MICROFOUNDATIONS?

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Abstract

It is widely believed by both mainstream and heterodox economists that macroeconomic theory must be based on microfoundations (MIFs). I argue that this belief is unfounded and potentially dangerous. I first trace the origins of MIFs, which began in the late 1960s as a project and only later hardened into a dogma. Since the case for MIFs is derived from methodological individualism, which itself an offshoot of the doctrine of reductionism, I then consider some of the relevant literature from the philosophy of science on the case for and against reducing one body of knowledge to another, and briefly discuss the controversies over MIFs that have taken place in sociology, political science and history. Next I assess a number of arguments for the need to provide macrofoundations for microeconomics. While rejecting this metaphor, I suggest that social and philosophical foundations (SPIFs) *are* needed, for both microeconomics and macroeconomics. I conclude by rebutting the objection that 'it's only a word', suggesting instead that foundational metaphors in economics are positively misleading and are therefore best avoided. Convergence with the mainstream on this issue has gone too far, and should be reversed.

‘Metaphor and analogy can be helpful or they can be misleading. All depends on whether the similarities the metaphor captures are significant or superficial’ (Simon 1962, p. 467).

‘The role of metaphors in science is not well understood. Indeed, the role of metaphors is still controversial on its home ground in language. It should be no surprise that when we metaphorically or otherwise extend literary metaphor to scientific practice, matters become quickly obscure’ (Rosenberg 1994, pp. 406-7).

1 Introduction

In the same week that the registration form for this conference arrived, my friend and near-neighbour Ian McDonald sent me his latest paper. It is an original, thought-provoking and to my mind convincing application of the principles of behavioural economics to the macroeconomic question of the trade-off between inflation and unemployment. McDonald also provides an extremely clear statement of the methodological position that I shall be attacking in this paper: ‘This paper has microeconomic foundations, the *sine qua non* of macroeconomic research’ (McDonald 2008, p. 2). A week later I read Pavlina Tcherneva’s lucid account of the ‘New Consensus’ on fiscal policy: ‘It is an approach *whose defining characteristic* is the claim that choice-theoretic microfoundations determine macroeconomic outcomes’ (Tcherneva 2008, p. 4; stress added). The proposition that macroeconomic research requires microeconomic foundations would command the assent of very many mainstream macroeconomists and a distressingly large proportion of Post Keynesians. I think it is wrong, and it has the potential to do some damage to Post Keynesian macroeconomics. The position that I shall be defending is this: consistency between microeconomics and macroeconomics is desirable, but it does not entail that the former is the *foundation* of the latter. In more general terms, the fact that there is (or may be, or appears to be) some inconsistency between two related bodies of knowledge, A and B, does not entail that A must become the foundation for B, or for that matter that B must become the foundation for A.

This rather obvious proposition is quite routinely denied. I suspect that we have all been to seminars where the presenter, prior to making some interesting and plausible points about the state of the economy, or an intelligent assessment of the effects of macroeconomic policy, apologises for not having provided any microfoundations (hereafter, MIFs) for their judgements. Eminent heterodox economists have made the same confession in print. Thus in his Schumpeter lectures Duncan Foley (2003, p. 30) interrupts a very interesting discussion of classical theories of accumulation to apologise for not having provided microfoundations; the paragraph is headed ‘caveat lector’. My own attempt three years ago to define the core of heterodox macroeconomics led me to set out a minimum programme differentiating it from the mainstream, in response to the accusation of Bradley Bateman (2004) that we had all been attacking a straw man (King 2009). The paper was presented at two well-attended conferences and drew no criticism at all, except on this one point, Tae-Hee Jo disagreeing strongly with my objections to MIFs .[1] Jo also drew my attention to some of the other non-orthodox macroeconomists who take the same line, in particular Amitava Dutt (2006); I have since discovered that there are many others.[2]

[1] The paper was presented at the December 2005 conference of the Society of Heterodox Economists in Sydney and (in an abbreviated form by John F. Henry in my absence) at the September 2006 Post Keynesian conference at the University of Missouri-Kansas City.
[2] See Jo (2006) for a very clear statement of his own position.

It was not always thus. In section 2 I trace the origins of MIFs, which began in the late 1960s as a project and only later hardened into a dogma. Since the case for MIFs is derived from methodological individualism, which itself an offshoot of the doctrine of reductionism, I consider in section 3 some of the relevant literature from the philosophy of science on the case for and against reducing one body of knowledge to another. This section includes a brief discussion of the controversies over MIFs that have taken place in sociology, political science and history. Then, in section 4, I consider a number of arguments for the need to provide *macrofoundations* for microeconomics. While rejecting this metaphor, I suggest that social and philosophical foundations (SPIFs) *are* needed, for both microeconomics and macroeconomics. Finally, in section 5, I try to rebut the objection that 'it's only a word', suggesting instead that foundational metaphors in economics are positively misleading and are therefore best avoided, particularly by Post Keynesians. Convergence with the mainstream on this issue has gone too far, and should be reversed.

2 Some history of economic thought

Consider the following statements by two distinguished macroeconomists (both subsequent Nobel laureates):

> Many of the newly-constructed mathematical models of economic systems, especially the business-cycle theories, are very loosely related to the behaviour of individual households or firms which must form the basis of all theories of economic behaviour….These aggregative theories have often been criticized

> on the grounds that they mislead us by taking attention away from basic individual behaviour. (Klein 1946, p. 93)

> It is notorious that the conventional neoclassical theory of the supply decisions of the household and of the firm are [sic] inconsistent with Keynesian employment models and with the post-Keynesian economics of inflation…It seems clear that macroeconomics needs a microeconomic foundation. (Phelps 1969, p. 147)

Phelps's is almost (but not quite) the first explicit statement of the need for MIFs that I have been able to trace. Although Klein uses a similar metaphor – 'basic' or 'basis' rather than 'foundation' – his solution to the problem that he has identified is quite different from that of Phelps. Klein's 15-page article is devoted to a mathematical analysis of aggregation conditions, since 'the problem of bridging the gap between the traditional theories based on individual behaviour and the theories based on community or class behaviour is, to a large extent, a problem of proper measurement' (Klein 1949, p. 93). Note that the metaphor ('bridging the gap') is now a horizontal and not a vertical one. Klein concludes as follows: 'It is only in models of macroeconomics that we can see through all the complex interrelationships of the economy in order to form intelligent judgements about such important magnitudes as aggregate employment, output, consumption, investment' (*ibid*., p. 108). There is no suggestion of MIFs here.

As Luigi Pasinetti has noted, the Cambridge school of Keynesian economics put macroeconomics before microeconomics. They

> ...proposed an analysis in which the macroeconomic dimension always came first with respect to the microeconomic dimension. The theoretical propositions of each member of the School always avoided starting from subjective behaviour (or preferences) and from the study of single individuals…This does *not* mean a denial of the role of microeconomics as a field of economic investigation, but it *does* mean the impossibility of explaining crucial economic phenomena on the sole basis of microeconomic behaviour. (Pasinetti 2005, p. 843; original stress)

However, a JSTOR search[3] produced the sobering result that the first writer to use the term 'MIFs' in a leading economics journal was Sidney Weintraub, the pioneer of Post Keynesian economics in the United States. It was in fact the key word in the title of his *Economic Journal* article on 'The Micro-Foundations of Aggregate Demand and Supply' (Weintraub 1957).[4] After that the trail goes dead for some years. Fritz Machlup came very close in his book on *Economic Semantics*:

> Needless to say, it is not a duty for every macro-theorist to search for the hidden micro-relations that lie at the root of the macro-relations. No-one is obliged to adapt his scientific curiosity to any methodological norm. To specialize in the construction of macro-models without worrying about the underlying micro-theories is neither unsound nor dishonourable. But to deny that all macro-theory requires a micro-theoretical underpinning, or to deride the efforts of those who do investigate it, would be unreasonable and obtuse.

[3] Incidentally, the results of a search for 'microfoundations' (no hyphen) and 'micro-foundations' (with hyphen) were significantly different.
[4] This, however, is purely an exercise in economic theory, with no explicit methodological pretensions.

> Fortunately such narrow-mindedness is becoming less frequent than it used to be. (Machlup 1963, p. 109)

Here both biological and constructional metaphors are employed ('the root'; 'underpinning'). In the ninth of Machlup's 'Twenty Main Theses' he abandons biology:

> 9. The decision to seek micro-economic explanations of macro-economic generalizations, that is, to search for the micro-theoretical foundations of macro-theoretical propositions, can be interpreted as a recognition of 'methodological individualism' and of the methodological primacy of micro-theory. (*ibid.*, p. 140)

I doubt whether Machlup's book was read by many macroeconomists, though it may have influenced the later complaints of his fellow Austrian émigré Oscar Morgenstern (1972, p. 1184) about the dangers of 'concentration on undifferentiated aggregates', a concern that he traced back to Richard Cantillon. There were a couple of rather casual allusions to MIFs in rather obscure papers in 1966,[5] and in the following year Kenneth Arrow criticised Paul Samuelson for neglecting 'one of the major scandals of current price theory, the relation between microeconomics and macroeconomics' (Arrow 1967, p. 734). Significantly, Arrow did not call for MIFs but instead concluded that 'the Great Depression and the problems of developing countries

[5] Summarising his doctoral thesis on the term structure of interest rates, Liang-Shing Fan (1966, p. 131) criticised 'the lack of proper micro-foundations' in existing interest rate theory, and P.A. Neher (1966, p. 855), commented on the 'extremely shaky' nature of 'the micro foundations [two words] of the agricultural sector's private investment function' in some contemporary theories of economic development.

remind us dramatically that something beyond, but including, neoclassical theory is needed' (*ibid*., p. 735).

Almost certainly the debate on MIFs was kick-started by Phelps in 1969 in the article that I have already cited. (Interestingly almost nothing is said about MIFs in the influential volume that he co-authored in the following year, even though it bore the title *Microeconomic Foundations of Employment and Inflation Theory*). One very important milestone was the 1975 S'Agora conference of the International Economic Association, though the published proceedings suggest that for much of the time the participants simply talked past each other (as was noted by Weintraub 1978). Only one of the conference papers (by the Austrian, Erich Streissler) was explicitly devoted to the question of MIFs. Other presentations, and the transcript of the informal discussions that followed, indicate some uncertainty on the part of both mainstream economists and Post Keynesians. Both Edward Nell (Harcourt 1977, pp. 392-4) and Yves Younès (pp. 378-9) denied the need for MIFs altogether, while Luigi Spaventa (p. 137) alluded to the fallacy of composition in Streissler's claim that banks could create credit 'out of thin air'. Takashi Negishi mixed his metaphors, combining the vertical with the horizontal: 'the microeconomic foundation of macroeconomics meant a bridge or at least some stepping stones over the water between the temporary equilibrium analysis of general equilibrium theory and the short-run Keynesian theory of involuntary unemployment' (p. 380). Earlier in the proceedings Negishi had stated that 'the most important constraint to be considered was the constraint of effective demand. Perhaps we should really be talking about the macroeconomic foundations of microeconomics!' (p. 142).[6] Apart from Streissler, the clearest endorsement of MIFs came from Paul Davidson (pp. 313-14, 391-2), who insisted that 'Marshall-Keynes

[6] The exclamation mark is in the original. For his second thoughts on these matters, see Negishi (1979).

microeconomic theory' (p. 314) had in fact already supplied the necessary microfoundations for Post Keynesian macroeconomics. The other Post Keynesians at the meeting (Tom Asimakopulos, Geoff Harcourt and Mario Nuti) did not address the issue.

Over the next twenty years MIFs became a juggernaut, which most mainstream and Post Keynesian economists were unable to resist, even if they had wished to, as it rolled over them. A number of prominent Post Keynesians endorsed (or were supposed to have endorsed) the need for MIFs, beginning with Michal Kalecki. This position is attributed to him[7] by several authors, including his first biographer, George Feiwel (1975, p. 93), Alberto Chilosi (1989**)** and Jerry Courvisanos (1996, p. 26). In the United States, where I have already noted the commitment of Weintraub and Davidson, a very similar position was taken by Alfred Eichner, who believed that his own model of oligopoly pricing and investment behaviour by the 'megacorp' supplied the 'micro foundations of the corporate economy' (the title of chapter 2 of Eichner 1985), and thus of his entire macrodynamic analysis. In Britain both Philip Arestis and Geoff Harcourt concurred. Thus Arestis (1992, p. 139) adopted the Eichner model as 'our micro-foundations of Post-Keynesian economics', while Harcourt told me, in a 1993 interview, that 'the microfoundations of macroeconomics ha[s] been my most sustained interest all my life' (King 1995, p. 181).[8]

Post Keynesian dissenters were very much in the minority. They included Kurt Rothschild (1988) and Peter Kriesler (1989), whose interpretation of Kalecki will be considered in section 4. Rothschild's argument is particularly interesting, as his celebrated

[7] Wrongly, I think; see section 4 below.
[8] *Mea culpa*! Several of the Post Keynesians whom I interviewed for this book referred approvingly to MIFs, without challenge, and I myself criticised the neoclassical nature of Keynes's microfoundations in the *General Theory* (King 1995, p. 3).

1947 paper on price theory and oligopoly is sometimes seen as an early exercise in Post Keynesian MIFs. [9] His case against MIFs was based on the virtues of methodological pluralism in a complex and uncertain world:

> A freedom from narrow methodological prescriptions has often proved to be an essential precondition for new insights and the birth of new theories; and this is no less true for the natural sciences than for the social sciences. The greater the complexity of a phenomenon and its interrelated elements, the greater the importance of attacking the problem from different angles and with a variety of methods. (Rothschild 1988, p. 17)

Rothschild was unconvinced by mainstream accusations of ad hocery, which, he argued (citing the philosopher Paul Feyerarbend),'is not a theoretical weakness but can be…a necessary element in the difficult stages of developing new theories or extending old ones' (*ibid*., p. 17). In a slightly different vein, Victoria Chick (2002, p. 55) argued that 'consistency between a theory of decision making (microeconomics) and the overall outcome of decisions (macroeconomics) cannot, in general, be achieved. Some "slippage", some compromise of internal consistency, is bound to arise'. Keynes's theory of investment, she concluded, was precisely such a compromise, which 'favours his macroeconomics, allowing him to determine the properties of equilibrium in his system while giving a result for disequilibrium also' (pp. 66-7). Chick therefore rejected the possibility of 'impeccably logical microfoundations', which could be provided only at the expense of 'the logic of the whole' (p. 55).

These were minority voices. Surprisingly, resistance to MIFs was more evident among mainstream economists. The monetarist David Laidler, for example, defended the autonomy of macroeconomics: 'Microfoundations may be interesting in their own right', as one critic summarised Laidler's position, 'but from a macroeconomic perspective they are technical details which can safely be left for later. In contrast it was Hicks' view, shared by

[9] See for example Harcourt in King (1995, p. 181).

this reviewer, that monetary theory is largely a waste of time until these microfoundation issues are addressed squarely' (Bryant 1985, pp. 122-3, reviewing Laidler 1982).[10] Some New Keynesians were equally sceptical about the need for MIFs. Alan Blinder (for example) criticised Robert Lucas on precisely this question: 'Must we be restricted to microfoundations that preclude the colossal market failures that created macroeconomics as a subdiscipline?', Blinder asked. 'This is a judgement call', he concluded,

> [B]ut I judge the Keynesian approach more scientific. First, good science need not always be built up from solid microfoundations. Thermodynamics and chemistry, for example, have done pretty well without much micro theory. Boyle's Law applies directly to aggregates, much like the marginal propensity to consume. And the microfoundations of medicine are often very poor; yet much of it works. Empirical regularities that are formulated and tested directly at the macro level *do* have a place in science. (Blinder 1987, p. 135; original stress)[11]

In his *New Palgrave* entry on 'Macroeconomics: relations with microeconomics', Peter Howitt argued in similar vein:

> Thus the quest for microfoundations has been a mainspring of development in macro theory. However, this does not mean that macro has been developing into a branch of applied micro. The forces tending to make macro theory conform more closely with micro principles have been opposed by equally important forces requiring those principles to be modified radically before being applied to macro questions.
>
> More specifically, what has restrained the urge to apply micro principles is a widespread recognition that some of the most important phenomena manifest defects

[10] Hicks was present at, and partly responsible for, the S'Agora conference. Drawing a rather long bow, it is possible to trace the argument for MIFs right back to *Value and Capital* (Bronfenbrenner 1981).
[11] Blinder had changed his mind on this issue (cf. Blinder 1982, where he included MIFs in the title and raised no objection to the concept).

> in the economic system that standard micro theory rules out with its basic assumption of equilibrium. (Howitt 1987, p. 273)

Howitt's conclusion was optimistic:

> The disunity between micro and macro that has motivated so many contributors is shrinking rapidly on the frontiers of research where micro theory is being transformed by the explicit consideration of informational problems like those so often adduced by macroeconomists and where macroeconomics without specific reference to individual transactors, their decision problems and conditions of equilibrium is becoming increasingly rare. (*ibid*., p. 275)

There is no foundational metaphor here. Subsequent expressions of hostility towards or doubt concerning MIFs can be found in the writings of other well-known mainstream – or non-heterodox – economists, including Olivier Blanchard, who complained about 'the quasi-religious insistence on micro foundations' (1992, p. 126), Huw Dixon (1994), Kevin Hoover (1995, pp. 729-31) and Roger Backhouse (2000).

Why, then, did so many Post Keynesians come to endorse the MIFs metaphor? At first, I suspect, it was because it appeared to offer an additional, powerful weapon with which to attack the mainstream ('our MIFs are better than yours'; 'oligopoly is easier to reconcile with the principle of effective demand than perfect competition'). This analytical confidence and self-assertion is very clear in Davidson's contributions to the S'Agora debates, cited above. The worries expressed more recently by Dutt, Jo and others suggest that this confidence was misplaced. The current Post Keynesian quest for MIFs, I think, is a defensive reaction to an increasingly aggressive and dogmatic economics profession: MIFs seem to have become essential if Post Keynesians are to engage the mainstream (and get published in any of the leading journals). Given the relatively underdeveloped state of heterodox microeconomics, this is unfortunate. As I shall argue in section 5, by accepting the need for MIFs Post Keynesians turned out to have put themselves at a real competitive disadvantage.

3 The Perils of Reductionism

'Intertheoretic explanation, in which one theory is explained by another theory, usually formulated for a different domain, is generally termed *theory reduction*' (Schaffner 1967, p. 137; original stress). In order for the MIFs dogma to be correct, three propositions concerning reduction must be true. First, reduction is possible (and mandatory). Second, reduction must take place in the vertical (and not the horizontal) plane. Third, vertical reduction must be downwards (from macro to micro), not upwards (from micro to macro). All three propositions are dubious. I shall deal with the first two in this section, leaving the third for section 4.

There is a very substantial literature in the philosophy of science on proposition 1. Almost thirty years ago William Wimsatt's survey article contained 132 references, all of them on the question of reduction in the natural sciences. He himself was broadly favourable to the principle of reduction, but with significant reservations (Wimsatt 1979, p. 361). I suspect that a full bibliography of the relevant literature since then would contain many hundreds of items, especially if it included work on reduction in the social sciences. This is not the place for such a survey, even if I were competent to provide one (which I am not). Instead I will note a few important contributions, before summarising in a little more detail the recent work of

Steven Horst (2007), who presents a convincing case against reduction, with specific reference to the reduction of psychology to the physiology of the brain.

Possibly the first philosopher of science to pay serious attention to the issue of MIFs in economics was Alan Nelson, who concluded that the project was most unlikely to succeed; he also raised the possibility that macrofoundations might be provided for microeconomics (Nelson 1984, p. 586). A very thorough critique of reduction in social science more generally was made by Rajeev Bhargava (1992) in the course of a systematic attack on the principle of methodological individualism. Five years later Harold Kincaid was also highly critical of reductionist strategies in social science, at least when applied to explanation rather than ontology. As he noted, wryly, 'The fallacious inference from "W is composed of P" to "W is fully explainable in terms of P" seems irresistible' (Kincaid 1997, p. 6). Finally, Theo Kuipers rejected both radical reductionism, 'the belief that every macro-concept and macro-law can be reduced', and radical holism, 'the belief that all (interesting) concepts and laws of the domain cannot be reduced' in favour of 'restricted reductionism', 'the belief that some concepts and laws may be reducible, but others may not be'. In this spirit he endorsed a 'mixed strategy', which 'favors reduction when possible. Roughly speaking, in the mixed strategy one describes the macro-phenomena and their possible relations in macro-terms, and tries to explain them in micro-terms as far as possible, and hence in macro-terms as far as necessary' (Kuipers 2001, p. 156).

For Steven Horst, however, any form of reduction represents an outdated remnant of 1950s philosophy of science. 'Often discussed cases of failed or incomplete intertheoretic reduction in the literature' (Horst 2007, p. 59) include the reduction of thermodynamics to statistical mechanics; the reduction of thermodynamics/statistical mechanics to quantum mechanics; the reduction of chemistry to quantum mechanics; the reduction of classical mechanics to quantum mechanics; the reduction of evolutionary biology to genetics; and the reduction of genetics to molecular genetics. Horst concludes that ‘explanation is not generally derivation, and indeed the majority of scientific explanations cannot even be successfully *reconstructed* as reductions’ (p. 53; original stress). None of these examples is taken from the social sciences; Horst’s own interests are in the philosophy of mind, and he has nothing to say about economics. Unknowingly giving support to Kurt Rothschild’s methodological pluralism, however, he argues for ‘the methodological autonomy of the special sciences’ with ‘a plurality of explanatory types’; for ‘a variety of separate good-making qualities or explanatory virtues, each of which contributes to the epistemic quality of scientific understanding’; and for ‘a variety of interdomain relations’ (pp. 56-8; original stress deleted). Horst concludes that there are ‘a variety of fruitful ways that two scientific domains can come into contact with one another, but that fall far short of the kind of derivation relation that is distinctive of broad reduction’ (p. 60). Whether this entails the fundamental ‘disunity of science’, as Horst himself suggests (p.60), or instead vindicates Kincaid’s (1997, p. 1) picture of ‘a non-reductive yet unified science’ remains open, along with the even more intriguing question as to whether macroeconomics and microeconomics (now) constitute two *separate* – but related – ‘special sciences’.

So much for proposition 1: there is a big question mark over the possibility of successful theory reduction, and no good reason to make it mandatory. Proposition 2 fares no better. Even if intertheoretic reduction were possible, and mandatory, it need not be vertical, since it could legitimately take place in the horizontal plane. The very word ‘reduction’ seems to deny this possibility, but it turns out to be a rather slippery term.[12] Its common meaning, of course, is to make smaller, to diminish in number or size. This is the way in which it is used in arithmetic, and also in cookery (where 'reducing' a stock or sauce means boiling off some of the liquid, leaving a smaller volume). The *Shorter Oxford Dictionary*, however, gives no less than ten definitions of 'reduction' and 23 of 'reduce', including the one that is most relevant to the philosophy of science: 'decompose or resolve (a compound) into a simple compound or its constituent elements'. This is taken from chemistry, where it is apparently now obsolete and has been replaced by a rather different meaning: 'cause to combine with hydrogen or to undergo reduction; add an electron to, lower the oxidation number of, (an atom)'. No clear diminution in number or quantity is implied here, and hence no necessary movement from higher-level entities (the 'macro') to lower-level entities (the 'micro'). It is not difficult to think of 'reductions' that operate in the opposite direction. For example, in vulgar Marxism (and sometimes also in more sophisticated versions), an individual's beliefs are explained in terms of her class position, in the context of the hegemonic ideology of capitalist society: 'in the final analysis', that is to say, such individual characteristics as national identity or religious affiliation are attributed to the demands of capital accumulation (Halliday 1992). There is a long tradition of this form of reductionism, going back in the case of religious beliefs to the literature inspired by Weber and Tawney, and while you may find it unconvincing it is not obviously absurd or contradictory. It is a legitimate form of reductionism, and it operates *upwards*, from micro to macro. Another example comes from socio-biology. There is some difficulty in providing a Darwinian explanation of altruistic behaviour: how are the genes of an unselfish individual transmitted from one generation to the next when such an individual is more likely to be killed and eaten than a non-altruistic

[12] At least in English. It would be instructive to know if similar complications arise in other languages.

member of the species? One answer runs in terms of group selection. Groups made up of unselfish individuals are more likely to survive than groups dominated by selfish ones (Kropotkin 1902; van den Bergh and Gowdy 2003, pp. 71-3). Again, this may as a matter of fact be incorrect, but it is not absurd or contradictory, and it involves a reduction upwards, this time from the individual to the group. You can probably think of other examples. I shall have more to say in section 4 about upward reduction in economics.

In practice, *downward* reduction is certainly more widespread and more influential. In the social sciences this is normally associated with the principle of methodological individualism. The case against it was stated very clearly, forty years ago, by the philosopher Steven Lukes (1968) in a short paper in the *British Journal of Sociology*.[13] Lukes defines methodological individualism as ‘a prescription for explanations, asserting that no purported explanations of social (or individual) phenomena are to count as explanations, or (in Watkins’s version) as rock-bottom explanations, unless they are couched wholly in terms of facts about individuals’ (Lukes 1968, p. 121). He distinguishes four types of ‘facts about individuals’ or ‘individual predicates’. Type (i) predicates ‘are about human beings *qua* material objects and make no reference to and presuppose nothing about consciousness or any other feature of any social group or institution’ (pp. 123-4); examples include genetic make-up and brain states. Predicates of type (ii) ‘presuppose consciousness but still make no reference to and presuppose nothing about any feature of any social group or

[13] While he notes that methodological individualism originated with Thomas Hobbes and has been sporadically influential ever since, Lukes does not explain why the question should have become important to social theorists at this time. It was surely too early for a rebuttal of economics imperialism to be required; in the mid-1960s Gary Becker was largely unknown even to economists outside Chicago. Possibly concern with methodological individualism arose out of the contemporary controversy between Marxists and Weberians on bureaucracy and related issues – a very hot topic at the time. As Mike Howard reminds me, Weber was not a consistent methodological individualist; sometimes he was as structuralist, or materialist, as Marx.

institution' (p. 124); they include aggression, gratification and stimulus-response. Type (iii) predicates 'do have a minimal social reference: they presuppose a social context in which certain actions, social relations and/or mental states are picked out and given a particular significance' (p. 124), for example co-operation, power and esteem (also utility maximisation?). Finally, type (iv) predicates 'are maximally social, in that they presuppose and sometimes directly entail propositions about particular types of groups and institutions (p. 124), for example cashing cheques, saluting and voting.

Lukes's case against methodological individualism is that it is either incredible or innocuous:

> If the methodological individualist is saying that no explanations are possible (or rock-bottom) except those framed exclusively in terms of individual predicates of types, (i), (ii) and (iii), i.e. those not presupposing or entailing propositions about particular institutions and organizations, then he is arbitrarily ruling out (or denying finality to) most ordinarily acceptable explanations, as used in everyday life, but also by most sociologists and anthropologists for most of the time. If he is prepared to include individual predicates of type (iv), he seems to be proposing nothing more than a futile linguistic purism. Why should we be compelled to talk about the tribesman but not the tribe, the bank-teller but not the bank?' (p.125)

A distinction might be drawn between 'strong versions of methodological individualism, which suggest that all should be explained only in terms of individuals and their interaction, and weak versions of methodological individualism, which also assign an important role to social institutions and/or social structure in social science explanations' (Udehn 2002, p. 479). It is doubtful whether the weak version really qualifies as methodological individualism at all; it would certainly not satisfy those mainstream economists who demand MIFs. The case against the strong version of methodological individualism has recently been restated by Geoff Hodgson (2007) in terms very similar to those of Lukes. I think it is a very convincing one.

Despite these serious weaknesses, methodological individualism has proved influential not only in economics but also in other social sciences. By the 1980s it was being re-branded as the requirement for MIFs, and in sociology it proved highly contentious. Randall Collins's influential article 'on the microfoundations of macrosociology' may have borrowed the term MIFs from economics, but it described a very different project. It originated in the ethnomethodological research of Harold Garfinkel and other 'radical microsociologists', who had studied 'real-life interaction in second-by-second detail' (Collins 1981, p. 84). One product of this new field of 'social phenomenology' was 'its discovery that actual everyday-life microbehavior does *not* follow rationalist models of cognition and decision-making' (p. 985; stress added), which should therefore be discarded. Collins concluded that it had given

> …a strong impetus toward translating all macrophenomena into combinations of micro-events. A micro-translation strategy reveals the empirical realities of social structures as patterns of repetitive micro-interaction. Microtranslation thus gives us a picture of the complex levels of abstraction involved in causal explanations'. (p. 985)

Evidently these ‘translations’ do not amount to MIFs as understood by mainstream macroeconomists, who have no time for ‘complex levels of abstraction’.[14]

Another important landmark was the publication of a volume edited by Michael Hechter (1983) with the same title as Collins’s article; it was reviewed in all the leading sociological journals. Contributors claimed that ‘rational choice models are useful for the explanation of macrosociological phenomena’ (Cohn 1985, p. 221), and offer ‘a fruitful guide for conducting empirical research’ (Opp 1985, p. 362). But the claims that the sociologists were making for their MIFs were much less ambitious than those of the economists:

> It is not entirely clear…whether we are being asked to regard rational choice theories as merely complementary to structural or normative social theories (p. 8), or stand-alone and ‘better explanations of key macrosociological problems’ (p. 10). Hechter waivers between these two positions and stops a long way short of views like those of Chicago economist Gary Becker. Becker claims that the method of economics…explains any and every aspect of social and economic behaviour, and eliminates the need for other theories therefore. (Dex 1985, p. 301).

Like Shirley Dex, Karl-Dieter Opp concluded that the claims of the economists were unwarranted: ‘rational choice explanations may incorporate many sociological variables as specific preferences and constraints. Thus, the approach chosen by the authors does not lead to the abandoning of sociology’ (Opp 1985, p. 362). Charles Tilly (1985) and Randall Collins (1985) were equally sceptical concerning the

[14] Indeed, as Michael Schneider has observed, the ‘micro’ that is used in MIFs is profoundly impoverished, excluding the firms, industries and markets that most working (that is, applied) microeconomists would still regard as their stock in trade.

prospects for MIFs in sociology. Only George Homans (1985, p. 877) endorsed the attempt to '[explain] social phenomena by general propositions about the behaviour of individuals as members of the human species…Horrible as this may seem to many, the theory is inherently reductionistic: it reduces the social to the individual'. Not that Homans would bring much comfort to mainstream economists, for he was an opponent of maximising models and favoured replacing the label 'rational choice theory' with the more accurate tag of 'behavioral-cognitive psychology' (p. 879); his version of MIFs was thus much closer to Ian McDonald than to Gary Becker.

Homans was, in any case, part of a fairly small minority. Most sociologists would have agreed with Geoffrey Ingham that

> …sociology differs from mainstream economics in the following ways:
> (i) social and economic structures have properties that cannot be reduced to those of individuals taken singly;
> (ii) social and economic action has 'meaning' that cannot be reduced to the calculus of want satisfaction or utility maximisation;
> (iii) social and economic life is based upon power (and associated) 'asymmetries' and inherent uncertainty. (Ingham 1996, p. 252)

Where they do still refer to the 'microfoundations of sociology', sociologists seem to be making much less ambitious claims, methodologically speaking, than the economists. Thus Karen Cook (2000, p. 685) uses the term to encompass 'theoretical perspectives developed at the micro level that link in different ways to more macro levels of sociological analysis…[and] highlight the linkages between individual behaviour and social structure emphasizing interdependence and the social context of interaction'. These are not MIFs in the economists' sense of that term, and it is significant that the vertical metaphor has dissolved into a horizontal one: 'linkages' are not foundations.

In political science the most prominent advocates of MIFs have been the Analytical or Rational Choice Marxists,[15] who 'argue that class analysis requires microfoundations at the level of the individual to explain why and when classes are the relevant unit of analysis' (Roemer 1982, p. 513). The same principle has been extended to the explanation of banditry, peasant revolutions and other forms of social change (Taylor 1988), and to questions of identity (Little 1998). While Roemer and some of his collaborators are inventive practitioners of Walrasian economics, and endorse the MIFs project in its entirety, this is not true of all (or perhaps most) of the political scientists who have written on the subject. Daniel Little (1998, pp. 7, 10), for example, makes the more modest claim that a 'macro-explanation is insufficient unless it is accompanied by an analysis at the level of individual activity – a micro-analysis – which reveals the mechanisms which give rise to the pattern to be explained', so that 'an assertion of an explanatory relationship at the social level (causal, functional, structural), must be supplemented by' micro explanations. This rather innocuous statement is, however, immediately followed by a 'stronger version':

> …we must have at least an approximate idea of the underlying mechanisms at the individual level if we are to have a credible hypothesis about explanatory social regularities at all. A putative explanation couched at the level of high-level social factors whose underlying individual-level mechanisms are entirely unknown is no explanation at all (Little 1998, p. 10)

[15] See Howard and King (1989) and Weldes (1989) for extended critiques.

But Little proceeds to use both horizontal and vertical metaphors,[16] and at one point he concedes that ‘there may be legitimate forms of macro-explanations in social science which are not subject to these specific criticisms; and in that case microfoundational arguments would be silent’ (p. 13).

Something rather similar to Little’s ‘weaker version’ of MIFs was detected by one reviewer of Michael Taylor’s (1988) edited volume on rational choice models of revolution: ‘Though none of the authors views structural variables as wrong, each sees structural-determinist models as incomplete without satisfactory microfoundations that make the actions of revolutionary participants intelligible or rational…A recurring theme in most of these essays is that structural-determinist models should be *complemented* , not replaced, by rational-choice explanations’ (Downing 1990, pp. 679-80; original stress). Again, these are not MIFs as mainstream economists would understand them.

In historical scholarship, too, there has been some discussion of ‘the micro-macro link’, or ‘the microfoundations of macrotheory’. While historians are aware of the debates over MIFs in economics and sociology, they have been generally unwilling to follow the lead of the economists. ‘In new microhistory the link between micro and macro levels is not a simple reduction or aggregation. The movement from one level or sphere to another is qualitative, and generates new information’ (Peltonen 2001, p. 357). ‘Words like “link”, “nexus” or “foundation”’ (p. 359) are often used by historians, indicating some confusion in the use of vertical and horizontal metaphors. When the historian ‘crosses over boundaries’ between micro and macro there is often ‘a double bind’ (p. 357), since if the micro is either typical or exceptional no new

[16] Micro and macro phenomena are ‘linked together’ (p. 12); the macro is ‘based on’ and ‘grounded in’ the micro (p. 13).

information is generated. Matti Peltonen concludes that ‘historical study is, at least in methodological questions, an independent and original mode of research’ (p. 359).

Mainstream economists, then, have largely failed to convince other social scientists of the merits of their crusade for MIFs. As the example of George Homans demonstrates, they have something to fear from economics imperialism, as well as much to gain. Further unintended consequences should also be of some concern to them. Even if the principle of downward reduction is accepted, there is no obvious reason why it should stop at the individual.[17] Why not go down even further, to the gene (Dawkins 1991) or the subatomic particle (Wilson 1998)?[18] If this seems fanciful, consider the implications of the burgeoning science of neuroeconomics, which seeks to explain individual behaviour through studying small areas of the individual’s brain and seems to have discovered that there are distinct neurological grounds for doubting the relevance of ‘rational economic man’ (Camerer, Loewenstein and Pelec 2005). And mainstream macroeconomists have more to fear than most. As the labour economist Daniel Hamermesh recently observed, ‘the creation of a micro-foundation for macro means that it is now an applied field, no longer central’. Hence it should be removed from the core of postgraduate training in economics: ‘the atavistic inclusion of macro in the core may be based less in intellectual rigor and more in the job-security concerns of macroeconomists and their Ph.D. students’ (Hamermesh 2008, p. 409).

4 Macrofoundations for Microeconomics?

[17] Howard and King (1989) made precisely this point almost twenty years ago in a critical appraisal of Rational Choice Marxism.

[18] Even this is full of problems. As Herbert Simon (1962, p. 468) once noted, in the context of physics rather than economics, ‘elementary particles have a disconcerting tendency not to remain elementary very long’.

Finally we arrive at proposition 3. Negishi's (tongue in cheek?) proposal that we might need macrofoundations (hereafter, MAFs) for microeconomics was noted in section 2. There are good reasons for taking it seriously, first because what may be termed the prevailing macroeconomic regime has profound implications for individual behaviour, and second because of the irreducibly macroeconomic nature of many important problems.

The first point was neatly made by David Colander, whose 'Post Walrasian perspective maintains that before there is any hope of undertaking meaningful micro analysis, *one must first determine the macro context within which that micro decision is made*' (Colander 1996, p. 61; original stress). The macro context has both a normative and a positive dimension (if I may be permitted to use this contentious terminology). Ian Mcdonald (2008, p. 5) cites a well-known neoclassical text by Tibor Scitovsky, the subtitle of whose *Welfare and Competition* is *The Economics of a Fully Employed Economy*. Right at the start Scitovsky notes that when

> a scarce resource becomes underemployed, it no longer matters whether it is used efficiently or not…it does not matter if too large a proportion of underemployed resources is devoted to the satisfaction of one particular want, because this again results in less unemployment and not in the lesser satisfaction of other wants. Efficiency in the use of underemployed scarce resources is as irrelevant as it is in the administration of free resources, and for exactly the same reason (Scitovsky 1952, p. 9).

The early Keynesians also believed that a 'full employment economy' would be quite different from an 'unemployment economy', especially in the labour market, where full employment posed a potentially very serious threat to ‘discipline in the factories’ (Kalecki 1943).

I think this important point can be taken much further. Perfect competition in the labour market is only *possible* under full employment, since only then is it possible for the supply of labour to the individual firm to be perfectly elastic; only then does the worker have no reason to value her job; only then is there no cost to losing that job; only then are there no wage rents. In an unemployment economy, all employers have some degree of monopsony power, and this has a number of important consequences (Manning 1994).[19] Interestingly enough, the opposite is probably true in the product market, where generalised excess capacity is probably a necessary condition for perfect competition between suppliers. If customers are uncertain that their orders will be filled immediately by hard-pressed producers in a totally impersonal market, they will face the prospect of substantial ‘shopping costs’, and will therefore have a strong incentive to form long-term relations with their regular suppliers, whose demand curves will in consequence be downward-sloping, not perfectly elastic (Okun 1981, pp. 138-55).[20] Thus the condition required for perfect competition in the labour market (full employment) is inconsistent with that needed for perfect competition in the product market (excess capacity), and this conclusion is derived from macroeconomic considerations. MAFs for microeconomics!

A more familiar argument for MAFs involves the fallacy of composition: a statement that may be true of any individual considered separately may be false when applied to them all taken together. There are some obvious cases in everyday life:

[19] There are, of course, other reasons to doubt the widespread existence of perfect competition in the labour market, even under full employment (Kaufman 2007).
[20] Oddly, Okun does not mention the cyclical nature of shopping costs. The value to the supplier of repeat orders, which he does emphasise, presumably works in the opposite direction, being greatest when the market is slack.

with the introduction of all-seater football stadiums in the UK after the 1989 Hillsborough disaster, any individual supporter who stands up to get a better view does indeed get one, but if they all stand up no-one's view is improved. In Keynesian macroeconomics the standard example is the *paradox of thrift*: a decision by any individual to save a larger proportion of her income will lead to more saving by that individual, but (in the absence of increased investment) this will not be true of an increase in everyone's savings propensity, which will simply reduce their incomes and leave the volume of aggregate saving unchanged.[21] The Kaleckian *paradox of costs* is rather similar: a wage rise is very bad news for any individual capitalist, but it may be good news for them all, taken together, if the consequent rise in consumption expenditure raises the level of economic activity and thereby increases aggregate profits. *Kalecki's profit equation* offers a third example: in a closed economy with no government, and on the assumption that workers do not save, aggregate profits are equal to and determined by the sum of capitalist expenditure on consumption and investment. Thus 'capitalists get what they spend', but only as a class; any capitalist who thinks that it applies to him as an individual will end up in jail, like Conrad Black. I offer one more example (there will certainly be others). Nicholas Kaldor's defence of his macrodistribution theory against the Modigliani-Samuelson objection that it relied on special assumptions about parameter values was that:

> A capitalist system can only function so long as the receipts of entrepreneurs exceed their outlays; in a closed system, and ignoring Government loan expenditure, this will only be the case if entrepreneurial expenditure exceeds

[21] Janssen (1993, pp. 87-9) claims that the paradox of thrift is not inconsistent with MIFs, but his argument is, to say the least, rather obscure.

> workers' savings. Unless one treats the consumption expenditure of entrepreneurs as an exogenous variable, given independently of profits, it is only the 'Kaldor-Pasinetti inequality' (i.e the excess of business investment over non-business savings) which can ensure the existence of profits. (Kaldor 1978, p. xvi)

These are all cases where individual behaviour is governed by macroeconomic requirements. The logic of our macroeconomic analysis tells us that, in aggregate, saving cannot increase unless investment rises; under some circumstances increased wages may lead to higher profits; total profits always depend on total spending; and individual saving decisions must be consistent with the survival of capitalism. None of these results is immediately obvious, or could be derived from knowledge of microeconomics alone. They are all examples of what sociologists (following Talcott Parsons) call *emergent properties*, and they strongly indicate the fallacy of MIFs. In complex systems,

> …the whole is more than the sum of the parts, not in an ultimate, metaphysical sense, but in the very important pragmatic sense that, given the properties of the parts and the laws of their interaction, it is not a trivial matter to infer the properties of the whole. In the face of complexity, an in-principle reductionist may be at the same time a pragmatic holist. (Simon 1962, p. 468)

Whether this establishes a case for MAFs is another matter. As Peter Kriesler suggests, there are two hypothetical extremes:

> [W]e can regard the first as seeing macroeconomics as a pure aggregation from the micro, with no new information resulting from the aggregation that is not already in the microtheory. On the other hand, the second view can be characterised as regarding the micro as a pure disaggregation from the macro, with no new information about the functioning of the economy being generated by the procedure. (Kreisler 1989, p. 123)

These are the two positions that I refer to as the arguments for MIFs and for MAFs. Kriesler argues – correctly, I think – that neither extreme accurately represents Kalecki's views. On the contrary,

> [T]he way in which micro and macro theories are interrelated in Kalecki's analysis is quite different to either of these two approaches. In particular, neither theory dominates nor forms a constraint on the other. Rather than any form of hierarchical relationship, *the two theories lie side by side* (so to speak), and both give information which the other cannot give, while the interrelation of the two yields further information not obtainable from either in isolation. (Kriesler 1989, p. 123; stress added)

I shall return to this useful horizontal metaphor in section 5.

I do not wish to be misunderstood. Rejection of MIFs does not entail acceptance of 'the doctrines of universalism, conceptual realism, holism, collectivism, and some forms of *Gestaltpsychologie*', according to which 'society is an entity living its own life, independent of and separate from the lives of the various individuals, acting on its own behalf and aiming at its own ends which are different from the ends sought by the individuals' (von Mises 1949, p. 145).[22] Neither does it mean that economics needs no foundations whatsoever. On the contrary, I think a strong case can be made for *social and philosophical foundations* (hereafter, SPIFs). Both macroeconomists and microeconomists need to be aware that they are attempting to model capitalism, not simple commodity production. There are (at least) two classes of agents, capitalists and workers, and it is the former who own the means of production and control the production and sale of commodities. Production is motivated by profit, not – at least, not directly – by the utility functions of asocial, classless 'consumers'. Since profit is by definition the difference between revenue and costs, that is, the difference between two sums of money, it is pointless to model a capitalist economy in terms of barter. These *social foundations* of any meaningful economic theory are exceedingly obvious, but they are routinely violated in the MIFs that the majority of mainstream economists are so proud of.

There will perhaps be less agreement among Post Keynesians when it comes to the *philosophical foundations* of economics (and any other form of scientific activity), but I think a strong case can be made for some form of scientific realism, whether it is the finely detailed Critical Realism defended by Tony Lawson (2003) or the rather looser version derived from the writings of Rom Harré (Keat and Urry 1975). Either way, most of us would concur in rejecting Friedman-style instrumentalism, on the one hand, and the more extreme postmodernist forms of relativism, on the other, and would agree with Kaldor that economic models are 'not intended to "explain" non-existing phenomena of a purely imaginary world: the purpose of the static abstraction, here and elsewhere, is to enable us to isolate the relevant factors from the irrelevant ones *in the world as it exists*' (Kaldor 1955, p.

[22] I cannot think of anyone who still asserts this Hegelian absurdity, if indeed anyone ever did.

158). To repeat: 'Scientific hypotheses are invented in order to account for the phenomena actually observed' (*ibid.*, p. 158).

Kaldor also offers a salutary example of the difficulties caused by confusing MIFs and SPIFs. In the final years of his life he became convinced that the New Keynesian economist Martin Weitzman had

> .demonstrated that constant returns to scale, strictly interpreted, are a sufficient condition for the absence of involuntary unemployment. The latter arises because a worker who is not offered a job cannot turn himself into his own employer (in the manner originally suggested by Wicksell) since he cannot compete effectively with firms organised for large-scale production. (Kaldor 1983, p. 12)

Kaldor agreed with Weitzman that Keynesian macroeconomics, which hinged on the principle of effective demand, was inconsistent with Keynes's own assumption of perfect competition. 'There is a sense therefore in which the natural habitat of effective demand macroeconomics is a monopolistically competitive micro-economy. Analogously, perfect competition and classical macroeconomics are natural counterparts' (Weitzman 1982, p. 801, cited by Kaldor 1983, p. 13). To the best of my knowledge Kaldor never referred to MIFs in so many words, but I suspect that, if pressed, he would not have rejected it.

Now Kaldor was quite clearly wrong, on two levels. First, there is the fatal slide from 'perfect competition' to 'long-run equilibrium in perfect competition', which is all the more surprising since it came from someone who had become a strident opponent of any form of equilibrium theorising in economics. Outside long-run equilibrium, though, there is absolutely no reason why a perfectly competitive firm should not operate with excess capacity.[23] Second, and more important, there is a neglect of capitalist reality. The reason why the unemployed cannot (all) employ themselves has nothing to do with the shape of the firm's demand or cost curves, and everything to do with their lack of capital. To take an historical example: the mid-nineteenth century British cotton industry was probably as close to the textbook case of perfect competition as any manufacturing industry before or since, with a near-homogeneous product, free entry and exit, and a minimum-cost scale of output that was small relative to the industry's output. Yet the suggestion that with a little more ambition or initiative the many unemployed cotton operatives could have become self-employed would have been treated with derision in (say) 1842 or 1878, and rightly so. *They didn't have the capital*; otherwise they would have been capitalists, not (unemployed) workers. Kaldor seems to have believed (wrongly) that he needed MIFs, yet it was his unaccountable neglect of SPIFs that led him so badly astray.

5 'Just a Word?

One obvious objection to all this is that it greatly exaggerates the importance of language: 'MIFs' is just a word, after all, an instance of Lukes's 'futile linguistic purism', and it should not be taken seriously. This, I think, would be a serious mistake. In what is by far the best analysis to date of the use of metaphors in economics, Arjo Klamer and Thomas Leonard deny that they are invariably purely decorative. They instead distinguish three classes of economic metaphors. The first, and least important, are *pedagogical metaphors*, which 'simply serve to illuminate

[23] The neoclassical firm will remain in operation so long as price exceeds average variable cost; in a recession this will normally involve a profit-maximising (loss-minimising) output lower than the level that would minimise average total cost. Thus it could increase output without an increase in average cost, which is a sufficient condition for the existence of excess capacity.

and clarify an exposition and could be omitted without affecting the argumentation as such' (Klamer and Leonard 1994, p. 31). Second, and rather more influential, are *heuristic metaphors*, which 'serve to catalyze our thinking, helping [us] to approach a phenomenon in a novel way' (p. 32). Heuristic metaphors encourage the use of analogy, which is 'sustained and systematically elaborated metaphor' (p. 35).[24] Third, and operating at 'an even more fundamental level', are *constitutive metaphors*, 'those necessary conceptual schemes through which we interpret a world that is either unknowable (the strong position, per Nietzsche) or at least unknown' (p.39). Constitutive metaphors are extremely important. They 'frame a discursive practice in the way that the U.S. Constitution frames U.S. legal discourse…They determine what makes sense and what does not' (p. 40), thereby serving to define the Kuhnian 'disciplinary matrix', or the Lakatosian 'hard core' of any science. This perhaps explains why, for so many mainstream economists, the requirement for MIFs is self-evident and seems to need no justification.[25]

According to Klamer and Leonard, constitutive metaphors are [u]sually implicit' (p. 40). MIFs, I suggest, represents a constitutive metaphor that is (unusually) explicit. It comes from architecture, or the building trades, or constructional engineering. Foundations have to come first, to be solid and to be reasonably extensive.[26] You cannot construct a high-rise building first and then put in the foundations, as an afterthought. They must be solid: we all know what happened to the foolish man in the Bible who built his house upon sand (Negishi's stepping

[24] On the use of analogy in economic theory, see also Murphy (1994).
[25] When I presented an earlier version of this paper at a seminar at La Trobe University this became exasperatingly clear. 'Are you saying that we don't need MIFs?', asked one very smart young neoclassical theorist. 'Yes', I replied. 'Why do you think that you do need them? 'Because they're micro*foundations*….'.
[26] Victoria Chick reminds me that in earthquake zones foundations need also to be quite flexible. This is food for thought (especially perhaps for inflexible mainstream economists), but I do not think that it substantially affects my argument in this paper.

stones would not be a great improvement). And they must be extensive: you would not want an entire shopping centre to have the same meagre foundations as a single suburban house.

Mainstream economists claim that their MIFs satisfy all three conditions. The model of the rational, forward-looking, utility-maximising representative agent already exists; it is logically sound; and it has been applied to a very wide range of (micro)economic behaviour, extending imperialistically well beyond the former borders of the discipline to provide MIFs also for sociology, political science and social psychology (Lazear 2000 is a classic statement of this claim). Post Keynesians would object to all of this, and they would be right to do so. However, they would find it much more difficult to rebut the mainstream accusation that their own Post Keynesian MIFs are simply not up to the job. This is certainly true as regards *solidity*: fundamental and apparently irreconcilable differences continue to exist between the adherents of Davidson's 'Marshall-Keynes microeconomics' and supporters of the Kalecki-Eichner alternative. There is no single, generally accepted version of Post Keynesian microeconomics, and (arguably) agreement is further away now than it was half a century ago. Neither is Post Keynesian microeconomics as *extensive* as its neoclassical rival – it is not even close. The Kalecki-Eichner approach to pricing and investment under oligopoly is probably its strong point (I am revealing my own preferences here), but other areas of microeconomics are still much less well developed. There has been some interesting work on the labour market, for example, but there is still no clear, comprehensive alternative to mainstream labour economics. The same can be said of consumer theory, which has in turn prevented the emergence of a distinctively Post Keynesian welfare economics (how *should* economic goods be valued if the neoclassical notion of consumer surplus is repudiated?). And this is not a purely academic problem. There have been some perceptive heterodox critiques of the Stern Report, but what, precisely, is the *positive* Post Keynesian alternative to mainstream policy prescriptions on global warming?

Foundations, to repeat, *must come first*. The constitutive nature of the foundational metaphor is most evident, and most damaging, here. It suggests that if you have no MIFs your macroeconomics is necessarily flimsy and unsafe; in order to construct them, you must concentrate your energies on microeconomics. Probably the great majority of Post Keynesian macroeconomists would reject this conclusion, but it seems to follow inescapably if the MIFs metaphor is accepted. They would be well advised to shift their rhetoric from the vertical to the horizontal plane. As Kriesler (1996, p. 66) argued, again interpreting Kalecki, 'micro and macro stand side by side, with important feedbacks between them'. Returning to the constructional metaphor, micro and macro should be thought of as two separate buildings, equal in height and adjacent to each other, connected by bridges. Both rest on a single, solid and extensive set of SPIFs. The people who work in the two buildings are on friendly terms and in frequent (preferably, increasingly frequent) contact with each other. What they do is different, but not incompatible, and they aim to reduce the inconsistencies over time. These efforts, however, should not consume all their energies. There are no methodological grounds for evacuating the macro building and moving all its occupants into the micro building (and none for the opposite course of action).

The rhetorical consequences of the microfoundational metaphor were anticipated, several centuries ago, by John Locke: 'nothing else but to insinuate wrong *Ideas*, move the Passions, and thereby mislead the Judgement; and so indeed are perfect cheat' (Locke 1975, p. 508, cited by Klamer and Leonard 1994, p. 25).[27] Thus I can confidently claim Locke's support for my conclusion: Post Keynesian macroeconomists do not need MIFs.

[27] A 'rhetorical swindle' as Robert Solow puts it (cited by Hamermesh 2008, p. 409).

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