearing and involuntary unemployment. The chapter ends with an assessment in which I try to identify the reasons for this deep clash.

SIMS'S CRITICISM OF THE LUCAS CRITIQUE

From the onset of his research career, Sims, a time-series statistician, was driven by the strong conviction that business cycles should be studied with as little as possible a priori economic theory. In 1969, Granger had introduced what became known as the 'Granger causality' test, aiming at detecting sequential

¹ The "old Keynesian" term should not be taken negatively. Tobin did not mind using it (Tobin 1992, 1993).

BOX 12.1 The VAR approach

The VAR approach is a statistical method allowing one to grasp the linear interdependence among time series. The following three quotations will allow the non-informed reader to get a first understanding of what the VAR and structural VAR approaches are about. The first one is an extract from the Scientific Background note issued by the Royal Swedish Academy of Science in 2011 on the occasion of Sargent and Sims's Nobel Prize, the second and the third one from a *Journal of Economic Perspectives* article by Stock and Watson (2001).

VAR analysis can be described in simple terms as a method for extracting structural macroeconomic shocks, such as unexpected exogenous shocks to the central bank's main policy instrument (e.g., the federal funds rate in the U.S.) or unexpected exogenous changes in productivity, from historical data and then analyzing their impact on the economy. Thus, this analysis is a tool for (i) estimation of a forecasting model, by separating unexpected movements in macroeconomic variables from expected movements; (ii) identification, by breaking down these unexpected movements into structural shocks, that is, shocks that can be viewed as fundamental causes of macroeconomic fluctuations; (iii) impulse-response analysis, by tracing out the dynamic impact of these shocks on subsequent movements in all of the macroeconomic variables. (Royal Swedish Academy of Science Citation 2011: 13)

A univariate autoregression is a single-equation, single-variable linear model in which the current value of a variable is explained by its own lagged values. A VAR is a n -equation, n -variable linear model in which each variable is in turn explained by its own lagged values, plus current and past values of the remaining $n-1$ variables. (Stock and Watson 2001: 101)

A *structural VAR* uses economic theory to sort out the contemporaneous links among the variables. Structural VARs require "identifying assumptions" that allow correlations to be interpreted causally. These identifying assumptions can involve the entire VAR, so that all of the causal links in the model are spelled out, or just a single equation, so that only a specific causal link is identified. (Stock and Watson 2001: 101)

causality between time-series variables (Granger 1969). Granger's work influenced both Sims and Sargent, who for most of the sixties were colleagues at the University of Minnesota. This led them to write a joint paper entitled "Business Cycle Modeling without Pretending to Have too Much *a priori* Economic Theory" — a title that perfectly captured what Sims had in mind, namely letting the data speak freely. It was presented at a Conference at the Minneapolis FED held in 1977 (Sargent and Sims 1977), where it stirred a strong opposition from Keynesian economists such as Klein, Ando and Gordon.² Unlike Sargent, Sims stuck to this vision for the remainder of his career, and became the leading figure of the VAR approach, an identification strategy, in which independent and transient random variables drive the model.³ Later, Sims realized that VAR models did not provide the theoretical restrictions needed for policy analysis. As a result, he moved towards VAR with restrictions, known as structural VAR models, the tool which later second-generation new Keynesian economists were to use to get their stylized facts.

Returning to my main thread, Sims, unlike the Keynesians, did not regret the 'rational expectations revolution'. The latter was needed, he believed, because "the old regime was corrupt and in some sense deserved its fate." To him, Keynesian models could not be taken seriously because

... they included so many *ad hoc* assumptions that were treated as if they were certain *a priori* knowledge and because they were estimated by methods that were clearly dubious in this sort of application, were not taken seriously as probability models of the data, even by those who built and estimated them. Measures of uncertainty of forecasts and policy projections made with the models were not reliable. (Sims 2011: 11)

Sims's complaint about Keynesian econometric models was that they were badly identified, not that they were too big. Actually, he had no objections against estimating "profligately"; it was just that one should avoid accumulating restrictions as haphazardly as in Keynesian models (Sims 1980: 14). Furthermore, he was of the opinion that these models, in spite of their flaws were useful tools because, in his view, structural identification was not needed for forecasting and policy analysis. On the contrary, false restrictions could

² To limit myself to one reaction, Klein found their approach "disappointingly retrogressive and contrary to the mainstream of econometric analysis today" and the causality lines they claimed to have established made no sense (Klein 1977: 203). He argued that the famous criticism that Koopmans had addressed to the NBER research program, namely that it lacked any theoretical basis, could validly be extended to Sargent and Sims's paper.

³ Sargent soon diverged from Sims over the usefulness of the VAR method. Contrary to Sims, Sargent wanted macroeconomics to be anchored in *a priori* theory. In his eyes, the task ahead for escaping the Lucas critique was to construct convincing specific models of the economy, based on 'deep parameters' and to be estimated in a second stage (Hansen and Sargent 1980). For a description of the many twists that Sargent's endeavor encountered, the reader is referred to Sent (1998) and Boumans and Sent (2013).

help the modeling process (Sims 1980: 11). What he disagreed on with Lucas and Sargent was their view that providing macroeconomic models with stronger microfoundations would solve the identification problems that plagued Keynesian models.

As for the Lucas critique itself, Sims raised three objections against it. The first and main one was that the notion of policy regime, which is central in Lucas's argument, is ambiguous (Sims 1980: 12). He argued that changes which deserve to be labeled a 'change in policy regime' are scarce. Policy changes usually consist in a mere modification within an already established rule – for example, a modification of the tax rate or the interest rate – instead of a full-fledged change of the rule. In other words, what Lucas considers as 'policy changes' are usually changes in the modality of the implementation of a given policy rule rather than a one-time, unprecedented, shift in regime as assumed by Lucas. For such cases, reduced form modeling does a fine job (Sims 1980: 13; 1982: 107–108).

Sims's second criticism followed from his endorsement of Trygve Haavelmo's conception of empirical work. Distancing himself from Ragnar Frisch, Haavelmo had argued that econometric models must include a characterization of their error terms and hence adopt a probability approach.⁴ Sims's contribution consisted in trying to improve on Haavelmo's work on two scores. The first was to replace "frequentist-hypothesis testing" with the Bayesian perspective. The second was that, while praising Haavelmo for having introduced policy behavior equations in his models, he nonetheless disagreed with his depiction of policy changes as exogenous to the stochastic structure of the model. In Sims's view, policy changes were to be portrayed as the realization of random variables, yet they needed to be part of the model's structure. Once a Bayesian perspective on inference is adopted, policymaking can be considered as endogenous from the point of view of the policymaker, but exogenous from private agents' viewpoint. Sims's criticism of new classical macroeconomics *à la* Lucas and Sargent was then that it perpetuated Haavelmo's mistaken standpoint (Sims 2012).

Finally, Sims's third criticism was that the admission of the Lucas critique had exerted too strong an influence on the profession. As a result, the view that "the bread and butter of quantitative policy analysis . . . was somehow deeply mistaken or internally contradictory" became unduly widespread (Sims's interview by L-P. Hansen [2004] 2007: 224). By the same token, the type of issues faced by the staff of monetary policy institutions vanished from the theoretical agenda.

The general conclusion drawn by Sims was that the goal of Lucasian macroeconomics was too ambitious and will probably never be attained. Lucas's too demanding principles should not condition the agenda of the profession. Instead, "We should be improving our methods for estimating

⁴ Morgan (1989), Louçã (2007) and Qin (1993) are standard sources on the early history of econometrics.

and using statistical models that do not require identifying such parameters. Most policy analysis does not require that kind of identification” (Sims 1982: 151). Quite normally, Sims’s ideas evolved over time. Nonetheless, he did not turn away from the basic insight that triggered his theoretical quest. When asked by Hansen whether his thinking about identification changed over time, he answered negatively:

I’m still skeptical of tightly parameterized models. I think the most reliable way to do empirical research in macroeconomics is to use assumptions drawn from ‘theory,’ which actually means intuition in most cases, as lightly as possible and still develop conclusions. (Hansen [2004] 2007: 218)

As for the positive program that Sims recommended, it consisted in devising more convincing identification schemes. Many different types of shocks can be studied. For his part, from his early research years onward, Sims focused his attention on the issue of the real effects of monetary changes, which had marked the dividing line between Keynesians and ‘classical’ economists since Modigliani’s 1944 paper.

Eventually Sims’s views became a game changer, yet it took time for the structural VAR approach to be widely adopted by macroeconomists, first because of the very reason he had himself pointed out, namely the inhibiting effect of the Lucas critique, and, second, because of the lack of attention to monetary effects resulting from the prominence of RBC modeling. This is why I postpone the examination of Sims’ study of the real effects of monetary changes until Chapter 18, where I will study second-generation new Keynesian models.

LUCAS AND KEYNESIANS: THE FIRST SKIRMISHES

Mankiw once commented on the Lucasian revolution by writing, “During the 1970s, these young Turks led a revolution in macroeconomics that was as bloody as any intellectual revolution” (Mankiw 1992a: 21). He was a right. Unlike Friedman, who always adopted a subdued tone, never shy of complimenting Keynes before firing at Keynesian policy, Lucas did not pull his punches. The “After Keynesian Macroeconomics” paper, co-authored with Sargent and presented at a conference organized by the Federal Reserve Bank of Boston in June 1978, strongly exemplifies this attitude. It was a kind of manifesto for the new classical approach, its mere title acting as a bombshell.⁵ As for the paper’s aim and motivation, Luca and Sargent summarized it as follows:

⁵ In later interview by Snowdon and Vane, Lucas commented on the paper in the following way: “*Snowdon and Vane*: That paper contains a lot of powerful rhetorical statements. Were you conscious of this at the time of writing? *Lucas*: Yes. We were invited to a conference sponsored by the Boston Fed. In a way it was like being in the enemy camp and we were trying to make a statement that we weren’t going to be assimilated” (Snowdon and Vane [1998] 2005: 282).

Our intention is . . . to extend the equilibrium methods which have been applied in many economic problems to cover a phenomenon which has so far resisted their application: the business cycle. (Lucas and Sargent [1979b]1994: 28)

The paper starts by re-exposing the Lucas critique. Next, Lucas and Sargent developed the same argument as in the “Understanding Business Cycles” paper, indicting Keynes for having abandoned the equilibrium discipline. They continued by explaining the reason why the traditional center of gravity concept of equilibrium should be replaced with a really dynamic concept. They also stressed the intertwining of rational expectations with general equilibrium analysis. Finally, they responded to the different criticisms addressed to new classical models pertaining to the market clearing hypothesis, the lack of accounting for persistence, linearity, and the neglect of learning behavior. Though its argumentation was solid and principled, the paper added little to Lucas’s earlier papers, yet its tone was scathing.

Jointly with William Poole, Solow was in charge of reviewing the different papers presented at the conference. In his comments, in addition to finding their proposal for constructive research hard to talk about sympathetically, he picked up from the paper a list of characterizations of Keynesian macroeconomics that went too far in his eyes: “wildly incorrect,” “fundamentally flawed,” “wreckage,” “failure,” “fatal,” “of no value,” “dire implications,” “failure on a grand scale,” “spectacular recent failure,” and “no hope” (Solow 1979 203–204).⁶

Benjamin Friedman, whose name we already encountered in Chapter 4, was their discussant (Friedman, B. 1979). His response was as sharp-tongued as their paper. He stated that he would have accepted Lucas and Sargent’s criticism had they merely argued that the treatment of expectations in modern macroeconomics was wanting and that they had made significant progress in this respect. What he strongly objected to was that a “fundamental methodological departure from the corpus of Keynesian macroeconomics” ensued (Friedman, B. 1979: 80) – the very point that they wanted to make.

Friedman’s main objection was that equilibrium business cycle models had no more monopoly over optimizing behavior than Keynesian macroeconomics had over *ad hoc* arbitrary restrictions (Friedman, B. 1979: 80). He listed the Keynesian models that have explicit microfoundations, such as life-cycle models of consumer behavior and the study of portfolio behavior. He also mentioned Klein’s early writings underlining the importance of microfoundations. As to the arbitrariness present in Lucas’s model, Friedman pointed out the assumption made about the chronological order in which agents learn

⁶ Poole, associated with the Federal Reserve Bank of Missouri, a stronghold of monetarism, disagreed with Solow arguing that “The criticisms of Sargent-Lucas reflect model-defenders’ efforts to maintain the intellectual case for large macromodels when everyone realizes that the models cannot possibly provide correct predictions of the effects of certain policies” (Poole 1979: 212–213).

about prices. Changing this order results in a supply function different from that of Lucas and Rapping, with prices and quantities now negatively correlated. Friedman also criticized Lucas and Sargent for their exclusive adoption of a long-term perspective which he regarded as failing to address issues related to a shorter time frame. Finally, he expressed his irritation with Lucas and Sargent's arrogance which, he felt, pervaded their paper: "A lower rhetorical profile would better advance the cause of scientific interchange" (Friedman, B. 1979: 80).

The problem for Friedman was that he had a tough task before him. The new approach that Lucas and Sargent were proposing was such a radical change in paradigm and had invaded the scene so suddenly that those trained in the old paradigm were caught unprepared to react to it. Friedman should have admitted rather than denied that the Lucas-Sargent program marked a fundamental departure from Keynesian macroeconomics. They wanted the equilibrium discipline to be a compelling standard, while Keynesians advocated a choice between the assumptions of optimizing behavior and 'rule of thumb' behavior according to the problem at hand. Again, looming behind this choice is the attitude toward the neoclassical synthesis. On this basis, Friedman might have tried to make his argumentation bear on the appositeness of Walrasian macroeconomics instead of focusing on secondary points, none of which were compelling. In their "Response to Friedman," written after the conference, Lucas and Sargent hardly soften their tone.

If this research [Keynesian models] is flawed in some essential way, it is difficult to see how softening our rhetoric will help matters. If the implications we have drawn are close to the mark, how can "the cause of scientific interchange" be best served by summarizing them in a way which averages what we believe to be true with what others find pleasant or familiar? (Lucas and Sargent 1979b: 82)⁷

A second example of the stormy exchanges that were taking place at the time is a talk given by Lucas in 1979 at the Graduate School of Chicago's Annual Management Conference entitled "The Death of Keynesian Economics," a paper already mentioned in Chapter 9. Here, my concern is its inaugural paragraph. It runs as follows:

The main development I want to discuss has already occurred: Keynesian economics is dead (maybe 'disappeared' is a better term). I do not exactly know when this happened but it is true today and it was not true two years ago. This is a sociological not an economic observation, so evidence for it is sociological. For example, one cannot find a good, under-40 economist who identifies himself, works as 'Keynesian.'

⁷ After victory comes the time for magnanimity. In a 2011 round table on rational expectations, Lucas commented on the Lucas-Sargent article in the following terms: "The paper that Tom and I gave at that conference was badly written and I can't stand to reread it. The writing is mine. It's not a milestone in economic research" (Hoover and Young 2011: 28).

Indeed, people even take offence if referred to in this way. At research seminars, people do not take Keynesian theorizing seriously any more – audience starts to whisper, giggles to one another. Leading journals are not getting Keynesian papers submitted any more. I suppose I, and with many others, were in on the kill in an intellectual sense, but I do not say this as any kind of boast, even with much pleasure. Just a fact. True, there are still leading Keynesians – in academics and government circles – so Keynesian economics is alive in this sense – but this is transient, because there is no fresh source of supply. Only way to produce a sixty year old Keynesian is to produce a thirty year old Keynesian, and wait thirty years. So implications for policy will take a while to be evident – but they can be very accurately predicted. ([1979] 2013: 500–501)

This passage eventually took on a life on its own, separated from the rest of the paper it inaugurated, and was quoted time and again as a charge against Lucas.⁸ Interestingly enough getting hold of the full paper has proved difficult. My requests for a copy to the people who had quoted it were always met with the answer that they did not have it, what suggests that they had not read the paper in its entirety. Lucas gave me a similar answer. Luckily, I was able to find a draft version of the paper in the Lucas Archives). Since then, it has been republished in Lucas (2013).

This passage is clumsy and arrogant. Lucas was right in predicting that traditional Keynesian macroeconomics had little future, but he should not have derided Keynesians and the Keynesian project.⁹ The irony is that the rest of the paper is different from its inaugural passage, as it focuses on the disappearance of the neoclassical synthesis consensus as discussed in Chapter 9. In short, Lucas would have been better inspired to entitle his lecture “The fall of the neoclassical synthesis” instead of “The death of Keynesian economics” and delete its introductory paragraph (but at the time he probably did not imagine that it would be seized on later).¹⁰

⁸ Mankiw quoted it in his paper, “The Macroeconomist as Scientist and Engineer” (2006[p. 34]. In Google Scholar, it is mentioned as being quoted sixty-five times (April 24, 2014).

⁹ In his review of McCracken *et al.*'s “Towards Full Employment and Price Stability” OECD report (Lucas [1976b] 1981a), Lucas shows an awareness of this point: “It seems certain that Keynes's thought will continue to stimulate economic theorists in various and unpredictable ways for the foreseeable future, so much that many economists will think of themselves as ‘Keynesians.’ In advance of seeing these developments, one cannot presume to pronounce them failed. I am here using the term ‘Keynesian’ much more narrowly, to refer to the multiplier calculation, which all of us understood Heller to be discussing and applying, together with the underlying, if less precisely specified, theory which provided guidance as to the range of circumstances under which these calculations might be expected to yield accurate answers” (Lucas [1976b] 1981: 265–266).

¹⁰ In 1977, Sargent wrote a working paper with a similar title, “Is Keynesian Economics a Dead End?” developing ideas close to Lucas. Though it remained unpublished, it was more accessible than Lucas's, yet it led to almost no comments (Sargent 1977).

THE RATIONAL EXPECTATIONS HYPOTHESIS: FROM
RESISTANCE TO A RELUCTANT ACCEPTANCE

Macroeconomists' first reaction to rational expectations was one of deep distrust.¹¹ Two objections were made. The first was its restricted relevance. For example, in his comments on Prescott's paper criticizing optimal control, Modigliani (Modigliani 1977b: 89) argued that the rational expectations assumption perfectly fits some particular markets, such as speculative markets, but not others, in particular the labor market, and hence is invalid at the macro level. Such a criticism misses the point, however, because as seen earlier, Lucas defended rational expectations as a "technical model-building principle, not a distinct, comprehensive macroeconomic theory" (Lucas 1981a: 1) and hence should not be judged on its empirical validity.

The second, stronger, objection related to learning and the acquisition of information. Tobin alluded to it at a 1970 Washington Conference where he was the discussant of Lucas's "Econometric Testing of the Natural Rate Hypothesis" paper. In his comment, Tobin noted that Lucas ascribed too much knowledge to agents "The participants must be better econometricians than any of us at the conference" (Tobin [1972] 1987: 30). Later, the same judgment was voiced by Robert Shiller (1975) and other economists, amongst whom, again, Benjamin Friedman (Friedman, B. 1979). The dust had settled, and Friedman was able to improve on his earlier comments on Lucas and Sargent. He argued that rational expectations can be understood in a weak or in a strong sense. In the first, it is just assumed that agents use whatever information is available efficiently. The second implies a more precise specification of the information available. It is taken for granted that "people form their expectations as if they know, to within a set of additive white-noise disturbances, the structure of the economic process itself" (Friedman, B. 1979: 26). The problem is that nothing is said about how agents derive their knowledge about the overall behavior of the economy.¹² In terms of substance, this second criticism is on point, except for one crucial aspect, tractability. Learning is part of a broader process, the

¹¹ The following extract from a letter Lucas wrote to Prescott dated December 2, 1968 and describing a seminar experience testifies to this: "... The rest of the time was spent on the assumption of rational expectations: what does it mean, is it reasonable, and so on. On the whole, I think I won very few converts to our view on this: most people felt it was an unreasonable assumption – and that we somewhat 'cheated' in postulating equilibrium at each point in time" (Lucas. Various. Box 1. Letter to Prescott, December 2, 1968. Correspondence 1968 folder).

¹² Frydman alone and in joint work with Phelps recurrently made the same claim (Frydman 1982: 664; Frydman and Phelps 1983: 5 and 17). Frydman and Phelps (2013) is a more recent restatement of their view. The fact that Phelps distanced himself from the rational expectations assumption is worth underlining in view of his closeness to Lucas in other respects.

formation of equilibrium. We thus fall back on the issue of the auctioneer hypothesis and its associated assumption of instantaneous formation of equilibrium. So, new classicists might retort to Friedman that the construction of learning models indeed should be high on the agenda, but that as long as no sufficient results have been obtained, one should continue using the traditional premises.

With the passing of time, the outright rejection of the rational expectations hypothesis receded. This happened in a rather convoluted way. To Keynesians, what mattered was less rational expectations per se than the Phillips curve. In the beginning, they did not feel threatened by Lucas's conclusion, no more than they had by Milton Friedman's. They could accept a vertical long-period Phillips curve as long as the short-period curve remained negatively sloped and allowed for the effectiveness of monetary policy. The policy implications of the natural rate of unemployment and the Lucas-Rapping supply function, they argued, could be ignored as long as the coefficient of the feedback of price inflation on wages was significantly lower than 1. Unfortunately for them, while this had been the case earlier, it stopped when inflation accelerated from the second half of the 1960s (Gordon 1976: 193). An even stronger thrill was caused by Sargent and Wallace's 'policy ineffectiveness proposition,' evoked in Chapter 9, but it also was short-lived. A way out for Keynesians came when they realized that, while Sargent and Wallace attributed their result to the presence of the rational expectations assumption, an alternative interpretation was possible, namely, that it stemmed from their model being based on the assumption of flexible prices and wages. This awareness paved the way for new research, staggering contract modeling, in which rational expectations coexisted with partially rigid wages, this time better founded than in the earlier days, an argument initiated by Fischer and to be discussed in the next chapter.

Two consequences ensued. First, the attitude of Keynesians changed as they came to the viewpoint that the rational expectations assumptions could be useful for some purposes without therefore becoming hegemonic. Second, the burden of the dispute between Keynesians shifted from a discussion about the acceptability of rational expectations to that of market clearing. As stated by Tobin, of the two pillars of new classical macroeconomics (rational expectations and market clearing), it is the second "which is crucial for the far-reaching implications of the doctrine" (Tobin 1980: 22).

TRYING TO SALVAGE THE PHILLIPS CURVE

The problems of Keynesian macroeconomics started with the transformation of the empirical Phillips relation into the 'Phillips Curve,' a move that was supposed to fill the gap of the otherwise 'missing equation' of the Keynesian system. However, what began as a solution ended up as "wreckage" (Lucas and Sargent [1979] 1994: 6), itself the first step in a broader demise. In this section, I describe

Gordon's exercise in damage control. If there was one economist who took it as his mission to rescue the Phillips curve, it was him. Acting, on the motto of not throwing the baby out with the bathwater, he decided to enrich the Phillips relation.¹³ Up to then, the demand shock had been the prime mover of this relation. Gordon came to thinking that two additional factors needed to be considered: inertia and supply shocks. Because three elements were present, he called it the 'triangle conception.' Inertia is just another name for sluggishness in wage adjustment.¹⁴ Like Friedman, Gordon claimed that shocks have a delayed effect on quantities. For example, he documented that a series of monetary shocks shows a one-year lead over variations in employment, a sign of inertia. As for supply shocks, Gordon's preferred example was an adverse oil shock (Gordon 2011: 22). The model he constructed to study its impact rested on three assumptions. First, he assumed that oil prices are fixed in auction markets, while other markets function as 'customer markets' *à la* Okun. As a result, the oil price is flexible in contrast to the prices of other goods and services, which are sluggish. Additionally, he assumed that at the start of his analysis the economy is in full employment and that the demand for oil is inelastic. It follows from these assumptions that an increase in the price of oil leads to a higher relative share of expenditures on oil and a lower one for all the other components of spending. The effect of the oil shock on unemployment then depends on two factors, nominal wages and governmental action. If nominal wages are flexible, full employment can be maintained and the growth rate of nominal GDP remains unchanged. In the opposite case of rigid nominal wages, full employment can be maintained through an accommodating monetary policy. The latter "must boost nominal GDP growth by the amount needed to 'pay for' the extra spending on oil; but this will lead to an inflationary spiral if expectations respond to the observed increase in the inflation rate" (Gordon 2011: 21). Finally, there is the intermediate case in which rigid nominal wages and a partially accommodating monetary policy are combined. Now, an upward-bending Phillips curve ensues. More generally, whenever the triangle conception is adopted, different output/inflation configurations are conceivable: "aggregate output and the rate of inflation could be positively or negatively correlated depending on the relative importance of aggregate demand or supply shocks" (Gordon 2011: 22). Gordon concluded that, once the Phillips relation is augmented in this way, stagflation can be accounted for. Hence, there is no reason to proclaim the wreckage of

¹³ Gordon (2011) is a resumption of his early contributions.

¹⁴ Gordon has constantly endorsed the non-Walrasian equilibrium paradigm *à la* Malinvaud: "The essential truth of this paradigm is evident in almost every country in the world in 2008–09 when we ask, does each member of the labor force have the free choice of working the desired number of hours at the going wage and price? Does each firm, whether a large or a small business, find it possible to sell the optimal level of production at the current wage and price? The answer of course is a resounding 'no'" (Gordon 2009: 12).

Keynesian macroeconomics: the view that the Phillips curve is necessarily downward sloping must be abandoned, but Keynesian macroeconomics is rescued.

Gordon's argumentation provides a simple yet clever retort to Lucas's claim that the stagflation episode marked the demise of Keynesian macroeconomics. What Gordon missed, however, is that Lucas's argumentation *a propos* the Phillips curve was only one shot within a broader criticism of the Keynesian paradigm. This was another illustration of Keynesian economists' failure to take the measure of Lucas's criticism of Keynesian macroeconomics. In Gordon's eyes, the line of research opened by Lucas was so implausible that he could hardly believe that it could be taken up in earnest by the profession. Nor did he realize that the demise of Lucas's monetary shock model was hardly tantamount to that of the Lucasian program. Therefore, Gordon's incessant pleas for macroeconomists to return to traditional Keynesian macroeconomics on the rationale that the triangle argument saved it from Friedman's and Lucas's attack, were met with less success than he hoped for.

OKUN'S SEARCH PERSPECTIVE

Another attempt at giving a new life to Keynesian theory was due to Okun. Okun's career led him from academics (at Yale) to policymaking (at the President's Council of Economic Advisers (CEA), first as a staff member and eventually as its chair in 1968 and 1969), after which he returned to research at the Brookings Institution. His posthumous book, *Prices and Quantities: A Macroeconomic Analysis* (Okun 1981) sprung from his desire to re-assess the problems that were his daily bread at the CEA, in particular unemployment and inflation, while taking stock of the collapse of the Keynesian Phillips curve manifested by the stagflation episode.¹⁵ Like Lucas, Okun was dissatisfied with Keynesian macroeconomics, but his aim was to reconstruct it rather than push it aside. Nor did he conceive his research line as an alternative to Lucas's; Lucasian macroeconomics was not yet a compelling reference when he started working on his book. Yet, this was no longer the case at the end of the 1970s, when the book was close to completion. The last piece he wrote before passing away was a critical assessment of Lucas's theory (Okun 1980). Its gist was a refusal to buy the idea that a valid theory of business cycles could be obtained from merely distilling a drop of misperception in an otherwise standard simplified Walrasian model. The assumption of a universal auction market on which it was based made it inadequate for such a task (Okun 1980: 875). While such a judgment was hardly original, in Okun's case it was backed up by an articulated alternative research line, the very one he had developed in his then almost finished book.

¹⁵ At the time of Okun's death, the manuscript was almost completed. His Brookings colleagues edited and finished it.

In Chapter 9, I described Lucas's theoretical contribution using Leijonhufvud's decision tree. It amounted to returning to the basic Marshall-Walras bifurcation node and deciding for the Walrasian rather than the Marshallian route. Okun's work also followed some backtracking process, but his preferred backtracking was different. His departure from Keynesian macroeconomics was twofold and bold. First, he was of the opinion that supply and demand analysis failed to come to grips with the real way in which markets function. Second, unlike most Keynesian macroeconomists, he saw no reason to keep Keynes's involuntary unemployment notion. To him, Keynes macroeconomics would be better off if based on frictional unemployment. He believed that by making these modifications Keynesian macroeconomics would be revived.

Okun's starting point was that, with a few exceptions (asset markets), in reality markets functioned differently from what could be found in economic theory. To capture this feature, he renamed labor markets 'career markets' and product markets 'customer markets.' As for the agents in these markets, they were depicted as stakeholders involved in a long-term personal relationship. An eclectic mind, Okun did not hesitate to borrow ideas from several of the scattered models that were emerging at the time, search theory, natural rate of unemployment theory, and even the Lucas-Rapping model.¹⁶ To convey a flavor of the richness of his analysis, let me enumerate the steps he took when studying the way the labor market operates. He began by adopting the search perspective. Workers sample firms, for instance one firm per day. They have no knowledge of individual offers but some knowledge of the market-wide distribution of wages. The acceptance of offers depends on their reservation wage (the higher it is, the longer the longer unemployment spell) and the time horizon they attach to any wage premium. This framework suffices to make unemployment a normal outcome, unlike what happens when supply and demand analysis is adopted. Okun's second step consisted in introducing what he called the 'stale information amendment,' that is, assuming that workers' information about the distribution of wage offers is imperfect, to the effect that they can be fooled by changing labor market conditions. "Information lags lead people to search less optimally when labor markets tighten (and unduly long when they ease) thereby shrinking (swelling) unemployment" (Okun 1981: 38). This is how expectations enter the picture as an explanation for the fluctuations of unemployment. In a third step, Okun

¹⁶ To Okun, misperception needed to be an ingredient of business cycle theory but not its unique explanatory factor. Other points about which he agreed with Lucas were: (a) the view that the separation between micro and macro should be bridged; (b) the view that agents should be depicted as rational and forward-looking; (c) the criticism that Keynes was unable to link micro with macro except by tampering with the supply of labor, and (d) the conclusion that the failure of the Phillips curve was the sign of the flawed character of Keynesian macroeconomics (Okun 1980: 817-818).

purported to bring additional elements of reality into the earlier material, such as the existence of lasting spells of unemployment and layoffs. To this end, he proposed the ‘toll’ assumption: hiring implies a one-time set-up cost, which is divided between employers and employees. Their mutual ‘investment’ in the employment relationship ensues as well as a series of other implications – the emergence of a seniority premium, firms preferring to post no-help-wanted signs rather than engaging in bargain hiring, the need to screen the quality of applicants, and so on. This is a framework in which all employment is frictional. It has no room for the involuntary unemployment notion in the individual disequilibrium sense yet perfectly accommodates involuntary unemployment in the casual sense

Okun and Lucas evolved in two different methodological worlds. Okun was looking for a direct explanation of reality. Such a quest implied the identification of the main elements that are likely to play a causal explanatory role. No important suspect could be left aside. This is the type of approach in which historians engage. But being an economist and not a historian, Okun wanted to go beyond mere description and engage in modeling. Taken in isolation, his analyses of the product market and of the labor market are appealing. They effectively convey the feeling that the main relevant mechanisms have been detected and a better understanding of the working of these markets provided. However, the task of piecing together these partial equilibrium investigations exposed the limits of Okun’s method. He contented himself with constructing a three-equations reduced-form model. It abounded with *ad hoc* assumptions, making it an easy prey for criticism. Against Lucas’s newly defined standards, it was definitely sub-standard.

As seen in the previous chapter, Lucas regarded macroeconomic models as analogous systems, which are bound to be non-realistic. Pushing things to the extreme, it can be stated that they do not pursue an explanatory purpose, at least in the way a historian or a sociologist would understand this term. However, while he was less ambitious as far as the explanatory purpose was concerned, Lucas was more demanding in terms of methodological rigor. To him, verbal modeling as done by Okun’s work could not do. So, while Okun argued that Lucas’s model was unable to provide an adequate *explanation* of the business cycle because of its ill-adapted conceptual apparatus, Lucas could respond that, by grafting the misperception insight onto a simple Walrasian model, he had succeeded in constructing an equilibrium *model* of the business cycle, the starting point of a series of better such models.

Be that as it may, the odds were unevenly distributed between the two competing methods. When *Prices and Quantities* was published, the chances that Okun’s conception of macroeconomics might win the day over the new theoretical style initiated by Lucas were thin, not counting the fact that Okun was no longer there to be the advocate of his views. As Tobin put it, “the intellectual climate of professional macroeconomists was inhospitable to *Prices and Quantities* when it was published” (Tobin 1987: 700–701).

THE BATTLE OVER MARKET CLEARING

Of all the methodological standpoints taken by Lucas, the most shocking to Keynesian macroeconomists was that market clearing was ever present in his approach. To them, this was blatantly false. They argued that such an outcome might well be apposite for a few special markets but could not be generalized. That this hypothesis could be applied to the labor market, thereby removing the possibility of involuntary unemployment, looked baffling to them. The aim of this section is to disentangle the two opposite viewpoints about market clearing. To this end, I base myself on one of the few examples of a more or less direct confrontation between their respective advocates. In his book, *Asset Accumulation and Economic Activity* based on his Yrjö Jahnsson Lectures (Tobin 1980), Tobin devoted a full chapter to criticizing new classical macroeconomics. The views expressed in it were definitely shared by most of his Keynesian colleagues. Invited to write a review of the book for the *Journal of Economic Literature*, Lucas dedicated most of it to reacting to Tobin's criticism (Lucas 1981b).

Tobin's judgment of Lucas's work mixed praise and criticism. Though he appreciated its ingeniousness and elegance, he bemoaned the reorientation of macroeconomics to which it led. In his judgment, Lucas's fault was to have transgressed the division of territory between short-period and long-period analysis, the Keynesian paradigm 'owning' the first and the Walrasian the second. Market clearing, Tobin believed, is a fine assumption for the long period, but a calamitous one for the short period because "obviously, we do not live in an Arrow-Debreu world" (Tobin 1980: 24). Therefore, he wrote, any "literal application of the market-clearing idea" constitutes a "severe draft on credulity" (Tobin 1980: 34). He also praised earlier economists for being more willing than Lucas to admit the limits of Walrasian theory, for example, Schumpeter.¹⁷ In other words, he was on the side of Hicks on the relationship between Keynes and the classics or, still in other words, a partisan of the neoclassical synthesis. In short, Tobin had no qualms about having alternative modeling strategies tailored to specific objects of study. For pragmatic reasons,

¹⁷ "The great economist, political theorist, and social scientist, Joseph Schumpeter, my teacher, liked to say that Léon Walras 'gave economics its *magna carta*.' ... Walras's vision was indeed powerful. In the hand of subsequent theorists it has been refined and made rigorous. ... Schumpeter himself used the Walrasian equilibrium only as a reference point for the destabilizing departures he regarded as the essential story of capitalism. In any case, contemporary classical theorists are bolder than their predecessors in assuming that the economic world can be described in terms of continuous clearing in competitive markets of supplies and demands derived from utility and profit maximization. This surge of confidence is not based, so far as I can see, on new empirical evidence for the assumption. ... Older theorists, even Pigou, were more cautious. While they may have believed that there were strong tendencies toward the Walrasian equilibrium, they did not expect that markets were simultaneously clearing every moment of time. They were willing to acknowledge that the system was generally in disequilibrium, perhaps en route from one Walrasian solution to another" (Tobin 1980: 32-33).

he admitted differences in standards depending on whether the analysis is short or long period, with laxer standards applying to the former.¹⁸

Lucas's reaction to Tobin's criticism was one of frustration. He felt that Tobin believed so adamantly that his own way of posing issues was right that he considered the mere fact of thinking differently a dramatic mistake. Here is a passage from the last draft of his review yet which was absent in the published version:

He [Tobin] consistently assumes the point of view that there is some economic Truth which, without doing anything very fancy econometrically or theoretically, he can simply see, and urges his reader to shed his preconceptions so that he can see this truth too. (Lucas, Various, Box 23, drafts folder)¹⁹

To Lucas, Tobin evaded the debate instead of engaging in an internal criticism of his theory. His reflections, Lucas wrote, are mere 'exercises of reassurance,' a denial of the deep trouble into which Keynesian theory had fallen. According to him, Tobin attributed the demise of the neoclassical synthesis to Lucas's negative influence rather than to any weakness in the scientific underpinnings of the consensus (Lucas 1981b: 558). On reading Tobin's criticism, "the reader is left to wonder how this 'school,' however ingenious its techniques, can have helped to precipitate what some see as a crisis in so practical an area as macroeconomic policy" (Lucas 1981: 559). Lucas's bitterness comes out even more strongly in a passage from his last draft, which he eventually deleted yet which I find most telling:

One [loose end] is 'cleared markets.' Tobin heaps scorn on the idea that any sane person would approach a macroeconomic problem with this particular simplifying assumption in hand. I see Tobin use it in *all* the substantive analysis in the present volume and in all of his most valuable earlier work, and I see my colleagues in every applied field in economics put it to good use on a wide variety of problems, without apologies and without philosophizing. Yet, at the same time I *know* that if a plebiscite were taken among macroeconomists Tobin's view (when he philosophizes, I mean, not when he is actually producing economics) would win over mine hands down. Well, so much the worse for science by plebiscite. I will work my side of the street, and let others work theirs, and if mine be less crowded, perhaps I shouldn't complain. (Lucas, Various, Box 23, Tobin folder)

Tobin accepted market clearing as far as the long period is concerned, but to Lucas, even such an outward compromise was off the mark. As market clearing

¹⁸ "Disequilibrium adjustment and aggregation make for messy specifications, in which *ad hoc* empiricism plays a disturbingly large role. It is a noble enterprise to extend to this range of economic behavior the paradigms of optimization with which we are trained to feel more comfortable. But it is a very difficult enterprise, far from fruition; and the paradigms may not apply in any more than a tautological sense" (Tobin 1981: 391).

¹⁹ My presumption is that Lucas deleted this passage to avoid being rude rather than because it did not convey what he believed.

exclusively relates to a given period of exchange, it makes no sense to associate it with either the short period (a limited number of successive periods of exchange) or the long period (a greater number of such periods).

The difficulty (how many times does this lesson have to be learned?) is with Marshall's terminology: short-run and long-run. This language is intended to make difficult dynamic problems easy; instead, its use makes them impossible. (Lucas. Various. Box 14, Understanding business cycles 1979-1982 folder).

THE BATTLE OVER INVOLUNTARY UNEMPLOYMENT

To many traditional Keynesians, involuntary unemployment, understood as individual disequilibrium, was the most significant case of alleged market non-clearing. To shed light on the dispute between Lucas and traditional Keynesians, I propose to examine how Tobin and Solow, taken as the spokesmen of Keynesian macroeconomists, on the one hand, and Lucas, on the other, would answer two questions. The first is whether the proposition, Involuntary unemployment exists, is valid as a proposition pertaining to reality? Assuming that this question receives a positive answer, a second one arises, Should the notion of involuntary unemployment therefore be integrated into macroeconomic theory?

Beyond doubt, Tobin's and Solow's answer to the first question would be an unqualified 'Yes.' The underlying rationale goes back to the 1930s: considering that the huge number of unemployed people during the Great Depression had chosen the situation they were in stretches credulity. To Keynesian macroeconomists, what was true for the Great Depression remains true for less dramatic situations.

Though he would definitely be less affirmative than Tobin and Solow, my guess is that Lucas's answer to the first question would not be totally negative. He argued that, in all unemployment situations, there are both voluntary and involuntary elements (Lucas [1978] 1981: 242). In as far as the mix between these can vary, the voluntariness-involuntariness divide is a matter of degree rather than a yes or no choice. This divide boils down to a judgment about agents' responsibility. When agents happen to be in some unfortunate state is it through no fault of their own? The answer is almost always mixed, 'yes in one sense' and 'no in another'. Moreover, it can plausibly be stated that the threshold separating voluntary unemployment from involuntary unemployment will vary with economic conditions. During a severe depression, one can expect the average degree of responsibility of the unemployed people to be lower than when prosperity prevails. Thus, I conclude that, with a few caveats, Tobin, Solow and Lucas might agree that some real-world situations qualify as involuntary unemployment.

My second question is whether the real-world existence of the involuntary unemployment proposition warrants it being transposed from the real

world to the fictitious world of economic theory. Keynesians would definitely answer positively yet with little justification, except the authority of reality. To them, the mere fact of raising this question must seem bizarre. Explaining involuntary unemployment was the very motivation that led to the ascent of the new sub-discipline of macroeconomics. Hence, in their eyes, macroeconomics without involuntary unemployment would lose its meaning. As the point is important, I will give no less than three quotations to illustrate their standpoint:

It is as plain as the nose on my face that the labor market and many markets for produced goods do not clear in any meaningful sense. Professors Lucas and Sargent say after all there is no evidence that labor markets do not clear, just the unemployment survey. That seems to me to be evidence. Suppose an unemployed worker says to you 'Yes, I would be glad to take a job like the one I have already proved I can do because I had it six months ago or three or four months ago. And I will be glad to work at exactly the same wage that is being paid to those exactly like myself who used to be working at that job and happen to be lucky enough still to be working at it.' Then I'm inclined to label that a case of excess supply of labor and I'm not inclined to make up an elaborate story of search or misinformation or anything of the sort. (Solow 1978: 208)

It is clear that the labor market does not operate in this way. Wages are not flexible in the short term in the way assumed by this form of the law of supply and demand. They are not completely insensitive to pressure on the labor market, but they adjust much less than would be required for permanent market clearing. (Malinvaud 1984: 18-19)

Whatever its long-run steady-state properties, the economy spends most of its time in disequilibrium adjustment. (Tobin in Kmenta and Ramsey 1981: 391)²⁰

For his part, Lucas was adamant: involuntary unemployment must not enter the theoretical lexicon. This exclusion concerns the narrow understanding of the term, namely individual disequilibrium, a case in which an agent fails to implement his optimizing plan. One reason for Lucas's rejection was that this research trail has led to no progress.²¹ But his main point was that a matter of principle was at stake: methodologically, the equilibrium discipline and involuntary unemployment are incompatible notions. Lucas's choice was straightforward: equilibrium must prevail over disequilibrium, not for some ontological reason but because it is the most constructive way of proceeding.

²⁰ Other examples among many are Tobin (1992: 391), Modigliani (1977a: 199), and Gordon (1980: 56). Moreover, even economists who cannot be suspected of a Keynesian bias implicitly concurred with Tobin and the others. A fine example is Martin Feldstein's comment on a preliminary version of what was to become Kydland and Prescott's seminal "Time to Build and Aggregate Fluctuations" paper (Kydland and Prescott 1982) at the NBER 1978 Conference (Feldstein 1980: 189).

²¹ Involuntary unemployment is "a theoretical construct which Keynes introduced in the hope that it would be helpful in discovering a correct explanation for a genuine phenomenon: large-scale fluctuations in measured total unemployment. Is it the task of modern theoretical economists to 'explain' the theoretical constructs of our predecessors, whether or they have proved fruitful? I hope not, for a surer route to sterility could scarcely be imagined" (Lucas [1978] 1981: 243).

Much can be done on the basis of the equilibrium discipline, he argued, more than what was originally believed, for example, constructing a theory of the business cycle. By contrast, to borrow the expression of Sims, hardly a follower of Lucas, disequilibrium is the wilderness (Sims 1980: 4).²²

With hindsight, the flaw in Keynesians' attitudes was that, somewhere in their reasoning, they made a wrong inference. Take Solow's quotation. His point is that the presence of an excess supply of labor (or market non-clearing) can be inferred from observing that some agents who are willing to work at the given wage are unable to get a job. Let it be admitted that such an observation can validly be made (e.g., through aptly designed interviews of unemployed people). Solow's mistake is to have taken for granted that it is possible to translate it into the supply and demand theoretical language by stating that market non-clearing exists. As already argued, neither Marshallian nor Walrasian theory has any room, not only for involuntary unemployment but also for unemployment *tout court*. The right inference to be made is that, if the observation is right, its theoretical incorporation requires a different theory based on another trade technology. Search models are the natural candidates, but Keynesians hardly cared for them. For example, Solow wrote, "I can't believe an answer is to be found in search theory. I regard search notions as simply empirically discredited. People don't do it. Job search is simply not a major occupation of the unemployed" (Solow 1978: 207).

AN ASSESSMENT

What explains this basic disagreement between economists who all belong to the Pantheon of the discipline? This is the question that I want to address in this final section. My guess is that the reasons for it must be deep-seated, linked to their basic conceptions of good macroeconomics and the modeling standards that they imply. In Chapter 9, I described Lucas's conception of macroeconomics as consisting of the following points: (1) the microfoundations requirement; (2) the simplified general equilibrium analysis; (3) model and theory being the same thing, with theoretical propositions relating only to the fictitious economy; (4) macroeconomics being concerned with unrealistic imaginary constructions; (5) macroeconomics being concerned with policy matters; and (6) macroeconomic models having to be assessed empirically.

²² In Lucas's eyes what often is called "involuntariness" should rather be labeled "frustration." In an interview with Klammer, Lucas calls John Steinbeck, a left-leaning author, to his rescue to make the point: "Did you ever look at Steinbeck's book *The Grapes of Wrath*? It's a kind of protest pamphlet from the '30s about migrant farmers in California. There's one passage in there that is a better anecdote than I could have written for the kind of models I like. It illustrates the auction characteristic of the labor market for migrant farm workers. He writes about a hundred guys who show up at a farm where there are only ten jobs available. The farmer will let the wage fall until ten people are willing to work for that wage and ninety people say 'the hell with it,' and just go on down the road" (Klammer, 1984: 46).

Traditional Keynesians were totally opposed to items (3) and (4), the latter being just a corollary of (3). To them, a theory and a model should not be confused. Theory is more important than models, the latter serving the ancillary purpose of verifying the consistency of theory. Theory is thus a set of propositions about reality rather than about a fictitious construction. This is the view underpinning Lipsey's statement that I used as the epigraph for this chapter.²³

A second remark is that, except for items (3) and (4), Keynesians would accept all the other ones, most of which are associated with Walrasian theory, on the condition that their purview be limited to the "long period" so that Keynesians may still own the "short period." What they refuse, in other words, is that Walrasian principles become hegemonic, Lucas's very aim. Thus, at bottom, what is looming in the disputation is the attitude taken about the neoclassical synthesis. Tobin and the others support it while Lucas rejects it. This, in my eyes, is the irreducible difference between them.

As far as consistency is concerned, Lucas had the upper hand (which Lipsey admits). The criticisms that Lucas addressed to Keynesians were internal ones and most of them were on point. By contrast, Tobin's criticisms were external; they amounted to stating that Lucas's views were to be disqualified because they were opposed to his own, which he found better. Moreover, Lucas was able to demonstrate the possibility of constructing macroeconomic models based exclusively on individual equilibrium, while Keynesian theory was beleaguered with the non-solved conceptual issues which I discussed in Chapter 8 and which Lucas was prompt to bring to light. Because of their excess pragmatism, Keynesians had little ammunition against Lucas. Finally, those on Lucas's side could always bait economists such as Tobin, Solow or Modigliani, who made so distinct contributions to neoclassical theory, telling them that one should either be fully neoclassical or fully non-neoclassical rather half-neoclassical.

However, presenting the confrontation between Lucas and Tobin as a heavy-weight boxing combat conveys too static a picture. Their opposition should be interpreted in a more dynamic way by viewing it as the meeting of the tail end of one epoch and the beginning of another one. The two paradigms cannot be compared statically because they did not coexist in time. The notion

²³ An example may be useful to flesh out this basic divide. Recently, I attended a seminar given by an eminent DSGE economist. As the audience was comprised of a good proportion of economists working in other fields, the speaker, when presenting the assumptions of his model, time and again commented them by saying "Do I believe in this assumption? No I don't." But this did not prevent him continuing with his reasoning and arguing that his model was a useful contribution to macroeconomics. He might have said (but did not) "There is no involuntary unemployment in my model. Do I believe there is none in reality? No!" A traditional Keynesian would have protested, saying "This is a serious problem; one should not build models on assumptions one does not believe in." Had the speaker retorted that he did not know how to integrate it in his model and whether it was useful, the Keynesian economist could then have responded with Lipsey's words.

of theoretical progress should not be accepted in too naïve a way, but it cannot be rejected either. Progress takes the form of adopting new tools which allow to do better what was poorly done before. Therefore, in terms of conceptual and technical endowment, by definition Lucas and his fellows were dealt a richer hand than the Keynesians. Moreover, reading Lucas's correspondence or Sargent's 1996 article reveals their admiration for Tobin's work – as well as for Modigliani and Solow, "our heroes in those days [the early 1970s]" in Sargent's words (Sargent 1996: 10). So, behind the controversy I have studied, we have the tumultuous passing of the baton from one generation to the next.

In his paper, Sargent expressed his regret that his heroes "missed the boat by resisting the intrusion of rational expectations into macroeconomics, instead of commandeering it" (1996: 10). What may explain this? An obvious answer is that people of a certain age are less prone to change their long-held view. However, there is certainly more to it than just that. Keynesian economists' strong conviction that a pluralistic conception of macroeconomics is superior to a methodologically unified (and, in their eyes, hegemonic) one is certainly an important factor. But, in my opinion, there was also an ideological dimension.²⁴ Keynesians associated the rational expectations revolution with conservatism. They viewed it as going against their ideology and hence they wanted to fight it. There is no doubt that the policy conclusions of Lucas's model as well as those of subsequent RBC models were conservative. The real point, however, is to know whether these policy conclusions were so deeply built-in that their removal was highly improbable. If that was the case, traditional Keynesians were right to resist. If it was not, another strategy was possible, following Friedman's footsteps in his Presidential Address, namely to try to modify the prevailing theory in a way that at one and the same time leads to its enrichment and to a reversal of established policy conclusions. Today after the ascent of second generation new Keynesian models, we may realize with the advantage of hindsight that such a subversion was conceivable.

²⁴ As argued in Chapter 4, I am of the opinion that having an ideological motivation in theory should not be considered reprehensible.

Reacting to Lucas: First-Generation New Keynesians

In 1991, the MIT Press published a two-volume book edited by Gregory Mankiw and David Romer entitled *New Keynesian Economics* (Mankiw and Romer 1991).¹ It gathered articles that had in common to react to Lucas's criticisms of Keynesian macroeconomics. Their authors admitted that some of these criticisms were valid and could not be easily dismissed. In particular, they accepted the microfoundations requirement, that is, the need to depict agents as behaving in an optimizing way. However, this acceptance did not make them Lucasian. On the contrary, their aim was to rehabilitate what they regarded as the basic Keynesian ideas, involuntary unemployment, sluggishness and money non-neutrality.

There was no single new Keynesian research program. This becomes clear when looking at the articles collected by Mankiw and Romer; they follow very different lines. In this chapter, I study a sub-set that has the following traits in common: (a) they are partial equilibrium models, (b) they are imperfect competition models with price-making agents, (c) they assume price or wage rigidity, in the latter case either nominal or real, and (d) the economists who built them support the neoclassical synthesis vision. It will be seen that, even within these boundaries, significant differences subsist. The main models that fit these four criteria are, in order of publication of their inaugural papers, implicit contract models (Baily 1974, Gordon 1974, Azariadis 1975), staggered wage-setting models (Fischer 1977, Phelps and Taylor 1977, Taylor 1979), efficiency wage models (Salop 1979, Weiss 1980, Shapiro and Stiglitz 1984), and menu cost and near-rationality models (Mankiw 1985, Akerlof and Yellen 1985a, 1985b). I will only comment on one model per type. An additional remark is

¹ The first economist who introduced the 'new Keynesian' term was Michael Parkin in the 1984 edition of his *Macroeconomics* textbook (Parkin 1984). Other early uses are Phelps (1985) and Colander and Koford (1985). Lawrence Ball, Mankiw and Romer took it up more systematically (Ball, Mankiw and Romer 1988).

that the new Keynesian label cannot be regarded as generic. For reasons that will become clear later, I refer to the economists studied in the present and in the subsequent chapter as ‘first-generation new Keynesian economists.’²

In the last section of this chapter, I will examine a research track that is sometimes also put under the ‘new Keynesian’ label, although it significantly differs from the tracks mentioned earlier. What it really tries to do is to modernize the IS-LM model apparatus by providing it with an imperfectly competitive labor market. Wendy Carlin and David Soskice’s book, *Macroeconomics and the Wage Bargain: A Modern Approach to Employment, Inflation and the Exchange Rate* (1990), a textbook that was widely used in Europe, is a good example of this approach.³ I will show that their analysis has an intriguing twist: it starts from a quasi-Marxist premise and ends up endorsing Friedman’s policy conclusions!

MAIN FEATURES

Mankiw and Romer’s introduction to their 1991 volume and Ball and Romer’s paper, “A Sticky-Price Manifesto” (Ball and Mankiw 1994) presented at the 1994 Carnegie-Rochester Conference, acted as manifestos for the new Keynesian approach. In their Introduction, Mankiw and Romer declared that new Keynesian models share two propositions. The first is that money is not neutral, meaning that fluctuations in nominal variables exert a real effect, due to the existence of rigidities. The second is that market imperfections, imperfect competition and price and/or wage stickiness, must play a central role in explaining business fluctuations (Mankiw and Romer 1991: 2). In their paper, Ball and Mankiw presented new Keynesian economics as the faithful heir to the traditional conception of monetary theory. They argued that it can be traced back to David Hume according to whom money is neutral in the long period and non-neutral in the short period. Taking RBC modeling as their target, they depicted RBC economists as heretics firing at this long-standing conception, arguing that the objective of new Keynesians was to restore the traditional viewpoint: “A macroeconomist faces no greater decision than whether to be a traditionalist or a heretic. This paper explains why we chose to be traditionalists” (Ball and Mankiw 1994: 1).

The gist of new Keynesian modeling can be summarized in four points. The first is that new Keynesians were ready to admit that earlier macroeconomics models had the twin defect of lacking explicit microfoundations and of failing to justify the nominal rigidity assumption. However, unlike Lucas, they did not consider these flaws as irreparable and set the aim of

² Whenever the context makes it clear that I am discussing first-generation new Keynesianism, I will omit the ‘first-generation’ qualifier.

³ A new, richer edition of the book was published in 2006. I will not refer to it because my purpose is to study reactions to the first installment of the DSGE program.

developing sticky-price models based on robust microeconomic foundations (Ball and Mankiw 1994: 15–16). Second, Mankiw and Romer underlined that they did not want new Keynesian models to be associated with the Keynesian side in the monetarist debate. This is a point on which Mankiw time and again insisted:

There are two schools of thought: new classical and new Keynesian economics. Monetarists now are members of the new Keynesian family, which shows how much the debate has changed. The distance between the new classical and new Keynesian schools is so large that it makes the monetarist-Keynesian debate of the 1960s look like sibling rivalry. (Mankiw 1992: 22)⁴

Third, in their criticism, new Keynesians did not really separate the new classical and the RBC phases of the DSGE program (most of the new Keynesian models were devised before RBC modeling took off). Nonetheless, in their introduction, Mankiw and Romer designated RBC modeling as their main adversary. They were right both in terms of substance, because of its non-monetary feature, and in terms of tactic, since at the time when they wrote their introduction, RBC modeling had become the main game in town.

A last general feature of first generation new Keynesian macroeconomists is their stern support of the neoclassical synthesis vision. In Mankiw's words:

New Keynesians accept the view of the world summarized by the neoclassical synthesis: the economy can deviate in the short run from its equilibrium level, and monetary and fiscal policy have important influences on real economic activity. New Keynesians are saying that the neoclassical synthesis is not as flawed as Lucas and others have argued. (Snowdon and Vane 2005: 438; the interview took place in 1993)

Blanchard and Fischer's famous *Lectures on Macroeconomics* book (1989) can be regarded as a testimony to the wide adoption of the neoclassical synthesis viewpoint by new Keynesians. This book is a balanced account of macroeconomics, putting the overlapping generations models, multiple equilibria, bubbles models, and partial equilibrium models of involuntary unemployment, on the same level as equilibrium business cycle models. This way of proceeding amounts to admitting the coexistence of several disparate models, each deemed to have its own usefulness. The book was published in 1989. A few years later, the picture had radically changed with RBC modeling occupying most of the research space, the sign of the defeat of the neoclassical synthesis viewpoint.⁵

⁴ See also Ball and Mankiw (1994: 4), and Mankiw's interview by Snowdon and Vane (2005: 437–438).

⁵ Romer's *Advanced Macroeconomics* textbook perpetuates the Blanchard-Fischer tradition (Romer 2011).

IMPLICIT CONTRACT MODELS: THE AZARIADIS MODEL

My discussion of implicit contract models is limited to Azariadis's model (1975), a one-period, partial equilibrium model of the relationship between a firm and its labor pool. Uncertainty exists as to the level of demand faced by the firm. To simplify, it can be assumed that two states are possible, a favorable and an unfavorable one, corresponding to high and low demand (and hence to high or low prices). Given that technology remains the same, two levels of the marginal value product of labor are possible. It is assumed that the firm is risk neutral – its utility function is linear in profits – while workers are risk-averse. As a result, firms and workers have a mutual interest in fixing a wage and employment contract contingent on the state of the world.

A two-tier process is at work. Its first stage concerns the distribution of workers across firms. It occurs through an auction market bearing on contracts and delivering a given utility level, \bar{v} , to workers. The value of the contract plays the same role as the price in a competitive market. In equilibrium it must be the same for all firms. It is assumed that every worker gets an employment contract. Thus, nobody is formally jobless. The second process, the subject matter of the model, bears on the contents of the contract between a firm and its labor pool. Here, \bar{v} acts as a constraint on the firm's objective function. At stake is whether the wage and the employment level – that is, the rate of utilization of the members of the labor pool – will be identical or vary with the state of the world. That is, is it optimal in all circumstances to have a wage equal to the marginal productivity of labor? Is the full employment contract optimal?⁶

Azariadis's model is based on a series of precise assumptions that I will not detail, except for two crucial ones. The first is time indivisibility: it is assumed that workers are endowed with one unit of time that must be used fully either as labor or as leisure. Second, the class of admissible contracts is restricted in that firms can insure against volatility in wages, but not against volatility in employment by providing severance payments to laid-off workers.

A twofold result is obtained. First, the firm and its workers have a mutual interest in having the same wage in different circumstances. This is Azariadis's 'rigidity' result (rigidity here meaning selfsameness across the states of the world). It is the straightforward consequence of firms and workers having different attitudes towards risk. As a result, the wage will be higher than the marginal value product in the unfavorable state, as workers receive an insurance benefit, and lower in the favorable state, since an insurance premium

⁶ Azariadis defined full employment as a state where the number of workers that the firm will put at work is equal to the size of the firm's workforce.

is now deducted from the marginal value product. Thus, the employment contract is coupled with an insurance contract. Azariadis's second result is that full employment is not necessarily optimal. Layoffs can be in the mutual interest of the firm and the workers in the unfavorable state of the world. They are the more likely (a) the lower the marginal productivity in the unfavorable state, (b) the smaller workers' risk premium, and (c) the higher the opportunity cost of leisure. Layoffs, which go along with a zero wage, are decided randomly. Those who suffer a bad draw are worse off than those who are put to work. Therefore, according to Azariadis, they must be considered involuntarily unemployed: "The employed workers . . . are to be envied by their laid-off colleagues – a situation that many economists would call 'involuntary unemployment'" (Azariadis 1987: 734).

For all its cleverness, Azariadis's model was short-lived, at least as far as its place in macroeconomics was concerned. Its main weakness is to be *ad hoc*. As soon as the assumption that firms can pay indemnities to the laid-off workers is adopted, they are no longer worse off. A mixed broader judgment eventually ensued. It was admitted that his model paved the way for a new field of research, contract theory, yet it was considered of limited interest as far as its initial purpose of justifying Keynesian claims was concerned.

EFFICIENCY WAGE MODELING: SHAPIRO AND STIGLITZ'S SHIRKING MODEL

Efficiency wage theory refers to different types of models that have flourished in the 1980s, and which have focused on issues of information and incentives. According to Akerlof and Yellen, "they have in common that in equilibrium an individual firm's production costs are reduced if it pays a wage in excess of market clearing, and, thus, there is equilibrium involuntary unemployment" (Akerlof and Yellen 1986: 1). Different reasons, such as asymmetric information, fairness or insider power, may explain such behavior. The main aim of efficiency wage models is to demonstrate the possibility of involuntary unemployment. With respect to traditional Keynesian macroeconomics, this would be a case of "nothing new under the sun," except for two differences. First, these models purport to achieve this ambition while abiding by the equilibrium discipline. By contrast, in traditional models it was taken for granted that involuntary unemployment was a breach of the equilibrium discipline. Second, while the old Keynesian models focused on nominal rigidity, efficiency wage models zeroed in on real rigidity. I will focus on one of them, the popular shirking model developed by Shapiro and Stiglitz (1984).

Their model is based on the premise that work is disagreeable. When not monitored, workers will shirk rather than exert effort. Firms' ability to monitor whether workers actually work is imperfect. When caught shirking workers are fired. If full employment prevails, there is no cost of shirking for the worker

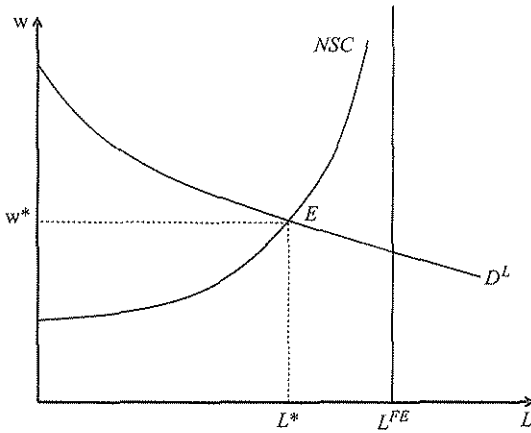


FIGURE 13.1 Involuntary unemployment in the shirking model

(although there are clearly costs for the firm in recruitment and loss of production). To eliminate shirking, firms will raise wages. This will be true for all firms, so the average wage will rise above the market-clearing wage. As a result, workers will be deterred from shirking lest they lose their job. In other words, firms set their wage in order to minimize labor costs per efficiency unit – the efficiency wage – and not their cost per worker. The equilibrium wage must be equal to or higher than a given value determined by a series of parameters. Shapiro and Stiglitz called this the non-shirking condition.⁷ As Figure 13.1 illustrates, the labor market equilibrium is determined by the intersection of aggregate labor demand with the aggregate non-shirking condition rather than with the aggregate labor supply.

D^L is the demand for labor, NSC the non-shirking schedule (NSC), and the vertical line is the supply of labor; w^* the efficiency wage, L^* the level of employment associated with w^* , and L^{FE} the full employment level.

Full employment is inconsistent with non-shirking, because in this situation there would be no incentive for workers to work. Involuntary unemployment is equal to the horizontal distance between E and the vertical labor supply. “Those without jobs would be happy to work at w^* or lower, but cannot make a credible promise not to shirk at such wages” (Shapiro and Stiglitz [1984] 1991: 131).⁸

⁷ “The critical wage for non-shirking is greater: (a) the smaller the detection probability, (b) the larger the effort, (c) the higher the quit rate, (d) the higher the interest rate, (e) the higher the unemployment benefit, and (f) the higher the flow out of unemployment” (Shapiro and Stiglitz 1984: 438).

⁸ As Katz commented, “Equilibrium unemployment is involuntary in this model since identical workers are treated differently and since the unemployed strictly prefer to be employed” (Katz 1986: 242).

The main criticism against the shirking model is that alternative mutually advantageous contracts, involving no unemployment – in particular, posting bonds (Carmichael 1985, 1989) – can be devised.⁹ Other economists have criticized it for its relevance for modern industrial economies; it only applies, the criticism runs, to menial jobs and hence to the secondary unskilled sector. My own take is to assess its contribution to macroeconomics.

Shapiro and Stiglitz's purpose was to salvage the involuntary unemployment notion after Lucas's fierce attack against it. Their strategy was to beat Lucas on his home turf by demonstrating the possibility of involuntary unemployment while adopting the equilibrium discipline. Can such a project succeed? It all depends on how involuntary unemployment is defined. If it means individual disequilibrium – the Keynes-Patinkin definition – it is bound to fail as it amounts to trying to integrate the idea of a non-chosen outcome in a language premised on the idea that agents behave optimally. However, the task is amenable when the casual definition of involuntary unemployment, that is, the Phelps definition, is adopted. This is the line taken by efficiency wage models and also by implicit contract models. In these models, the unemployed behave in an optimizing way, yet they have good reasons for being jealous of the employed and hence for feeling frustrated. /

A similar ambiguity arises regarding the market-clearing notion as advocates of the shirking model claim that it also successfully demonstrate market non-clearing as supply exceeds demand. Thereby, it is argued that the traditional Keynesian claim has been vindicated and Lucas proven wrong. As explained when discussing non-Walrasian equilibrium modeling, this conclusion rests on an inapposite definition of market clearing, the right definition being generalized optimizing behavior. Efficiency wage theorists failed to address this definitional choice, they just took it for granted that market clearing non-equivocally meant that supply and demand balanced.

It is true that the efficiency model succeeds in demonstrating casual involuntary unemployment based on sound microeconomic principles. This is an intellectual victory, and also a symbolic one. At last, Keynes's project has been implemented, though with a looser definition of involuntary unemployment. However, little more than that has been achieved. It is hard to argue that the shirking model can explain the bulk of the unemployment phenomenon. Moreover, I find it surprising that it is regarded as an important contribution to macroeconomics as witnessed by its frequent presence in macroeconomics textbooks. Macroeconomics is concerned with the study of the economy as a

⁹ McLeod and Malcomson (1998) have shown that, under certain conditions, pay based on subjectively assessed performance is more efficient than efficiency wages.

whole, while these models evolved in a partial equilibrium context. Nor can I understand that such models are regarded as contributions to Keynesian macroeconomics. For Keynesian theory, unemployment is a problem that needs to be remedied. In efficiency wage models, the problem is shirking and imperfect information, and unemployment is the solution to these! As noted by Kolm, the situation depicted in efficiency wage models is Pareto optimal; for sure, in these models there is no reason for the government to engage in demand activation (Kolm 1990: 230).¹⁰

STAGGERED WAGE SETTING MODELS: FISCHER'S MODEL

As noted in Chapter 9, using a model similar to Lucas's, Sargent, and Wallace (1979) were able to demonstrate that rational expectations result in a vertical Phillips curve in both the short and the long run. They dubbed this result the 'policy ineffectiveness proposition.' Among the different ways explored to respond to it, staggered wage-setting models proved to be particularly effective (Fischer 1977, Phelps and Taylor 1977, Taylor 1979). These models aimed at demonstrating that rational expectations and Keynesian theory, understood as the defense of money non-neutrality, were compatible. That is, the flexibility assumption rather than rational expectations *per se* is the key factor behind the policy ineffectiveness proposition. Dropping it makes the invalidation of the policy ineffectiveness proposition possible. The positive contribution of these models was thus to have rescued Modigliani's claim that a crucial link exists between nominal wage rigidity and money non-neutrality, while also providing what was lacking in earlier Keynesian macroeconomics, namely a justification to rigidity.

I will focus on Fischer's model. Fischer pursued a twofold aim: demonstrating that the rational expectations assumption does not prevent output from varying with monetary policy once wage rigidity is assumed, and furthermore showing that, this assumption made, a policy of output stabilization is advisable. Fischer's concern was nominal wage rigidity. The difference with old Keynesian models is that in his model rigidity is no longer associated with a wage floor or viewed as the embodiment of sluggishness. Rather, it originates from the assumption that wages are fixed through long-term nominal contracts.

Fischer's reasoning proceeded in two steps. First, he assumed that the nominal wage is fixed for a single period. Hicks's week device, in which trade is supposed to take place every week on Monday, can be invoked here. The story is that a new labor contract is decided upon on the eve of every Monday. In this case, the policy ineffectiveness proposition holds. In the second step, Fischer assumed that labor contracts cover several weeks – for the sake of

¹⁰ Phelps (1985: 421) made the same point.

simplicity, he supposed just two. When this assumption, which is implicitly declared as more realistic than the earlier one, is made, the policy ineffectiveness proposition is invalidated. I start with describing the functioning of the economy under the one-period contract assumption.¹¹

Wage setting

Wages are set in advance of a new trading period (on a Friday for instance, goods trading starting on the Monday). Rational expectations imply that agents are as knowledgeable about the equilibrium allocation of the economy as the model-builder. The goal of wage setting, Fischer assumed, is to keep the real wage constant. In the absence of disturbances, the nominal wage would be set at a magnitude ensuring the attainment of the real wage rate, allowing the realization of the natural rate of employment. Any departure from the natural rate takes the form of (suboptimal) under- or overemployment. Once disturbances are considered, the nominal wage is fixed in accordance with the expected price level:

$$w_t^{t-1} = E_{t-1}p_t \tag{13.1}$$

where w_t^{t-1} is the logarithm of the wage set for period t set at $t-1$, $E_{t-1}p_t$ is the wage-earner's expectation about p_t made at $t-1$, and p_t is the price of output at t .

The supply function

Output supply is a decreasing function of the real wage:

$$y_t^S = (p_t - w_t^{t-1}) + \mu_t \tag{13.2}$$

where y_t^S is the logarithm of the output level, and μ_t a stochastic disturbance.

Because of (13.1), equation (13.2) can be transformed into:

$$y_t^S = (p_t - E_{t-1}p_t) + \mu_t \tag{13.3}$$

This equation is a simplified Lucas supply function.

The demand function

$$y_t^D = m_t - p_t - v_t \tag{13.4}$$

where y_t^D designates the logarithm of demand, m_t is the logarithm of the money stock, p_t the logarithm of price, and v_t a disturbance term. This is a standard aggregate demand curve. Equation (13.4) indicates that the monetary authority reacts to shocks by 'leaning against the wind.'

¹¹ In what follows, I draw from Heijdra and van der Ploeg (2002: 71-76).

Market clearing

$$y_t = y_t^S = y_t^D$$

Disturbances

Each of the two disturbance terms are supposed to follow a first-order autoregressive scheme:

$$\mu_t = \rho_1 \mu_{t-1} + \varepsilon_t \quad |\varepsilon_t| < 1, \quad (13.5)$$

$$v_t = \rho_2 v_{t-1} + \eta_t, \quad |\eta_t| < 1 \quad (13.6)$$

where ε_t and η_t are uncorrelated white noises. The monetary rule is based on the disturbances that occurred up to and including $t-1$:

$$m_t = \sum_{i=1}^{\infty} a_i \mu_{t-i} + \sum_{i=1}^{\infty} b_i v_{t-i} \quad (13.7)$$

where a and b are coefficients expressing the monetary authority's reactions to disturbances.

Agents have perfect foresight about the money supply. However, because of the presence of disturbances, neither the monetary authority nor the agents can predict the price level. Through a few steps that I skip, Fischer demonstrated that the gap between the effective and the expected price level is exclusively determined by ε_t and η_t :

$$p_t - E_{t-1} p_t = \frac{1}{2} (\varepsilon_t + \eta_t) \quad (13.8)$$

By substituting equation (13.8) into equation (13.2), one obtains the equilibrium output path:

$$y_t = \frac{1}{2} (\varepsilon_t + \eta_t) + \mu_t \quad (13.9)$$

Equation (13.9) expresses that monetary creation is irrelevant for the behavior of output.

However, a different conclusion is obtained when two-period non-indexed contracts are brought into the picture. Now, at t , half of the firms operate under a contract set at $t-1$, the other half operating under one set at $t-2$.

$$w_t^{t-1} = E_{t-1} p_t \quad w_t^{t-2} = E_{t-2} p_t \quad (13.10)$$

Aggregate supply is now:

$$y_t^S = \frac{1}{2} (p_t - E_{t-1} p_t) + \frac{1}{2} (p_t - E_{t-2} p_t) + \mu_t \quad (13.11)$$

Again, skipping the steps in Fischer's reasoning, I go straight to his conclusion, which is supported by two equations about the evolution of money and output corresponding to (13.8) and (13.9):

$$p_t - E_{t-2} p_t = a_t \varepsilon_{t-1} + b_t \eta_{t-1} \tag{13.12}$$

$$y_t = \frac{1}{2}(\varepsilon_t - \eta_t) + \frac{1}{3}[\varepsilon_{t-1}(a_t + 2\rho_1) + \eta_{t-1}(b_t - \rho_2)] + \rho_1^2 \mu_{t-2} \tag{13.13}$$

Equations (13.12) and (13.13) differ from equations (13.8) and (13.9) because they comprise the coefficients of the money creation rule, a_t and b_t . The novelty is that at $t-2$, wage-setters need to set the nominal wage for periods $t-1$ and t . Although they are able to correctly forecast the money supply at $t-1$, they are unable to do so for the money supply at t since they are unable to predict any 'innovation' – the “measures taken by the monetary authority to react to new information about recent economic disturbances” (Fischer [1977] 1991: 223) – which may occur at $t-1$. As a result, the real wage at t , as bargained at $t-2$, will be different from the real wage that would have occurred with period-by-period contracts, in as far at least as nothing is done to prevent this from happening.

Can and should something be done about this possible outcome? Fischer's answer is 'Yes.' Workers have fixed their wage on stale information. The ensuing fluctuations in output mean a welfare loss. The monetary authority can and should react to these unwanted fluctuations by choosing appropriate values for the a and b coefficients, neutralizing the effects of the shock that occurred at period $t-1$.

While Fischer's article was a one-off piece, Taylor took up the task of developing the staggered price and wage-setting idea in a more systematic way (Taylor 1979, 1980).¹² The Fischer and the Taylor models have in common the aim of demonstrating that the rational expectations hypothesis is compatible with the view that monetary changes can have real effects and, moreover, that monetary policy can be efficient in increasing social welfare.¹³ This was the achievement of these models: they salvaged the Keynesian vision, delivering it from the Sargent-Wallace indictment, thereby countering the view that rational expectations is necessarily anti-Keynesian. Another important feature of these models is that by adopting a dynamic perspective they fell into line with the turn that macroeconomics was taking at the time (unlike most other new Keynesian models, which were static). It must also be noticed that

¹² There are differences between the Fischer and the Taylor model. For example the latter changes assumptions leading to more protracted effects of monetary shocks on the price level and output. To differentiate his model from Fischer's, Taylor calls Fischer's "an expected-market clearing" model, keeping the staggered contract model for his own line of modeling (Taylor 1999: 1027). Taylor (1999) presents a general assessment of the developments that came after the inaugural papers.

¹³ These models are both 'time-dependent' (i.e., the adjustment depends on time). Others, in particular the Caplin-Spulber model (1987), are 'state-dependent.'

staggered contract models move away from the involuntary unemployment research line by focusing on underemployment and policy effectiveness. They belong to the Modigliani tradition in which monetary expansion is the policy which should be undertaken in case of underemployment, the latter being due to rigidities. Finally, these models improve on the old versions of Modigliani's model by providing an equilibrium foundation to the existence of rigidity.

As for the shortcomings of the staggered contract model, its main recognized drawback relates to persistence. Another weak point is that it relies on *ad hoc* assumptions, a highly elastic labor supply, weak money shocks, and high menu costs. For my part, I want to bring out a less noticed ambiguity in Fischer's model that I find typical of the difficulties encountered when trying to introduce more realistic assumptions into a highly abstract setup. Fischer's aim was to invalidate Sargent and Wallace's policy ineffectiveness proposition by resorting to an argument of realism, namely that in reality firms and workers have long-term contracts for the reasons explained by Okun. To make his point, Fischer wanted his model to be as close as possible to Sargent and Wallace's, which itself is based on Lucas's model. For the sake of tractability, these models were concerned with self-employed agents. The same is true for Fischer's model. There is however a difference. In Lucas's model as well as in Sargent and Wallace's, there is a congruency between the model and the story told. This is no longer the case for Fischer's model. His narrative is about workers setting a wage, and we, the readers, think of a contract between a firm and a worker. However, in his model agents are self-employed. So, the two partners in the contract are not a firm and a worker but the worker and herself. That is, the wage is a shadow wage that an agent acting in her quality of firm pays to herself in her quality of wage earner. In this context, one may wonder what the rationale is for the contract idea.

MENU-COST AND NEAR-RATIONALITY MODELS

Menu-cost models exist in two forms, menu-cost proper (Mankiw 1985) and models of near-rational behavior (Akerlof and Yellen 1985a, 1985b).¹⁴ Although they are close as far as substance is concerned, they differ in accent and generality. As to motivation, Ball and Mankiw started their "Sticky Price Manifesto" stating: "There are two kinds of macroeconomists. One kind believes that price stickiness plays a central role in short-term economic fluctuations. The other kind doesn't" (Ball and Mankiw 1994: 1) – and they make it clear that they belong to the first type.

The story underpinning menu-cost models is as follows. Assume that a change in money supply occurs. According to the money neutrality standpoint,

¹⁴ Other remarkable papers about menu costs are Ball, Mankiw and Romer (1988), and Ball and Romer (1990).

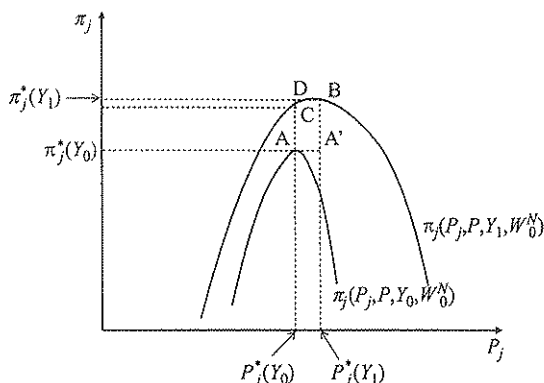


FIGURE 13.2 The profit function of a monopolistic firm

assuming flexible prices and wages, its effects are purely nominal. What if changing prices is costly however? Several reasons may explain that this is the case. Beyond the narrow cost of changing the price list, other explanatory factors are the time and attention needed to judge whether changing prices is the right thing to do and whether it risks upsetting customers. In this context, firms need to weigh the loss incurred from not changing their prices against the menu cost. Whenever the menu cost exceeds this loss, not reacting to a negative demand shock is optimizing behavior. This may happen in an imperfectly competitive framework, as the slope of the profit function of the firm is close to zero in the neighborhood of its maximum, as explained by the envelop theorem. The upshot is that money neutrality vanishes.

Akerlof and Yellen made a similar point except for the fact that they expressed it in terms of inertial behavior or near-rationality. This type of behavior may well be suboptimal; however the individual loss it imposes on the concerned is very small.¹⁵ The snag is that what is true for the individual agent is not for the system:

The potential losses due to non-maximization are second order small (varying with the square of the shift parameter). Nonetheless we show that such a behavior does commonly lead to changes in equilibrium in familiar models (relative to the equilibrium with full maximization) which are first-order, that is, vary approximately proportionately with the shift parameter. (Akerlof and Yellen 1985b: 708)

Akerlof and Yellen's reasoning can be illustrated with the help of Figure 13.2, drawn from Heijdra and van der Ploeg (2002: 388), themselves inspired by Akerlof and Yellen (1985b: 710). The graph describes a given monopolistic

¹⁵ In the following paragraphs, I again draw on Heijdra and van der Ploeg (2002: 384, seq.).

firm, firm j . Its profit (π_j) is a function of the price of the good it produces (P_j), the aggregate nominal income (Y), the price index of the composite good (P), and the nominal wage (W^N):

$$\pi_j = f(P_j, Y, P, W^N).$$

The supply of labor is assumed to be highly elastic. Let it be assumed that the government engages in demand activation through money creation. As a result, nominal income increases from Y_0 to Y_1 . Figure 13.2 illustrates this. The maximized values of the objective function can be expressed as $\pi_j^*(Y_0)$ and $\pi_j^*(Y_1)$. The purpose of the example is to show that near-rational behavior may lead the firm to keep its price at $P_j^*(Y_0)$ instead of increasing it to $P_j^*(Y_1)$ as in the standard case. Were the price adjusted, the profit would increase by $A'B$. Leaving the price unchanged, it increases by AC . The profit loss associated with not adjusting the price is CD , a relatively small quantity. If the cost of changing the price is higher than CD , the price should not be changed.

Explaining price rigidity *per se* was not the main driving force of menu-cost and near-rationality economists. Rather, like staggered contract models, what they were striving at was providing a counter-argument to the neutrality of money claim. Let us consider the case of a positive nominal shock. According to the neutrality claim, it leads to an increase in nominal prices, the output remaining unchanged. Here, a different result emerges. The demand for all the firms in the industry will increase, which triggers an increase in their demand for labor. Assuming a highly elastic supply of labor, more hours will be worked at the unchanged nominal wage rate W^N . Assume additionally a wider framework characterized by nominal price rigidity, real wage rigidity (associated with the assumption of a highly elastic supply of labor), and monopolistic competition, which implies that the output is initially below its socially optimal level. In such a context, any demand activation, either fiscal or monetary, exerts first-order welfare effects (Heijdra and van der Ploeg [2002: 390]). Under these conditions, again, the claim that monetary policy is inefficient is reversed, and the Keynesian vision is reestablished.

AN ASSESSMENT

First-generation new Keynesian economists demonstrated that Lucas and his followers had no monopoly over theoretical inventiveness. Nonetheless, they were unable to stop the DSGE bandwagon. Several reasons explain this. The stance they took was defensive rather than offensive; fighting for the defense of a tradition, to use Ball and Mankiw's vocabulary, often verges on rearguard battles, especially if the heretics carry with them a series of appealing new ideas and tools. In particular, except for staggered contracts, new Keynesian models were static. Therefore, they paled in comparison with the dynamic program set up by Lucas.

Moreover, new Keynesian models were caught unprepared by the ascent of RBC modeling. With new classical macroeconomics *à la* Lucas, they had two things in common. First, they shared the same practice of reasoning with abstract qualitative models. Second, they had a bone of contention to fight about, the issue of the lasting real effects of monetary changes. The ascent of RBC modeling changed the scene. Money became conspicuous by its absence. More importantly even, Kydland and Prescott re-installed macroeconomics in its status of applied field, the validity of theoretical models being gauged by their empirical replication ability. In this respect, first-generation new Keynesians were wrong footed since their models were mainly qualitative.¹⁶ A decade later, second-generation new Keynesians proved able to take it account. In exchange for the abandonment of a few earlier positions, they were to reorient the course of DSGE modeling in a direction more in tune with Keynesian themes.

CARLIN AND SOSKICE'S WAGE BARGAINING-AUGMENTED IS-LM MODEL

The economics of Carlin and Soskice is of the realistic, pragmatic, Marshallian kind. Their decision to work with the adaptive expectations assumption unlike the other economists studied in this chapter testifies to this attitude. The adaptive expectations assumption has the drawback that workers are systematically wrong in their price expectations, while the rational expectations assumption has the opposite flaw of giving agents too much knowledge, thus leaving no room for mistakes and hence excluding inertia from the equilibration process. Facing this dilemma, Carlin and Soskice standpoint was that it all depends on the problem studied. Their take on inflation and unemployment, the main issues they are interested in, was that the adaptive expectations assumption is the lesser of two evils (Carlin and Soskice 1990: 105).

The wage bargaining model

Carlin and Soskice's attempted to revitalize the IS-LM model by adhering to a new way of coming to grips with the working of the labor market. Although standard macroeconomists considered it similar to all other markets except for wage rigidity, they regarded it as the venue of a distributional conflict between firms and workers, who were represented as organized in unions. Rowthorn (1977), Sawyer (1982), and Layard and Nickel (1985) are mentioned as the precursors of their model. Three elements are involved in this conflict, the wage

¹⁶ Of the thirty-four articles composing the Mankiw-Romer volume, twenty-one lack any empirical content, six have a small empirical part (comprising less than three tables presenting data) and seven are heavily empirical.

setting process, the level of employment, and inflation. The first step in their analysis is to define the short-period normal equilibrium of a given industry (i.e., with capital being fixed). Only two markets are considered, one commodity market and one labor market. Both are imperfectly competitive. In the commodity market, the price is set by firms imposing a markup over unit labor costs. Workers are represented by identical unions. The labor market equilibrium is determined by the interaction of two different functions the 'bargained real wage' (BRW) and the 'price-determined real wage' (PRW) functions. The first expresses the behavior of unions, the second that of firms.

As is current in Marshallian theory, markets operate in nominal terms and production takes place in advance of trade, which means that the labor market closes before the opening of the goods market. Unions are concerned with the real wage accruing to their members, yet at the closure of the labor market the real wage is still unknown. They just have expectations about it, for that matter adaptive ones. The crucial factor influencing their decision is the level of employment. The gist of Carlin and Soskice's analysis of unions' behavior is that there exists an inverse relationship between the expected real wage rate, which unions are able to secure through the money wage bargaining, and the level of unemployment. If the market is tight, and hence unemployment low, they can negotiate higher wages, for example, by threatening to start a strike. Carlin and Soskice formalize this idea in the bargained real wage function:

$$w^{UN} = b(U), db/dU < 0,$$

where w^{UN} is the real wage claim of unions, underpinning the nominal wage they set, and U is the number of unemployed workers, that is, the labor force (LF) minus the number of employed workers (N). The labor force is assumed to be given and independent from the real wage. It is assumed that daily hours of work are fixed.

Firms' price-determined real-wage function is a function of labor productivity (LP) and markup (m), both assumed to be constant. Hence, the price-determined real wage is a constant. Carlin and Soskice justify this assumption on the grounds that there is a general agreement among economists that under imperfect competition prices do not respond much to changes in demand.

$$w^F = \frac{W}{P} = LP(1-m) = w^*$$

where w^* is the equilibrium real wage. Because of the sequential setting adopted, firms have the last word in the determination of the real wage rate. The real profit rate per worker is the markup times labor productivity.

Between them, the two functions determine the short-period normal equilibrium of the labor market. It exists whenever the claims of firms and unions are consistent ($w^F = w^{UN}$). An equilibrium rate of unemployment ($U^* = LF - N^*$) ensues. Figure 13.3 illustrates this. Moreover, Carlin and Soskice show that the equilibrium rate unemployment implies a constant inflation rate. Hence, the

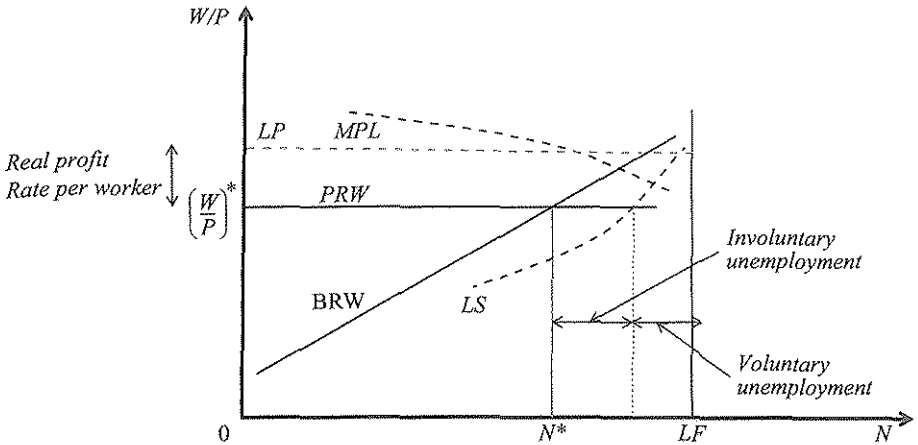


FIGURE 13.3 The labor market short-period normal equilibrium

NAIRU acronym ('non-accelerating inflation rate of unemployment').¹⁷ The NAIRU equilibrium is equivalent to Friedman's natural rate of unemployment and is likewise expressed as a vertical line.¹⁸

Figure 13.3 calls for the following comments. First, the functioning of the labor market cannot be dissociated from that of the other sectors of the economy. Therefore, the equilibrium level of employment, N^* , as determined in the labor market, must coincide with employment as determined by the intersection of the IS and LM functions, the production function allowing to go from output to employment. Second, the graph illustrates the fact that in equilibrium, at N^* , the labor demand function (the dashed MPL, for marginal productivity of labor, line), and the labor supply function (the dashed LS line) do not match: the price-determined real wage lies below the marginal productivity of labor, and the bargained real wage curve lies above the labor supply curve. Thus, at $w^F = w^*$, trade takes place off the supply curve. From this observation, Carlin and Soskice draw the conclusion that involuntary unemployment is present. Finally, Figure 13.3 also indicates that total unemployment can be sub-divided into two components, involuntary unemployment and voluntary unemployment (or choice of leisure).

¹⁷ "Only at the equilibrium level of employment is a constant monetary growth rate consistent with an unchanging level of employment. Another way of stating this is to say that the long-run equilibrium output level for the economy, in the sense that inflation is constant, is fixed by the intersection of the bargained real-wage and price-determined real-wage curves" (Carlin and Soskice 1990: 160).

¹⁸ "In both the imperfect competition model and Friedman's model, there is a unique unemployment rate at which inflation is constant and equal to the growth rate of the money supply, and this defines a vertical long-run Phillips curve" (Carlin and Soskice 1990: 158).

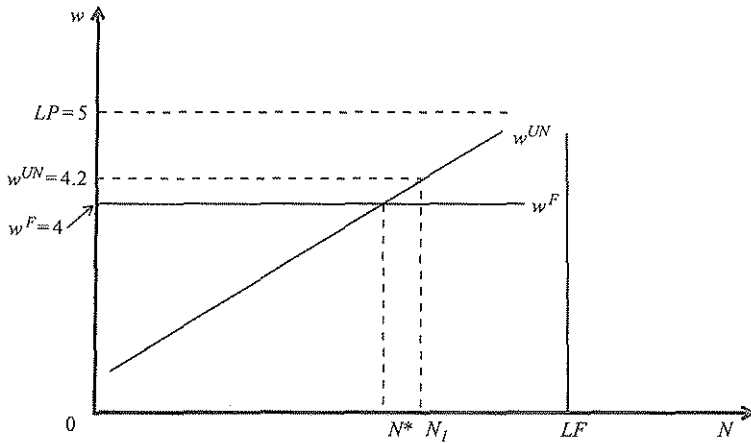


FIGURE 13.4 Inconsistent claims

I now turn to the disequilibrium part of the model. Carlin and Soskice's reasoning starts from a situation in which equilibrium prevails. In their model, unions are the first movers. The parameter on which they react is the level of employment. Thus, the element triggering the disequilibrium must be an observable change in the level of employment. As for the causing factor, they put forward a shift in either fiscal or monetary policy (or both), the effect of which is to move the output level away from its equilibrium quantity (Carlin and Soskice 1990: 157).¹⁹ Let us assume a fiscal boost, that is, a shift to the right of the output level. It generates an upward movement in the employment level, from N^* to N_t in Figure 13.4. It follows from the bargained real wage function that unions will take advantage of the situation to secure a higher nominal wage in the hopes of getting an increase in the real wage. Carlin and Soskice give the following numerical example. Suppose labor productivity is equal to 5 monetary units. When the NAIRU prevails, 4 units accrue to workers and 1 to firms. They assume that in the new employment context, unions are able to secure a nominal wage of 4.2 (see Figure 13.4).

The claims of firms and unions are now incompatible. Firms realize that they will incur a decrease in profit if they stay put, and they react by increasing the price of the output, thereby ensuring that the real profit/wage distribution remains unchanged. Carlin and Soskice also assume an accommodating monetary policy. Thus, an upward pressure on the inflation rate is also exerted. At this juncture, we fall back on the Friedman Phillips curve reasoning, as Figure 13.5 illustrates.

¹⁹ They also mention a shift in private-sector demand in passing, yet they do not pursue this line of thought.

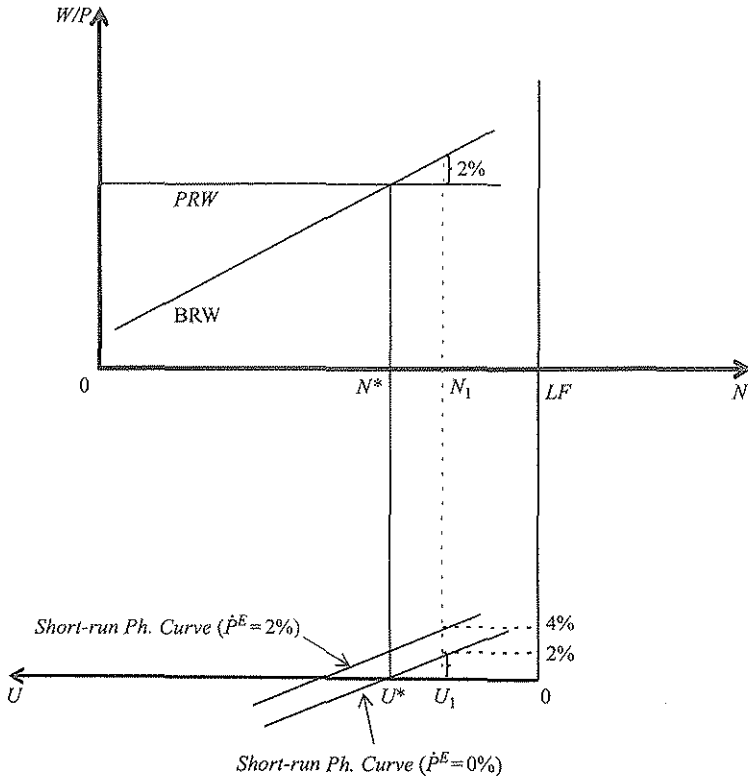


FIGURE 13.5 Labor market disequilibrium and inflationary dynamics

The situation described in Figure 13.5 is the same as in Friedman’s Presidential Address. Let us start from a situation in which inflation is nil. The increase in the price of goods translates into an upward move along the initial short-run Phillips curve up to the point where the inflation rate is 2 percent, in accordance with the percent increase in price. In view of the assumption that workers hold adaptive expectations, to keep employment at N_1 (or unemployment at U_1) a further increase of the rate of inflation is required in the next period, and the spiraling process described by Friedman is set in motion. It will lead to hyper-inflation, at which point the authorities will have to reverse their fiscal activation.

Contrasting the NAIRU and Friedman’s natural rate of unemployment

In their book, Carlin and Soskice were eager to underline the differences between Friedman’s model and their own. They mentioned several, but only two of them matter. The first one is that their model evolves in an imperfect competition framework while Friedman’s rests on perfect competition.

The second is that Friedman's model features labor market clearing and hence fails to account for involuntary unemployment while, they claimed, theirs does. Because to them, involuntary unemployment is an important stylized fact, this difference is crucial.²⁰ The first difference is obvious, yet what matters is its impact. I disagree with the second.

Involuntary unemployment

Carlin and Soskice defined involuntary unemployment in the usual sense of a situation in which workers are prepared to accept work at the going real wage yet find no job (Carlin and Soskice 1990: 5, 123). Their only vindication of its existence was through a graph – that is, the fact that in Figure 13.3 at the equilibrium real wage rate, trade occurs off the supply curve (p. 377). To me, it does not hold water.

A first reason is the argument that I have put forward when discussing non-Walrasian equilibrium modeling (see my discussion of market clearing in Chapter 7), namely, that trading off the labor supply is not a sufficient condition for drawing a market non-clearing conclusion. What matters is efficiency-cum-generalized individual equilibrium; if this condition requires trading off the supply, so be it, and no conclusion of an inefficient outcome should be drawn. The second reason relates to the pitfalls associated with reasoning through graphs. When in chapter 1 of their book, Carlin and Soskice constructed the labor supply curve, they used the standard neoclassical reasoning to solve the problem of how a rational agent allocates her daily time between leisure and consumption. This is a decision made at the intensive margin. In their wage bargaining model Carlin and Soskice discussed an outcome arising along the extensive margin, the number of agents participating in the labor market under the indivisibility assumption. My point is that the labor supply as obtained in the standard neoclassical leisure/consumption decision cannot be naively transposed to the NAIRU graph, as they do.

Carlin and Soskice's loose account of involuntary unemployment also shows up when delving into the issue of providing microfoundations for their aggregate functions. To this end, they resorted to the 'right to manage-cum-bargaining' argumentation based on Zeuthen (1980). They assumed that there is a single union in the industry studied and that it maximizes the expected utility of a representative agent. Such a framework automatically excludes any involuntary

²⁰ "*Stylized fact (ii)*: A large proportion of unemployment in the 1980s is involuntary and cannot be explained either in terms of mistaken expectations about the rate of inflation or monetary growth, or in terms of search activities" (Carlin and Soskice 1990: 372). "Friedman's model of inflation with an expectations-augmented Phillips curve was an attractive one, yet the insistence of Friedman, and even more of the New Classical economists, on a market-clearing microeconomic basis for the theory did not appear to fit easily with the policy problem at hand – the persistence of high rates of unemployment" (Carlin and Soskice 1990: 6).

unemployment result as the latter requires heterogeneity, that is, the employed and the unemployed enjoy a different level of utility. Carlin and Soskice did not seem to be aware of this. Just after having introduced the representative worker assumption, they wrote:

Utility is assumed to depend positively on the real wage in the industry and on employment in the industry. The usual justification for this is that the union is thought of as maximizing the expected utility of their employed *and* unemployed members where members have a random choice of being employed. (Carlin and Soskice 1990: 389)

How can the representative agent be at the same time employed and unemployed? Once the representative agent assumption is adopted, the discussion bears on the intensive margin; as a result, the outcome attained can possibly be referred to as underemployment with respect to a given benchmark – that is, the perfectly competitive level of employment – yet not as unemployment in the joblessness meaning. An alternative possibility is to assume a large number of identical workers. Then, Carlin and Soskice's quotation makes sense. However, if employment is decided through a lottery (as their random choice expression in the quote suggests), one can just as well imagine a way of mutualizing the risk of being unemployed, to the effect that agents will enjoy the same utility whether they are employed or unemployed. Hence, there is no *ex post* heterogeneity and referring to involuntary unemployment notion is misplaced. In short, this is a far cry from a robust demonstration of involuntary unemployment.

At the end of the day, Carlin and Soskice's model turns out to be basically similar to Friedman's model. What is particularly striking (and surprising in view of their outward new Keynesian affiliation) is that they share the same policy conclusion. Carlin and Soskice agreed with Friedman that demand activation is a foreclosed policy, as it fails to have a durable effect on employment.

With adaptive expectations, the government can run the economy at a rate of unemployment below the equilibrium rate by pursuing an accommodating monetary policy. The cost is rising inflation. Further should the government wish to reduce the stable rate of inflation at the NAIRU, then with adaptive expectations a lengthy adjustment process will ensue, with unemployment initially rising above the equilibrium rate of unemployment. (Carlin and Soskice 1990: 203)

Carlin and Soskice hardly commented on this conclusion, while, to me, it is crucial. It amounts to abandoning the money non-neutrality claim, which from Modigliani to first-generation new Keynesian models was a pillar of Keynesian macroeconomics. The policy conclusion of their model is then quite orthodox: taxation, expenditures on training and restricting the power of unions:

The idea of using income policy to reduce the equilibrium rate of unemployment derives from the possibility of introducing measures that lower the bargained real wage curve. The most familiar form of income policy directed towards this end is an agreement

between unions and employers over wage restraint which may or may not involve the government. (Carlin and Soskice 1990: 176)

I conclude that the Carlin-Soskice model is a striking case of backfiring. The starting point of their enterprise combines insights from Marx and Keynes, yet its end point is Friedman! The model they built ends up strengthening Friedman's policy conclusion by showing that it remains valid even when one departs from the perfect competition framework. As for Keynes, he is left on the side of the road.

Reacting to Lucas: Alternative Research Lines

In the two previous chapters, the reactions to Lucas's program that I discussed had a strong defensive component. Traditional Keynesians expressed an all-out rejection. First-generation new Keynesians accepted some of Lucas's principles, in particular the microfoundations requirement. However, they were of no mind to abandon those tenets of Keynesian theory they deemed crucial and which were absent from new classical and RBC modeling. In this chapter, I study the works of economists who reacted to Lucas by devising modeling strategies different from his own while nonetheless abiding by his standards. I have decided to study three of them: Peter Diamond's search and coordination model (1982), Oliver Hart's model of imperfect competition (1982), and John Robert's coordination failure model (1987). All three import new perspectives into the standard general equilibrium framework: search externalities and multiple equilibria in Diamond's model, imperfect competition in Hart's model, and a radically non-Walrasian trade technology in Robert's case.

DIAMOND'S SEARCH EXTERNALITY PROGRAM¹

Reading Diamond's interview by Moscarini and Wright (2007) as well as his Nobel Prize lecture (Diamond 2011) reveals that his vision of economics was shaped by two experiences, both occurring during his undergraduate years at Yale.² The first was his mathematical education; he majored in mathematics yet gradually moved toward economics. A course taught by Gérard Debreu, based on his *Theory of Value* (Debreu 1959) shortly after its publication, gave him an indelible neo-Walrasian general equilibrium orientation.

¹ In this section, I draw from Dantine and De Vroey (2014).

² Cf. Diamond's interview by Moscarini and Wright (2007), the autobiography he wrote for the Nobel Prize Committee (Diamond 2010), and his Nobel Prize lecture (Diamond 2011).

However, his attitude toward neo-Walrasian general equilibrium theory was that of a reformer:

My deep grounding in general equilibrium (Arrow-Debreu) theory includes a keen awareness of its limitations. The limitation that particularly interested me was the completeness of the coordination of agents that happens with complete competitive markets. (2010: 8)

The second factor was that in between his undergraduate and his graduate studies, he served as research assistant to Koopmans at the Cowles Foundation, “hired, he wrote, to help with the mathematics” (Diamond 2010: 2). There, “over memorable coffee time discussions,” he befriended Yale’s Keynesian macroeconomists, Tobin, Okun, and Brainard, picking up the “Yale macro vibe” from them (Moscarini and Wright 2007: 547). Yet there was also a caveat here. Although Diamond definitely considered himself a Keynesian economist as far as his vision of capitalism was concerned (a belief in the possibility of market failure on which government can remedy), he was less enthusiastic about Keynesian macroeconomics because of its lack of micro-foundations and its undue emphasis on wage rigidity:

While I started working on search theory out of dissatisfaction with general equilibrium theory, I gravitated to seeing search also as a way to address my dissatisfactions with macro theory. My dissatisfaction did not relate to basic Keynesian concepts, but to the nature of modeling. I wanted to see a microfoundation that would enhance the ability to do normative analysis and to develop policy insights. (Diamond 2011: 1056)

What Diamond found important in Keynes’s *General Theory* was (a) the possibility that the economy can be stuck in a suboptimal equilibrium due to suboptimal aggregate demand, (b) the view that rigidity at best plays a secondary role in explaining such a state of affairs, and (c) the ‘animal spirit’ insight – all ideas that Keynesian macroeconomics had swept under the rug. Hence the task he set for himself was to construct a model abiding by neo-Walrasian standards, except for trade technology, in which these three traits would receive pride of place.

After Lange, Patinkin, and non-Walrasian equilibrium economists, Diamond’s name must be added to the list of economists wishing to ground Keynesian theory on Walrasian principles. Yet, here also he danced to a different tune. These economists were sticking to the view that the way to do this was to introduce sluggishness or rigidity into the auctioneer trade technology. By contrast, Diamond advocated that the auctioneer hypothesis and its correlates, such as a price-taking behavior, needed to be fully dispensed with. Here is how he put his standpoint, contrasting it with the views held by economists such as Hahn, Takashi Negishi, and Franklin Fisher, who all strived to study the convergence toward equilibrium while assuming the existence of “false trading” (Hahn and Negishi 1962; Fisher 1983):

Diamond: I had been interested in the issues of convergence to competitive equilibrium, something Frank Fisher went on working with, and I was staying abreast with the

literature. What struck me was the question being asked: let's explore mechanisms that have some plausibility and see whether they converge to competitive equilibrium. And it crossed my mind that that was the wrong question. The right question was: Let's set up a credible mechanism and see where it goes. (Moscarini and Wright 2007: 553)

Diamond's aim was to start the analysis from a plausible trading process and to look at the allocation to which it would converge, which surely will be different from the Walrasian one. The process in question was search behavior, the underlying idea being that finding partners is costly and that externalities are involved.

The paper I study in this section (Diamond 1982) can be regarded as the culmination point of a series of previous papers that lay the groundwork for it. Although Diamond did not feel it was necessary to state this explicitly, the gist of the paper was to capture the elements cited earlier that Diamond considered to be the central message in *The General Theory*.

Diamond's paper stands out as an example of what neoclassical theory can do at its best, conveying a major insight with a simple yet elegant model. Its basic insights were further expanded in a 1984 book, *A Search-Equilibrium Approach to the Micro Foundations of Macroeconomics* based on his 1982 Wicksell lectures (Diamond 1984a). Another development was his "Money in Search Equilibrium" article (Diamond 1984b). In this, Diamond extended his coconut model to encompass money, transforming it into a fully-fledged two-sided search model. Finally, a joint paper with Drew Fudenberg (Diamond and Fudenberg 1989) aimed at drawing a theory of business fluctuations on the basis of the coconut model. My attention in what follows will be focused on the 1982 article and the 1984a book.

The coconut model

In his 1982 paper, Diamond's concern was a tropical island model economy. It comprises a continuum of risk-neutral self-employed identical agents living on an isolated island and having coconuts as their exclusive means of subsistence. To be able to eat coconuts, they must find a tree and climb it. Climbing has the cost c . Every coconut tree bears the same number of coconuts, yet there are variations in terms of the effort needed to reach the trees, and to pick the coconuts. The decision rule of this search takes the shape of a reservation cost c^* . Agents will get the coconuts from all the trees for which the cost is below c^* ($c < c^*$) and wait for the next opportunity otherwise. Diamond calls "unemployed" agents who fail to find a fitting tree and hence hold no inventory, and "employed" to those who found one and, hence, hold inventory.³ A further trait of the model is that, because of some taboos, agents are forbidden to consume the coconuts they have picked. There is thus a second

³ This terminology is misleading because the economy comprises no labor market.

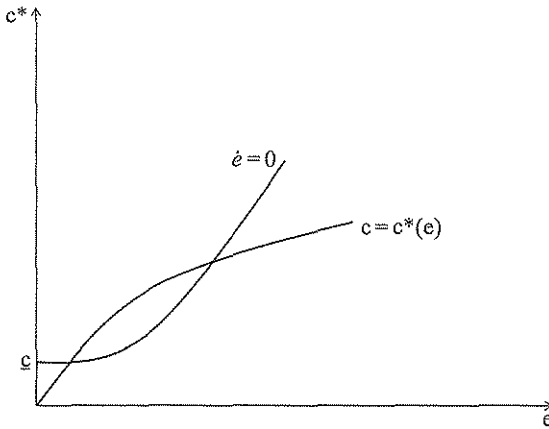


FIGURE 14.1 Different levels of activity in Diamond's search model

search operation, consisting of finding a trading partner. Trading opportunities arise according to a Poisson process. The driving assumption of the model is that trade technology exhibits increasing returns to scale. "If more people are attempting to trade, it becomes easier to carry out trades" (Diamond 1984a: 4). More precisely, the rate of arrival of trading partners, denoted b , is strictly increasing in the level of activity, e that is, the ratio of agents holding inventory to the total population; b is a function of e , $b(e)$, with $b' > 0$.

Diamond's next step is to look for steady-state equilibria where the level of activity does not change over time ($\dot{e} \equiv de/dt = 0$). In every period, there are agents who have found a tree that is worth climbing and are looking for a trade partner, and others who, having found a partner, no longer hold inventory and search for a tree. In steady state, these two measures are identical. Given a homogeneous distribution of trees, the probability of finding a suitable tree increases with c^* . Thus in the (c^*, e) quadrant, $\dot{e} = 0$ is an increasing function. Moreover, the cutoff cost c^* is an increasing function of e . A steady-state equilibrium is a situation in which a pair (c^*, e) satisfies both the locus of points for which $\dot{e} = 0$ and the function $c^*(e)$. Because both functions are increasing, for an equilibrium with a positive level of activity to exist, it suffices that one of these curves be concave and the other convex. Diamond also proves that, to ensure the concavity of (c^*, e) , it is sufficient that the arrival rate $b(e)$ be increasing and concave. As for the c^* curve, as long as there is a c^* at which the probability of meeting a tree with $c \leq c^*$ is one and it is reached asymptotically, it is convex. Finally, to guarantee the existence of multiple equilibria with positive employment, Diamond assumes there is a minimum level of production cost \underline{c} . This gives him the figure reproduced in Figure 14.1.

A shown in Figure 14.1, Diamond's model features multiple Pareto-rankable equilibria (in the figure they amount to three, the two intersections plus the origin). Put differently, the economy exhibits several levels of 'natural employment' and it can get stuck in a sub-optimal one, a situation that can be remedied by exogenous demand activation:

One of the goals for macro policy should be to direct the economy toward the best natural rate (not necessarily the lowest) after any sufficiently large macro shock. (Diamond 1982: 881)

This reference to macro shocks indicates that Diamond was interested in macroeconomics *à la* Lucas and had business fluctuations as its object of analysis, rather than macroeconomics *à la* Keynes aiming at demonstrating involuntary unemployment. In his model, "there is also no hired labor. I am therefore claiming that it is possible for a barter economy of self-employed individuals to have business cycles" (Diamond 1984a: 7). He made the same point in the Wicksell lectures:

The model I will present is a steady state equilibrium model. It has that form primarily for its simplicity. But I am interested in the model as a description of phenomena that are important in the context of a business cycle and must therefore evaluate the appropriateness of the assumptions to an economy that is subject to cycles. (Diamond 1984a: 4)

In the 1982 article, Diamond offered no explanation of what may explain fluctuations, but in his 1984 book he proposed one, based on waves of optimism and pessimism fuelled by self-fulfilling effects. The underlying idea is that the economy can bounce back and forth along different levels of activity, the result of changes in agents' expectations about the future economic environment. Assume that they are all optimistic thusly expecting easier trading. As a result, they will accept more production opportunities. The economy will then reach a higher equilibrium level of activity, thus warranting agents' initial optimism. The same is true for pessimism. The $c^*(e)$ curve will shift accordingly, upward when agents move toward more optimism and downwards in the opposite case. These changes in the equilibrium points of the economy resemble the business cycle.

Diamond and Lucas

Reading the second of Diamond's Wicksell Lectures strongly suggests that he regarded his model as an alternative to Lucas's. He was actually entitled to hold such an opinion. Few contemporary models could be deemed to belong to the same league as Lucas's "Neutrality of Money" paper in terms of originality and elegance, but Diamond's was certainly among these. If Diamond's paper was published in 1982, this means that he must have been working on it in the last years of the 1970s. In these years, the future of macroeconomics was still quite open. Hence it was indeed possible to nurture such an ambition.

TABLE 14.1 *Comparing the Lucas and the Diamond approaches*

	Lucas	Diamond
Aim of the model	1972: demonstrating that money non-neutrality does not warrant Keynesian policy conclusions 1976: extending the model to the study of business fluctuations	1982: demonstrating the possibility of sub-optimal levels of activity related to business fluctuations 1984a: extending the model to the study of business fluctuations
Aegis	Neo-Walrasian theory	Neo-Walrasian theory
Equilibrium	General equilibrium analysis; single equilibrium ; equilibrium discipline; rational expectations	General equilibrium analysis; multiple equilibria ; equilibrium discipline; rational expectations
Labor market and type of economy	Absent (self-employed workers inhabiting separate islands)	Absent (self-employed workers inhabiting a single island)
Price/wages	Flexible	Flexible
Money	Present	Absent (yet present in 1984b)
Trade technology	Auctioneer	Search
Explanations of business fluctuations	Money supply surprises	Animal spirits
Policy conclusion	Non-interventionism	Demand activation

A comparison of the Lucas and the Diamond programs shows that they have much in common. Both placed themselves under the aegis of the Arrow-Debreu model. They also shared the same preoccupation of making Walrasian theory amenable for macroeconomic purposes by simplifying it and modifying some of its assumptions. As a result, they developed a keener interest for policy matters than traditional Walrasians. Although they both insisted on empirical relevance, their two most important models (Lucas 1972 and Diamond 1982) were theory without measurement. Finally, they both regarded their seminal models, which were not directly concerned with business fluctuations, as fine stepping-stones for broaching this topic.

Lucas's and Diamond's modeling strategies are compared in Table 14.1. While it illustrates how close they are, it also brings out their differences (the items in bold). The triggering element is their respective relations to neo-Walrasian theory, resulting in their divergence about the trade technology to be adopted, the auctioneer or search. Once this bifurcation is taken, far-reaching consequences ensue: a single equilibrium versus multiple equilibria outcome, distinct ways of dealing with business fluctuations, and opposite policy conclusions.

This differentiation between Diamond and Lucas did not arise by chance. As Diamond's interview by Moscarini and Wright attests, when he conceived his 1982 model, Diamond had the firm intention of reacting to Lucas and Prescott's own search model (Lucas and Prescott 1974):

MACRO DYNAMICS: Why did you take that direction – as opposed to, say, Lucas and Prescott's island model?

PETER DIAMOND: The island model, I believe, as Lucas and Prescott set it up, fits the Welfare Theorem of Arrow-Debreu. They've got efficiency properties, and I believe the route into seeing that would be to think about it from an Arrow-Debreu perspective where the role of the island is a constraint on your consumption possibility set. There you have the property that I was trying to get away from – there is some central mechanism, something similar to the Walrasian auctioneer, which is controlling the flows between islands in a way that is a central mechanism (Moscarini and Wright 2007: 554).

Diamond had no qualms admitting that he pursued a policy motivation. Like Keynes, he believed that the market system is susceptible to fall prey to malfunctioning, in particular under the form of coordination failures. Demand activation is the policy measures to be adopted when they arise. Diamond's problem was translating this insight, and its ensuing policy conclusion, into an emendation of a simplified Arrow-Debreu model. Ideally, the new factor introduced needed to have the status of a compelling "fact of life." As already stated, Diamond was sensitive to the time dimension involved in the attainment of equilibrium, but he ended up introducing it only laterally, through the thickness idea: the thicker the market, the bigger the number of potential traders, the shorter the time spent finding them. To Diamond, this idea was beyond dispute in terms of real-world relevance, and its neglect hardly benign, as taking it into account reversed the policy conclusion of the non-amended model:

The importance of this basically different way of viewing the world is that it shifts the presumption from limitations on policy to the potential for good policy. (Diamond 1984a: 63)

In the Wicksell Lectures, Diamond also raised the question, "How can one decide whether competitive markets or search represents a better starting point for theoretical macro analysis?" (1984a: 46). His answer was, of course, no mystery. On the one hand, he argued that his model had a better fit. On the other hand, he brought out the limits of the rational expectations assumption. With hindsight, none of these arguments was really biting. To limit myself to his criticism of rational expectations, Diamond walked a tightrope since, after all, his model was also based on this assumption. This led him to make Salomon-like judgments, writing for instance that rational expectations are "still the right assumption to use for most, but not for all, analyses" (1984a: 54) – a typical neoclassical synthesis assertion. In his

view, its main defect was that, as soon as multiple equilibria are present, agents are unable to pick up the equilibrium state that is highest in welfare ranking, although they are equipped with rational expectations. Another defect he mentioned was that making the rational expectations assumption prevents from coming to grips with factors such as optimism or pessimism, mimetic behavior and self-fulfilling prophecies. Both remarks are valid if one accepts that multiple equilibria is the route to be taken, but irrelevant for those who decide to take another one. In other words, as is often the case, the criticism of Diamond rested on his premise that his starting point is better than alternative ones.

The development of macroeconomics in the 1980s did not go in the direction advocated by Diamond. From the start, the Lucasian program had the edge. First, it was ten years older and this time span had been used in strengthening it. Second, the Lucas model belonged to a well-established stream of literature pertaining to the real effects of monetary change. The model was innovative, but the subject was well known. By contrast, Diamond traveled a less-known territory; the theoretical study of coordination failure was nascent. Third, multiple equilibria may well be an appealing idea but managing single equilibrium models was definitely easier. The extension Diamond did with Fudenberg bears witness to this. Their aim was to show how from any initial conditions one can find multiple dynamic paths to each steady-state equilibrium. But, although the paper seemed to make a general observation, it did not deliver in this respect, just providing a few examples. More daunting even was the problem of how to test a multiple equilibrium model empirically.⁴ Fourth, Diamond admitted that his model differed from Lucas's on only point, the increasing return assumption.⁵ To him, this point was crucial and justified. Nonetheless, in such circumstances, there exists an easy way of discarding the challenging model, namely to declare it to be a special case of the classical model rather than a radical alternative.⁶ Fifth, although Lucas's 1972 model and Diamond's 1982 model might have the same persuasiveness ability, the same was not true for their respective extensions. Lucas's equilibrium model of the business cycle flowed from his 1972 model in a straightforward way. This cannot be said of the Diamond-Fudenberg extension of Diamond (1982).

⁴ "Lucas prefers to describe fluctuations using models that have unique equilibria. ... Lucas's preference for uniqueness is not a consequence of the general modeling strategy that he endorses, since we now have many examples of models that meet those standard and in which multiple equilibria exist. Rather uniqueness is desired because it seems much easier to interpret time series observations in terms of a model with a unique equilibrium" (Manuelli and Sargent 1988, p. 538).

⁵ "The model I presented in the first lecture looks a great deal like the classical market model if one removes the assumption of trade externalities. That is, if the relative availability of trading partners does not affect the length of time to find a trade, then the search model must behave like a classical market model" (Diamond 1984a: 49).

⁶ Albrect (2011) is an example.

Howitt

One of the most enthusiastic supporters of Diamond's model was Peter Howitt.⁷ In a lecture given in 1986 at the Canadian Economics Association Meetings, he declared that he saw macroeconomics at an important crossroads, one route being RBC modeling, the other transaction externality modeling. "Which of these two paths will be the main attractor of graduate students in the years to come is impossible to predict" (Howitt [1986] 1990, p. 79). The reason for Howitt's support was that he was a 'Leijonhufvudian.' He believed that the issue of coordination failures was the gist of Keynes's theory. Hitherto, Keynesian economists had been unable to formalize this intuition. According to Howitt, Diamond had broken the spell.⁸ Hence, at the same meeting of the Canadian Economics Association, Howitt branded transaction cost theory as the vehicle of 'Keynesian recovery' (Howitt [1986] 1990).

According to Howitt, what was needed was to bring Diamond's model closer to reality. He wanted the long-term bilateral relationships typical of real-world labor markets in particular to be part of the model. This implied replacing Diamond's self-employed worker economy framework with an economy comprising a labor market. Howitt pursued his program in several papers, either written alone (1985, 1986, 1988) or jointly with Preston McAfee (1987, 1988, 1992).

The aim of all these different papers was similar to Diamond's, that is, introducing search externalities leading to multiple equilibria, this time a labor market occurrence. For example in Howitt (1985), the 'thin-market externality' (the thinner the market, the higher the transaction cost) affects both firms and workers, both having their selling efforts decrease with the quantity of either output or labor traded. "These effects of externalities make it possible for D [the demand for labor] and S [the supply of labor] to intersect more than once in a non-pathological cases" (Howitt 1985: 96). A similar conclusion was drawn in the Howitt/McAfee 1987 paper:

Multiple equilibria exist with different rates of unemployment. Both high and low unemployment rates can exist at the same real wage rate for reasons that are related to the communication failures analyzed by Leijonhufvud; the expectation of a low level of labor-market communication can be self-fulfilling. (Howitt and McAfee 1987: 106–107)

⁷ Howitt, who started as a monetary economist, was close to Clower and Leijonhufvud's reappraisal of Keynes's theory. Later, he joined with Philippe Aghion in writing two influential books on growth theory (Aghion and Howitt 1999, 2009).

⁸ "[Keynesian economists] tell you that the slowness of wages to fall is supposed to represent transaction costs and coordination problems. It takes time for people to find matches in the job, job search is inhibited by the unwillingness of firms to recruit during recession, and so on. The main problem with these explanations is that they come from Keynesian economists but not from their models. The new literature on transaction externalities start with a more or less explicit conception of the market organization that gives rise to transaction costs and shows how they impinge directly on individual decision-making" (Howitt 1990: 79).

The irony of Howitt's efforts is that it is not sure that, for all their cleverness, his models ended up strengthening Diamond's results. It is true that the dismissal of the rigidity assumption remained present, an important result as it marks a breach from the line taken by traditional Keynesianism. Unfortunately, Howitt's explicit examination of the labor market displays significant drawbacks. To begin with, the analysis is no longer general equilibrium. Another issue concerns policy conclusions. In Diamond's model, demand activation allowed the economy to move towards a more optimal level of activity. This result is no longer obtained in Howitt's models. Finally, one may wonder whether the explicit introduction of a labor market really improves Diamond's suboptimal level of activity result. Howitt's claim is that thereby involuntary unemployment re-enters into the theoretical picture. This can be questioned. His models exhibit market clearing, both in the matching of supply and demand and in the generalized individual equilibrium sense. Therefore, it is more apposite to state that their concern is underemployment. But then, if it is simpler to demonstrate this result in a framework with self-employed agents, is it worthwhile to use Howitt's heavier conceptual apparatus?

ROBERTS'S COORDINATION FAILURES MODEL

Roberts's aim when constructing his model was to capture Clower's idea of self-confirming conjectures. Roberts was also heir to non-Walrasian economists in emphasizing perceived quantity constraints as well as the idea that demand constrains employment. Yet he dispensed with the fix-price assumption. Roberts shared with Diamond the intuition that to obtain Keynesian results, getting rid of the Walrasian trade-organization assumption is necessary. "The key is in the modeling of the processes determining prices and individual transactions" (Roberts 1987, p. 856).

To achieve this project, Roberts assumed a separation and specialization in production and consumption. There are two types of producers (A and B), two types of worker-consumers (J and K), two types of labor (r and s), and two flows of goods (x and y). In total, the economy comprises five commodities, money (m), which is non-produced, being the fifth. The model is based on the generalized absence of a 'Ford effect.' No worker can supply inputs to a producer from whom she might buy outputs. Symmetrically, a worker buys output only from producers to whom she supplies no labor. As shown in Figure 14.2, the J s can supply input only to A s, while they can purchase output only from B s (the arrows mean sales). All agents are endowed with money and goods. Only producers have the technical knowledge to produce goods. Labor is the only factor of production. Returns to scale are constant. The input-output coefficient is set at unity. Production is made to order. No inventories are present. Prices and wages are flexible, in that each producer may set the price and wage she controls at any level she wishes.

Three stages in the formation of equilibrium prices and quantities are separated: first prices are announced by producers, second workers react by making

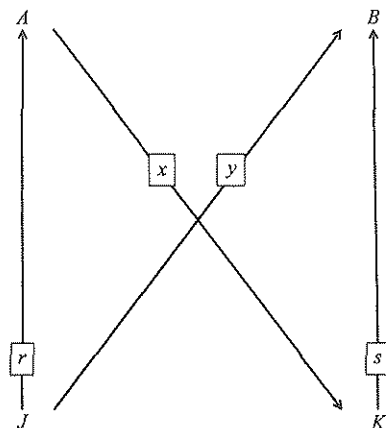


FIGURE 14.2 The trade structure in Roberts's model

a trading proposal, third, producers make their quantity decisions. The economy and institutions together define a game in extensive form. Examining the subgame perfect equilibria, Roberts shows that a continuum of equilibria is possible. Walrasian equilibrium is one of these. In spite of the special trade technology assumption made, the observed price and wage and quantities traded are the same as those that would have been obtained had the price formation process been led by an auctioneer. Another possible equilibrium is when agents do not trade at all. Keynesian equilibria can also exist, in which some of the consumers trade their Walrasian quantities at the Walrasian price and wage, while others consume their initial endowments. They can be observed as supplying no labor and demanding no output.

The reason behind such a result has to do with the noncooperative character of the game coupled with its institutional arrangement, on the one hand, and with the presence of a nonproduced good, on the other. Involuntary unemployment will result from households of one type developing self-fulfilling pessimistic conjectures about the quantity choices made by households of the other type (Roberts 1987, p. 868).

Roberts goes deeper than Diamond in addressing the coordination problems likely to arise in a decentralized economy. Like Diamond and Okun, he puts his finger on trade technology. Like Diamond's, his model also succeeds in clearing wage rigidity of causing unemployment. However, no vindication of demand activation is to be found in his model.

HART'S MODEL OF IMPERFECT COMPETITION

Hart's model is a static general equilibrium model with imperfect competition, based on a Cournot-Nash definition of equilibrium. The economy

studied comprises three goods, a produced commodity, a non-produced commodity, and labor. The non-produced good serves as *numéraire*. The economy is composed of a large number of firms and households. The firms all produce a single good. The number of individuals is a multiple of that of firms. Agents are endowed with units of the non-produced good and labor. Each agent is assumed to own a fraction of every firm. Labor is supplied inelastically. Households' demand for the produced good is a function of its price but also depends on their income. The latter is formed from wages and profits and their endowment in the non-produced good. Although income is taken as given on every market, it is endogenously determined at the level of the economy as a whole. Different market structures are considered. Consider the output market first. At one extreme, all firms may belong to the same market. In this case, perfect competition prevails, all firms being small with respect to the market. In the other cases, it is assumed that the goods market is sub-divided into separated entities, each of them being a reduced version of the economy, with the same ratio of firms to consumers. Because the number of firms is given, the higher the number of separate markets, the higher the prevailing degree of monopoly. The same type of subdivision characterizes the global labor market. That is, it is subdivided into sub-markets in which workers form syndicates and hold market power. The objective of the syndicate is to maximize the total income of the workers on the market. The smaller the number of unions per market, the higher their market power. It is assumed that in both markets the oligopolistic suppliers know the objective demand functions. Perfect competition and monopoly are the two limiting cases, but Hart was interested in the case of oligopolies. Their existence results in the economic activity being lower than under perfect competition. Efficiency is equated with the latter. Hart's reasoning proceeded in four steps.

First stage: firms' optimal decisions

In every output market, firms act as Cournot oligopolists. An individual firm chooses its output level in order to maximize profits, taking as given the amount of output sold by other firms (as well as the income of the buyers and the wage rate). In equilibrium, marginal revenue equals marginal cost.

Second stage: unions' optimal decisions in a partial equilibrium context

Unions know the solution to the firm's optimal decision. Therefore, they know the demand for labor in each labor market. In a partial equilibrium analysis, the choice problem of an oligopolistic syndicate simply consists of choosing the point on its residual demand for labor curve that maximizes its objective function, taking as given the quantity of labor traded by the other unions.

Third stage: unions' optimal decisions in a general equilibrium context

In a general equilibrium context, syndicates must take into account the fact that any reduction in labor traded will drive the wage rate up, which in turn will generate a higher price and lower output and hours worked. To take this into account, Hart reexpresses the elasticity of demand for labor in terms of the elasticity of demand for output.

Fourth stage: making income endogenous

The joined income of the buyers on an output market at the Cournot equilibrium price must be identical to the level of income that underpins the demand curve faced by firms.

The level of employment attained under perfect competition is called full employment. Hart's aim is to produce a result of underemployment. It arises if two conditions are fulfilled. First, the labor market must be oligopolistic. If the goods market is oligopolistic and the labor market competitive, full employment is always reached. The second condition concerns the relation linking two magnitudes that the price of the good can take, \hat{p} and p^* . \hat{p} is the price of the good that is optimal for the unions. p^* is the price of the produced good allowing the prevailing level of demand for it to ensure full employment. If $\hat{p} > p^*$, underemployment exists. The underlying idea is that the higher the price of the produced good, the higher the income spent on the non-produced good and the lower the demand for labor.

Hart's claimed that his model vindicates demand activation can be questioned. He argued that the quantity of labor traded and the quantity of goods produced can be increased without changes in prices. The underlying device is either an increase in the per capita endowment of the non-produced good (an increase in the stock of the non-produced good) or changes in consumers' tastes in such a way that the demand for produced goods is multiplied by a positive factor. The problem is that this 'policy' merely amounts to changing the data of the economy.

CONCLUDING REMARKS

Let me now turn to what I think is the basic reason why these models failed to be game changers. Although they were all theoretical gems, and were appreciated as such, their impact was limited. They shared the same basic weakness of being one-shot achievements. New models always start this way. What really matters, however, is whether they can be transformed into a progressive, workable research program – 'progressive' meaning that it gives rise to a succession of cumulative developments, 'workable' that the needed tools and recruits for such developments show up at the right time. Against this criterion, the Lucas program fared better than those of Diamond, Roberts and Hart. If this was the case – and Lucas was fortunate in this respect – it is because, more or less at the same time when the models studied above were conceived,

Kydland and Prescott came and took over from Lucas, inaugurating the RBC variant of the DSGE program. This was the crucial turn. The 'victory' of the DSGE program must be associated with the ascent of RBC modeling. Kydland and Prescott transformed Lucasian qualitative modeling into quantitative modeling (a transformation that Lucas had called for with his FORTRAN injunction). Adding the 'replication discipline' to the 'equilibrium discipline,' they were able to impose the idea, which had been implemented by Keynesian macroeconomists but had somewhat been set aside when a Walrasian perspective came to be introduced into macroeconomics, that there is no salvation outside of applied work. RBC modeling was thus a game changer. It stabilized the Lucasian revolution into a narrow research program providing the bread and the butter for regiments of economists for more than a decade. This is what I will expound in the next three chapters.

Real Business Cycle Modeling: Kydland and Prescott's Contribution

The year 1982 saw one of the most important papers in modern macroeconomics published. That was Kydland and Prescott's "Time to Build and Aggregate Fluctuations". It ushered in the era of quantitative theory. . . . It probably has changed the course of macroeconomics in much the same way as Keynes's *General Theory* and Lucas's "Expectations and the Neutrality of Money" have. . . .

Greenwood ([1994] 2005: 16)

I will devote three chapters to the study of real business cycle (RBC) modeling. In the present one, my attention is focused on Kydland and Prescott's trail-blazing work. In Chapter 16, I will examine critical reactions to and further developments of the RBC approach. In Chapter 17, I will offer my personal assessment.

My aim in this chapter is twofold, both descriptive and reflexive. First, I want to describe Kydland and Prescott's paper, "Time to build and aggregate fluctuations" (1982) and its transformation into a baseline model. Second, I wish to bring out the originality of the RBC methodology. To this end, I will ponder the nature of the economy that is analyzed in the baseline RBC model. To the reader's possible surprise, I will show that the gist of the RBC methodology can be drawn from such a reflection. Another point that I wish to underline is the role played by Kydland and Prescott in strengthening the Lucasian revolution. I will claim that what they did for this revolution was similar to what Hicks and Modigliani did for the Keynesian revolution, namely putting it more firmly on track.

THE PASSING OF THE BATON TO KYDLAND AND PRESCOTT

While Lucas spearheaded a new way of doing macroeconomics, his own model of the business cycle centered on the idea that business fluctuations were

generated by monetary shocks did not prevail long. At first, its prospects seemed fine. In a well-received paper, Barro undertook to test the main conclusion of Lucas's model, namely, that only unanticipated changes in money supply affect real variables (Barro 1977). The result he arrived at was that the current and two annual lag values of unanticipated money growth had a high explanatory value for variations in unemployment (Barro 1977: 114). Soon, however, more disappointing results surfaced, and Barro himself admitted that further developments cast doubt on his initial results (Barro 1989).¹ The main criticism leveled at Lucas's model was that the prompt availability of data about monetary aggregates goes against Lucas's assumption that agents face a difficult signaling problem. Moreover, several empirical studies were able to put forward that the cyclical effects of monetary shocks are small relatively to the variability of output and employment.²

Lucas gamely admitted to these shortcomings and came to endorse Kydland and Prescott's new type of modeling from which monetary shocks were absent and replaced by technological ones. Here is how he explained himself in his Professional Memoir and in a later interview:

Though I did not see it at the time, the Bad Peak conference [organized by the Federal Reserve Bank of Boston in 1978] also marked the beginning of the end for my attempts to account for the business cycle in terms of monetary shocks. At that conference, Ed Prescott presented a model of his and Finn Kydland's that was a kind mixture of Brock and Mirman's model of growth subject to stochastic technology shocks and my model of monetary shocks. When Ed presented his results, everyone could see they were important but the paper was so novel and complicated that no one could see exactly what they were. Later on, as they gained more experience through numerical simulations of their Bad Peak model, Kydland and Prescott found that the monetary shocks were just not pulling their weight: by removing all monetary aspects of the theory they obtained a far simpler and more comprehensible structure that fit postwar U.S. time series data just as well as the original version. (Lucas 2001: 28)

That '75 paper was a dead end. I mean, it was an attempt to introduce some kind of usual dynamics into my '72 paper, and it didn't work. I think that Kydland and Prescott's 1982 paper followed from that. (Lucas's interview by Usabiaga 1999: 181)

KYDLAND AND PRESCOTT'S 1982 MODEL

Kydland and Prescott's article, "Time to Build and Aggregate Fluctuations" (1982) and John Long and Charles Plosser's "Real Business Cycles" (1983) are

¹ For general accounts, see, e.g., McCallum (1989), Cooley and Hansen (1995), King (1981), Taylor (1999) and Gordon (2011).

² Cf. McCallum (2000). The studies in question are Sims (1980), Litterman and Weiss (1985), Eichenbaum and Singleton (1986), and Nelson and Plosser (1982).

the two papers that started the RBC line of research.³ Both strove to model business fluctuations as the result of real shocks to the economy occurring in a Pareto-optimal environment. But Kydland and Prescott's paper had the additional feature of wanting to assess their model empirically, inaugurating a new methodology to this end.

Kydland and Prescott's 1982 article is an applied piece aiming at showing that real-world economic fluctuations in the United States from 1950 to 1975 can be replicated by a model in which fluctuations are generated by economic agents' optimizing adjustment to exogenous technological shocks.⁴ Let me start by pointing out its distinctive features with respect to Lucas's approach. First of all, Kydland and Prescott engage in a new modeling strategy by taking advantage of the second welfare theorem, according to which any Pareto-efficient allocation can be decentralized into a competitive economy allocation. As the equilibrium of a planning economy is easier to compute than that of a competitive economy, one can first solve the planning economy equilibrium and next calculate the equilibrium prices prevailing in the supporting competitive economy. Such a line had been opened by eminent predecessors. The first of these was Ramsey, who devised a one-sector growth model (Ramsey 1928). It solved a planning problem, the intertemporal optimizing program of a representative agent over an infinite horizon subject to a budget and a technology constraint, Ramsey's model was developed in a general equilibrium framework by Koopmans ([1965] 1966) and Cass (1965). Their optimal growth models were further extended by Brock and Mirman (1972) to cover the stochastic case. Kydland and Prescott's clever idea was to import the ideas of these neo-Walrasian economists into macroeconomics, making them the stepping stone of an applied project, a usage that these predecessors had hardly thought of.

Kydland and Prescott's second distinctive feature was to have taken Lucas's FORTRAN injunction in earnest. Taking advantage of the tremendous progress in computational ability and of the increased availability of data bases at the time, they decided to bring the model to the data. To obtain numerical values for the parameters of the model, they initiated a new empirical strategy, calibration, different from econometric estimation. Until then, calibration had been used only in the remote and narrow circle of computational general equilibrium theorists. The advantage of resorting to it was negative: it avoided, first, the refutation conclusion that would have followed with econometric

³ In the duo formed by Kydland and Prescott, Prescott was at the forefront when it came to discussing and defending their work. Therefore, I shall sometimes refer to his name alone. Prescott did this job brilliantly and effectively, but also in a way that some have found dogmatic (Duarte and Hoover 2012: 21).

⁴ For a fine introduction to RBC modeling see Plosser (1989). King, Plosser, and Rebelo (1988) and King and Rebelo (2000) are more technical. For the genesis of Kydland and Prescott's model, see Young (2014).

testing and, second, it sidestepped the difficult enterprise of constructing ‘deeply structural’ econometric models.

So, while Lucas blazed the trail at the theoretical level, Kydland and Prescott’s claim to fame follows from their having made models *à la* Lucas quantitative. In Woodford’s words:

The real business cycle literature offered a new methodology, both for theoretical analysis and for empirical testing. . . . It showed how such models [of the Lucas type] could be made *quantitative*, emphasising the assignment of realistic numerical parameter values and the computation of numerical solutions to the equations of the model, rather than being content with merely qualitative conclusions derived from more general assumptions. (Woodford 1999: 26)

Another crucial modification Kydland and Prescott made consisted in changing the nature of the shock, the monetary shock being replaced by stochastic autocorrelated technological shocks.⁵ This change was crucial because previously economists on all sides admitted that monetary changes exerted real effects in the short run. Finally, Kydland and Prescott also abandoned Lucas’s signal extraction difficulty insight.

These differences should, however, not hide that the Kydland and Prescott model directly stemmed from Lucas’s work. Its construction fully abided by the standards defined by Lucas. It was based on the rational expectations assumption as well as on the Lucas-Rapping supply function with its intra- and intertemporal leisure substitution possibility.

In a nutshell, Kydland and Prescott wanted to answer a question of an applied nature: to which extent observed output fluctuations in the United States could be attributed to technology shocks? The answer to this question required confronting model-generated time series with real-world ones, the parameters of the model being drawn from independent micro observations. The dimensions selected were the following ones: output, consumption, investment, capital stock, hours worked (i.e., person-hours employed), and productivity (output per hour).

The task involved was colossal. To begin with, it implied modifying the national accounts to make them consistent with the theory. Next came a series of steps, each of them highly demanding: (a) constructing the fictitious model economy, (b) giving values to its parameters using calibration, (c) solving the model using numerical methods, (d) stimulating time paths for the selected variables, and (e) comparing the model-generated time series with actual U.S. statistics.

⁵ Prescott recurrently stated that, at the beginning, he was convinced that monetary shocks were the cause of business fluctuations and that, when Kydland and him started their empirical work, it came as a surprise to realize that no such effect appeared. See, for example, Young (2014) and Prescott ([1986a] 1994: 266).

TABLE 15.1 *Kydland and Prescott's main results*

Variable	US Time series (1947:1-1991:3)	RBC standard model
Standard deviation of output (σ_Y)	1.92	1.30
Consumption versus output (σ_C/σ_Y)	0.45	0.31
Investment versus output (σ_I/σ_Y)	2.78	3.15
Hours versus output (σ_H/σ_Y)	0.96	0.49
Correlation ($H, Y/H$)	-0.14	0.93

Source: Hansen and Wright (1992); Y is output, C consumption, I investment and H hours worked.

To a significant extent, Kydland and Prescott succeeded in their enterprise, as they were proud to report in a later paper:

To answer this question [of how volatile the U.S. postwar economy would have been if technology shocks had been the only contributor to business-cycle fluctuations] a model economy with only technology shocks was needed. Using the standard neoclassical production function, standard preferences to describe people's willingness to substitute intra- and inter-temporally between consumption and leisure, and an estimate of the technology shock variance, we found that the model economy displays business cycle fluctuations 70 percent as large as did the U.S. economy. This number is our answer to the posed question. (Kydland and Prescott 1996: 74)⁶

As shown in Table 15.1, their model reproduces both the lower variability of consumption and the higher variability of investment as compared to the output volatility. The same is true for the procyclicality and the persistence of most of the variables considered. However, as readily admitted by Kydland and Prescott, their model also displayed important discrepancies with respect to the data. They pertained mainly to hours worked and productivity (in view of the functional forms adopted, average productivity is proportional to marginal productivity, and the latter equal to the real wage rate). The main problem concerned the relative variability of real wage and hours worked, the 'wage-employment variability puzzle.' Time series indicated (a) that hours worked fluctuated considerably more than the real wage and (b) that the correlation between them was near zero. By contrast, Kydland and Prescott's model predicted that they have the same variability and that their correlation is close to one. Another shortcoming of Kydland and Prescott's analysis concerned changes in the wage rate and the interest rate. Labor productivity (and thus the wage rate) was strongly pro-cyclical in the artificial economy but only weakly so in the data.

⁶ In Kydland and Prescott's inaugural study, productivity shocks explained 50 percent of the U.S. volatility of output. The figure of 70 percent quoted earlier is drawn from a subsequent paper based on additional data.

All in all, however, Kydland and Prescott's model was regarded an impressive achievement. As stated by Plosser, that such a simple model "with no government, no money, no market failures of any kind, rational expectations, no adjustment costs and identical agents could replicate actual experiences this well is most surprising" (Plosser 1989: 65). What made the Kydland and Prescott model stunning was that, while resting on just one shock and six parameters, it delivered as much as models containing dozens of equations and many more free parameters.

Still, the initial reception of the paper was lukewarm. It came to prominence only gradually, one factor of its diffusion (on top of its intrinsic value) being Prescott's strong dedication to his doctoral students at the University of Minnesota and his ability to create an *esprit de corps* between them. The change in attitude that took place can be grasped by comparing two papers written by King, each with a different co-author, assessing the state of macroeconomics and published in 1987 and 2000. In the first of these, an entry on "Business Cycles" in the *New Palgrave Dictionary* (1987), Dotsey and King considered that four approaches in macroeconomics could be put on equal footing as promising research lines. They were the RBC model, Lucas's money-surprise model, the staggering contracts model and several types of models based on the goods nominal rigidity assumption. About fifteen years later, a contribution to *Handbook of Macroeconomics*, this time co-authored with Rebelo (King and Rebelo 2000), witnessed that a Darwinian process had been at play. The real business cycle model had ousted its rivals. Kydland and Prescott, it turned out, had set the agenda (McCallum 2000).

ANCHORING RBC MODELING IN THE SOLOW MODEL

In the years following their 1982 paper, Kydland and Prescott consolidated their approach in several ways. In a more or less contrived way, they traced their work back to illustrious forerunners such as Havelmoo and Frisch. They also took pains to spell out their methodology in several papers, some of them jointly authored, others written only by Prescott.⁷ By far, the most important advance was Prescott's "Theory Ahead of Business Cycle Measurement" paper ([1986a] 1994), in which he anchored the study of business fluctuations in Solow's growth model. Now, business cycle fluctuations were understood as events disturbing the secular growth of the efficiency of labor, either positively or negatively.

The Solow model originated in a 1956 article entitled "A Contribution to the Theory of Economic Growth" (Solow 1956).⁸ Solow's article was a reaction to the Harrod-Domar model. The latter exhibited a knife-edge long-run

⁷ See Kydland and Prescott (1991; 1996), Prescott ([1986a] 1994; [1986b] 1994; 2006), Cooley and Prescott (1995), and Prescott (2006).

⁸ Accounts of the Solow model abound. See, e.g., Jones (1998) and Romer (2011). Solow (1988) provides interesting afterthoughts.

equilibrium, departures from which automatically led to growing unemployment or prolonged inflation. Solow showed that this gloomy conclusion was due to the assumption of fixed proportions between factors of production, and that a more optimistic view about stable growth ensued from assuming that they were substitutable.⁹ The Solow model also provided a broader framework enabling to assess the secular growth of particular economies as well as differences in wealth across countries.

Prescott claimed that RBC modeling enriched the Solow model by adding microfoundations to it – hence the ‘neoclassical growth model’ label.¹⁰ Its scope was also broadened since it was now claimed that it could be used for the study of both growth and business fluctuations. There is an irony in the Solow model being proclaimed the fountainhead of RBC macroeconomics. Kydland and Prescott agreed with Lucas about the judgment that “Keynesian theory is dead.” For his part, Solow had always been a stern defender of Keynesian theory. It is a small wonder then that he had mixed feelings about such cooptation of his work. He disagreed with adding microfoundations to his model:

I deliberately avoided recourse to the optimizing representative agent and instead used as building-blocks only aggregative relationships that are in principle observable. (Solow 2008: 244)

Solow also disliked using the same model for the study of growth and fluctuations. A fierce supporter of the neoclassical synthesis, he strongly believed that business fluctuations and growth should be analyzed with different conceptual apparatus. The proper concern of a growth model, he declared, is a twenty- to fifty-year time interval; one should not use it “to track an economy in deep depression or in a runaway boom” (Solow 2001: 25).

One important reason why the Solow model was useful to Kydland and Prescott relates to the growth-accounting exercise that Solow undertook in a subsequent article, “Technical Change and the Aggregate Production Function” (Solow 1957). In his 1956 article, Solow had argued that technical progress rather than capital accumulation was the driving force in the long period. Theoretically, this amounted to declaring that shifts of the aggregate production function, that is, total factor productivity (TFP) growth, were more important than movements along it.¹¹ The fact that, so understood,

⁹ Halsemeyer and Hoover (2013) contest the standard interpretation of Solow’s relation to Harrod.

¹⁰ By contrast, Prescott characterized the Solow model proper as ‘classical’: “With this model [the Solow model], however, labor is supplied inelastically and savings is behaviorally determined. There are people in the classical growth model economy, but they make no decisions. This is why I, motivated by Frisch’s Nobel address delivered in 1969, refer to this model as the classical growth model” (Prescott 2006, p. 215).

¹¹ My preferred explanation of total factor productivity is an old one, Jorgensen and Griliches’: “Our definition of changes in total factor productivity is the conventional one. The rate of growth of total factor productivity is defined as the difference between the rate of growth of real product and the rate of growth of real factor input. The rates of growth of real product and real factor

technology is unobservable spurred Solow's attempt to assess it indirectly by carrying out a growth-accounting exercise on U.S. data for the 1909–1949 period. Solow's clever hunch was to differentiate the production function using the firm's first-order condition for cost minimization. Under a series of conditions, the most obvious ones being perfect competition and constant returns to scale, output elasticities receive an observable counterpart, the revenue share of factors. The impact of technology can then be obtained as a residual by subtracting the weighted rates of growth of capital and labor inputs from the rate of growth of output, all observable variables. This residual came to be called the 'Solow residual' (see Box 15.1). Solow's starting conclusion of the exercise was that:

... while gross output per man hour had doubled over the 1909–1949 span, technical change was responsible for $87^{1/2}$ per cent of this increase, the remaining being due to an increased use of capital. (Solow 1957: 320)

Up to its cooptation by Prescott, the Solow residual was considered as a growth indicator, a trend measurement. But Kydland and Prescott were interested in business fluctuations (i.e., departures from trend). They viewed these departures as stochastic while the trend was considered deterministic. Jointly with Robert Hodrick, Prescott constructed a detrending technique which became known as the HP filter and widely used (Hodrick and Prescott 1986).¹² King and Rebelo summarized it as follows:

In essence this method involves defining cyclical output as a current output less a measure of trend output with trend output being weighted average of past, current and future observations. (King and Rebelo 2000: 932)

This disentangling of the cycle from the trend components of time series needs to be extended to the Solow residual by separating its deterministic component from its stochastic one. It is the latter which, according to Prescott, acts as a signal upon which agents react by possibly changing their optimal allocation. The idea that technology shocks drive business fluctuations can then be captured by the observation that the stochastic component of the Solow residual moves in close relation with GDP fluctuations. As noted by Williamson:

input are defined, in turn, as weighted averages of the rates of growth of individual products and factors. The weights are relative shares of each product in the value of total output and of each factor in the value of total input. If a production function has constant returns to scale and if all marginal rates of substitution are equal to the corresponding price ratios, a change in total factor productivity may be identified with a shift in the production function. Changes in real product and real factor input not accompanied by a change in total factor productivity may be identified with movements along a production function" (Jorgensen and Griliches 1967, p. 250).

¹² Nelson and Plosser (1982) and Campbell and Mankiw (1987) had shown that a simple random walk with drift was a better approximation of the evolution of real GNP in the United States than the view that cycles gravitate around a trend. But this interpretation was set aside in favor of Prescott's.

BOX 15.1 The Solow residual

Assume a constant return to scale aggregate production function with a Hicks neutral shift parameter, A , measuring the shift in the production function at given levels of labor and capital:

$$Y_t = A_t F(K_t, L_t)$$

Totally differentiating the production function gives:

$$\frac{\dot{Y}_t}{Y_t} = \frac{\delta Y}{\delta K} \frac{K_t}{Y_t} \frac{\dot{K}_t}{K_t} + \frac{\delta Y}{\delta L} \frac{L_t}{Y_t} \frac{\dot{L}_t}{L_t} + \frac{\dot{A}_t}{A_t}$$

That is, the growth of output is factored into three components, the rates of growth of capital and of labor weighted by their output elasticity and the growth rate of the efficiency index. Assume that capital and labor are paid their marginal product, that is:

$$\frac{\delta Y}{\delta K} = \frac{r_t}{p_t} \quad \text{and} \quad \frac{\delta Y}{\delta L} = \frac{w_t}{p_t}$$

Thereby the observable income shares of these inputs can be substituted for the unobserved elasticities. The Solow residual ensues:

$$\mathcal{R}_t = \frac{\dot{A}_t}{A_t} = \frac{\dot{Y}_t}{Y_t} - s_t^K \frac{\dot{K}_t}{K_t} - s_t^L \frac{\dot{L}_t}{L_t}$$

where s_t^K and s_t^L are the income shares of the two inputs. Since all the variables in this equation, except the change in technology, have a measurable counterpart, the technology change can be calculated as a residual.

Source: Hulton (2000)

The Solow residual moves very closely with GDP, so that fluctuations in total labor productivity could be an important explanation for why GDP fluctuates. This is the key idea in real business cycle theory. (Williamson 2005: 204).

Figure 15.1 illustrates.

THE EARLY RECEPTION OF SOLOW'S GROWTH-ACCOUNTING EXERCISE

Three early reactions to the works purporting to measure TFP are worth mentioning. The first one, much earlier than Solow's article, comes from Milton Friedman in one of his first public interventions, acting as a discussant of a

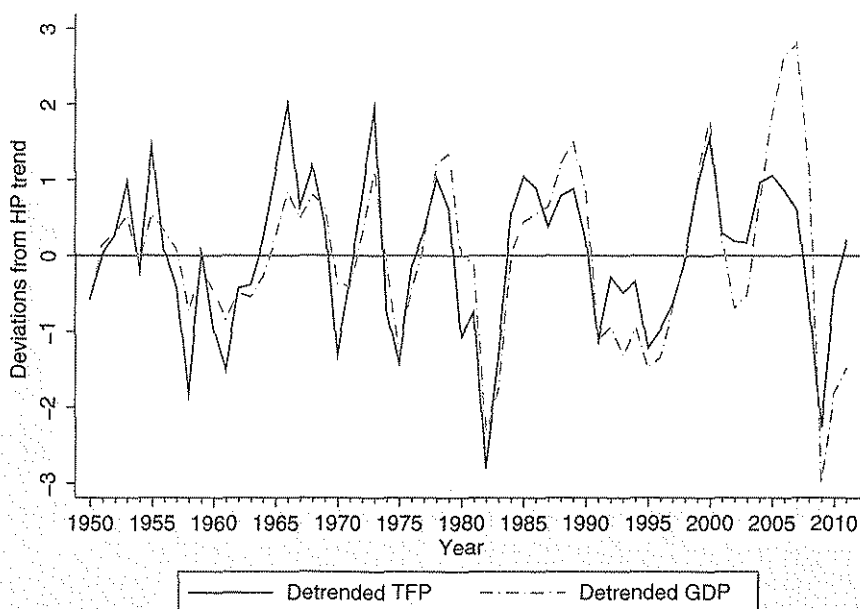


FIGURE 15.1 TFP and real GDP¹³

1938 paper by Copeland and Martin. Friedman's simple point was that the separation between changes along the production function and shifts in the production is artificial. "Technological change affects not only the way in which resources are employed but also the quantity and the character of the resources themselves" (Friedman 1938: 127, note 1). He wrote that to disentangle these two changes, one should be able to calculate the "volume of 'real output' that would have been produced had techniques remained unchanged" (ditto, p. 127), an almost insuperable task. As a result, "solutions offered will be in the nature of assertions rather than of answers susceptible to 'proof,' and the choice among alternative solutions will be almost entirely arbitrary" (ditto, p. 124).¹⁴

A second interesting comment came from Abramovitz. He underlined that TFP must relate to "costless" advances in applied technology managerial efficiency" (Abramovitz 1962: 764). Any change that is costly should be

¹³ Sources: for TFP, Feendra, Inklaar and Timmer (2013), for real gross domestic product, U.S. Bureau of Economic Analysis: *A Guide to the National Income and Product Accounts of the United States* (NIPA) (www.bea.gov/national/pdf/nipaguid.pdf)

¹⁴ Copeland and Martin vividly reacted to Friedman's criticism, writing: "It must . . . be conceded that [such] measurements . . . are certain to be rough under present conditions. However, those who insist on a high degree of precision had best choose some field of activity other than estimating national wealth and income" (Copeland and Martin, 1938, p. 134).

counted as a change along the production function. Thereby, many items that one should spontaneously include as technical change cease to qualify for TFP, for example, R&D, in so far as it uses capital and labor inputs. Thus, TFP growth resembles 'manna from heaven.' Relatedly, Abramovitz noticed that one would like to know more precisely what the Solow residual refers to exactly, a point on which Solow had remained elusive:

'Technical change' is a short-hand expression for *any kind of shift* in the production function. Thus slowdowns, speed-ups, improvement in the education of the labor force, and all sorts of things will appear as 'technical change.' (Solow 1957: 312)¹⁵

In short, it is hard to give substantive content to the idea of a costless increase in the efficiency of labor. All this, Abramovitz concluded, amply justifies calling the Solow residual a 'measure of ignorance.'

The third reaction was a paper by Jorgensen and Griliches (1967), the most systematic criticism of work aimed at measuring TFP, be it Solow's or other papers. Their claim was that most conclusions about the importance of TFP merely resulted from measurement mistakes, themselves following from a failure to fully exploit the underlying economic theory. What was needed was a conceptual change bringing the measured concepts in closer correspondence with the economic theory of production (Jorgensen and Griliches 1967: 275). At the end of a painstaking re-measurement operation, Jorgensen and Griliches reached the conclusion that growth in total output is largely explained by growth in total input (movements along the production function). They claimed that 96.7 percent of the output growth was due to changes in the rate of growth of input, the part played by TFP dramatically being reduced to the remainder.¹⁶

To conclude, TFP measurement is a murky subject for the above reasons and additional ones.¹⁷ Growth theorists were well aware of the tenuousness of their calculations and the need to take their results with caution. Hence their reaction of amazement to Kydland and Prescott's take-over bid of the TFP notion, well-captured by King and Rebelo when writing the following:

Growth accountants were horrified when they saw the 'measure of their ignorance' recast as the main impulse to the business cycle. (King and Rebelo 2000: 963)

Nonetheless, this is what happened. Prescott hardly bothered with the above restrictions. The opportunity was too good to be left aside. Quite boldly, he

¹⁵ In Note 8 of his 1957 article, Solow acknowledged a remark made by T. Schultz pointing out that what may appear as a shift in the production function is not one. "I owe to Professor T. W. Schultz a heightened awareness that a lot of what appears as shifts in the production function must represent improvements in the quality of the labor input, and therefore a result of real capital formation of an important kind" (Solow 1957, p. 317).

¹⁶ Jorgensen and Griliches's provocative conclusion did not remain unanswered; e.g., Denison (1962, 1972) took it at heart to rescue Solow's result.

¹⁷ Cf. Hulten (2000).

decided that the variations in the Solow residual could be used as a signal triggering changes in agents' behavior playing a similar role of impulse as the money signals in Lucas's model.

THE RBC BASELINE MODEL

A complex piece, Kydland and Prescott's paper soon evolved into a simpler baseline model. It can be described as follows.¹⁸

Preferences

There exists a continuum of identical infinitely-lived households whose preferences are defined over stochastic sequences of consumption and leisure. Their expected utility is defined as:

$$E_0 \sum_{t=0}^{\infty} \beta^t u(C_t, L_t) \quad \text{with } \beta < 1$$

where β is the discount factor, C_t consumption, and L_t leisure.

Instantaneous utility $u(C_t, L_t)$ is restricted to a CES function, supposedly on the grounds that key growth observations allow using it, but surely also as a matter of convenience:

$$u(C_t, L_t) = \frac{1}{1-\theta} \{ [Cv(L)]^{1-\theta} - 1 \} \quad \text{for } 0 < \theta < 1 \quad \text{and}$$

$$u(C_t, L_t) = \ln C_t + v(L_t) \quad \text{for } \theta = 1,$$

where θ is the constant relative risk aversion coefficient. Instantaneous utility is strictly concave and twice continuously differentiable. $v(L)$ is increasing and concave. These functional forms imply (a) the invariance of the intertemporal elasticity of substitution in consumption ($\sigma = 1/\theta$) and (b) the cancellation of income and substitution effects on the labor supply.

Technology

Agents produce a single output with a constant return production function. Omitting its deterministic labor augmenting technology component, it is defined as:

$$Y_t = A_t F(K_t, N_t),$$

where K_t is capital and N_t labor, and A_t a stochastic technology shock evolving along the following law of motion:

$$A_{t+1} = \rho A_t + \varepsilon_t \quad \text{with } -1 < \rho < 1$$

¹⁸ My presentation is drawn from King, Plosser, and Rebelo (1988).

The ε_t s are uncorrelated innovations (or disturbances) with zero mean and a given standard deviation.

The production function is restricted to the Cobb-Douglas form:

$$Y_t = A_t K_t^{1-\alpha} N_t^\alpha$$

The law of motion of the capital stock is given by:

$$K_{t+1} = (1-\delta)K_t + I_t,$$

where δ is a positive constant capital depreciation rate and I new investment equal to saving.

Resource constraints

Agents face two binding resource constraints. First, time devoted to leisure and work must equal the time endowment, normalized to 1:

$$N_t + L_t = 1$$

Second, consumption and investment cannot exceed output:

$$C_t + I_t = Y_t$$

The agents' planning problem consists in maximizing the intertemporal utility function subject to the resource constraint and the laws of motion of technology and capital. Since all agents are identical, there is no need to make individual plans compatible.

In this model, agents' individual equilibrium consists in an equilibrium time path, resulting from a twofold optimizing process, the choice between leisure and work being the first margin, and that between consumption and saving/investment the second one. The first-order conditions of their decision are summarized in a Euler equation. In the simplest case of a one-commodity, two-period and finite horizon economy, it states that:

$$u'(C_t) = \beta u'(C_{t+1}) \cdot f'(K_{t+1})$$

The Euler equation comprises three components. The first one, on the left of the equality sign, is the loss in utility at t associated with the additional saving. The second one is the gain in utility per unit of increase in c_{t+1} in the next period. The third one is the return of the invested unit, indicating the number of units by which c_{t+1} increases. The first term must be equal to the product of the other two. In short, the marginal cost of saving must equal the marginal benefit of saving. This condition defines a multiplicity of feasible time paths. Only one of these satisfies the transversality condition. It constitutes the equilibrium path.

The existence of shocks leads agents to modify their equilibrium path. A positive technology shock increases both current and future output. It generates an increase in the productivity of labor, and hence in the real wage: leisure

becomes more expensive, and so agents will decide to spend more hours working. In other words, the shock shifts the labor demand curve along a stable labor supply curve. This is what explains the model economy's tight correlation between hours and productivity noticed earlier. The same effect is true for the real interest rate. Savings and investment also increase, implying a higher future capital stock.

No economy can be more rudimentary than this one. It contains no heterogeneity, no externalities, no government sector, and no monetary variables (hence no central bank). It also assumes complete markets. Imperfections are absent. All these restrictions ensure the existence of a unique Pareto-optimal equilibrium path.

The above solution pertains to the planning economy. In the latter, prices only have a shadow existence – it cannot be said, as one does about the market economy model, that prices drive the system. However, the second welfare theorem allows extending the allocation of the planning economy to the competitive model economy. The equilibrium price vector of the competitive economy supporting the planning economy can easily be identified. It must be such that the quantity components of the allocation it imposes are identical to the equilibrium quantities of the planning economy. That is, the equilibrium real wage rate must be equal to the marginal productivity of labor, and the real rental price of capital to the marginal productivity of capital. In other words, were these shadow prices transposed to a market setup, they would induce economic agents to supply and demand the optimal quantities that emerge in the planning context.

The move between the planning economy and the competitive economy goes two ways. On the one hand, as just stated, the knowledge of the equilibrium quantities of the planning system allows that of the equilibrium prices in the competitive economy. On the other hand, the restrictions on the utility and the production functions are grounded in the fact that they comply with key growth observations. For example, with respect to the utility function, “the leisure per capita has shown virtually no secular trend while, again, the real wage has increased steadily” (Prescott [1986a] 1994: 274). Once these restrictions are made, referring now to the Cobb Douglas production function, marginal productivity is proportional to average productivity. As a result, when it comes to assigning values to the parameters of the planning system (i.e., in the calibration stage), these values can be drawn from the average real wage and rental price of capital obtained from real-world statistics. As for technology shocks, their magnitude are drawn from the calculation of the Solow residual in the real-world economy under study.

THE STORY BEHIND THE MODEL: A COLOSSAL ‘AS IF’

As seen in Chapter 4, Friedman claimed that no objections should be raised against non-realistic assumptions. Indeed, what matters for the validity of a theory is whether its predictions are verified rather than the realism of the assumptions upon which it is based. In this section, I argue that Kydland and

Prescott have followed Friedman's principles to an extent well beyond what Friedman himself had considered. To make my point, I need to delve into the story behind the baseline RBC model.

The standard answer about the kind of economy the baseline RBC model studies is that it pertains both to a command economy and a competitive economy. On reflection, this answer is less simple than it seems. In a command economy, a central authority dictates the quantity of goods and services produced and their allocation across the agents inhabiting the economy. Characterizing the RBC baseline model in this way is odd. The problem solved consists in finding the decision rules that single agent living in autarky should adopt. In this context, there is no need for a central planner since the central planner and the agent supposedly under her command are the same person.¹⁹ Similar skepticism applies to the competitive economy interpretation. To make the point, look first at the following observation made by David Romer, who is not particularly fond of RBC modeling, about the Ramsey model:

The Ramsey model is the natural Walrasian baseline model of the aggregate economy: the model excludes not only market imperfections but also all issues raised by heterogeneity among households. (Romer 2006: 178)

This statement is incorrect. The initial model in Walras's *Elements* deals with an economy comprising two goods and a great number of different agents. To Walras, this heterogeneity of agents and goods was co-substantial to the notion of an economy.²⁰

We need thus to fall back on another story. One candidate is the time-honored story of poor Robinson Crusoe who was cast ashore on an island and had to manage living off of what he produced. The reference to Crusoe is telling because of the place this character occupies in our culture. Unfortunately, the model requires a story with a large number of isolated agents, thus there would have to be many shipwrecks and many islands. Therefore, a better metaphor would be that the model is about a large number of identical hermits, who have all decided to live in confinement, one per island, and have prepared

¹⁹ Therefore Plosser lapses when making the following remark: "Any attempt by a social planner to force Crusoe to choose any allocation other than the ones indicated [the equilibrium decisions MDV], such as working more than he currently chooses, or saving more than he currently chooses, are likely to be welfare reducing" (Plosser 1989: 56).

²⁰ Shell and Cass wrote a joint paper in which they criticized the Cass model, Kydland and Prescott's source of inspiration, for its lack of place for any diversity of households and commodities. They write that its flaw is to depart from "... perhaps the oldest tradition in economics: there is a clear distinction between economic agents' objective and constraints – and hence the mainspring of their individual behavior – and the economic system's coherent resolution of their joint interaction" (Cass and Shell 1978: 253). "We firmly believe that a satisfactory general theory must, at a minimum, encompass some diversity among households as well as some variety among commodities" (Cass and Shell 1978: 256). They concluded that the line to be taken is adopting the overlapping generations modeling strategy.

themselves for this. They live off of what they produce without trading. The problem they need to solve is to reach an optimal consumption/saving path. Since they are identical, they will reach the same equilibrium path. Hermits are assumed to be rational, to have a good grasp of the technology constraint they face, and to have the ability to make any calculation needed to define decision rules. Over time, the story goes, they experience a steady growth in TFP, yet the latter is affected by exogenous stochastic disturbances. More importantly, they must be able to quantify both the deterministic and the stochastic component of the evolution of TFP. These shocks must be identifiable, to the effect that they can act as signals upon which hermits may base themselves to decide on their equilibrium path. Wondering what the nature of the shock might be, one may imagine that they are meteorological. At times, the climatic conditions exert a negative impact on production and at other times a positive one. In all these juxtaposed 'one-person economies,' there are no prices. However, it is possible to describe these economies through the prism of prices by saying that every hermit shoulders two roles, household and firm, and is 'trading' with herself at prices calculated from their quantity decisions.

Thus, to return to my initial question, about the type of economy in the RBC baseline model, the answer is that it deals with hermit *economies* (a myriad of one-person disconnected economies) rather than with a single economy.²¹ As they are all similar, only one of them needs to be studied. In my opinion, this is the most plausible story underpinning the RBC baseline model. No story can be more far-fetched, but it must be regarded as a parable, which is only defensible if interesting conclusions can be drawn from it.²² While it more or less holds up in term of consistency, in terms of relevance to the theoretical *quaesitum*, its value is nil. Nobody expressed this better than Solow. To him, such a starting point is "far-fetched," "hard to swallow," and "unconvincing" (Solow 1988: 310):

One important tendency in contemporary macroeconomic theory evades this problem in an elegant but (to me) ultimately implausible way. The idea is to imagine that the economy is populated by a single immortal consumer, or a number of identical immortal consumers. . . . Thus any kind of market failure is ruled out from the beginning, by assumption. There are no strategic complementarities, no coordination failures, no prisoner's dilemma. The end result is a construction in which the whole economy is assumed to be solving a Ramsey optimal-growth problem through time, disturbed only by stationary stochastic shocks to tastes and technology. To these the economy adapts optimally.

²¹ Following Walras, I consider the notion of a 'one-person economy' an oxymoron, but I will leave this aside.

²² RBC macroeconomists have devoted little time to reflecting on it. Otherwise, one would not stumble on statements like the following one, drawn from Woodford's otherwise excellent account of the history of macroeconomics: "The 'equilibrium business cycle models' of Lucas had really only been parables; they could not be regarded as literal descriptions of an economy, even allowing for the sort of idealization that all models of reality have. . . . Real business cycle models are instead quantitative models, that are intended to be taken seriously as literal depictions of the economy, even if many details are abstracted from" (Woodford 1999: 26).

Inseparable from this habit of thought is the automatic presumption that observed paths are equilibrium paths. So we are asked to regard the construction I have just described as a model of the actual capitalist world. What we used to call business cycles – or at least booms and recessions – are now to be interpreted as optimal blips in optimal paths in response to random fluctuations in productivity and the desire for leisure. I find none of this convincing. The markets for goods and for labor look to me like imperfect pieces of social machinery with important institutional peculiarities. They do not seem to behave at all like transparent and frictionless mechanisms for converting the consumption and leisure desires of households into production and employment decisions. I cannot imagine shock to taste technology large enough on a quarterly or annual time scale to be responsible for the ups and downs of the business cycle. (Solow 1988: 310–311)

For their part, RBC economists did not feel concerned by the kind of criticism voiced by Solow. For example, Plosser had no qualms exposing the RBC methodology on the basis of the Robinson Crusoe analogy. According to him, what makes it legitimate is that the totally non-realistic nature of the model is counterweighted by its replication ability:

If the measured technological shocks are poor estimates (that is, if they are confounded by other factors such as 'demand' shocks, preference shocks or change in government policies, and so on) then feeding these values into our real business cycle model should result in poor predictions for the behavior of consumption, investment, hours worked, wages and output. (Plosser 1989: 63)

This last quotation brings me back to what I wrote at the beginning of this section. The conclusion of my investigation about the story behind the model is that Kydland and Prescott have engaged in the most colossal 'as if' procedure *à la* Friedman that can be conceived of. When using his 'as if' vindication, Friedman was referring to particular assumptions. But he still wanted the stories behind his models to be in direct relation to his object of study. For their part, Kydland and Prescott have no qualms taking a Robinson Crusoe or a hermit economy as a proxy for the U.S. economy on the grounds of Friedman's principle.

It is worth examining more in depth how technology shocks are dealt with in the baseline RBC model. As far as the parable is concerned, the story must be that the hermit has the ability to ascertain climatic disturbances in a quantitative way. As far as reality is concerned, the matter is more complicated because it is hard to identify specific technology factors that could be the cause of the recessions and recoveries observed during the post-World War II era. Prescott agreed with this, stating that fluctuations are "the result of the sum of many random causes," such as new knowledge and changes in the legal and regulatory system (The Region 1996: 6). He further admitted that these causes cannot be observed individually. How then can they act as a signal? Salvation comes with the Solow residual. It supposedly sums up all these shocks. It must also be assumed that agents have the ability to mentally reconstruct its magnitude. For sure, Kydland and Prescott cannot be blamed for a lack of methodological audacity. The bottom line is that, in their vision, what matters is not how relevant the story behind the model is to the theoretical *explanandum*, 'real-world economies' evolving in real time, but the result of the simulation of the model economy.

PRESCOTT ON THE CONTRAST BETWEEN KEYNESIAN AND REAL BUSINESS CYCLE MACROECONOMICS

According to Prescott, RBC macroeconomics differed from Keynesian macroeconomics on two main scores. First, it replaced a 'system of equations' approach with a general equilibrium approach; and second, it substituted calibration for econometric testing. After having expounded Prescott's argumentation on these matters, I will show that it is underpinned by the belief that macroeconomics has reached a stage where the validity of the existing paradigm (i.e., the relevance of the neoclassical growth model) is deemed to be beyond debate.

The system of equations versus the general equilibrium approach

The 'system of equations' expression (Kydland and Prescott 1991) refers to Keynesian macroeconometrics models *à la* Klein-Golberger and Cowles Commission. In these models, the economy is subdivided into separate sectors of activity. In the beginning, these sectors were limited in number, each of them being accounted for by one or a few equations: the consumption sector, the investment sector, the monetary sector, the employment sector, the government sector, and the international sector. The parameters of the models were selected using statistical estimation tools. Progress consisted in dynamizing a few of these, but also and mainly in adding new equations to the model. These additions were presumed to make the models more descriptively accurate. They also made them bigger and bigger to the point where it became necessary to outsource the study of sets of equations to different research centers.

This approach, Kydland and Prescott argued, is twice faulty. First, the parameters in its models are not 'deeply structural' as argued by Lucas in his Critique. Second, although they deal with the economy as a whole, they are nonetheless not general-equilibrium models since each equation is solved separately (Kydland and Prescott 1991: 163). To abide by the Lucas Critique, reasoning in general-equilibrium terms becomes a *sine qua non*. This has an implication on the size of models. Unlike Klein and his followers, who believed that the bigger the model the better, Kydland and Prescott's motto was, rather, 'the simpler the model, the better.'

Econometric testing versus calibration

Kydland and Prescott were aware that standard econometric tests would reject their model.²³ Not pulling their punches, they argued that the problem did not lie with the model but with econometrics, and that it was necessary to resort to a new methodology, calibration (Kydland and Prescott 1982: 1360). This

²³ "We choose not to test our model versus the less restrictive vector autoregressive model. This most likely would have resulted in the model being rejected, given the measurement problems and the abstract nature of the model" (Kydland and Prescott 1982: 1360).

method had already been used in the physical sciences as well as in computational general equilibrium theory. But they gave it a new impetus, to say the least. Calibration consists in choosing values for the parameters of the model economy in accordance with observed long-run regularities, and by drawing from existing empirical studies, independent research, and national accounting data. Whenever such sources are unavailable, these values should be determined in accordance with economic theory. The more the parameters can be evaluated in the first way, the better it is. Clearly, econometric estimation and calibration are of two different ilks. The following quotation aptly points out their basic difference:

The dominance of theory in the choice of models lies at the heart of the difference between estimators and calibrators. To throw the difference into high relief, one can think of estimators pursuing a competitive strategy and calibrators pursuing an adaptive strategy. Under the competitive strategy, theory proposes, estimation and testing dispose. *In fine*, alternative theories compete with one another for the support of the data. The adaptive strategy begins with an unrealistic model, in the sense of one that is an idealized and simplified product of the core theory. It sees how much mileage it can get out of that model. Only then does it add any complicating and more realistic features. Unlike the competitive strategy, the aim is never to test and possibly reject the core theory, but to construct models that reproduce the economy more and more closely within the strict limits of the basic theory. (Hoover 1995: 29)

RBC economists want their work to be assessed on the basis of whether their models generate times series that replicate real-world ones. Calibration is deemed to be the tool to be used to this end. As a result, the assessment of RBC modeling hinges on whether calibration is a commendable method. This in turn depends on whether enough non-questionable independent data are available to be plugged into the model economy. This is not always the case. In their 1982 paper, Kydland and Prescott found themselves with six 'free' parameters still in need of a quantitative value. Intertemporal substitution and the technology shock were the most prominent. Different values could be calculated for their combination. Kydland and Prescott chose those that brought about a close correspondence between the moments predicted by the model and the moments of the real-world series. A less sympathetic way of telling the same story is Hoover's: "the free parameters of the model are set to force the model to match the variance of GNP without any attempt to find the value of empirical analogues to them" (Hoover 1995: 25). Another trait of calibration is that it makes sense only if the theory is well established.²⁴ For his part, Prescott is hardly troubled by such a requirement.

²⁴ As also noticed by Hoover, "Calibration does not provide a method that could in principle decide between fundamentally different business-cycle models" (Hoover 1995: 30).

THE UNDERLYING METHODOLOGICAL STANDPOINT: THERE EXISTS AN ESTABLISHED THEORY

Klein constructed his models with the purpose of assessing the validity of Keynes's theory against the classical one (Klein 1955: 280). He wanted to put these theories to the test with the purpose of assessing which one was the best, an endeavor that had a flavor of falsification about it.²⁵ This idea that empirical work might sort out the respective qualities and flaws of rival theories, which as seen was also at the heart of Friedman's methodology, is alien to Prescott. To him, "macroeconomics has progressed beyond the stage of searching for a theory to deriving the implications of theory" (Prescott 2006: 203–204), and the neoclassical growth model is "established theory" (Prescott and Candler 2008: 3):

I view the growth model as a paradigm for macro analysis – analogous to the supply and demand construct of price theory. (Prescott [1986a] 1994: 266)

This analogy reflects well what Prescott must have had in mind. Similarly to the reasoning about supply and demand in microeconomics, empirical work in macroeconomics serves the purpose of assessing the validity not of the growth model itself but of questions that are asked using this model. If the question receives a poor answer, this may entail revising the theory but certainly not dismissing it.

Moreover, according to Prescott, it may well be the case that when the model and the data fail to fit, the cause may lie with measurement rather than with theory, a blunt statement which is in the title of Prescott's "Theory Ahead of Measurement" paper, already referred to earlier. In its conclusion, Prescott summarizes his standpoint as follows:

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 the 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 the key economic series. Even with better measurement there will likely be significant deviations from theory which can direct subsequent theoretical research. The feedback between theory and measurement is the way mature, quantitative sciences advance. (Prescott [1986a] 1994: 286)

STABILIZING THE LUCASIAN REVOLUTION

The notion of scientific revolution has often been overused. But if there is one episode in the history of macroeconomics that deserves the label, it is the transformation initiated by Lucas and brought to completion by Kydland and Prescott – the movement from the Keynesian to the Lucasian program. Prescott

²⁵ It is true that Klein had a somewhat biased way of engaging in this exercise by automatically putting the economy in a Keynesian regime with excess supply in the labor and the good markets. Cf. De Vroey and Malgrange (2012).

did not shy away from using it in the opening sentence of his Nobel lecture: "What I am going to describe to you is a revolution in macroeconomics, a transformation in methodology that has reshaped how we conduct our science" (Prescott 2006: 203).

It is clear that this revolution was triggered by Lucas. However, one may wonder whether it would have succeeded had Kydland and Prescott not taken up the baton by finding a way to implement Lucas's injunction. In his Nobel Lecture, Prescott gave a ringing endorsement of Lucas's work, while making it clear exactly where Kydland and he had stepped in to play their part in the revolution:

Lucas's work is not *quantitative* dynamic general equilibrium, and only nine years later did Fin and I figured out how to quantitatively derive the implications of theory and measurement for business cycle fluctuations using the full discipline of dynamic stochastic general equilibrium theory and national accounts statistics. (Prescott 2006: 231–232)

While models *à la* Lucas could mobilize only a tiny fraction of macroeconomists, Kydland and Prescott's applied research program provided the bread and butter to legions of macroeconomists (both top-notch and more run-of-the-mill) for more than a decade. This is what it takes to have a successful revolution. Lucas himself contributed little to this enterprise. His criticism of the standard econometric practice and his dismissal of the policy use of the Phillips curve alone might have been insufficient to overthrow the Keynesian paradigm. Therefore, I like to regard the relationship between Lucas, and Kydland and Prescott, as mirroring that between Keynes and his immediate successors such as Hicks, Klein, and Modigliani. What would have happened to *The General Theory* if it had not been transposed into the IS-LM model and if Klein had not extended it into an econometric framework? The same question can be asked about Lucas's work. Although no answer can be provided to such questions, they reminds us that in a field like economics there is no single compelling way in which theory evolves.

Real Business Cycle Modeling: Critical Reactions and Further Developments

I begin this chapter with a presentation of three early reactions to RBC modeling, by Summers, McCallum, and Mankiw. An outstanding feature of RBC theory has been its cumulative development ability, finding ways to address some of its initial shortcomings. Therefore, I will set forth some examples of such achievements. As the material is abundant, I will proceed selectively, retaining only a few papers and themes. Those I have picked are (a) indivisible labor and unemployment, (b) the replacement of individual agents with households, (c) alternative types of shocks, and (d) the possibility of introducing Keynesian ideas into RBC modeling.¹ Next, I will turn to two further developments that could be put under the tutelage of the god Janus as they are somewhat critical while also opening new research paths: first, criticisms of the empirical success of RBC modeling and second, the dismissal of Prescott's claim that the Solow residual acts as a good indicator of technology shocks. To finish the chapter I document the general opinion within the macroeconomics profession about RBC modeling, namely that it was a significant methodological breakthrough. My own appraisal is postponed until the next chapter.

EARLY CRITICAL REACTIONS

Summers (1986)

Prescott's "Theory Ahead of Measurement" ([1986] 1994) article provoked a stern rebuttal by Summers ([1986] 1994). This witty paper is interesting for my purpose for two reasons. First, it captured the general feeling of skepticism with

¹ For lack of time, I have been unable to address the cutting-edge issue of heterogeneity. Two papers that blazed the trail for this research track are Rios-Rull (1995) and Krusel and Smith (1998). Heatcote and Storelesletten (2009) and Guvenen (2011) are interesting surveys.

which the Kydland-Prescott project was met. Second, Summers was able to put his finger on the points which have turned out to be the weak spots of RBC modeling.

Summers's paper started with the general methodological remark that predicting is not explaining – citing Ptolemaic astronomy and Lamarckian biology as examples. “Many theories can approximately mimic any set of facts; that one theory can does not mean that it is even close to right” (Summers ([1986] 1994: 290). For all its generality, this remark is perfectly to the point.

Summers continued by presenting three fundamental objections. The first one concerned Prescott's parametrization:

Prescott's growth's model is not an inconceivable representation of reality. But to claim that its parameters are securely tied down by growth and micro observations seem to me a gross overstatement. The image of a big loose tent flapping in the wind comes to mind. (Summers [1986] 1994: 291)

Summer's second fundamental objection related to technological shocks. To him, there is no independent evidence of their existence. He wondered which technology shocks may explain downturns like the 1982 recession, while other more plausible explanations are easy to support.

Finally, Summers raised the question of how a theory of business fluctuations that gives no role to market failures can have any explanatory power. Referring to the Great Depression, he argued that attempts to explain it in terms of intertemporal substitution and productivity shocks stretch credibility; other, more important, factors need to be considered. But then these forces should also be at work when studying fluctuations of a smaller amplitude:

It seems clear that a central aspect of depressions and probably economic fluctuations more generally, is a breakdown of the exchange mechanism. . . . This is something that no model, no matter how elaborate, of a long-lived Robinson Crusoe dealing with his changing world, is going to confront. A model that embodies exchanges is a minimum prerequisite for a serious theory of economic downturns. (Summers [1986] 1994: 294)

McCallum (1989)

McCallum, an eminent monetary theorist, wrote several papers appraising the RBC approach. Here, I discuss an early one, entitled “Real Business Cycle Model” (1989). Although McCallum's paper ended with praising Kydland and Prescott's methodological breakthrough, the bulk of the paper consisted in bringing out questions and shortcomings.² To begin with, like Summers,

² This is what McCallum has to say in praise of Kydland and Prescott's work: “It would seem to be virtually indisputable that the RBC literature has provided a substantial number of innovative and constructive technical developments that will be of lasting benefit in macroeconomic analysis. In particular, the example of Kydland and Prescott (1982) has suggested one route toward the goal

McCallum expressed misgivings about Kydland and Prescott's central claim that business fluctuations are triggered by technology shocks. First of all, he regretted that RBC modelers had adopted the hardly justified high autoregressive coefficient of technology shocks (McCallum 1989: 19). What is at stake is whether it is right to attribute a crucial role in the explanation of growth to the Solow residual (a point to which I will return below). Quite normally for a monetary theorist, McCallum was also highly critical of Kydland and Prescott's omission of money. He deplored King and Plosser's (1984) reverse-causation argument according to which the correlation between monetary and real variables reflect the response of the monetary system to fluctuations caused by technology shocks. As for the empirical case against the real effects of monetary shocks, McCallum found it unconvincing, as it is based on the wrong assumption that the central banks' main policy instrument is the money supply. On another score, Kydland and Prescott's Pareto optimality conclusion hardly impressed McCallum, as it follows from excluding externalities, government and monetary variables from their model. Finally, McCallum was skeptical about the much-heralded integration between fluctuations and growth, finding it "misplaced or excessive" (1989: 34). In his view, Kydland and Prescott may well have emphasized its desirability, but their work failed to achieve it.

Mankiw (1989)

While McCallum wrote his paper for the cognoscenti, Mankiw's paper was addressed to the broader audience of the readers of the *Journal of Economic Perspectives*. But the themes broached were similar. Mankiw argued that observable changes in productivity should be attributed to capacity utilization rather than to technology shocks. Similarly to Summers, he found the claim that declining rates of output can be attributed to technological regress hard to swallow. "If society suffered some important adverse technological shock, we would be aware of it" (Mankiw 1989: 86). Mankiw was even more emphatic than McCallum about the omission of money, indicting RBC theory for its embrace of the classical dichotomy. Kydland and Prescott liked to say that when they started investigating the role of money, they discovered, to their surprise, that it had no explanatory power: hence they discarded it. For his part, Mankiw contended that they should have drawn the opposite conclusion. If a given model cannot capture a well-observed outcome, it should be presumed

of a dynamic equilibrium model with optimizing agents that can be used for quantitative macroeconomic analysis. The type of model employed does not, it should be emphasized, require a belief that the workings of the economy are socially optimal. In addition, the literature has spurred interest in several purely methodological topics, including alternative methods for the investigation of the dynamic properties of nonlinear general equilibrium and for the detrending of time series data" (McCallum 1989: 40-41).

that it is flawed instead of declaring that the effect does not exist! Turning to efficiency, Kydland and Prescott's proposition that fluctuations are nothing more than the result of economic agents' efficient reactions to technology surprises was shocking to Mankiw. To him, "it seems undeniable that the level of welfare is lower in a recession than in the boom that preceded it" (Mankiw 1989: 81).

All in all, Mankiw's opposition to RBC modeling was stronger than McCallum's. Indeed, he views it as potentially dangerous:

In my view, real business cycle theory does not provide an empirically plausible explanation of economic fluctuations. Both its reliance on large technological disturbances as the primary source of economic fluctuations and its reliance on the inter-temporal substitution of leisure to explain changes in employment are fundamental weaknesses. Moreover, to the extent that it trivializes the social cost of observed fluctuations, real business cycle theory is potentially dangerous. The danger is that those who advise policy-makers might attempt to use it to evaluate the effects of alternative macroeconomic policies or to conclude that macroeconomic policies are unnecessary. (1989: 79)

Behind Mankiw's opposition looms his adhesion to the neoclassical synthesis viewpoint, the gist of which is to reject an any all-out dominance of Walrasian general equilibrium theory. The view that fluctuations are efficient is the direct result of giving this theory free rein over the field of macroeconomics. And with such a theory, involuntary unemployment slips through the net:

Real business cycle theory pushes the Walrasian model farther than it has been pushed before. (Mankiw 1989: 81)

The Keynesian school believes that understanding economic fluctuations requires not just studying the intricacies of general equilibrium, but also appreciating the possibility of market failure on a grand scale. (Mankiw 1989: 79)

THE HANSEN-ROGERSON INDIVISIBLE LABOR MODEL³

In a 1983 Discussion Paper, Richard Rogerson demonstrated that an economy comprising non-convexities, for example, fixed cost, could gain back its otherwise lost welfare properties if a lottery was introduced in the consumption set.⁴ Building on Rogerson's paper, Gary Hansen modified the RBC baseline model in a paper that can be hailed for having killed two birds with one stone (Hansen, G. 1985). First, micro studies using panel data on individual working hours indicated that existing intertemporal substitution was far too low to explain the large aggregate fluctuations in total hours worked.⁵ Hansen offered a counter-argument to this criticism. The second contribution of Hansen's model is to have made progress on the wage-employment variability puzzle.

³ Cf. Hansen, G. and Wright (1992).

⁴ It was published as a journal article only in 1988 (Rogerson 1988).

⁵ Cf. Alltonji and Ashenfelter (1980), Alltonji (1982), Ashenfelter (1984), Mankiw, Rotenberg, and Summers (1985).

Hansen attributed the shortcomings of the Kydland-Prescott model to the fact that it considered only the intensive margin (hours worked per capita). His intuition was that a better result would be obtained by focusing on the extensive margin (the employment level). In his model, agents face a binary choice: either they work a fixed number of hours or they do not work at all. Four additional assumptions are made. First, it is assumed that the agents' optimizing choice bears on the probability of working, rather than on the number of hours worked. A second assumption is that the distribution of agents across working and not working occurs through a lottery. Third, an additional commodity is introduced into the economy, an insurance contract between firms and households. It stipulates that the firm will hire a given agent with a probability π_t , while providing a substitution income otherwise. As a result, all agents will reach the same utility irrespective of whether they are employed or not. Fourth, agents' utility function is separable in consumption and labor, instantaneous utility being written:

$$u(C, L) = \log(C) + A \log(L)$$

The existence of indivisible labor is expressed in the equation $H_t = \pi_t \hat{h}$, where H_t is the number of hours worked per capita, π_t is the probability of working in equilibrium, an endogenous variable depending on the sign and strength of the technology shock, and \hat{h} the fixed length of the working day. None of the agents exert labor in quantity H_t as people either do not work at all or work \hat{h} hours. However, H_t can be ascribed to the representative agent. Against this background, Hansen now compares two economies differing only by the fact that one has indivisible labor and the other not. In the indivisible case, the expected utility is:

$$Eu(C_t, H_t) = \log(C_t) + \pi_t A \log(1 - \hat{h}) = \log(C_t) - BH_t$$

$$\hat{h} \in [0, 1)$$

and where $B = -A \log\left(\frac{1 - \hat{h}}{\hat{h}}\right) > 0$

In the divisible labor case, the expected utility relates to the representative agent's utility, \tilde{u} , which is linear in hours worked and is expressed as:

$$\tilde{u}(C_t, H_t) = \log(C_t) - BH_t.$$

As a result, the indivisible labor model can be solved as if it was a divisible labor model. Thus, the observation that individual agents have a low intertemporal elasticity hardly matters theoretically. What counts is that the aggregate substitution elasticity, which is independent from individual utilities, is large.

Hansen also contended that his model, when simulated, improves upon the shortcomings of the standard RBC model as far as volatility of output is concerned. Moreover, a main weakness of the Kydland-Prescott model was the

wage-employment variability puzzle. What is at stake is the amount of variability in hours worked relative to the variability in productivity, deemed to be the proxy for real wages (i.e., σ_H / σ_W). For the U.S. data (1955.3–1984.1), this ratio is 1.4. Kydland and Prescott's model was unable to replicate it satisfactorily, obtaining a result close to one. Hansen's model has the opposite problem: the ratio it implies is too large (2.7). However, he claimed that this result is easily explained, as it is due to the model's exclusive attention to the extensive margin.

Prescott promptly took advantage of Hansen's result, declaring that the case was closed with the objection rejected. As he declared in his Nobel Prize lecture, "aggregate observations are consistent with individual observations with respect to people's willingness to intertemporally substitute leisure" (Prescott 2006: 221).

However, as is often the case, such martial declarations convinced only those who were so inclined. For their part, econometricians were not. In their *Handbook of Macroeconomics* contribution, Martin Browning, Lars Peter Hansen, L-P. and James Heckman took the Hansen-Rogerson employment allocation mechanism as the target of their criticism, arguing that it "strains credibility and is at odds with the micro evidence on individual employment histories" (Browning, Hansen, and Heckman 1999: 602). These micro studies, they argued, clearly indicate a high persistence of either the employed or the unemployed status. Macroeconomists also remained unconvinced. One example is Robert Hall (1988a). RBC models rest on the assumption that households respond to changes in the expected rate of interest by changing their consumption path, deferring consumption when expecting a higher real interest rate. Thus the reaction of consumption to changes in this rate can be regarded as a measure of intertemporal substitution. Engaging in different types of econometric measurement on U.S. data, Hall claimed that most of the measures considered indicated that this reaction is weak. He then concluded that there was a low value of intertemporal substitution.

In the mid-1990s, a new development in the integration of unemployment into RBC modeling took place due to Andolfatto (1996) and Monika Merz (1995) working in parallel.⁶ Like Diamond's, these papers aimed at introducing search unemployment in a dynamic general equilibrium model. However, the context and the motivation were radically different. Although Diamond was striving to support a Keynesian claim and to create an alternative to Lucas, Andolfatto's and Merz's basic aim was to enrich the RBC program. They both perceived that a version of the random search and bargaining model existing in the search literature comprised specific features, in particular efficiency, enabling its integration in RBC modeling. Using that specific version, they were indeed able to operate a merger between the two research lines and, hence, to disclaim

⁶ Cf. Danthine and De Vroey (2014).

the criticism that RBC modeling was unable to integrate unemployment. As in the Hansen-Rogerson model, both Andalfatto and Merz assumed the presence of an insurance scheme, this time within the extended household. Its effect is that the unemployed enjoy the same utility as the employed. So, there was an integration, but it was contrived. It certainly was poles apart from what motivated both Diamond and labor economists to tread the search trail, namely, to bring out the existence of inefficiencies in the working of labor markets.

HOUSEHOLD PRODUCTION: BENHABIB, ROGERSON, AND WRIGHTS' MODEL

In a 1991 paper, Jess Benhabib, Rogerson, and Randall Wright endeavored to integrate home production in RBC modeling, "an important missing element in existing models of the aggregate economy." The underlying motivation was that the home sector is important empirically: "an average married couple spend 33 percent of their discretionary time working for paid compensation and 28 percent working in the home" (Benhabib, Rogerson, and Wright 1991: 1167). Evidence also suggests that substitutability between the home and market sector is important.

Their model aims at capturing these features. Agents have three rather than two alternative ways of allocating time, leisure, hours of work in the market sector and hours of work in the home sector. Leisure is defined as:

$$L_t = 1 - H_{Mt} - H_{Ht}$$

H_{Mt} are hours worked in the market sector and H_{Ht} hours worked in the home sector. These two activities are assumed to be perfect substitutes. Two types of consumption goods exist: C_H is home-produced and C_M market-produced, C being the composite consumption good. Benhabib *et al.* assume a constant elasticity of substitution between market and home consumption:

$$C_t = [aC_{Mt}^e + (1-a)C_{Ht}^e]^{1/e},$$

with $1/(1-e)$ the constant elasticity of substitution. There are two production functions (of a Cobb-Douglas type), one for market production and another one for the home sector.

$$f(Z_{Mt}, K_{Mt}, H_{Mt}) = \exp(Z_{Mt})K_{Mt}^\alpha H_{Mt}^{1-\alpha}$$

$$g(Z_{Ht}, K_{Ht}, H_{Ht}) = \exp(Z_{Ht})K_{Ht}^\beta H_{Ht}^{1-\beta}$$

where Z_{Mt} and Z_{Ht} are technology disturbances following a stochastic process. It is thus assumed that agents are able to move in and out of market activity according to their willingness to substitute and the correlation between the two shocks.

Studying the static home production model, Benhabib *et al.* showed that, for any home production model, there exists a model without home production

(i.e., the baseline model) with different preferences yet generating identical outcomes for market quantities. This observational equivalence is also valid for dynamic analysis. It follows that one can study a model mixing home and market production through the proxy of the baseline model.

When simulated, the Benhabib, Rogerson, and Wright model marks a three-fold improvement with respect to the standard model. First, output is more volatile than in the baseline model, and thus more in line with the data. Second, the relative volatility of hours relative to productivity increases significantly. Third, the correlation between hours and productivity also decreases considerably. This is progress as conceived of by Kydland and Prescott.

GOVERNMENT SPENDING SHOCKS

Looking at the U.S. data, the correlation between hours and productivity is near zero. However, in the standard RBC model, it is close to one. In a 1992 paper, Lawrence Christiano and Martin Eichenbaum addressed this shortcoming, which they related to the fact that in the baseline model technology shocks shift the labor-demand curve while keeping the supply of labor curve unchanged. Their hunch to bring the model closer to the data was to introduce demand shocks in addition to technology disturbances, more precisely a government consumption shock. This shock, which involves a more or less full drain on output, is supposed to modify the supply of labor. Financed by a lump-sum tax and entering neither into the utility function nor into the production function, it causes a negative wealth effect on households. As a result, these are induced to work more hours, and the supply of labor shifts to the right. Average productivity decreases as well. The technology shock and the government spending shock generate opposite effects on the relationship between hours and productivity. Which will prevail depends on the size of the shock and the parameter in the law of motion of the shock.

Using L-P. Hansen's generalized method-of-moments (Hansen 1982) instead of calibration, Christiano and Eichenbaum obtained results that were close to those of the standard model, except for the correlation between hours and productivity, the variable that they decided to focus on. In their paper, it was lower than the standard model result (0.49 against 0.93), yet still higher than what the U.S. data indicated.

DANTHINE AND DONALDSON'S SHIRKING MODEL

Jean-Pierre Danthine and John Donaldson wrote two papers aiming at introducing the efficiency wage idea into RBC modeling (Danthine and Donaldson 1990, 1995). I limit myself to discussing the second one purporting to integrate Shapiro and Stiglitz's shirking model, studied in Chapter 13, into the RBC language.

Such a project faces several hurdles. A first one concerns the identity of the decision-making agent. In the standard RBC model, the decision maker is a single self-employed agent. In efficiency wage models, firms, as distinct from workers, are the decision makers. To tackle this problem, Danthine and Donaldson assumed that firms are owned by infinitely-lived dynasties of shareholders. These dynasties undertake all investment and hiring decisions. Furthermore, they assumed, firstly, that there exists a representative shareholder, and, secondly, that workers do not own firms and have no access to credit, to the effect that their decision problem is static. Thereby, Danthine and Donaldson were able to fall back on the standard way of formulating the objective function. The equilibrium of their economy is tantamount to the solution of the optimal investment decision to be made by a representative agent (here, the representative shareholder) on the basis of her contractual arrangement with workers about wages and transfer payments. Next came the problem of introducing efforts in the production function. Danthine and Donaldson classified workers in three types, the young, the old with experience and the old without it. Workers are supposed to live two periods, inelastically supplying indivisible labor at each of them. Effort exertion results in disutility. If caught shirking during the first period, workers are fired. Danthine and Donaldson assumed that the workers who happen to be fired when young are inexperienced when entering the second period of their life, and therefore receive a lower wage. A complex taxonomy ensued since for each of the three types a further distinction must be made according to whether they shirk or not, this in turn depending on the wage rate.

Another hurdle facing Danthine and Donaldson was that, in the shirking model, the unemployed have a lower utility than the employed. Such heterogeneity is incompatible with the representative agent framework. Therefore Danthine and Donaldson were compelled to fall back on the Rogerson solution of assuming the existence of an insurance scheme. As a result, all agents end up with the same utility whatever their employment status. The price to be paid for introducing the efficiency wage insight in the RBC thus amounts to depriving it from its distinctive trait!

The above remarks make it clear that Danthine and Donaldson's model is complicated. For my purpose, it is unnecessary to enter into the details of their demonstration. What matters is that when it comes to the issue of whether their model fitted the data, they felt authorized to declare that "the performance of the model is strikingly good" (Danthine and Donaldson 1995: 238). Not only does it perform as well as the standard model where the latter fared fine, it also improves upon its shortcomings. Their model, Danthine and Donaldson claimed, succeeds in particular in decoupling the volatility of hours worked from that of productivity – although in an exaggerated way (Danthine and Donaldson 1995: 227).

Danthine and Donaldson's work is more interesting for its programmatic character than for its substantive contribution. The 1980s and the beginning of

the 1990s were years of strong dissension within the macroeconomics profession. First-generation new Keynesian economists judged that Lucas was right about the microfoundedness requirement, but they stuck to their guns on the topics of market non-clearing, rigidity, and money non-neutrality. Their opposition to the DSGE program became even stronger with the ascent of RBC modeling. Danthine and Donaldson took a more conciliatory position by showing that one emblematic new Keynesian model, the shirking model, could be integrated into RBC modeling. In other words, unlike first-generation new Keynesians, they defended the view that RBC modeling was a good bifurcation to be taken, and that it was amenable to Keynesian themes. What was needed, they argued, was “to open the RBC methodology to non-Walrasian considerations, and, reciprocally, to submit some non-Walrasian models to the discipline of the RBC approach” (Danthine and Donaldson 1990: 1293). Danthine and Donaldson’s originality was thus to have been among the first to defend the view that one could simultaneously adhere to methodology set out by Kydland and Prescott and pull RBC modeling away from the purist line that had characterized it initially. Such an opinion belonged to the minority when Danthine and Donaldson started expressing it, but it became the established view with the ascent of second-generation new Keynesian modeling.

Another remark to be made about Danthine and Donaldson’s work is that one might have expected that it would be anathema to RBC economists (Danthine and Donaldson 1993: 27). Efficiency-wage modeling was hardly these economists’ cup of tea, and its identification with the new Keynesian program did not help. Additionally, RBC modeling was uncongenial to several features of their models (e.g., a wage floor and an exogenous labor supply). However, no such dismissal occurred. On the contrary, Danthine and Donaldson’s paper was included in the standard-bearing *Frontiers of Business Cycle Research* volume (Cooley 1995). Prescott also quoted it in the section of his Nobel Prize lecture in which he surveyed subsequent studies using the business-cycle research methodology (Prescott 2006: 220).

Several factors may explain this positive reception.⁷ The main one is that Danthine and Donaldson respected the standards of RBC modeling and they did not reach Keynesian policy conclusions. They repeatedly underlined the fact that ideology was not at stake, which must also have been music to new classical and RBC economists’ ears.⁸ Finally, the accreditation of their work may have served as a token of the openness of the DSGE program: macroeconomic theory is seen as a language, and any model respecting its syntax is welcome.

⁷ The fact that Danthine wrote his dissertation under Lucas’s and Prescott’s joint supervision may have helped in their being considered part of the family. Donaldson wrote his under Cass’s supervision.

⁸ “In reality, the RBC methodology is by nature ideologically neutral in the sense that it prefers the model or set of models that is (are) best able to replicate the stylized facts independent of the hypotheses underlying it (them)” (Danthine and Donaldson 1993: 3).

LATER CRITICISMS: A LIMITED EMPIRICAL SUCCESS

Danthine and Donaldson also wrote general assessment papers of the RBC approach. In one of these, entitled "Recent Developments in Macroeconomics: The DSGE Approach to Business Cycles in Perspective," they pondered what economists had learned from the RBC research program (Danthine and Donaldson 2001). On the negative side, their main complaint was that the basic RBC model focuses on too small a subset moments. They argued that models that perform well with the small lists of moments considered in the early studies cease to do so when other moments are considered. Moreover, different models with diverging policy implications have proved to be observationally equivalent (Danthine and Donaldson 2001: 59). If this is true, replication insufficiently guarantees the validity of a model. Thereby, Danthine and Donaldson came close to Summers's criticism that RBC modeling rests on the weakest possible conception of explanation.

Another attack against the empirical results of RBC modeling is to be found in two papers, written by L-P. Hansen and Heckman (1996) and by Sims (1996) for a *Journal of Economic Perspectives* symposium on calibration. The symposium comprised three papers. The first one was a defense of calibration signed by Kydland and Prescott. They boasted that their approach was successful with such conviction that the uninformed reader is baffled when turning to the two other papers which, on the contrary, tore apart Kydland and Prescott's results.

According to Hansen and Heckman, Kydland and Prescott's argument was mere bluff:

While Kydland and Prescott advocate the use of 'well-tested theories' in their essay, they never move beyond this slogan, and they do not justify their claim of fulfilling this criterion in their own research. 'Well tested' means more than 'familiar' or 'widely accepted' or 'agreed by convention,' if it is to mean anything. (Hansen and Heckman 1996: 86)

Hansen and Heckman's main criticism concerned the practice of calibration. They did not fault Kydland and Prescott's advocacy of tightly parameterized models. What they disapproved of is that Kydland and Prescott behaved as if a large store of data from which the modeler can draw existed. To them, the charges of weak empirical foundations that were made against earlier computational general equilibrium models was even more relevant when it comes to real business cycle models:

It is simply not true that there is a large shelf of micro estimates already constructed for different economic environments that can be plugged without modification into a new macro model. In many cases, estimators that are valid in one economic environment are not well suited for another. Given the less-than-idyllic state of affairs, it seems foolish to look to micro data as the primary source for many of the macro parameters required to do simulation analysis. Many crucial economic parameters – for example the effect of product inputs on industry supply – can only be determined by looking at relationships among aggregates. Like it or not, time series evidence remains essential in determining many fundamentally aggregative parameters. (Hansen and Heckman 1996: 100)

Sims was no less harsh: "I think it is fair to say that most RBC research has ignored most of the known facts about the business cycle" (Sims 1996: 113). To him also, Prescott's claim that there exists an 'established theory' on which macroeconomists can rely was a farce:

The neoclassical stochastic-growth model that Kydland and Prescott put forth as the foundation of dynamic, stochastic, general equilibrium modeling is legitimately labeled accepted theory in one limited sense. There is an interacting group of researchers working out the implications of models built on this base; within this group the theory is accepted as a working hypothesis. But even within this group there is no illusion that the theory is noncontroversial in the profession at large. Most in the group would not even assert confidently that it is clear that theory of this type will deliver on its promise, any more than did Keynesian simultaneous equations models or natural rate rational expectations models. ... Dynamic, stochastic, general equilibrium modeling has delivered little empirical payoff so far. (Sims 1996: 113)

LATER CRITICISMS: QUESTIONING TECHNOLOGICAL SHOCKS AS THE CAUSE OF BUSINESS FLUCTUATIONS

Prescott's claim that the Solow residual provides an apt measurement rod of technology shocks has played a central part in the development of RBC modeling. This contention enabled to cut through a series of complex problems. Yet it also amounted to sweeping under the rug the earlier objections that were leveled against Solow's growth accounting methodology, the possibility of mismeasurement, and the assumptions of perfect competition and constant returns to scale. It is thus small wonder that Prescott's contention quickly led to skeptical reactions. In his comments on Prescott, Summers had evoked the disturbing role of labor hoarding. McCallum and Mankiw expressed a similar distrust, especially about the possibility of a sufficiently high technological regress. It took just a few years for these first, intuitive reactions to be followed by more systematic examinations. In this section, I present an overview of these developments.

Pioneering this critical line were two articles by Hall (1988b, 1990) and one by Charles Evans (1992). Here, I limit myself to discussing Hall's article, "Invariance Properties of Solow's Productivity Residual" (1990).⁹ Presented at a conference in Solow's honor, it critically examined Prescott's use of the Solow residual (probably with Solow's endorsement). Hall's aim was to study what happens when the perfect competition and constant returns to scale assumptions are lifted, a context in which the so-called Solow residual is no longer a residual. Moreover, in this context, output elasticities can no longer

⁹ Evans showed that the Solow residual could be forecast using the lagged values of various monetary aggregates, money, interest rates, and government spending. This claim came to be substantiated in the series of papers that Evans wrote with his coauthors, Christiano and Eichenbaum.

play the role they did in Solow's paper. Hall's take was to bring in the 'invariance property', namely that the residual must be invariant with respect to variables which are known not to trigger a shift in productivity and not to result from productivity changes caused by other sources. The aim of Hall's paper was to put this property to the test by treating the Solow residual equation ($\mathcal{R}_t = \frac{\dot{A}_t}{A} = \frac{\dot{Y}_t}{Y_t} - s_t^K \frac{\dot{K}_t}{K_t} - s_t^L \frac{\dot{L}_t}{L_t}$; see Table 15.1) as a regression and regressing it on a few instrumental variables. The three he considered were military spending, the world oil price and the political party of the U.S. president. Any positive correlation between these variables and the Solow residual would mean that the Solow residual captures other influences than "true productivity."¹⁰ Hall's conclusions were dismissive of Prescott's claim:

The assumptions Solow made in developing the now-standard approach to productivity measurement are clearly false. In fact, productivity growth is highly correlated with oil prices, quite correlated with military spending, and somewhat correlated with the political party of the president. (1990: 110)

Hall put forward three causes explaining why the invariance property was refuted. The first is increasing returns, which are due to the presence of overhead costs. Measurement errors, he argued, were a second leading explanation. Finally, he also mentioned the thick-market external benefit enjoyed by firms at times when output is high. All in all, however, to him increasing returns seemed to play the most salient role.

Hall's paper paved the way for further studies aiming at bringing out explanatory factors of business fluctuations other than technology. These borrowed Hall's methodology (attention to costs instead of revenue, the invariance property and the search for instrumental variables), yet often explored a different explanatory track. Following Jorgensen and Griliches' mismeasurement insight, they argued that the conventional Solow residual is an unreliable measure of TFP.

A seminal paper in this line was Burnside and Eichenbaum (1994), which was later expanded in Burnside, Eichenbaum and Rebelo (1995), a broadly encompassing piece, based on the exploitation of more detailed data than those considered by Hall. Burnside, Eichenbaum and Rebelo's hunch was that capital utilization needed to be better measured. What needs to be measured is the service of capital rather than its stock, while the latter is the only directly observable variable. To overcome this problem, they used two proxies. The first one was the industrial use of electricity, an indicator that Griliches had already used; the second one was data on the 'workweek' of capital, that is, the

¹⁰ Hall rearranged the conventional Solow residual in a few ways, into which I cannot delve into, except for noticing that he replaced the revenue-based measurement of inputs, used by Solow, with a cost-based measurement, which enabled discarding the possible effect of market power. Cf. Hall (1990).

length of use of capital over the week as indicated by the number of workers' shifts. They concluded that rates of capital utilization turned out to be strongly procyclical which implied that the uncorrected Solow residual mingles different types of determinants.

Other important contributors to the research line opened by Hall were Susanto Basu, John Fernald and Miles Kimball. They wrote several papers aiming at starting the examination of the productivity issue afresh.¹¹ Here, I comment on Basu and Fernald (2001). Like Burnside *et al.*, they drew the attention on the existence of a discrepancy between real and measured factor input.¹² This was true, they argued, for capital utilization, but also for the labor input. The problem is that the intensive margin carries two components, hours and effort, the first being observable, the second not. The true input of labor (hours and effort) and its measured output thus differ. They also argued that the true input use is highly cyclical. When demand is high, hours worked and effort increase jointly. If this is true, increases in measured output can act as a proxy for the non-observable component of the input, effort, to the effect that the true use of the labor input can be apprehended (Basu and Fernald 2001: 228). A broader conclusion is that any measure that neglects taking effort and its cyclical character into account will overestimate the cyclical character of technology. Basu and Fernald concluded from their empirical testing of these insights that the Solow residual overestimates the role of technology.¹³

A further step in the debate occurred when papers were published arguing that in the short run, technology shocks lead a reduction in the level of activity, the opposite effect of the basic RBC claim. Jordi Gali (1999) was to first to argue this.

The basic RBC model assumes that a technology shock makes the demand for labor shift to the right, the supply of labor schedule remaining unchanged. This implies a high positive correlation between hours and average labor productivity. The problem is that the data exhibit almost no such correlation. Christiano and Eichenbaum constructed their governmental shock model discussed above in the hopes of solving this conundrum. Gali's aim was to approach the problem using a new Keynesian model with monopolist competition and sticky prices (the model that I will study in Chapter 18 under the 'second-generation new Keynesian' label). His reasoning can be summarized as

¹¹ Basu and Fernald (2001) is the more systematic exposition of their analysis. Other papers are Basu (1996), Basu and Fernald (1997), and Basu and Fernald (2002).

¹² "In essence, the problem is cyclical measurement error of input quantities: True inputs are more cyclical than measured inputs, so that measured productivity is spuriously procyclical" (Basu and Fernald 2001: 228).

¹³ "Our corrected series have about the same mean as the Solow residual. However, the variance is much smaller: The variance of the fully corrected series is less than one-third that of the Solow residual, so the standard deviation is only about 55 percent larger" (Basu and Fernald 2001: 271).

follows. In his model, unlike in the RBC model, a technology shock exerts only a light impact on a predetermined nominal aggregate demand schedule, the result of the price sluggishness assumption. Hence the quantity produced also varies little. Because technology is more efficient, less input is needed to produce this quantity. Hours worked should thus also decrease. Yet such a negative correlation between hours worked and average productivity is of no help since what is needed is a quasi-zero correlation. In order for the the model to be in accord with to the data, it must be presumed that a non-technology shock crops up on the technology, thereby offsetting the negative impulse (Gali 1999: 250).

In their 2006 paper, entitled “Are technology improvements contractionary?,” Basu, Fernald, and Kimball came to a result close to Gali’s. In this paper, they controlled for the effect on aggregate TFP of three factors: (a) the unobserved utilization of labor (effort) and capital (workweek), (b) non-constant returns to scale and imperfect competition, and (c) aggregation. Separating immediate and lagged effects, they claimed that the results of the standard model were verified if lags were considered: “after a year or two, the response to our estimate technology series more or less matches the prediction of the standard, frictionless RBC model” (Basu *et al.* 2006: 1419). However, they argued that, within a one-year time span, a technology leads to different results: total hours worked decrease, output changes little, and non-residential investment falls sharply.

Neither Gali’s nor Basu *et al.*’s papers can be considered to have made a definitive point. The interest of this literature follows from different considerations. First of all, it reveals a trend towards qualifying Kydland and Prescott’s initial bold claim as to the causes of business fluctuations. Other shocks than technology were gradually brought into the picture, a move that would culminate in the ascent of second-generation new Keynesian modeling. Thereby the notion of friction that had almost disappeared from the radar was making a comeback.

However, questioning Kydland and Prescott’s claims is hardly tantamount to dismissing their method. These developments remained within the track that Kydland and Prescott opened. This is the conclusion that King and Rebelo drew in their contribution to the *Handbook of Macroeconomics*, entitled “Resuscitating Real Business Cycles”:

While we think that economists may have prematurely dismissed the idea that the business cycle may originate from real causes, we also think that many of the lessons drawn from current and future RBC research are likely to be independent of the main source of business fluctuations. This is one important reason why the RBC literature has been a positive technology shock to macroeconomics. (King and Plosser 2000: 995)

A METHODOLOGICAL BREAKTHROUGH

The criticisms I have evoked in the previous sections are strident, suggesting irreconcilable standpoints. Amazingly enough, over the years, dissension has

been smoothed. After their strong words against the calibration method, one would have expected Hansen and Heckman, and Sims to conclude that nothing less than rejecting it was admissible. Not at all! Instead, in their papers, they eventually called for collaboration between estimation and calibration.¹⁴ In a 2005 interview, Sargent, also an early opponent of calibration, confirmed this appeasement process. Asked whether he believed that the use of calibration in macroeconomics was an advance, he answered that the following case for calibration can be made. At the time, there were skeptics and opponents to the DSGE program. Maximum likelihood tests were too severe, rejecting good models. By lowering standards, calibration models “could capture and focus attention on the next unexplained feature that ought to be explained.” Thus, in Sargent’s eyes, in view of the big tasks involved in constructing the new approach, proceeding in a stepwise strategy was the thing to do. That is as he said in his interview by, George Evans and Seppo Honkapohja:

Let’s first devote resources to learning how to create a range of compelling equilibrium models to incorporate interesting mechanisms. We’ll be careful about the estimation in later years when we have mastered the modeling technology. (Evans and Honkapohja 2005: 7–8)

Likewise, for all their harsh attacks on RBC modeling, the criticism by Danthine and Donaldson (2001) and by Hall and Basu should not be taken as meaning that they favor abandoning the broader DSGE program to which RBC modeling belongs. These economists’ final judgment, shared by a large fraction of the macroeconomics community, was more nuanced and could be summarized in an ‘on the right track but still inadequate’ judgment.

The topic of intertemporal elasticity of substitution is another example of the ascent of a more consensual state of mind. After econometricians and RBC economists dug in their heels for more than a decade, new developments may mark a way out of the stalemate. To limit myself to one example, in a 2006 paper, Ljungqvist and Sargent proposed a new way of conceptualizing “time-averaging” aggregate theory. Instead of conceiving a lottery or the existence of a large household deciding about sending a fraction of its members to work, they envisaged that agents’ decision problem relates to their life-long allocation of work and leisure, with consumption smoothed across active and non-active periods through trading a risk-free asset. If an interior equilibrium solution exists, their model reaches the same high labor supply elasticity as the Hansen-Rogerson model. However, as Ljungqvist and Sargent (2011) made clear, this

¹⁴ “We envision a symbiotic relationship between calibrators and empirical economists in which calibration methods like those used by Frisch, Tinbergen, and Kydland and Prescott stimulate the production of more convincing micro-empirical estimates by showing which gaps in our knowledge of micro phenomenon matter and which gaps do not. Calibration should only be the starting point of an empirical analysis of general equilibrium models” (Hansen and Heckman 1996: 101).

narrowing in the range of opinion about labor supply elasticity is hardly the last word on the matter because a new choice between two bifurcations arises. The first of these is to stick to the earlier RBC framework and hence to their general conclusion.¹⁵ The other one, pushed by Ljungqvist and Sargent, consists in giving a more prominent role to institutional aspects, taxes, retirement age, and unemployment benefits. Their consideration brings about corner solutions, thus ending up supporting the econometricians' viewpoint.¹⁶

The lesson to be drawn from the contributions studied in this chapter is that the judgment about Kydland and Prescott's new approach evolved over time. Many of its earlier opponents came to admit that the methodology Kydland and Prescott initiated was more powerful than what they believed in the beginning. In a context where computation ability tremendously increased and the availability of databases expanded almost exponentially, it turned out that the conceptual and applied machinery they had set in place carried an impressive development potential. The consensus that arose can be couched in terms of Leijonhufvud's metaphor. What was deemed needed was to go ahead creating a bifurcation of a lower order, that is, within the existing research branch, rather than engaging in a backtracking towards an earlier neglected basic bifurcation. Importing distortions (stickiness, imperfect competition) and bringing money into the picture are examples of such lower-order bifurcations. In other words, the widespread feeling was that macroeconomics should go beyond RBC modeling, while still remaining within the bounds of the DSGE program.

¹⁵ It has been taken up by Keane and Rogerson (2010).

¹⁶ The Ljungqvist and Sargent paper was presented during a session on "Micro versus Macro Labor Supply Elasticities" at the 2010 AEA Conference, along with other papers assessing the state of the art on the confrontation of micro and macro measurement of labor supply elasticity. Cf., Bozio and Laroque (2011). See also Ljungqvist and Sargent's open letter to Professors Heckman and Prescott (Ljungqvist and Sargent 2014).

Real Business Cycle Modeling: My Assessment

It has been said that a brilliant theory is one which at first seems ridiculous and later seems obvious. There are many that feel that [RBC] research has passed the first test. But they should recognize the definite possibility that it may someday pass the second test as well

(Rogoff 1986).

All men know the use of the useful, but nobody knows the use of the useless
(Chuang Tzu [c.360 BC – c. 275 BC] in Watson 1968: 66).

In Chapter 8, I assessed Lucas's vision of macroeconomics. Much of what I wrote there is still valid for RBC modeling. However, in view of the novelties that RBC modeling introduced in the DSGE program, additional comments are called for. This is the task I tackle in the present chapter.

THE DISCREPANCY BETWEEN THE MODEL AND ITS EXPLANANDUM

A first observation to be made is that the new configuration that surfaced in the wake of Kydland and Prescott is paradoxical. The ultimate *explanandum* of macroeconomics is the functioning (or malfunctioning) of capitalist economies. However, the type of economy with which RBC models are concerned is poles apart from this *explanandum*. Solow's dismissing judgment, quoted in Chapter 15, perfectly expresses the perplexity generated by such a discrepancy.

In the 1930s, a fierce debate took place among economists on the possibility of a socialist economy, the so-called 'socialist calculation debate.'¹ It opposed defenders of the market system, such as Hayek and von Mises, and of socialism, such as Dickinson and Lange.¹ The latter group used Walrasian theory to

¹ See Lavoie (1985) and Caldwell (1997).

demonstrate the possibility of an efficient planning system. They argued that the planners' role was to gather information about agents' choices, enter it into the model of a competitive economy, calculate its equilibrium price vector and its associated allocation, and take the quantities of goods and services obtained as the plan's production targets. Thereby, planning was considered as efficient as the market in providing for an efficient deployment of activities. The debate did not evolve at the same level of abstraction as when referring to the two welfare theorems, but the object of concern was the same. This use of Walrasian theory in defense of socialism caused indignation among defenders of the market system, who found it outrageous to consider the competitive and the planning economy as similar. They argued that their functioning mechanisms were poles apart. In a famous article, Hayek made it clear that the advantage of the market economy consisted in the tremendous advantages in the use of knowledge implied by its functioning, while, in contrast, gathering the necessary information was the Achilles heel of the planning system (Hayek [1945] 1948).²

Fifty years later, we see this isomorphism making a comeback, courtesy of the second theorem of welfare economics. Thus, ironically enough, an adamant defender of *laissez faire*, such as Prescott, is the intellectual heir of the supporters of central planning (De Grauwe 2009: 3). A theorem is a theorem, and as such cannot be wrong, but there might be a snag somewhere. I believe it lies in the competitive economy leg of the isomorphism: the theoretical notion of a Walrasian competitive economy, taken up in a trivial form by Kydland and Prescott, represents a totally biased representation of its alleged real-world counterpart, the capitalist economy.

The market and the planning systems ought to be contrasted in terms of how they ensure that the 'direction of employment' – to use Adam Smith's term, meaning the allocation of equipment and the labor force to specific production activities – conforms to social wants. Let me use the notion of 'validation' to designate the process involved. In the planning system, social wants are defined by the planning bureau, itself an emanation of the political authority. In this system, decisions come from above and are validated, at least formally, as soon as they are enacted. This process can be called automatic or *ex ante* validation, as it takes place before the start of production. The relevance of this allocation mechanism extends beyond strict planning systems. It governs authoritarian regimes in general. When Louis XIV decided that a fraction of the French labor force was to be directed toward the construction of the Versailles Palace, this decision had to receive no further confirmation of meeting social wants. The market system stands in sharp contrast. Its hallmark is that economic decisions are taken privately. The direction of employment follows from private initiatives

² Friedman's antagonism to the work done at the Cowles Commission and to Walrasian theory resulted to some extent from this debate.

made mostly by firms. Pursuing the profit motive, firms strive at anticipating (and possibly shaping) future social wants. To this end, they must invest in equipment and human capital in order to produce goods and services that, they hope, will meet demand once produced. In this context, validation is far from automatic as it hinges on the correctness of firms' private anticipation of social wants in a context where externalities are prevalent. The market system is thus characterized by *ex post* validation. It arises as a matter of degree ranging from super-profits to bankruptcy (the latter being the result of large, wrong, private decisions). Even in good times, validation failures are a normal occurrence in the market system. Depressions are periods when non-validation has become widespread and cumulative. Mistaken private initiatives have to be liquidated by shutting down excess capacity or, possibly, by the elimination of inept firms.

This brief depiction suffices for my purposes as it can address the question: to which of these two polar ideal types does the 'Walrasian economy' (the object of study of neo-Walrasian theory) belong? The answer is that it is a hybrid. Like the market system, it is a private ownership and private decision-making economy. But it shares the functioning mechanism of the planning system: validation occurs in an *a priori* way. This is due to the auctioneer assumption and its correlate, the exclusion of out-of-equilibrium trading. Nothing happens – neither production, nor trade – before equilibrium is reached. With complete markets, unpleasant surprises cannot arise. Thus, to return to a point that Hayek stressed long ago (although to little avail), that the Walrasian notion of a competitive economy runs counter to the deep nature of its *explanandum* (Hayek [1946] 1948). Among other things, the Darwinian side of competition is totally lost. Calling a Walrasian economy 'competitive' is a misnomer!

SHOULD RBC MODELING BE DISMISSED?

If I am right in saying that there is a basic methodological problem with the RBC model, in that it is at odds with its *explanandum*, which conclusion should be drawn? To many economists, the answer is straightforward: this approach should be abandoned. To Post-Keynesians, RBC macroeconomics is to be indicted as an extreme case of generalized betrayal of Keynes's project (Chick 2006, Chick and Till 2014).³ On different grounds, this is also Skidelsky's (2009) and Leijonhufvud's viewpoints (2008, 2009). To others, for example Kirman (2010), the basic flaw is the representative agent assumption plus having put aside aggregation problems. This is also Hoover's viewpoint (2001, 2007). In addition, Hoover along with many others, for example Ray (2001, 2012) rejects RBC modeling on the grounds of using the flawed calibration method. To others still, for example Laidler (2010), the RBC approach is

³ This is also Skidelsky's (2009) standpoint.

to be condemned because it is based on the market-clearing assumption (and hence the rejection of any disequilibrium situation), a criticism to which Austrian economists rally. Another line, taken by De Grauwe (2010), regards the lack of recognition of agents' cognitive limitations as the basic flaw.

These are all strong indictments. Nonetheless, in my view, the dividing line between opponents and supporters of the RBC approach (and more widely of the DSGE program) relates less to diagnosing its defects than to drawing the implications of a shared diagnosis. I surmise that most defenders of DSGE macroeconomics are ready to recognize that the representative agent assumption is wanting, that it is unfortunate that coordination failures cannot be addressed, that rational expectations are too strong an assumption, that calibration involves arbitrariness and fiddling, etc. However, their belief is that these flaws are not sufficient to condemn the program. They view them as shortcomings to be remedied upon while keeping the line taken. In spite of its limitations, to them, this program is a fine platform for subsequent developments.

As a historian of economics, I do not need to take a stance on this divide. Let me, however, note that two arguments can be made in favor of the DSGE program. First, it has produced an impressive cumulative progress, both conceptually and empirically. It has also had a remarkable ability to bounce back from criticism. With respect to my criterion that modeling strategies should be judged on their posterity, it has fared surprisingly well. The second argument is a negative one. It concerns the alternative bifurcations. I have praised Hayek for having aptly captured the essence of a market system. But the problem with Austrian economics lies in the difficulty encountered in transforming its founding texts into a progressive research program. The same is true for the various programs defended by the anti-Lucasian economists listed above. Therefore, I definitely cannot share their view that the line opened by Kydland and Prescott is unworthy of pursuing.

There is, however, a serious issue with it. In my eyes, the global judgment to be formed about research programs depends not only on their intrinsic achievements, but also on the meta-theoretical (i.e., interpretative) comments made about them. Inapposite meta-theoretical comments cast a negative shadow on theoretical achievements. In this respect, RBC economists have often fared badly, testifying to a lack of awareness of the limits imposed by the adoption of Walrasian principles.

THE LIMITATIONS OF RBC MODELING

RBC modeling is heir to Lucas's vision of macroeconomics and hence must abide by the methodological principles that he set out and that I described in Chapter 7. As it was seen, they are guided by taking internal consistency as their overarching aim and by keeping ideology and theory separate. This is their positive side. The negative side is that the adoption of these principles implies a series of interpretative restrictions. I will dwell on three of them.

A first limitation relates to the scope of relevance of RBC modeling. Adopting the equilibrium discipline implies that the explanatory capacity of macroeconomics is circumscribed to the explanation of mild or normal business fluctuations, while more dramatic episodes such as the Great Depression (or for that matter, the 2008 recession) fall outside its scope. As noticed in Chapter 8, Lucas was aware of this limit; he admitted that RBC modeling is ill-equipped for tackling serious pathologies.

In the beginning, Prescott shared Lucas's viewpoint.⁴ However, in the wake of Harold Cole and Lee Ohanian's work (1999; 2004) on the Great Depression, which concluded that the weak recovery of the U.S. economy from 1934 to 1939 was due to New Deal policy measures, he changed his mind declaring that RBC modeling actually could tackle great depressions.⁵ In 2000, Timothy Kehoe and he organized a Conference at the Federal Reserve Bank of Minneapolis where a dozen papers applying RBC tools to country studies in the Great Depression years were presented.⁶ This conference led Kehoe and Prescott to propose an equilibrium theory of great recessions, the hallmark of which is that the attribution of the 'great recession' label is based on rather broad criteria. As a result, the Great Depression of the 1930s has ceased to be declared a single, isolated episode but one among several others.

I find that Lucas's restrained viewpoint is more sensible. The view that RBC models can only explain normal fluctuations rather than crises is congruent with his inaugural justification of equilibrium business-cycle theory, namely that what makes such a theory possible is that all business fluctuations are basically alike. What is at stake then is whether the same can be said of 'great depressions.' By broadening their definitional criteria, Kehoe and Prescott answered this question positively, but the problem with using very broad categories is that too many different occurrences end up in the same bag. The alternative conception is to view great depressions as idiosyncratic events, the result of the concatenation of small, specific events. As a result, if the elements of singularity indeed dominate those of commonality, narrative historical accounts are more suited than models for explaining such events.

A second limitation of RBC modeling concerns the type of explanation provided by RBC models. My claim is that they offer a poor explanation of business fluctuations if by 'explanation' we mean exposing the causes underlying the rise of given historical events. On the one hand, general explanations are by definition incomplete. Lucas's starting point was that all business cycles were alike. He argued that this was what allowed the creation of a general theory of

⁴ "The inability of either the equilibrium-monetary or the technology-shock theories to explain the Great American Depression is evidence of the discipline of the methodology. If any approach can be rationalized with some approach, then that approach is not scientific" (Prescott 1983 12). Prescott made the same point in his reply to Summers (Prescott 1986b: 29).

⁵ De Vroey and Pensieroso (2006).

⁶ Its proceedings became a book (Kehoe and Prescott 2007a).

business fluctuations. But the counterpart is that such a theory by definition sheds little light on the specificities of particular cycles, their causes, their pattern, and so on – and when it comes to great recessions, these particularities matter a lot. On the other hand, everybody believes that the explanatory value of theories increases when they are confronted with reality. However, such a belief ought to be qualified. It cannot be declared that a model which succeeds in replicating the data using the calibration methodology provides a compelling explanation of reality. Even if the shortcomings of the calibration method mentioned above were absent, verification is a poor criterion of validity for scientific propositions as compared to falsification. Let it be admitted that economists are right in believing that adopting the Popperian criterion amounts to setting the bar too high. It remains that a duty of modesty must still be respected. As shown by Danthine and Donaldson, models based on different causal explanations may get a similar empirical fit, which means that replicating and explaining are two different things.

A related point is Prescott's firm belief that the neoclassical growth model is established theory, to the effect that what remains to be done by economists is to apply it, and in this process to enrich it. For a historian of economics, this is a very naïve standpoint. 'Established theories' are often short-lived. Moreover, one person's established theory is not that of another, and Prescott offers no clue about choosing between them; replication will not do. I surmise that, for example, Marxian economists, armed with the same energy and creativity as Kydland and Prescott, would be able to construct a model economy based on Marxian theory, calibrate it, simulate the model, and do a good job in replicating real-world time series to the effect that they will be comforted in their view that Marxian theory is the established paradigm.

Finally, a last limitation relates to Lucas's view that theoretical propositions pertain to the fictitious model economy and not to reality. A stringent methodological precept ensues: it is mistaken to extend theoretical conclusions to reality, even if the replication discipline has led to satisfactory results. Abiding by this precept is difficult, probably heroic. Time and again, RBC economists have failed to respect it. As the matter is important, I want to give three examples drawn from Prescott's writings.

The first one concerns the way in which Prescott comments on the policy conclusions of the Kydland-Prescott model, arguing that there is nothing wrong with business fluctuations from a social welfare viewpoint. This marks a breach from the view that dominated before, namely that the business cycle is a manifestation of market failures. Here is what he writes:

The policy implication of the research is that costly efforts at stabilization are likely to be counter-productive. Economic fluctuations are optimal responses to uncertainty in the rate of technological change. (Prescott [1986] 1994: 286)

My problem with this quote comes from its last sentence. The reader will interpret it as a proposition about reality, while its relevance is limited to the model economy.

My second example is drawn from Prescott's Nobel Lecture. Referring to earlier economists, he writes that they "falsely concluded that business cycle fluctuations were not in large part equilibrium responses to real shocks" (Prescott 2006: 210). This implies that, to Prescott, the correct view is that business fluctuations are in large part equilibrium responses to real shocks. Again, such a statement is a proposition about reality rather than about a fictitious economy. To dot the "i"s, it is mistaken to write, as Prescott's phrasing suggests, that the Kydland-Prescott model explains that, for the period studied, U.S. business fluctuations up to a certain percentage result from agents' optimizing reactions to exogenous shocks. The correct phrasing should rather be: "Kydland and Prescott have constructed a model economy in which agents are by definition in equilibrium; simulating it, they found that several of its moments replicate the moments calculated from U.S. statistics over the period considered." Methodologically, this difference is far from negligible: neglecting it makes one think that rational expectations, the equilibrium discipline, and so on are features of reality, while in fact they are just devices to construct models.

My last example is another quote, this time drawn from Kehoe and Prescott's reply to Temin's harsh review of their volume collecting papers on explaining the Great Depression (Kehoe and Prescott 2008):

The underlying hypothesis in our book is that the general-equilibrium growth model is a useful tool for studying great depressions episodes. The tentative findings are that bad government policies can turn ordinary economic downturns into great depressions. (Kehoe and Prescott 2008: 21)

Again, what is at stake is the separation between the fictitious model economy and the real-world economy. Kehoe and Prescott's fault is neglecting the point that the policy conclusions from models follow from the premises adopted. In their model, there is no other suspect for bad economic outcomes than inappropriate governmental policy, in the form of distortive taxation. The government being the only villain in the play, it is no surprise that it will have to bear the brunt of any bad result. One of the reasons why Lucas defended the use of mathematical models is that it serves the purpose of disentangling ideology and theory. It can now be seen that mathematics and a complex replication methodology do not suffice to keep ideology at bay.

These three examples suggest that Prescott, though having praised Lucas for his epistemological mastership, does not heed Lucas's non-exploitation principle evoked in Chapter 7. My guess is that it is likely that he is not alone in this respect.

This state of affairs is hardly surprising. What I have denounced above is methodological inconsistency. This criticism should not be taken as bearing of the theoretical corpus itself. It remains that the tendency to lapse into them follows from the nature of this corpus. I cannot but think that RBC macroeconomics is a bizarre object. At first sight, because of its concern with empirical confrontations and its inclination for policymaking, it gives the impression that it is an applied discipline. And it actually is. Nonetheless, there are also good

reasons to regard it as a 'pure' discipline in Walras's sense, that is, concerned with matters of principles, giving priority to internal over external consistency. For the better, if one believes that conceptual work, formalization, and rigor pay off in the long run. The flip side is that the ensuing epistemological discipline of abstaining from peddling the conclusions of the models to policy-makers is hard to abide by.

The conclusion to be drawn is that, notwithstanding the progress that took place in macroeconomics, civil society should not have too high expectations about what present-day macroeconomic theory can deliver as far as policy-making is concerned. Likewise, macroeconomists should avoid pretending that they have an edge on policy matters and endorsing the role of expert that they are often unduly invited to play (this remark applying as well to Keynesians as to non-Keynesians).

Does all this make macroeconomics *à la* Kydland and Prescott useless? I do not think so. It is just that there are different degrees of usefulness. Having based macroeconomics on Walrasian principles is commendable, but it also comes at a price. In my view, Walrasian theory ought to be regarded as a branch of political philosophy, different in language from others (because of the use of the mathematical language), yet nonetheless close to, for example, Rawls's theory of justice. It thus comprises an unavoidable normative dimension. If this is true, as a matter of social usefulness, economists should not consider themselves superior or inferior to other political philosophers. If they are experts, it is in the way political philosophers are.

Doing economic theory in such a way may look gratuitous and useless. But as the second epigraph at the beginning of his chapter suggests, what looks useless is not necessarily so.

It is true that things used to be different. Somebody like Keynes was able to span economic theory and policy advice. Such a possibility is no longer viable today, the result of an increased division of labor. A standard DSGE macroeconomist produces models and has little to say about real-world policy issues, except voicing general principles. For her, being mute on concrete issues is more honest than expressing her prejudices. This state of affairs had led to a widening of the gap between the production of economic science and the art of economic policy.⁷ The latter is becoming a specialization of its own, the economists engaged in it acting as the middlemen between the academic and policy worlds.

⁷ This is a point that Gregory Mankiw and David Colander have made repeatedly. Cf. Mankiw (2006) and Colander (2010: 41).

Second-Generation New Keynesian Modeling

In Chapter 16, I discussed modifications of the baseline RBC model that resulted in strengthening the RBC approach with little alteration of its research direction. Other modifications, though pursuing the same aim of improving the fit between the model and the data, had a different impact on the unfolding of macroeconomic theory as they led to drift away from RBC modeling as it stood before. They gave rise to the ascent of a third wave in the development of the DSGE program, ‘second-generation new Keynesian modeling.’ My aim in this chapter is to present its main features.¹

The two important modifications that took place were, first, the replacement of the initial perfect-competition/flexible-price framework with a monopolistic-competition/rigid-price one and, second, the return to the forefront of the monetary side of the economy. Although all this happened step by step, at some point, it became obvious that a tipping point had been reached. As Gali, a leading figure of the new approach, wrote, an “explosion of work” took place directed toward new themes, the study of “the effects of alternative policy rules, and other aspects of monetary economics which had been put aside during the era of RBC hegemony” (Gali 2000: 4). It was accompanied by a change in terminology, with the introduction of the new Keynesian label.²

¹ The reader must remember that what I call ‘second-generation new Keynesian modeling’ sometimes goes under the DSGE label. As explained in Chapter 9, I consider that it is better to reserve this label to the wider program initiated by Lucas.

² [By appending ‘New Keynesian Perspective’ to the title of our article] “we wish to make clear that we adopt the Keynesian approach of stressing nominal price rigidities, but at the same time base our analysis on frameworks that incorporate the recent methodological advances in macroeconomics modeling (hence the term ‘New’)” (Clarida, Gali, and Gertler 1999: 1662).

The issue of the real effects of monetary changes and its policy implications has been a key dividing line between Keynesians and ‘classical’ economists since Modigliani’s 1944 paper. Keynesians held sway on the matter until, under Friedman’s and Lucas’s lead, the ‘classicists’ reversed the situation and got the upper hand. Second-generation new Keynesian modeling marks a new turnabout. First, after the RBC episode, money is re-introduced into the picture, and for that matter in a much richer way than in the past. The objective pursued by the central bank in particular is spelled out more rigorously. Second, the non-neutrality of money is attributed to the existence of nominal price rigidity, sluggishness. Third, to have the latter, it is necessary to move away from the assumption that agents are price-takers and assume price-making agents. This, in turn, involves a shift from a perfect to an imperfect competition structure. These elements were already present in first-generation new Keynesian modeling. The basic difference, however, is that now they are grafted onto an RBC framework, while first-generation new Keynesians regarded the latter as unacceptable. Table 18.1 summarizes the differences between first- and second-generation new Keynesian approaches.

As a result of these developments, RBC came to be viewed like a thing of the past.³ This impression is incorrect, however: second-generation new Keynesian modeling is an outgrowth of RBC modeling. It abides by DSGE standards as defined by Lucas and augmented by Kydland and Prescott.⁴ Both second-generation new Keynesian and RBC modeling belong to the DSGE program of which they constitute two successive transformations.

The different sections of the chapter could be gathered into three parts. In the first one, I outline the monopolistic competition framework and Calvo pricing, the procedure through which price stickiness enters the picture. In the second, I expound the different monetary factors allowing the comeback of the money non-neutrality claim. It starts with a retrospective preliminary in which I briefly discuss Sims’s role in paving the way for the new approach. The last part deals with further developments and broader commentaries.

³ A testimony to this ‘old hat’ feeling is that King and Rebelo found it apposite to entitle their contribution to the *Handbook of Macroeconomics* “Resuscitating Real Business Cycles” (King and Rebelo 2000).

⁴ As stated by Kimball when motivating the new approach, “I will argue that a hybrid model should be taken much more seriously – a model following the RBC paradigm as closely as possible except for adding what is logically necessary in order to graft in sticky prices” (Kimball 1995: 1241). In the same vein, Galí portrays second generation new Keynesian models as having a “core structure that corresponds to an RBC model on which a number of elements characteristic of Keynesian models are superimposed” (Galí, 2008: 2).

TABLE 18.1 *The differences between first- and second-generation new Keynesian modeling strategies*

	First-generation new Keynesian modeling	Second-generation new Keynesian modeling
Main object of study	Unemployment or underemployment	Optimal monetary policy
Dynamic approach?	Mostly static	Dynamic
General equilibrium?	Incomplete general equilibrium	General equilibrium
Market clearing	Market non-clearing (in the matching of supply and demand meaning)	Market clearing
Standpoint on neoclassical synthesis	Defense of the eclectic vision	Defense of a homogeneous Walrasian-style macroeconomics

A NEW FRAMEWORK: MONOPOLISTIC COMPETITION WITH STICKY PRICES

Monopolistic competition

Monopolistic competition theory originated with Edward Chamberlin’s 1933 book, *The Theory of Monopolistic Competition*. As the title suggests, Chamberlin wanted to build a theory combining competitive and monopolistic aspects – large numbers of firms and free entry on the one hand, the uniqueness of each product and firms’ ensuing market power, on the other. Although it had some success, Chamberlin’s theory did not become the dominant tool of the industrial organization community. According to Brackman and Heijdra (2004), the first reason for this was Chamberlin’s inability to come up with a clear workable model embodying the key elements of his theory. The second reason was bad timing; Chamberlin’s book was somewhat eclipsed by the Keynesian revolution. Moreover, it encountered fierce opposition from different of sides. Friedman and Stigler unrelentingly attacked monopolistic competition theory on both conceptual and empirical grounds (more on this below).

The fate of monopolistic competition changed thirty-five years ago when Dixit and Stiglitz were able to resuscitate Chamberlin’s insight in what became known as the ‘Dixit-Stiglitz’ model (see Box 18.1). This model became widely used in various branches of economics – trade, economic geography, and growth. Ironically enough, while this model is essentially about product differentiation, product differentiation theorists were less enthusiastic about it than economists from other sub-disciplines (Archibald 1987). The Dixit-Stiglitz model was also adopted in macroeconomics. Blanchard and

Kiyotaki ([1987] 1991) used it in a static framework. Hairault and Portier (1993) were the first to cast monopolistic competition in a dynamic framework in a model combining technological and monetary shocks and striving at comparing business fluctuations in France and the United States.⁵

BOX 18.1 The Dixit-Stiglitz monopolistic competition model

Preferences

The utility function is specified as follows:

$$u = U[c_0, V(c_1, \dots, c_n)],$$

where utility bears on the consumption of a numéraire commodity, c_0 , and on a sub-utility function bearing on the consumption of an unbounded number of i substitutable commodities indexed from 1 to n . This second sector can be called “manufacture.” The sub-utility function V is defined over the consumption of all possible varieties, including those that may exist only virtually.

It is assumed that u is separable in the two types of goods and homothetic in both arguments. Three further restrictions are made: (a) symmetry of V in the c_i goods, (b) CES specification for $V(\cdot)$, and (c) Cobb-Douglas specification for $U(\cdot)$. With restriction (c), equation (1) becomes:

$$u = c^{1-\alpha} V^\alpha, \quad V = \left[\int_{i=1}^n [c_i^\rho d_i] \right]^{1/\rho}$$

where α is the share of nominal income, Y , spent on manufactures; ρ ($0 < \rho < 1$) is a diversity parameter, measuring the ‘love for variety.’ Were $\rho = 1$, all the varieties would be perfect substitutes.

Instead of using ρ , it is convenient to use the parameter σ , the elasticity of substitution between any two varieties. It is related to ρ as follows:

$$\sigma \equiv 1/(1-\rho), \text{ hence } \sigma > 1.$$

⁵ Instead of assuming the other leg of DSGE modeling, Calvo pricing, Hairault and Portier adopted a quadratic adjustment cost function, the drawback of which was that the cost of adjustment varied with the change in price envisaged.

BOX 18.1 (cont.)

Demand

If p_i is the price of c_i , the manufacturing price index, assumedly strictly increasing in its arguments, is defined as:

$$P \left[\int_{j=1}^n p_j^{1-\sigma} \right]^{1/1-\sigma}$$

Utility maximization leads to n demand functions; demand for i is log-linear in its own price and in total spending on manufactures, both deflated by the manufacturing price index:

$$c_i = \alpha \left(\frac{p_i}{P} \right) \frac{Y}{P}$$

As it is further assumed that firms are atomistic and take Y and P as fixed, demand functions are of the constant-elasticity type with the elasticity equal to σ .

Production

It is assumed that the production of all manufactures involves a fixed set-up cost, FC , and a constant marginal cost, MC . They are assumed to be identical for all firms. Internal increasing returns ensue. The average cost is equal to $(MC + FC)/x$.

Equilibrium

Firms have market power, the extent of which is indicated by σ . Firms' markup is $\sigma/(\sigma-1)$. It is constant, depending exclusively on the elasticity of substitution. The symmetrical equilibrium price is:

$$p = \frac{\sigma}{\sigma-1} MC$$

As long as production yields a positive profit, new firms will enter the industry, starting the production of a non-existing variety. As consumers reallocate their expenditures, the quantity of every variety purchased diminishes. Rising average costs lead to a decrease in profits. Equilibrium occurs when the marginal firm breaks even. Symmetry results in this being the case for intra-marginal firms as well. The equilibrium level of output is equal to:

BOX 18.1 (cont.)

$$c = (\sigma - 1) \frac{FC}{MC}$$

For the equalities between marginal revenue and marginal cost and price and average cost to be jointly satisfied, the demand and average cost curves must be tangent, as shown in Figure 15.1.

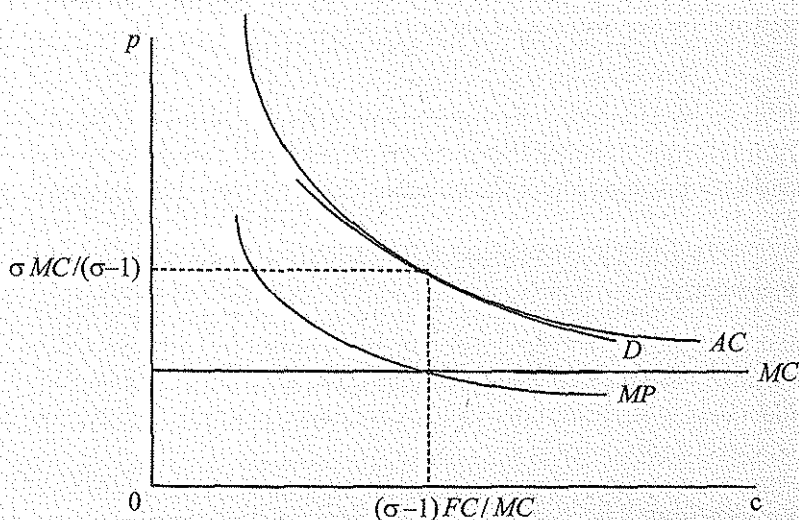


FIGURE 18.1 The Dixit-Stiglitz monopolistic competition model⁶

Calvo pricing

While RBC modeling rests on a supply-side determination of activity, Keynesian economists were eager to give prominence to the demand dimension by stressing that demand activation, acting as the remedy for some imperfection, can have a durable effect on activity. Earlier on, wage rigidity or wage sluggishness, either real or nominal, was taken as the emblematic imperfection. Here, the same type of defect is invoked except it now bears on the nominal price of goods. Calvo pricing has been the most used channel for importing

⁶ Drawn from Neary (2004).

such an occurrence into the monopolistic competition framework (Calvo 1983).⁷ Its basic feature is that prices are not synchronized across firms. The underlying assumption is that at each period of exchange only a given proportion of all firms are allowed to change their prices, this authorization being randomly determined. If, for instance, this proportion is one-third, then on average firms will only be able to reset their prices once every three periods.

Calvo pricing may well be a clever and convenient solution, but its wide acceptance in an approach predicated on microfoundations comes as a surprise. It could have been indicted as not being directly theoretically consistent, and hence discarded, but this did not happen, at least until recently. According to Simon Wren-Lewis (2007, 2009), the reason why it was nonetheless accepted is that it was deemed to be a proxy for the more solidly grounded menu cost model *à la* Rotemberg.⁸

SIMS ON MONEY NON-NEUTRALITY

Sims's name was evoked in Chapter 12 in relation to his skeptical attitude towards the Lucas Critique. Here, I want to dwell on his contributions to the issue of the real effects of monetary changes. A first step in this theoretical journey, taking place in the early 1970s, was his attempt at assessing the controversy between Keynesians and monetarists. The latter argued that central banks should conform to the monetary rule of a constant increase in money and that such a policy would reduce business cycle fluctuations. If this was true, future money growth should exert no influence on current income once the impact of current and past money growth on income had been taken into account. Using Granger causality, Sims came to the conclusion that future money growth did not help predicting current income (Sims 1972). Thus, the causal link seemed to be similar to the one defended by monetarists. Coming from an economist with a Harvard PhD with no connection with Chicago, this conclusion stirred strong reactions. "So, there was a lot of artillery brought to bear against the conclusions in my paper" (Sims's interview by L-P. Hansen [2004] 2007: 213).

However, Sims's views changed over time. First, he grew dissatisfied with both the Keynesian and monetarist way of positing the issue, which he considered basically similar. Both assumed that the money supply was exogenous while postulating that governments controlled it. Their common flaw was to fail to translate the policymaking idea into a policy equation behavior. As for

⁷ The introduction of Calvo pricing in a monopolistic competition framework was achieved by Yun (1996), one of Woodford's students.

⁸ "At first sight, [Calvo pricing] seems like a contract story rather than coming from menu costs, and it appears to share the inconsistency problems of assuming fixed contracts. However again it is possible to tell an *as if* story: the model works as if firms face menu costs, which are sometimes important enough to keep prices fixed, but not always" (Wren-Lewis 2009: 18).

his earlier conclusion, a student of his, Yash Mehra, tested the opposite causal link in a model where money was the dependent variable and interest rates and output the shock. Mehra found that the equations passed the test for the exogeneity of the last two variables (Mehra 1978). This led Sims to realize that drawing causal direction from predictive power can be misleading. He also decided to study what happens in systems in which the monetary shock consists in a change in the interest rate rather than in the money basis (Sims 1980). In this new context, money supply turns out to be endogenous, a radical change from both the monetarist and the Keynesian conceptions. This also means that it has a predictable real effect. It was this predictable effect that Sims had detected in his 1972 paper. The non-predictable part was to become the monetary policy shock which second-generation new Keynesian economists decided to focus on.

BRINGING MONEY AND MONETARY POLICY BACK TO THE FOREFRONT

While Keynesian macroeconomists, on the one hand, and Friedman and Lucas on the other, diverged on the issue of whether monetary expansion could durably expand the level of activity, they nonetheless agreed on the view that monetary changes exerted a short-period impact on real variables. By contrast, Kydland and Prescott boldly contended that money was as neutral in the short period as it was in the long period. To many economists, including several who had few qualms with the RBC approach on other scores, this standpoint was hard to swallow. For example, Taylor referred to the years during which RBC modeling held sway as a “dark age” (Taylor 2007).⁹ The reaction took the form of a stream of papers attempting to empirically bring out the presence of shocks of a new type exerting an impact on the course of the real economy, monetary policy shocks.

However, this striving to reintroduce money into macroeconomic analysis did not mean a return to the line that Friedman had taken. Years of lobbying by Friedman had led a few central banks to adopt his money growth rule. As documented in Chapter 4, the result of these experiments was disappointing: once a given monetary aggregate was chosen as the policy target, it started to

⁹ Taylor expressed his discontent with RBC modeling in the following way: “I find this extreme view far from reality. Even if we ignore the evidence of Friedman and Schwartz for the Great Depression in the US there seem to be problems with the extreme business cycle view. I find it difficult to explain the 1981–82 recession without a reference to the role of the Federal Reserve Board in attempting to reduce the rate of inflation. I also find it difficult to explain differences between economic fluctuations in the US and Japan without reference to differences in nominal wage rigidities and monetary policy. Finally, there are other factors about the business cycle, the correlations between prices and output, that the real business cycle cannot explain” (Taylor 1989: 188).

move erratically – the famous so-called Goodhart’s Law (Goodhart 1981). From a more theoretical viewpoint, Friedman’s advocacy was underpinned by the assumption that the velocity of circulation of money was reasonably stable and predictable, an assumption that, as seen, ended up to be disconfirmed. Therefore, along with the return of money came a new way of posing issues.

One aspect of this renewal was the realization that earlier on scant attention had been paid to what may go on in central bankers’ minds. It was as if, for decades, economists had remained under the spell of a remark made by Klein in the 1950s in his Cowles Commission monograph:

Economists have formulated no laws of behavior which the Federal Reserve Board will obey in making its decisions as to the supply of money. It is possible that some social theory might explain the behavior patterns of the Federal Reserve Board, but this theory would probably be so complicated that we should have difficulty in making use of it, even if we could develop it. (Klein 1950: 3)

In Klein’s time, avoiding the problem was probably judicious, but after so many decades, this ceased to be true. Economists decided to tackle the issue of the behavior of central banks more in line with their current practice. Thereby a new perspective opened. The money growth rule, as advocated by Friedman, gradually gave way to interest rate rules as the instruments of monetary policy.

The two main leading figures in the process of reconciling monetary theory and macroeconomics were John Taylor and Michael Woodford. Taylor’s work was based on two strongly held beliefs. The first was that sluggishness is a basic fact of life that needs to be part of any valid model, which led him to champion staggered contract modeling.¹⁰ “Perfectly flexible prices and market clearing was something that made little sense to me” (Taylor interview by Snowdon and Vane: Snowdon and Vane 1999: 186). His second belief was that policymaking, monetary rather than fiscal policy, ought to be the central concern of macroeconomics.¹¹ The long-term objective of economic policy, in his eyes, is to keep inflation steady and low, thereby avoiding disturbances in the real economy.

By responding to economic shocks in a systematic fashion, economic policy can offset their impact or influence the speed at which the economy returns to normal. It can thus change the size of the fluctuations. (Taylor [1984] 1986: 159)

¹⁰ While he was among the first macroeconomists to endorse the rational expectations assumption, what Taylor refused, however, was the policy inefficiency conclusion that Sargent and Wallace drew from its adoption.

¹¹ To Taylor, any model that has no precise policy conclusion is of little interest. This is the basis for his dismissal of many new Keynesian models, such as menu cost and efficiency wage models that aim at demonstrating the existence of involuntary unemployment but lack any precise policy conclusion. Cf. his interview with Snowdon and Vane (1999: 198).

Taylor's distinctive traits is that, pushed by a desire to think about economic policy both in theory and in practice, he went back and forth between academic positions (Princeton and Stanford) and stints in the policy apparatus (two terms at the Council of Economic Advisers, Research Adviser at the Philadelphia Federal Reserve Bank, and Undersecretary for International Affairs at the U.S. Department of Treasury under the G. W. Bush administration).

As for Woodford, his originality is threefold. First, he studied law, obtaining Juris Doctor from Yale, before getting a PhD in economics from MIT. From this law education, he has retained the desire to address questions of public policy.¹² Second, before specializing in monetary theory, his main field of research was multiple equilibria and sunspot models, that is, qualitative complex general equilibrium modeling. Third, in an age when most macroeconomists only write papers, Woodford did not shy away from penning an eight-hundred-page book, *Interest and Prices: Foundations of a Theory of Monetary Policy* (Woodford 2003).

Woodford's ambition was to construct a theory abiding by three requirements: (a) respecting Lucas's methodological principles, (b) being empirically plausible, and (c) providing central banks with guidelines for their actions. As far as central banks are concerned, his judgment was less pessimistic than Friedman's. Abandoning the view that central banks "could not be relied upon to take the public interest to heart" or "did not know what they were doing," Woodford had no qualms about assuming that they are devoted to the public good. The problem, he declared, is less that they care for their own interest than the fact that, in a changing world where money no longer has a metallic anchorage, their job has become more difficult.

The Taylor rule

Among the different attempts at devising new monetary rules, the winner was the 'Taylor rule.' It started as a proposal made by Taylor in a paper presented at the 1992 Carnegie-Rochester Conference. Taylor introduced it modestly as an empirical regularity rather as a theoretical conjecture, a "hypothetical but representative policy rule that is much like that advocated in recent research" (Taylor 1993: 197). In this inaugural paper, Taylor underlined the descriptive performance of his rule: "What is perhaps surprising is that this rule fits the actual policy performance during the last few years remarkably well" (Taylor 1993: 202). It did not take long for Taylor's observation to become famous and to become gradually transformed into a policy prescription. A reaction

¹² As Woodford stated in his interview by Parkin, "I am able to address questions of public policy, which is what originally drew me to law, but in a way that also allows me to indulge a taste for thinking about what the world might be like or should be like, and not simply the way that it already is" (Parkin 2002: 702).

function, the Taylor rule has the short-period nominal rate of interest of the central bank (r) as its policy instrument. The rule stipulates that it should be fixed in response to the output gap (the difference between effective, y_t , and potential output, y^*) and the inflation gap (the difference between the observed inflation rate, π_t , and the desired inflation rate, π^*):

$$r_t = \pi_t + \alpha_1(y_t^t - y^*) + \alpha_2(\pi_t - \pi^*)$$

Taylor's original formulation of the rule was:

$$r = 2 + .5y + .5(p-2) + 2$$

In this equation, r is the FED funds rate, p the rate of inflation over the previous four quarters, y the percent deviation of real GDP from trend real GDP (2.2% per year for the period considered, 1984.1-1992.3). Taylor assumed that the inflation target was 2.0 percent, on a par with real GDP growth. The rule states that the Federal funds rate ought to be raised if inflation is above target and/or if the output gap is positive. "If both the inflation rate and real GDP are on target, then the federal funds rate would equal 4 percent, or 2 percent in real terms" (Taylor 1993: 202).

The Taylor rule has several advantages. First, it is simple. Second, it is put in a way that makes it useful for central bank decision making. Third, its empirical fit is excellent. However, it also has a few drawbacks, the result of its pragmatic character: it is backward-looking, it has no microfoundations and no stochastic component.

One implication of the Taylor rule is a new tradeoff, which became encapsulated in the so-called Taylor curve, between the variability of output and that of inflation. While the Phillips tradeoff is temporary, the Taylor tradeoff is deemed to be permanent. As a result, "policymakers can choose the degree to which monetary policy is used to buffer the unemployment rate against non-fundamental disturbances" (Chatterjee 2002: 29).

ENRICHING THE MONETARY POLICY RULE WITH MICROFOUNDATIONS

Of Woodford's many contributions to monetary theory, the only one I will comment is his paper, co-authored with Julio Rotemberg, "An Optimization-Based Econometric Framework for the Evaluation of Monetary Policy" (Rotemberg and Woodford 1997). Its purpose was to provide microfoundations for the erstwhile pragmatic formulation of a monetary policy rule.¹³

¹³ "Our main hope with this paper is precisely to shift the debate over optimal monetary policy so that it will involve different optimizing models, all of which fit the data reasonably well, instead of involving equations that fit well by construction but that have only a tenuous connection to

Rotemberg and Woodford's innovation was to import a type of analysis that was standard in public-finance theory into macroeconomics by declaring that the objective of monetary policy was to maximize the agents' welfare in the economy. Although a rather trivial statement, up to then it had not been considered a compelling basis for monetary theory. Traditionally, the policy makers' objective has been represented by a quadratic loss function in output and inflation minimizing the squared output and inflation gaps.¹⁴ Rotemberg and Woodford's contribution is to have devised a utility-based measure of this loss.

This vision of the role of a central bank implies that the central bank must be as knowledgeable about the economy as the modeler economist is. In the context of DSGE modeling strategy, this means that it must be able to reconstruct the representative agent's optimizing conditions, including her response to any policy she may decide on and opt for the policy that ranks first in terms of the representative agent's welfare.

As long as a perfect-competition/flexible-price framework is adopted, the problem of formulating a monetary-policy rule remains of secondary importance. But this changes when the economy has a monopolistic-competition/sticky-price structure since then monetary policy faces the crucial task of counterbalancing the negative effects of the distortions associated with this framework.¹⁵ Hence the central role of Calvo pricing in Rotemberg and Woodford's argumentation. In an inflationary context, Calvo pricing causes price and output dispersion across the different producers of manufactured goods (since some of the firms are impeded when it comes to resetting their prices). As a result, the representative household's allocation becomes inefficient. The solution to avoid this welfare loss is to create a situation in which the firms which are authorized to reset their prices have no incentive to do so. This is the case as soon as inflation is absent; hence the prescription that the mission of monetary authorities is to impede the rise of inflation.¹⁶ If this is the case, price dispersion is also absent. All firms produce the same amount of

explicit behavioral hypotheses at the microeconomic level" (Rotemberg and Woodford 1997: 343-344).

¹⁴ According to the traditional account, the objective of the central bank is to minimize

$$\frac{1}{2} E_t \left[\sum_{i=0}^{\infty} \beta^i (\alpha x_{t+i}^2 + \pi_{t+i}^2) \right]$$

where x is the output gap and π the inflation gap; $\alpha > 0$ expresses the importance given by the central bank to activity as compared to inflation.

¹⁵ To Rotemberg and Woodford, the analysis of monetary policy is mainly concerned with the sticky-price distortion, leaving the loss associated with monopolistic competition to be solved through fiscal policy.

¹⁶ As Gali (2008) pointed out, what is needed is "a policy that stabilizes marginal costs at a level consistent with firms' desired markup, *given the prices in place*. If that policy is expected to be in place indefinitely, no firm has an incentive to adjust its price, because it is currently charging the optimal markup and expects to keep doing so in the future without having to change its price" (Gali 2008: 75).

manufactured goods and the overall economy behaves as if prices were flexible. Current output coincides with its natural level (although it still fails to be efficient because of the monopolistic competition structure). Thereby, the old view that price stabilization matters is vindicated anew.

While the overarching aim of this type of monetary policy is to stabilize the price level, this is not the whole story. In Rotemberg and Woodford's model, the quest for inflation stabilization also has an output stabilization result. As Woodford commented later:

This is because . . . the time-varying efficient level of output is the same (up to a constant which does not affect the basic point) as the level of output that eliminates any incentive for firms on average to either raise or lower their prices. . . . Furthermore, because of the difficulty involved in measuring the efficient level of economic activity in real time – depending as this does on variations in production costs, consumption needs, and investment opportunities – it may well be more convenient for a central bank to concern itself simply with monitoring the stability of prices. (2003: 13)

If the central bank's feedback rule of keeping inflation constant also minimizes the variability of the output gap, there is no tradeoff between inflation and output, contrary to the view conveyed by the traditional Phillips curve. Blanchard and Gali dubbed this a 'divine coincidence' (Blanchard and Gali 2010: 16). However, such a result is too good to be true. It is a small wonder then that it was soon challenged.¹⁷

THE BASELINE SECOND-GENERATION NEW KEYNESIAN MODEL¹⁸

The new Keynesian baseline model is an outgrowth of the RBC baseline model. Like the latter, it boils down to laying bare how the representative agent solves a dynamic optimization problem under given resource constraints, her decision rule being encapsulated in an Euler equation. Of course, the presence of monopolistic competition accompanied with temporary nominal price rigidities, and of a monetary authority makes the solution more complex, as equilibrium conditions have to be found for the representative agent, firms and the monetary authority. The main originality of second-generation new Keynesian modeling consists in having recast each of these optimizing conditions as the aggregate demand equation, the new Phillips curve equation and a modified Taylor rule equation, respectively. The first two of these are a log-linearization around the steady state of the decision rules of the representative agents and manufacturing firms. The third one describes the central bank's optimal

¹⁷ Erceg, Henderson, and Levin (2000), where Calvo pricing is extended to wage formation, testifies to this.

¹⁸ This section is based on Clarida, Gali, and Gertler (1999) and Gali and Gertler (2007). Other useful references are Ireland (2004), Gali (2008), and Duarte (2012).

monetary policy rule. The result is a system of equations that is branded as a modernized dynamic version of the old IS-LM model:

Our baseline framework is a dynamic general equilibrium model with money and temporary nominal-price rigidities. . . . It has much of the empirical appeal of the traditional IS/LM model, yet it is grounded in dynamic general equilibrium theory, in keeping with the methodological advances in modern macroeconomics. (Clarida *et al.* 1999: 1664–1665)

The model studies the deviations of a closed economy from its deterministic steady-growth trend.¹⁹ It assumes complete asset markets allowing idiosyncratic shocks to be insured. All variables are in logs. There is a natural level of output, z_t , which would exist if wages and prices were fully flexible. The output gap, x_t , is the deviation in output, y_t , from its natural level:

$$x_t \equiv y_t - z_t$$

The aggregate demand equation states that the present output deviation depends positively on the expected next period deviation and negatively on the expected future path of inflation, taking possible demand shocks over time into account. This resembles the old IS function – hence the “intertemporal IS equation” or “expectational IS curve” appellation:

$$x_t = E_t x_{t+1} - \frac{1}{\sigma} [r_t - E(\pi_{t+1})] + g_t \quad (18.1)$$

where r_t is the nominal interest rate, σ the constant intertemporal elasticity of leisure substitution, (see Box 9.1), and π_t the period t inflation rate.²⁰ g_t is a stochastic disturbance term:

$$g_t = \mu g_{t-1} + \hat{g}_t,$$

with $0 \leq \mu \leq 1$, and \hat{g}_t a white noise (with constant variance and zero mean). g_t is a function of expected changes in expected government spending relative to expected changes in the natural level of output. Thus, g_t can be interpreted as a demand shock.

Equation (18.1) conveys two main features. First, because σ is positive, when expecting a decrease in prices ($E(\pi_{t+1}) < 0$), agents postpone their consumption to the next period and x_t decreases, it being assumed that the substitution effect dominates the income effect. Second, it shows how the central bank can exert an impact on real variables by modifying the short-term nominal interest rate. This triggers a change in the real interest rate leading agents to change their optimizing decisions.

¹⁹ While, for convenience, Clarida, Gali, and Gertler abstracted from investment, Gali, and Gertler's paper took it into account. I follow the Clarida *et al.* line.

²⁰ $r_t - E(\pi_{t+1})$ is the Fisher relation defining the real interest rate.

The second equation represents the ‘new Phillips curve,’ a forward-looking version of the traditional one. It relates the current price level to the output gap and to expectations about future inflation:

$$\pi_t = \lambda x_t + \beta E_t \pi_{t+1} + u_t. \quad (18.2)$$

where $\lambda < 0$, $0 < \beta < 1$; $u_t = \rho \mu_{t-1} + \hat{u}_t$, with $0 \leq \rho \leq 1$; \hat{u}_t is a random variable with zero mean designating shocks other than excess demand. Gali and Gertler call them cost-push shocks.

This equation is a log-linearization around the steady state of the aggregate pricing decision of those firms which are allowed to change their prices. Their prices are set on the grounds of a weighted average of current and expected marginal costs. Other firms, whose prices remain unchanged, adjust output when their price exceeds marginal cost. It can be shown that an increase in the expected rate of inflation ($E(\pi_{t+1}) - \pi_t$) increases the output gap. Moreover, as shown by Clarida, Gali, and Gertler (1999: 1667), iterating this equation reveals that current output depends positively on expected future output and inversely on the real interest rate.

Of the differences between the old and the new Phillips curve, one is particularly noteworthy. The hallmark of the Phillips curve is the existence of a short-term trade-off between inflation and real activity. In its traditional version, whenever expectations are introduced, the expectation term entering the Phillips relation is $E_{t-1}(\pi_t)$. In this context, to reduce current inflation, current activity must decrease. In the new Phillips curve, the expectation term is $E_t(\pi_{t+1})$. It is expected future inflation that matters. Therefore the trade-off between current inflation and activity is absent (at least as long as movements in u_t , the cost-push term, are also absent and as long as the central bank’s commitment to stabilizing current and future prices is credible).²¹

Finally, the third equation is a modified Taylor rule, describing the way in which a central bank should fix the nominal interest rate:

$$r_t = \nu \cdot r_{t-1} + \phi \cdot \pi_t + x_t + \varepsilon_t \quad (18.3),$$

where ν indicates the degree of interest rate smoothing, ϕ is the relative weight that the central bank attaches to the inflation gap, and ε is a monetary shock.²² The monetary authority is expected to adjust the nominal interest rate more than proportionally to a change in inflation. The ensuing increase in the real interest rate allows the economy not to go on an off-equilibrium path. If the monetary authority acts in this way and if it is furthermore assumed that, when

²¹ However, there is a tradeoff between the acceleration of inflation and activity.

²² Having the nominal rate of interest as a policy instrument makes constructing an LM equation unnecessary.

devising its fiscal policy, the government respects its intertemporal budget constraint, a unique equilibrium solution will prevail.²³

MONETARY POLICY SHOCKS

The notion of monetary policy shock requires an explanation. Indeed, it seems to be an oxymoron since a policy is usually conceived as a reaction to some occurrence. The opening gambit for this notion can be traced back to Leeper's proposal to reverse the usual asymmetry, in which random disturbances affect private agents' behaviors while policy authorities obey deterministic rules, by assuming that private agents' decisions are deterministic while policy authorities follow ad hoc rules with random terms (Leeper 1991: 135). Therefore, error terms need to be included in the policy rules. Leeper described their causes rather offhandedly, mentioning factors related to the technology for implementing policy choices, as present in Dotsey and King (1983) under the name 'control errors.' Alternative factors mentioned by Leeper were policy makers' incentives and policy responses to non-modeled shocks or shocks whose origins lay outside economics.

Monetary policy shocks are thus "changes in policy that could be interpreted as autonomous, that is, not the result of the central bank's response to movements in other variables" (Gali 2008: 8), such as a change in the nominal interest rate unrelated to changes in the output gap or inflation. This characterization is rather vague and suggests an occurrence of secondary importance. It is a fact, nonetheless, that monetary policy shocks came to take center place in new Keynesian modeling.

Building on Rotemberg and Woodford's 1997 article, Christiano, Eichenbaum, and Evans played a leading role in developing this line of research. Their 1999 article, "Monetary Policy Shocks: What Have We Learned and to What End?," pursued two aims. The first was to identify monetary policy shocks. These shocks need to be disentangled from the normal behavior of central banks, in other words, their reactions to non-monetary developments in the economy. Christiano, Eichenbaum, and Evans (1999) made the 'recursiveness assumption,' stating that monetary policy shocks are independent from the information set of the monetary authority. That is, in reference to the equation $r_t = f(\Omega_t) + \varepsilon_{rt}$ (where r_t is the nominal interest rate, f is a linear function representing the feedback rule, Ω_t the Federal Reserve Bank's information set and ε_{rt} the monetary policy shock), the variables in Ω_t are assumed not to respond immediately to a monetary policy shock. Two other ways of identifying monetary policy shocks were proposed by Ben Bernanke (1986) and Sims (1986), on the one hand, and by Christina Romer and David Romer (1989), on

²³ A unique equilibrium solution can still prevail even when the government violates its intertemporal budget constraint on the condition that the central bank allows a higher inflation rate.

the other. Romer and Romer's 'narrative method' consisted in looking at evidence drawn from the historical record rather than statistical evidence, in particular by examining the minutes of the Federal Reserve Bank's policy deliberations. Romer and Romer's purpose was first to identify episodes testifying to shifts in monetary policy which could not be considered a reaction to what happened in the real economy, and second to assess whether such shocks translated into changes in real variables.

Christiano, Eichenbaum, and Evans's second aim was to empirically assess the reactions of endogenous variables to these monetary policy shocks. To this end, they adopted the structural VAR method of imposing restrictions based on economic theory results – in the case in point, the recursiveness assumption. They approached the data using impulse response functions.²⁴ Taking the case of a contractionary monetary policy shock, they were able to bring out the following stylized facts: (a) the federal fund rate initially rises before returning to its initial value after a time span of six quarters; (b) aggregate output falls, reaching a trough after six quarters, and still remaining below its initial value after fifteen semesters; (c) the price level initially responds very little, starting to decrease only in the sixth quarter, which suggests the existence of price rigidity; (d) real wages decrease modestly; and (e) profits drop. Note that (b) and (c) indicate the existence of a trade-off between an immediate decrease in output and a deferred decrease in inflation. The inverse effects hold for an expansionary monetary policy shock.

NEW KEYNESIAN MODELS WITH MULTIPLE DISTORTIONS

RBC modeling cannot replicate the stylized facts above, but what about the new Keynesian baseline model? That is, do the impulse response functions of the monetary shock resulting from the log-linearization of the new Keynesian baseline model match those estimated from the data by the VAR exercise? The answer is 'No.' As it stands, the baseline model does not deliver: (a) it fails to exhibit the hump-shaped pattern displayed in the data, (b) it predicts that inflation and output peak at the same time as the shock, and (c) it lacks persistence.²⁵

To solve the issue, Christiano, Eichenbaum, and Evans (2005; first circulation in 2001) decided to expand the baseline model by increasing the number of distortions. Monetary policy shocks remained the only source of uncertainty. They also kept the recursiveness assumption. But many additions to the baseline model were brought in. To begin with, their model comprises financial intermediaries and monetary and fiscal authorities. It extends the monopolistic

²⁴ An impulse response function describes the result of plugging a shock on the economy supposedly in equilibrium and studying the path taken by the variable under study in returning to the original equilibrium.

²⁵ Cf. Estrella and Fuhrer (2002).

structure to labor. Following Christopher Erceg Dale Henderson and Andrew Levine (2000), they also assumed Calvo-style pricing for wage contracts as for price contracts.²⁶ This last change turned out to play a central role in the conclusions of the model – according to Christiano *et al.*, “the critical nominal friction in our model is wage contracts not price contract” ([2001] 2005: 2). Finally, four further additional assumptions were made: introducing (a) past consumption as an argument in the utility function, that is, habit formation in preferences for consumption, (b) adjustment costs in investment to the effect of smoothing capital accumulation, (c) variable capital utilization, and (d) the assumption that firms must borrow working capital to finance their wage bill. As far as parametrization was concerned, like Rotemberg and Woodford (1997), Christiano *et al.* adopted an eclectic approach. The numerical value of some parameters, for which empirical evidence seemed compelling, was obtained by calibration. For the others, they resorted to the structural VAR method.

All these changes were geared toward obtaining a better match between the impulse-response functions resulting from the log-linearization of the model and those estimated from the data. Christiano, Eichenbaum and Evans claimed that the fit between the model and the empirical data significantly increased. Just to refer to output and inflation, and to borrow their words:

The model succeeds in accounting for the inertial response of inflation. Indeed, there is no noticeable rise in inflation until roughly three years after the policy shock. . . . The model generates a very persistent response in output. The peak effect occurs roughly one year after the shock. The output response is positive for nine quarters, during which the cumulative output response is 3.14 percent. (Christiano, Eichenbaum, and Evans 2005: 21)

The drawback, however, is that the number of distortions increases significantly from the baseline model, making the model more complex and thereby drifting away from the parsimony requirement that Lucas and Prescott had favored.

The next step forward occurred when Frank Smets and Raf Wouters (2003) extended Christiano, Eichenbaum, and Evans’s 2005 model and applied it to the euro zone viewed as a closed economy. Using Bayesian techniques, they estimated seven variables (GDP, consumption, investment, prices, real wages, employment, and the nominal interest rate) under ten structural shocks (including productivity, labor supply, investment preferences, cost-push, and monetary policy shocks), not all of which were serially correlated. Their model also incorporates the distortions introduced by Christiano *et al.* ([2001] 2005)

²⁶ Households are monopoly suppliers of a differentiated labor service. Each household sells a differentiated labor service to a representative, competitive firm that transforms it into an aggregate labor input L_t . The aggregate wage is given; households set wages in a similar staggered way as firms. In each period, a household faces a constant probability of being able to re-optimize its nominal wage.

(sticky prices and wages, habit formation, cost of adjustment in capital accumulation, and variable capital utilization). As in the Kydland-Prescott model, the average labor productivity is procyclical, but now the source of this lies in the introduction of fixed costs of production.

The Smets-Wouters model was tremendously successful in particular within the research departments of central banks. Until then, the penetration of RBC modeling in central banks had remained thin: they continued to use models that, for all their sophistication, remained based on the Kleinian tradition.²⁷ This situation changed rapidly, as many central banks around the world adopted the Smets-Wouters model for their policy analysis and forecasting (without, however, abandoning the traditional Keynesian model!).

A SYNTHESIS BETWEEN NEW KEYNESIAN AND RBC MODELING

In Chapter 2, I discussed the notion of theoretical synthesis. There, I argued that it should be narrowly understood as designating the outcome of a process whereby two theoretical frameworks, which at a certain stage were viewed as unrelated, are integrated. In other words, a synthesis consists in a new theory merging some elements from the two separate ones at the cost of leaving other elements aside. I claimed that, as far as the traditional Keynesian and Walrasian paradigms were concerned, no such synthesis had been solidly achieved. I also contended that what came to be called the neoclassical synthesis was the opposite of a synthesis, as it consisted in defending the idea that Keynesian and Walrasian theory deserve to live side by side even if they cannot be integrated. Here we face a different state of affairs because a synthesis has effectively taken place. It brought together the new Keynesian and the RBC research lines.²⁸

New Keynesians' contribution to the synthesis was monopolistic competition and sluggishness, as well as a focus on the role of the central bank. In exchange, they accepted the basic tenets of RBC (and hence DSGE) modeling. They relinquished the aim of demonstrating market non-clearing results, in particular involuntary unemployment. They also ceased to support the neoclassical synthesis viewpoint. As for RBC economists, they abandoned the perfect-competition/flexible-price framework as well as the classical dichotomy. Table 18.2 summarizes these tradeoffs.

²⁷ For example, the model used by the European Central Bank, the Area Wide model (AWM), was still constructed from a neoclassical synthesis perspective. "The model is designed to have a long-run equilibrium consistent with classical economic theory, while its short-run dynamics are demand driven" (Fagan, Henry, and Mestre [2001], abstract).

²⁸ Goodfriend and King rightly perceived the phenomenon, yet misleadingly decided to label it 'new neoclassical synthesis.' Contrary to what they wrote ("We call the new style of macroeconomic research the new neoclassical synthesis because it inherits the spirit of the old synthesis" Goodfriend and King [1997: 255]), there is no lineage between the 'old' neoclassical synthesis and the so-called new neoclassical synthesis. Cf. Duarte and De Vroey (2013).

TABLE 18.2 *The new Keynesian-RBC synthesis*

The new Keynesian side of the synthesis		The RBC side of the synthesis	
<i>Tenets of first-generation new Keynesian modeling relinquished</i>	<i>Tenets retained</i>	<i>Tenets of RBC modeling that were relinquished</i>	<i>Tenets retained</i>
– involuntary unemployment	– imperfect competition	– perfect competition	– the DSGE program and Lucas's standards
– the neoclassical synthesis	– rigidity	– flexibility	
	– money non-neutrality	– money neutrality	

The first column of the table indicates the features of first-generation new Keynesian modeling that the second generation relinquished, the second column those that were kept. The third and the fourth columns do the same for RBC modeling. The end result is that second-generation new Keynesian modeling is neither first-generation new Keynesian nor RBC modeling; it is of a different ilk.

The new synthesis should not be regarded as the result of an explicit compromise made between two groups of economists, new Keynesians and proponents of RBC modeling, who after sitting down together would have decided to settle on a common platform. Rather, the transformation that occurred was gradual and unplanned. The realization that macroeconomics had drifted away from RBC modeling came only as an *ex post* occurrence.

Blanchard, one of those first-generation new Keynesians who endorsed the move towards the second-generation type of modeling, expressed his relief at this reconciliation²⁹ and gave an explanation of its occurrence in the following words:

... After the explosion (in both the positive and negative meaning of the word) of [macroeconomics] in the 1970s, there has been enormous progress and substantial convergence. For a while – too long a while – the field looked like a battlefield. Researchers split in different directions, mostly ignoring each other, or else engaging in bitter fights and controversies. Over time however, largely because facts have a way of not going away, a largely shared vision both of fluctuations and of methodology has emerged. (Blanchard 2009: 210)²⁹

There is no doubt that there is a large part of truth in Blanchard's viewpoint that facts are judge and jury. The confrontation of models with data is central

²⁹ Woodford (2009: 268–289) expresses a similar opinion. However, Mankiw (2006) and Solow (2010) have hesitated to follow suit. They were glad to see price rigidity and market failures return to the forefront of the macroeconomics research agenda, but they remained lukewarm regarding the DSGE program and its methodological Lucasian principles.

to the DSGE program, and it imposes a tremendous discipline on the acceptance of models. Nonetheless, some dose of skepticism is also welcome. In a recent article, "Economics and Reality," Harald Uhlig, an econometrician who teaches at Chicago, wondered whether it is true that theoretical transformations in macro followed from empirical evidence. To this end, he considered four case studies, the Phillips curve, price stickiness, the impact of monetary policy on output, and the cause of business cycles, each of which were deemed to be a "poster child for the success of empirical evidence influencing economic thinking" (Uhlig 2012: 32). Uhlig showed that for each of them, this standard view turns out to be equivocal. Take the case of second-generation new Keynesian models. He noted that, by grafting Keynesian standard demand channels as key drivers of the business cycle onto a real business cycle framework, the Smets-Wouters model has put "root cause of business cycle back to where it was according to Keynes" (Uhlig 2012: 40). No paper looks more empirical than theirs. However, according to Uhlig this return of Keynesian themes should not be taken as the manifestation that facts have at last imposed themselves. He rather suggested that the change occurred "by design of the theory and per views held a priori" (p. 20). Uhlig's point was not that the matter can be sealed in one way or another. Rather, he argued that the ambiguity is pervasive and cannot be lifted. This is also my viewpoint. When studying the emergence of second-generation new Keynesian modeling, it would be preposterous to exclude the factual dimension, but it would be naive to think that it has been the exclusive factor at work.

Another question which comes to mind is 'who won what?' in the realized synthesis. As asked by Wren-Lewis in his blog, *Mainly Macroeconomics*, "Are New Keynesian DSGE models a Faustian bargain?"³⁰ Like Wren-Lewis's, my answer is negative. To me, the synthesis is globally a win-win outcome. However, while both sides may claim victory, it is on different grounds. RBC economists may rejoice at viewing a broad rallying around the Lucas/Kydland and Prescott methodology after years of resistance towards it. Thus, from a methodological viewpoint, the RBC approach can be considered the winner. However, from the viewpoint of the orientation taken and the ensuing results, it is the reverse. The mere acceptance that sluggishness should be a cornerstone of modern macroeconomics constitutes an astonishing comeback – think of the earlier brutal rejection of non-Walrasian equilibrium models on the grounds that they were based on the rigidity hypothesis. Moreover, a (slight) shifting away in policy conclusions from those of RBC models took place. Therefore, the feeling of relief about the reconciliation expressed by economists such as Blanchard and Woodford might also have the flavor of a winners' magnanimity.

³⁰ <http://mainlymacro.blogspot.be/2014/02/are-new-keynesian-dsge-models-faustian.html>

A LACK OF RESISTANCE TOWARDS THE MONOPOLISTIC COMPETITION FRAMEWORK

When Chamberlin initiated the monopolistic competition framework, it faced strong opposition from Stigler and Friedman. Initially, Stigler gave some credit to Chamberlin's insight, endorsing it in the 1946 edition of his textbook, *The Theory of Price*. But he soon changed his mind. In lectures given at the LSE (Stigler 1949), he launched a fierce attack against monopolistic competition, arguing that it was a useless curiosity. To him, the notions of groups of producers and product differentiation upon which it was based evaded any rigorous definition. Stigler's verdict was that there was no need for an intermediary model between perfect competition – whose key theoretical role was to act as a point attractor of the competitive process – and pure monopoly. Friedman fully concurred with Stigler. In "The Methodology of Positive Economics" paper, he declared that the theory of monopolistic competition "possesses none of the attributes that would make it a truly useful general theory" (Friedman 1953: 38), the reason for his indictment being the same as Stigler's.³¹ According to Friedman him, the only thing that Chamberlin's model had going on for it was the false impression it gave that the model was able to bring theory closer to reality.

The rationale for my detour into discussing Friedman's and Stigler's views is that I would have expected Lucas and Prescott, and their followers, to have trodden in their footsteps. This was not the case. Actually, the adoption of the monopolistic competition framework *à la* Dixit-Stiglitz occurred almost inadvertently, as if its introduction was a normal step in the course of theoretical development. As for Prescott, I found almost no writings wherein he expresses his opinion on the subject. An exception is a passage in a 1998 interview by Snowdon and Vane (2005) where he voices an open-minded opinion on this line of research. Asked "How do you view the more recent development of introducing nominal rigidities, imperfect credit markets and other Keynesian-style features into RBC models," his answer was:

I like the methodology of making a theory quantitative. Introducing monopolistic competition with sticky prices has been an attempt to come up with a good mechanism for the monetary side. I don't think it has paid off as much as people had hoped, but it is a good thing to explore. (Snowdon and Vane 2005: 350)

The first sentence of the quotation confirms that Prescott's overarching criterion for appreciating a theory lies in its quantitative character. For the rest, he was rather evasive; nonetheless, he did not close the door on such research.

³¹ "By [Chamberlin's] definition each firm is a separate industry. Definition in terms of 'close' substitutes or a 'substantial' gap in cross-elasticities evades the issue, introduces fuzziness and undefinable terms into the abstract model where they have no place, and serves only to make the theory analytically meaningless" (Friedman 1953: 38–39).

A possible reason for this attitude was the existence of Cole and Ohanian's work (2002, 2004).³² Using an imperfect competition general equilibrium model, they were able to support the conclusion that, far from helping the U.S. economy to overcome the Great Depression, Roosevelt's New Deal policy resulted in prolonging it. This was music to Prescott's ears.

If I am right in believing that, from their own viewpoint, RBC economists (or more broadly any adherent of the Lucasian program) should have been lukewarm with respect to the new direction taken by macroeconomics in the 1990s, one would expect a retort at a later date. Indeed, this happened although not in a frontal way.

CRACKS IN THE CONSENSUS

A first crack in the new Keynesian-RBC consensus was questioning Calvo pricing on both conceptual and empirical grounds. Golosov and Lucas (2007) disputed the view that Calvo pricing is a good proxy for behavior under menu costs. By building a two-shock model (an aggregate inflation shock and a shock idiosyncratic to the firm), they showed that changes in prices are mainly due to the idiosyncratic shocks. Moreover, with such idiosyncratic shocks, they argued, Calvo pricing models generate little persistence. In the same line, several studies questioned real-world rigidity. For example, Bils and Klenow (2004) studied unpublished data from the U.S. Bureau of Labor Statistics (a sample of 350 consumer goods and services representing 70 percent of consumers' expenditures over the years 1995–1997). They concluded that half of the goods displayed prices that lasted 4.3 months or less. This result reduces the impact of earlier ones without deciding the matter. Nonetheless, it vindicates Patrick Kehoe's suspicious remark about price formation in DSGE models:

The serious work on incorporating interesting price rigidities into serious DSGE models capable of confronting the data is still in its infancy. Price rigidities may turn out to be important, but the current models we have for addressing them seem not very promising quantitatively. I see a large number of economists writing papers that take the existing sticky price models as they stand and try to use them to address a number of issues, especially policy issues. I think that this is not a productive use of time. (Kehoe, P.2003: 9–10).

Were a consensus to arise on figures close to those derived by Bils and Klenow, the conclusion could be drawn that, after all, it is defensible for the DSGE program to stick to the flexible-price assumption. As a result, there would no longer be a rationale for the monopolistic-competition framework and the associated stabilization policy.

A second line of attack was launched by a trio of productive and pugnacious Minnesota economists, V. V. Chari, Patrick Kehoe, and Ellen

³² Cf. Pensirosso (2007).

McGrattan in a 2009 article entitled “New Keynesian Models: Not Yet Useful for Policy Analysis.” Its immediate target was the Smets-Wouters model, at the time considered to be the state-of-the-art of new Keynesian modeling, but their criticism extended to the broader practice of second-generation new Keynesian modeling. According to them, the problem with the Smets-Wouters model is that several of its shocks are dubiously structural (Chari, Kehoe and McGratten 2009: 246–247). This is particularly true for shocks to wage markups (i.e., shocks to the labor aggregator covering a continuum of different types of labor services), which are central to Smets and Wouters’s results. Chari, Kehoe, and McGrattan claimed that these shocks are reduced-form rather than structural shocks. They may result either from fluctuations in the bargaining power of unions or from an exogenous shock on household preferences. Depending on the interpretation, distinct policy conclusions ensue. In the first case, the government should take measures diminishing the power of unions; in the second one, it should accommodate the shock. The conclusion drawn by Chari, Kehoe and McGrattan is that “until we have concrete micro evidence in favor of at least one of these interpretations, the New Keynesian model should not be used for policy analysis” (Chari, Kehoe, and McGratten 2009: 255).

Chari, Kehoe, and McGrattan were also skeptical about second-generation new Keynesians rallying to RBC principles. According to them, it still makes sense to draw a methodological opposition between neoclassicists (i.e., them) and new Keynesians. Neoclassicists are parsimonious. They wanted “to keep a model simple, keep the number of its parameters small and well motivated by microfacts, and [put up with the reality that no model can, or should, fit most aspects of the data]” (Chari *et al.* 2009: 243). In their eyes, new Keynesians do not abide by these principles as their priority is to fit the data at all cost, by adding “many shocks and other features to their model and then to use the same old aggregate data to estimate the associated new parameters” (Chari *et al.* 2009: 243). The result is the presence of free parameters, which again leads “to models that simply cannot be relied on for policy analysis” (Chari *et al.* 2009: 243).

In their paper, Chari, Kehoe, and McGrattan did not go as far as declaring that new Keynesian modeling is a dead end. They expressed no qualms about incorporating frictions into RBC modeling, including imperfections in markets. They also emphasized that the policy conclusions obtained in new Keynesian models are much closer to those of RBC economists than to those of old Keynesians. It is just, in their words, that new Keynesians should “resist the urge to add parameters undisciplined by micro data simply because they help the model better fit the same old aggregate time series” (Chari *et al.* 2009: 265). Still, this conciliatory tone should not hide the existence of a deep gulf: what Chari, Kehoe, and McGrattan asked from second-generation new Keynesian economists is nothing other than foregoing the features of their modeling strategy which they were the most keen on. Not surprisingly, new Keynesians

were quick to react to their paper, not by following Chari and his colleagues' recommendation and making amends, but rather by finding a device to overcome the lack of identification of the wage markup shock.³³

MY ASSESSMENT

As second-generation new Keynesian modeling is an outgrowth of RBC modeling, the latter being in turn heir to Lucasian macroeconomics, most of the points I made earlier when assessing them remain valid. Nonetheless, a number of additional remarks have come to my mind.

The limits of monopolistic competition

At first sight, it looks like DSGE macroeconomics took an important step towards realism by adopting the monopolistic competition framework. Instead of having all agents be price makers, we now have price-setting firms, which hold market power and are able to impose a markup on their marginal costs. This move seems to be a perfect example of the standard vision of progress. According to the latter, economic theory must start by describing the economy in the most heroic way, and then introduce imperfections ('frictions' or 'distortions').³⁴ Personally, I think that the introduction of monopolistic competition is only a small step in such a march toward more realism. Take market power: the incidence of its existence is annihilated by the free entry and fixed cost assumptions, the result of which is that profits are equal to zero. Take the notion of imperfect substitution: conceptually, its meaning and relevance has not improved since Chamberlin's time; the only thing that has been achieved is providing a technical definition for it, the Dixit-Stiglitz aggregator. Take agents' preferences: not only is love for variety a heroic assumption, but in addition, it is assumed that agents have a preference for commodities which do not exist yet. Or take the way in which firms are accounted for. As described wittily by Neary, no more evanescent vision of them can be conceived of:

Where entering firms come from, and where existing ones go, is never explained in models of monopolistic competition, any more than it is in models of perfect competition. New firms, exact replicas of existing firms, are assumed ready to spring up like dragon's teeth whenever a tiny profit opportunity present itself and existing firms

³³ See Gali, Smets, and Wouters (2011).

³⁴ Kocherlakota (2010) gives a candid description of this point of view: "Macroeconomists always leave many possibly important features of the world out of their model. It may seem to outside observers that macroeconomists makes these omissions out of choice. Far more often, though, macroeconomists abstract from aspects of reality because they must. At any given point in time, there are significant conceptual and computational limitations that restrict what macroeconomists can do. The evolution of the field is about the eroding of these barriers" (Kocherlakota 2010: 6).

exit without a murmur following any downturn in industry fortunes. (Neary 2004: 177–178)

The conclusion to be drawn is that monopolistic competition paradoxically represents the most minimal departure from perfect competition possible, while nonetheless giving the impression that it is an important step forward in terms of realism. The reason for its wide acceptance must lie elsewhere, namely in its ingeniousness and tractability.

No longer a mono-causal explanation of business fluctuations

While second-generation new Keynesian macroeconomics stems from RBC modeling, at the end of the day, a significant change has taken place concerning the explanation of business fluctuations. Kydland and Prescott provided a straightforward claim about the origin of business cycles, namely that technology shocks were their main cause. Whether or not this is a good answer, it is a one-sentence answer. By contrast, when looking at Christiano, Eichenbaum, and Evans's and Smets and Wouters's models, no such straightforward explanation is present. Rather, a series of joint distortions are invoked. It is as if, in terms of substance not technique, theory has gone all the way back to the eclectic type of explanation of fluctuations to be found in Haberler's seminal *Prosperity and Depression* book (1937).

Revolutionary purity

I have argued that the revolutionary metaphor fits the transformation of macroeconomics spearheaded by Lucas, Kydland, and Prescott. The problem with revolutions nurtured by a high ideal – the ideal here being methodological – is that once success has come, it may not be possible to remain true to the principles at its heart. Often, with the passing of time, revolutionary purity is tarnished, pragmatism taking over. In the early phases of the DSGE program, methodological purity was strong; think of the insistence on parsimony, the *sine qua non* character of microfoundations, the priority given to internal over external consistency, or the trust in the existence of a rock-solid established theory.³⁵ I regard the transition from RBC to second-generation new Keynesian modeling as evidence that these strict rules were loosened (for better or worse). Chari *et al.*'s paper is then a plea for restoring purity.

³⁵ “Ideally a model would be both internally and externally consistent. In reality, perfection is not possible, particularly in macroeconomics. I want to claim that the methodological approach that now characterizes macroeconomics (and which has generated the consensus) holds that internal consistency should never be compromised. Under this view, a model that is internally inconsistent is simply incorrect (and should be rejected), while a model that is externally inconsistent can be tolerated, at least until a better model is found” (Wren-Lewis 2007: 48).

The story behind the model

In Chapter 15, wondering what the story behind the RBC model was, I came to the conclusion that it run about a number of identical autarkical one-person economies, a long way from reality. Now with second-generation Keynesian models, the impression given is that the model economy resembles reality more significantly. There are price-making firms which moreover are declared to hold market power, there is a central bank, and there is an insurance market, all important features of real-world market economies. Thus, the gulf noticed earlier between the model and its real-world equivalent seems to have been filled.

To me, this impression is smoke and mirrors. The story behind the model is the same as in RBC modeling, except it has become more complicated. A first change is that there is an additional layer in the production process. Instead of having the hermits consume a single good, they consume a bundle of varieties of a composite good, purchasing each of these varieties from a distinct firm and assembling them costlessly. A second change is that trade across distinct agents, firms and hermits, takes place. The snag, however, is that firms are no real characters in the story. Their presence is purely technical; they are the purveyors of a technical transformation, supposedly operating efficiently. Nothing would be lost if they were dispensed with, as the story could run that each hermit produces a particular variety which is sold to the other hermits.³⁶ The main point is that the linchpin of equilibrium formation is the same as in the RBC approach: the formation of the equilibrium of the economy and the formation of the representative agent's optimizing plan are part and parcel of each other. The interactive dimension of general equilibrium, which McKensie underlined when stating that general equilibrium amounts to the generalization of individual equilibrium, remains absent as no generalization is needed. As for the monetary authority, its presence and role is also contrived. Its function of stabilizing the price level certainly has the ring of realism, but again this is a smokescreen because the price index of the model is a pure artifact (not mentioning the cashless character of this so-called monetary economy).

The conclusion to be drawn is that, appearances to the contrary notwithstanding, second-generation new Keynesian macroeconomics is no less of a colossal 'as if' enterprise than RBC macroeconomics. Again, I do not claim that this is a sufficient reason for dismissing it, yet there should be no make-believe.

³⁶ Having an unbounded number of hermits surely stretches credibility, but this is also true for varieties or hermits living infinitely.

Progress?

In spite of these critical remarks, I consider that second-generation new Keynesian modeling marks further progress in the development of the DSGE program. Having taken this fork rather than others may be contested, especially because tackling the issue of coordination failures is thereby swept under the rug. But then the fork chosen can be regarded as a detour taken for a reason of tractability. Be that as it may, in view of the developments that took place, it becomes more difficult than thirty years ago, to state that is the DSGE program headed macroeconomics the wrong way. This is especially true when comparing the progress made on this bifurcation with that which occurred in rival ones.

I nonetheless believe that all the limitations I have underlined about RBC modeling remain valid for new Keynesian modeling. The fact that the narrative and the models have become more complex does not change the earlier conclusion that their explanatory power is weak. New Keynesian modeling is no more directly explanatory of reality than RBC modeling. This is the price to pay for having given the priority to internal consistency. The same restrictive judgment must hold for policymaking.

Readiness for policy?

Are Chari, Kehoe, and McGrattan right in proclaiming that, unlike the neoclassical side, new Keynesian modeling is not ready for policy analysis? A problem with this claim is that the meaning of policy analysis is ambiguous. Policy conclusions are conterminous to macroeconomics. Hence it makes little sense to state that a model is not ready for policy analysis. If this line is taken, Chari and his co-authors just mean that the Smets-Wouters model is bad macroeconomic theory (as old Keynesian models were). They also established a logical link between the perfectly structural character of a model and its readiness for making policy recommendations.³⁷ This is why they contended that new Keynesian macroeconomists should not use their models to make policy recommendations to real-world governments: “New Keynesian models are not ready for quarter-to-quarter policy advice” (Chari *et al.* 2009: 264).

I agree with this last proposition, but only in so far as it also applies to Chari, Kehoe and McGrattan’s preferred “neoclassical” modeling strategy! In my view, their non-readiness conclusion applies to the whole DSGE program. Thus, the conclusion in Chapter 17, that economists working in this program, be they neoclassicists or new Keynesians, must remain subdued as far as policy

³⁷ “The practical effect of the Lucas critique is that both academic and policy-oriented macroeconomists now take policy analyses seriously only if they are based on quantitative general equilibrium models in which the parameters of preferences and technologies are reasonably argued to be invariant to policy” (Chari *et al.* 2007: 4).

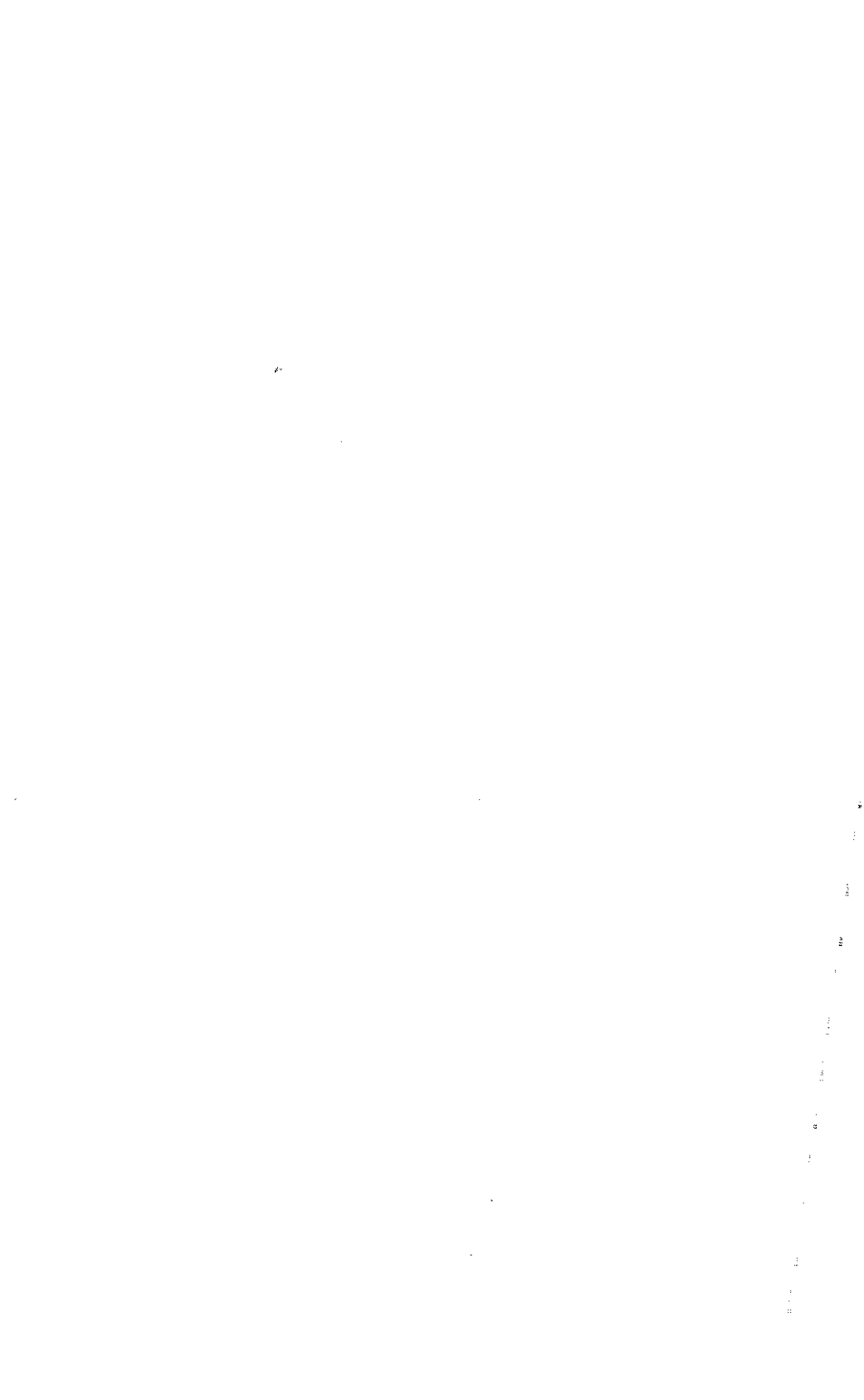
advice is concerned, still holds. They may well express general principles, but then they should admit that these follow from both their ideological vision and their general expertise in unclear proportions.

CONCLUDING REMARKS

In this chapter, I have documented how second-generation new Keynesian modeling grew out of RBC modeling. This occurred gradually up to a point when it became apparent that a new installment of the DSGE program had seen the light of day. At a certain juncture, this development was hailed as a synthesis between two rival approaches, suggesting that earlier ideological and methodological brawls had come to an end. In a later stage, however, cracks in the consensus began to appear.

The irony is that at the very moment when this new modeling strategy was coming close to stabilization, the surge of the 2008 recession brought out its limitations (it being a closed economy model, without heterogeneity, no unemployment, and especially with no financial frictions). The fact that it belongs to the DSGE program means that it has almost nothing to say about states where the economy is outside the 'corridor,' to use Leijonhufvud's expression (2009). Uhlig's following remark aptly captures this frustrating state of affairs:

The nearly successful agenda of constructing reliable DSGE models may be in the process of being abandoned in the halls of academia (though still pursued in institutions close to economic policy). Just when calibration had been replaced by statistical inference, and when questions about structural change were raised in a quantitatively sophisticated and interesting manner, events destroyed the consensus on which these models were built and economic science turned its attention into a different direction. (Uhlig 2011: 23–24)



PART III

A BROADER PERSPECTIVE

The History of Macroeconomics through the Lens of the Marshall-Walras Divide

In this chapter, I argue that the Marshallian and the Walrasian approaches constitute two alternative research programs within the neoclassical school of thought.¹ At first sight, such a claim looks unrelated to the purpose of this book, yet this is untrue. I will show that the Marshall-Walras divide plays a key role for understanding the history of macroeconomics.

THE MARSHALL-WALRAS DIVIDE

Table 19.1 summarizes the contrast and commonalities that exist between the two approaches as they stand in their basic representation. My aim is to draw the reader's attention to the differences between them. It must, however, be noticed that they share some basic features. Three of these are mentioned in the table: (a) they are both based on the subjective theory of value, (b) they adopt the same equilibrium concept, static equilibrium with disequilibrium occurrences gravitating towards the equilibrium allocation, and (c) in both of them market-clearing is a foregone conclusion. Another trait, not mentioned in the table, is that in neither of the two approaches does the labor market receive special attention. For my purpose, it is unnecessary to comment on these commonalities. As for the differences, several of them have already been touched upon in the previous chapters. Therefore, I will zero in on those requiring special attention.

General purpose

Whitaker characterizes Marshall as “an author dubious about the value of unadorned theory and anxious to adapt that thinking to an ever-changing reality” (Whitaker 1990: 220). To Marshall, according to the celebrated

¹ De Vroey (2012) is a more systematic exposition of the Marshall-Walras divide.

TABLE 19.1 *The Marshallian and the Walrasian approaches: Contrasts and commonalities*

	The Marshallian approach	The Walrasian approach
Theory of value	Subjective theory of value	Subjective theory of value
General purpose	Solving concrete issues up to the empirical verification stage	Tackling issues of principle without engaging in empirical work
Methodological priority	External consistency	Internal consistency
Way of addressing complexity	Analysis must start with the study of particular markets; the full piecing together of their results is postponed to a later time (partial equilibrium analysis)	From the start, the analysis must bear on an entire economy, yet the latter should be as simple as possible (general equilibrium analysis)
Representation of the economy	A set of separate markets each of which is an autonomous venue for equilibrium	A grand market involving all agents and commodities in a single transaction
Mathematics	Ancillary role	<i>Sine qua non</i>
Microfoundations	They can be left in the background	<i>Sine qua non</i>
Equilibrium	Static equilibrium concept	Static equilibrium concept (with one caveat)
<u>Trade technology:</u>		
– Price formation	– Agents are price-quantity makers	– Auctioneer hypothesis
– Information	– Perfect information including the knowledge of market S&D functions	– Perfect information excluding the knowledge of market S&D functions
Implication of the trade technology	Possibility of shifting from perfect to imperfect competition	No allowance for imperfect competition
Market clearance	Market clearing exists as a matter of premise	Market clearing exists as a matter of premise

expression, economic theory was an engine for the discovery of concrete truth. His purpose was to explain everyday business, to solve practical issues – such as the question of what impact an increase in demand will have on the price of a particular good.

By contrast, Walras was interested in matters of principle, in questions of a more philosophical nature – in particular, the logical existence and the efficiency of the equilibrium of a decentralized economy, a query that can be traced back to Adam Smith's attempt to elucidate the mechanism behind his invisible hand metaphor. However, Walras addressed this issue at an incomparably higher level

of abstraction than Smith. He was well aware that his theory was about an ideal type, a theoretical parable. As he explained in the Preface of his *Elements of Pure Economics*, Walras believed that the role of theory vis-à-vis reality was to be a foil, an ideal to be attained, not a description of reality (Walras 1954: 71–72).

The following quote from Negishi aptly summarizes the differences between Marshall and Walras for what concerns the basic aim they purported to pursue:

Marshallian models are practically useful to apply to what Hicks called particular problems of history or experience. On the other hand, Walrasian models are in general not useful for such practical purposes. . . . Walras' theoretical interest was not in the solution of practical problems but in what Hicks called the pursuit of general principles which underlie the working of a market economy. (Negishi 1987: 590)

However, the conclusion that Negishi drew from this difference was hardly the one that I myself want to bring out as the following quotation makes clear:

It cannot be denied, in any case, that Marshall's partial equilibrium analysis is an indispensable complement to Walras's general equilibrium analysis in forming the foundations of current mainstream economics. (Negishi 1989: 345)

Addressing complexity

The standard way of differentiating Marshall and Walras is to state that the first engaged in partial equilibrium analysis and the second in general equilibrium theory. This is of course true. However, it is important to note that such choices followed from these authors' perception of the difficulties involved in constructing economic theory.

According to Marshall, in order to break the complexity deadlock one needed, first, to divide the economy into branches of activity to be studied separately, and, second, to separate three time categories. Figure 19.1 illustrates this.²

Marshall's point was not that theory should be confined to the study of a single branch. It was rather that economists needed to proceed gradually. In his fishing industry argument (see Figure 19.2), Marshall studied the gravitational process between temporary (market-day) equilibrium, short-period and long-period equilibrium, relating the outcomes in this industry to changes in demand in the substitute meat branch, thus mixing inter-temporal interdependency as well as the interdependency across two close branches.³ Such a study could be called 'extended partial equilibrium' analysis, but it is not general equilibrium analysis.

² The rectangles represent the branches of the economy during a given time span, the circle surrounding the different branches (from 1 to n) the economy as a whole.

³ Subsequent Marshallian economists have fully adhered to this strategy. See, e.g. Friedman (1951: 114) and Clower and Due (1972: 158).

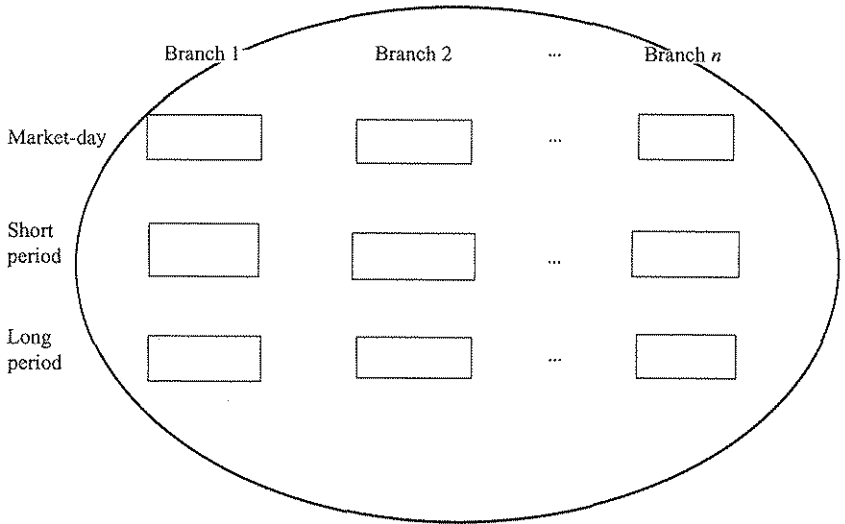


FIGURE 19.1 Marshall's breakdown of the economy into separate elements

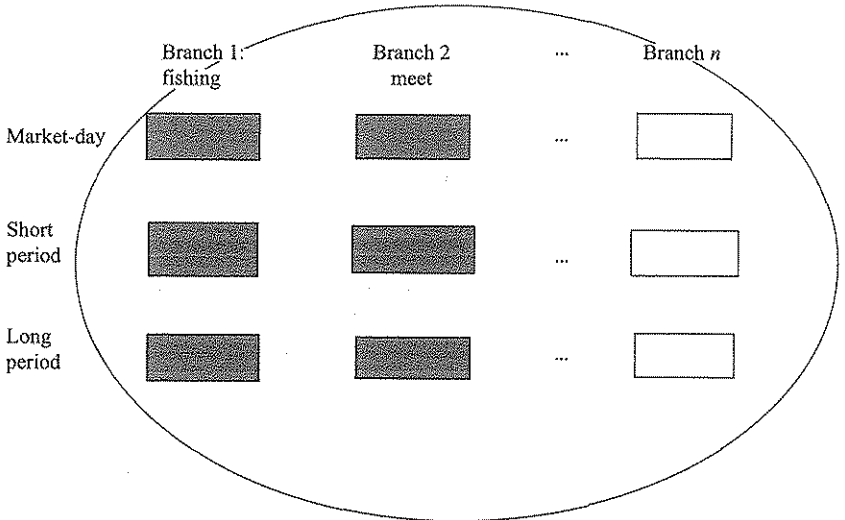


FIGURE 19.2 Marshall's fishing industry example

No such two-tier strategy is to be found in Walras's work. Walras rather started from the premise that the construction of general equilibrium analysis requires that the object of study be an entire economy from the onset onwards. Walras's particular way of circumventing the complexity problem involved in such a project was to start his analysis with the most rudimentary economy

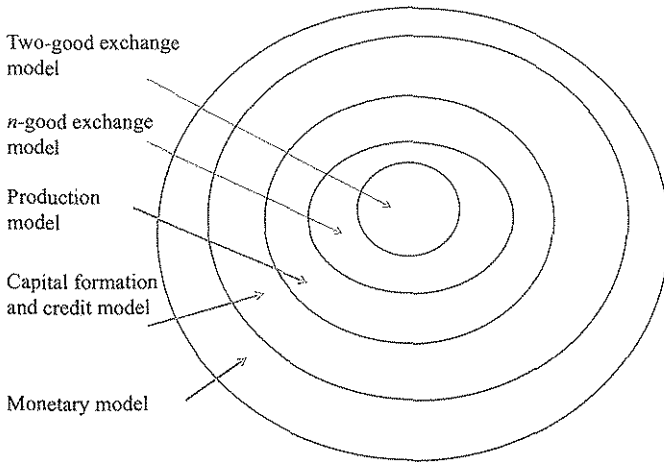


FIGURE 19.3 Walras's strategy for dealing with complexity

possible, a two-good exchange model, in which the economy comprises only two goods (oats and wheat). Once the principle determining the equilibrium of this simple economy was established, Walras went on to study a slightly more complicated economy, a n -good exchange economy. His next step was to introduce the production of final goods into the picture. In the end, he had a chain of encompassing models, starting from the simplest and moving toward more and more completeness: the two-good exchange economy model, the n -good exchange economy model, the production model, the capital formation and credit economy model and, finally, the monetary economy model. However, even the most complex model held no claim of realism. Figure 19.3 illustrates this.

Microfoundations

The microfoundations requirement states that macroeconomic models ought to be choice-theoretically grounded. Walras definitely abided by it – actually, in parallel to Jevons, he was at its origin. His analysis starts with solving agents' optimizing program. Only in a second stage are these choices translated into, first, individual and, next, market excess demand functions. Marshall certainly adhered to the idea that agents' behavior is underpinned by optimizing choice. He recurrently underlined the importance of what he called the "substitution principle." However, he had no qualms about starting the analysis at the level of market supply and demand functions, agents' underlying choices being merely evoked. Marshall's corn model in Book V, chapter II of the *Principles* is a fine testimony to this. He takes one paragraph to describe how a person decides how much blackberry she will pick balancing the disutility of picking and the utility of eating blackberries to conclude that she will equalize marginal

utility and marginal disutility. This principle is exposed in a single paragraph, and Marshall then jumps to his main object of study, the interaction between market supply and demand. He proceeds in the same way in the other chapters of Book V of the *Principles*, the most theoretical part of the book. While frequently referring to agents' choices, especially firms' decision-making process, his analysis proper is concerned with market supply and demand analysis (though microfoundations received pride of place in the mathematical appendix of the *Principles*).

Equilibrium

As has been seen earlier, both Walras's *Elements* and Marshall's *Principles* are based on the static state of rest conception. However, one caveat must be made for what concerns Walras. In the *Elements*, Walras also reasoned away from the traditional conception of equilibrium, though unwittingly. The traditional view is congruent with his simplest models, at least up to the production model. Examining his capital formation and credit model, it turns out that Walras's reasoning anticipated the neo-Walrasian concept of equilibrium, intertemporal equilibrium, sketched out by Hicks in *Value and Capital*, which is at the core of the Arrow-Debreu-McKensie model and which Lucas imported into macroeconomics.⁴

Trade technology and the representation of the economy

In the general equilibrium literature, the notion of an economy is usually understood in a narrow sense as referring to a list of agents (with their endowments, preferences and objectives), a list of commodities and a list of firms (with their ownership structure and technical constraints). The task usually assigned to general equilibrium theory is to analyze the existence, uniqueness, stability, and welfare characteristics of the equilibrium of a given economy at one point in time and over time. However, the theoretical investigation should not be stopped at this stage. The economy whose equilibrium is discussed ought also to be depicted as a social system, comprising a minimal set of institutions, trade arrangements, rules of the game, and means of communication between agents. Using the notion of an economy in a broader sense than usual to include these institutional features, the Marshallian and the Walrasian economy turn out to be two distinct objects.

A Walrasian economy consists of a single grand market involving all agents and goods in a single collective transaction, the price-formation process being directed by a third-party character, a 'non-agent' person, the auctioneer. The latter announces prices – hence agents are price-takers. His

⁴ On this, see Diewert (1977), Donzelli (1990) and Van Wittenloostuijn and Maks (1990).

role consists in establishing the price vector making agents' optimizing plans compatible. The auctioneer is a key element in the functioning of a Walrasian economy. Most general equilibrium theorists recognize the auctioneer's role only grudgingly and often openly declare their dislike for it. For my part, I think that this fictive third-party character is integral to the Walrasian research program.

Adopting the auctioneer trade technology has several implications, two of which must be underlined. The first is that the auctioneer hypothesis and perfect competition are part and parcel of each other. This follows from reflecting on the communication structure of an auctioneer-led system. This economy is a set of bilateral relationships between the auctioneer and isolated individual agents. Before equilibrium is reached, agents' exclusive social link is with the auctioneer. They do not interact or communicate with each other. As a result, whenever a given agent makes a trading offer by responding to the prices announced by the auctioneer, she does not know the other agents' reactions to the price that is announced. An agent can be in a monopolistic position without being aware of it, and so is unable to take advantage of it! To look at the same matter in a slightly different way, a central trait of monopoly or oligopoly theory is that agents holding market power must know the objective demand function for the good they sell. This runs counter to the auctioneer trade technology; in the latter, agents have no knowledge of market excess demand functions. Hence the Walrasian trade technology implies perfect competition.

The second trait I want to underline is that the same incompatibility holds for Walrasian theory and price rigidity. The auctioneer is an artifact invented by economists in order to sidestep the daunting issue of how some states of equilibrium which are logically conceivable can be attained. As argued by Lucas (1987: 1952), it makes little sense to impede her doing the task which she was created for. Thus the propositions, 'in a Walrasian economy prices are flexible' and 'in a Walrasian economy equilibrium prices are formed by the auctioneer', amount to the same.

Turning to Marshallian theory, I have pointed out in Chapter 1 that it rests on the assumption that agents have the ability to mentally reconstruct the equilibrium allocation of the markets in which they participate. In other words, the absence of the auctioneer is counterweighted by a tremendous increase in agents' knowledge and calculation ability. Nonetheless the Marshallian set up has one important advantage: it permits to release the perfect competition gridlock that tightens up the Walrasian approach. As far as competition is concerned, Frank Knight depicted the Marshallian standpoint by declaring that there were no less than nine requirements for perfect competition, among which perfect mobility, perfect communication between individuals, and the exclusion of all forms of collusion (Knight 1921: 76 seq.). What is interesting for my purpose in Knight's view is that he accepted that competition is a matter of degree. In other words, the Marshallian approach, unlike the Walrasian one, admits departures from perfect competition.

Up to now, my characterization of the Marshallian trade technology assumptions has pertained to the working of a single market. To know what a Marshallian economy may look like it suffices to extrapolate to the entire economy the institutional setup upon which Marshall's partial equilibrium analysis is based (as done in Hart's model studied in Chapter 14). It then turns out that the Marshallian economy constitutes a construct which is quite different from the Walrasian one. A Marshallian economy is composed of different separate markets in contrast to the Walrasian economy which is actually a single market.⁵

Is one of these two approaches superior?

Finally, let me ask whether the Walrasian or the Marshallian approach may be deemed to be superior. The economists who brought the Marshall-Walras divide view to the fore, such as Friedman, Stigler, Clower, and Leijonhufvud, did it to complain about the predominance of the Walrasian approach and to plead for the Marshallian one. For my part, I see no reason to bestow the laurels to Marshall and disparage Walras. Rather, I regard the two approaches as alternative programs, each having their pros and cons. The Marshallian research strategy permits an in-depth study of particular topics, but it has its own drawbacks. First, the delineation of its subject of study is always arbitrary (strictly speaking, one cannot separate an industry or a market from the rest of the economy). Second, the *ceteris paribus* assumption is always a *coup de force*. Third, the piecing together phase has proven to be a highly difficult task. In view of this difficulty, Walras's decision to consider an economy as a whole from the onset of his investigation was a clever move. But the drawback of the Walrasian methodological strategy is obvious: the analysis evolves at such a level of abstraction that its real-world relevance is limited.

THE HISTORY OF MACROECONOMICS AGAINST THE MARSHALL-WALRAS DIVIDE

I am now able to raise the issue of whether the Marshall-Walras divide helps to shed light on the development of macroeconomics. In the previous chapters, I have taken a preliminary stance on this issue by declaring that the Lucasian revolution was tantamount to the transition from a Marshallian to a Walrasian type of macroeconomics. My aim in this section is to substantiate and also

⁵ The terminology used in describing a Walrasian economy is often misleading, a transposition of Marshallian traits into the Walrasian universe. The Walrasian economy is not subdivided into separate markets. One may for example speak of markets x being in excess demand, but then the term market does not, like in Marshallian theory, mean a specific institutional setup for the formation of equilibrium. It just means that good x stands in excess demand with respect to the price vector of all the commodities.

nuance this claim. Table 19.2 summarizes how the Keynesian (i.e., IS-LM) and the DSGE programs fare with respect to the benchmarks of Table 19.1.

As far as the Keynesian program is concerned, Table 19.2 indicates that it stands on the Marshallian side for all criteria except one, which is hardly benign, market clearing. As for the DSGE program, a preliminary remark is needed. Up to now, I have used the ‘Walrasian’ label in a broad sense covering both Walras’s theory and subsequent neo-Walrasian theory associated with the names of Arrow, Debreu, and McKensie. This makes sense because the latter is a highly formalized extension of what Walras did inchoatively. However, on one point, equilibrium, the difference is sharp as the intertemporal equilibrium concept, underpinning neo-Walrasian theory, stands as an alternative rather than as an extension to the static state of rest concept, usually ascribed to Walrasian theory. Thus, when in Table 19.2, I characterize the DSGE approach as Walrasian, this means ‘Walrasian in the broad sense,’ that is, common to Walras and to neo-Walrasian theory. But when it comes to the ‘equilibrium’ benchmark, I need to specify that DSGE models are neo-Walrasian because they adopt the intertemporal equilibrium concept.

Let me now turn to a more detailed analysis of how the main episodes in the history of macroeconomics fare with respect to the above benchmarks.

Keynes’s *General Theory*

Many interpreters of Keynes’s work have insisted on the Marshallian lineage of his work.⁶ A basic reason for this assessment was given by Clower:

Keynes did not become an economist in a vacuum; he lived at a time when economics was dominated by the teaching of Alfred Marshall. Keynes was not ignorant of the work of Walras, but his family background, education and life as a Fellow of King’s College added strength to other influences conducive to Marshallian habits of thought. (Clower 1997: 36)

But there is more than just a general belonging to the Marshallian tradition. Keynes also followed Marshall on precise theoretical and methodological points. I have shown this in Chapter 1 for what concerns Keynes’s theory of effective demand, which is a mere extension of Marshall’s analysis of firms’ optimal short-period production decision.

Keynes’s name does not appear in Table 19.2, but for all the benchmarks, except one, he fares like IS-LM modeling. The exception concerns the relation between theory and empirical work. Indeed, Keynes held the view that *The General Theory* should not be assessed empirically. In chapter 1, Note 25, I alluded to Tinbergen’s pioneering econometric work, *Statistical Testing of Business Cycle Theories* (1939). Keynes was asked by the authorities of the League of Nations to referee it (Keynes 1973: 277–320). This led to a rather

⁶ E.g. Friedman ([1989] 1997), Clower (1975), Leijonhufvud (2006b), Hayes (2006), Lawlor (2006).

TABLE 19.2 *A comparison of the Keynesian and the DSGE programs*

	Standpoint taken ...	
	by Keynesian macroeconomics	by DSGE macroeconomics
General purpose (including the attitude towards empirical work)	Marshallian	Walrasian, except the attitude towards empirical work
Methodological priority (internal or external consistency)	Marshallian	Walrasian
Way of addressing complexity	Marshallian	Walrasian
Representation of the economy	Marshallian	Walrasian
Role of mathematics	Marshallian	Walrasian
Microfoundations	Marshallian	Walrasian
Equilibrium	Marshallian	Neo-Walrasian
Trade technology (price-making and information)	Marshallian	Different standpoints according to which stage of the DSGE program is concerned

extensive correspondence between Keynes and Tyler (his correspondent from the League of Nations), and between Keynes and Harrod and Tinbergen on methodological matters. It culminated in a review article in the September 1939 issue of the *Economic Journal*, to which Tinbergen wrote a reply, followed by a last comment by Keynes in the March 1940 issue. Keynes was dismissive of Tinbergen's work because he believed that little was to be gained from trying to test theoretical models empirically. Too much arbitrariness was involved in such an exercise. Here is how he made his point in a letter to Tyler:

The coefficients arrived at are apparently assumed to be constant for 10 years or for a larger period. Yet, surely, we know that they are not constant. There is no reason at all why they should not be different each year. . . . Is it assumed that the future is a determinate function of past statistics? What place is left for expectations and the state of confidence relating to the future? . . . If you have a fair number of variables and can play about at will with the coefficients and time lags, is it or is it not the case that more than one equally plausible result can be obtained? In short, how far are the results mechanically and uniquely obtainable from the data, and how far do they depend on the way the cook chooses to go to work. letter to Tyler, August 23 1938, Keynes & 973: 285-9)

The Keynes-Tinbergen controversy episode illustrates Keynes's razor's edge standpoint. While in *The General Theory* he incessantly insisted on the view that facts of life needed to be incorporated into theory, he refrained from taking the further, seemingly natural, step of asserting that, if a model is realist, one must be able to test it empirically. To him, progress in economic theory

consisted in improving models conceptually rather than in engaging in empirical verification. On this score, Keynes was on Walras's side.⁷

In my eyes, it is indubitable that Keynes's *General Theory* belongs to the Marshallian approach. Nonetheless, from the beginning, there were several voices claiming that it was Walrasian. Lange was the first of them.⁸ In a 1938 article, entitled "The Rate of Interest and the Optimum Propensity to Consume," he argued that Keynes's simultaneous equation system was the same as Walras's, underscoring that the preference for liquidity was nothing else than Walras's *encaisse désirée*. Likewise, Modigliani in his influential 1944 paper presented his model, which he believed to be a fair representation of Keynes's central ideas, as a simplified Walrasian model. However, the main advocate of this interpretation was Patinkin. Here is, for example, what he wrote in his entry on Keynes in the *New Palgrave Dictionary*, more than thirty years after his first foray into the interpretation of *The General Theory*:

Thus a basic contribution of *The General Theory* is that it is in effect the first practical application of the Walrasian theory of general equilibrium: 'practical' not in the sense of empirical ... but in the sense of reducing Walras' formal model of n simultaneous equations in n unknowns to a manageable model form which implications for the real world could be drawn. (Patinkin 1987: 27)

The analysis of *The General Theory* is essentially that of general equilibrium. The voice is that of Marshall, but the hands are those of Walras. And in his IS-LM interpretation of *The General Theory*, Hicks quite rightly and quite effectively concentrated on the hands. (Patinkin 1987: 35)

Neither Lange and Modigliani nor Patinkin justified their standpoint. What happened in my eyes is that they rightly perceived that Keynes aimed at studying interdependency across markets as Walras did. As they took it for granted that the Walrasian approach is the exclusive way of engaging in general equilibrium analysis, they concluded that Keynes was Walrasian. If other ways are possible, their conclusion is no longer valid.

Hicks's IS-LM model

A possible offspring of these interpretations, the IS-LM model has also often been characterized as Walrasian.⁹ Again, this happened offhandedly, without serious justification. One possible reason for this portrayal is that Hicks, the initiator of the IS-LM tradition, is considered a Walrasian economist – at least, he supposedly was at the time when he wrote his "Mr. Keynes and the 'Classics'." This was also the period during which he was working on *Value and Capital*, which played an important role in reviving Walrasian theory.

⁷ See his letter to Harrod, dated July 4, 1938 (Keynes 1973: 296).

⁸ See Rubin 2011.

⁹ See, e.g., Vercelli (2000). This view is also taken for granted in several of the contributions to the Young-Zilberfarb volume on IS-LM (2000).

Hence the almost automatic conclusion that the IS-LM model must be Walrasian as well. My own alternative interpretation is that Hicks had no qualms about jumping from one approach to the other because, to him, they served different purposes instead of being alternative research lines.¹⁰

To assess more in earnest whether the IS-LM model belongs to the Marshallian or the Walrasian approach, we must examine how it behaves with respect to the criteria used in Table 19.1. Let me zero in on just a few of them. As far the representation of the economy is concerned, the economy that the IS-LM model analyzes is composed of markets that function separately, each of them being an autonomous locus of equilibrium. Turning to trade technology, no auctioneer is supposedly present. As for the information assumption, it is true that economists using the IS-LM model scarcely evoke the possibility that it might rest on the assumption that agents are omniscient. But then nobody seems to have raised the issue of how equilibrium is reached in this model. Once raised, I see no other explanation than assuming agents' ability to reconstruct the equilibrium values of the economy, that is, their being omniscient! On all these scores, the IS-LM model is Marshallian.

Like Keynes himself, one point where IS-LM macroeconomists have proven to be non-Marshallian is that they aim at demonstrating involuntary unemployment. To them, the introduction of this concept in the Marshallian framework did not seem to be a daunting task. The previous chapters have amply demonstrated that they were wrong. Except for the contrived wage floor assumption, there is no room for rationing in the standard supply and demand apparatus.

Later developments within IS-LM macroeconomics, related consumption, portfolio choice, investment, and the labor market, confirm its Marshallian belonging as they consisted in analyzing one sector of the economy viewed in isolation from the others, Marshall's way of tackling complexity. Note, finally, that my claim that the IS-LM model belongs to the Marshallian approach is in accordance with the implicit view taken by most defenders of IS-LM macroeconomists. They firmly believed that their approach is poles apart from Walrasian microeconomics.¹¹

Disequilibrium à la Clower-Leijonhufud

Beyond doubt, this approach fully belongs to the Marshallian approach.

Disequilibrium à la Patinkin, non-Walrasian equilibrium models

With the exception of Benassy's, these models belong to the Walrasian approach. Their aim was to slightly modify the Walrasian base line model

¹⁰ Moreover, it can also be argued that *Value and Capital* is less Walrasian than is usually claimed, and that Hicks read Walras through Marshallian glasses. (Cf. Donzelli (2012).)

¹¹ See, e.g., Lipsey (2000: 69).

in order to get what their builders deemed to be a Keynesian result. Patinkin did it by assuming slow adjustment toward equilibrium allowing for the temporary existence of involuntary unemployment. Non-Walrasian economists did it by assuming a rigid price vector to the effect that an allocation different from the purely Walrasian is obtained. The case of Benassy is interesting for my purposes, as his models inaugurated a new configuration in my typology, featuring a mix of Walrasian and Marshallian elements. On the one hand, Benassy dropped the Walrasian trade technology assumptions, the auctioneer, replacing it with perfect information *à la* Marshall. On the other hand, he stuck to the other Walrasian benchmarks: microfoundations, the equilibrium discipline, market clearing (in spite of his involuntary unemployment rhetoric), and the priority given to internal consistency over pragmatism.

Lucas

Walras's *Elements of Pure Economics* and the neo-Walrasian model have in common describing a complex abstract economy with a large number of different agents and commodities. On top of that, neo-Walrasian theory takes the passing of time in earnest, adopting Hicks's conception of dynamics according to which the criterion for dynamic analysis is that all events need to be dated. A richer notion of commodity ensues. Defining the object of macroeconomics as the study of the interactions between the sectors of an entire economy makes it a sub-species of general equilibrium analysis. However, because of its applied nature and its interest for policy, macroeconomics cannot evolve at the same level of complexity as Walrasian and neo-Walrasian theory. The creation of Walrasian macroeconomics then involved the transformation of complex general equilibrium analysis into simplified general equilibrium analysis.

Proceeding in this way limits the analysis to some version of the most simple of Walras's chain of models. Although neither he nor his commentators presented things like this, what Lucas did in his "Expectations and the Neutrality of Money" article was to construct a variant of Walras's exchange economy model. As Table 19.3 shows, the filiation is striking.

Although the basic configuration of the two models is similar, the result of Lucas's absorption of a series of neo-Walrasian insights – the intertemporal substitution idea, a more complete definition of a commodity, the states of the world notion, and equilibrium defined as an intertemporal consumption/leisure path – puts his model on a totally new trajectory allowing it to tackle business fluctuations.

Lucas's main departure from the Walrasian approach concerns the need for empirical verification which, as stated in Chapter 11, led pure neo-Walrasians to judge that he was not really one of theirs.

TABLE 19.3 *Lucas's neutrality of money model as an amended Walrasian two-good exchange model*

	Walras's model	Lucas's model
1) Type of economy	A two-good exchange economy; the two goods are physically different and are consumed during the same time period; substitution is intra-period	A three-good production self-employed economy (c , c' and leisure); c and c' are physically identical yet are consumed at different dates; they are non-storable; substitution is both intra and inter-period
2) Price formation	Tâtonnement: an auctioneer presides over a single exchange setup	One tâtonnement process per point in time
3) Money	No medium of exchange	Existence of a medium of exchange which does not enter the utility function and whose magnitude changes stochastically from one point in time to another
4) Agents	n dissimilar agents each of whom is endowed with only one of the two goods	$2-n$ agents belonging to two overlapping generations; all are identical except for their age
5) Information	Agents hold perfect information about all relevant states of the world (which in this context means the quality of the two goods)	Imperfect information: young agents ignore the present-day drawing of the two stochastic variables (although they know their density functions)

First-generation new Keynesian and alternative models

These models form a disparate group. One can classify them in three categories according to their scope of study: (a) general equilibrium models (they study an entire economy), (b) partial equilibrium models (they study either one market or a group of markets), and (c) models that study economic relations at a level lower than the market (say, a firm and its employment pool). Efficiency wages, implicit contract and menu costs models belong to the last of these levels; hence they should be considered as neither Marshallian nor Walrasian. Staggering wage contracts models must be considered as Marshallian. The same is true for Carlin and Soskice models.

The alternative models studied in Chapter 14 are all general equilibrium models. However, they cannot be bunched up. Diamond and Roberts, in common with Benassy, wanted to build models that are Walrasian except

for the trade technology. This is a far from trivial departure because the auctioneer hypothesis is the most formidable obstacle to a Keynesian result. However, neither Diamond nor Roberts followed Benassy in replacing the auctioneer with the Marshallian trade technology assumption. Instead, their models are based either on search or on a specific sequential trade technology, which I find a more commendable line. As for Hart, his research is also interesting. His aim was to construct a general equilibrium model with oligopolistic firms and unions. This implied moving away from the Walrasian approach. When gauged against the benchmarks of Table 19.1, we obtain a mixed result. The Marshallian elements of his model comprise the way of addressing complexity, the representation of the economy, the equilibrium concept, trade technology (both price formation and information), the competitive structure (remember that Marshallian theory allows for imperfect competition), and finally market clearing (his model is about underemployment rather than unemployment). What must be underlined is that Hart's model marked a significant progress within the Marshallian approach because it was probably the first time that the piecing together part of the Marshallian strategy had been carried out. Therefore Hart's model can be declared the first Marshallian general equilibrium model – for that matter, an imperfect competition Marshallian general equilibrium model (although it comprises perfect competition as a special case) and a simplified rather than a complex general equilibrium model. However, as far as the other benchmarks are concerned, Walrasian principles turn out to be at work. Hart's model is not fit for empirical assessments, internal consistency prevails over external consistency, it is highly mathematical and, finally, it is microfounded in the Walrasian way. As a result, my final judgment about Hart's model is that it is a Marshallian simplified general equilibrium model built in a Walrasian style!

RBC modeling

At first sight, the same conclusion should be drawn about RBC modeling as about Lucas: it belongs to the Walrasian approach except for one point, the claim that the validity of the models is to be assessed through empirical verification. This is a Marshallian element (although calibration is an odd way of doing empirical work).

However, whether RBC modeling is Walrasian can be questioned on another score. The aim of Walrasian and neo-Walrasian theory was and still is to determine whether the optimizing trading plans of heterogeneous agents composing a given economy can efficiently be made compatible. Thus, the economy studied must comprise heterogeneity and trade. These two features are absent from Kydland and Prescott's model. Indeed, its object of study is a range of one-person economies having no connection to each other. The conclusion to be drawn is that their model does not address the Walrasian problem. They may put their model under the patronage of the second welfare

theorem, a jewel of Walrasian general equilibrium theory, but at the end of the day, theirs could be labeled a ‘bastardized’ Walrasian model, to use an expression that was used about IS-LM macroeconomists by fundamentalist Keynesians. Nor could it be Marshallian because Marshall would have also found a Robinson Crusoe economic theory irrelevant.

There are, however, attenuating circumstances. On the one hand, blaming RBC modeling for its lack of addressing heterogeneity applies to all macroeconomic theories, including Keynesian macroeconomics. Were an alternative research line tackling the issue of heterogeneity in earnest available, the matter would be different. But this is not yet the case. On the other hand, as I have argued earlier, research lines ought to be judged on their offspring, the cumulative development ability of their inaugural model rather than their immediate virtues and defects. In this respect, I am among those who find that DSGE macroeconomics is a progressive program. A defense of the one-person economy assumption of the “don’t put the cart before the horse” kind therefore makes sense. It runs as follows. The project of dynamizing macroeconomics in a rigorous way by making use of tools available from applied mathematics was a daunting enterprise. Lucas, in his 1972 paper, had still been able to adhere to the heterogeneity requirement by using an overlapping generation model, yet no clear way of pursuing his line surfaced. Kydland and Prescott found a way of recasting it differently, while still keeping the basic concepts and standards Lucas had introduced. True, the line they opened amounted to a betrayal of Walrasian principles – subsuming the issue of interactive equilibrium formation under that of individual equilibrium formation – but it was done for the sake of tractability. Theorizing equilibrium paths instead of static equilibrium positions was a sufficiently demanding project that it made sense to tackle issues one after the other, and postpone trade and heterogeneity for a later stage of the program’s development. This amounts to having the validity of the whole bifurcation taken hinge on the promise of a later achievement. This was and still is a risky promise, although given the low level of epistemological alertness of the macroeconomic profession, it was hardly acknowledged, but at the time I am writing there are positive signs that it might be held. Therefore, I find it acceptable to classify RBC modeling as belonging to the Walrasian approach although, for the time being, it does not fully deserve it.

Second-generation new Keynesian models

The third stage of development of the DSGE program is also problematic for my taxonomy. At first sight, one is tempted to rank second-generation new Keynesian models in the same way as Hart’s model since monopolistic competition and oligopolistic competition models are akin. The trade technology elements of these models, in particular, are Marshallian. Moreover, the fact that I have argued that Walrasian theory requires perfect competition seems to

exclude classifying them as Walrasian. However, again, we must refrain from too hastily drawn conclusions.

As seen, there are four differences between RBC and second-generation new Keynesian models: goods variety, imperfect competition, rigidity, and the presence of a central bank following a monetary rule. Variety can take different forms. One is to assume an infinite variety of intermediary goods, which later are transformed into a final good in a costless way. These intermediary goods are assumed to be imperfect substitutes for each other to an extent captured by the Dixit-Stiglitz aggregator. Thus, imperfect competition takes the specific form of monopolistic competition. Increasing returns due to fixed costs is also assumed. A standard story is that the intermediary goods are produced by firms and purchased by households. As for rigidity, initially at least, it supposedly affects the price of the intermediary goods, rather than wages. This implies a replacement of a price-taking behavior with a price-making one. Rigidity results in an allocation different from the Walrasian one. The existence of the central bank requires no special comment at this juncture.

Outwardly, these are all important changes with respect to RBC modeling. However, upon reflection, these changes turn out to be conceptually less fundamental than they seem. Actually, they can all be absorbed into the RBC apparatus, which is centered on the problem of a representative household having to find its optimizing consumption/leisure equilibrium path. Take firms. Actually they are just a name put on the production function. The only assumption needed about them is efficiency. Free entry ensures that firms make no profit although they hold market power and can impose a markup on their costs. These features are unnecessary for the model to hold. To take my version of the Crusoe parable, hermits could produce the intermediary goods on their own. No labor market is really needed. Each hermit would specialize in one variety of the composite good and sell it to all other hermits. One novelty with respect to the RBC baseline model is that now trade is present, the result of the composite good and production specialization assumption. All this can be managed falling back on the RBC baseline model. What about the central bank? Unlike firms, it cannot be dispensed with. To be an active participant in the economy, it must have an objective function of its own. But the objective ascribed to it is to maximize the utility of the representative agent, which means that eventually no additional objective function is needed. So, at the end of the day, the RBC baseline model remains the core apparatus.

Thus, second-generation new Keynesian economists have been able to bring in new trade technology and market structure assumptions, giving the model economy a more realistic outlook and resulting in an equilibrium path different from the neo-Walrasian one, while still using the RBC baseline conceptual apparatus. The setting is more complex, but the story has not changed. This observation should not be deemed negative. A quick contrast with Hart's model

TABLE 19.4 *Classifying macroeconomic models against the Marshall-Walras divide*

	Marshallian	Walrasian	Mixing Walrasian and Marshallian elements	Mixing Walrasian and non- Marshallian elements
<i>The General Theory</i>	✓			
IS-LM macroeconomics	✓			
Patinkin		✓		
Monetarism	✓			
Leijonhufvud	✓			
Non-Walrasian equilibrium models		✓		
– Benassy			✓	
New classical macro (Lucas)		✓		
First generation new Keynesian models	✓			
Alternative models				
– Diamond, Roberts				✓
– Hart	✓			
RBC models		✓		
Second-generation new Keynesian models			✓	

explains why. Imagine that Hart, after having worked on his model, had decided to try to make it dynamic. He would soon have realized that his model is too complex for achieving this purpose.¹² A much simpler representation of the economy than his needed to be found. Taking the RBC bifurcation was an interesting alternative. So, second-generation new Keynesian modeling can be regarded as a way of carrying out the project I fancied Hart could have envisaged.

Table 19.4 summarizes the results of my analysis in this section.

CONCLUDING REMARKS

After I spent some time studying the history of macroeconomics, the idea dawned on me that the Marshall-Walras divide was an interesting key to use for understanding this history. At the end of the day, it appears that a blunt

¹² Hart, for his part, decided to move away from general equilibrium analysis and to study contract theory.

opposition between a Marshallian and a Walrasian stage in the development of the field will not do. Already, at Lucas's stage, there was an important necessary caveat concerning the attitude toward empirical work. As Table 19.4 illustrates, the further unfolding of macroeconomics has brought about a more complex picture. However, in my eyes, these developments do not make the Marshall-Walras divide benchmark useless.

Standing up to DSGE Macroeconomics

Few will deny that the DSGE approach holds sway over present-day macroeconomics, even if it lost some of its shine in the aftermath of the 2008 recession. Nonetheless, many dissenting voices have been raised. In this chapter, I study a small sample of them: the criticisms addressed to DSGE macroeconomics at a 2010 hearing of a subcommittee of the U.S. House of Representatives, Roger Farmer's project to revive the self-fulfilling prophecy insight, agent-based modeling as drawing its inspiration from Leijonhufvud's criticism of Keynesian macroeconomics, and, finally, Paul Krugman's advocacy for a return to Keynes.

DSGE GOES TO WASHINGTON

The progress of the DSGE program documented in the previous chapters hardly resulted in making it popular in the wider economics profession. In this section, I document this critical state of mind relying on a surprising source, a hearing by a Subcommittee of the Committee on Science and Technology of the U.S. House of Representatives held on July 20, 2010 whose topic was: "Building a Science of Economics for the Real World." The intention behind this title is summarized in the following question to be found in the opening statement of the Hearing: "To what extent is the [DSGE] model, a highly theoretical model that appears to bear little resemblance to everyday life, used in shaping policy that affects people and events in the real world?" (Hearing, p. 4).¹ The economists invited were Varadarajan V. Chari, David Colander, Scott Page, Robert

¹ In this book, I draw a distinction between the DSGE program and its successive installments, new classical macroeconomics, REC models, and second-generation new Keynesian models (to which the DSGE name is often apposed). It is unclear to which of these two understandings the organizers and the discussants referred.

Solow, and Sydney Winter.² As was the case with the discussion between Friedman and Modigliani studied in Chapter 4, the study of this session is useful for my purpose because real-life exchanges often are more revealing than written ones. In the latter, by definition, people present their views in an advantageous way. By contrast, in oral discussions they can be caught off guard by a disconcerting question or be interrupted, which may allow a better grasp of their views. I will begin by documenting what these views are as expressed in the discussants' preliminary statements; in a second step, I will zero in on just one of the several binary oppositions present when five opinions are confronted, the contrast between Chari and Solow.

The preliminary statements

In his statement, Winter strongly criticized neoclassical theory in general and DSGE modeling in particular for being oblivious to the behavioral realities of business practice, meaning by this "habits, organizational routines, organizational capabilities, business systems, business processes." To illustrate his claim, Winter focused on the chain of events that led to the subprime crisis and further to the recession. DSGE modeling, he argued, has room for none of them. As for future research, Winter's view was that a radical turn should be taken: "we need to make sure that adequate intellectual resources are applied to the task of understanding what is happening in the economy, as opposed to what is happening in the model" (Winter 2010: 25).

Page's and Colander's viewpoints were close to Winter's, yet their catchword was Complexity rather than Institutions:

Complex systems consist of diverse, connected, interdependent and adaptive actors, who collectively produce phenomena that are difficult to explain or to predict. Complex systems are neither ordered nor chaotic. They lie in between. (Page 2010: 29)

In their statements, they both admitted that DSGE models are powerful – Colander described them as "wonderful" and "impressive" (p. 48) – but declared that the times were ripe for turning to a dimension that these models totally left aside, namely complexity. On top of recapitulating DSGE modeling's obvious shortcomings (assuming away heterogeneous agents and sectors, unemployment, and networks of connection), Page also criticized it for postulating 'negative feedbacks,' – feedbacks exerting a stabilizing impacts – while in

² Solow needs no further introduction. Winter is the coauthor with R. Nelson of the celebrated book, *An Evolutionary Theory of Economic Change*, which blazed the way for a revival of institutional economics (Nelson and Winter 1982). Page's work deals with complex systems. Chari, a stalwart Minnesota-school economist, has coauthored several influential papers, one of which I discussed in the previous chapter. Finally, Colander is a reputed historian of economics with an interest in present-day theory.

reality ‘positive feedbacks’ often prevail, the way to crashes and crises. Both Colander and Page concurred in complaining about the excessive place taken by DSGE modeling in the profession at the expense of other promising programs, in particular agent-based models:

While the initial idea was neat, and an advance, much of the later research was essentially dotting i’s and crossing t’s of that original DSGE macro model. What that meant was that macroeconomists were not imaginatively exploring the multitude of complex models that could have, and should have, been explored. (Colander 2010: 40)

Colander also made the point that pure scientific research and applied policy research must be kept separate from each other. To him, DSGE modelers, such as Chari and his co-authors, have “failed society” by crossing the line between pure science and policy.³ What is needed, he contended, is to have more “researchers who have a solid consumer’s knowledge of economic theory and econometrics but not necessarily a producer’s knowledge of that” (Colander 2010: 39), such as Goodhart or De Grauwe.

As documented in earlier chapters, from the start Solow was put off by the DSGE program. His preliminary statement to the Hearing conveyed the same message: the DSGE approach “does not pass the smell test” (Solow 2010: 12)!

The basic story always treats the whole economy as if it were like a person, trying to consciously and rationally to do the best it can on behalf of the representative agent, given its circumstances. This cannot be an adequate description of a national economy, which is pretty conspicuously *not* pursuing a consistent goal. (Solow 2010: 14)

To Solow, this approach amounts to eliminating crucial realities from the theoretical picture, such as conflicts of interest, incompatible expectations, and market failures. In particular, “the DSGE story has no room for unemployment of the kind we see most of the time and especially now: unemployment that is a pure waste” (p. 13). Nor does it have any room for government interventions on the grounds that “conscious public policy can only make things worse” (p. 13).

While the first three speakers acted as prosecutors, it fell to Chari to play the role of the defense attorney. The first point he made was to assert that the criticisms addressed to DSGE models attacked a straw man, caricaturing models from a generation ago:

³ “The economics profession failed society was by letting policy makers believe, and sometimes assuring policy makers, that the topography of the real-world matched the topography of the highly simplified DSGE models, even though it was obvious to anyone with a modicum of institutional knowledge and educated common sense that the topography of the DSGE model and the topography of the real-world macro economy generally were no way near a close match. Telling policy makers that existing DSGE models could guide policy makers in their search in the dark was equivalent to telling someone that studying tic-tac toe models can guide him or her in playing 20th dimensional chess” (Colander 2010: 41).

The state-of-the-art DSGE model in, say, 1982 had a representative agent, no unemployment, no financial factors, no sticky prices and wages, no crises, no role for government. What do the state-of-the-art DSGE models of today look like? They have heterogeneity, all kinds of heterogeneity arising from income fluctuations, unemployment and the like. They have unemployment. They do have financial factors. They have sticky prices and wages. They have crises. And they have a role for government. (Chari 2010: 32)

Summers used to compare RBC modeling to a big tent using this metaphor in an accusatory way. In a clever reversal, Chari transformed it into a positive feature:

Macroeconomic research, as I said, is a very big tent and accommodates a very diverse set of viewpoints. There is a shared language and a shared methodology but not necessarily a shared substance in terms of policy issues. This openness and flexibility is best summarized by an aphorism that macroeconomists often use: "If you have an interesting and a coherent story to tell, you can do so within a DSGE model. If you cannot, it probably is incoherent." (Chari 2010: 32)

Finally, Chari asked himself why DSGE macroeconomists had not foreseen the crisis and what could be done about it. His answer was lack of attention. They had been paying too much attention to studying the U.S. economy where severe financial crises have been the exception and not enough to what happened elsewhere in the world. As to what could be done, his answer was candid: more resources must be devoted to finance macroeconomic research. When the AIDS epidemic surged, the answer of governments was to increase the resources devoted to studying the disease. He claimed that the same should hold for the economic crisis. The macroeconomics profession should receive more research resources!

Solow versus Chari

Winter, Colander, and Page voiced criticisms, but they are not macroeconomists. Therefore the most interesting opposition to focus on is between Solow and Chari, both eminent members of the macroeconomic profession. Three exchanges in the debate are worth dwelling on to grasp their differences. The first is their answers to Chairman Miller's question, "Can the government help?" Chari's response was that policy had become central to DSGE modeling. As a testimony, he referred to the program of the last conference of the Society for Economic Dynamics, declaring that fifty out of the four hundred papers presented dealt with policy. Yet he hardly went into the contents of their policy conclusions. Solow was quick to jump in, pointing out that the philosophy of these conclusions usually was "to make the world more like the neat model" (Solow 2010: 47), which actually amounted to putting forward the idea that the best thing the government can do is do nothing. So, the bottom-line answers to Miller's question were respectively 'No' for Chari and 'Yes' for Solow.

A second bone of contention was involuntary unemployment. To Solow, this concept is central because it points to an important fact of life, particularly visible in disarranged districts. As for Chari, he repeated Lucas's point that involuntary unemployment is a construct which has no theoretical leverage. Hence macroeconomists would do better if they focused on explaining variations in the level of activity and outsourced the unemployment topic to labor economists.

A third interesting exchange in the Hearing, was when Representative Broun raised the following question, which in view of the dismissive opinions on DSGE models expressed by four of the five participants was no surprise: "Should the economists throw away the DSGE model approach outright or cautiously use the model with the understanding that there are limitations and shortcomings?" (p. 52).

In his preliminary statement, Solow presented himself as an indomitable opponent of the DSGE approach. Therefore one would have expected him to join the radical critics of DSGE macro and advocate its immediate dismissal. And yet, this was not the case:

I think it is the latter. . . . I don't want to discard DSGE. The people who do it are among the brightest macroeconomists we have. . . . But I do think it has to loosen up. I do think that one wants to give up the representative-agent presumption; I think that one has to give up the devotion to equilibrium and keep the broader methodology. And in the course – let me just add one more thing – in the course of meeting criticisms from sourpusses like me and from the data, the DSGE people have made a lot of modifications, and in the course of doing that they have done a lot of good work. (Solow 2010: 52–53)

Such an answer must have been music to Chari's ears. Nonetheless, Solow did not feel like accepting Chari's invitation to "come on board" the DSGE vessel. To him, "there are other traditions with better way[s] to do macroeconomics" (Solow 2010: 15).⁴

What can explain Solow's conciliatory standpoint? Above, I have split the five participants to the Hearing in two groups, prosecutors and defense attorneys. The matter is more complex because, compared to Winter, Page, and Colander, Solow is of a different ilk. On several aspects, he is closer to Chari than to them. Winter defends an institutional paradigm, Page and Colander an agent-based one. Solow, for his part, is a neoclassical economist and hence a reductionist, somebody who believes that complexity cannot be tackled straight on. Unlike the other three, his reasoning revolves around the

⁴ "Dr. Chari suggested, "Oh, well, DSGE can be a big raft, climb aboard." I don't want to climb aboard. I would rather have the DSGE people take a swim off the raft. There have been, and still are, long traditions of work, theoretical and applied, in macroeconomics which have had their ups and downs, but they are not all novelties or things that we ought to try for the first time" (Solow 2010: 57).

notion of equilibrium. Where he differs from DSGE theorists such as Lucas and Chari is that to him macroeconomics theory must have room for disequilibrium occurrences, including individual disequilibrium (such as involuntary unemployment). Solow is not against microfoundations; he is just against the principle that one single modeling strategy must prevail. In short, he is a neoclassical synthesis economist. Moreover, as long as new classical and RBC modeling held sway, Solow could not find anything likeable in it; hence his vilifying words. However, the emergence of second-generation new Keynesian models changed the game, leaving Solow faced with a thorny dilemma. On the one hand, he now had a reason for rejoicing since assumptions that there were dear to him – stickiness, imperfect competition and money non-neutrality – had found their way into the DSGE program. On the other hand, two other ingredients of his vision of macroeconomics, which were as important to him as the other three, involuntary unemployment and methodological eclecticism, were rejected. In my opinion, this is what explains the convoluted standpoint he voiced in the Hearing.

As for Chari, his job was tough, yet he managed well. He cleverly used his trump card, which was that tremendous progress occurred between Kydland and Prescott's inaugural paper and the present. He was also right in saying that DSGE macroeconomics is the only game in town, although the weight which should be given to such an argument is dubious – after all, before Keynesian macroeconomics was dethroned, it too was the only game in town! Chari also benefitted from the fact that, all in all, with the exception of Winter, the criticisms addressed to DSGE modeling were more moderate than expected. Indeed, at the end of the day, complaints seemed to bear less on the substance of the DSGE approach than on the imperialistic tendencies of its members. Denouncing a lack of pluralism in the profession is hardly an internal criticism. Finally, Chari also adopted a soft-spoken, engaging tone. It was clever of him to invite skeptics to come on board with present-day DSGE modeling. Here, however, Chari cannot be taken on his word. DSGE modeling may well be a large tent but it has compelling entry conditions, Lucas's standards, to which other economists will not want to yield. Solow's invitation to DSGE economists to give up their "devotion" to equilibrium is in the same vein. Accepting the invitation would amount to returning to the neoclassical synthesis, which they have no intention of doing. Moreover, as documented in Chapter 14, consistent models different from DSGE modeling have seen the light the day.

I also regret that Chari showed no awareness of the limitations of the DSGE program. In my assessment, I underlined two crucial ones, both of which Lucas acknowledged. The first is that the DSGE program is useful only for the study of moderate fluctuations. It cannot come to grips with great depressions (as is made clear by comparing what Winter and Chari had to say about the 2008 recession). The second limitation is that policy conclusions are embedded in the premises of the models to the effect that they cannot be translated into direct policy recommendations for real-world policy makers. As I argued earlier, it is

normal for DSGE models to bear policy conclusions, but DSGE macroeconomists should admit that these conclusions cannot be peddled to policymakers.

I draw two conclusions from my study of this Hearing at the House of Representatives. The first one is that the debates were less harsh than what one could have been expected. I would not be surprised that, were they interviewed, the attendants of the session, say the Representatives, might express the feeling that economists form a unified body and tend to think the same way. Of course, had other experts been invited, the outcome would have been different. My second conclusion concerns Solow's standpoint. His case illustrates how complex being a Keynesian economist can be. Solow still felt close to 'old' Keynesians such as Tobin.⁵ At the same time, he was a contributor and a mentor of first-generation new Keynesian macroeconomics. He staunchly adhered to the neoclassical synthesis; not only in words but also in acts (as he contributed as much to classical as to Keynesian theory). However, the Hearing shows that the one step he hesitated to take was to fully endorse second-generation new Keynesian modeling, feeling that the price to be paid for it (discarding deep market failures) was too high. The result is that Solow became the minority within the family of neoclassical Keynesian economists.

FARMER'S SELF-FULFILLING PROPHECIES MODEL

Farmer belonged to the group of neo-Walrasian economists who in the 1980s proposed sunspot and self-fulfilling prophecies models as an alternative to the research line opened by Lucas's article.⁶ Although these models were in the same league as Lucas's as far as technical virtuosity was concerned, they did not fare as well for different reasons, one of them being the difficulty of making them empirically assessable. Farmer's originality is that he decided to pursue the same objective in a different way by crossing the border line separating neo-Walrasian theory (i.e., complex, cut-off-from-empirics and policy-free general equilibrium analysis) and macroeconomics (i.e., simplified, applied and policy-oriented macroeconomic general equilibrium analysis).⁷

The result of Farmer's foray in macroeconomics was the 'Farmer model,' to use his own expression. It was expounded in two books – *Expectations, Employment and Prices* (2010a) and *How the Economy Works. Confidence, Crashes and Self-Fulfilling Prophecies* (2010b) – and several articles (among which Farmer 2008, 2013). In these, Farmer pursued the same aim as Diamond: breathing new life into the joint concepts of animal spirits and self-fulfilling expectations by reformulating them in the language and with the tools of modern theory.

⁵ To one attendant at the Hearings, who declared that Tobin was his hero, at which juncture Chari jumped in saying that his own hero was Tobin (p. 57).

⁶ Cf. Azariadis (1981), Azariadis and Guesnerie (1986), Benhabib and Farmer (1994), Cass and Shell (1983), Farmer (1993), and Guesnerie and Woodford (1992).

⁷ Woodford made a parallel move in a different direction and for a different purpose.

My attention in this section is limited to Farmer's *Expectations, Employment and Prices*. This is an amazing book. A first reason is that it is doing so much in so few pages (175 pages). On top of what I will insist on, his theoretical model, Farmer proposed an alternative to the Hodrick-Prescott filter allowing to take into account medium-term frequencies that this filter leaves aside. He also debunked the notion of a natural rate (or path) of employment. Finally, he contended that his model is able to explain both the Great Depression and the 2008 recession in the United States, as well as business fluctuations in between these. A second reason is that Farmer proved able to reconcile the two parts that were split in *The General Theory*, the effective demand model and animal spirits. This is a real prowess, not counting that in his effective demand model he found a way of getting rid of nominal wage rigidity. A third reason for amazement is that, to make a Keynesian case, Farmer used tools that are altogether un-Keynesian: the equilibrium discipline, search theory, the household framework (to the effect that the unemployed enjoy the same utility as the employment), dynamic optimization, the social planner. One could hardly be more versatile: Farmer's way of thinking out of the box is to think across boxes!

The Farmer model

Of all of these mentioned topics, I will only study Farmer's core theoretical intuition, his combining the effective demand and the animal spirits claims. He proceeded in two main steps. He first constructed a static one-good production multiple equilibria model. In a second step, he extended it into an intertemporal model, studying an economy comprising multiple final goods and one non-produced capital good. In this model, the indeterminacy is closed by agents' beliefs about the future state of the economy. In both models, the labor market has a search technology. Here is how he summarized his project:

I develop a model in which the labor market is cleared by search, but instead of closing it with an explicit bargaining assumption, I assume only that all firms must offer the same wage. This leads to a new theory in which there are many wages, all of which are consistent with a zero profit equilibrium. And it provides a microfounded analogue of Keynes's idea that there are many levels of economic activity at which the macroeconomy may be in equilibrium. (Farmer 210a: 9)

Farmer's starting point was the Mortensen-Pissarides matching model (1994). This model comprises two steps, the random search and the matching process. The latter bears on how the two parties share the surplus brought about by the match, thereby closing the model. This second step did not suit Farmer's project because, to have multiple equilibria, he needed indeterminacy. Hence he dropped it, assuming instead that firms and workers are price-takers with respect to the good-labor relative price. This paves the way for modeling an

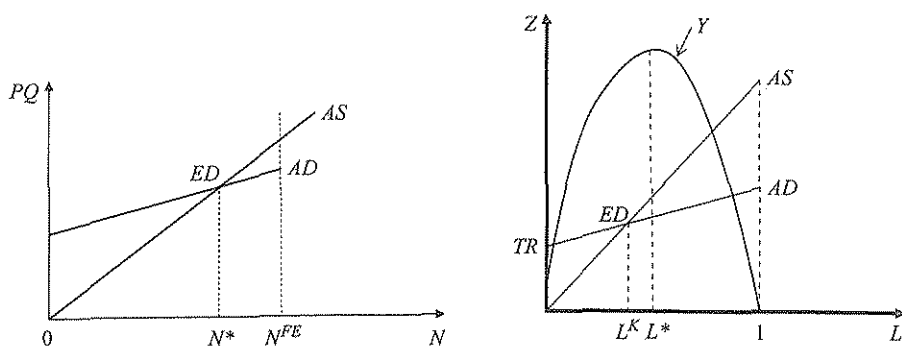


FIGURE 20.1 Effective demand *à la* Keynes and *à la* Farmer

outcome in which a continuum of equilibria, each associated with a different real wage, prevails. Some way of closing the model is then needed. Farmer's claim was that agents' beliefs about the future prospects of the economy are an apt solution.⁸

The self-fulfilling result that Farmer was striving for requires the level of activity be driven by aggregate demand. In this respect, he was returning to Keynes. Two flaws of Keynes's effective demand model were that it rested on wage rigidity and lacked microfoundations. In constructing his model, Farmer had to fix them.

Figure 20.1 compares the standard 'Keynesian cross' graph, derived in Chapter 2, and Farmer's own graph. The similarity is striking: in both cases, though expressed differently, the level of employment is determined by the intersection of aggregate supply (AS) and aggregate demand (AD), and happens to be below full employment (N^{FE} or L^*).

Farmer's is a highly simplified general equilibrium model. It pertains to a one-good production economy. The latter comprises a unit mass of identical households, each of which has a unit mass of members. Leisure yields no utility; hence all household members look for a job. There is no saving, all income is consumed. The employed workers' income is equally distributed across the household. Farmer also assumed that leisure has no utility, hence everybody looks for a job.

The originality of his search model is that he expressed the matching function as $m = m(H, V)$, with H designating the measure of job searchers and V the

⁸ As his basic model was static, Farmer needed to temporarily resort to another solution for solving the indeterminacy, namely government's fiscal policy. This translates into expressing households' budget constraint as $pC = wL(1 - \tau) + TR$ (where p is the price level, C consumption, w the nominal wage, the tax rate and TR a lump-sum monetary transfer to households). This assumption, which allows to present aggregate demand as in the Keynesian cross diagram graph (see Figure 20.1), was dropped in his intertemporal model.

workers engaged in the recruiting activity rather than vacancies. Farmer assumed that a fraction of the workers hired by firms are assigned to the recruiting activity. Hired labor (L) is thus larger than labor as acting as an input in the production function ($Y = AX$, where Y is the output, A is a productivity parameter and X are the 'productive' workers). Let q be the ratio of productive workers to recruiting workers ($q = X/V$). Labor used in production is proportionate to the total labor ($X = \Theta L$), hired where Θ , which Farmer called the 'hiring efficiency,' is defined as equal to $1 - 1/q$. Hiring efficiency is endogenously determined but taken parametrically by firms. Its magnitude depends on market tightness: the larger the market tightness, the lower q , and the lower the hiring efficiency; by contrast, if market tightness is small, a small number of recruiters can support a large workforce:

The match technology leads to a production externality across firms. When all other firms have high levels of employment, it becomes harder for the individual firm to recruit workers and this shows up as an external productivity effect. (Farmer 2010a:39)

In a spot framework, equilibrium requires the real wage to be equal to the marginal product of labor. In the search framework, this condition becomes $A\theta = w/p$. This equation is compatible with a range of equilibrium real wages or hiring effectiveness sizes. In a zero profit equilibrium, w/p will be high if q is large and small if q is small. The determination of output, the bumped curve in Figure 20.1, ensues. Starting from zero, it increases with L up to a maximum and then decreases downwards to $L = 1$ where all workers would do recruiting (the unit measure of employment as defined by Farmer).

The equilibrium of this economy as calculated by a social planner is described in Figure 20.2.

On the basis the parameters chosen by Farmer, L^* and U^* form the optimal allocation of the labor force between employment and unemployment, this

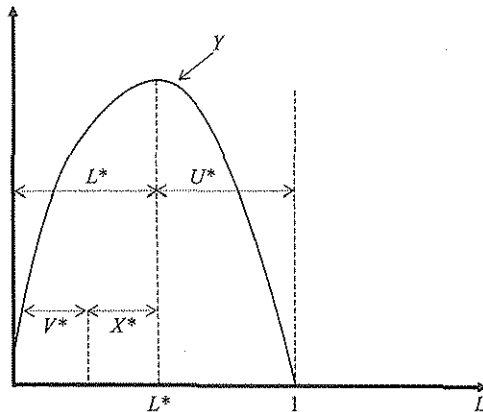


FIGURE 20.2 The determination of output

allocation being underpinned by X^* and V^* , the optimal sizes of workers engaged respectively in production and recruiting.⁹ Farmer called this efficient allocation 'full employment.'

For my purpose, it is unnecessary to describe how Farmer drew the aggregate supply and demand functions from households' and firms' optimizing program. His point is that many equilibria are conceivable and that the chances that they coincide with full employment are small. Figure 20.1, where the level of employment L^K (with K for Keynes) prevails, illustrates. Like in *The General Theory*, in Farmer's model there is no reason for effective demand to be equal to full employment. However, behind this similarity, important differences loom. Full employment is defined as an inefficient equilibrium. It is not defined as the opposite of involuntary unemployment. There is no disparity in utility between the employed and the unemployed. Moreover, in Farmer's model, the effective demand can be either lower (as in Figure 20.1) or higher than full employment. Also, in Farmer's model wages are flexible, as Keynes wanted them to be. Finally, unlike in Keynes's model, there is no straightforward policy conclusion.

The next step in Farmer's reasoning was to transform the static model into a dynamic one dealing with firms' and households' optimizing equilibrium paths. The economy is now a multi-final goods economy comprising n commodities ($1, \dots, i, \dots, n$). The assumption that there exists a government taxing households and operating transfers in their favor (see Note 8) is dropped, being no longer necessary to close the model. There is no investment and hence no saving, which is of course odd for a dynamic model; hence consumption and production are equal by definition. Capital is introduced yet in an unusual way. It consists in a unit measure of input or capital good, K_t – say, land. It is allocated at each period across industries in a competitive rental market, leading to a sequence of prices ($p_{k,t}$). Farmer considers this market as analogous to a one-equity stock market. In other words, the value of the capital good and the value of the stock market are one and the same thing. Expectations are rational except when bearing on a variable that features multiple equilibria.

The solution of the households' problem is represented by an Euler equation and implies an equilibrium allocation between consuming final goods and the capital good path. As for firms, the aggregate supply equilibrium path is derived from the first-order conditions for rental capital and labor. Farmer demonstrated that it can be expressed as a proportionate relation between supply and employment. The efficient equilibrium as calculated by the social

⁹ Farmer's quantitative results (in his model the optimal unemployment rate is 50 percent at which a quarter of the employed workers are engaged in recruiting activities) have no claim to realism. They follow from his choice of coefficients in his matching function, which he expressed as λ ; since $H = 1$, it reduces to $m = V^{1/2}$. Farmer also assumed a 100 percent turnover and that all jobs last just for one period.

planner has the same equilibrium as the basic model yet it is now expressed as time path (i.e., $L_s = \frac{1}{2}$, $s = t \dots, \infty$). Associated with it is an efficient value of p_k , p_k^* . As in the basic model, the non-priced externality associated with the search technology results in multiple equilibria. There is thus a different equilibrium path for every sequence of the hiring effectiveness parameters. Farmer solved this indeterminacy in a Keynesian way by focusing on households' beliefs about the long-period state of the economy. Keynes wrote about them in general terms. In his model, Farmer took the further step of making them bear on a precise object, the value of the capital good in future periods $\{p_{k,t}\}$:

Changes in belief about the value of capital will have an effect on expenditures since long-term expectations influence wealth, which in turn influences consumption expenditures. (2010a: 92)

Still in other words, "confidence selects the unemployment rate that we observe" (2010b: 114). There is thus a continuum of "Keynesian equilibria" (L^K) sequences indexed by $p_{k,t}$. The chances are thin that any given sequence might coincide with the equilibrium sequence. So, most of them are inefficient. If $p_{k,t} < p_{k,t}^*$, unemployment is "inefficiently high" (2010a: 93). This is the situation of low confidence that Keynes had in mind. If $p_{k,t} > p_{k,t}^*$, unemployment is too low. The policy conclusion that Farmer drew from his model was that the action to be taken by the government in order to stabilize the employment level is to stabilize the intertemporal value of $p_{k,t}$. In the applied part of his book, Farmer made the further step of suggesting that it would be a good idea to extend this policy conclusion to reality. His original proposition was to create an index fund based on a basket of securities and assign the Federal Reserve Bank the task of pegging its value by selling or buying its shares. Through such a policy of direct targeting, the global value of the stock exchange would be stabilized and hence a high level of employment preserved.

Assessment

In times of high standardization of theoretical production, economists who are able to strike a balance between going off the beaten track and yet abiding by the reigning methodological standards are scarce. Farmer is one of them. His work stands out for its originality and boldness. It also attests to Farmer's knack at transposing complex ideas into simple models. His ability to meld ideas coming from different horizons, his new mix of Keynesian and DSGE insights, is praiseworthy.

Multiple equilibria are a great idea that many would like to adopt were it not for the daunting character of its implementation. Farmer's work attests to this. Nonetheless, it is hard to resist the judgment that, for all its panache, it is based on a few, hard to swallow, *coups de force*. The first pertains to his use of the

search model. Search theorists adopted a new trade technology giving pride of place to agents' and firms' heterogeneity: differences in skills, in the nature of the jobs offered by firms, in location, viewing employment as an enduring and stable relation, giving pride of place to wage dispersion, and depicting agents as price makers. These elements are all absent from Farmer's model. Wage bargaining looks like a natural way to finalize the matching process. Farmer discarded it, replacing it with a convoluted solution that makes more sense in a neo-Walrasian than in a search framework. This is an ad hoc twist. A similar judgment must be made about the belief function. Farmer claimed that "confidence is a separate independent fundamental just like preferences, endowments and technology" and that "it drives the business cycle" (Farmer 2010b: 113). This proposition comes out of the blue instead of being substantiated. Farmer's view was that changes in belief in the value of capital exert a self-fulfilling effect. But what explains them? The opposite, which is that changes in the value of capital, themselves reflecting the real economy, have an impact on beliefs, is as plausible. Clearly, these ambiguities cast doubt on Farmer's bold policy prescription (which, it must be said, he presented as tentative).

I regard Farmer's work as the fruit of a solo exercise attesting to both the inventiveness of its performer and the plasticity of the neoclassical toolbox. Yet in the Preface of his book, he expressed a bigger ambition, "to overturn a way of thinking that has been established among macroeconomists for twenty years" (Farmer 2010a: VII). To this end, he will need to gather a following of economists working on enriching his model.

LEIJONHUFVUD AND AGENT-BASED MODELING

Leijonhufvud's *On Keynesian Economics and the Economics of Keynes* was a criticism of Keynesian macroeconomics for having taken "the wrong track away from where it had begun" (Leijonhufvud 1993: 7) by focusing on wage rigidity instead of coordination failures. The fact that the last installment of DSGE models were called 'new Keynesian,' was hardly enough for Leijonhufvud to change his mind.¹⁰

As seen in Chapter 6, Leijonhufvud regarded Keynes's real contribution to economic theory as having pinpointed the problem that macroeconomist should tackle, the self-coordinating ability of market economies, rather than

¹⁰ "The technically sophisticated DSGE theory of today shares with the simple atemporal general equilibrium theory of 1950s vintage a fundamental preconception, namely, that the economy can be truly represented as a stable self-regulating system in which effective 'market forces' will always tend to bring it into a state of general equilibrium except in so far as 'frictions' of one sort or another put the brake on the equilibrating process. I believe that this macro-theoretical preconception is false, that it is based on a fundamental misunderstanding of the nature of the market economy, and that further technical innovations in mathematical modeling or econometrics will not bring real progress as long as this remains the ruling paradigm" (Leijonhufvud 2009: 2).

as solving it in any significant way. We also saw in Chapter 1 that to achieve such a project, it was necessary to zero in on the laws of motion of the economy. Marshall and subsequent Marshallian economists took it for granted that these laws worked well. The Great Depression destroyed this optimistic view. This prompted Keynes to set himself the task of revising Marshallian theory by trying to demonstrate that, under certain circumstances, the laws of motion could go astray to the effect that markets, in particular the labor market, would fail to converge towards their normal point attractor. Unfortunately, Keynes lacked the tools for such a project. The result is that *The General Theory* hardly implemented Keynes's intuition. Except for a few occasional remarks and Chapter 19's vague argumentation, Keynes was unable to tackle the issue of adjustment. Instead, to all intents and purposes, he fell back on explaining the existence of involuntary unemployment in a given market period. In Leijonhufvud's words, "Keynes's theory was dynamic in substance, but static in form" (Leijonhufvud 1976: 95).

Leijonhufvud repeatedly argued that Keynes's failure was no reason for abandoning the view that coordination failures are the main threat to the functioning of the market system. Therefore, they should constitute the overarching object of study of macroeconomics. Leijonhufvud hammered home the point that faithfulness to this project was the yardstick to be used in assessing the progress of macroeconomics. Good reasons exist in support of this viewpoint. The economy is a complex system, its pathologies should not be excluded as a matter of premise, and adjustment failures constitute a prime suspect. But Leijonhufvud's admission that the tools needed to achieve this program were lacking provided an easy explanation for not following his line. Friedman (it takes a new model to dismiss an old one) or Wittgenstein (one should keep quiet on topics for which the adequate concepts are lacking) could easily be invoked in defense of existing theory. All this, confined Leijonhufvud to the position of righter of wrongs for decades. The high respect in which he was held notwithstanding, his critical remarks had little impact.

In the 1990s, however, things began to change for the simple reason that the tools that were lacking earlier became available, the result of the computer revolution. The complex non-linear dynamics of the Marshallian laws of motion may still lack analytical solutions, but this failure pales into insignificance in view of the possibility of producing computational ones. As a result, Leijonhufvud was no longer a lone voice in the wilderness. Followers started to gather around him, not philosophically-minded people like him, but technicians holding computational expertise. Their work evolved into 'agent-based computational economics', and Leijonhufvud decided to endorse it:

Keynes's theory of how a monetary economy can fail to coordinate activities 'automatically' was flawed. But what we have on the other side is little more than blind faith in the stability of general equilibrium. The matter cannot in all intellectual decency be left there. Agent-based methods provide the *only* way in which we can explore the self-regulatory

capabilities of complex dynamic models and thus advance our understanding of the adaptive dynamics of actual economies. (Leijonhufvud 2006c: 1636–7)¹¹

Tesfatsion, one of its leading proponents, characterizes agent-based computational economics as “essentially a collection of algorithms (procedures) that have been encapsulated in the methods of software entities called ‘agents’” (Tesfatsion 2006: 179). These are tools borrowed and adapted from disciplines such as psychology, biology, physics, and computer science. Howitt provides a more colorful description:

As described by Tesfatsion (2006), agent-based computational economics is a set of techniques for studying a complex adaptive system involving many interacting agents with exogenously given behavioral rules. The idea motivating the approach is that complex systems, like economies or anthills, can exhibit behavioral patterns beyond what any of the individual agents in the system can comprehend. So instead of modeling the system as if everyone’s actions and beliefs were coordinated in advance with everyone else’s, as in rational expectations theory, the approach assumes simple behavioral rules and allows a coordinated equilibrium to be a possible emergent property of the system itself. The approach is used to explain system behavior by ‘growing’ it in the computer. Once one has devised a computer program that mimics the desired characteristics of the system in question, one can then use the program as a ‘culture dish’ in which to perform experiments. (Howitt 2008: 157)¹²

A detailed description of agent-based modeling is beyond the scope of this book. Let me just mention three features, which align it with Leijonhufvud’s vision. First, agents’ laws of motion, which are now more numerous, are the building blocks of these models. Second, the representative agent assumption is removed (in a more radical way than in DSGE models with the introduction of heterogeneous agents). By construction, agent-based models give ample room to the structure of agents’ interactions. A third trait is a new attitude towards equilibrium, a rejection of the equilibrium discipline, and the integration of effective individual disequilibrium states in the model – a major difference with the DSGE methodological standpoint:

One important characteristic of this new work is that it is uninterested in full agent model equilibria. As biologist Stuart Kaufman has remarked, “An organism in equilibrium is dead.” Instead, the new work looks for *system equilibria*, in which agent disequilibria offset each other so that the aggregate system is unchanging, even though none of the components of the individual agents in the model is in equilibrium. (Colander *et al.* 2008: 238; their emphasis)

¹¹ In his endeavor to redirect the course of macroeconomics, Leijonhufvud has had many allies and supporters. Some of them, namely, Colander and Page, participated in the U.S. Congress Hearing discussed in the previous section. Examples of papers making a similar plea are Colander *et al.* (2008), De Grauwe (2009, 2010), Hoover (2001, 2006), and Kirman (2010). For example, Kirman writes: “Macroeconomic theory has insisted on having ‘sound micro-foundations’ and I argue that this has taken us down the wrong road” (Kirman 2010: 507). This is Leijonhufvud’s claim.

¹² For more technical descriptions, see Colander (2006).

KRUGMAN'S PLEA FOR A RETURN TO KEYNES

Joseph Stiglitz (already discussed in Chapter 12) and Paul Krugman are two famous economists, both Nobel Prize winners, who in books, magazines and lectures, have time and again voiced strong critics of the DSGE approach. In this section, I propose myself to zero in on the case of Krugman by examining one piece that I find representative, an essay entitled "How Did Economists Get it so Wrong"? published in the *New York Times Magazine* issue dated September 6, 2009.¹³

As its title makes it clear, Krugman's article is a full-scale attack against present-day macroeconomics. To him, it has gone astray by ceasing to be Keynesian and becoming neoclassical, the result of DSGE macroeconomics holding sway. In his words, the discipline has fallen into a "dark age, in which hard-won knowledge has been forgotten" (Krugman 2009: 2). He argued that this regression spans four dimensions. The first one is methodological: "economists, as a group, mistook beauty, clad in impressive-looking mathematics, for truth" (p. 3). The second concerns the economic worldview that underpins the theory. Neoclassical economics is based on the presumption that one must have full faith in the market system (p. 4) and that, to be worthwhile, economic analysis must be based on "the premise that people are rational and markets work" (p. 13), traits that run counter to what can be observed in reality. From these premises ensues the unacceptable depiction of unemployment as tantamount to "a deliberate decision by workers to take time off" (p. 15). With such a Panglossian vision (Krugman's expression borrowed from Keynes), macroeconomics is hardly equipped to address market failures. It is a small wonder hence that it was unable to predict the crisis, his third indictment. Having no tools for analyzing it, it offers no guidance for policy. Finally, Krugman attacks finance theory: "the belief in efficient financial markets blinded many if not most economists to the emergence of the biggest financial bubble in history" (p. 19). According to Krugman, none of these flaws is to be found in Keynesian theory. By contrast, the latter succeeded in capturing the essence of recession, the insufficiency of aggregate demand; it also pointed to the right policy measure, fiscal policy (rather than monetary activation).

Krugman's article is well-crafted and beautifully written. Appraising it is a delicate enterprise. On the one hand, it is a magazine rather than a scientific piece. On the other hand, since it consists in an account of the state of macroeconomics, its validity must be gauged. On this score, I am on the side of his detractors.¹⁴

¹³ I will not discuss Stiglitz's case because it is more complicated since he kept contributing to the theoretical conversation in its meta-theoretical niche by writing substantive criticisms of the DSGE program. Cf. e.g. Stiglitz (2011).

¹⁴ Cochrane has been one of them, and for that matter a vehement one. Cf. Cochrane (2009).

My criticism is fourfold. The first one is that the opposition between Keynesian and neoclassical theory made by Krugman is too rudimentary. To him, the 'neoclassical' term designates the defense of the *laissez-faire* policy. I find this definition unsatisfactory. Old classical economists, such as Smith and Ricardo, defended such a policy, yet they cannot be called neoclassical. Likewise, neoclassical synthesis economists are neoclassical yet they criticize *laissez faire*. A second criticism is that Krugman's rhetoric is misleading. Let me just give one example:

But the self-described New Keynesian economists weren't immune to the charms of rational individuals and perfect markets. They tried to keep their deviations from neoclassical orthodoxy as limited as possible. (Krugman 2009: 7)

A more neutral account would run as follows:

For tractability reasons, new Keynesians came to the view that it was a good strategy to produce theory under the standards set by Lucas. They also decided to follow Plosser's view that it is sounder to start the analysis with an idealized description of the economy with the objective to depart from it in an incremental way.

Turning to my third point, I have claimed that the DSGE program, though progressive, carries a series of limitations that cannot be overlooked. Two of them are especially important. The first is that theoretical propositions pertain to the model economy and not to reality. The second is that its scope is limited to moderate fluctuations. Ignoring these limitations is a fault, sadly enough in my eyes a too frequent one. A macroeconomist, who declares that market clearing is a feature of reality, that real-world people have rational expectations, and that involuntary unemployment cannot exist in reality errs on a wrong epistemological track. But the existence of these misinterpretations does not make the theory wrong. So, Krugman's criticisms do not point to intrinsic theoretical flaws.

Finally, Krugman's declaration that Keynesian theory is superior to neoclassical theory is nothing more than an argument of authority. No vindication in favor of it is given. For my part, I have argued that both the economics of Keynes and Keynesian economics have been plagued with important unsolved conceptual ambiguities. I also disagree with Krugman's view that Keynesian theory is a fine toolbox to explain great depressions. I rather think that economic historians are more apt than macroeconomists, be they Keynesian or Lucasian, to shed light on such occurrences.

Where I depart from the other economists who strongly reacted to Krugman is that to me their criticisms cannot be the last word on the matter. To make my point, a wider perspective must be taken. To begin with, let me repeat an observation that I already made earlier in the book about the specificity of macroeconomics. It studies the workings of market economies, aiming at answering the question of what is the best way of organizing society in its economic dimension. To a large extent, this amounts to deciding the respective

role and place of government and market forces. Policy matters are thus at the center of macroeconomics at a higher pitch than in the hard sciences and, probably, also than in other social sciences and other sub-disciplines of economics. Hence normal citizens are also stakeholders in macroeconomics.

As a result, 'conversations' about macroeconomic issues evolve at different levels. Let me distinguish three such levels: the 'peer-controlled conversation (or theoretical conversations),' the 'public intellectual conversation,' and the 'armchair conversation.' In this book, I have exclusively been concerned with the 'production-of-theory conversation,' a sub-component of the peer-controlled conversation. Its main aim is to produce new scientific knowledge. An essay like mine belongs to another section of the peer-controlled conversation. It is concerned with commenting on the production of theory; it is meta-theoretical in that it does not make theory progress but (hopefully) provides some interesting light on the evolution of theory. Another section of the peer-controlled conversation is the 'art-of-policymaking conversation'; its protagonists are also professional economists. As said by Colander in the U.S. Congress Hearing, these economists are consumers of theory rather than producers of it. The task they have set out for themselves is to operate the transition from theory to concrete policy decision making, which usually implies significant emendations of theoretical propositions. Theoretical conversations, whatever its variant, mainly occur in written by pieces published in scientific journal and having to pass a peer review control. They also take place in conferences and seminars but there also they are addressed to a professional audience and undergo professional control. Such control schemes are absent from the other two levels. Discourses by public intellectuals most often have a policy purpose. Their aim is persuasion. They are led by personalities who, for one reason or another (possibly an earlier acquired theoretical eminence), have become notable enough to have access to the media. Here, according to people's talents and backgrounds, the discourse may be more or less articulated, yet little control can be exerted on the value of its argumentation. Almost anything goes as soon as access to the media exists. Other public intellectuals may react, yet usually with little avail. Finally, we all, whether economists or not, engage in armchair conversation on macroeconomic subjects. Here, as Keynes noticed in the last section of *The General Theory* (1936: 383), people argue without being aware that they are usually distilling the views expressed by the great forefathers ancestors, Adam Smith, Karl Marx, and John Maynard Keynes, sometimes in odd amalgamations.

All these conversations are legitimate and they partially overlap, yet they follow different rules of the game. People may engage in several of these. Throughout his career, Friedman spanned all of them (assuming that he talked economics at the dinner table). Lucas is the opposite because he scarcely moved away from the theoretical conversation. It may also happen that at some point in their career people shift from the theoretical conversation toward the public intellectual conversation. This brings me back to Krugman.

It is clear that Krugman's article is a partisan one, but this is true of any public intellectual contribution. Attempts at persuading people about a cause in which one believes implies presenting things in an advantageous way. Caricaturing the ideas one is fighting is part of the game. Bad faith and blind spots are thus omnipresent. Hence the criticisms addressed to his paper when gauged as a contribution to theory (of the meta-theoretical type) are deserved. However, there is another issue that should not be eluded. It consist in asking whether there is an acceptable rationale behind the decision made by economists such as Krugman to change gears by abandoning the theoretical conversation in favor of the political one. I believe there is.

If the DSGE program has been so progressive, it is because it took the Walrasian bifurcation with its priority given to internal consistency. To refer to Walras's *note d'humeur*, quoted in Chapter 17, taking this bifurcation is tantamount to planting an oak. The benefit to be gained (the shadow the tree will provide) will take a long time to materialize. A high dose of patience is needed. Such an attitude was not a problem for earlier neo-Walrasian economists. They knew that their theory had no direct utility and ascribed it no precise policy conclusion. To them, the non-exploitation principle of any general conclusion reached in their model was easy to abide by.

Things changed drastically when macroeconomics became Walrasian. Indeed, macroeconomics is concerned with the *hic et nunc* of policy making. Policy conclusions are at its heart. In such a new context, abiding by the non-exploitation principle becomes more difficult, almost untenable. For better or worse, decision makers have been used to considering macroeconomists as experts. They want them to continue to play this role, and it is hard to refuse such flattering solicitations. Moreover, all macroeconomists are not pure technicians. Several also hold an ideological vision (and let me repeat that to me there is nothing wrong about this).

Let us then assume the presence of two subgroups of macroeconomists, supporters of the laissez-faire ideology, on one side, and of the Keynesian ideology, on the other. Present-day state-of-the-art macroeconomics makes it biased against the Keynesian because of the decision of starting the analysis with a rosy description of the economy. As a result, laissez faire economists are in a comfortable position. The policy conclusions of DSGE models are congruent with their prior ideological preference. Therefore, except if their degree of epistemological alertness is high, they may fall prey to unduly selling these conclusions to policymakers and wider audiences. The opposite is true for macroeconomists with a Keynesian inclination. With macroeconomics as it stands after the Lucasian revolution, making a theoretical case favoring Keynesian conclusions amounts to fighting with one hand tied behind one's back: the task is not impossible but it is difficult.

Therefore, for economists such as Stiglitz and Krugman who have a strong ideological motivation, it is understandable that at some point in their careers they might decide to leave the theoretical conversation to tackle the real issues

that to date cannot be addressed in the theoretical language because of the straightjacket that its standards impose. It is also no wonder that Stiglitz and Krugman became more vehement after the outbreak of the recession because the defects of DSGE macroeconomics emerged as more outrageous than in the times of the great moderation. That their writings as public intellectuals will have no impact on the development of macroeconomics, I guess, matters less to them than its impact on wider audiences (and voters).

Looking Back, Looking Ahead

In this epilogue, I want to bring together the different threads I have explored in the book. I begin by drawing three general lessons. Then, I identify the most significant decisional nodes that the development of macroeconomics encountered. Finally, I briefly ponder the impact of the 2008 recession on the future development of the field.

GENERAL LESSONS

Progress

My aim in writing this book was to put some order in the history of macroeconomics from Keynes to the 2008 recession, excluding the latter. Throughout, Leijonhufvud's decision-tree metaphor has served as a guiding thread. Adopting it implies that the evolution of the field cannot be considered in a linear way, contrary to what many commentators argue.¹ Research programs wax and wane. Methodological conflicts abound. There are 'winners' and 'losers'. Writing the history of a discipline almost by definition amounts to writing about the successful tracks rather than about those which could have been followed, yet hardly were, or thrive to a lesser extent. Although this amounts to focusing on the contributions of the winners rather than on those of the losers, it does not amount to writing the history from the point of view of the winners, so-called Whiggish history. Indeed, one of the advantages of Leijonhufvud's grid of analysis is to bring out that the future of a discipline is

¹ For example, Chari writes: "This tendency to mark all key developments in economics as revolutionary is popular enough but, in my view, it is a misreading of the history of economic thought. My thesis is that Lucas's work is very much a part of the natural progress of economics as a science" (Chari 1998: 171).

open. Theoretical victories can only be temporary. Once these restrictions are admitted, my judgment is that the DSGE program, through its three stages of development, has proved able to make impressive progress *within the given track that it decided to take*.

Revolutions

Macroeconomists have been fond of using the revolution metaphor. In addition to the ‘Keynesian revolution,’ we encountered the ‘rational expectations revolution,’ the ‘monetarist revolution’ (or ‘counterrevolution’). For my part, I find Kuhn’s scientific revolution concept useful as long as it is not overused. In my opinion, only two episodes in the history of macroeconomics qualify for it: the Keynesian revolution, the inaugural revolution, and what I have termed the ‘Lucasian or DSGE revolution.’ The Keynesian revolution consisted in a threefold transformational attempt: generalizing Marshallian partial equilibrium analysis, getting rid of market clearing as a necessary outcome of economic theory, and merging monetary and real theory which used to be separate. Although Keynes paved the way for this innovative process, its stabilization occurred only in a second stage with the ascent of the IS-LM model. This model came to be at the core of macroeconomics as a new sub-discipline of economics. As for the Lucasian revolution, I characterize it as a transformation from Marshallian to Walrasian macroeconomics, accompanied with more stringent standards for theory construction. It was a methodological revolution, which replaced outward direct relevance with internal consistency. Like in the case Keynes, Lucas was the initiator of a revolution but its stabilization was left to others, namely Kydland and Prescott. I have proposed to separate three stages (up to now) in the evolution of the DSGE program: new classical modeling, associated with Lucas’s money-supply surprise model, RBC modeling, and what is usually called either ‘new Keynesian’ or ‘DSGE’ modeling. I have opted for the first of these appellations while adding to it the ‘second generation’ qualifier because of my contention that their methodological positions significantly differ from those found in new Keynesians’ writing in the 1980s.

The revolution notion points to a breach of consensus within a profession. Hence the history of macroeconomics can also be examined through this lens. While Keynesian macroeconomics was consensual at first, it was eventually attacked by monetarism, an attack that I have judged non-lethal in contrast to the new classical offensive, which successfully overthrew the Keynesian paradigm. The ascent of DSGE macroeconomics took place amidst harsh controversies. The new classical attack was less disguised than Friedman’s – think of Lucas and Sargent’s “After Keynesian Macroeconomics” paper or Lucas’s “Death of Keynesianism” lecture. Keynesians’ reactions, first from traditional Keynesians and, then, from first-generation new Keynesians, were equally fierce. However, after a decade of brawls, the hatchet of war was buried

and a new generation of self-styled new Keynesian economists (including a few from the first generation) came to the view that after all it might be a good strategy to try to bend the RBC methodology towards Keynesian themes.

Ideology

Another general lesson that I draw from my analysis is that it is misleading to describe macroeconomics as a merely positivistic discipline. It comprises a compelling ideological dimension. Not that every macroeconomist is an ideological fighter – the contrary is most probably true. It is rather that this dimension is inherent to the object of the field. Policymaking is its ultimate concern. Hence what is at stake is the ideal way of organizing society in its economic dimension, an eminently political matter. This being said, I agree with Lucas's standpoint that, if ideology cannot be erased, it must be controlled. Holding theoretical macroeconomic conversations would resemble the arguments on economic policy that we have with friends at the dinner table or the battle of ideas in newspapers of opposed ideological opinions were it not for one important difference. Those who engage in theoretical controversies accept to abide by some standards for acceptable theorizing. The more explicit they are, the better. As stated, the rise of DSGE macroeconomics amounted to giving prominence to internal consistency over realism. I find this line defensible yet it bears a heavy price, namely that macroeconomists must refrain from claiming that the policy conclusions of their models have a direct policymaking bearing.² In an age when expertise is so valued, macroeconomists should refuse to play the part of experts and admit that their social usefulness is of the same subdued variety as that of political philosophers!

SIGNIFICANT DECISIONAL NODES

Comparing the development of macroeconomics to a decision tree raises the question of which decisional nodes played a significant role in it. In this section, I examine the most notable ones.

The Marshall-Walras divide

As I have devoted a full chapter to the Marshall-Walras divide, at this late juncture I have nothing more to add about it except to repeat that, in my eyes, it is a powerful benchmark to understand how macroeconomics evolved.

² It is not that, when Keynesian macroeconomics held sway, economists were in a better position to act as experts. But then the incongruity of acting in such a way was less obvious since their models looked realistic.

The involuntary/underemployment divide

The first node in the macroeconomic decision tree is a bifurcation between models reaching a welfare-optimizing conclusion and those reaching a sub-optimal outcome conclusion. Within the second bifurcation, a further fork separates the rationing track from the underemployment track. The rationing track is further sub-divided between involuntary unemployment regarded as a case of individual disequilibrium and involuntary unemployment in the casual sense. Table 21.1 describes how the main models I have studied can be classified across these different tracks.

The involuntary unemployment notion makes sense as far as the real-world is concerned. The problem lies in its integration into the theoretical language. This is small wonder: neoclassical theory is predicated on

TABLE 21.1 *Claims made about labor market outcomes*⁴

	Sub-optimal outcome			Optimal outcome
	Involuntary unemployment (indiv. disequil.)	Involuntary unemployment (casual sense)	Underemployment	
Keynes's <i>General Theory</i>	✓			
IS-LM macroeconomics (à la Hicks)	✓			
IS-LM macroeconomics (à la Modigliani)			✓	
Disequilibrium <i>à la</i> Patinkin and <i>à la</i> Clower-Leijonhufvud	✓			
Natural rate of unemployment modeling:				
– Phelps		✓		
– Friedman			✓	
Non-Walrasian equilibrium models	✓			
DSGE I: Lucas				✓
FGNK modeling:				
– Efficiency wages		✓		✓
– Staggering contracts			✓	
Alternative programs:				
– Diamond			✓	
– Roberts		✓		
– Hart			✓	
– Carlin and Soskice			✓	
DSGE II: RBC modeling				✓
SGNK modeling			✓	

the premise that agents are optimizing decision makers, which runs counter to non-chosen outcomes. *The General Theory*, IS-LM models à la Hicks, and non-Walrasian equilibrium models produce an involuntary unemployment result only by forcing exogenous rigidity onto the model. As shown, Patinkin's sluggishness approach was not a success either. As for efficiency wages modeling, the specific model I have studied, the shirking model, reaches a result that can be called involuntary unemployment, yet this result is efficient, which hardly fits the usual motivation of Keynesian economists.

My study illustrates that economists with a Keynesian inclination face a dilemma about the objective they should pursue. On the one hand, involuntary unemployment is a rallying theme for them, yet demonstrating it in a non-trivial way has proven difficult and has not necessarily supported demand activation. On the other hand, demonstrating the usefulness of demand activation, the overarching Keynesian policy measure, does not require the existence of involuntary unemployment; it can be achieved by adopting the underemployment line. Moreover, as witnessed by staggering contracts models, this line allows for giving some foundation to the rigidity notion. First-generation Keynesian economists were divided about this choice. They all proclaimed that they were concerned with involuntary unemployment, yet to all intents and purposes models pursuing the Modigliani line had only underemployment as their object of analysis. Gradually and up to the present, the underemployment strategy came to prevail. However, involuntary unemployment has kept such a totemic status, as if it were the last hill to fight for, that it keeps cropping up even if the label is inappropriate.³ For those who put a high price on conceptual clarity, as I do, this is unfortunate.

Supporting or rejecting the neoclassical synthesis viewpoint

Another benchmark that I find useful for understanding the evolution of macroeconomics is the standpoint taken about the neoclassical synthesis viewpoint. This term can be understood in two ways: either as a synonym of IS-LM modeling or as a reference to the relation between Keynesian and classical theory. In the first case, it has no value-added. In the second one, a further distinction must be made between two possible meanings, one which makes sense semantically, while the other is semantically troublesome. I start with the former. At stake is the establishment of a Keynes-Walras synthesis, more precisely demonstrating how short-period Keynesian disequilibrium states might gravitate towards a long-period Walrasian equilibrium state. We have seen that Patinkin's attempt at doing this was a failure. I now turn

³ See, e.g., Christiano, Trabandt, and Walentine (2011) and Gali, Smets and Wouters (2011).

⁴ FGNK is the acronym for first-generation new Keynesian and SGNK for second-generation new Keynesian.

to the other meaning. Here, we have a case of semantic reversal in which an expression has come to mean the exact opposite of its literal meaning. That is, the neoclassical synthesis viewpoint has turned out to be a consensus on the non-necessity of building a synthesis between Keynesian and Walrasian analysis, while nonetheless asserting that both are necessary. As explained in Chapter 2, it all started with Hicks. Not only did he praise Keynes for having shifted the emphasis in monetary theory from the long to the short period. He also made the point that the short and the long period could be studied independently from each other. This view carried on throughout the reign of Keynesian macroeconomics. The neoclassical synthesis then came to designate the defense of an eclectic macroeconomic field, with room for different, admittedly incompatible, modeling strategies. As seen in Chapter 8, Lucas struck a blow against this view by proposing a set of standards specifically tailored to Walrasian theory and by declaring that this theory could take over the job assigned to Keynesian theory earlier. First-generation new Keynesian economists regarded the defense of the neoclassical synthesis idea as an important component of their response to Lucas. More or less grudgingly admitting the validity of Walrasian theory for the long period, they adamantly refused its hegemony. Thus, there was a time when the confrontation between new Keynesians (of the first generation) and new classical economists could be seen as bearing on the acceptance or the rejection of the neoclassical synthesis. This state of affairs came to an end with the ascent of second-generation new Keynesian modeling. Second-generation new Keynesians took a different standpoint. Abandoning the defense of an eclectic macroeconomics was one of the concessions that they made when they created a Keynesian/RBC synthesis (this time, a synthesis in the strong sense of the term).⁵

The march toward full general equilibrium analysis

In the Preface to the French edition of *The General Theory*, Keynes wrote that he used the adjective ‘general’ in the title of his book to indicate his concern with the behavior of “the economic system as a whole” (Keynes [1939] 1979, p. XXXII). That is, he wanted to study unemployment as an occurrence resulting from the interaction between the different sectors of the economy. He did not use the expression, yet it corresponds with what we call general equilibrium analysis today. The latter has two basic features: (a) the study must bear on an entire economy and (b) it must deal with the interactions of the composing sectors of the economy rather than study them separately.

⁵ The neoclassical synthesis is still alive as a minority viewpoint. The following statement, drawn from the back cover of a 2012 book edited by Solow and Touffut, *What’s Right with Macroeconomics?*, illustrates this: “The contributors, nine highly-renowned macroeconomists, highlight the virtues of eclectic macroeconomics over an authoritarian normative approach.”

TABLE 2I.2 *Classifying models against the requirement for full general equilibrium analysis*

	No full general equilibrium analysis	Full general equilibrium analysis
The theoretical IS-LM model	✓	
Empirical IS-LM modeling (the Klein-Goldberger model)	✓	
Non-Walrasian equilibrium modeling		✓
DSGE I: Lucas		✓
FGNK models	✓	
Alternative models (Diamond, Hart, Taylor)		✓
DSGE II: RBC model		✓
SGNK modeling		✓

The extent to which the different models that marked the history of macroeconomics have abided by these two criteria provides another interesting benchmark for bringing order to the history of macroeconomics. Table 2I.2 illustrates that it took time for macroeconomics to become a fully general equilibrium type of analysis.

The Keynesian/non-Keynesian divide

If there is one divide that outwardly seems important to understand the history of macroeconomics it is the Keynesian/non-Keynesian divide. However, at the end of my inquiry, it turns out to be murky.

During the heyday of Keynesian macroeconomics, the meaning of these notions was relatively straightforward. The Keynesian qualifier comprised two elements. The first one relates to a vision of the pros and cons of the market economy. It can be viewed as a catchword encompassing authors who think that, for all its virtues, the market economy may exhibit market failures, which can be remedied by state interventions, in particular demand stimulation. The opposite, or anti-Keynesian, standpoint is then the view that the unfettered working of competition will lead the economy to the best attainable position. In short, it can be associated with the *laissez faire* motto. These are the two ideologies I referred to in previous chapters, the ideology term receiving no pejorative connotation. The second element refers to the methodological tradition that started with Marshall, was extended to the study of the economy as a whole by Keynes, and systematized in a formal model by Hicks and Modigliani – the ‘Marshall–Keynes–IS-LM conceptual apparatus.’ In this respect, being a Keynesian meant importing the wage rigidity notion into this broadened Marshallian apparatus, supposedly causing

unemployment, the latter having then to be fought by demand activation. At the time, non-Keynesian macroeconomics also belonged to the same Marshallian tradition and their conceptual apparatus was close to the Keynesian one. The difference was the prevalence of the wage flexibility assumption, which in turn led to the uselessness of demand activation. Friedman was the emblematic non- or anti-Keynesian economist, but his antagonism related to the vision rather than to conceptual apparatus dimension.

Things changed with the concomitant ascent of Lucas's model and of non-Walrasian equilibrium in the 1970s. A real alternative to the Marshall-Keynes-Hicks conceptual apparatus was offered, the neo-Walrasian conceptual apparatus. Lucas was an economist who, like Friedman, was non- or anti-Keynesian on the score of ideological vision. Unlike him, he was also anti-Keynesian on the score of the conceptual apparatus. Non-Walrasian equilibrium economists were the opposite of Friedman. They were Keynesian on the score of policy, but non-Keynesian on the score of the conceptual apparatus. Table 21.3 summarizes this.

Up to then, the Keynesian/non-Keynesian divide was still manageable. Subsequently, semantics got out of control. Models were labeled Keynesian because they assumed stickiness or imperfect competition, whether or not they demonstrated involuntary unemployment or underemployment, and whether or not they recommended demand activation. In Lucas's words:

If the term Keynesian now means the use of any policy that improves economic efficiency (antitrust policy, say) then the term is meaningless. Why not Marshallian or Chamberlian? (Lucas. Various. Box 6, correspondence 1985-88).

Many economists of the first rank continue to describe themselves as 'Keynesians,' and many interesting new research ideas are motivated as addressing 'Keynesian questions' or as taking a 'Keynesian approach.' ... These modern economists do not, I think, claim detailed precedent in Keynes for their ideas or method, and many have not even read him. In adopting the label 'Keynesian' they are identifying themselves not so much with particular economic theories as with an activist, freewheeling spirit in applying economics to practical problems. (Lucas 1995: 917)

Does this mean that the Keynesian/non-Keynesian divide is useless? Not necessarily. It still sheds light on the history of macroeconomics, if understood in the narrow ideological meaning and further confined to models in which market failures can be remedied on by demand activation. It then turns out that when taking the different episodes that I have surveyed and looking at their policy conclusions, a to and fro between the Keynesian and the non-Keynesian standpoints can be observed. Table 21.4 illustrates this.⁶

My opinion is that this to and fro will continue in the history of our field in the future.

⁶ The picture given in Table 21.4 is too rough regarding the two generations of new Keynesian modeling. Some first-generation models do justify demand activation, yet others do not. As for

TABLE 21.3 *A typology of macroeconomic models in the 1970s*

		The policy cause defended	
		<i>Demand activation</i>	<i>Laissez faire</i>
Conceptual apparatus	<i>The Marshall-Keynes-IS-LM line</i>	<i>The IS-LM model</i>	<i>Friedman's monetarism</i>
	<i>The Walrasian line</i>	<i>Non-Walrasian equilibrium models</i>	<i>Lucas's model</i>

TABLE 21.4 *The policy conclusions of the models*

	Justifying demand activation	Laissez faire
<i>The General Theory</i>	✓	
The IS-LM model	✓	
Disequilibrium theory and non-Walrasian equilibrium models	✓	
Monetarism		✓
New classical models (Lucas)		✓
First-generation new Keynesian models	✓	
RBC modeling		✓
Second-generation new Keynesian models	✓	

THE IMPACT OF THE 2008 RECESSION

Readers are probably eager to learn about what happened to macroeconomics as a consequence of the Great Recession. Unfortunately, this is a matter on which, for the time being, historians of economics cannot deliver. The reason is simple: history can be written only after the dust has settled. Therefore, I will content myself with making three brief general remarks.⁷

The first bears on the question of whether the DSGE approach can explain the 2008 recession. The answer is yes if by 'explaining' one means telling a story. For example, Prescott did this in a conference given in Paris in July 2009 (Prescott 2009). His story runs as follows. An exogenous shock, occurring in the financial sector, affected the economy. As a reaction, households started fearing future high taxes deemed necessary to compensate for the bailing out

second-generation models, they clearly favor price stabilization, but they feature less unanimity about demand activation.

⁷ A point of secondary interest is the criticism leveled at present-day macroeconomics for having failed to predict the outbreak of the crisis. To me, this objection carries little weight. It is in the nature of a crisis to come as a surprise. Had it been anticipated, measures to avoid it could have been taken.

of the banking system by the State. Because of this fear, businesses, and in particular small businesses, cut investment and took more cash out of the business sector. Employment fell because of a shift in both the demand for and the supply of labor. Households cut their durable consumption. This accounted for the drop in activity. As far as the nature of the shock was concerned, not surprisingly, Prescott viewed it as a government failure, the addition of two mistakes. The first, which went back to Clinton's presidency, was the U.S. government's political pressure on state-controlled mortgage companies to extend mortgages to households that could not afford them. The second was the FED's mistaken low interest rate policy.

The problem with this story is that it is just one among other possible ones, which actually partially overlap. Limiting myself to economists I have discussed in this book, I personally prefer Winter's story presented during the House of Representatives session. I am of the opinion that when it comes to explaining large-scale recessions – like the Great Depression or the present recession – macroeconomists should yield to economic historians. Hopefully, the latter should use economic theory as much as possible. Yet, their specific job is to go beyond it by integrating the chain of events, political and institutional factors, all the *hic et nunc* that is important to understand history, and which almost by definition falls through the cracks when doing theory. Lucas wrote that what made business cycle theory possible was that all cycles share the same features. The contrary could be written about great recessions: their specificities are more important than their commonalities. Moreover, the DSGE program is based on the premise that it is wise to start the analysis by constructing a highly idealized description of an economy. Because of the exigencies of internal consistency, at present departures from the idealized state are still minor. Therefore, as a matter of construction, DSGE models, at least as they stood before the recession, exclude the possibility of integrating important pathologies into the workings of the market system, and certainly any collapse in the trading system of the extent that we have experienced. Thus, we are back to the remark I made earlier in this chapter and to Chuang Tzu's motto quoted in Chapter 17: theoretical macroeconomics, as it stands today, has a more limited usefulness than what the general public and epistemologically naïve macroeconomists tend to think.

My second remark is that the crisis has resulted in a shift in visibility between the defenders of the free market and Keynesian economists. The former are now on the defensive and the latter are cheering up after two decades of gloom. Nonetheless, as argued in the previous chapter, to get the right perspective, a distinction must be drawn between what is going on in the sphere of the media and meta-theoretical essays, on the one hand, and in the academic world, on the other. Two prominent defenders of Keynes are Skidelsky, Keynes's biographer and the recent author of *The Return of the Master* (Skidelsky 2009), and Krugman, discussed in the previous chapter. They share the same simple message: one should return to Keynes! I disagree with them.

Their mistake is to fail to distinguish between Keynesianism as a policy vision and as a conceptual apparatus. Instead, they assume that these two aspects are organically intertwined. For my part, I think that the Keynesian idea of market failures will be emphasized anew, but I doubt that any return to the Keynesian conceptual apparatus will occur. Claiming that one should return to a theory that was proposed more than seventy years ago amounts to assuming that no progress has been made since, and that the methodological choices that offered themselves at the time are still worth considering today. On the contrary, I think that the criticisms that Lucas made about Keynesian theory were relevant, and that his positive contributions, as well as those of Kydland and Prescott and the many economists who treaded their footsteps, will not be written off, though they will surely be superseded. Moreover, whereas macroeconomics used to revolve around exchanges of ideas about reality, it was transformed by the requirement to demonstrate propositions pertaining to a model economy (or, in other words, the conflation of the notions of theory and model). I believe that there is no going back. Any dilemma between the tractability constraint and that of real-world direct relevance will be solved in favor of the former. Therefore, I believe that Krugman's and Skidelsky's injunctions will have little impact on academic work.

My third and last remark on which I will be even briefer concerns the impact of the recession on theoretical developments. The 2008 recession will certainly leave an imprint on the course of macroeconomics. The clearest sign of this is the widespread admission that the loose integration of finance into macroeconomic models was a serious mistake, and the ensuing surge of research aiming to fill this gap. At this juncture, it is however still difficult to gauge whether a mere integration of the financial sector within the existing framework will suffice, or whether a more radical reorientation of macroeconomics will see the day of light.

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Index

- Abramovitz, M., 270-1
Ackley, G., 79
Agent-based models, 229, 358, 370-2
Aghion, P., 255
Akerlof, G., 225, 229, 236-7
Allison, F., 195
Alternatives to Lucas, 247
Altig, D., 170
Altonji, J., 285
Andolfatto, D., 285, 287-8
Andersen, L. C., 84
Ando, A., 41, 80-1, 206
Animal spirits, 7-8, 108, 248, 364-5
Archibald, G. C., 309
Arrow, K., 51, 55, 72, 180-1, 193, 347
Ashenfelter, O., 192, 285
Attfield, C., 158
Auctioneer, 12, 53-4, 114-17, 119, 121-2, 128, 131, 133, 135-6, 140-1, 157-8, 184-5, 194, 213, 248, 257, 301, 344-5, 350-3
Azariadis, C., 175, 364
 incomplete contract, 228-9

Backhouse, R., 24, 123, 129, 173
Baily, M., 225
Ball & Mankiw, 226-7, 236, 238
Ball, L., 225
Baranzini, R., 195
Barro, R., 57, 123, 136, 141-2, 151, 177, 262
Barro-Grossman model, 57, 123-9, 131, 134-5, 137, 139-41
Basu, S., 295-7

Bateman, B., 25
Batyra, A., 14, 45
Becker, G., 65
Benassy, J.-P., 123-4, 131-6, 138, 141, 144, 350-3
 Benassy model, 131-6, 140-1
Benhabib, J., 288-9, 364
Bernanke, B., 322
Bertrand, J., 53, 63
Beveridge, W. H., 14-15
Bils, M., 329
Blanchard, O., 47, 49, 108, 227, 309, 319, 326-7
Blinder, A., 199
Bliss, C., 156
Blundell, R., 298
Bodkin, R., 37, 40
Boianovsky, M., 56, 123, 129, 173
Bozio, A., 298
Brackman, S., 309
Branson, W., 10
Brock, W., 262-3
Browning, M., 287
Brunner, K., 65, 80, 85-6
Buiter, W., 195
Burns, A., 67, 72, 167
Burnside, G., 294-5
Business fluctuations, 25, 68, 74, 78-9, 152, 161-2, 175, 196-8, 226, 249, 251-2, 261-4, 266-8, 283-4, 293-6, 303-5, 310, 332, 351, 365
Business cycle theory. *See* DSGE macroeconomics

- Cagan, P., 65, 69, 76
 Caldwell, B., 299
 Calibration, 263-4, 274, 278-9, 289, 292, 297,
 301-2, 304, 324, 335, 353
 Journ. Econ. Persp. symposium on, 292
 Calvo, G., 310, 313
 Calvo pricing, 308, 310, 312-13, 318-19,
 324, 329
 Cambridge cash balance equation, 75
 Campbell, J., 268
 Caplin, A., 235
 Carlin, W., 226
 Carlin & Soskice model, 246, 352
 Carmichael, L., 231
 Carroll, C., 67
 Cass, D., 138, 193-4, 263, 275, 291
 Central bank, 49, 63, 66, 80, 92, 102-3, 159,
 274, 284, 308, 313-21, 325, 333, 335,
 355
 microfoundations, 317
 Chamberlin, E., 309, 328, 331, 385
 Chari, V. V., 329-32, 334, 378
 Chari versus Solow, 361-4
 Testimony U.S. House of Representatives,
 358-64
 Cherrier, B., 89
 Chatterjee, S., 317
 Chetty, C., 297
 Chicago, 35, 50, 55, 66, 72, 153-4, 170, 313,
 327
 Chick, V., 25
 Christ, C., 40, 72, 73
 Christiano, L., 289, 293, 295, 322-4, 332, 382
 Chuang Tzu, 299, 387
 Clarida, R., 307, 319-21
 Clower, R., 7, 16, 24, 57, 112-27, 131-2, 135,
 138-9, 141, 143-4, 255-6, 341, 346-7
 Clower's Counter-Revolution model,
 118-22
 Coase, R., 200
 Cochrane, J., 373
 Coddington, A., 49, 115
 Colander, D., xiii, 23, 225, 306, 372, 375
 Testimony U.S. House of Representatives,
 358-64
 Cole, H., 303, 329
 Complexity, 341, 351, 353
 Marshall and Walras on, 341-3
 Cooley, T., 262, 266
 Coordination failures, 118, 120, 247, 253-7,
 276, 302, 334, 370-1
 Copeland, M., 270
 Cournot, A.-A., 258-9
 Cournot-Nash, 257
 Cowles Commission, 26, 35, 38, 50-1, 54-5,
 72-3, 167, 189, 194, 278, 300, 315
 Danthine, J.-P., 289, 304
 Danthine, S., 287
 Danthine & Donaldson, 289
 assessing RBC modeling, 292
 RBC model with shirking, 289-91
 Darity, W. Jr., 21, 48
 Davidson, P., 85
 De Grauwe, P., 300, 302, 360
 De Long, B., 77, 92
 de Marchi, N., 65, 70
 De Vroey, M., 7, 10, 14, 30, 34, 45, 47-8, 71,
 106, 176, 199, 247, 280, 287, 303, 325,
 339
 Debreu, G., 51, 55, 72, 131, 180, 181, 184,
 193, 247, 347
 Decision tree metaphor, xvi, 173, 378
 Decisional nodes, 378, 380-6
 Demery, D., 158
 Dennis, R., 323
 Diamond, P., 247, 256-7, 259, 287-8, 352-3,
 364
 Diamond versus Lucas, 251-4
 search externalities model, 247-55
 Dickinson, H. D., 299
 Diewert, E., 344
 Dimand, R., 14, 96
 Disequilibrium. *See also* Equilibrium
 individual disequilibrium, 6, 14, 33, 36,
 56-7, 61, 64, 110, 139, 145, 217,
 220-1, 231, 363, 372, 381
 market non-clearing, 11
 Patinkin's disequilibrium interpretation of
 Keynes, 55
 Dixit, A., 309
 Dixit-Stiglitz monopolistic competition model,
 309-10, 312
 Donaldson, J. B., 289-92, 297, 304
 Donzelli, F., 53-4, 136, 181, 344, 350
 Dotsey, M., 266, 322
 Douglas, P., 57
 Drèze, J., 123-4, 129-31, 133-6, 139-41
 DSGE macroeconomics, xv, 90, 94, 141,
 151-73, 186, 191-2, 302, 331, 348,
 354, 358-77, 379-80
 Dual decision hypothesis, 118
 Duarte, P., 47, 167, 263, 319, 325
 Duck, N., 158

- Due, J., 341
 Dütte, T., 51
- Econometrics. *See* Macroeconometric models
- Economy, 54
 planning economy, 54, 263, 274, 300
 Marshallian economy, 55, 119, 301, 344-6
 Walrasian economy, 54
- Edgeworth, F. Y., 53-4, 59, 63
- Efficiency wages
 the shirking model, 229-32
- Eichenbaum, M., 262, 289, 293-5, 322-4, 332
- Epstein, R., 50
- Equilibrium. *See also* Disequilibrium
 'normal equilibrium', 10-13, 104, 182, 240-1
 center of gravity, 10, 50, 106-7, 182, 209
 common-sense understanding of, 183
 Cournot-Nash equilibrium, 257
 equilibrium as a state of rest, 10, 181-3
 equilibrium discipline, 34, 57, 139, 163-4, 184, 186, 188, 209-10, 221-2, 229, 231, 252, 260, 303, 305, 351, 365
 full equilibrium, 10-11, 46, 58, 106-7, 115
 intertemporal equilibrium, 181, 183, 186, 344, 347
 market clearing, 9-10, 17, 20, 32, 34, 106, 138-40, 152, 163, 209, 229, 231, 256, 339, 347, 351
 Marshallian general equilibrium, 8, 74, 86, 116, 135, 353
 multiple equilibria, 227, 247, 250, 252-5, 316, 365, 369
 non-Walrasian equilibrium, 64, 89, 117, 123-42, 142, 144, 188, 214, 231, 244, 248, 327, 350-1, 382, 385
 partial equilibrium, 8, 16, 57, 74, 128, 132, 157-8, 188, 217, 225, 227-8, 232, 258, 341, 346, 352, 379
 steady state equilibrium, 97, 100-1, 250-1, 254
 temporary equilibrium, 9-11, 107, 115, 129-30
 Walrasian general equilibrium, 7, 55, 86, 105, 116, 285, 354
- Erceg, C., 319, 324
 Estrella, A., 323
 Evans, G. W., 297
 Expectations, 8
 adaptive expectations, 105, 155, 239, 243, 245
 rational expectations. *See* Rational expectations
- Fagan, G., 325
 Fair, R., 301
 False prices, 53, 125, 127, 129
 Farmer, R., 358, 364, 367, 370
 Farmer's model, 365-9
 Feiwel, G., 107
 Feldstein, M., 221
 Fellner, W., 95
 Fernald, J., 295-6
 First-generation new Keynesian models, 225
 assessment, 238
 authors and main features, 225-8
 Fischer, S., 213, 225, 227. *See also* staggering contracts model
- Forder, J., 46, 107
 Friedman, B., 91, 209-10, 212
 reacting to Lucas & Sargent, 209
 velocity of money (Friedman on), 91
 Friedman, M., 4, 48, 65-111, 144, 158, 165, 182, 197, 213, 269-70, 328, 341, 347
 Friedman & Schwartz, *Monetary History of the U.S.* (1963), 68
 Friedman & Stigler on Chamberlin, 309, 328
 Expectations-augmented Phillips curve model, 102-8
 Friedman on Keynes, 73-4
 Friedman on method, 70-3
 Friedman versus Modigliani, 81-5
 Friedman versus Phelps, 108-11
 the man and his work, 65-70
 Friedman, R., 66, 90
 Frisch, R., 80, 182, 266, 267, 297
 Frydman, R., 96, 212
 Fudenberg, D., 123-9, 249
 Fuhrer, J., 323
- Gali, J., 107, 136, 139-42, 195, 296, 331
 Garrone, G., 25
 Georgescu-Rodan, N., 7
 Gertler, M., 117, 307
 Goldberger, A., 26, 35, 38-9
 Golosov, M., 329
 Goodfriend, 325
 Goodhart, C., 293-5, 315
 Gordon, D. F., 225
 Gordon, R. J., 65-6, 68, 70, 204, 206, 221, 225, 262

- reviving the Phillips curve, 213-15
 Government spending shocks
 model, 289
 Grandmont, J.-M., 25, 123, 131, 185
 Granger, C. W., 204, 206, 313
 Great Depression, 3
 Friedman & Schwarz on, 68-9
 Lucas on, 198-9
 Greenwood, J., 261
 Griliches, Z., 202, 267-8, 271
 Grossman, H., 57, 123-9, 136, 139-42
 Grunberg, E., 82
 Guesnerie, R., 364
 Guren, A., 298

 Haavelmo, T., 207
 Haberler, G., 25, 195, 331
 Hahn, F., 35, 38-9, 107, 117
 Hairault, J.-O., 310
 Hall, R., 70, 287, 293-5
 Halmeyer, V., 267
 Hammond, D., 65-6, 68
 Hands, W., 72
 Hansen, A., 35
 Hansen, G., 131, 262
 Hansen, L. P., 204, 206-7, 221, 225, 262
 Journ. Econ. Persp. symposium on
 calibration, 292
 Harrod, R., 24-5, 185, 204, 206, 313
 Hart, O., 69
 Hart's model, 198-9, 268, 271
 Hayek, F., 200, 202, 299-302
 Hayes, M., 347
 Heckman, J., 287, 297-8
 Journ. Econ. Persp. symposium on
 calibration, 292
 Heijdra, B. J., 171, 233, 237-8, 309
 Heller, W., 211
 Henderson, D., 319, 324
 Henry, J., 325
 Hermits, 275, 276, 333, 355
 Hetzel, R., 61, 65, 289-91
 Hicks, J. R., 10, 27-30, 35, 46, 54, 76, 189,
 191
 Hirsch, A., 65, 251
 Hodrick, R., 268
 Hodrick-Prescott filter, 208, 268
 Honkapohja, S., 297
 Hoover, K., 46, 48, 152, 167, 171, 263, 267,
 279, 372
 Horn, B., 21
 Household production model, 288-9

 Howitt, P., 21, 96, 116, 236, 326
 Hulton, C., 269
 Hume, D., 226
 Humphrey, T., 41
 Hurwicz, L., 51
 Hysteresis, 108

 Ideology, 72, 87-9, 169, 199, 224, 302, 305,
 376, 380, 384, 200
 presence in Friedman's work, 89-90
 presence in Lucas's work, 201
 Implicit contract models, 140, 225, 228-9,
 231
 Indivisible labor model, 285-8
 Inflation, 12, 39, 42, 45-6, 66, 74, 79, 90,
 95-6, 101, 105, 125, 155-7, 243,
 317-24
 Information, 7, 12, 112, 114, 116, 162, 212,
 216, 300, 322, 340, 350, 352
 Misperception, 105
 perfect information, 8, 12, 18, 47, 110,
 161-2, 232, 351-2
 Involuntary unemployment. *See* Unemployment
 Intertemporal elasticity of substitution, 156,
 272, 297
 Ireland, P., 319
 IS-LM model, 24, 27-49, 53, 84, 116, 147,
 239-46, 320, 349-51, 382, 384, 386,
 See Keynesian macroeconomics
 assessment, 48
 Hicks's model, 27-37, 46, 47, 49
 Marshallian or Walrasian?, 43, 349
 Modigliani's model, 27, 30-4, 36, 41

 Jaffé, W., 54, 55, 71
 Jahnsson, Y., 136, 218
 Johnson, H., 65, 68, 79, 87, 170
 Jones, C., 266-7
 Jordan, J. L., 84
 Jorgensen, D., 267-8, 271, 294

 Katz, L., 230
 Keane, M., 298
 Kehoe, P., 329-30, 334
 Kehoe, T., 303, 305
 Keynes, J. M., 3-9, 16-24
 as a Marshallian economist, 106, 114, 341,
 347, 371
 Keynes on Tinbergen, 25, 51, 72, 74, 192,
 297, 347-8
 Keynesian macroeconomics. *See* IS-LM model
 compared with DSGE program, 347-8

- Keynesian macroeconomics. (cont.)
 compared with new classical
 macroeconomics, 186-90
 emergence, 3-26
- Keynesian program. *See* Keynesian
 macroeconomics
- Keynesians against Lucas
 assessment, 222-3
 first skirmishes, 208-12
 Tobin versus Lucas on market clearing,
 218-20
 the battle over involuntary unemployment,
 220-2
- Kimball, M., 295-6, 308
- King, R., 262-3, 266, 268, 271-2, 284, 296,
 308, 322, 325
- Kirman, A., 301, 372
- Klamer, A., 151, 162, 188, 198, 202, 222
- Klein, L., 26, 34-40, 50-3, 206, 280, 315
The Keynesian Revolution (1948), 34-7
 implementing the neoclassical synthesis
 program, 50, 55
- Klein-Goldberger model (1955), 35-7
- Klenow, P. J., 329
- Kmenta, J., 221
- Kocherlakota, N., 152-3, 331
- Koford, K., 225
- Kolm, S-C., 232
- Koopmans, T., 51, 72, 167, 206, 248, 263
- Kregel, J., 17
- Krugamn, P., 358
New York Times Magazine, 373-7
- Kydland and Prescott, 93, 142, 171-2, 196,
 198, 261-81, 284, 292, *See* RBC
 modeling
 on time inconsistency, 171-2
 "Time to build and aggregate fluctuations"
 (1982), 261, 262
Journ. Econ. Persp. symposium on
 calibration, 292
- Kydland, F., 93, 151, 171, 221, 262-6, 278
- Labor market, 6, 12, 29, 42, 49, 60-1, 103,
 125, 128, 137, 147, 216, 240-1, 243,
 381
 labor rationing, 6, 34, 120, 127
 vacancies, 14, 44, 97
 wage floor, 13, 22
- Labor supply, 6, 36, 40, 119, 145, 155, 230,
 241
- Laidler, D., 24-5, 65, 67, 75, 79, 301
- Laissez faire, 4, 88, 168-9, 189, 200, 300, 374,
 376, 386
- Lange, O., 31-2, 51, 55-6, 248
- Laroque, G., 123, 138, 298
- Lavoie, D., 299
- Lawlor, M.S., 21, 347
- Layard, R., 239
- Leeper, E. M., 322
- Leeson, R., 42, 73
- Leijonhufvud, A., 4-5, 9, 25, 112-22, 197, 335,
 347, 370-2
*On Keynesian economics and the Economics
 of Keynes*, 112-14, 370
 mentor of agent-based modeling, 370-2
- Leontief, W., 31
- Lerner, A., 95
- Levels of conversation, 375
- Levin, A., 319
- Lewis, G., 57
- Lipsey, R., 43-5, 181, 204, 223
 on the Phillips curve, 42-5
- Liquidity trap, 30, 35, 76
- Litterman, R., 262
- Liviatan, N., 55
- Ljungqvist, L., 297-8
- Long, J., 151, 262
- Louçã, F., 50, 207
- Lucas, R. E. Jr., 56, 93, 139, 151, 155, 161,
 166, 174, 188, 191, 252, 329, 345, 385
 'Lucas Critique', 166-7, 204-8
 "After Keynesian macroeconomics" (1979),
 208
 "Expectations and the neutrality of money"
 (1972), 49-157
 political agenda, 199-201
 on Keynes, 162-4
 on Keynesian macroeconomics, 164-9
 on the Great Depression, 198, 303
 on Tobin, 218-20
 Lucas-Rapping supply function, 155, 213, 264
 the Keynesians-Lucas battle over involuntary
 unemployment, 220-2
 on method, 176-81
 ambiguities, 196-9, 203, 202
 on the neoclassical synthesis, 48, 56, 168,
 201, 219
 comparing Keynesian and new classical
 macro, 186-90
 on equilibrium, 183-6
- Lucasian macroeconomics. *See* DSGE
 macroeconomics

- MacLeod, B., 231
- Macroeconometric models, 25, 27, 34-41, 49, 166-7, 170, 207
 econometric testing versus calibration, 278
 Granger causality test, 204
 structural models, 48, 51
 structural VAR models, 206
 system of equations, 32, 38, 77, 86, 278, 320
 VAR approach, 205-6, 208
- Mäki, U., 70
- Maks, J. A., 344
- Malcomson, J., 231
- Malgrange, P., 34, 152
- Malinvaud, E., 51, 136-8, 171, 263
 classical unemployment, 137, 267
 Keynesian unemployment, 137, 279
 repressed inflation, 137, 372
- Mankiw, N. G., 3, 47, 116, 225, 236, 306, 326
 on RBC modeling, 284, 285
- Manuelli, R., 34, 152
- Marchiotti, R., 25
- Markets (the working of), 45-6, 54, 94, 152, 154, 158, 161, 163, 166
 speed of adjustment, 63, 170-1
 period of exchange, 9, 6, 10, 103, 155, 219-20, 313
 adjustment process, 8, 22, 54, 58, 61, 101, 113, 117-19, 183, 245
 islands parable, 96, 111
 the week device, 10
 trade technology, 13, 14, 45, 97, 98, 116, 184, 222, 248, 340, 344-6, 355, 370
 trading off the supply curve, 56, 139
- Marschak, J., 51, 55, 57
- Marshall, A., 8-9, 14, 153, 182, 341
 Marshall on equilibrium, 9-14
 lack of unemployment in, 9-14
 corn market model, 9
 fishing industry model, 9
- Marshall-Walras divide, 71-3, 143, 339-57, 380
 Friedman on the Marshall-Walras divide, 71-3
- Martin, M., 270
- Marwah, K., 37, 40
- Marx, K., 246, 375
- Matsuyama, K., 200
- Matthews, R., 12
- Mayer, T., 65, 80
- McAfee, P., 255
- McCallum, B., 91, 151, 171, 198, 262, 266, 282-4
 on RBC modeling, 283, 334
- McCracken, P., 211
- McGrattan, E., 330, 334
- McKenzie, L., 55, 193, 333, 344, 347
- Meade, J., 24
- Mehra, Y., 314
- Meiselma, D., 69, 79
- Meltzer, A., 65, 85, 86, 197
- Menu-cost and near-rationality models, 236-8
- Merz, M., 287, 288
- Mestre, R., 325
- Microfoundations, 48, 49, 97, 188, 206, 222, 248, 267, 317-19, 343-4
- Miller, B., 361
- Mirman, L., 262
- Mirowski, P., 72
- Mishkin, F., 151
- Mitchell, W., 67, 72, 167
- Modigliani, F., xiv, 27, 30-6, 51, 56, 64, 80, 117, 147, 155, 164, 170, 189, 204, 208, 212, 223-4, 232, 236, 261, 281, 349, 359
 Modigliani versus Friedman, 81-5
 IS-LM model, 30-6, 51, 84, 147
- Moggridge, D., 3, 192
- Monetarism, 65-94, 196-8, *See* Friedman, M., limitations of, 85-7
 main tenets of, 74-80
 the fall of, 90-4
- Monetary growth rule, 75, 80, 85
- Monetary Policy, 69, 102, 109, 314-19, 322-3, *See* Taylor rule
 in second-generation new Keynesian modeling, 314
 policy ineffectiveness proposition (Sargent & Wallace), 171, 232-3, 236
- Monetary History of the United States* (Friedman & Schwartz), 68
- Money, 3, 20, 28, 75-8, 83-4, 96, 157-61, 188, 314-17
 demand for, 20, 28, 30, 68-9, 74, 76, 79, 81, 85, 90, 92, 166
 supply of, 58, 69, 74-8, 85, 315
 preference for liquidity, 76, 349
 in second-generation new Keynesian modeling, 92, 291, 308, 314, 326, 355, 363, 383
 money neutrality, 95, 236-7
 money non-neutrality, 81, 103, 225, 232, 245, 291, 308, 363
 Sims on money non-neutrality, 313-14

- Money, Interest and Prices* (Patinkin 1956), 50, 55
- Monopolistic competition, 238, 307, 309-13, 319, 325, 328-9, 331-2
- Morgan, M., 50, 177, 207
- Mortensen, D., 365
- Moscarini, G., 247, 248, 249, 253
- Muellbauer, J., 123
- Mulhearn, C., 96, 109
- Muth, J., 158, 176
- National Bureau of Economic Research, 67
- Natural rate of unemployment, 40
 - Friedman's model, 102-8
 - Phelps's model, 97-102, 109
 - Phelps versus Friedman, 108-11
- Neary, J. P., 312, 331-2
- Negishi, T., 123, 135, 248, 341
- Nelson, C., 262, 268
- Neoclassical growth model, 267, 278, 280, 304,
 - See also* Solow model
- Neoclassical synthesis, 27, 46-8, 96, 141, 168, 198, 227, 285, 325, 382-3
 - Lucas's dismissal of, 165, 168
 - neoclassical synthesis program, 35, 47-8
 - neoclassical synthesis program (Klein), 50-64
 - neoclassical synthesis program (Patinkin), 50-64
- Neo-Walrasian theory, 55, 168, 176, 193-4, 202, 252, 347, 351, 353, 364
- New classical macroeconomics. *See* Lucas, R. E. Jr.
- New Keynesian. *See* First-generation new Keynesian models and second-generation Keynesian models
 - difference between first- and second generation, 308, 309
- New Keynesian/RBC synthesis, 325-7
 - cracks in the consensus, 329
- Nickel, S., 239
- Non-exploitation principle, 195, 202-3, 305, 376
- Non-Walrasian equilibrium
 - models, 89, 123-44, 327, 350-1, 382
 - notional versus effective supply and demand, 121, 125, 127
 - an aborted takeoff, 140
 - constrained quantities, 126
 - non-Walrasian (the meaning of), 138
 - regimes, 125, 128, 134, 137
- Nosal, E., 170
- Obstfeld, M., 199
- Ohanian, L., 303, 329
- Okun, A., 204, 236, 248, 257
 - Prices and quantities* (Okun 1981), 215, 217
- On Keynesian Economics and the Economics of Keynes* (Leijonhufvud 1968), 112-17
- Page, S., 358-9, 362
 - Testimony U. S. House of Representatives, 358, 364
- Panglossian vision of the economy, 194
- Pareto, 232, 251, 263, 274, 284
- Parker, R., 167-8
- Parkin, M., 79, 168, 316
- Patinkin, D., xv, 5, 21, 50-64, 82, 85, 88, 112-14, 117, 122-7, 138-9, 141, 143-4, 146-7, 173, 349-51, 382
 - disequilibrium theory of unemployment, 131
 - Money, Interest and Prices*, 50, 55-7, 59
 - on Keynes and Walras, 56-9
- Pensieroso, L., 199, 303, 329
- Permanent income, 67, 76, 81-2
- Phelps, E., xv, 65, 95-111, 212, 225, 232
 - Phelps versus Friedman, 108-11
 - Phelps's model, 97-102
 - the man and his work, 96-7
- Phillips curve, 41
 - Gordon's reconstruction, 213-15
 - Lipsey's contribution, 43-5
 - new Phillips curve, 319, 321
 - Phillips' 1958 article, 41-2
 - Samuelson & Solow on the Phillips curve, 45
- Phillips, A. W. H., 13, 27, 41-4, 95, 213
- Picard, P., 123, 129, 131, 133
- Pierce, J., 91
- Pigou effect. *See* real-balance effect
- Pigou, A. C., 7, 14-16, 58, 191, 218
- Piore, M., 176
- Plosser, C., 195, 262-3, 266, 268, 272, 275, 277, 284, 296, 374
- Poole, W., 91, 209
- Portes, R., 123, 128, 140
- Portier, F., 310
- Prescott, E., xv, 142, 158, 171-2, 174, 181, 196-9, 239, 253, 260-81, 283, 287, 291, 303-5, 386
 - basic methodological standpoint, 280
 - contrasting Keynesian and RBC models, 280

- “Theory ahead of business cycle measurement” (1986), 266, 280
- Qin, D., 50, 207
- Quantity theory of money, 55, 75, 82, 85, 197
Friedman’s rehabilitation of, 75–7
- Ramsey, F., 263
- Ramsey, J. B., 221
- Rapping, L., 155–6, 198, 210
- Rational expectations, 96, 152, 154, 158–61,
204, 213, 232, 305
gradual acceptance of, 212
implications of, 171–2
rational expectations revolution, 151,
169–72, 206, 379
- RBC modeling, xvi, 94, 261–306. *See* Kydland
and Prescott model
early criticisms, 282–5
further developments, 282–98
assessment, 299–306
baseline model, 261, 272, 275–6, 285, 319,
355
the story behind the model, 274
limitations of, 302–6
methodological breakthrough, 282–3,
296–8
questioning the causal role of technology
shocks, 293–6
Walrasian character of, 353
- “Real wages, employment and inflation”
(1969), 155–7
- real-balance effect, 58, 88
- Rebeño, S., 263, 266, 268, 271–2, 294, 296,
308
- Rees, A., 198
- Ricardo, D., 73, 165, 176, 374
- Roberts, J., 247, 256–7
coordination failures model, 256–7
- Robinson Crusoe, 275, 277, 283, 354
- Rogerson, R., 107, 285, 288–9, 298
- Rogoff, K., 199, 299
- Romer, C., 322
- Romer, D., 225, 227, 236, 266, 275, 322
- Rotemberg, J., 285
- Rowthorn, R., 239
- Rubin, G., xix, 32, 34, 56–7, 61, 88, 119, 122,
349
- Salop, S., 225
- Samuelson & Solow
on the Phillips curve, 45
- Samuelson, P. A., 25, 34, 45–6, 54, 55, 152
- Sargent, T., 94, 152, 154, 158, 161, 163, 166,
170–1, 176, 187, 188, 224, 254, 298
- Sawyer, M., 239
- Schultz, T., 271
- Schwartz, A., 67–9, 74, 77–8, 84, *See* Friedman
and Monetary History of the U.S.
- Scientific revolution, 88, 151, 174, 280, 379
- Search externalities model, 118, 247
Diamond, 247–56
Howitt, 255–6
- Search, 14, 118, 215–17, 222, 247–56, 353,
365–7
Diamond’s search eternity model, 247–56
Howitt on, 255–6
Phelps’s search model, 97–102
- Second-generation new Keynesian modeling, 92,
153, 206, 208, 239, 291, 296, 307–35,
354–6, 363, 383
- Smets-Wouters model, 325, 327, 330, 334
multiple distortions, 323–5
assessment, 331–5
baseline model, 319–22
cracks in the consensus, 329–31
money and monetary policy in, 314–17
monetary policy shocks in, 322–3
new Phillips curve, 319, 321
Walrasian or Marshallian?, 354
- Self-fulfilling prophecies, 254, 358, 364–70
- Sent, E.-M., 154, 176, 206
- Serletis, A., 75
- Shackle, G. L. S., 8
- Shapiro, C., 225, 229–32, 289
- Shell, K., 193, 275, 364
- Shiller, R., 212
- Shirking model, 229–32, 289–91
- Shocks, 58, 61, 157, 160, 205, 208, 214,
233, 251, 272, 282, 289, 293–6, 322,
387
governmental shocks, 295
monetary policy shocks, 314, 322–4
multiple distortions, 323–5
technology shocks, 93, 272, 277, 283, 289,
293, 295–6, 332
- Silvestre, J., 134
- Simon, H., 176
- Sims, C., 167, 201, 206–8, 222, 262, 292–3,
313–14, 322
Journ. Econ. Persp. symposium on
calibration, 292
- Sims on Keynesian econometric
models, 206
- Sims on money non-neutrality, 313–14
- Sims on the Lucas Critique, 204–8

- Singleton, K. J., 262
- Skidelsky, R., 3-4, 301, 387
- Sleeman, A. G., 42
- Smets, F., 324-5, 331, 382
- Smets-Wouters model, 325, 327, 330
- Smith, A., 6, 53, 181, 282, 300, 341, 374-5
- Snowdon, B., 4, 47, 65-6, 74, 89, 151, 154, 157, 174, 185, 193, 208, 227, 315, 328
- Socialist calculation debate, 299
- Solow model, 266-9
 early reception, 269-72
 Solow residual, 268-9, 271-2, 274
- Solow, R., 45, 47, 137, 194, 195, 209, 220-2, 266-8, 271, 276-7, 326, 360-2
 Solow versus Chari, 361-4
 Testimony U.S. House of Representatives, 358-64
- Soskice, D., 226, 239-46
- Spill-over effect, 120, 125, 132
- Spulber, D., 235
- Stability, 41, 63, 80-3, 90, 121, 319
 monetarism on stability, 80-1
- Staggering contracts model, 232-6, 266, 382
- Steinbeck, J., 222
- Stigler, G., 65, 153, 309, 328, 346
- Stiglitz, J., 225, 229-30, 376-7
- Stock, J., 205
- Summers, L., 108, 282-5, 293, 303, 361
 on RBC modeling, 282-3
- Tâtonnement, 52-4, 58-9, 114, 128-9, 131, 352
- Taylor rule, 316-17, 319, 321
- Taylor, J., 66, 89, 94, 111, 201, 225, 232, 235, 262, 314-17
- Temin, P., 80, 305
- Tesfatsion, L., 372
- The General Theory of Employment, Interest, and Money* (Keynes 1936), 3
 animal spirits, 7-8, 108, 364-5
 assessment, 23-4
 effective demand model, 5, 16-22, 365-6
 Keynes's program, 8, 17, 23, 34
- Thygesen, N., 65, 79, 107
- Till, G., 301
- Time, 3-8, 10, 12, 13, 14, 16, 18, 23, 29, 33, 37, 39, 41, 52, 54, 63, 66-8, 72, 73, 78, 103, 106, 115, 143, 145, 154, 156, 163, 170, 171, 176, 178, 182-5, 191-2, 208, 213, 227, 273, 305, 315, 323, 331, 369, 385
 logical time, 12, 54, 63
 points in time, 184
 short-/long-period distinction, 13
 time inconsistency, 171-2
- Tinbergen, J., 25, 51, 72, 74, 192, 297, 347-8
- Tobin, J., 31, 80, 146, 200, 204, 212-13, 217-19, 221, 223, 364
 Accumulation and Economic Activity (Tobin 1980), 218
 Tobin versus Lucas on market clearing, 218-20
- Total factor productivity, 267-71, 276, 294, 296
- Touffut, J.-P., 383
- Trabandt, M., 382
- Trevithick, J., 21
- Triffin, R., 71
- U.S. House of Representatives, 358, 364, 387
 Hearings on DSGE, 358-64
- Uhlig, H., 327, 335
- Underemployment, 33-4, 56, 64, 82, 102, 147, 236, 245, 381, 385
 difference from unemployment, 333
 underemployment in Modigliani's model, 30-4, 236
- Unemployment. *See* natural rate of unemployment
 equilibrium rate of unemployment, 96, 240, 245
 frictional unemployment, 6, 9, 14-16, 40, 44-5, 102, 108, 110, 145, 216-17
 involuntary unemployment, 6-7, 16-23, 32-3, 36, 44, 58, 64, 110, 113, 121, 136, 139-40, 144-5, 220-2, 229, 231, 244-6
 in the casual sense, 110, 145, 217, 381
 in the individual disequilibrium sense, 110, 145, 217
 the battle over, 220-2
 search unemployment, 14, 111, 287
- Usabiaga Ibanez, 151
- Validation, 261-81, 300
 ex ante validation, 300
 ex post validation, 301
- van der Ploeg, F., 171, 233, 237-8
- van Wittenloostuijn, A., 344
- Vane, H., 4, 47, 65-6, 74, 89, 96, 109, 151, 154, 157, 174, 185, 193, 198, 208, 227, 315, 328
- Varian, H., 123, 135
- Velocity of money, 75-6, 90-1

- Vercelli, 176, 349
 Viner, J., 181
 Visco, L., 34
 von Mises, L., 299
- Wage, xiv, 5-6, 9, 13, 37, 52, 64, 98, 100, 125,
 155, 216, 228-9, 239, 257, 290, 324,
 330, 352, 370, 385
 money wage, 21, 24, 32, 35, 62, 98, 101,
 103, 240
 real wage, 4, 30, 36, 39, 56, 59, 103, 119,
 128, 137, 155-7, 233, 235, 240, 244,
 255, 265, 273-4, 287, 323-4, 366-7
 wage rigidity, xv, 6, 21-2, 24-5, 29, 31-2,
 119, 146-7, 185, 187, 225, 232, 238,
 239, 248, 312, 365-6, 370, 384
- Walentine, K., 382
 Walker, D., 7, 54, 135
 Wallace, N., 151, 170-2, 213, 232, 235-6, 315
 Walras, L., 7-8, 50, 53, 63, 72, 105, 141, 153,
 168, 176-7, 188, 195, 218, 275-6, 340,
 349
 Walrasian theory, 47, 50, 52, 54, 55, 57-8, 61,
 64, 72, 77, 114, 116, 135, 141, 146, 194,
 199, 202, 218, 222-3, 299, 306, 325,
 345, 347, 349, 351, 353-4, 364, 383
 Walras on tâtonnement, 53, 55
- Walras's Law, 21, 118-19, 120, 122, 125,
 129
 Walrasian auctioneer. *See* Auctioneer
- Watson, B., 299
 Watson, M., 205
 Wealth effects, 63
 Weintraub, E. R., 51, 123, 185, 194
 Weiss, L., 225
 Whitaker, J., 339
 Williamson, S., 232, 268-9
 Winter, S., 359, 361-3
 Testimony House of Representatives, 358-64
 Woodford, M., 47, 264, 276, 315-16, 324,
 326-7, 364
Interest and Prices (2003), 316
 Wouters, R., 324, 330-1, 382
 Wren-Lewis, S., 288, 313, 327
 Wright, R., 193, 249, 252-3, 285, 288
- Yeager, L., 57, 115
 Yellen, J., 225, 229, 236-7
 Younes, Y., 123
 Young, W., 24, 48, 210, 263-4
 Yun, T., 313
- Zeuthen, F., 244
 Zilberfarb, B. Z., 349