1. Essentials of heterodox and post-Keynesian economics*

1.1 THE NEED FOR AN ALTERNATIVE

1.1.1 The Global Financial Crisis

The Global Financial Crisis has been a wake-up call for economists. The alarm should have rung much earlier, when Japan and then East Asia were struck by a huge financial crisis in the 1990s, but few economists in the Western world paid much attention to the difficulties of these far-flung countries. The Global Financial Crisis is sometimes said to have begun in the summer of 2006, when real-estate prices in the USA stopped rising and started to fall, but few of us thought that this local phenomenon would induce a world crisis. Surprisingly, and demonstrating the importance of globalization, the first signs of financial tension arose on the European interbank markets at the beginning of 2007, when European banks started to express anxiety over the value of their financial investments in the USA. A mini-crisis occurred during the summer of 2007, and despite the difficulties encountered by issuers of asset-backed commercial paper, most of us believed that central banks had played their role and had relaxed the tensions.

This illusion persisted until September 2008, when the government-sponsored agencies Freddie Mac and Fanny Mae had to be rescued, when Wall Street banks tumbled one after the other, when two large banks – Washington Mutual and Wachovia – had to be acquired, and when the giant insurer AIG had to be bailed out by government, as was then a string of large European banks, including the whole Icelandic and Irish banking systems. The culmination of all this was that the US government decided to let go the Wall Street bank Lehman Brothers, sending a chilling message all over the banking world. Then, with the usual sources of finance cut off, as corporate paper markets started to collapse, and as banks became reluctant to grant lines of credit to new or returning customers, the real sector got into trouble, and even General Motors needed to be rescued by the American and the Canadian governments. The economic recession, caused by the imprudence of bankers and the incompetence or fraudulent behaviour of the rating agencies, led to large government deficits as tax revenues fell and some countries tried to counteract the slowdown with stimulus programmes, which achieved some success.

But this was not the end of such troubles. In late December 2009, it was noted that a small country of the eurozone, Greece, had particularly bad economic indicators and had hidden from official statistics some of its debt, thus creating worries about its capacity to redeem it. Investors realized that the eurozone had a peculiar setup, designed for a world in which financial crises could not occur, as the European Central Bank, in contrast to most other central banks, did not normally purchase government bonds. This exacerbated the worries of investors about the capacity of (some) eurozone countries to redeem
their debt. Worries over Greece spread to other countries – Ireland, Portugal, Spain and Italy – as the European Central Bank declined to intervene and purchase sovereign bonds except when it was too late, thus causing a sovereign debt crisis. With the possible feedback effects of sovereign defaults on the banks detaining sovereign debt, and with all European countries pursuing fiscal austerity policies, at the time of writing (2013) it is hard to see where and when all this globalized turmoil will end; some economists forecast a ‘perfect storm’.

Roughly speaking, economists have had three reactions to the financial crisis. The middle reaction has been to say that existing mainstream theory is fine, but that it needs to be slightly tweaked and improved so as to take into account elements that were previously left aside and which explain why the crisis could not be predicted. The second reaction, associated with neo-Austrian and new classical authors, or those that Paul Krugman has called fresh-water economists, is to argue that the crisis was caused by misguided regulations, bad government interventions, ill-advised decisions by central banks, unsound government budgets, and by the naughty Chinese who had rigged their exchange rate. Finally, the third reaction is to claim that recent institutions, regulations and economic policies have been based on erroneous economic theories, and that these need to be scratched out. Of course, this last opinion has always been the belief of heterodox authors, and post-Keynesian authors in particular, but with the advent of the financial crisis, several former partisans of mainstream economics have changed their mind and been quite critical of standard theory.

1.1.2 Recantations

Perhaps the most surprising such recantation is that of Richard Posner, a judge and a senior lecturer at the University of Chicago School of Law. Posner was a stern defender of free markets and Milton Friedman’s ideology. In his book, titled The Failure of Capitalism, Posner (2009a) argues that deregulation went too far and that financial markets need to be heavily regulated, because banking has a systemic significance that other industries do not have. In a follow-up article, provocatively titled ‘How I became a Keynesian’, Posner (2009b) goes further, arguing that ‘we have learned since September [2008] that the present generation of economists has not figured out how the economy works’. Posner believes that Keynes’s General Theory, despite its apparent antiquity, is the best guide to the crisis, because ‘Keynes wanted to be realistic about decision-making rather than explore how far an economist could get by assuming that people really do base their decisions on some approximation to cost–benefit analysis’. A very similar point, showing disarray at the obviously counterfactual assumptions about human behaviour entertained by mainstream economists, was also made by Akerlof and Shiller (2009, p.268) when they wrote that ‘in their attempt to clean up macroeconomics and make it more scientific, the standard macroeconomists have imposed a research structure and discipline by focusing on how the economy would behave if people had only economic motives and they were also fully rational’. Robert Skidelsky (2009, p.x), the historian biographer of Keynes, claimed that to understand economics it was better not to be a professional economist, the advantage being ‘of not having been brainwashed to see the world as most economists view it: I have always regarded their assumptions about human behaviour as absurdly narrow’.
Former winners of the Bank of Sweden prize in economic sciences in memory of Alfred Nobel (to which we will simply refer as the Nobel Prize in economics from now on), such as Paul Krugman and Joseph Stiglitz, have been unrelenting critics of mainstream economics, as reflected in the works of most of their peers, criticizing both their assumptions and their apparent lack of knowledge of elementary Keynesian economics, as the most famous new classical authors reverted to pre-Keynesian arguments to counter the justification of stimulus programmes. The most abrasive indictment of mainstream economics is probably that of Willem Buiter, an LSE professor and a former member of the Monetary Policy Committee of the Bank of England. In the following long quote, Buiter questions the usefulness of all the fads in macroeconomics over the last 30 years. Indeed, he would thus like us to go back to Old Keynesian authors such as Tobin, or post-Keynesian authors such as Minsky, or else to authors who have demonstrated originality, such as Shiller, Akerlof and Stiglitz, whose works show concerns that are close to those found in the works of post-Keynesian authors.

Indeed, the typical graduate macroeconomics and monetary economics training received at Anglo-American universities during the past 30 years or so may have set back by decades serious investigations of aggregate economic behaviour and economic policy-relevant understanding. It was a privately and socially costly waste of time and other resources. Most mainstream macroeconomic theoretical innovations since the 1970s (the New Classical rational expectations revolution associated with such names as Robert E. Lucas Jr., Edward Prescott, Thomas Sargent, Robert Barro etc, and the New Keynesian theorizing of Michael Woodford and many others) have turned out to be self-referential, inward-looking distractions at best. Research tended to be motivated by the internal logic, intellectual sunk capital and esthetic puzzles of established research programmes rather than by a powerful desire to understand how the economy works – let alone how the economy works during times of stress and financial instability . . .

In both the New Classical and New Keynesian approaches to monetary theory (and to aggregate macroeconomics in general), the strongest version of the efficient markets hypothesis (EMH) was maintained. This is the hypothesis that asset prices aggregate and fully reflect all relevant fundamental information, and thus provide the proper signals for resource allocation. Even during the seventies, eighties, nineties and before 2007, the manifest failure of the EMH in many key asset markets was obvious to virtually all those whose cognitive abilities had not been warped by a modern Anglo-American Ph.D. education. But most of the profession continued to swallow the EMH hook, line and sinker, although there were influential advocates of reason throughout, including James Tobin, Robert Shiller, George Akerlof, Hyman Minsky, Joseph Stiglitz and behaviourist approaches to finance. (Buiter, 2009)

There is indeed a great deal of dissatisfaction with economic theory and economists, at all levels. The administrators at the IMF have set up an enquiry to find out why IMF advice has led to such disastrous results in so many countries, discovering that the fault lay in the theories defended by their economists. Government and central bank officials are ever more wary of the advice proposed by their professional economists. Managers of large investment funds, burned by the financial crisis, search for alternative views on the economy. Students, in particular those in France who launched the post-autistic economics movement as a protest in 2000 and followed up with the PEPS-économie (2013) movement, have long been complaining that they are being brainwashed by their economics professors, who put forward a single view without telling the students that there exist other theories. Students further complain that all the emphasis is on techniques and formalization, with little link with actual economic events (Fullbrook, 2003). Even the
Queen of England complained in November 2008 that nobody had apparently been able to forecast the Global Financial Crisis (Earl, 2010). It took more than half a year for British orthodox economists to send a reply to the Queen, arguing that all this was caused by a lack of imagination on their part.

1.1.3 The Necessity of a Post-Keynesian Alternative

The argument put forward here is that, while prediction in economics has always been difficult, the danger of following bad advice has been greatly increased by the hegemony of neoclassical economics, that is, the fact that departments of economics throughout the world have been monopolized by this single broad view. This is in contrast to what occurs in other university departments, such as sociology or psychology, where directly opposite views are given pride of place in first-year textbooks. Dissent, or at least dissent of a certain kind, has been repressed in economics departments. But dissent is what is needed for a vibrant academic environment. Dissent, however, must go beyond criticism: a positive alternative must also be put forward. This is the main purpose of the book.

The crisis has clearly demonstrated, if such a demonstration were needed after the failure of the Washington Consensus just a few years earlier, that there is something drastically wrong with the dominant theory that has provided such bad advice to the decision-makers. As could be found on the website of the rather conservative Financial Times in 2009, ‘the credit crunch has destroyed faith in the free market ideology’. In view of these failures, it is our social duty as economists, a duty that should have a high social rate of return, to develop an alternative outlook of the economic system. It is our duty to sustain and develop the heterodox traditions that question the efficiency and stability of unfettered markets.

In this book, I wish to highlight the ‘post-Keynesian’ tradition in economics. We shall see later that this school of thought can be subdivided into several strands. But for now we can say as a first approximation that this tradition extends and generalizes the seminal ideas that were developed by the radical followers of John Maynard Keynes (hence the term ‘post-Keynesian’). These developments initially occurred mainly at the University of Cambridge, where Keynes was located. The originality of these ideas became pretty obvious in the 1950s, as researchers such as Nicholas Kaldor and Joan Robinson came to prominence. Of course, there were also other famous heterodox economists in Cambridge, most notably Richard Kahn, Pierro Sraffa and Maurice Dobb. This generation was then followed by another one, that of Luigi Pasinetti, Geoffrey Harcourt and Wynne Godley, who came with ideas of their own, albeit compatible with this radical Cambridge tradition. Outsiders also made contributions to this tradition, the most notable certainly being Michal Kalecki, the Polish economist. Starting with the early 1970s, several American economists contributed in their own way to this tradition and helped to institutionalize post-Keynesian economics. Naturally, the contributors to post-Keynesian economics can now be found throughout the globe and in certain cases can be associated with other schools of thought, as is the case for John Kenneth Galbraith, who is usually perceived as some kind of radical Institutionalist.
1.2 HETERODOX ECONOMICS

1.2.1 Heterodox versus Orthodox Economics

At this stage some definitions are required. Table 1.1 shows the alternative names that have been given to the two wide traditions that exist in economics. We chose to call these heterodox economics and orthodox economics; an economist who is not part of the heterodox group then by definition must belong to the orthodoxy. We shall see in the next section that these two traditions can be defined by key methodological characteristics and beliefs. Orthodox economics is often referred to as neoclassical economics, marginalism, the dominant paradigm or mainstream economics. Over the last decade or so, various authors such as David Colander (2000) and John Davis (2006) have contended that all these terms are not synonyms. In particular, these authors have been arguing that important works in the orthodox tradition do not use some of the key assumptions that define neoclassical economics and the use of marginalist methods, making references to game theory, experimental economics, behavioural economics, neuroeconomics and non-linear complexity economics. While this may true, particularly in the field of microeconomics, despite obvious elements of continuity with the neoclassical framework, it is clear that macroeconomics, with its current use of the representative agent with rational expectations (RARE, as John King (2012a) calls it), is still fully within the neoclassical berth. Hence, until contrary evidence is truly convincing, I see nothing wrong in assimilating orthodox economics to the neoclassical paradigm.

In the 1992 version of the book, I made references to the ‘post-classical’ paradigm, in opposition to the neoclassical paradigm, and also because some of the concerns of the post-classical economists reflected the concerns of classical economists such as Ricardo and Marx. In the same spirit, Heinrich Bortis (1997) has suggested the name ‘classical-Keynesian’ political economy. I also occasionally used the term ‘unorthodox’ or ‘non-orthodox’ economics. Malcolm Sawyer (1989) proposed the term ‘radical political economy’ to identify a more or less homogeneous set of dissident schools, so as to distinguish it from the stand-alone ‘political economy’ that has also been used by right-wing authors concerned with public choice and the growth of the public sector. Edward Fullbrook (2013) has suggested the use of two expressions, ‘new paradigm economics’ and ‘old paradigm economics’, proposing ten distinguishing characteristics. In his efforts to regroup all those frustrated with orthodox economy, and as a follow-up to the post-autistic economics movement, Fullbrook has created the Real-World Economics Review,

<table>
<thead>
<tr>
<th>Heterodox economics</th>
<th>Orthodox economics</th>
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<tr>
<td>Post-classical paradigm</td>
<td>Neoclassical economics</td>
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<tr>
<td>Radical political economy</td>
<td>The dominant paradigm</td>
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<td>Non-orthodox economics or unorthodox economics</td>
<td>Mainstream economics</td>
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<td>Real-world economics</td>
<td>Marginalism</td>
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<td>New paradigm economics</td>
<td>Old paradigm economics</td>
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Table 1.1 Heterodox versus orthodox economics: alternative names
the main publication of the World Economics Association. The converse of orthodox economics could thus also be called real-world economics.

I decided to adopt the denomination ‘heterodox economics’. Over the years, in particular since the late 1990s, but even more so since the mid-2000s, the term ‘heterodox’ has become increasingly popular to designate the set of economists who view themselves as belonging to a community of economists distinct from the dominant paradigm. Indeed, there is now a huge Heterodox Economics Directory (Jo, 2013), which provides useful information to all those young scholars looking for an alternative economics. As a result, I shall speak of ‘heterodox economists’, as has been suggested in particular by Frederic Lee (2009).

Is it possible to summarize in a nutshell the difference between orthodox economics and heterodox economics? While much more will be said in the next section, at this stage we can focus on the definition of economics as an exemplar. The most accepted definition, which can be found in all orthodox textbooks, is that of Lionel Robbins (1932, p. 16), who defined economics as ‘a science which studies human behaviour as a relationship between ends and scarce means which have alternative uses’, summing this up by saying that economics is the study of ‘behaviour conditioned by scarcity’ (p. xxxi). When asked, some of my students defined neoclassical economics as the study of an upward-sloping supply curve with a downward-sloping demand curve! Lee (2013a, p. 108) by contrast defines heterodox economics as ‘a historical science of the social provisioning process’. I find this rather ambiguous, and personally I prefer the definition offered by John Weeks (2012), who objects to the standard definition of economics based on scarcity, proposing instead that ‘economics is the study of the process by which society brings its available resources into production, and the distribution of that production among its members’.

1.2.2 Heterodox Schools of Thought

Who are these heterodox economists? Frederic Lee (2009, p. 7), in his History of Heterodox Economics, lists the following: ‘Post Keynesian-Sraffian, Marxian-radical, institutionalist-evolutionary, social, feminist, Austrian and ecological economics’. Table 1.2 gives a similar list of the various schools of thought that I have associated with heterodox economics in the past. Post-Keynesians are listed first, not because of their numerical importance, but rather because they are the subject of this book, although it should be pointed out that Radicals/Marxians are probably most numerous among heterodox economists, with the Institutionalists next. As we shall see later, these various schools of thought have methodological features in common, although this may not always be obvious because the members of each school usually specialize in different fields or because they provide different sorts of critiques against orthodox economics, so that the contacts between the various schools can be rather sparse.

The financial crisis has given a boost to all alternative schools of thought, in particular heterodox Keynesian economics. Students in some economics departments devoid of heterodox courses have created their own sets of lectures. Journalists, who up to very recently, were still in awe of Milton Friedman, now turn to Keynes to provide some explanations of what is happening. The books of John Kenneth Galbraith, a post-Keynesian Institutionalist, are back in fashion, notably his 1955 book The Great Crash 1929. Furthermore, the financial crisis has brought to the fore the views of a well-known
Table 1.2 Heterodox schools of thought in economics

<table>
<thead>
<tr>
<th>School of thought</th>
<th>Associations</th>
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<tr>
<td>Post-Keynesians</td>
<td>Post Keynesian Economics Study Group (PKSG)</td>
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<td>Association des Études Keynésiennes (ADEK)</td>
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<td></td>
<td>Associação Keynesiana Brasileiro (AKB)</td>
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<td>Radicals, Marxists, Marxians</td>
<td>Union for Radical Political Economy (URPE)</td>
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<td></td>
<td>Association for Heterodox Economics (AHE)</td>
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<td></td>
<td>International Initiative for Promoting Political Economy (IIPPE)</td>
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<td>Institutionalists (old)</td>
<td>Association for Evolutionary Economics (AFEE)</td>
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<td></td>
<td>Association for Institutional Thought (AFIT)</td>
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<td>Evolutionary political economy</td>
<td>European Association for Evolutionary Political Economy (EAEPE)</td>
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<td>Feminist economics</td>
<td>International Association for Feminist Economics (IAFFE)</td>
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<td>Social and humanistic economics</td>
<td>Association for Social Economics (ASE)</td>
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<td></td>
<td>Political and Ethical Knowledge on Economic Activities (PEKEA)</td>
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<tr>
<td>(Social) Ecological economics (green economics)</td>
<td>International Society for Ecological Economics (ISEE)</td>
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<tr>
<td>Development Structuralists</td>
<td>International Celso Furtado Center for Development Policies</td>
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<td>Schumpeterian economics,</td>
<td>International Joseph A. Schumpeter Society</td>
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<td>innovation economics,</td>
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<td>evolutionary economics</td>
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<td>French Regulation School</td>
<td>Association Recherche et Régulation</td>
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<td>Social structure of accumulation school</td>
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<td>The economics of conventions</td>
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<td>Monetary circuit school</td>
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<td>Behavioural economics (old)</td>
<td>Society for the Advancement of Behavioral Economics (SABE)</td>
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<td>Polanyi economics</td>
<td>Karl Polanyi Institute of Political Economy</td>
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<td>Gesellian economics</td>
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<td>Ghandian economics</td>
<td>Ghandi Foundation</td>
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<td>Georgian economics</td>
<td>Henry George Institute</td>
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<td>Neo-Austrian economics (?)</td>
<td>Society for the Development of Austrian Economics (SDAE),</td>
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<td>Ludwig von Mises Institute</td>
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<td>Agent-based modelling</td>
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<td>System dynamics</td>
<td>System Dynamics Society</td>
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post-Keynesian economist – Hyman P. Minsky – to such as extent that journalists at the *Wall Street Journal* and other newspapers were making references to a ‘Minsky moment’. Minsky conferences, organized by the Levy Economics Institute, now attract the presidents of some of the Federal Reserve banks in the USA. All this action around Minsky has led to new editions of three of his books, which for a while could be found even in airport bookstores.
But the revival of alternative economic thinking does not stop there. It extends to all brands of heterodox economics (see Table 1.2), in particular Marxism and the French Regulation School, whose credibility has also been given a boost. Indeed, in their explanations of the crisis, there are substantial similarities between the writings of several post-Keynesians, notably those concerned with the study of a monetary production economy, and those of members of the French Regulation School (for instance Robert Boyer, Jacques Mazier, Dominique Plihon, Frédéric Lordon), the French Convention School (notably André Orléan and his remarkably prescient 1999 book), and some Keynesian Marxists who share close ties with the post-Keynesian school (such as James Crotty and Gerald Epstein in the USA or Gérard Duménil and Dominique Lévy in France). A major reason why these authors of various backgrounds and traditions have a common understanding of the events of the last decade is that they hold a common view of what economics is all about.

Readers may have noted that Institutionalists and behavioural economists are listed in Table 1.2 with the additional ‘old’ qualifier. This is because, as we shall see, some of behavioural economics remains within the neoclassical tradition, while new Institutionalism is a variant of neoclassical economics. As a result, old Institutionalism and old behavioural economics could equally be called original Institutionalism and original behavioural economics. The labour economists of the old Institutionalist tradition helped to create a new field – industrial relations – which is still impervious to neoclassical influence (Kaufman, 2010a). Neo-Austrians carry a question mark, because, as we shall see in the next section, although they consider themselves as heterodox economists, they do not endorse the key features common to the other heterodox schools. The appearance of agent-based modelling, of which there are several brands, in this list may surprise some readers. But after discussion with some of these practitioners, I have come to the conclusion that several agent-based modellers share many of the criticisms that post-Keynesians would address to neoclassical economists, and that the key features of their models are genuinely of a heterodox nature. As to system dynamics, Michael Radzicki (2008, p. 157) has persuasively argued that system dynamicists ‘view the world through the same lens’ as Institutionalists and post-Keynesian economists, and there are certainly tight links between the feedback loops and the stock–flow analysis promoted by system dynamicists and the stock–flow coherent analysis advocated by some post-Keynesians. In a recent article, based on nearly 20 criteria, Earl and Peng (2012) attempt to assess to what extent eight of these schools, plus new Institutionalism and new behavioural economics, are more heterodox than orthodox.

### 1.2.3 Dissenters and Heterodox Economists

One problem in distinguishing heterodox and orthodox economics is that some orthodox economists, in particular New Keynesian economists such as Krugman and Stiglitz, are very critical of their orthodox colleagues. Some of their critiques are not dissimilar to those put forward by heterodox authors. Furthermore, sometimes the economic policies that they recommend are very similar to those that post-Keynesian economists advocate, albeit with less visibility. Thus one needs to make a further distinction, inspired by the proposals of Roger Backhouse (2004), whose article is devoted to understanding the nuances between disagreement, controversies and dissent in economics.
Besides heterodoxy and orthodoxy, economists can be divided into two broad groups: the mainstream and the dissenters. The mainstream essentially corresponds to the textbook view. ‘Its existence as a coherent intellectual whole is generally most strongly expressed in textbooks at the upper undergraduate and at the graduate levels’ (Colander et al., 2007–08, p. 306). The dissenters, as argued by Backhouse (2004), are themselves subdivided into two additional groups: the orthodox dissenters and the heterodox dissenters. Those three groups are represented in Figure 1.1, with the heterodox dissenters on the left, the mainstream on the right, and the orthodox dissenters in between the other two groups. Schools of thought such as the post-Keynesians, the Marxians, the Radicals and the old Institutionalists are clearly heterodox dissenters. Orthodox dissenters include authors such as the Institutionalist Oliver Williamson.

In his paper on the nature of heterodox economics and neoclassical economics, Davis (2006, p. 27) does not use the terminology proposed by Backhouse, but I think that this is what he has in mind when he says that ‘heterodox economics post-1980 is a complex structure, being composed of two broadly different kinds of heterodox work . . . : the traditional left heterodoxy and the “new heterodoxy” resulting from other science imports’. His ‘traditional heterodoxy’ is Backhouse’s heterodox dissent, while his ‘new heterodoxy’ (later called ‘mainstream heterodoxy’ in Davis (2008, p. 359)) is orthodox dissent. Similarly, those that Colander et al. (2007–08, p. 309) call ‘outside the mainstream heterodox economists’ are Backhouse’s heterodox dissenters, while their ‘inside the mainstream heterodox economists’ are Backhouse’s orthodox dissenters. Lawson (2009b, pp. 93–114), without, however, using the terms orthodox dissent and heterodox dissent, but obviously being in agreement with such a nomenclature, explains at length why Davis’s new heterodoxy is not part of the heterodox programme, and why heterodox dissenters are instead committed to the methodological project pursued by mainstream economists.

Frederic Lee (2009, p. 4) also utilizes a slightly different nomenclature, a more provocative one as it is akin to religion, an analogy that, nevertheless, may be quite adequate for economics, calling ‘heretics’ those that Backhouse would name orthodox dissenters, while the heterodox dissenters are named ‘blasphemers’. Here Lee uses the term heretics in a
sense different from that of Keynes in the *General Theory*. In the context of economics, heretics believe in the mainstream and its methodology, but they advocate modifications to the doctrine. Thus they are not really a threat and are tolerated, the more so if they come from the upper ranks of the hierarchy. By contrast, the blasphemers are non-believers. They reject the core of the mainstream, deny its relevance and truth, and do not really wish to improve its doctrine. They have their own agenda, unrelated to that of the mainstream. They are apostates, who have apostatized from the mainstream, giving it up entirely. They are the heterodox dissenters.

Thus heterodox economists are dissenters in economics. But the concept of dissent is much broader than that of heterodoxy. Heterodox dissenters are unlikely to become part of the mainstream, and their position in the pecking order is likely to remain precarious. By contrast, orthodox dissenters may turn into heterodox dissenters or may become part of the mainstream, either from their own volition or because the bulk of the profession moved towards their propositions. Backhouse offers some examples of orthodox dissenters, such as the French Disequilibrium School in the late 1970s, with Malinvaud and Bénassy. Milton Friedman was certainly a dissenter in the 1950s, but then his views became mainstream in the late 1960s. Similarly, the new consensus model, now best known as the dynamic stochastic general equilibrium model (the DSGE model), based as it was on a central bank reaction function involving the rate of interest rather than the money supply stock, was certainly considered as orthodox dissent at its beginning, but it is now the bread and butter of central bank researchers. Keynes himself, with the publication of the *General Theory* in 1936, was most probably perceived as an orthodox dissenter. As Herbert Simon (1997, p. 14) says, ‘without the acceptance of the marginalist methods of thought, *The General Theory* would not have had the enormous and relative quick impact that it had on the thinking of mainstream economists’. This, by the way, raises a problem mentioned by Wladimir Andreff (1996) and by Earl and Peng (2012, p. 466): what if some heterodox dissenting stances were to become the most accepted paradigm? Could we still call them heterodox views? This is a somewhat rhetorical question, because, as pointed out earlier, it is a rather unlikely possibility now.

Other examples of orthodox dissent may include the work of authors as diverse as Robert Shiller, Richard Thaler, Colin Camerer, Harvey Leibenstein, Dan Rodrick, Herbert Simon, Ronald Coase, Wassily Leontief, Amartya Sen, George Akerlof, Paul Krugman, Joseph Stiglitz, Oliver Williamson or William Vickrey, the last nine economists having won the Nobel Prize in economics. Some have explicitly stated that they certainly did not want to rock the mainstream boat. For instance Thaler, the behavioural economist, is cited as saying that he did not want ‘to lay waste to the entire mathematical, hard science apparatus that economists had built after World War II’ (Fox, 2009, p. 187). Others, like Simon and Vickrey, have turned towards heterodox economics.

### 1.3 Presuppositions of the Heterodox and Orthodox Paradigms

So far I have claimed that there exist two communities of economists, heterodox and orthodox. The philosophers of science would call these research programmes (Imre Lakatos) ‘research traditions’ (Laudan) or ‘paradigms’ (Kuhn). Both research pro-
grammes extend through all fields and domains of economics; within each field, each encompasses several theories or schools of thought; each theory entertains several models. Our task in this section is to identify the essentials of each of the two broad research programmes, what Leijonhufvud has called the presuppositions of a research tradition, that is, the set of commonly held metaphysical beliefs, which cannot be put in a formal form, and which are anterior to the constitution of the assumptions that rule specific models. These are the essentials of the research programme or their ‘meta-axioms’. They are ‘grand generalities somewhat in the nature of cosmological beliefs’ (Leijonhufvud, 1976, p. 72). Tony Lawson (2006) expresses this by saying that orthodox and heterodox economists do not share the same ‘ontology’: they disagree on their preconceptions of the nature and structure of reality.

Although Marxians, Institutionalisls, Structuralists, Evolutionarisls, Socio-economists, the French Circuit and Regulation schools, Sraffians and post-Keynesians may have substantially different opinions on various topics, such as the theory of value or the relevance of long-period analysis, I believe they hold the same metaphysical beliefs, prior to the elements constituting the hard core of their respective theories. Similarly, Lawson (2009b, p. 123) argues that these various heterodox schools of thought hold a common implicit conception of social phenomena, and that to a large extent they can be mostly identified through the kind of questions that they ask, so that ‘we can view the separate traditions as divisions of labour’. These heterodox economists are thus linked by something more than their dislike of neoclassical economics. If they dislike orthodox economic theory it is precisely because orthodox economics exudes presuppositions that are contrary to the metaphysical beliefs held by these economists. This is why they have become heterodox economists.

Showing that heterodox economists hold presuppositions that are different from those entertained in the mainstream will help to answer the main objection to the conception of an alternative to neoclassical economics. Mainstream economists rarely understand why any economist would want to work outside the framework of neoclassical economics. It is often believed that neoclassical theory offers the only viable approach to economic problems. Those who are not within the orthodoxy are said to be on the fringes of science. What is argued here is that there are two research traditions in economics, each with its own presuppositions, and that one cannot be called more scientific than the other, even though the orthodox research programme is much more in awe of formalization.

Several economists have attempted to identify what makes heterodox economics distinct from orthodox or neoclassical economics. This is not an easy task, as Andrew Mearman (2012a) reminds us. Over the last 20 years or so, I have argued that heterodoxy and orthodoxy can be distinguished through four pairs of presuppositions, to which I have recently added a fifth one; all these can be found in Table 1.3. These five pairs result from my understanding of the two research programmes as well as from my reading of fellow economists interested in methodology, some of which, like Malcolm Sawyer (1989, pp. 18–21) and Mauro Baranzini and Roberto Scanzieri (1986, pp. 30–47), have suggested the same essentials. More recently, Mark Setterfield (2003) has endorsed these same presuppositions. I am not claiming that Table 1.3 represents the absolute truth, or that the pairs could not be rearranged, or new pairs put forward. I am only alleging that it is a convenient way to describe two broad visions of economics. Indeed, in trying, a posteriori, to verify if these presuppositions also applied to feminist and ecological economics,
two fields about which I knew little, I discovered that these five presuppositions did a good job of describing these two traditions (Lavoie, 2003a; 2009a).

1.3.1 Instrumentalism versus Realism

Most outsiders would agree that ‘economics should be about economic reality and should be demonstrably relevant to it’ (Werner, 2005, p.17). Some economic methodologists, most notably Lawson (1994), argue that the only crucial presupposition is that of realism. He argues that all the other presuppositions follow from it. This may be so, although Lawson himself seems to pay quite a bit of attention to another presupposition, atomicism, but I think it is worth spelling out all the others.

Realism and realisticness

Lawson, notably through his Cambridge Realist Workshop, has had a remarkable impact on economic methodologists in promoting the discussion of ontology, that is, the examination of the nature and existence of the phenomena under consideration, and a number of post-Keynesian economists have given explicit support to his philosophical views of transcendental realism and critical realism (e.g. Arestis, 1996; Dunn, 2008; Fontana, 2009). As Patrick Baert (1996, p. 513) has ironically written, ‘a spectre is haunting the philosophy of the social sciences. It is called “critical realism”, and, needless to say, it is spreading steadily throughout the academic community.’ Indeed, several books in economics have been devoted to studies in critical realism and a full book has been devoted to a debate between Lawson and his critics on the topic of ontology and economics. In this book, Fullbrook (2009, p.1) claims that Lawson’s major point is ‘that success in science depends on finding and using methods, including modes of reasoning, appropriate to the nature of the phenomena’. Lawson believes that, although some stylized facts can be identified, a constant conjunction of events rarely occurs when analysing economic and social phenomena. As a result, the researcher must go beyond surface phenomena and find the true structures and causal mechanisms that explain the observed phenomena, by going to the essential rather than the most general. This, according to Lawson, is not what orthodox economists do.

If this sounds overly philosophical and vague – after all, it seems to me that everyone tries to go beneath surface phenomena – one may prefer instead to refer to the more down-to-earth concept of realisticness, with which I am more at ease. According to Muskali Mäki (1989, p.179), when talking about the assumptions of a theory, ‘we should

<table>
<thead>
<tr>
<th>Presupposition</th>
<th>Heterodox schools</th>
<th>Orthodox schools</th>
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<tr>
<td>Epistemology/Ontology</td>
<td>Realism</td>
<td>Instrumentalism</td>
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<tr>
<td>Rationality</td>
<td>Environment-consistent rationality, satisficing agent</td>
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</tr>
<tr>
<td></td>
<td>Holism, organicism</td>
<td>Individualism, atomicism</td>
</tr>
<tr>
<td>Method</td>
<td></td>
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</tr>
<tr>
<td>Economic core</td>
<td>Production, growth, abundance</td>
<td>Exchange, allocation, scarcity</td>
</tr>
<tr>
<td>Political core</td>
<td>Regulated markets</td>
<td>Unfettered markets</td>
</tr>
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</table>

Table 1.3 Presuppositions of the heterodox and orthodox research programmes
not talk about “realism” of assumptions and theories, but rather about their realist
ness ... “unrealisticness” means being not about reality or observables, being about in-
essentials, being false, disconfirmed in tests, idealizing, exaggerating, oversimplified,
implausible, practically irrelevant. Both realist
ness and unrealisticness are properties of represen-
tations. Lawson (2009c, p. 171) himself agrees with this distinc-
tion, saying that ‘realisticness’ applies to the properties of ac-
tual theories, and suggesting that one should not say that mainstream theories lack real-
ism, but rather that they lack realisticness.

There are certainly strong indications that heterodox authors attach great importance
to discussing and modelling the economy on the basis of realistic assumptions. Caldwell
(1989, p. 55) assesses that the most damning criticism of post-Keynesians against neo-
classical economics is that it lacks realism, or rather that it lacks ‘realisticness’, arguing
further that post-Keynesians value explanation rather than prediction, a characterization
that can certainly also be applied to Institutiona
lists, who emphasize the storytelling
method. Similarly, Morris Altman (2006, p. xvi), an author closer to the radical branch
of behavioural economics, claims that ‘what is critical to behavioral economics is the
appreciation of the significance for economic analysis of the realism of one’s modelling
assumptions in terms of their behavioral and institutional dimensions’. Thus, as Lee
(1994) says, theories should be ‘empirically grounded’. And indeed, as Smithin (2009,
p. 56) concurs, the ‘emphasis on the realism of assumptions in macroeconomic models
seems to be analogous to Lee’s . . . concept of grounded theory’.

Instrumentalism at work

By contrast, take the neoclassical presupposition of instrumentalism, defended by Milton
Friedman (1953) in his famous essay on methodology. Instrumentalism is the belief
that the truth of a theoretical statement is irrelevant. An assumption is sound when it
allows precise predictions, in particular when it can help to find and calculate the value
of an equilibrium position. Whether the assumption is realistic or not is irrelevant, and
Friedman (1953, p. 14) even went so far as to argue that assumptions based on ‘wildly
inaccurate descriptive representations of reality’ were more useful; ultimately, whether
the prediction is accurate or not is also irrelevant (cf. Taleb, 2007, p. 280). The stance
taken by Friedman, which Nathan Berg and Gerd Gigerenzer (2010) call ‘Friedman’s as if
doctrine’, gave neoclassical economists the freedom to start from wildly unrealistic foun-
dations. Robert Lucas (1981, p. 270), the founder of new classical economics, continued
this tradition, claiming that ‘insistence on the “realism” of an economic model subverts
its potential usefulness in thinking about reality’, adding that good models had to ‘neces-
sarily be artificial, abstract, patently unreal’.

By contrast, for (most) heterodox economists, a theory cannot be correct unless it
starts from realist or realistic hypotheses, although it is recognized that assumptions are
always abstractions and simplifications, and hence means to avoid cluttering a model
with insignificant details. However, one should not start from assumptions that are
descriptively false. The structure of a model cannot be built on foundations describing an
imaginary or idealized economy. What is needed is an abstraction, not a fiction. Many
heterodox economists would agree with Nicholas Georgescu-Roegen’s (1971, p. 319)
statement that ‘when abstraction loses touch with reality, science becomes dogmatism’.
The heterodox desire for realistic assumptions can be related to the fact that heterodox
economists attach great importance to the storytelling method. An explanation has to be
provided, usually associated with some causal mechanism, that goes beyond such simple claims as that supply or demand has shifted; therefore this or that has happened. Thus one has to go beyond surface phenomena, and examine the mechanisms or the structures that cause what is happening. What is required is an explanation of the causal processes at work. Obviously, if a story has to be told and explanations provided, one needs to pay more attention and scrutinize the starting assumptions, which need to be appropriately descriptive.

Heterodox economists are not interested in the counterfactual economies that have been the playground of researchers in general equilibrium theory such as Arrow, Debreu or Frank Hahn, and that are now the subject of state-of-the-art orthodox models. For instance, when Bliss (1975, p. 301) presents the intertemporal general equilibrium model, which is the twin of the now popular DSGE model, he claims that ‘of course, that model does not serve to represent reality and that is not its purpose’. Hahn has made many similar claims regarding the irrelevance of his work for public policy. Unfortunately current neoclassical researchers do not appear to get this, as they maintain that variations on the Ramsey model, first designed to describe a planned economy, ought to be good enough to study capitalism.

Take as another example of neoclassical instrumentalism the so-called ‘Gaussian copula function’ that was used by financial engineers to model default correlation in the transformation of asset-backed securities (ABS) and the pricing of collateralized debt obligations (CDO), which were made up of tranches of ABS, and in the pricing of CDO-squared, which were made up of tranches of CDO (Salmon, 2009). As we know, these financial derivatives arising from securitized loans were at the core of the financial crisis. Instead of relying on the records of borrowers to assemble historical data about actual defaults to assess correlation and risk, finance economists looked instead at the evolution of the prices of credit default swaps (CDS) – the asset-backed securities index (ABX) –assuming that CDS markets can price default risk correctly. Another instance is the value-at-risk models that were based on high-frequency and very precise calibrated estimates; but they relied on samples that did not include catastrophic events and that were based on a particularly low volatility of the stock market, as pointed out by Boyer et al. (2005, p. 145). In those two instances we have instrumentalism in action. What counts most is to get a number. Whether that number is reliable is not so important. The fact that previous financial crises in the past, such as the Tequila crisis, have shown that markets do not necessarily correctly price risk is put aside; the fact that CDS markets had only been in existence for a short time, that is, only since housing prices had been on the rise, did not seem to matter either; the fact that the convenient normal distribution has long been shown by physicist Benoît Mandelbrot not to describe financial data, by under-representing extreme events, also seemed of little importance; and finally, the fact that the (recent) past is no guarantee of an uncertain future was also ignored.

Instrumentalism, in contradistinction to realism, implies, as Paul Davidson (1984, p. 572) would put it, that it is better ‘to be precisely wrong rather than roughly right’. By contrast, post-Keynesians ‘believe it is better to develop a model which emphasizes the special characteristics of the economic world in which we live than to continually refine and polish a beautifully precise, but irrelevant model’ (ibid., p. 574). Nassim Taleb (2007, pp. 284–5) says nearly the same thing, arguing that heterodox economists ‘want to be broadly right rather than precisely wrong’, seeking ‘to be approximately right
across a broad set of eventualities’ instead of being ‘perfectly right in a narrow model, under precise assumptions’. Storytelling puts less emphasis on formalistic methods. For instance, Lawson (2009a) has argued, rightfully so it seems, that one could certainly put forward an adequate explanation of the Global Financial Crisis while omitting formal economics altogether.

Some may object that there is a good deal of realisticness in many mainstream models, in particular in the models put forth by New Keynesian authors. This can certainly be granted. Realisticness is integrated into the auxiliary hypotheses – asymmetric information, credit rationing, liquidity-constrained households, sticky prices. Some realism is also now being added to the state-of-the-art DSGE models, by introducing frictions in the financial system and by assuming the existence of banks (!). The main assumptions, however, based on an all-knowing agent attempting to maximize some utility function for eternity, defy common sense, as argued below. Orthodox economists, even many dissident orthodox authors, dress up their unrealistic foundations with realistic auxiliary hypotheses. The question, then, is whether it is possible to arrive at a model that describes the real world adequately by adding auxiliary realistic characteristics.

Nicholas Kaldor (1966, p. 310), for one, thought it was not possible: in an attempt to relieve the programme of its unrealistic foundations, the whole edifice would crumble. As he put it, removing the scaffolding ‘is sufficient to cause the whole structure to collapse like a pack of cards’. Indeed, Kaldor thought that this defect of neoclassical theory was so important that he repeated the same argument six years later.

The process of removing the ‘scaffolding’, as the saying goes – in other words of relaxing the unreal basic assumptions – has not yet started. Indeed the scaffolding gets thicker and more impenetrable with every successive reformulation of the theory, with a growing uncertainty as to whether there is a solid building underneath. (Kaldor, 1972, p. 1239)

This can certainly be observed of neoclassical macroeconomics, which strives on ever more extraordinary and unrealistic foundations. The same can also be said about new behavioural economists: while they intend to relax the most unrealistic features of the neoclassical model of the rational man, such as the belief that agents have access to all information at no cost, they are being forced to superpose other, even more unrealistic assumptions, as their utility-maximizing agents now need extraordinary computational abilities to handle their new information-costly environment.

1.3.2 Model-consistent Rationality versus Environment-consistent Rationality

Closely related to realism and instrumentalism is the kind of rationality that is assumed in our economic models. Following the rational expectations revolution, the only type of rationality admissible to mainstream macroeconomists is model-consistent rationality, which we can also call unbounded rationality. Not only are economic agents assumed to know all contingencies, from now to eternity; since the rational expectations revolution they are further assumed to know how the world operates. Despite the fact that economists have been arguing with each other for centuries about the proper representation of the economy, modellers must assume that there is a single accepted model of the economy out there and that everyone agrees about how it functions. This is the RARE assumption of new consensus macroeconomics, as noted earlier. As Philip Mirowski
(2011, p. 503) puts it, ‘orthodox macroeconomists came to conflate “being rational” with thinking like an orthodox economist. What this implied was that agents knew the one and only “true model” of the economy (which conveniently was stipulated as identical with neoclassical microeconomics’.

It is true that behavioural economists have tried to modify this by introducing heterogeneous agents into the realm of expectations, traders and chartists who rely on trends, alongside presumed truly rational investors who still look at the fundamentals, but they have made little headway in the more reputable journals. Most of the behavioural economists who have published in reputable journals agree with the as-if Friedmanian doctrine and argue ‘that the goal of their models is not to provide a veridical description of the actual decision processes being used by economic agents, but to predict the outcome’ (Berg and Gigerenzer, 2010, p. 159). Still, in my view, the more radical segment of behavioural economics – the group still devoted to a description of actual decision-making rather than to the study of biases relative to neoclassical rationality – must be classified under the umbrella of heterodox economics. This other group deals with what I call ‘environment-consistent rationality’.

Economic agents, on this view, live in an environment either devoid of relevant information or characterized by an overload of unreliable information, and hence must follow some simple rules to make decisions without wasting too much time and resources. Agents attempt to achieve norms and will modify their short-run behaviour when these norms are not satisfied, thus reacting to what they perceive as disequilibria. In the long run, norms will be modified if they are continuously under- or over-achieved, or if changes in society at large have an impact on what is considered normal in the economic field. A good example of this is the gradual acceptance of the claim that a ‘normal’ return on equity, the famous ROE norm imposed by financial investors to managers, ought to be no less than 15 per cent, although this norm is incompatible with average macroeconomic conditions in Western economies, as has been demonstrated by Plihon (2002). More about reasonable rationality will be said in Chapter 2.

1.3.3 Atomicism versus Holism

The third pair of presuppositions concerns methodology: methodological individualism or atomicism versus holism or organicism. For Lawson, the conception of an economy based on isolated atoms is an essential feature of the lack of realism of orthodox theories. Atomicism, as practised by neoclassical economists, has a long history. Voltaire, in his famous novel, Candide, was already making fun through his Pangloss character of those who, like Leibniz, thought that non-interacting ‘monads’ ensure that we live in the best of all possible worlds. There is certainly a great deal of similarity with the neoclassical claim that all analysis must start at the level of the isolated optimizing individual and that competition between free atomistic firms will generate a Pareto optimum. Similarly, uncertainty in neoclassical analysis is often represented in terms of subatomic particles being subjected to a random Brownian motion. Within the framework of the subprime crisis, atomism is exemplified by the long-held belief that risk analysis could focus exclusively on individual firms and banks, without taking into account the macroeconomic conditions and implications, that is, by ignoring systemic risk. Another example would involve consumer behaviour: neoclassical authors assume that consumption expen-
ditures are hardly influenced by marketing and publicity, and that consumers are not interdependent.

By contrast, heterodox authors have taken a more holistic approach. In relation to consumers, they have emphasized the predisposition to replicate the behaviour of others or to catch up with others, the search for status and positional goods, and the role of innovations in consumer credit. Many post-Keynesian authors see this interdependence as a driving force in the Global Financial Crisis, as low-income and median-income households, whose purchasing power has remained flat over the years, have attempted to keep up with the consumer and real-estate behaviour of upper classes, whose real incomes have quickly risen during the same time period (Brown, 2008; Barba and Pivetti, 2009; Zezza, 2008). Even more relevant to the crisis, perhaps, is the observation that herd or ‘group behavior is the essence of financial markets’ (Wojnilower, 1983, p. 179).

In (nearly) all heterodox models there are social classes, workers, capitalists, entrepreneurs, bankers and rentiers. The consideration of these classes, for income distribution purposes or for the theory of effective demand, arises from the presupposition that the definition of individual preferences is not sufficient to allow us to understand society. The consideration of individuals as social beings rather than atomistic ones allows not only for the explicit introduction of dominant and powerful institutions. But whereas mainstream economists view institutions as imperfections that prevent perfect competition, heterodox authors see them as providing some stability (Hodgson, 1989, p. 116). Stability, in a world of uncertainty, develops along the lines of organic interdependence, thanks to social conventions.

One may wonder how agent-based modelling fits within this atomistic/holistic dichotomy. On the one hand, agent-based modelling obviously starts with a multitude of atomistic agents, households or firms. On the other hand, modellers must design rules that their agents have to follow as they interact with each other. These rules constitute a social structure that is way more complex than the standard excess demand rules that are associated with orthodox economics, not to speak of the inanity of the single representative agent that populates DSGE models. In addition, a key characteristic of agent-based modelling is the ‘emergence’ of macroeconomic properties, which cannot be derived from the individual behaviour. From that perspective, it seems clear that agent-based modelling, or at least some versions of it, belongs to the heterodox tradition.

Emerging properties can be considered as macroeconomic paradoxes, or fallacies of composition, that contradict the pure aggregation of a representative agent. Heterodox authors pay considerable attention to these, and they are reminiscent of Marx’s contradictions of capitalism. What seems reasonable for a single individual or nation leads to unintended consequences or even to irrational collective behaviour when all individuals act in a similar way. By way of example, we may think of the following eight paradoxes noted in the past by post-Keynesian authors, and summed up in Table 1.4: the paradox of thrift; the paradox of costs; the paradox of public deficits; the paradox of debt; the paradox of tranquillity; the paradox of liquidity; the paradox of risk; and the paradox of profit-led growth. Some of these paradoxes, most certainly the paradox of thrift, are well known, and need little discussion. And all these paradoxes are certainly relevant to the Global Financial Crisis, either as a cause or as a way out of the crisis.
Table 1.4 Holism: some post-Keynesian macro-paradoxes

<table>
<thead>
<tr>
<th>Paradox of thrift (Keynes, 1936)</th>
<th>Higher saving rates lead to reduced output</th>
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<tbody>
<tr>
<td>Paradox of costs (Kalecki, 1969; Rowthorn, 1981)</td>
<td>Higher real wages lead to higher profit rates</td>
</tr>
<tr>
<td>Paradox of public deficits (Kalecki, 1971)</td>
<td>Government deficits raise private profits</td>
</tr>
<tr>
<td>Paradox of debt (Fisher, 1933; Steindl, 1952)</td>
<td>Efforts to de-leverage might lead to higher leverage ratios</td>
</tr>
<tr>
<td>Paradox of tranquillity (Minsky, 1975)</td>
<td>Stability is destabilizing</td>
</tr>
<tr>
<td>Paradox of liquidity (Dow, 1987; Nesvetailova, 2007)</td>
<td>New ways to create liquidity end up transforming liquid assets into illiquid ones</td>
</tr>
<tr>
<td>Paradox of risk (Wojnilower, 1980)</td>
<td>The availability of individual risk cover leads to more risk overall</td>
</tr>
<tr>
<td>Paradox of profit-led demand (Blecker, 1989)</td>
<td>Generalized wage restrictions lead to a slowdown in growth even when all economies seem to be profit-led</td>
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</table>

Three formalized macroeconomic paradoxes

Keynes’s paradox of thrift says that an increase in the propensity to save will lead to reduced output. In its growth version, it says that it will lead to a decrease in the actual growth rate of output. With households being over-indebted, the paradox of thrift acts against the recovery, as households desperately try to restore their past levels of wealth by saving a larger proportion of their revenues. A quick check confirms that the notion of the paradox of thrift has now disappeared from most principles of economics textbooks. The Global Financial Crisis illustrated the lack of awareness of this paradox, as several new classical economists seemed to endorse Hayek’s view that purchasing additional consumption goods would increase unemployment (Robinson, 1973, p. 94). Luckily, some decision-makers understood the paradox of thrift: Mark Carney (2008, p. 2), former Governor of the Bank of Canada and now Governor of the Bank of England, referred to it in a speech made during the financial crisis, when he pointed out that it would be ‘individually rational for people to want to save more’ in uncertain times, although if all individuals do so, then ‘it becomes collectively irrational’.

The paradox of costs, in its static version, says that a decrease in real wages will not raise the profits of firms and will instead lead to a fall in the rate of employment. This was explained by Kalecki (1969, p. 26) in a Polish paper first written in 1939, where he concluded that ‘one of the main features of the capitalist system is the fact that what is to the advantage of a single entrepreneur does not necessarily benefit all entrepreneurs as a class’. Its dynamic version has been proposed by Robert Rowthorn (1981). It says that rising real wages (relative to productivity) can generate higher profit rates. This flies in the face of a microeconomic analysis that would demonstrate that lower profit margins generate lower profit rates. But if higher real wages generate higher aggregate consumption, higher sales, higher rates of capacity utilization and hence higher investment expenditures, profit rates will be driven up. This of course is nothing else than a variant of Marx’s problem of the realization of profit, underlined for instance by Amit Bhaduri (1986). In the midst of a crisis, it is important to resist calls to reduce labour costs in an
effort to improve the profitability of individual firms. While this will be profitable to the firms that achieve the greatest real wage reductions, the overall effect will be detrimental to the overall economy, and most certainly to the overall world economy, as we shall discuss further in later chapters.

The paradox of public deficits can be directly attributed to Kalecki (1971). He showed that higher government deficits play a role similar to that of higher net exports on corporate profits. Higher public deficits lead to higher corporate profits, just like higher public deficits lead to higher GDP and employment following the teaching of Keynes. While mainstream authors used to argue about the crowding-out effects of government activity, based on Ricardo-equivalence effects or rising real interest rates, several governments have engaged in expansionary fiscal policies in 2009 so as to sustain aggregate demand and corporate profits despite the financial crisis. When things go really wrong, neoclassical theories are thrown out of the window, being replaced by more pragmatic and realistic theories. It must be admitted, however, that pragmatism did not occur for long, as governments quickly called for fiscal consolidation programmes, especially in Europe.

Four paradoxes tied to the financial system

Then there is the paradox of debt. This paradox is also based on the concept of effective demand, and it was put forward by Joseph Steindl (1952, ch. 9), who was a follower of Kalecki. From a strictly microeconomic point of view, one would be led to believe that it is always possible for economic agents to reduce their debt or leverage ratios by simply deciding to do so. While this may be true for households, it may be quite difficult for firms and financial institutions taken as a group. To reduce the weight of indebtedness, firms may decide to cut their investment expenditures and hence the amounts they borrow. However, if all companies are pursuing this scheme, cutting back on borrowing and investment may not put matters right, for the slowdown in capital accumulation reduces the overall profitability of businesses and hence the accumulation of retained earnings. In the end, the actual leverage ratio may rise, moving in a direction that is the opposite of what is intended by the entrepreneurs. This is what Steindl (1952, p. 119) and Jan Toporowski (2005, p. 126) call ‘enforced indebtedness’. The paradox of debt may also apply to governments: as they reduce government expenditures and pursue other austerity measures to reduce public debt, the government debt to GDP ratio may rise instead.

Something quite similar may happen to banks and other financial institutions as they try to reduce their leverage ratios. This is linked to Irving Fisher's debt-deflation effect. As banks sell some of their assets, in an effort to reduce leverage or recover liquidity, such forced sales bring down the price of these assets, which are now sold at a loss, thus reducing the banks’ own funds, so that the leverage ratio is rising instead of falling. Other efforts to reduce the amount of loans may put borrowers in financial distress, as is observed in times of credit crunch, so that again individual attempts to reduce the leverage ratio (or to increase the capital to asset ratio) may indeed lead to the opposite macroeconomic effect. This can be associated with what we could call the paradox of banking refusal. When the economy is slowing down or is entering a recession, it may be rational for each individual bank to take protection measures against loan losses by rationing credit and refusing to grant new loans. But, as is recognized by the Governor of the Bank
of England, if all banks do the same, ‘their actions will exacerbate the downturn and increase their eventual losses’ (Carney, 2008, p. 2).

Also closely tied to the financial system is the paradox of tranquillity. This is an expression that I coined nearly 30 years ago (Lavoie, 1986a, p. 7), when studying the works of Minsky. According to Minsky, a stable growing economy is a contradiction in terms. A fast-growing free-market economy will necessarily transform itself into a speculative booming economy. In a world of uncertainty, without full information about the fundamentals, a string of successful years diminishes perceived risk and uncertainty. People tend to forget the difficulties encountered in the past: turning points, falling asset prices, credit crunches and recessions. As time goes on, memories fade and economic agents dare to take on higher levels of risk. Or else, as time goes on, the risk level as computed by engineering models of finance, such as the very popular value at risk model, appears to get smaller because the last recession is just one remote observation among a series of more recent successful years. The longer an economy is in a tranquil state of growth, the less likely it is to remain in such a state. As Minsky himself says, ‘each state nurtures forces that lead to its own destruction’ (Minsky, 1975, p. 128). In three words, the paradox of tranquillity says that ‘stability is destabilizing’ (Minsky, 1982, p. 26). Applied to a monetary economy, this implies that a string of successful financial operations will induce banks to indulge in ever riskier financial structures.

What Minsky was claiming 30 years ago seems quite prescient today: ‘Over a period in which the economy does well, views about acceptable debt structure change. In the deal-making that goes on between banks, investment bankers, and businessmen, the acceptable amount of debt to use in financing various types of activity and positions increases’ (Minsky, 1977, p. 24). The cushion for safety – the difference between the additional revenues expected from some new activity and the financial commitments required by this activity – gets reduced through time. For Minsky, instability and the rising fragility of the financial system are inherent features of an unregulated capitalist economy. Part of this destabilizing stability is tied to financial innovations, which will be introduced or expanded when things go well (ibid.). This view of the financial system is reminiscent of that of John Kenneth Galbraith, who, in his various books, most notably *A Short History of Financial Euphoria* (1990), has argued that speculative euphoria in market capitalism was an inevitable outcome, as speculators and bankers ride the wave by using leverage, and believe they become rich because they are smart.

The paradox of tranquillity is certainly at the heart of the Global Financial Crisis. But no less important for the subprime crisis is the paradox of liquidity. In modern finance theories of the neoclassical type, most assuredly the efficient-market hypothesis, liquidity is of little concern. It is assumed that well-informed market participants always manage to arrive at a transaction price reflecting the correct fundamental value of an asset. What is at issue is only the expected return and the estimated risk of the asset. By contrast, liquidity is a crucial element of post-Keynesian economics (Davidson, 2009). Investors should always be concerned about the impossibility of cashing in their assets. There must be some market-maker who guarantees to purchase assets if the market suddenly goes one way. These market-makers are dealers, with access to lines of credit issued by banks, or the banks themselves, with access to central bank liquidity.

The paradox of liquidity can be seen from two angles. First there is the obvious fact, also linked to Fisher’s debt-deflation proposition, that the attempt of economic agents
to become more liquid transforms previously liquid assets into not-so-liquid assets. The frenzy to get rid of assets drives down the price of these assets and may transform the markets for these assets into one-way markets, with no purchaser, leading to a total freeze, as occurred in some markets during the Global Financial Crisis. As Sheila Dow (1987, p. 85) says, ‘attempts to increase the stock of liquid assets only succeed in reducing it; this is a paradox of liquidity on a par with Keynes’ paradox of saving’. But there is a second paradox of liquidity, tied to innovations in the financial system that we just mentioned. Financial innovations seem to increase liquidity when they are really diminishing it. This second paradox was already pointed out by Minsky, but it has recently been underlined in a book. Anastasia Nesvetailova (2007, p. 78) claims that ‘to Minsky and his followers, therefore, every institutional innovation that leads to both new ways to finance business and new substitutes for cash assets, decreases the volume of liquidity available to redeem the debts incurred’. Thus, she continues, ‘in the process of financial expansion the financial system, contrary to appearances, becomes progressively illiquid’. The financial system gets ever more layered, with everybody thinking that they can easily access means of payment, but with virtually nobody holding safe assets without capital-loss risk.

The paradox of liquidity can be extended to a paradox of risk. Financial innovations designed to reduce risk at the microeconomic level, by spreading it over a larger number of financial institutions – as is the case with securitization, collateralized debt obligations, credit default swaps, equity default swaps, interest rate swaps, and the whole gamut of financial futures and financial derivatives – end up creating a larger amount of macroeconomic or systemic risk. For instance, it is now widely believed that the extensive use of mathematical models to quantify risk, yielding the illusion of precise and objective assessments, encouraged banks and other financial institutions to pursue more risky strategies and to use more leverage. Famous US regulators such as Alan Greenspan – the former Federal Reserve Chairman – and Tim Geithner – the former President of the New York Fed and former US Secretary of Treasury – both claimed as late as 2006 that credit derivatives were a stabilizing factor in the financial system, as they reduced the concentration of individual exposure to risk, spreading credit risk to those best able to handle it. Even left-wing economists such as Michel Aglietta (1996) argued that securitization would have beneficial effects on the economy. Each microeconomic agent believes that he or she is now covered against risk; but the risk is still there, in the form of counterparty risk. Indeed, even if the counterparty seems to be safe, the counterparty’s counterparty may not be, and its failure may well spill over. The illusion of liquidity induces agents to take even more risky decisions. Thus risk-reducing microeconomic financial innovations end up producing a more risky macroeconomic environment. Derivatives were likened to the contingent markets of the general equilibrium model à la Arrow–Debreu. But we do not live in such a world. It is completely imaginary. We live in a world of fundamental uncertainty à la Keynes and Knight.

Derivative financial products do not stabilize the economy. While they are a tool of risk management, ‘derivative markets actually increase the credit risks’, since ‘at the first whiff of crisis or instability, the first thing to evaporate is the liquidity’ that these tools are supposed to provide (McKenzie, 2011, p. 212). Thus, ultimately, as summed up long ago by another Minsky follower, Albert M. Wojnilower (1980, p. 309), the ‘supposed immunity to financial risk always turns out to be illusory, and the risks and costs of shattering
the illusion may be considerable’. Wojnilower was particularly perceptive about this, since, as far back as 1984, he predicted the bailout of AIG from its CDS sales:

The recent entry of major insurance companies into the business of insuring banks and bond investors against loan defaults represents another effort to stretch the safety net. Now, it can be presumed, the authorities will have to intervene to interdict a cascading of defaults only if to save the insurance industry. (Wojnilower, 1985, p. 356)

**An open-economy paradox**

The final fallacy of composition shown in Table 1.4 is the paradox of profit-led demand, which has some relationship with the paradox of costs. While a country taken in isolation may succeed in raising its net exports and its economic activity by imposing reductions in nominal and real wages, thus gaining a competitive advantage, this scheme will be unsuccessful if all other countries do the same. As will be discussed in Chapters 6 and 7, when only domestic demand is taken into consideration, thus omitting demand arising from abroad, all countries benefit from an increase in real wages (or in the wage share), mainly because of their positive impact on consumption expenditures. Because Planet Earth is a closed economy, the exports of one country are necessarily the imports of another country, and hence globally net exports are nil. Even though all countries may individually benefit from a change in income distribution towards profits, if other countries do not follow suit, such a change will have detrimental effects on the economic activity of the world economy if all countries pursue wage restrictions.

### 1.3.4 Scarcity versus Abundance

I have already alerted the reader to the fact that the orthodox definition of economics focuses on the notion of scarcity, a definition that heterodox authors reject. Scarcity is the fulcrum of neoclassical economics. As Parguez (2012–13, p. 55) points out, scarcity in neoclassical economics plays a role akin to that of austerity in religion, where austerity is ‘the supreme virtue of renouncing pleasures of worldly life to attain the joy of the afterlife’. Hayek (1941, pp. 373–7), when rejecting Keynes’s economics, is precisely invoking the crucial importance of scarcity. To proclaim the existence of an economy of plenty or an economics of abundance, as did Keynes, was to negate the foundations of orthodoxy. A similar point is made by Galbraith (1958) in his book *The Affluent Society*. In the neoclassical model, the main feature of a capitalist market economy is the proper allocation of resources, real and financial. Prices, as emphasized by Hayek, are supposed to provide all the information required to make the market system function efficiently, because prices are the measure of scarcity, so that the knowledge of prices allows agents to respond to changes in scarce resources.

But is this really the case? Certainly, with respect to recent events, we can assume that prices did misallocate financial resources, as securitization provided misleading prices and too many financial resources were put into real estate. This had just been preceded by the stock-market crash of 2001, when stock markets worldwide took a beating, while the NASDAQ in particular plunged and never fully recovered. And then the real-estate bubble was immediately followed by the super-high prices in commodities, food products and oil, with these prices falling sharply just a few months later, thus giving a strong indication that these prices had risen only as a result of unwarranted speculative activ-
ity rather than as a consequence of changes in fundamentals. Indeed, it has been argued that high commodities prices have arisen from the efforts of financial managers to find new conduits that would be uncorrelated with the returns on bonds and equities. Thus high oil, commodities and food prices result from inflows of funds in the futures markets of these products, as fund managers follow a strategy of portfolio diversification that leads them to speculate on futures indices (Wray, 2008; Davidson, 2008). Again, one may think that these markets for derivatives have no influence on the real world; but they do, because, being more liquid, they induce economic agents to base their decisions on these futures markets, with the result that spot prices depend on futures prices, instead of futures prices being (only) dependent on expected spot prices.

Various conditions will be set in orthodox models to preserve scarcity outside the standard conditions of exchange economies where endowments are fixed: the stock of money will be assumed to be exogenous; full employment and full capacity utilization will be assumed at all times. The crucial assumption in most of modern orthodox macroeconomics, an assumption that drives all the standard results and policies, is the existence of a unique natural rate of unemployment (or of a single non-accelerating inflation rate of unemployment, the NAIRU). Whatever realistic feature is introduced in the model, the assumed uniqueness of the natural rate of unemployment will remove any room for alternative policies. The same can be said about the natural rate of interest: it forbids any imagination in central bank policy.

Scarcity justifies supply and demand analysis. It governs the behaviour of the economy. It explains why neoclassical economists attach such importance to the allocation of resources or why so many of them define the techniques of constrained optimization as the epitome of orthodox economics and a condition for scientific endeavour. When all resources are scarce, they are fully employed, and therefore all questions revolve around the proper use of existing resources, rather than around the creation of new commodities. Scarcity is particularly obvious in pure exchange models. The supplementary hypotheses that can be found in the various sophisticated neoclassical production models are, however, being introduced precisely to safeguard all the main conditions and results of the pure exchange model (Rogers, 1983; Pasinetti, 2007, p. 20). Production in neoclassical economics is a form of indirect exchange, between individual consumer agents who own resources that transit to the same individual agents, then christened producers. These producers are nothing but arbitragistes attempting to benefit from existing scarcities.

In the heterodox research programme, in particular in the post-Keynesian tradition, the notion of scarcity is put aside, while that of reproducibility is put to the forefront (Roncaglia, 1978, p. 5; Pasinetti, 1981, p. 24). With their emphasis on production, heterodox economists embark on the tradition of the classical economists, with their concern for the causes of progress and accumulation. In his review of the Cambridge critique, Rymes (1971, p. 2) makes clear that the Sraffian concern for reproducibility is in the lineage of the economic thought of Robinson, Kaldor and even Harrod. As pointed out by Pasinetti (1981, p. 7), classical authors, in particular Ricardo, focused on the permanent feature of reproducibility, considering that produced goods could be multiplied without limits, and thus judging that, besides land, scarcity conditions could only be of a temporary nature. Thus, for post-Keynesians, prices are not an index of scarcity in general; rather, prices reflect the unit costs of producing these reproducible goods or services.
In post-Keynesian models where output is not disaggregated, the emphasis on production appears through the assumption that in general neither capital goods nor labour is fully employed. In this sense, resources are not scarce. The major problem is not how to allocate them, but how to increase production or the rate of growth. It is generally possible to increase the rate of utilization of capacity and there are reserves of labour. The principle of scarcity is replaced by the principle of effective demand. The true constraint is not supply, but effective demand. As Kaldor (1983b, p. 6) says, ‘for production to be demand-determined, excess capacity must exist as well as unemployed labour’. Arestis (1996, p. 112) concurs: ‘Effective demand in post-Keynesian analysis implies that it is scarcity of demand rather than scarcity of resources that is to be confronted in modern economics, so that output is ordinarily limited by effective demand, although it is recognised that supply constraints are present in modern capitalist economies.’

I would be prepared to argue that, if orthodox economics is the research programme of a world of scarcity, heterodox economics is the research programme of a world of abundance (sometimes in the midst of poverty). John Weeks (2012) has put things in a more striking fashion: ‘The economics of scarcity is pernicious foolishness. By contrast the economics of idle resources addresses reality. It is the same as the difference between alchemy and chemistry, astrology and astronomy, evolution and creationism.’

1.3.5 Unfettered Markets versus Regulated Markets

This leads us to the fifth and last of our key presuppositions, that of the role of markets relative to the role of the state. Mainstream economists exhibit great confidence in the ability of uninhibited markets to deliver stability and full employment, and to deliver solutions to any economic or social problem. The most extreme versions of neoclassical theory claim that instability and unemployment can prevail only when government interferes in the operation of markets, thus hampering the price mechanism from achieving equilibrium. In this version of orthodox economics, the market knows all and is the only provider of truth. All this is well expressed in the following quote:

[The key beliefs of neoclassical economics] are that the pursuit of individual self-interest will lead to a better society, that government intervention beyond the narrow maintenance of law and order should be minimized if not eliminated and that the powers of unfettered markets should be unleashed in virtually every part of society, at home and abroad. For this purpose, structural reforms are recommended to deregulate, liberalize, privatize and open up as many industries and aspects of the economy as possible, as the beneficial forces of the invisible hand, if only allowed to operate freely, would improve people’s lives, create wealth, produce prosperity and lead to maximum happiness. (Werner, 2005, p. 3)

By contrast, heterodox economists are very distrustful of unfettered markets. While post-Keynesian economists and their heterodox colleagues will recognize the dynamism imparted by entrepreneurship in a capitalist system, which along with Joseph Schumpeter they believe to be its main quality in opposition to static allocation efficiency, they question the wisdom of blindly relying on markets. They suspect their unfairness, their inability to self-regulate, their tendency for destabilizing paths and their squandering of resources. Indeed, some heterodox economists – the Marxians – would prefer to eliminate capitalism altogether.
Furthermore, heterodox economists believe that unbridled prices—highly flexible prices—generate instability rather than stability. By contrast, sticky prices with some inertia are more likely to generate stability. Thus they believe that state regulation is needed, both at the micro and macro levels, as the costs of such government intervention become dwarfed compared to the costs of unregulated capitalism. In the eyes of heterodox economists, it is no coincidence that the number of financial crises throughout the world in general and the USA in particular has occurred at such a rising rate ever since deregulation has spread over all economies in the early 1980s. Financial engineers—quants—made a mess of the financial system, as became obvious in 2007. Financial deregulation was based on the claim by orthodox economists that the regulators ought to leave finance to regulate itself because markets know the price of risk and allocate it efficiently, and that market discipline, along with the surveillance of shareholders, would keep a lid on excessive risk-taking. This intellectual edifice has collapsed, just as the intellectual edifice based on a self-adjusting system had collapsed in the 1930s, but the majority of economists still hold on to these ideas.

The fifth presupposition had been identified very clearly by Keynes himself 75 years earlier. Here is what he was saying then:

I have said that we fall into two main groups. What is it that makes the cleavage which thus divides us? On the one side are those who believe that the existing economic system is, in the long run, a self-adjusting system, though with creaks and groans and jerks, and interrupted by time lags, outside interference and mistakes. . . . On the other side of the gulf are those who reject that idea that the existing economic system is, in any significant sense, self-adjusting. (Keynes, 1973, xiii, pp. 486–7)

The post-Keynesian author Minsky has also been pretty clear about this divide between economists, focusing on the financial side:

In a world with capitalist finance it is simply not true that the pursuit by each unit of its own self-interest will lead an economy to equilibrium. The self-interest of bankers, levered investors, and investment producers can lead the economy to inflationary expansions and unemployment-creating contractions. Supply and demand analysis—in which market processes lead to an equilibrium—does not explain the behavior of a capitalist economy, for capitalist economic processes mean that the economy has endogenous destabilizing forces. Financial fragility, which is a prerequisite for financial instability, is, fundamentally, a result of internal market processes. (Minsky, 1986, p. 280)

Some readers may wonder whether it is really important to know whether the economy is stable or not. But think of it this way: how useful can be a theory that assumes from the start that financial markets can never be wrong, that they stabilize the economy, and that there can never be any defaults? Will such a theory be useful in designing rules to regulate the banking or the financial system? Another example could be the transition of Eastern European countries towards capitalism, as advisers and politicians did not seem to understand that capitalism has to be tamed and necessitates strong institutions (Marangos, 2004).

The divide here evoked by Keynes and Minsky is much more important now than it was 25 or 75 years ago. And the forces present are the same as those described by Keynes then, when he said that the ‘self-adjusted school depends on its having behind it almost
the whole body of organised economic thinking and doctrine’ (1973, xiii, p.488). This organized body today is made up of all the major US universities, along with foreign economics departments that attempt to imitate those renowned US economics departments, plus the myriad of well-endowed think-tanks devoted to market fundamentalism. On the side of the self-adjusted school we find the orthodox economists, who are a vast majority, controlling nearly all economics departments; on the side of the sceptics are the heterodox economists, who are a majority in only a handful of departments, and who are otherwise spread out in various economics departments, social sciences faculties and business schools.

1.3.6 Ideology

Another way to express the fifth set of presuppositions is to talk of ideology. Bernard Guerrien (2009), a French mathematical economist, agrees with Lawson and other methodologists that orthodox theories lack realism. In fact, he goes somewhat further than Lawson, claiming that the mainstream benchmark model, the neo-Walrasian perfect competition model, is not only unrealistic; it is completely irrelevant because, as pointed out earlier, it relies on mechanisms akin to a planned centralized economy and not on the adjustment mechanisms of a market economy. Guerrien wonders how such unwarranted conflations can be made. His answer is that orthodox economists are driven by ideology. Because they have conservative leanings, they wish to show that market economies, devoid of frictions and imperfections, yield efficient outcomes, even if this requires absurd assumptions. This thesis is also espoused by Marglin (1984a, p.481), and no doubt by a large number of Marxian economists, who believe that ideology is the crucial element separating most orthodox economists from most heterodox economists.

Neoclassical theory is flexible enough, through the introduction of auxiliary hypotheses (externalities, imperfections, increasing returns, asymmetric information), to generate a large range of results so as to arrive at just about any sort of economic policy recommendation. New Keynesian economics is a good illustration of this phenomenon, and some orthodox dissenters, like Stiglitz, have clearly expressed that this was their academic strategy. This perhaps explains why many graduate students with left-wing inclinations do not object to being drilled through neoclassical economics. Thus the relationship between one’s ideology and one’s economic tradition may not be overly tight.

From a historical perspective, there is no doubt that neoclassical theory and hence today’s orthodox theories have a link with ideology. In the midst of the various revolutionary waves that were hitting Europe in the second half of the nineteenth century, the simultaneous appearance of marginalist works, breaking away from several of the classical concerns and concepts, provided a breath of fresh air for the threatened political and economic establishment. Furthermore, marginalism, as it was then called, offered an alternative to Marx’s extension of the classical school (De Vroey, 1975; Pasinetti, 1981, pp.11–14). Because Marx’s premises were similar to those of the classics on so many points, it was difficult to reject his analysis and his conclusions altogether. Getting rid of the classical theory of value and of the classical explanation of the origin of profit by embarking on the path of marginalism was the answer of the European bourgeoisie. With the advent of Marx it became imperative for the establishment, long annoyed with
some of the conclusions drawn from classical economics, to find a less class-conscious and more apologetic alternative.

The economists also embarked on the bandwagon, so that by the 1900s marginalism had swept over economics. There was a convergence in those days, perhaps still now, between the presuppositions and agenda of neoclassical economics and the interests of the political and industrial establishment, just as today there is an obvious convergence between economists and the financial establishment. As George Soros (2010, p. 86), the former speculator, says, ‘by far the most powerful force working in favor of market fundamentalism is that is serves the self-interests of owners and managers of capital’. Indeed, the debates around the economic policies that should be or should have been pursued during and before the Global Financial Crisis, especially the debates that have raged around the consequences of budget deficits, the composition of tax cuts or tax increases, and the need for financial regulation, have clearly shown that ideology is a key determinant of the theoretical positions being advanced. ‘Economists produce the sorts of knowledge that [their] patrons desire’ (Mirowski, 2011, p. 508).

Perhaps there is an even simpler explanation. As the movie Inside Job highlighted, economists, or at least some of them, are driven by money. The popularity and the adoption of economic theories may depend more on their potential monetary rewards than on the search for truth. No less an author than Paul Samuelson (2007, pp.ix–x) has suggested that some theories take over others on the basis of the strength of political winds and monetary rewards, writing that ‘what establishment economists brew up is as often what the Prince and the Public are already wanting to imbibe’. Samuelson thus reversed the causality invoked by Keynes (1973, vii, p. 383), when, in an oft-rehearsed quote, he argued that: ‘Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back.’ Samuelson believes instead that ‘madmen in authority can self-generate their own frenzies without needing help from either defunct or avant-garde economists’. This is confirmed by Paul Krugman (2013), who asserts that ‘the austerity agenda looks a lot like a simple expression of upper-class preferences, wrapped in a facade of academic rigor. What the top 1 percent wants becomes what economic science says we must do.’ Or perhaps this is an instance of two-way causality.

1.3.7 A Sixth Presupposition?

Could there be a sixth presupposition? Lawson, based on his view of critical realism, believes that orthodox economics is bankrupt precisely because it is using methods of formalism that are not adequate to study economic and social phenomena. For Lawson (2009d, p.340), ‘the reliance on formalistic models forces the mainstream on the path of irrelevance’. Earlier, he says that ‘the insistence that mathematical methods should be everywhere used is not only mainstream doctrine; it is the cause of the discipline’s continuing ills’ (ibid., p.109). Lawson claims that the unity and the stability of the orthodox school is based on this single edict – the requirement to put arguments in a mathematical–deductivist form. Thus, if we were to follow Lawson on this, we could argue that the requirement for formalism constitutes a sixth presupposition of orthodox economics, while ‘the feature that characterises heterodoxy qua heterodoxy
According to Lawson, heterodox schools of thought, in opposition to orthodox dissenters, would have arisen as a response to the failure of the formalistic methods in orthodox economics.

It is certainly tempting to follow Lawson on this formalization track. Indeed, it could be argued that neoclassical economics has gradually evinced non-neoclassical schools of thought from economics departments because most heterodox economists paid little attention to formalization. We know that the profession has become ever more technical and mathematized, preoccupied with ever more abstract and unrealistic assumptions, often devised to keep the mathematics tractable rather than be pertinent to the subject at hand. Skidelsky (2009, p.x) concurs, saying that he has ‘come to see economics as a fundamentally regressive discipline, its regressive nature disguised by increasingly sophisticated mathematics and statistics’. Excessive formalization, as recognized in the speeches of many former recipients of the Nobel Prize in economics, is a scourge in the field of economics. As Leijonhufvud (1973, p. 329) recalled long ago, the priestly caste of mathematical economists occupies the highest caste-ranking. Their skills are looked at in awe by their colleagues. Indeed, as recalled by Beed and Beed (1996), 20 per cent of surveyed economists claimed to be familiar with a fictitious Journal of Economic and Statistical Theory, and placed it nearly in the top quarter of all listed journals, no doubt because of its mathematical-sounding name. Mathematics used to add some rigour to verbal arguments; now it plays a paradigm-preserving role. Formalization forces the focus of attention away from the larger issues and towards minute details. Such is their importance that the graduate students of the departments run by the invisible college now consider that being good at problem-solving and excellence in mathematics are more important for a successful academic career than a knowledge of the economy and of the overall economics literature (Klamer and Colander, 1990, p. 18). No doubt the situation has not improved since this survey was done about 25 years ago.

How far do heterodox authors go, or how far should they go, in their distrust of mathematical methods? Sheila Dow (2000, p.164) writes that ‘the guiding principle of orthodox economics is mathematical formalism’. She seems to go too far when she adds that ‘heterodox paradigms share a rejection of mathematical formalism’. Indeed, Lawson (2009e, p. 190) himself denies that he is ‘opposed to mathematical formalism per se’. What he is opposed to is ‘the abuse of mathematical formalism’. Thus the above statements are not a definite indictment of formalization in economics under all circumstances. Formalism should not be an end in itself. Formal models may illuminate comprehension. They provide some discipline. Thus several post-Keynesians endorse ‘the cautious use of formal methods in economics’ (Fontana, 2009, p. 39) or argue that ‘formalism is fine, but it must know its place’ (Chick, 1998, p. 1868). Indeed, Keynes (1973, xiv, p.296) himself thought that ‘economics is a science of thinking in terms of models joined to the art of choosing models that are relevant to the contemporary world’.

All the heterodox schools of economics carry mathematically trained economists. Some heterodox economists have led the way in some research areas, such as non-linear and complexity dynamics. Several heterodox authors enjoy formalization, and some of them have regrouped under the informal term of ‘analytical political economy’ (Setterfield, 2003). The big difference, as Amitava Dutt (2003, p. 58) points out, is that the only mathematical modelling that is acceptable to both the mainstream and the orthodox
dissenters ultimately relies on ‘the use of the optimizing agent’ instead of ‘empirically-based behavioral relations’. It is the insistence on modelling and on this straitjacket of individual optimization that heterodox authors reject. In his latest work, despite his obvious annoyance with heterodox authors who make significant use of formalization, Lawson (2013, p. 8) seems to be most opposed to modelling based on isolated or atomistic optimizing individuals, which seems consistent with Dutt’s above comment.

Another defining dichotomy between orthodox and heterodox economics, a candidate for a seventh presupposition, has been put forth by Lawson (1997) and Dow (2000). Heterodoxy entails a belief in open systems, while orthodox economics deals with closed systems. I have never been able to convince myself of the usefulness of such a dichotomy, even though some famous writers seem to approve of it. The distinction has been used by Stephen Pratten (1996) to argue that Sraffian economics was based on a law-like closed system, and therefore out of heterodoxy, despite the well-known fact that the profit rate in the Sraffian system is left up in the air, with no definite determinant. Indeed, Mearman (2006, p. 69) argues that there is a considerable amount of realism in Sraffian economics. The closed/open system dichotomy has also been used by Downward (2000) to argue that target-return pricing is not a truly heterodox theory, because it yields a determinate pricing formula, despite its kinship with Kalecki’s or Gardiner Means’s ‘open’ pricing proposals. Such pronouncements seem so obviously misleading that their open/closed basis cannot provide much useful information, a point also made earlier by John Smithin (2004, pp. 67–70) and by Mearman (2006).

1.3.8 Some Additional Reflections on Presuppositions

In the previous section, I asked whether neo-Austrian economists ought to be counted as orthodox or heterodox economists. On the matter of formalization just discussed, neo-Austrian economists clearly side with heterodox schools of thought, since neo-Austrians are rather sceptical about formalization in economics. Undoubtedly a large number of neo-Austrian economists consider themselves as heterodox economists, as they feel ostracized by their mainstream colleagues because of their lack of access to top academic journals. Indeed some journals, such as the Review of Political Economy, are explicit in including neo-Austrians within the heterodox tradition; this journal has the following blurb on its inside cover: ‘The Review of Political Economy welcomes constructive and critical contributions in all areas of political economy, including the Post-Keynesian, Sraffian, Marxian, Austrian and Institutionalist traditions.’ Various neo-Austrian economists have underlined the similarities between Keynesian uncertainty and Knightian uncertainty, Frank Knight being associated with the neo-Austrian tradition. Lawson (1994, pp. 533–4) himself puts the neo-Austrians in the heterodox camp.

Notwithstanding all of this, it remains that if the listed five presuppositions allow us to distinguish orthodox from heterodox economics, neo-Austrians do not fare well on at least three of these suppositions, being much closer to the atomistic, scarcity and self-adjusting presuppositions. Furthermore, a famous originator of neo-Austrian economics, Ludwig von Mises (1976, p. 228) has been very clear about this:

we usually speak of the Austrian, the Anglo-American Schools and the School of Lausanne [the Walrasians] . . . These three schools of thought differ only in their mode of expressing the same
fundamental idea and... they are divided more by their terminology and by peculiarities of presentation than by substance in their teaching.

This is also Davidson’s (1989, p.469) assessment; he says that ‘the Austrians have neither logically differentiated themselves from the neoclassical approach, nor raised major problems in it’. Thus, all in all, I would say that it is best to consider neo-Austrian economists as orthodox dissenters. The same could be said about analytical Marxism (Wrenn, 2007, p.102).

The situation is quite different for most of the other schools of thought mentioned in Table 1.2. This can be ascertained by looking at the descriptions provided by some journals of some of the associations. We have already discussed the statement of the Review of Political Economy. How about the Cambridge Journal of Economics? Its cover says that ‘The Cambridge Journal of Economics, founded in the traditions of Marx, Keynes, Kalecki, Joan Robinson and Kaldor, welcomes contributions from heterodox economics as well as other social sciences disciplines.’ As for the Review of Radical Political Economics, its short announcement says that ‘the Review presents articles on radical political economy and applied economy from a wide variety of theoretical traditions – including Marxian, institutionalist, post-Keynesian and feminist’. Turning now to academic associations, perhaps the best example of an encompassing heterodox tradition is that of the European Association for Evolutionary Political Economy. On its website, when presenting its theoretical perspectives, the Association says: ‘The Association accepts the relevance of writers as diverse as John Commons, Nicholas Kaldor, Michal Kalecki, William Kapp, John Maynard Keynes, Alfred Marshall, Karl Marx, Gunnar Myrdal, Edith Penrose, François Perroux, Karl Polanyi, Joseph Schumpeter, Herbert Simon, Adam Smith, Thorstein Veblen and Max Weber to institutionalist and evolutionary thought.’

All of this goes to show that there are tight links between the first three heterodox schools of thought identified in Table 1.2: the Marxians, post-Keynesians and Institutionalists. Indeed, it is sometimes said that Institutionalism provides the micro-economics of post-Keynesian economics while post-Keynesianism provides the macro-economics of the Institutionalist School. From now on, we focus on the post-Keynesian School.

1.4 ESSENTIAL CHARACTERISTICS OF POST-KEYNESIAN ECONOMICS

1.4.1 A Very Short History of Post-Keynesian Economics

Before we move on examine what are the essential characteristics of post-Keynesian economics, that is, their own specific presuppositions relative to other heterodox traditions, we provide a very short history of post-Keynesian economics so as to give some background information to readers; afterwards, because post-Keynesians are not a homogeneous group, we present the various strands of post-Keynesian economics, hoping that this will help readers to find their way in the literature.

In his book on the History of Post Keynesian Economics, King (2002) believes that
post-Keynesian economics started in the 1930s, when Keynes was testing his new ideas about depression economics with members of the Cambridge Circus, notably Joan Robinson and Richard Kahn. At the same time, Kalecki was creating his own version of effective demand, Kaldor (1934a) was already discussing multiple equilibria and path-dependence, two crucial heterodox concepts, and Robinson (1937) had written a toned-down version of Keynes’s *General Theory*. It could also be tempting to say that post-Keynesianism started in 1956, when Kaldor and Robinson put forth what was then called the Cambridge, Anglo-Italian or neo-Keynesian theory of income distribution as an alternative to the neoclassical theory based on marginal productivity. For several years, this income distribution theory, based on effective demand, was the best-known contribution of these earlier post-Keynesians. Or it could be said that post-Keynesianism started in the 1960s, during the controversies on capital theory between the two Cambridges (in the UK and in Massachusetts), when Cambridge authors truly started to realize that they were putting forth a view of economic theory distinct from that of their neoclassical colleagues, even hoping that their critiques would destroy neoclassical theory (Mata, 2004). These key moments in the history of post-Keynesian economics are presented in Table 1.5, along with the main themes that were studied by post-Keynesians thereafter.

A somewhat different point of view is offered by Lee in his *History of Heterodox Economics*. Lee (2009, p. 11) argues that ‘the history of Post Keynesian economics does not begin in 1936 with Keynes and the publication of the *General Theory*, but in the 1970s with the formation of a community of self-identified Post Keynesian economists in the United States who in part drew upon his ideas’. Lee shows that there were substantial and successful efforts at creating a community of post-Keynesian economists throughout the 1970s, notably by the Americans Alfred Eichner, Edward Nell and Paul Davidson. Social networking occurred in particular when American scholars visited Cambridge and when Joan Robinson visited the USA in the early 1970s. Institutionalization of post-Keynesian economics thus started in the 1970s, in particular with the creation of their two main journals, the *Cambridge Journal of Economics* and the *Journal of Post Keynesian Economics*.

### Table 1.5 The evolution of the main themes of post-Keynesian economics

<table>
<thead>
<tr>
<th>Timeline</th>
<th>Main theme</th>
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<tbody>
<tr>
<td>1930s</td>
<td>Unemployment</td>
</tr>
<tr>
<td>1950s</td>
<td>Neo-Keynesian (Cambridge) models of growth and distribution</td>
</tr>
<tr>
<td>1960s</td>
<td>Capital controversies</td>
</tr>
<tr>
<td>1970s</td>
<td>Theory of the firm, pricing, employment theory, definition of a paradigm</td>
</tr>
<tr>
<td>1980s</td>
<td>Kaleckian models of growth, endogenous money, the financial fragility hypothesis</td>
</tr>
<tr>
<td>Late 1980s,</td>
<td>Attempts at (a grand) synthesis and textbooks</td>
</tr>
<tr>
<td>early 1990s</td>
<td></td>
</tr>
<tr>
<td>1990s</td>
<td>Methodology, critical realism, history of economic thought</td>
</tr>
<tr>
<td>2000s</td>
<td>Economic policy, globalization, financialization, empirical and econometric work, the links between monetary and fiscal policies, new attempts at synthesis</td>
</tr>
<tr>
<td>2010s</td>
<td>Stock–flow real-financial coherence, path-dependence, financial instability, ecological economics</td>
</tr>
</tbody>
</table>
Another major event at the time – one that had a profound influence on my own thinking – was the publication of the paper by Eichner and Kregel in the *Journal of Economic Literature*. There, they argued (perhaps with too much enthusiasm) that a new paradigm, that of post-Keynesian theory, was in the making, pointing out that post-Keynesian economics was much more than a negative critique of the neoclassical theory of capital, and that it was providing a strong positive and original contribution to the study of economics. They contended, in reference to our first heterodox presupposition, that the purpose of post-Keynesian theory ‘is to explain the real world as observed empirically’ and not ‘to demonstrate the social optimality if the real world were to resemble the model’ (Eichner and Kregel, 1975, p. 1309). This paper came out just as I was discovering, as an undergraduate, the existence of this alternative theory, and it had a strong impact on my views as I moved on with graduate studies and my early academic career. An interesting feature of the Eichner and Kregel article is that they provided alternatives on methodology and macroeconomics, but also in the field of microeconomics. Studies of the firm and of pricing procedures, as well as models of employment, were a key feature of the 1970s.

The 1980s saw the appearance of the stalwart of post-Keynesian modelling, the Kaleckian model of growth and distribution, which replaced the neo-Keynesian model of growth and distribution, as we shall see in Chapter 6. Monetary economics, with the hypothesis of endogenous money, as well as financial economics, with the development of the financial fragility hypothesis, were also in the forefront in the 1980s. Starting at the end of the 1980s, for about a decade several authors attempted to provide a textbook view of post-Keynesian economics (Reynolds, 1987; Eichner, 1987; Arestis, 1992; Carvalho, 1992; Lavoie, 1992b; Palley, 1996a; Nell, 1998). These efforts at creating a grand theoretical synthesis led Giuseppe Fontana and Bill Gerrard (2006) to refer to this period as the ‘Romantic Age’ of post-Keynesian economics in their short history of post-Keynesian economics.

This then gave rise, again according to Fontana and Gerrard, to the Age of Uncertainty, as the attention of post-Keynesians turned toward methodological issues, spearheaded by Lawson’s fray into ontology and critical realism, as already discussed, as well as a re-examination of Keynes’s writing and an analysis of his methodological views. This was accompanied by a renewed interest in the history of economic thought, as if several post-Keynesians and other heterodox economics feared to engage in policy debates and sought refuge from the attacks of their mainstream colleagues. Indeed, this led Fontana and Gerrard (2006, p. 69) to comment that ‘Post Keynesian economists are seen to be more concerned with critique, methodology and the history of economic thought rather than the development of economic theory per se’. Indeed, an even harsher external appraisal was mentioned by Stephen Dunn (2000, p. 343): ‘Post Keynesianism is commonly referred to as a disintegrating research program, more obsessed with the exegesis of the ideas of long dead economists than it is concerned with explaining new, novel empirical facts or contributing to the development of new policy.’

But, as can be clearly ascertained when participating to various post-Keynesian conferences throughout the world over the last 15 years or so, this has given way in the 2000s to a concern for policy relevance, at both the domestic and global level, accompanied by a revived interest in empirical studies and in applied econometrics, no doubt helped by the appearance of novel agnostic time-series methods, despite the methodological concerns of post-Keynesians about econometrics. Much has been written on monetary and fiscal
policies, the links between monetary and fiscal operations through what has been called ‘modern monetary theory’ on the Web, and on globalization and financialization. There has been a clear shift from abstract methodological concerns towards more concrete ones. In other words, post-Keynesian economics today is not the same as it was 20 years ago, and it is not always clear that some of its critics are fully aware of this evolution.

Over recent years, tied in particular to the Global Financial Crisis, there has been renewed interest in the financial fragility hypothesis, in an attempt to integrate in a meaningful manner the analysis of the real and the financial sectors, particularly through what has been called the stock–flow consistent method, which will be discussed in Chapter 4. Post-Keynesians have also started to address the crucial environmental issue, linking up timidly with ecological concerns. This, in a reduced form compared to the story told in Lavoie (2011a), is my take on the themes that have marked the evolution of post-Keynesian theory, fully recognizing that this is a bird’s-eye-view nomenclature, one based on readings, recollection and participation at conferences. A rather similar picture is offered by Fontana (2009, ch. 2).

1.4.2 The Presuppositions of Post-Keynesian Economics

A difficult question, which keeps haunting post-Keynesian economists, is: what are the contours of post-Keynesianism? A recurrent question is whether the Sraffian School should or should not be included within post-Keynesianism. We will come back to this question in the next subsection. But there are other contour questions, which are just as mesmerizing. Should we say that the French Regulation School is part of the post-Keynesian School, since Regulation authors use so many behavioural equations taken from Cambridge Keynesians, or should we say instead that post-Keynesianism is part of the Regulation School, since the latter takes a wider historical and institutional approach to analysing economic systems? Furthermore, what are the links between Radical Marxians and post-Keynesians? Lee (2009) shows indeed that there were warm working relationships between the two schools from the very beginning. Is it that the Radicals focus on cycles while post-Keynesians of the Cambridge variety have focused on trend growth? There are no easy answers.

There have been many proposals identifying the key characteristics of post-Keynesian economics since the first attempt by Eichner and Kregel (1975). A survey of these attempts yields Table 1.6.

The presuppositions of post-Keynesian analysis can be divided into four groupings. The first five characteristics of Table 1.6 (realism, organicism, reasonable rationality, production, disequilibria and instability) correspond to what I have called the presuppositions of the heterodox research programme. This is not surprising since post-Keynesian economics is part of heterodox economics, and hence, as such, should share its presuppositions. For instance, dealing with realism, in a statement that applies just as well, and most likely much more, today as it did 40 years ago, Minsky (1975, p. 4) complained that ‘academic economics has recaptured much of the sterility and irrelevance with respect to the operation of the real-world economy which characterized the discipline before the appearance of The General Theory’. The only characteristic in need of some explanation is the fifth one, which in Table 1.3 was described as ‘regulated markets’ and which here is presented as ‘disequilibria and instability’. Post-Keynesian authors often consider that
there are endogenous destabilizing forces at work and that price mechanisms cannot in
general counteract these. As a result, multiple equilibria may arise, including of course
financial crises and situations of unemployment, so that government intervention and
the regulation of market forces are required.

About government intervention, a few more words may be required. I have already
pointed out that post-Keynesians in general do not wish to eliminate capitalism; they
wish to tame it, recognizing that it has important dynamic properties. While no doubt

Table 1.6  Presuppositions and key characteristics of post-Keynesian economics

<table>
<thead>
<tr>
<th>Concept</th>
<th>Authors endorsing the concept</th>
</tr>
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<tbody>
<tr>
<td>Non-ergodicity</td>
<td>Dow (2005), Dunn (2008), Pasinetti (2007)</td>
</tr>
<tr>
<td>Specific microeconomics</td>
<td>Eichner and Kregel (1975), Lavoie (2006b), Norman (2008), Sawyer (2010)</td>
</tr>
<tr>
<td>Income distribution</td>
<td>Eichner and Kregel (1975), Norman (2008), Sawyer (2011)</td>
</tr>
<tr>
<td>Pluralism</td>
<td>Dow (1991, 2005), Lavoie (2006b)</td>
</tr>
</tbody>
</table>
post-Keynesians do not hold homogeneous political views, my assessment is that the range of these views has tightened over the last two decades. Most post-Keynesians see some sort of alienation in both liberalism and socialism, and thus look forward to a ‘humanistic’ political system, which would constitute some ‘middle way between liberalism and socialism’ (Bortis, 1997, p. 33), as used to be found in Scandinavian countries, which, ironically, carry extremely few post-Keynesians in their ranks.

The next set of presuppositions is more specific to post-Keynesian economics: it is made up of three inter-related features: the principle of effective demand; the causality running from investment to saving; and the claim that institutions are important and make a difference. The principle of effective demand says that aggregate demand is the main force that determines output and employment. But while most economists would agree or concede that the economy is demand-led in the short run, few would agree with the claim that the economy is demand-led even in the long run, and thus the assertion that the economy is demand-led both in the short and the long run is most likely a specific feature of post-Keynesianism. More concretely, this means that post-Keynesians believe that the actual path taken by the economy has an impact on the supply-side determinants of long-run growth.

The statement that investment determines saving is intimately linked to the principle of effective demand, and thus it is no surprise to see some authors underlining this causality as a key feature of post-Keynesianism. Indeed, as pointed out early on by Nina Shapiro (1977), the autonomy of investment from the intertemporal decisions of households is most likely the revolutionary feature of post-Keynesian economics. This is even clearer now than it was then, with the focus of state-of-the-art orthodox macroeconomics on the representative agent and her maximization of intertemporal utility. Also connected to all this is the belief that institutions make a difference, meaning within the narrower context of the principle of effective demand that fiscal and monetary policies have an impact on real quantities, in both the short and the long run.

The third set of presuppositions might also be specific to post-Keynesianism. It brings together a monetized economy, the importance of historical and irreversible time, fundamental uncertainty and the concept of non-ergodicity. Once again it is tricky to disentangle these different notions. The idea of a monetized economy could also be associated with the principle of effective demand since it is difficult to imagine an independent investment function without a monetized economy. In orthodox state-of-the-art macro models, such as the DSGE approach, there is no need for nominal magnitudes, nor for money. Some commodity acts as a numéraire or unit of account. As in neo-Walrasian models, everything is known until the end of time, with some probabilistic degree; in other words there is risk and no fundamental uncertainty. Time in such models is an artificial construct, since all decisions are taken on day zero. The introduction of nominal magnitudes and money adds friction and reduces welfare – a result that contradicts intuition and shows how counterfactual these models really are (Rogers, 1989; 2011). In post-Keynesian models fundamental uncertainty is assumed from the start by considering that contracts, debts and assets are denominated in money terms, and by rejecting the possibility of proceeding to the maximization of intertemporal utility. Indeed, any model that rejects these state-of-the-art constructs integrates in some manner the concept of fundamental uncertainty, where, as Eichner and Kregel (1975, p. 1309) say, ‘only the past is known, the future is uncertain’.
Post-Keynesians take the notion of time very seriously, making the distinction between historical time and logical time (Robinson, 1980). Historical time, or chronological time, is irreversible, in contrast to logical time. Post-Keynesians are very much influenced by Kalecki’s (1971, p. 165) statement to the effect that ‘the long-run trend is but a slowly changing component of a chain of short-period situations; it has no independent entity’. For Robinson, everything occurs in the short period. In their debates with their critics, post-Keynesians have underlined the need to consider and describe the transition from one position to another, recognizing that the conditions under which this transition occurs may affect the final position of equilibrium. As Halevi and Kriesler (1991, p. 86) claim, long-period analysis in logical time is relevant only when ‘some coherent dynamic adjustment process is specified which can describe the traverse from one equilibrium position to another, without the traverse itself influencing the final equilibrium position, that is, without the equilibrium being path determined’. Thus post-Keynesians consider path-dependence and hysteresis phenomena as typical of their vision of economic phenomena set in historical time. As Robinson (1956, p. 58) put it, ‘in most economic reactions the path the market follows, while it is adapting itself to a change, has a long-persisting effect upon the position that it reaches’. It may be that not all history matters, but certainly some does.

The importance of time is also related to the notion of non-ergodicity put forth by Davidson (1982–83), meaning that the time and space averages may not coincide, implying that we cannot rely on current or past averages to discover what ought to happen in the future. This concept has certainly attracted some attention with the subprime financial crisis, as it is associated with black swans and fat tails, as well as large switches in expectations and confidence, leaving little room for the empirical worthiness of rational expectations and the efficient-market hypothesis.

We end this subsection with a discussion of the elements constituting the fourth set of key characteristics of post-Keynesian economics. These are a specific microeconomic theory, power relations, income distribution, open system modelling and pluralism. Obviously, judging by the number of supporting authors, these elements do not carry as much agreement as the presuppositions listed earlier. We shall say more about this specific microeconomics in Chapters 2 and 3, in particular when discussing the theory of the firm. The importance of power, for firms, and in explaining economic activity and issues related to income distribution is certainly not a feature unique to post-Keynesian economics. It figures prominently in both Institutional economics and Marxian economics. Still, functional income distribution was at the heart of post-Keynesian economics from the very beginning in the 1950s, and post-Keynesians very early on attributed the advent of the Global Financial Crisis to changes in income distribution – the increase in the profit share and the unequal distribution of the wage share. Post-Keynesians have indeed formalized previous concerns of under-consumptionists regarding the effect of unequal income distribution on aggregate demand.

I have already expressed my uneasiness regarding the relevance of the open system modelling condition. As to preoccupation for pluralism, whatever its merit, it does not appear as highly specific to post-Keynesian economics. It may have been the case in the past that pluralism – the Babylonian approach, as Dow (2005) calls it – could be specifically associated to the post-Keynesian school because it was one of the few schools of thought that was offering a ‘broad tent’ approach, accepting a variety of methods and
theories as being scientific, but this certainly is no longer the case, as the idea of pluralism has been picked up by methodologists of all horizons.

To summarize so far, post-Keynesians embrace all five presuppositions associated with heterodox economics. In addition, post-Keynesianism can be distinguished from other heterodox schools by its focus on the principle of effective demand, in both the short and the long run, the study of a monetized production economy, an environment of fundamental uncertainty, and the insistence that time is historical and irreversible.

The place of econometrics
One issue has been left aside, that of the place of econometrics in post-Keynesian economics. We discussed modelling when dealing with the issue of mathematical formalism and the presuppositions of heterodox economics, but what about econometrics? Here again it is convenient to refer to Lawson’s position. Lawson argues that the nature of the phenomena to be studied in economics is rarely appropriate for the use of standard econometric methods. There are not enough event regularities in the real world for these techniques to be useful. The use of econometrics is not justified except in very specific conditions. One branch of post-Keynesian economists tends to agree with this assessment – the Fundamentalist strand, as we call it in the next subsection – and Keynes (1973, xiv, p. 320) himself was rather pessimistic about the use of econometrics in macroeconomics, going so far as to talk of ‘statistical alchemy’. However, I think it is fair to say that, in general, post-Keynesians have embraced econometric methods, especially since 2000, although, from the very beginning, there have always been post-Keynesians who ‘defend the use of econometrics vigorously’ (Norman, 2008, p. 2), the best and earliest example, perhaps, being Kalecki.

As heterodox economists, and hence as naturally doubtful of the truth of any statement, post-Keynesians are keenly aware that a limited number of econometric results is robust and can be replicated, either because the stability of the calculated parameters is in question or because of excessive data-mining. They are also fully aware of the difficulties involved in using past econometric results to provide good predictions. ‘While Post Keynesians should be skeptical about prediction, this does not mean that they should abandon all empirical work’ (Holt, 2007, p. 101). As recalled by Bill Mitchell (2007), policy-makers require hard numbers for policy-making, and these can be provided either by back-of-the-envelope calculations or by econometrics. In addition, econometric results can offer a focus for thinking about a problem and for discussion. Econometric analysis, as a subset of empirical analysis, gives further ammunition in the heterodox quest for explanation and causal mechanisms – the two are certainly not incompatible. Another important and pragmatic argument is that econometrics is a powerful weapon in the battle of ideas. Economics is all about rhetoric, as Deirdre McCloskey (1983) has long argued, and econometrics is a potent rhetorical tool. Heterodox economists have to play this game, show their technical dexterity, even if empirical evidence rarely leads to changes in the dominant view (Mearman, 2012b).

Finally, as argued by Downward and Mearman (2002) and by Bill Gerrard (2002), there are econometric methods that are more congenial to the post-Keynesian view. The claim has been made that the David Hendry/LSE method, starting from the statistical properties of the time series rather than imposing a structure to start with, characterized by an interaction between the data and theory, is compatible with a heterodox perception
of realism. This is in particular the view of Katarina Juselius (2011), who argues that general-to-specific cointegrated vector autoregression (CVAR) reflects post-Keynesian concerns about the non-stationarity of data and the importance of short-run dynamics. Similarly, Gilberto Libânio (2006) contends that the statistical finding that most time series in macroeconomics have unit roots and are non-stationary reinforces the post-Keynesian belief that an equilibrium position cannot be independent of the path taken to get there, and thus that historical time matters. It follows that the econometric techniques that take these statistical properties into consideration ought to be more adequate from the standpoint of post-Keynesian methodology.

1.4.3 Strands of Post-Keynesian Economics

All post-Keynesians were not created alike. As of now, it is convenient to identify five strands within post-Keynesian economics: the Fundamentalists, the Sraffians, the Kaleckians, the Institutionalists and the Kaldorians, as described in the last column of Table 1.7.

However, the best-known description of post-Keynesian economists is that of Hamouda and Harcourt (1988), also found in Table 1.7, who identify only three strands: the American post-Keynesians and the Sraffians at the two extremes, and the Kaleckians somewhere in the middle. The American post-Keynesians have elsewhere been called the Marshallian post-Keynesians (Arestis and Sawyer, 1993; Arestis, 1996), the Fundamentalist post-Keynesians (Lavoie, 2006b) or Keynes’s School (Davidson, 1982). The main concerns of these authors are the descriptions of a monetized production economy, the fragility and instability of the financial system, questions tied to liquidity preference and fundamental uncertainty. As a result, they have also been referred to as Financial Keynesians or Monetary Keynesians (Fontana, 2009), which is probably a kinder denomination. The Fundamentalist post-Keynesians are those that most object to formalization or to the use of econometrics. An important concern of several of these authors is to amplify the true and fundamental meaning of Keynes’s writings, and as a result their microeconomics (with the assumption of decreasing returns and pure (but not perfect) competition) is often tied to that of Alfred Marshall, of whom Keynes was a student, which explains why they are also called Marshallian post-Keynesians.

This branch is also named ‘American’ because its initial proponents – Sydney Weintraub, Paul Davidson, Hyman Minsky, Victoria Chick, Basil Moore – all came from the USA, although many supporters of this branch can now be found all over the world.

Table 1.7 The various strands of post-Keynesianism

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<tbody>
<tr>
<td>American PK</td>
<td>Marshallians</td>
<td>Marshallians</td>
<td>Fundamentalists</td>
</tr>
<tr>
<td>Kaleckians</td>
<td>Kaleckians</td>
<td>Robinsonians</td>
<td>Kaleckians</td>
</tr>
<tr>
<td>Sraffians</td>
<td>Sraffians</td>
<td>(Sraffians)</td>
<td>Sraffians</td>
</tr>
<tr>
<td></td>
<td>Institutionalists</td>
<td>Institutionalists</td>
<td>Institutionals</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Kaldorians</td>
</tr>
</tbody>
</table>
Most work on post-Keynesian methodology is also associated with this branch, since a substantial amount of this work has been devoted to Keynes’s writings on methodology and probability theory. The Italo-French monetary circuit theory, associated with authors such as Augusto Graziani and Alain Parguez, is closest to this strand, although it also has some strong Kaleckian components, so much so that Arestis (1996) includes the monetary circuit school in the Kaleckian strand, which we now discuss.

The Kaleckians are made up of authors such as Michał Kalecki, Joan Robinson, Joseph Steindl and Tom Asimakopulos, along with modern authors such as Amit Bhaduri and Malcolm Sawyer, to whom we could add younger authors such as Amitava Dutt, Eckhard Hein, Engelbert Stockhammer, Robert Blecker and Steve Fazzari, the latter two having been students of Donald Harris. All these authors have been mainly concerned with output and employment, like the American post-Keynesians, but also business cycles, growth theory and pricing issues, in particular the link between mark-ups and growth, and hence income distribution. The potential conflicts regarding income distribution are an important object of analysis. Another major concern is that of the realization of profit, using here Marxian terms. Arestis (1996) calls this group the Robinsonians, adding to them the monetary circuit school, as pointed out earlier, thus supplementing the Kaleckian strand with a more obvious monetary element, which can also be found in the work of Jan Toporowski (2000). I have also added Lance Taylor to the Kaleckians, although he is closely associated with the Development Structuralists of Table 1.2, because many of his models are indeed Kaleckian.

The place of Sraffians

The third strand described by Hamouda and Harcourt (1988) is that of the Sraffians – the followers of Piero Sraffa. They used to be called neo-Ricardians, somewhat ironically, because of Sraffa’s resolve to solve the puzzles left by Ricardo (and indirectly by Marx), such as an invariable anchor of value, so that Dutt and Amadeo (1990) name them neo-Ricardian Keynesians. Many of the better-known Sraffians are Italians – Pierangelo Garegnani, Luigi Pasinetti, Alessandro Roncaglia, Neri Salvadori – but others are not, such as Ian Steedman, John Eatwell, Bertram Schefold and Heinz Kurz. Sraffians were very much concerned with the determination of relative prices, the choices of techniques, joint production and the interdependence inherent to the existence of a multisectoral production system, as in input–output analysis. These issues, according to Garegnani (1990b, p.123), constituted the core of Sraffian theory, because he thought that definite answers could be offered. Questions related to output and employment, capacity and capacity utilization, or to money and interest rates were out of the core, although this did not mean that they were unimportant or not worthy of study. Indeed, modern Sraffians are now mostly dealing with these out-of-core issues.

Several post-Keynesian methodologists, for instance Stephen Dunn (2000, p.350; 2008, p.45), argue that Sraffians ought to be dropped from the post-Keynesian School, on the grounds that Sraffian economics does not fit the strictures of critical realism, and thus the inclusion of this strand into post-Keynesianism creates methodological confusion. Sending off the Sraffians, in their view, would help to bring more coherence to post-Keynesian economics. Even John King (2012b, p.314) believes that ‘almost no one today regards “Post Keynesian–Sraffian” economics as a single coherent school of thought’. I am among the few who still see some coherence, as do a number of researchers such as
Post-Keynesian economics

Harcourt (2001a, p. 275), Andrew Trigg (2008) and Gary Mongiovi (2012), and as I have tried to explain in detail (Lavoie, 1992a; 2011b).

First, as we saw in the previous subsection, Sraffians are intimately linked with post-Keynesian analysis by tradition and by history. Second, Sraffians are in close agreement with other post-Keynesians on crucial issues such as the causality between investment and saving, the role of effective demand in both the short and the long run, the endogeneity of the money supply and the possibility for the central bank to set short-run interest rates at levels of their choice (Dutt and Amadeo, 1990). Third, Sraffian views are not homogeneous and have evolved through time, so that the distinctions between the Sraffians and the other strands may no longer be so obvious. Modern Sraffians do not assume that the economy is always running at normal or full capacity. Many of them do not even assume that the economy is running at normal capacity in the long run. Fourth, the Sraffians provide equations that explain production and distribution in an interdependent setting, within a multisectoral framework, something that is generally lacking in the other strands. Sraffian price theory can be seen as an idealized administered pricing theory, a specific kind of benchmark pricing (Nell, 1998), which abstracts from imperfect information, past disequilibria, non-uniform profit rates or target rates of return, debt structures and so on. Those who are interested in the study of relative prices can add these complications at will. Fifth, Sraffians have made contributions to monetary analysis. The Sraffians were the first to claim that relative prices and real wages are affected by the trend level of the rate of interest, through its proportional impact on the normal profit rate, that is, the target rate of return embedded in the pricing mark-up.

There is also some wide agreement about policy issues and the need for government intervention. Fontana and Gerrard (2006, p. 51) present what they call the ‘three interconnected characteristic Keynesian propositions’: there can be involuntary unemployment; output and employment variations play the key role in macro adjustments; economic policy is effective and will stabilize the economy. Certainly, Sraffians agree with all three of these key Keynesian propositions, in part because, like other post-Keynesians, they see the problem of output determination as being separate from price determination (Bhaduri, 2011a, p. 95). Sraffians would also feel at ease with the three components of Keynesianism as defined by Palley et al. (2012, p. 3): ‘(1) holding that output and employment are normally constrained by aggregate demand; (2) holding that the problematic of aggregate demand shortage exists independently of price, nominal wage, and nominal interest rate rigidities; and (3) rejecting the claim that the real wage is equal to the marginal disutility of labor’.

For all these reasons the Sraffians are still present in my taxonomy (the last column of Table 1.7) and in that of Arestis and Sawyer (1993). They seem to be absent from the three-way taxonomy presented by Arestis (1996), but when he comes to the discussion of pricing, Arestis reintroduces Leontief, Sraffa and Pasinetti, that is, the Sraffians.

Two additional strands
As can be seen from Table 1.7, other taxonomies add a fourth strand, that of the Institutionals. Arestis (1996) and Arestis and Sawyer (1993) give few examples of what authors they have in mind when they identify the Institutionalist post-Keynesians. They mention only Thorstein Veblen, and cite Hodgson’s (1988) book, thus probably believing at the time that the Institutionalist tradition could reinforce the microeconomic
analysis of post-Keynesians. Since John Kenneth Galbraith was the patron of the Journal of Post Keynesian Economics, I believe that it would be fair to present him as a main representative of the Institutionalist strand of post-Keynesianism, along with his son James Galbraith (2008). Within this strand one could include some works of the French Convention School, which deals with habits and routines. Some authors associate Minsky with an Institutionalist post-Keynesian school (O’Hara, 2007a; 20007b; Whalen, 2013).

Within the Institutionalist post-Keynesian branch, one could also include some of the more heterodox work in behavioural economics or psychological economics, some of which has been pursued or endorsed by post-Keynesians (Earl, 1986; Harvey, 1998; Fontana and Gerrard, 2004). There is also a substantial amount of work, linked to industrial organization, which examines the evolution of corporations in light of the financialization process and the development of new information and communication technologies. This work is at the juncture of the Marxian, Institutionalist and Regulation school traditions, and post-Keynesians certainly belong to this appraisal. Furthermore, the whole administered pricing literature – associated with Means, Andrews and Brunner, Kaplan and Lanzillotti – has been adopted by post-Keynesian authors such as Eichner (1976) and Lee (1998). Finally, the whole movement of the neo-chartalist school, as found in Wray (1998), can be considered as part of the Institutionalist post-Keynesians, since the neo-chartalists base their policy recommendations on a detailed analysis of monetary institutions and implementation procedures (Fullwiler, 2003).

Hamouda and Harcourt (1988) wonder where, within their three-way classification, they should put authors such as Kaldor, Godley and Goodwin. One way out, suggested in Table 1.7, is to add a fifth strand, that of the Kaldorian post-Keynesians. This strand is mostly concerned with the constraints arising from open economy considerations, such as the balance of payments constraints or the fundamental identity that links private financial saving, public deficit and the current account balance. In the 1970s, this strand became known as the New Cambridge School. In addition, one can certainly draw a filiation from Kaldor, Harrod and Godley towards the work pursued by John McCombie and Anthony Thirlwall (1994) on these open economy issues, and their empirical work in return has inspired quite a large following. Furthermore, Kaldor’s technical progress function and his empirical work on manufacturing growth and endogenous growth has generated a stream of research devoted to productivity regimes, which involved again McCombie and Thirlwall, authors such as Ro Naastepad and Servaas Storm, and also the French regulationists Robert Boyer and Pascal Petit. Indeed, as shown by Boyer (2011), the French regulationists cover both the Kaldorian and the Institutionalist strands of post-Keynesianism. One can also claim that the work being pursued on multiple equilibria, instability, path-dependence and hysteresis, initiated by Kaldor in the 1930s and 1940s and continued in the 1970s by John Cornwall (1972) and then in the 1990s by his student Mark Setterfield, is in this Kaldorian tradition, as is work on cumulative causation. I would also include Richard Goodwin within this broad Kaldorian strand, along with his student Vela Velupillai. Both Kaldor and Goodwin constructed growth models where the rate of capacity utilization is assumed to be equal to its normal rate, a tradition pursued by modern post-Keynesians such as Peter Skott and Tom Palley. Indeed, there is a truly huge literature on variations around the Kaldorian growth model (Baranzini and Mirante, 2013).

It should be made clear that the identification of these various strands is only indica-
tive. Many eclectic and productive economists go across all or at least two of the categories discussed above, and so could not fit neatly into one of the strands. This is the case of key senior authors such as Philip Arestis, Geoff Harcourt, John King, Barkley Rosser Jr and Edward Nell, or more junior ones like Steve Keen, Mathew Forstater, Mathias Vernengo and Louis-Philippe Rochon. Several young post-Keynesians feel at ease within all strands, taking the best elements from each. Some also look for cross-fertilization with other heterodox traditions. However, Table 1.8 attempts to recapitulate the major themes tackled by each strand, as well as the authors who have inspired the current authors working within the strand, establishing a kind of who’s who of post-Keynesian economics, although inevitably omitting some important contributors.

1.4.4 Controversies over the Definition of Post-Keynesian Economics

Narrow-tent versus broad-tent approach

One topic that has not yet been discussed is what spelling should be adopted, with or without the hyphen: ‘post-Keynesian’ or ‘Post Keynesian’, as in the Journal of Post Keynesian Economics. As recalled by King (2002, p.9), the spelling advocated here, with a hyphen, was used by Kaldor and Robinson as early as 1956 and 1959. As mentioned earlier, the term ‘post-Keynesian’ to designate Cambridge Keynesians was then in competition with the term ‘neo-Keynesian’, which was used by Davidson (1972) among many others. Robinson, however, thought that the latter expression best applied to neoclassical Keynesians (Samuelson, Solow, Hicks, Tobin), so that by the late 1960s and early 1970s, as noted by Lee (2009, pp. 81–2), both Robinson and Eichner were promoting the use of ‘post-Keynesian’ to define Cambridge Keynesianism, an expression that was picked up by Kregel (1973) and Eichner and Kregel (1975), as well as most UK writers. ‘Post Keynesian’ without the hyphen was proposed by Davidson and Weintraub (1978) when founding their journal. Their motive was to come up with something that would be broader and more consensual than Cambridge Keynesianism, which at the time was associated with a left-wing view of the world, closest to the Sraffian and Kaleckian strands of the research programme, which, rightly or wrongly, were also accused of not paying enough attention to monetary economics. As a result the spelling without the hyphen was adopted by a large number of economists.

Strangely enough, the tables have recently been turned. The long-time editor of the Journal of Post Keynesian Economics, Paul Davidson (2003–04) has recently expressed some frustration over his unsuccessful efforts to convince his mainstream colleagues of the worth of what he calls Post Keynesian economics, blaming this failure on the fact that Post Keynesian economics is perceived as being incoherent because it incorporates too many different views. Davidson now wishes to redefine Post Keynesian economics, in the hope of stopping the marginalization of his ideas, by restricting its meaning to Fundamentalist Keynesianism (only the first strand of Table 1.7), eliminating authors such as Eichner, Minsky, the Sraffians and all Kaleckians in the process. By doing so, Davidson (ibid., p.262) wishes to avoid what he calls a ‘Babylonian incoherent babble’, or a Tower of Babel. The ‘true Post Keynesian school’ that Davidson (ibid., p.258) refers to is what he used to call Keynes’s school: ‘Fundamental Keynesianism – what I would call Post Keynesianism – is based on throwing over the same classical axioms that Keynes discarded in the general theory’ (ibid., p.263). Ironically, Dutt and
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<th>Major themes</th>
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<td>J.M. Keynes</td>
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<td>Monetized production economy</td>
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<td>Financial instability</td>
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<td><strong>Kaleckians</strong></td>
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Amadeo (1990) had perceived this change of mind more than 20 years ago, since they assimilated Fundamentalist Keynesianism to the ‘Post Keynesian’ spelling. Thus we have gone full circle. The relevant taxonomy today seems to be that ‘Post Keynesian’ is a narrow-tent designation, while ‘post-Keynesian’ covers a broader tent. And this explains the use of the ‘post-Keynesian’ spelling in this book, a spelling also adopted by Harcourt (2012).

Personally, when analysing the contours of the post-Keynesian school, I tend to favour a ‘broad-tent’ approach. I am a ‘lumper’ more than a ‘splitter’, to use the expressions proposed by Mearman (2009), or, in the words of King (2002, p. 214), a ‘synthesizer’. To some extent, it is in the nature of heterodox economists to be critical rather than constructive; this is why they have decided to reject mainstream economics. This may explain why a number of heterodox authors are busily criticizing each other or the mainstream, or even dead authors. Pasinetti (2007, pp. 38–9) believes that the failure of post-Keynesian economics to have had a large impact on economics can be explained in part by this personal feature of the founding members of Cambridge Keynesianism. Pasinetti observes that too many post-Keynesians have been arguing with each other, ‘disputing over who had which particular idea first’. Cambridge economists in particular did not spend much time trying to build bridges between themselves. Each one of them was too jealous of his or her intellectual independence. He also notes that too many of them showed a doctrinaire attitude, declining to enter into fruitful discussions with those holding slightly different views. ‘What is not at all helpful is that economists following these different strands or approaches have so often attacked one another, stressing many times, even to the extreme, their differences and overlooking or rather refusing to investigate what they have in common’ (Pasinetti, 2007, p. 46).

This, however, should not deter us from trying to link together contributions that differ but which were made in the same spirit. It is our task to generalize them and find extensions. One of the objectives of this book is to show that a synthesis of the various streams of post-Keynesian economics is possible. It is acknowledged that some of the contributions cannot be easily integrated, or that some of the authors may make strange bedfellows, although my position differs from that of Hamouda and Harcourt (1988), who believe that a search for a coherent vision is a futile endeavour, thus adopting a ‘horses for courses’ attitude. Rather than following the idiosyncrasies of one or the other, this book presents the views of a sort of representative post-Keynesian, taking what I believe to be the strongest contributions of each strand.

My position is closer to that of Eichner and Kregel (1975), who called for the adoption of a new paradigm unifying the main Sraffian and post-Keynesian concepts. Robinson, even after she had denied the importance of the capital controversies, also argued that the task of post-Keynesians was to reconcile Keynes and Sraffa, claiming that post-Keynesian theory had ‘a general framework of long- and short-period analysis’ that enabled it ‘to bring the insights of Marx, Keynes, and Kalecki into coherent form’ (Robinson, 1978, pp. 14, 18). My views are aligned with those of Arestis (1996, pp. 129–30), who concluded his survey on post-Keynesian economics by arguing that it ‘draws on a body of method and theory which represents a consistent way of analysing economic phenomena’, adding that post-Keynesian economics ‘has now reached the stage of constituting a positive approach characterised by internal coherence’. Such a claim may, however, require relinquishing the most extreme views that cannot be entertained within
the synthesis, however fundamental these views seem to be from the point of view of their proponents.

Let me illustrate all this. In his article on the definition of post-Keynesian economics, Davidson (2003–04, pp. 254–5) does not understand why colleagues like Minsky decline to make use of the aggregate demand–aggregate supply apparatus found in Keynes’s *General Theory*, developed by Weintraub and later retrieved by Davidson himself. The reason, I submit, is that most post-Keynesians felt uncomfortable with this apparatus. Keynes’s treatment of price theory is deemed by many to be too closely associated with neoclassical views to be kept within a synthesis. Indeed, the endless debates about the appropriate representation of Keynes’s aggregate supply function, or what has become the Z function, as well as the recursive and inconclusive debates about Keynes’s classical postulates regarding the determination of employment, demonstrate that the adoption of neoclassical core assumptions within post-Keynesian economics leads only to sterile controversies, even if these assumptions are turned on their head. Indeed, the confusion about Keynes’s aggregate supply function has been so great over the years that the editors of the *Cambridge Journal of Economics* have felt it necessary to issue a statement to the effect that they wished ‘to discourage further submissions of comments on the Z function’ (Editors, 2011, p. 635).

**Keynes versus Kalecki**

A number of observers have complained that heterodox economists, including post-Keynesians, tend to avoid questioning founding contributors, acting as self-appointed Guardians of the Temple, who are defending the true faith and the holy scriptures of their favourite authors. If Keynes did not abandon, but only modified, the quantity theory in his *General Theory*, thus being overly monetarist for contemporary post-Keynesians, as noted both by Kaldor (1982, p. 21) and the recanted Hicks (1982, p. 264), this does not mean that we must try to rescue him by making fancy distinctions between a ‘constant’ and a ‘given’ supply of money. If Keynes gave his seal of approval to the reserve position doctrine and to the money multiplier concept in the *Treatise on Money*, as recalled by Ulrich Bindseil (2004b), this does not mean that we must follow him blindly and ignore all the evidence provided by the new operating procedures that demonstrate that the purpose of compulsory reserves is not to restrain money creation.

If Keynes assumed decreasing returns when discussing employment in the *General Theory*, this does not mean that we have to follow him despite all empirical evidence to the contrary. As recalled by Simon (1997, p. 14), ‘a large part of the book is an exercise in neoclassical analysis’. Keynes may have had good strategic reasons for presenting his analysis the way he did. These reasons are no longer valid. ‘Keynes’s choices probably were adequate to carry out a more convincing criticism of the neoclassical dominant paradigm. However, in my opinion, they cannot be regarded as acceptable when the emphasis of the analysis is shifted toward the explanation of how actual economies work’ (Sardoni, 2002, pp. 10–11). In any case, this strategy turned out to be a failure because, as pointed out by the French translator of Keynes, the adornment of Marshallian micro-foundations with diminishing returns in the *General Theory* ‘made it possible to invoke the authority of the latter in favor of opinions directly contrary to its essential teachings’ (de Largentaye, 1979, p. 9).
The purpose of post-Keynesian economics is ‘to understand reality as a central point of its research and theory development’ (Jespersen, 2009, p. 15). We must thus start the analysis with assumptions that are grounded in empirically observed facts, that is, use assumptions that are realistic. This is not the case of the neoclassical production function and the assumption of decreasing returns, which Keynes accepted (certainly until 1939, when Dunlop and Kalecki presented their objections to Keynes). As pointed out by Johann Deprez (1996, p. 141),

[The] Davidsonian approach to the labour market is one true to Keynes’s method of building with Marshallian tools and of giving the classicals as much as possible and still end up with unemployment conclusions. As such, it is not – nor is meant to be – a fully positive, descriptive approach to what actually happens in the labour market.

To understand the working of actual economies, these mainstream assumptions about the firm and production must be given up, as we shall discuss in Chapter 3. Kaldor (1983a, p. 10) issued a harsh warning when he wrote that ‘so long as one sticks to neoclassical micro-economics, Keynesian macro-economics amounts to very little’. It is for these reasons that a large number of post-Keynesians believe that the economics of Kalecki provide a preferable foundation for an alternative to orthodox theory, at least when dealing with problems tied to the concept of effective demand. It is clearly the opinion of Harcourt (1987, pp. xi–xii), who believes that ‘Kalecki’s analysis of the political economy of capitalism is the most profound of the twentieth century, as relevant today as when he originally developed it’. It is also the view of Bhaduri (1986, p. ix), according to whom the radical content of Keynesianism must be learned from Kalecki. Similarly, the historian of economic thought, Gilles Dostaler (1988, p. 134), maintains that ‘Kalecki can be considered to be the real founder of post-Keynesian theory’. Even those who contributed to the development of the Keynesian revolution have passed similar judgments. Kaldor (1983a, p. 15) has noted that ‘Kalecki’s original model of unemployment equilibrium which takes monopolistic competition as its starting point, is clearly superior to Keynes’s’. It can be said that, over time, both Kaldor and Robinson turned away from Keynes and tended towards Kalecki. Robinson has argued that, because ‘Kalecki was free from the remnants of old-fashioned theory which Keynes had failed to throw off’, he was better able ‘to weave the analysis of imperfect competition and of effective demand together and it was this that opened up the way for what goes under the name of post-Keynesian theory’ (1977, pp. 14–15). Robinson (1973, p. 97) further argued that Kalecki’s version of the General Theory was more coherent and ‘was in some ways more truly a general theory’, because it incorporated the influence of investment on profits. The economics of Kalecki are not, as Keynes (1973, xii, p. 831) once thought, ‘esoteric abracadabra’.

The point I am trying to make here is that post-Keynesian economics is much more than a modernized version of the economics of a single contributor – Keynes. In some sense, post-Keynesian economics is a misnomer, because it encompasses several founding contributors besides Keynes. Indeed, this is why post-Keynesian economics can be said to be made up of five distinct strands. All strands have something important to contribute to the development of post-Keynesian economics. Some strands turn out to provide a stronger background for the study of some fields. For instance, although Sawyer (2001a) has shown that quite a lot could be said about the monetary theories of Kalecki, the
Global Financial Crisis has demonstrated the particular relevance of Fundamentalist Keynesianism, notably with its emphasis on the fragility and instability of financial markets à la Minsky, as well as the need for liquid assets and the crucial role played by liquidity preference, a theme developed by Keynes, Davidson and several other authors in this tradition. Various historical events highlight the strength of different strands.

1.5 SOME LIMITS OF ORTHODOX ECONOMICS

Over the past 40 years, orthodox economic theories have faced three major setbacks. We have already discussed the first of these in Section 1.1 – the incapacity of state-of-the-art neoclassical models to predict the Global Financial Crisis and the incapacity of their authors to provide useful advice to policy-makers during the crisis. In addition, the crisis has demonstrated that markets do not behave in the way described by the fanciest orthodox theories. Enough has already been said about this. The other two setbacks have occurred at the theory level. The first of these two theoretical setbacks has to do with the stability of general equilibrium theory and is known as the Sonnenschein–Mantel–Debreu theorem. We shall also call it the impossibility theorem. The second theoretical setback involved Sraffian economists: it has mainly to do with production theory in aggregate models and is known under the name of the Cambridge controversies in the theory of capital. The denouement of the Cambridge capital controversies has involved empirical work, and so in this section we will also discuss some issues about econometrics, notably the following question: if orthodox theories rely so much on unrealistic assumptions, why is it that so many empirical studies seem to provide supportive evidence for these theories? We shall see that students of heterodox economics have no reason to be intimidated by those. We start with the Cambridge capital controversies.

1.5.1 The Cambridge Capital Controversies

The Cambridge capital controversies pitted a group of Cambridge Keynesians (Robinson, Sraffa and the Sraffians), in England, against a group of economists from the Massachusetts Institute of Technology (MIT), in Cambridge, near Boston, in the USA. Whereas the mainstream usually views the capital controversies as some aggregation problem, this is not the point of view of the Cambridge Keynesian economists, who see them as a more fundamental problem. Joan Robinson (1975, p. vi), for instance, has clearly indicated that ‘the real dispute is not about the *measurement* of capital but about the *meaning* of capital’. Nicholas Kaldor (1957, p. 595), who only briefly engaged in the controversies, nevertheless had a similar view when arguing that the distinction between the movement along a production function and the shift in the production function is entirely arbitrary.

The controversies arose as a combination of circumstances. The *coup d’envoi*, from the neoclassical side, was provided by Paul Samuelson’s (1962) attempt to demonstrate that Robert Solow’s growth model and empirical manipulations of the neoclassical production function were perfectly legitimate. Samuelson was also trying to respond to Joan Robinson, following her 1961 visit to MIT. One can suspect that this rare opportunity of exchange between rival research programmes was provided by the fact that
both Robinson and Samuelson were dealing with linear production models, so that mainstream economists could grasp to some extent what the heterodox economists were up to. Robinson had in mind the Sraffian model that was then in the making (Sraffa, 1960), while MIT economists were working on linear programming and activity analysis (Dorfman et al., 1958). Samuelson (1962, pp. 201–2) claimed that the macroeconomics of aggregate production functions was ‘the stylized version of a certain quasi-realistic MIT model of diverse heterogeneous capital goods’ processes’.

The controversies made use of static models based on profit maximization (which led the older Robinson (1975) to argue that in the end the controversies were irrelevant), with fixed technical coefficients, but with several techniques, or even an infinity of techniques. It was finally resolved, among other things, that the main properties of aggregate production functions could not be derived from a multisector model with heterogeneous capital, nor for that matter even from a two-sector model with one machine, but several available techniques. This put in jeopardy the neoclassical concepts of relative prices as a measure of scarcity, substitution effects, marginalism, the notion of the natural rate of interest, and capital as a primary factor of production.

The controversies provided examples where standard results of neoclassical theory, as presented in undergraduate textbooks or when giving policy advice, were no longer true (Cohen and Harcourt, 2003). For instance, with aggregate production functions, it is usually argued that, economy-wide, the rate of profit is equal to the marginal productivity of capital, and that there exists an inverse relationship between the capital/labour ratio and the ratio of the profit rate to the real wage rate. Counter-examples were shown to exist and are well illustrated in Moss (1980), and more recently in Lazzarini (2011) and Harcourt (2012, ch. 4). Here we mention three of them:

- **Reswitching**: a technique which was optimal at high profit rates (or low real wages), and then abandoned, becomes optimal again at low profit rates (or high real wages); this however has been shown to be unlikely in practice (Han and Schefold, 2006);
- **Capital reversal**, also called reversed capital deepening or negative real Wicksell effects: a lower profit rate is associated with a technique that is less mechanized (a lower capital/labour ratio), even without reswitching; this, through simulations, has been shown to be quite likely (Zambelli, 2004);
- **Discontinuity** or rejection of the discrete postulate: an infinitely small change in the profit rate can generate an enormous change in the capital/labour ratio.

Figure 1.2 illustrates the implications of these results for the theory of labour demand. Neoclassical authors thought that an infinite number of fixed-coefficient techniques would yield a labour demand curve that has the standard downward-sloping shape shown in Figure 1.2(a). However, Pierangelo Garegnani, who was a student of Sraffa, has shown that it is quite possible to build examples of a continuum of techniques that do not generate the downward-sloping curves that are needed by neoclassical theorists to assert their faith in the stability of the market mechanisms. Garegnani (1970) provides a numerical example that gives rise to the labour demand curve shown in Figure 1.2(b), and Garegnani (1990a) suggests the possible existence of a labour demand curve that would have the shape shown in Figure 1.2(c). Because the neoclassical theories of value and output are, nearly by definition, one and the same thing, it should be clear that these
results have destructive consequences not only for neoclassical price theory but also for neoclassical macroeconomic theory, which relies on substitution and relative price effects.

What has been the response of neoclassical authors to the Cambridge–Sraffian arguments? This response can be summarized under six headings:

1. neoclassical authors minimize the capital paradoxes, making an analogy with Giffen goods in microeconomics, which do not question the entire neoclassical edifice;
2. they look for the mathematical conditions that would be required to keep production functions ‘well behaved’, or they claim that this is a simple aggregation problem that can be resolved;
3. they claim that they have the ‘faith’;
4. they ignore these possible capital paradoxes altogether;
5. they claim that Walrasian general equilibrium theory is impervious to the critique;
6. they rely on empiricism (it works, therefore it exists).

Today, the last three responses are the most common ones; it is no longer fashionable to say that ‘placing reliance upon neoclassical economic theory is a matter of faith’ (Ferguson, 1969, p. xvii). Ignorance is the fad: aggregate production models with the standard neoclassical properties still abound, despite the results of the Cambridge
capital controversies, with no apology. We discuss the last two responses in the next two subsections.

1.5.2 The Stability Nightmare of Neoclassical Theory

The consequences of the Cambridge controversies described above have also sometimes been perceived as a stability problem that would mar the results of an aggregate production economy. The Sraffians themselves volunteered this interpretation with some of their statements. For instance, Garegnani (1983, p. 73) argues that the Cambridge controversies ‘deny plausibility to the traditional argument about a long period tendency towards the full employment of labour’. Colin Rogers (1989, p. 33) speaks of the ‘problematic stability’ of the long-period equilibrium solution. Nevertheless, the Sraffians have generally preferred to emphasize the fact that if the overall employment curve for labour looks the way it does in Figure 1.2(b) or 1.2(c), then one cannot really talk of a demand for labour as such. A claim is then made that demand and supply conditions, based on the flexibility of prices, can explain neither the prevailing wage rate nor the level of employment (Mongiovi, 1991, p. 28). Some other explanation, not based on these price functions, must then be the pertinent one, based on norms, conventions or notions of fairness. Another way to present this is to say that, since the Sraffians have shown that there can be unstable equilibria, looking at it from the point of view of standard supply and demand analysis, and since we observe no such blatant instability in the real world, the mechanisms at work must be something else than the standard supply and demand price theory.

A further debate has erupted concerning whether or not the Cambridge capital critique applies not only to the aggregate version but also to the intertemporal version of the neoclassical version, that is, the Walrasian general equilibrium model, which is still considered as the nec plus ultra of neoclassical theory. As we saw above, a major defence against the Cambridge critique was to suggest that the general equilibrium model was impervious to it. Garegnani (2011) for one has argued that the most sophisticated versions of neoclassical theory are also subjected to the Cambridge critique, because ultimately agents need to deal with an aggregate value of capital to take their decisions. Schefold (2011, p. 87) is more prudent about the claim that reswitching and capital reversal are causes of instability in the intertemporal general equilibrium model, saying that it is instead ‘a hypothesis, supported by preliminary results’.

But whatever is the case, results most damaging to the neo-Walrasian general equilibrium model, derived within the model, have been arrived at independently by three researchers, putting in jeopardy the stability of the model. The damaging result proven by Sonnenschein and others is the following (cf. Kirman, 1989; Guerrien, 1989; Rizvi, 2006). Starting from the usual maximizing behaviour of individuals, resulting from the assumptions required for the demonstration of the existence of a general equilibrium of the Arrow–Debreu type, it is shown that the excess demand functions satisfying Walras’s law in an exchange economy can take almost any form. This is damaging to neoclassical theory because one would have hoped that the excess demand functions would always be downward-sloping. This would ensure that, when the price of a good is too low, and consequently the excess demand positive, the tâtonnement process leads to a decrease in the excess demand as a result of the commissaire-priseur calling higher prices. This is
illustrated by Figure 1.3(a). What the so-called Sonnenschein–Mantel–Debreu theorem, or impossibility theorem, demonstrates is that nothing in the standard hypotheses of individual choice behaviour precludes the excess demand functions from looking like Figure 1.3(b). As can be seen, there are several equilibria, and increasing the price when
at point A would initially increase excess demand to point B. Small changes in the value of the data may lead to large changes in prices, precisely what the Cambridge controversies had underscored in the context of long-period positions of aggregate production economies. The only constraints on the shape of the function are that for some high price excess demand should be negative, and as price approaches zero the curve should tend towards infinity.

Neo-Walrasians may thus be able to prove the existence of an equilibrium, but they are unable to prove its uniqueness and its stability even in the simple case of an exchange economy without production. The same problem plagues the intertemporal model à la Arrow–Debreu and the temporary equilibrium models, where expectations of future prices add a further arbitrary dimension. Thus all comparative results become useless. This also implies that the invisible hand, even if prices are perfectly flexible, may be of no help in attaining an equilibrium, not to speak of the optimum one. This may explain, as pointed out by Abu Rizvi (2006, p.230), that orthodox economists have virtually abandoned all research in general equilibrium theory, moving towards game theory and experimental economics, since the Arrow–Debreu research programme has reached a dead end. As Walsh (2011, p.463) puts it, ‘the canonical Arrow–Debreu model is a mansion deserted by its owners, the party is over, and the rigour has become rigor mortis’. Neo-Walrasian economics is no longer even taught in graduate programmes. Furthermore, and perhaps more damaging, the only way out of these negative results on stability seems to be to relinquish at least one of the four presuppositions on which the whole research programme is built, that of methodological individualism.

It has been noted that one way to get out of this impasse is to assume that all agents have identical preferences and equal income. One is back to the single representative agent, so much in vogue in DSGE modelling. This implies, furthermore, that the realms of microeconomics be left alone and that the methodology of building the foundations of economics from independent individuals be abandoned. The consequences have been well summarized by a participant to these negative theorems:

The independence of individuals’ behaviour plays an essential role in the construction of economies generating arbitrary excess demand functions. As soon as it is removed the class of functions that can be generated is limited . . . If we are to progress further we may well be forced to theorise in terms of groups who have collectively coherent behaviour. The idea that we should start at the level of the isolated individual is one which we may well have to abandon. (Kirman, 1989, p.138)

The consequences of the Cambridge controversies were a setback for the neoclassical research programme, but it was said that they applied only to the aggregate versions of neoclassical theory. They dealt with production economies in steady states. The impossibility theorems demonstrated by Sonnenschein and others turned out to be a major setback for the neoclassical programme. The stability of the neoclassical model, whatever its degree of sophistication, the highbrow version or the vulgar one, thus cannot be demonstrated. This implies that comparative analysis cannot be performed within the standard neoclassical framework of supply and demand responding to market forces, at whatever level of aggregation. Furthermore, the standard assumptions made in macroeconomics or in partial equilibrium microeconomics have no justification whatsoever. Barring imperfections of all sorts, the flexibility of prices will not guarantee the attain-
ment of the optimal Walrasian equilibrium. The problem is not one of imperfections; it is one of structure.

To get around the impossibility theorem, and avoid the near emptiness of general equilibrium theory, orthodox economists have adopted the unique representative agent. What have they done to get around the Cambridge critique affecting neoclassical aggregate models? They have adopted a pragmatic approach, claiming that the use of the neoclassical production model is justified because it ‘works’.

1.5.3 Neoclassical Production Laws as Artefacts

Empiricism is the last line of defence of neoclassical economics, but we will show that it is a very weak one. As soon as MIT recognized its defeat over the Cambridge capital controversies in the 1966 symposium of the Quarterly Journal of Economics, neoclassical authors quickly moved on to the empirical front. Several orthodox economists have taken the view that the validity of neoclassical theory is an empirical question, not a logical one. One presumes that the stance implicitly taken is that neo-Walrasian theory does not have much to offer when it comes to more practical issues and that one then has to rely on the more pedestrian versions of neoclassical theory to be able to make any practical recommendations. What these authors then argue is that the Cambridge critique is right in a formal sense, but they deny that it has any real-world consequence. The empirical proof usually advanced to support this position is given by the numerous successful regressions that have been performed with various neoclassical production functions, with the regressions yielding the expected coefficients. Some orthodox economists were initially rather critical. Frank Hahn, a neoclassical economist from Cambridge, UK, was, at least initially, quite critical of the empiricist defence, claiming that the simplicity of the aggregate neoclassical theory ‘is obtained at the cost of logical coherence’ and that ‘the view that nonetheless it “may work in practice” sounds a little bogus and in any case the onus of proof is on those who maintain this’ (Hahn, 1972, p. 8). Nonetheless, in the end, the empiricist view has prevailed.

Modern orthodox authors justify their use of aggregate production functions on the basis of past successful regressions of neoclassical production functions. As Nobel Prize recipient Prescott (1998, p. 532) points out, ‘the neoclassical production function is the cornerstone of the [neoclassical] theory and is used in virtually all applied aggregate analyses’. Without it, very little or no applied aggregate economic analysis can be pursued by orthodox economists. And very little policy advice could be offered, because, for instance, as again pointed out by Prescott (ibid.), ‘the aggregate production function is used in public finance exercises to evaluate the consequence of alternative tax policies’. This is why it is so important for mainstream economists, even well-known ones such as Hamermesh (1986, pp. 454, 467), to claim that ‘the estimated elasticities that seem to confirm the central prediction of the theory of labor demand are not entirely an artefact produced by aggregating data . . . The Cobb–Douglas function is not a very severe departure from reality in describing production relations.’

But is this the case because the world behaves as if it were a neoclassical Cobb–Douglas function, or is it for some other, more credible, reason?

One can draw up a long list of authors who have argued, in one way or another, and with more or less clarity, that neoclassical production functions (such as the Cobb–
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Douglas function, the constant elasticity of substitution (CES) function, or the translog production function) often provide good empirical results because they simply reproduce the underlying identities of the national accounts. The argument applies both to cross-industry estimates and to time series. The list goes back to Phelps-Brown (1957). It incorporates previous winners of Nobel Prizes in economics, Paul Samuelson (1979) and Herbert Simon (1979a), with the latter thinking that the issue was important enough to be mentioned in his Nobel Prize lecture. As one would suspect, some heterodox economists have driven the point on numerous occasions: Anwar Shaikh (1974; 1980a; 2005), John McCombie (1987; 1998; 2000–2001; 2001), McCombie and Dixon (1991). Felipe and McCombie (2001; 2005; 2006; 2009; 2011–12) have written together numerous papers on this topic, which are recapitulated in their book (2013). I have myself dealt briefly with the subject in two of my books (Lavoie, 1987; 1992b) and more extensively in two articles (Lavoie, 2000a; 2008).

Orthodox authors often marvel at the apparent key result that their estimates of the output elasticities of capital and labour turn out to be nearly equal to the shares of profit and wages in national income. Since neoclassical theory predicts that this will be so in a competitive economy with diminishing returns and constant returns to scale, where firms are pursuing profit maximization and thus pay their production factors at the value of their marginal product, neoclassical economists usually conclude that, even though they know that the real world is made up of oligopolies and labour unions, as well as many other imperfections, in the end it behaves as if it were subjected to competitive forces. This assertion is rather hard to swallow, but all kinds of reasons will be advanced to justify such a result, such as the theory of contestable markets, whereby the threat of entry by newcomers will be sufficient to ensure that incumbent members of an industry behave in a competitive way. The (apparent) amazingly successful estimates of neoclassical production functions thus reinforce the belief of many neoclassical economists that the idealized supply and demand analysis is good enough to describe the real world, since economic agents ultimately behave as if pure competition prevailed. Similarly, in the realm of labour economics, when the profit-maximizing first-order conditions of a well-behaved neoclassical production function (with diminishing marginal product of labour, perfect competition, factor pricing at the value of the marginal product etc.) are fulfilled, the labour demand equation of Layard et al. (1991), which they call the price-setting equation, yields parameter values that are exactly reproduced in empirical studies. Researchers then marvel at how well neoclassical theory describes empirical reality.

The reality, however, is that the very same labour parameters can be obtained through the identity of the national accounts (Lavoie, 2000a). Similarly, Shaikh (1974) has shown that estimates of the production function based on deflated values simply reproduce the identities of the national accounts and that the pseudo estimates of the output elasticity of capital (labour) are really approximations of the profit (wage) share. The latter claim can be seen in the following way, by rewriting the Cobb–Douglas production function and the national accounts in logs or in growth terms. Start with the Cobb–Douglas function with technical progress, where $q$ is real output, $L$ the number of workers and $M$ the stock of machines:

$$q_t = A_0 e^{at} L_t^a M_t^b$$

(1.1)
As is standard, $\alpha$ and $\beta$ are presumed to be the output elasticities of labour and of capital respectively. Assume constant returns to scale, so that: $\alpha + \beta = 1$. Now consider output per head and capital per head, $y = q/L$ and $k = M/L$. Taking logs, the Cobb–Douglas function yields

$$\log y = \mu t + \beta \log k$$

Or, in growth terms, taking the log difference, $\Delta \log$, we have

$$\dot{y} = \mu + \beta \dot{k}$$

where the caret mark over a variable signals the growth rate of the variable.

We may now compare the two equations (1.2) and (1.3) with those obtained from the national accounts. Start with the national account identity, given by equation (1.4):

$$pq = wL + rpM$$

where $q$, $L$ and $M$ are defined as before, and where $p$ and $w$ stand for prices and the nominal wage rate, while $r$ is the profit rate. Thus $pq$ is nominal GDP, $wL$ is the wage bill and $rpM$ are nominal profits. Now divide equation (1.4) through by the prices and the number of workers. One gets output per worker:

$$q/L = w/p + r(M/L)$$

or

$$y = \omega + rk$$

where $y$ represents output per head, or labour productivity, $\omega$ is the real wage rate $w/p$, and $k$ is the number of machines per worker. Taking the derivative of equation (1.6) with respect to time yields

$$dy/dt = d\omega/dt + k.dr/dt + r.dk/dt$$

This can be rewritten as

$$dy/dt = \omega(d\omega/dt)/\omega + kr(dr/dt)/r + rk(dk/dt)/k$$

We now divide this whole expression by $y$. Recalling that $(dy/dt)/y$ is the rate of growth of output per head, we get

$$\dot{y} = \left(\frac{\omega}{y}\right)\dot{\omega} + \left(\frac{rk}{y}\right)\dot{r} + \left(\frac{rk}{y}\right)\dot{k}$$

Denoting the profit share in national income by the Greek letter $\pi = rk/y$, the logarithmic derivative of equation (1.6) turns out to be
\[ \dot{y} = \tau + \pi \dot{k} \]  
(1.7)

where

\[ \tau = (1 - \pi)\dot{\omega} + \pi \dot{r} \]  
(1.7A)

Or else, by integrating, we have in logs

\[ \log y = \tau t + \pi \log k \]  
(1.8)

Equations (1.7) and (1.8), derived from the national identities, are highly similar to equations (1.3) and (1.2), which came from the Cobb–Douglas production function. Thus it is not surprising that these equations will perform well, as long as technical progress \( m \) in equations (1.2) or (1.3) is adequately represented. Indeed, Anwar Shaikh (1974; 1980a) has shown that even a production relation that would trace the word HUMBUG, with capital per head on the horizontal axis and output per head on the vertical axis, can be successfully represented by a Cobb–Douglas production function, using the method advocated by Solow (1957). Thus, as should now be clear following the exercises of Fisher (1971), any technological relation will yield the appearance of a Cobb–Douglas production function as long as its income shares are relatively constant.

Still, there are cases where Cobb–Douglas functions will yield nonsense, and hence are not ‘verified’, as pointed out by various authors such as Lucas, Romer and Solow, who try to justify such empirical tests. Such a situation does not normally occur when there is no technical progress. The problem is that technical progress is sometimes represented by a linear trend, whereas in reality the growth rate of labour productivity is highly variable, as shown by Shaikh (2005). Technical progress cannot be represented by some linear function; one must introduce a non-linear trend, given by a Fourier series or some trigonometric function, because the rate of technical progress is fluctuating in a wild way.

In the article that started the growth-accounting craze, Solow (1957) managed to overcome this problem by constructing a variable measuring technical progress. Solow’s favourite equation is the log version of the Cobb–Douglas production function, given by equation (1.2) above, which we repeat here for convenience: \( \log y = \mu t + \beta \log k \). Then, for each period, he introduces a value for the technical progress growth rate, \( \mu \), which he defines in a way that is analogous to equation (1.7A), thus deriving the measure of his \( \mu \) parameter straight from the national accounts (more precisely, he derived it from the quantity dual of equation (1.7A)). In other words, Solow tested the national accounts identity, while claiming to have corroborated the neoclassical theory of income distribution and neoclassical production functions, as well as claiming to have found a simple way to distinguish between shifts of aggregate production functions and movements along the production function. No wonder he got a good fit!

Indeed, nowadays, neoclassical economists who still ‘test’ the Cobb–Douglas production function adjust the data by making corrections to the capital stock, deflating the capital index by taking into account the rate of capacity utilization, which is tightly correlated to the rate of technical progress, thus obtaining a good ‘fit’ with their regressions. Otherwise regression results of the Cobb–Douglas production function with technical
progress would be catastrophic. If technical progress is misrepresented (for instance through a linear function in time, rather than by a non-linear one), the output elasticity estimates will not equal the profit and wage shares, and the elasticities may even turn out to be negative. This explains why Cobb–Douglas functions sometimes seem to misrepresent production relations, giving the illusion that neoclassical production functions can be falsified by empirical research.

All in all, the Shaikh and Solow episode has demonstrated that Kaldor’s assessment of the neoclassical empirical work, as found below, was quite on the mark. Orthodox authors often ‘decorate’ their theories by calibrating them; they do not actually attempt to verify, let alone falsify, them.

In economics, observations which contradict the basis hypotheses of prevailing theory are generally ignored. And where empirical material is brought into conjunction with a theoretical model, as in econometrics, the role of empirical estimation is to ‘illustrate’, or to ‘decorate’ the theory, not to provide support to the basic hypothesis (as for example, in the case of numerous studies purporting to estimate the coefficients of production functions). (Kaldor, 1972, p. 1239)

1.5.4 ‘Reductio ad Absurdum’ Proofs against Neoclassical Empiricism

Students are rarely convinced by the demonstration found in the previous subsection. They need a proof that is more stunning. Here we present three successive reductio ad absurdum proofs, all related to the use of neoclassical production relations.

Shaikh’s proof
We start with a proof offered by Shaikh (2005). He constructs a fictitious economy, where the value taken by variables through time is generated by data obtained from a Goodwin-cycle model, with Leontief input–output technology (fixed technical coefficients), Harrod-neutral technical progress and mark-up pricing. Hence none of the usual neoclassical constructs exists (diminishing returns, marginal productivity, marginal cost pricing). Still, once technical progress is introduced in an adequate way, any data can appear to be fittingly represented by a Cobb–Douglas function. This is the case of the US data also compiled by Shaikh, which yield a nearly perfect adjusted $R^2$ and an estimated output elasticity of capital that nearly perfectly equates the actual profit share, as neoclassical theory would have it; but more surprisingly, it is also the case of the Goodwin data, which, by construction, violate all the standard neoclassical assumptions.

One way to understand what is going on is to look at Figure 1.4, which represents a Leontief production function with fixed coefficients, with a dominant technology at each point of time. With technical progress arising at a constant capital to output ratio (given by $v_o$), that is, technical progress is of the Harrod-neutral sort, the real wage–profit frontier (here assumed to be linear for simplicity) rotates to the north-east, as shown on the left-hand side of the figure. On the right-hand side of the figure, one can observe the true relationship between output per head and capital per head: it is a simple straight line, $y = (1/v_o)k$. Neoclassical analysis, however, will assume that there exists a standard production function, with diminishing returns and the standard curvature, so that it needs to distinguish between a shift of the production function and a move along the production function, from $k_0$ to $k_2$. Even when technology is of the Leontief type, as depicted...
in Figure 1.4, neoclassical economists running standard regressions on deflated variables will manage to ‘prove’ the existence of a well-behaved pseudo-neoclassical production function.

**McCombie’s proof**

We now move to the second *reductio ad absurdum* proof, this one offered by McCombie, who has devoted quite a bit of attention to these issues. McCombie (2001) takes two firms each producing in line with a Cobb–Douglas function:

\[
q_{it} = A_0 L_{it}^a M_{it}^b \tag{1.9}
\]

with \( \alpha = 0.25 \).

The other variables are defined as in previous equations. Thus \( \alpha \) is still the output elasticity of labour and is equal to 0.25 for both firms. Similarly, for both firms, the output elasticity of capital, \( \beta \), is equal to 0.75 since the sum of the two elasticities is assumed to be unity (there are constant returns to scale). Inputs and outputs of the two firms are perfectly identical. Hence there is no aggregation problem of the sort noted by Franklin Fisher (1971), which led him to conclude that if the Cobb–Douglas production produces good fits, it is because the share of wages is roughly constant, and not the other way around.

McCombie (2001) constructs a hypothetical economy, where \( L \) and \( M \) grow through time, with no technical progress, but with some random fluctuations. Running an econo-

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*Figure 1.4 True Leontief technology and fitted neoclassical production functions*
metric regression directly on this constructed physical data set (the \( q, L \) and \( M \) variables) yields an \( \alpha \) coefficient close to 0.25, as expected. Running a regression over the equation in log values, McCombie obtains the following relationship (with the absolute \( t \)-statistics in parentheses):

\[
\log q = -0.02 + 0.277 \log L + 0.722 \log M \\
(22.5) \quad (55.5)
\]

In this case, the estimate is based on physical data, and there is no problem: the regression estimates of the output elasticities correspond nearly exactly to those that exist by construction. With such a result, a neoclassical economist would conclude that neoclassical theory has been vindicated. Things turn out to be entirely different, however, when monetary values are used.

McCombie (2001) reconstructs the same hypothetical economy, with the same two firms, each again with identical output elasticities, but this time he tries to estimate an aggregate production function using deflated monetary values (also called constant-price values), as must always be done in macroeconomics and most often in applied microeconomics at the industry level. To do so, he assumes that firms set prices on the basis of the simplest of the cost-plus pricing procedures – a mark-up equation. Prices then depend on a percentage costing margin \( q \) applied to unit labour costs \( (wL/q) \), about which we will say more in Chapter 3:

\[
p = (1 + \theta)w(L/q) \\
(1.10)
\]

McCombie assumes that firms impose a mark-up equal to 1.33 (the percentage costing margin \( \theta = 0.33 \)). This implies that the wage share is 75 per cent and that the profit share is 25 per cent of national income. With this new regression, based on deflated monetary values, which we denote with the subscript \( d \) to make this clear, the regression yields an estimate of the \( \alpha \) coefficient – the apparent output elasticity of labour – that turns out to be 0.75, as shown in the regression equation that follows:

\[
\log q_d = +1.8 + 0.752 \log L + 0.248 \log M_d \\
(1198) \quad (403)
\]

Thus we started with production functions and physical data according to which we know that, by construction, the labour output elasticity \( \alpha \) is 0.25. Yet the estimated aggregate production function (in deflated monetary terms) tells us that this elasticity is 0.75 – which is the wage share in income. In other words, estimates of aggregate production functions – at both the industry and macro levels, since they are necessarily based on deflated values and not on direct physical data – measure factor shares. They do not measure the output elasticities of factors of production, in contrast to what neoclassical authors would like us to believe. Disaggregation will not solve the problem in the least as long as deflated variables are used.

These empirical estimates of aggregate production functions are completely useless to provide any information about the kind of technology in use, or about the values of output elasticities and elasticities of substitution. McCombie (2001) provides additional
proof of this. He starts with the base year data of the two firms mentioned above, but assuming now, by construction, that the inputs and outputs of these firms grow in a completely random way. Not surprisingly, when a regression is run on the physical variables of each firm, correlation coefficients are near zero and estimates of output elasticities are statistically insignificant, as they should be, since there is no relationship between inputs and output, by construction.

By contrast, when the same physical data set is combined with monetary value data obtained by assuming the same mark-up in each firm, with again a 75 per cent labour share and assuming a constant profit rate, the regression on the aggregated deflated values yields very promising results. The correlation coefficient is nearly unity, and the regression coefficients yield statistically significant values that reflect once more the labour and profit shares:

$$\log q_d = \text{constant} + 0.751 \log L + 0.248 \log M_d$$

(514) (354)

Thus, as McCombie (2001, p. 598) concludes,

no matter what form the underlying micro or engineering production functions take, so long as the average mark-up is roughly constant over time (so that factor shares are constant), a reasonable fit to the Cobb–Douglas relationship will always be found. However, this says nothing about the underlying technology of the economy.

So even if technology is from Mars, and Martians manage to produce output independently of inputs, provided Martian firms follow some form of cost-plus pricing, the regressions over deflated data will tell us that Martians use Cobb–Douglas production technology, with diminishing returns, constant returns to scale, and factor pricing following principles of marginalism.

Why is this so? It turns out, as we saw in the previous subsection, that regressions over the deflated variables of production functions, when they are correctly estimated, reproduce only the relationships of the national accounts. If the wage share is approximately constant, and if there is no technical progress or, if technical progress is adequately estimated, one will always discover that a Cobb–Douglas production function provides a good fit. If the wage share is not constant, for instance when the wage share trends upwards along with the capital to labour ratio, the CES function will yield better fits. But the CES production function, along with the translog production function, are subject to the very same criticisms that apply to the Cobb–Douglas function (McCombie and Dixon, 1991; Felipe and McCombie, 2001).

**Anyadike-Danes and Godley’s proof**

We now turn to a third and final reductio ad absurdum proof, provided by Michael Anyadike-Danes and Wynne Godley (1989). These post-Keynesian economists question the relevance of the kind of regression analysis that has been pursued by the economists who are convinced that overly high real wages are the main cause of the high European unemployment rates (Layard, Nickell and Jackman in particular, referred to as LNJ from now on). Godley and his associate intend to demonstrate that even when, by construction, there is no no relationship whatsoever in a hypothetical economy between employ-
ment and real wages, standard econometric analysis (based on OLS (ordinary least squares) estimates) will give the impression that it verifies a negative relationship between employment and real wages.

Before we move on to their econometric results, we start by showing how easy it is to retrieve the labour-demand price-setting (PS) equation of the LNJ model. The PS equation of LNJ, obtained through an appropriate profit-maximizing behaviour of firms and other standard neoclassical conditions, is

\[(\log w - \log p) = U + (\log q - \log N)\]  

(1.11)

where \(U\) is the rate of unemployment and \(N\) is active population (in contrast to \(L\), which is employed labour), while the other variables are defined as they were before. Equation (1.11) is usually interpreted, in particular in OECD offices, as saying that a higher rate of unemployment is being caused by higher real wages.

We simply wish to show that, by starting from the same mark-up pricing equation that McCombie used in his own proof, we will be able to arrive at LNJ's PS equation. Taking the log of the mark-up equation given by equation (1.10), we obtain

\[\log p = \log \theta + \log w - \log q + \log L\]  

(1.12)

or, rearranging in terms of labour employment, and dropping the constant, we get

\[\log L = - (\log w - \log p) + \log q\]  

(1.13)

Equation (1.13) reminds us that, for a given output level, we automatically get a negative relationship between employment and real wages when prices are set through a cost-plus procedure. But this negative relationship only reflects the fact, that, with a given costing margin, the real wage will be lower if labour productivity (measured by \(\log q - \log L\)) is lowered. It has nothing to do with a demand for labour function. It is simply an arithmetic relation that arises from the cost-plus pricing formula. Rewriting equation (1.12) yet once more, and dropping the constant, we see that

\[(\log w - \log p) = \log q - \log L\]  

(1.14)

LNJ arrive at their own PS equation by drawing on an approximation of the definition of the rate of unemployment \(U\). They use equation (1.15):

\[U = \log N - \log L\]  

(1.15)

Combining equations (1.14) and (1.15), thus by combining two quasi-identities, we obtain LNJ's PS equation – equation (1.11). Thus, having started from the simple mark-up pricing equation, with no marginalism content whatever, we can recover the PS equation that attributes high unemployment to excessive real wages – a result that neoclassical economists attribute to the profit-maximizing behaviour of firms making hiring decisions. Felipe and McCombie (2009, p. 165) confirm that, in general, estimates of labour demand functions based on value data reproduce the identities of the national accounts.
Post-Keynesian economics

and hence ‘will always yield a negative relationship between the level of employment and the real wage’.

Anyadike-Danes and Godley (1989) go one step further. Here is their reductio ad absurdum proof. They start by assuming, by construction, that nominal wages, output and employment all grow independently of each other, with prices set on the basis of a mark-up on current and lagged labour unit costs (75 per cent of sales are assumed to be based on current output and 25 per cent of sales arise from held inventories, produced in the previous period, and hence, in the pricing equation below, \( \varphi = 0.75 \)). Wage rates, output and employment are each assumed to grow at some specific trend rate (7 per cent, 5 per cent and 1 per cent respectively), with random fluctuations around it. We have:

\[
\begin{align*}
\log w &= (1.07 + \text{random}) + \log w_{-1} \\
\log q &= (1.05 + \text{random}) + \log q_{-1} \\
\log L &= (1.01 + \text{random}) + \log L_{-1} \\
\log p &= \log \theta + \varphi(\log w - \log q + \log L) + (1 - \varphi)(\log w_{-1} - \log q_{-1} + \log L_{-1})
\end{align*}
\]

Anyadike-Danes and Godley then run a regression on the data generated by this hypothetical economy. They get the following result:

\[
\log L = 1.3 - 0.94 (\log w - \log p) - 0.12 \log L_{-1} + 0.73 \log q + 0.01 t
\]

\[\text{(7.4)} \quad \text{(1.0)} \quad \text{(1.0)} \quad \text{(4.2)}\]

According to the regression equation, employment seems to entertain a statistically significant negative relationship with real wages (nearly equal to unity, as expected from equation 1.14), as well as a positive time trend, as LNJ and their orthodox colleagues would like it to be. In addition, note that employment does not seem to depend on actual output \( q \), in contrast to what post-Keynesians would argue, and that it does not depend on past employment \( L_{-1} \), since these two variables do not have statistically significant coefficients in the regression.

But we know that, by construction, employment \( L \) is completely independent of real wages, and that the current level of employment depends only on past employment. This is what the regression should reflect. As Anyadike-Danes and Godley (1989, p. 178) put it, ‘real wage terms turn out to be large, negative and strongly significant although we know, as Creator, that real wages have no direct causal role whatever in the determination of employment’. Thus empirical studies can manage to give support to the neoclassical theory of labour demand even in those cases where we know that, by construction, neoclassical theory is completely irrelevant (that is, when real wages and employment are independent of each other, while prices are set on a cost-plus basis and not on marginal-pricing principles).

**Mainstream instrumentalism: not even wrong**

Spurious correlation, as illustrated here, is an important problem in economics. It has been shown repeatedly that series of random walks that are absolutely independent of each other may exhibit high correlation coefficients. Granger and Newbold (1974), for instance, have shown that, on average, an \( R^2 \) of 0.59 could be obtained when regressing such a random walk over five variables also exhibiting a random walk. Variables that have nothing to do with each other may appear to have some economic relationship.
Indeed, Hendry (1980) humorously showed that he could provide empirical support for a new theory of inflation by relating the price index \( P \) in the UK to an exogenous variable \( C \) – cumulative rainfall in the UK! Luckily enough, time-series econometrics has taken great strides over the last 30 years, identifying legitimate (long-run) relationships through cointegration techniques.

This being said, the studies of Shaikh, McCombie, Felipe and others show that the econometric estimates of neoclassical production functions based on deflated monetary values, or constant-price value terms, as is the case at the macro and industry levels when direct physical data are not used, yield pure artefacts, that is, purely imaginary results. This affects all neoclassical applied aggregate work that relies in some way on well-behaved production functions and profit-maximizing conditions: NAIRU measures, labour demand functions and wage elasticities; investment theory; measures of multifactor productivity or total factor productivity growth; estimates of endogenous growth; theories of economic development; theories of income distribution; measures of output elasticities with respect to labour and capital; estimates of cost functions; measures of potential output; theories of real business cycles; estimates of the impact of changes in the minimum wage, social programmes, or tax rates. Even when setting aside problems of aggregation, these estimates are either completely off target (if the world is made up of neoclassical production functions) or imaginary (if economies are run on fixed technical coefficients, as most post-Keynesians believe). As Felipe and McCombie’s (2013) book title says, neoclassical production theory is ‘not even wrong’: it is so useless that you cannot even prove it wrong!

As pointed out earlier, orthodox economics relies on instrumentalism, which claims that assumptions need not be realistic, as long as they help in making predictions. It combines the ability to start from idealized imaginary models and the need to resort to empiricism. In the case of well-behaved production functions and their implied labour demand functions, orthodox economists are pushing instrumentalism to the hilt. What counts is their ability to make predictions, based on estimates of elasticities, even if these predictions are meaningless because the estimates do not measure output elasticities, measuring instead factor shares! Neoclassical economists are claiming to measure something, but are really measuring something entirely different. Their theories, such as the necessary negative relationship between real wages and employment, seem to be supported by the data, whereas the negative relationship arises straight from the identities of the national accounts, with no behavioural implication for the effect of higher real wages on employment.

I have discussed these issues with a few of my neoclassical colleagues. The most genuine answers have been that without these elasticity estimates they could no longer say anything. But they would rather continue making policy proposals based on false information than make no propositions at all. In other words, they would rather be precisely wrong than approximately right. As Felipe and McCombie (2011–12, p. 290), conclude,

given the importance of this critique, it is surprising that it has been almost totally ignored, misinterpreted, or even greeted with outright hostility within the mainstream profession. But perhaps on reflection it is not all that surprising. Few people are willing to concede that much of their academic work may be literally meaningless.

More than 50 years after the publication of Sraffa’s (1960) book, it seems that this empirical critique of neoclassical production functions, based on *reductio ad absurdum*
proofs, is the most damaging one. The theoretical critique, based on Sraffian theory, has been blunted by the empirical discovery, through the manipulation of actual input–output tables, that wage–profit curves are nearly linear, that few of them appear on the envelope of efficient techniques, and that both capital-reversing and especially reswitching are unlikely phenomena (Schefold, 2013). It could be argued, however, that this critique is itself blunted by the fact that input–output technical coefficients are not obtained from direct observation of engineering physical data. They are derived instead from the computation of deflated values, and are thus potentially subjected to the measurement problems associated with aggregate production functions (Felipe and McCombie, 2013, p. 42).

1.5.5 Further Limits of Orthodox Economics

Suspicion of publication bias
The multitude of research studies that supposedly demonstrate the validity of neoclassical theory in various fields of economics is one of the most puzzling features encountered by students dissatisfied with the mainstream approach. Whereas they confusedly perceive that several of the hypotheses that underlie the tested neoclassical models lack substance or realism, students are being swamped with successful tests of these models. It seems that the real world behaves according to these absurd hypotheses.

The preceding subsection has clearly shown that heterodox economists and their students need not fear the mountains of empirical evidence that seems to give support to neoclassical theory. Most, perhaps all, of this evidence is an artefact. The tons of regressions conducted on just-identified neoclassical production functions can provide estimates only of the model’s parameters, but they can in no way provide support for the theory. Neoclassical production theory, and its offshoots, cannot be falsified by econometric research, and hence, if we are to believe the philosopher of science, Karl Popper, they are not truly scientific. Even worse than that, the experiments recalled here have shown that estimates based on constant-price values do not measure what neoclassical economists claim to be measuring. Policy advice based on these estimates is bogus.

A second cause of this proliferation of fortunate empirical studies is the manner in which empirical research is being conducted. This applies to orthodox researchers as well as to researchers of other persuasions. The typical economist draws up a theory, outlines a simplified functional form that could be empirically tested, and adds a few secondary variables that could be of significance. With the help of the computer and some arbitrary algorithms, the analyst then searches for the best equation. Several rounds might be needed to find regressions that offer any fit, and on the way several variables and several specifications will have been tested and discarded, bringing as well a revision of the theory. Furthermore, the data might be divided into subperiods, part of the data might be discarded, the data might be weighted in a useful manner, dummy variables might be introduced, and so on. In the end the theory claimed to be tested may have only a remote relationship with the one originally posited. This is the so-called interaction between data and theory. Some prefer to speak of ‘data mining’, ‘data fishing’ or ‘data massaging’.

The famous study of Reinhart and Rogoff (2010) provides a striking example of data massaging. These two neoclassical economists have famously calculated that countries that run public debt to GDP ratios over 90 per cent grow much more slowly than coun-
tries with lower ratios. This study thus provided a convenient legitimacy to the calls for fiscal austerity and consolidation in the aftermath of the Global Financial Crisis. Notwithstanding the issue of reversed causality – slow or negative growth may generate high debt ratios rather than the converse – Herndon et al. (2014) show that if coding mistakes, omitted entries and a more conventional weighting method are taken into account, the growth rates of countries with high public debt to GDP ratios rise from −0.1 per cent to +2.2 per cent.

Here I wish to show that this is a generalized problem in economics, which questions several empirical results that seem to provide support for orthodox theory. In the 1992 version of this book, I asked: why do neoclassical theories always seem to be supported by some empirical evidence? Why do empirical facts in orthodox economic journals often appear to verify orthodox theory? Here is the answer that I then provided:

The first thing to notice is that journals usually do not publish inconclusive results, except when they provide a scapegoat which can then be used to highlight the rival pet theory of their editors. As a consequence, authors do not bother submitting results which are inconclusive. Only a biased sample of the empirical work that is going on is thus published. Most of the unsuccessful attempts at verification go unnoticed, and a lot of the unsuccessful attempts at replicating published empirical models do not end up in the learned journals. (Lavoie, 1992b, p. 21)

Since this was written, a considerable amount of work has been devoted to this issue, that is, the problem of publication bias, sometimes called reporting bias, ideological bias or the file-drawer problem. In psychology, this is called confirmation bias. This is a well-known problem in medicine, where the results of studies of the effectiveness of medical drugs may mean substantial additional profits for the pharmaceutical companies. A further, but related, problem is that different investigators come up with quite different findings when trying to assess the same phenomenon. A standard remedy to this multiplicity of results is to do a literature review of these empirical studies, possibly by eliminating studies that are perceived to be badly done or by taking a count, of say the number of studies that find that the demand for water has a high price elasticity (in absolute terms) compared to those that find that its price elasticity is low. But these methods are rather rudimentary because, if economic theory says that the price elasticity ought to be high, then it is likely that many investigators will find just that. The problem also arises in the natural sciences: in their experiments researchers tend to find the same values for some scientific constant, until a breakthrough in technology allows someone to decisively affirm that a new value has been found, at which point other researchers also tend to find the same new value, as was the case with the speed of light.

**Meta-regression analysis to the rescue**

For a number of years now, Tom Stanley has been arguing that economists should proceed to meta-regression analysis, as has been extensively developed in psychology, educational research and medicine. Meta-regression analysis (MRA) is simply a regression on regression results and their characteristics. It has been largely developed by economists. ‘Empirical models reported in economic journals are selected from a large set of estimated models. Journals, through their editorial policies, engage in some selection, which in turn stimulates extensive model searching and prescreening’ (Stanley and Jarrell, 1989, p. 161).
There are two problems with the reporting of empirical results. The first problem is that researchers, reviewers, and editors are predisposed to treat “statistically significant” results more favorably; hence they are more likely to be published. Studies that find relatively small and “insignificant” effects tend to remain in the “file drawer”. Such publication or selection biases make empirical effects seem larger than they are (Stanley, 2005a, p. 310). There is a second problem, however, which is of particular concern for heterodox economists: reviewers and editors may be predisposed to accept papers consistent with the conventional view. Researchers may use the presence of a conventionally expected result as a model selection test (ibid.). In other words, investigators will massage the data and search for new specifications until they get statistically significant results that fit conventional wisdom or their own views.

What can be done to gauge the true value of the parameter being investigated and to assess whether or not there is selection bias? At the heart of the identification of publication bias is the notion that investigators relying on smaller samples, with fewer degrees of freedom, will usually face larger standard errors. Hence the estimates of the parameter are likely to be less precise, and hence to be all over the map. Furthermore, to obtain statistically significant effects (say *t* ratios above 1.6), they will need to find large effects (recall that the *t*-statistic is the size of the coefficient divided by the standard error) and this may require quite a lot of tries with different specifications. By contrast, with large samples, the estimates are likely to be more precise. Also the standard errors will be smaller, and hence a statistically significant result can be achieved despite smaller values of the estimated parameter.

All this can be illustrated with the help of a graph, called a funnel plot. Figure 1.5 illustrates two such funnel plots, with the size of the estimated parameter on the horizontal axis, and with a measure of the precision of the estimate *e*, measured by the inverse of the standard error *SE*. The doublets (*e*, *SE*) form a pyramid-looking set, with estimates being more spread out when the degree of precision is weak (when the standard error is high). Figure 1.5(a) shows the case devoid of publication bias: the various estimates (shown on the horizontal axis) are distributed symmetrically on both sides of the presumed true value of the parameter. By contrast, Figure 1.5(b) shows publication bias: the estimates are distributed in an asymmetric way; here most studies provide estimates that are much larger than the presumed true value (of course, we could also have asymmetry, with a large number of studies finding overly large negative values). An example of such asymmetry is research on the effect of an increase in the minimum wage rate on employment. If we express these variables in logs, then the parameter that we are looking for is the percentage increase in employment following a 1 per cent increase in the minimum wage rate. David Card and Alan Krueger (1995), two (until then) respected economists, provoked an economic earthquake when they contended that raising the minimum wage has almost no negative effect on employment, and that previous research was exhibiting publication bias.

While funnel graphs are highly useful, they are only a visual aid. Furthermore, it could be that different estimates are being found because the investigators have included different variables, have used different techniques or have worked on different countries or time periods. These meta-explanatory variables could thus be included in the meta-regression analysis and help explain the variations in parameter estimates. Here, however, we wish to focus only on the relationship between standard error *SE* and the parameter estimate *e*. 
If we invert the funnel graph, now putting the parameter estimate on the horizontal axis and the standard error on the vertical axis, we obtain Figure 1.6, which is the counterpart of Figure 1.5(b).

Figure 1.6 is the exact illustration of the simple meta-regression analysis, which is given by equation (1.16), with \( e_i \), a random term:

\[
e_i = \beta_1 + \beta_0 SE_i + \epsilon_i
\]  

What do the two coefficients \( \beta_1 \) and \( \beta_0 \) of this meta-regression represent? If the standard error \( SE \) is zero, then the estimate \( e \) will be equal to \( \beta_1 \). Thus \( \beta_1 \) represents the estimated true value of the parameter. We can then proceed to standard tests and check whether the null hypothesis \( H_0: \beta_1 = 0 \) can be rejected or not. If it cannot be rejected, then it means that there is no effect. In the case of research on the effect of the minimum wage, this is indeed what Doucouliagos and Stanley (2009) find: using both Card and Krueger’s
data and a much larger number of previous empirical studies, meta-regression analysis shows that increases in the minimum wage have no effect on employment. Thus, ‘in spite of 678 reported statistically significant estimates of minimum wage’s adverse employment effect’ (Stanley et al., 2010, p. 75), no such effect exists.

This in itself is interesting. But now we can focus on the meaning of the $\beta_0$ parameter. This parameter measures the extent of publication bias. If there is no publication selection, the effects measured in the various empirical studies should vary randomly around the true value $\beta_1$, whatever the size of the sample and the standard error of the estimated parameter. Thus we can once more proceed to a standard test and check whether the null hypothesis $H_0: \beta_0 = 0$ can be rejected. If it can, then there is no publication bias in this field of research, or, possibly, the various forces which could lead to publication bias cancel each other out. If $\beta_0 \neq 0$, it means that the less precise studies tend to be more skewed, and hence there is evidence of publication bias. Again, Doucouliagos and Stanley (2009) show that this is the case with research on the effect of the minimum wage on employment.

In reality, because the regression equation given by (1.16) suffers from heteroscedasticity (there is more dispersion from the regression line for some of the observations), as is obvious from Figure 1.6, meta-regression analysis is usually conducted by using equation (1.17) below, which is obtained by dividing equation (1.16) by the individual standard errors $SE_i$, where $t_i$ is the conventional $t$-value of the effect $e_i$. This provides a proper correction to heteroscedasticity, with the intercept and the slope now being reversed, but with the parameters $\beta_1$ and $\beta_0$ keeping their previous meaning.

$$(e_i/SE_i) = t_i = \beta_1(1/SE_i) + \beta_0 + \epsilon_2$$  (1.17)
Evidence of publication bias
The higher the absolute value of $b_0$ in equation (1.17), the greater the publication bias. Doucouliagos and Stanley (2013) look at 87 areas of economic research. They conclude that approximately 60 per cent of these suffer from severe or substantial publication bias.

In microeconomics, the price elasticities of demand for residential water, tobacco, beer, spirits and alcohol all suffer from substantial or severe selectivity problems. They also show that these elasticities are very much overestimated, all of them being much below unity, so that the strength of substitution effects, which is at the heart of orthodox economics, is much weaker than usually described.

In macroeconomics, besides the issue of the minimum wage, the following fields suffer from severe publication bias: the inflation and the output coefficients in the Taylor rule equation of monetary policy; the wage curve (the negative relationship between unemployment rates and wages); business cycle correlations; and the (negative) relationship between unionization and productivity growth. The studies of the effect of central bank independence and inflation, as well as the field of the link between economic reform and economic growth, suffer from substantial publication bias, as does the assessment of the theory of efficiency wages. Krassoi-Peach and Stanley (2009) find that the estimated effect, the wage elasticity of production, corrected by the meta-regression analysis, turns out to be half the literature average measure ($0.30$ instead of $0.61$), which now makes these estimates incompatible with the assumption of profit maximization, which requires that this elasticity be equal to the labour share (in analogy with what we have already discussed regarding neoclassical production functions).

Meta-regression analysis may also be useful even if there is no apparent publication bias. For instance, in the research field of the Ricardian equivalence theorem, that is, the claim that larger deficits will generate lower household consumption expenditures, publication bias is modest. Still, meta-regression analysis permits to falsify this mainstream belief (Stanley, 1998), which played an important role in the debate over the effectiveness of stimulus expenditure programmes during the Global Financial Crisis.

Similarly, with regard to the rate of unemployment, meta-regression analysis shows conclusively that the best studies (those that have the lowest standard errors) find coefficients of persistence closest to unity. This means that, in a simple autoregressive equation, as illustrated by equation (1.18), where $U$ is the rate of unemployment, the coefficient $b_1$ is unity or nearly so:

$$U_t = b_0 + b_1 U_{t-1} + \varepsilon_t$$ (1.18)

This implies that there is unemployment hysteresis and thus that there is no natural rate of unemployment towards which the actual rate would be converging. This, plus another meta-regression analysis (Stanley, 2005b), which rejects the main assumption of the NAIRU hypothesis (that expected inflation leads to a one-on-one increase in the rate of inflation), leads Stanley (2004, p. 605) to conclude that the hypothesis of a natural rate of unemployment has been empirically and definitely refuted.

Doucouliagos and Stanley (2013, p. 318) observe that investigators often follow a ‘stopping rule’. Just like other individuals, they ‘satisfice’: they cease to go through various specifications when they reach ‘what they believe to be the “truth”, or a sufficiently close approximation to it’. The truth, or admissible results, however, will depend
on what economic theory claims. In the fields where theory is contested or where there
are pluralistic views, the range of admissible results will be much larger, and there will be
less selectivity bias. And indeed, this is what Doucouliagos and Stanley (2013) discover.
This further justifies the importance and usefulness of pluralism in economic theory.
Pluralism generates better economics. They further find that studies involving macro-
economics and calculations of demand elasticities are particularly subject to publication
bias. Since various surveys show that there is less consensus in macroeconomics (Fuller
and Geide-Stevenson, 2003), this result must be attributed to the greater role played by
ideology in macroeconomics, again as was demonstrated during the Global Financial
Crisis.

Another intriguing result is that the best-ranked journals are the worst offenders when
it comes to publication bias. Costa-Font et al. (2013) find through a meta-regression that
journals with the highest impact factors (the journals whose articles are most often cited)
publish the studies that most overestimate the elasticities of health care and the use of
prescription drugs. While such a result would need to be confirmed with other meta-
analyses of the sort, there is logic to it. The top academic journals attract more submit-
sions, and thus refuse a greater proportion of them. Thus editors and their reviewers can
demand more spectacular empirical estimates. Those who submit to these journals also
know it, and they will act accordingly. Thus the problems of data-mining, specification
search and reporting bias are likely to be more severe for high-impact journals. The most
highly regarded journals may turn out to be the worst ones!

1.5.6 Summing Up

Most post-Keynesians demonstrate scepticism when it comes to empirical and econo-
metric research. Still, one cannot but be impressed by the huge quantity of empirical
work that seems to provide support for orthodox theory. This section has shown that
this cynicism with regard to orthodox econometric research is largely justified, as many
of the studies that appear to verify or confirm orthodox theory are just artefacts. What
is an ‘artefact’? The most common definition, relevant to science, says that an ‘artefact’,
or ‘artifact’, is a spurious finding caused by faulty procedures. Meta-regression analysis
has certainly demonstrated that many of the empirical proofs of orthodox theory were
phony and arising from defective procedures. The word ‘artefact’ is also used in the
fantasy and sorcery literature. There, an ‘artefact’ is a magical tool with great power, like
a magic wand. This definition seems to be particularly relevant to neoclassical production
functions since all the predictions that can be drawn from a model of perfect competition
cannot be refuted, even when we know that the required conditions do not hold.

Heterodox and post-Keynesian economists can thus develop their own research pro-
gramme and their theories without mental reservations. Their theories are just as scien-
tific as those of the orthodoxy. Indeed, heterodox theories could even be said to be more
scientific since they are founded on realistic hypotheses.
1.6 AN ANTIDOTE TO TINA

The Global Financial Crisis has put in the limelight the huge debate that goes on in economics over whether austerity policies and low-wage policies are needed to achieve sustainable prosperity. On the one hand are those who claim that there is no alternative (TINA); this is the dominant view, which lost ground for a few months when a catastrophe seemed imminent in the midst of the crisis. On the other hand are those who say that there is an alternative; these dissenters are a mixed bag of orthodox and heterodox dissenters.

Orthodox dissenters, such as Joseph Stiglitz, reject many of the free-market policies that were adopted in the 1980s and 1990s or the austerity policies that were advocated during or after the Global Financial Crisis, readily admitting that such policies rest on an oversimplified neoclassical theory. ‘Stiglitz has admitted that his mission all along was to undermine free market fundamentalism from within’ (Mirowski, 2011, p.497). Orthodox dissenters argue that neoclassical models with more reliable and fancy assumptions convincingly demonstrate the limitations and errors of such policies. These critics start from the mainstream model and its tenuous theoretical foundations, on which they superimpose more realistic secondary assumptions. Yet this approach only yields models that are increasingly difficult to grasp. Although such an approach gets the hearing of economists heading large institutions, I do not think that offering a tortuous critique of the dominant view is the best strategy over time.

The purpose of the book is to offer a clear-cut alternative. Post-Keynesian economics is an antidote to TINA. It is much more than a critique of mainstream economics. It provides realistic foundations for proposals of feasible alternative policies.

NOTE