1. Introduction

1.1 THE NATURE OF THE PROBLEM

Take any advanced text in macroeconomics, written from a mainstream perspective. Wickens (2008) is a good example: it is an authoritative graduate text published by an Ivy League university press (Princeton). Here are the opening words of Chapter 1:

Modern macroeconomics seeks to explain the aggregate economy using theories based on strong microeconomic foundations. This is in contrast to the traditional Keynesian approach to macroeconomics, which is based on ad hoc theorizing about the relations between macroeconomic aggregates. In modern macroeconomics the economy is portrayed as a dynamic general equilibrium (DGE) system that reflects the collective decisions of rational individuals over a range of variables that relate to both the present and the future. These individual decisions are then coordinated through markets to produce the macroeconomy. (Wickens 2008, p. 1)

Wickens proceeds by setting out a formal model of the behaviour of a RARE individual (a representative agent with rational expectations), a model that is supposedly derived from the work of Frank Ramsey (1928), to provide the purported microfoundations of the macroeconomic analysis that follows. (Lance Taylor (2004) denotes them as ‘MIRA microfoundations’: methodological individualism with representative agents.) I say 'supposedly derived', because Ramsey himself was quite clear that he was modelling the decisions of a socialist government and made no claim that his analysis might be applied to individual economic agents under capitalism. The reference that Wickens makes to ‘the collective decisions of rational individuals’ unwittingly points to the difficulty: it is, of course, individual and not collective decisions that are, supposedly, being aggregated. If they really were collective decisions, the aggregation would be unnecessary.

An initial and utterly insuperable problem with RARE microfoundations, as many critics have noted, is the assumption of a single representative agent, which implies that all individuals in the economy that is being modelled are identical. There is thus no reason for them to trade with each
other, no reason why their decisions should be coordinated, and therefore no role for markets (Kirman 1992; see also Acemoglu 2009, pp. 149–55). Wickens soon acknowledges the problem:

The basic Ramsey model can be roughly interpreted as that of a closed economy without a market structure in which the decisions are coordinated by a central planner. A first step towards greater realism is to allow decisions to be decentralized. This requires us to add markets – which act to coordinate decisions, and thereby enable us to abandon the device of the central planner. (Wickens, 2008, p. 2)

This is not a ‘rough’ interpretation of the Ramsey model, but a precise statement of its domain (though his central planner makes decisions rather than ‘coordinating’ them). Whether the ‘greater realism’ that Wickens claims to provide has in fact been attained is very doubtful. ‘Firms act as the agents of households’ in his DGE model of ‘the decentralized economy’, not as their employers (ibid., p. 54). Neither employment nor unemployment plays any significant role. Since ‘including labor caused only minor changes to the previous results … we shall also exclude labor where appropriate and feasible’ (ibid., p. 83). This proves to be almost always; there is no entry in the index for ‘unemployment’. Such is ‘modern macroeconomics’, ‘based on strong microeconomic foundations’ that effectively eliminate the defining characteristic of the capitalist mode of production, the capital–wage labour (or employment) relationship.

These are important questions, for several reasons. Most obviously, the scientific status and (especially) the autonomy of macroeconomics are at stake. If the microfoundations dogma is correct, and the macroeconomy is ‘produced’ à la Wickens simply by aggregating the decisions of RARE individuals, then macroeconomics becomes nothing more than an application of microeconomic theory. There is no longer a case for teaching it as part of the core of graduate education in economics; it should instead be offered as an option, like industrial organization or environmental economics. This conclusion has been embraced with enthusiasm by the neoclassical labour economist Daniel Hamermesh (2008), and – in an amazing volte-face from his earlier advocacy of an autonomous ‘Post-Walrasian macroeconomics’ – by David Colander (2007), though in his case rather more tentatively. As we shall see in Chapter 6, this is the culmination of a lengthy process. A quarter of a century ago, Robert Lucas (1987) was both foreshadowing and welcoming the disappearance of macroeconomics as a separate intellectual discipline.
Even if things do not go quite this far, it is clear that the survival of any form of Keynesian (not just Post Keynesian) macroeconomics is now in question. Wickens’s claim that DGE theory ‘incorporates both Keynesian and New Keynesian macroeconomics’ is contentious, since on the very next page he praises the work of the avowedly anti-Keynesian Robert Barro and reports that he himself ‘had strongly disliked the Keynesian approach to macroeconomics, even from my undergraduate days at the London School of Economics’ (Wickens 2008, pp. xiii, xiv). The ‘New Keynesian’ theory that is supposedly incorporated in DGE models is in fact a travesty of Keynes, eliminating the principle of effective demand and with it any notion of involuntary unemployment as a consequence of effective demand failures (Dullien 2011). Keynes’s own reaction, had he been confronted with a graduate text in macroeconomics in which the index contained no entry for ‘unemployment’, can easily be imagined.

Finally, and most important, there is the issue of academic economists’ reactions to the global financial crisis that began in 2008. The microfoundations dogma has played an important role in the ‘business as usual’ strategy of the mainstream of the profession, in terms of both theory and policy. This is depressingly evident in the widespread popular support for ‘fiscal consolidation’ in many European countries, where public services are being slashed in the name of ‘debt reduction’, reducing effective demand and further increasing unemployment – and government debt. This is impossible to explain in terms of self-interest: only the very rich make no use of public libraries, schools, hospitals and swimming pools, and even they have found their property incomes threatened by the contractionary multiplier effects of government spending cuts. There seems instead to be a deep-rooted belief that the financial problems faced by governments are qualitatively identical to those confronting individual agents and must therefore have the same solution, albeit on a very much larger scale. When an individual or a household gets increasingly into debt, reducing expenditure is often a sensible course of action (not always, of course: taking out a mortgage to buy a family home is in normal circumstances a prudent decision). But the principles of ‘sound finance’ cannot be applied to governments without committing a serious fallacy of composition, as Abba Lerner (1943) demonstrated many years ago. The microfoundations dogma makes for bad politics, in addition to bad economics.

This book is part history of economic thought and part economic methodology. In it I trace the protracted and tortuous history of the microfoundations dogma since the earliest days of macroeconomics as a separate branch of economic theory in the early 1930s, having first scoured the
literature of the philosophy of science in an attempt to make sense of it all. I am conscious of trespassing here: I am an historian of ideas by trade, and have no training and very little experience in the philosophy of science. But I believe that the issues raised by the microfoundations dogma are so important that they have to be confronted by anyone with an interest in the future of macroeconomics. This book is an initial salvo in this confrontation.

I should make my own position very clear. As you will already have concluded, I am not a neutral observer. I am a heterodox economist, and on questions of macroeconomics my sympathies are with Post Keynesian theory, especially in the form of the Keynes–Marx synthesis sketched out by Michał Kalecki (1899–1970) and further developed by later writers in the Kaleckian tradition. Since the early 1990s I have become increasingly irritated by the microfoundations dogma, in particular by the regular apologies that authors, conference participants and seminar presenters seem obliged to offer for writing perfectly sensible papers on macroeconomics without having provided any microfoundations. I firmly believe that the critics of the microfoundations dogma are correct, for all that they are in a small minority in today’s economics profession. I object not just to RARE (or MIRA) microfoundations but also to the claim that superior, non-RARE microfoundations have been, must be or should be provided for heterodox macroeconomic theory. I strongly advocate the relative autonomy of macroeconomics as a separate ‘special science’, and agree with Robert Skidelsky (2009) that it should be taught by specialist macroeconomists and not by practitioners of mainstream microeconomics. I only wish that all Post Keynesians, and indeed all heterodox economists of other persuasions, agreed with me.

1.2 THE STRUCTURE OF THE BOOK

The next three chapters, which constitute Part I, focus on methodological and philosophical questions. Chapter 2 discusses the use of metaphors and analogies in scientific discourse, with particular reference to economics. I argue that ‘microfoundations’ is a bad metaphor and an unacceptable analogy. In Chapter 3 I consider some of the issues raised by another questionable metaphor, ‘micro-reduction’, drawing on the substantial literature on this question in the philosophy of science before criticizing its application to economics. I next consider (and reject) the suggestion that it is necessary to provide ‘macrofoundations’ for microeconomics. Then, in
Chapter 4, I consider two case studies: the continuing debate on micro-reduction in the life sciences, and the protracted controversy that took place in the 1950s and 1960s on the principle of methodological individualism in social science. The chapter concludes with a discussion of the growing interest in the study of emergent properties, which have very important and largely negative implications for attempts at micro-reduction. There are important lessons here, I will suggest, for economics.

Part II is devoted to ‘microfoundations’ in the history of economics and the other social sciences. Chapters 5 and 6 trace the very convoluted history of the word and the doctrine since 1936. Note that the two are separable: you can assert the dogma without using the word, as J.R. Hicks (1939) did in *Value and Capital*, and indeed all its adherents did before 1956 (when the Post Keynesian Sidney Weintraub became the first to use it in print), and often for some years thereafter. Conversely, it was possible to use the word while denying the dogma, as Weintraub himself did and many other Post Keynesians have done since. I have chosen to end Chapter 5 in 1975, when the International Economic Association held an important conference on the issue, a conference whose proceedings revealed continuing doubt and confusion on the whole issue (Harcourt 1977). Only with the rediscovery of the Ramsey model at the end of the decade was the microfoundations dogma really able to take off. Chapter 6 takes the economics story from 1975 down to the present day, while Chapter 7 shows how the word and (to a very much lesser extent) also the dogma of microfoundations have been taken up in the other social sciences: sociology, political science, history and – straddling all three – Analytical Marxism. As will be seen, the fate of microfoundations offers an instructive case study in the failure of ‘economics imperialism’.

It will be evident from Chapters 5–6 that not all practitioners of economics have accepted the microfoundations dogma. Part III is devoted to these dissenters. The story is, once again, a complex one. Among heterodox economists, many Post Keynesians have been strong critics of the dogma, but a surprisingly large proportion have been unclear, inconsistent or just plain confused on the issue. Chapter 8 is dedicated to the Post Keynesians, while Chapter 9 deals with other dissidents. Within mainstream economics, some but not all Old Keynesians have been consistently critical of microfoundations, while others have wavered. Austrians, who might have been expected to be firm supporters of the dogma, have been no less divided, along with institutionalists and evolutionary economists. I conclude the chapter by speculating on the reasons for these uncertainties. In Chapter 10 I consider another subset of economists, the economic methodologists.
How have economists with a knowledge of and interest in the philosophy of science dealt with the microfoundations dogma? Not quite as well as one might have hoped, I will suggest.

Finally, in Chapter 11, I try to draw together the various strands of the argument. Why did the microfoundations dogma become so popular, and why did it only take off in the late 1970s and early 1980s, and not (many) decades earlier? Why does the dogma matter? What can we learn from it about the use and abuse of metaphors in economics? What is the future of macroeconomic theory, with or without microfoundations? What are the broader lessons that a new generation of macroeconomists need to learn?

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