

**MICROFOUNDATIONS FOR  
MACROECONOMICS? THE PRE-HISTORY  
OF A DOGMA, 1936-1975**

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## **Abstract**

In this paper I trace the very chequered pre-history of the current dogma that macroeconomic theory must have rigorous microfoundations. After a brief introductory discussion of the concept of microfoundations (section I), I begin by citing Keynes's scattered and indirect remarks on this question in section II. Next, in sections III and IV, I deal with the period from 1933 to the mid-1960s, proceeding chronologically and distinguishing three categories of authors: those who asserted the need for microfoundations, those who attempted to provide them without saying so, and those who did neither. In section V, I discuss the increasing use of the term in the macroeconomics literature of the late 1960s and early 1970s. The 1975 S'Agaro conference of the International Economic Association, which revealed continuing doubt and confusion on the whole question, is the subject of section VI. The paper concludes, in section VII, by asking why the case for microfoundations had begun to resonate in the early 1970s, without achieving the canonical status that it would subsequently attain.

## **I Introduction**

I am in the early stages of writing a book entitled *The Microfoundations Delusion*, which will deal with both the methodology and the doctrinal history of this puzzling concept. In 2010 the dogma that macroeconomic theory must have ‘rigorous microfoundations’ is almost universally accepted by mainstream economists, and is distressingly popular among heterodox thinkers. It was not always so. In this paper, which is an early draft of the first chapter (or perhaps the first two chapters) of the book, I trace the very chequered pre-history of the dogma. It is a long and very complicated story, with many twists and turns. Part of it has been told by Teodoro Dario Togati (1998), in a sadly neglected book, and by Kevin Hoover (2010); Hoover’s account is, as will be seen, far from satisfactory.

The term ‘microfoundations’ was apparently invented by Sidney Weintraub in 1956, but the concept is somewhat older. My story begins in 1936, with the publication of the *General Theory*, and in section II Keynes’s scattered and indirect remarks on this question are assessed. Then, in sections III and IV, I deal with the period from 1933 (just before the appearance of Keynes’s great book) to the mid-1960s, proceeding chronologically and distinguishing three categories of authors: those who asserted the need for microfoundations, those who attempted to provide them without saying so, and those who did neither. In section V, I discuss the increasing use of the term in the macroeconomics literature of the late 1960s and early 1970s. The 1975 S’Agaro conference of the International Economic Association, which revealed continuing doubt and confusion on the question, is the subject of section VI. The paper concludes (in section VII) by asking why the case for microfoundations had begun to resonate in the

early 1970s (and not earlier), without yet achieving the canonical status that it would subsequently attain.

The microfoundations dogma is an instance of what philosophers of science term *microreduction*. Its antithesis is the concept of *emergence*, which is closely related to *irreducibility*, itself ‘often viewed as the sense in which emergent properties are autonomous from the more basic phenomena that give rise to them’ (Bedau and Humphreys 2008, p. 9). That is to say:

....a state or other feature of a system is emergent if it is impossible to predict the existence of that feature on the basis of a complete theory of basic phenomena in the system...A closely related idea is that emergent properties cannot be explained given a complete understanding of more basic phenomena. (*ibid.*, p. 10)

There is a very substantial (and generally very critical) literature on microreduction and emergence in the natural sciences, some of which is cited in King (2009), where I argue that the microfoundations project is yet another example of a failed microreduction.<sup>1</sup> This is still very much a minority view among economists of all persuasions. Contrary to what is often believed, it does *not* commit me to radical Hegelian holism or require me to deny the relevance of microeconomics to macroeconomics (or for that matter *vice versa*). All that I am claiming is that the two bodies of knowledge exist side by side, neither being the foundation of the other.<sup>2</sup> My target is what Steven Pinker describes as ‘bad’, ‘greedy’

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<sup>1</sup> Thus my definition differs from that of Hoover (2010), whose two categories of ‘non-eliminative microfoundations’ are not, from my perspective, microfoundations at all.

<sup>2</sup> See Kriesler (1989) for an early and very cogent statement of this principle in the context of Michal Kalecki.

or ‘destructive reductionism’. I have no objection to what Pinker terms ‘good reductionism’, which ‘consists not of *replacing* one field of knowledge with another but of *connecting* or *unifying* them. The building blocks used by one field are put under a microscope by the other’ (Pinker 2005, pp. 69-70; original stress).<sup>3</sup> My claim is that monetary macroeconomics (Smithin 2009)<sup>4</sup> is autonomous in the Bedau-Humphreys sense; it cannot be reduced to microeconomics; and it therefore constitutes a distinct ‘special science’, separate from (but of course closely related to) microeconomics.

## **II Keynes and Microfoundations**

Despite his deep, lifelong interest in questions of philosophy, John Maynard Keynes wrote very little specifically on economic methodology. Possibly he was deterred by having a father who was the author of the standard text on the subject (J.N. Keynes 1890). At all events, the attitude of the younger Keynes towards the relationship between macroeconomics and microeconomics is easily misunderstood. More than half a century ago the British philosopher J.W.N. Watkins, a disciple of Karl Popper, described him as a methodological individualist (Watkins 1952, pp. 157-9; 1957, p. 182). Claudio Sardoni goes even further, arguing that ‘Keynes indeed based his macroeconomic theory on microeconomic foundations that were essentially non-neoclassical’ (Sardoni 2002, p. 5).

Most recently, Kevin Hoover has taken a more nuanced – or perhaps the correct term is ‘more confused’ – position. ‘Taken together’, Hoover suggests, ‘Keynes’s analysis of the fundamental components of aggregate demand and supply display a firm

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<sup>3</sup> There are some interesting metaphors in this passage. Whether ‘good reductionism’ really counts as reductionism is debatable; I shall not concern myself with that question here.

<sup>4</sup> This might not be true of the macroeconomic properties of a non-monetary system, but such is not (to paraphrase Keynes) the world in which we actually live.

connection between microeconomic choices of firms and individuals and the macroeconomic outcomes. In every case, the causal mechanisms are driven by individual agents' (Hoover 2010, pp. 17-18). Very true, but this 'firm connection' is a (much) weaker assertion than the microfoundations dogma, as Hoover himself almost immediately acknowledges: 'The outcomes for the economy as a whole clearly *emerge* out of individual behaviors...*Emergence* is perhaps the most characteristic feature of Keynes's account of the relationship of microeconomic to macroeconomic behavior' (*ibid.*, p. 18; original stress). But, as already noted, it is precisely the existence of emergent properties, in economics, sociology<sup>5</sup>, the life sciences<sup>6</sup> and the natural sciences more generally,<sup>7</sup> which doom any microreduction project to inevitable failure. Hoover himself seems to recognize this, just a couple of pages later, in his discussion of J.R. Hicks, whose endorsement of the microfoundations dogma in all but name in *Value and Capital* is 'at least in part, a denial of Keynes's emergent properties of the economy as a whole' (*ibid.*, p. 20).

What, then, did Keynes himself have to say on these matters? His first and perhaps most telling comment came in his 1926 obituary of Edgeworth, where he explained why economics could never become a mathematical science like physics:

The atomic hypothesis which has worked so splendidly in Physics breaks down in Psychics. We are faced at every turn with the problem of Organic Unity, of Discreteness, of Discontinuity - *the whole is not equal to the sum of the parts*, comparisons of quantity fail us, small changes produce large effects, the

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<sup>5</sup> See, for example, Talcott Parsons (1937, pp. 31-2, 738-43, 765, 773).

<sup>6</sup> See, for example, Stephen Jay Gould (1985, pp. 575-7).

<sup>7</sup> See the volume edited by Bedau and Humphreys (2008) for an excellent collection of articles on this concept.

assumptions of a uniform and homogeneous continuum are not satisfied. (Keynes 1926, p. 150; stress added)

‘The whole is not the sum of the parts’: this alone is enough to rule out the possibility of microreduction in economics.<sup>8</sup> It is restated in the *General Theory*, as might be expected, but not at the very beginning, as one might have hoped. Instead, Keynes invokes it at the end of chapter 7 (‘The Meaning of Saving and Investment’), where he describes what has come to be known as the ‘paradox of thrift’. In the absence of an increase in investment spending, any increase in saving by an individual will reduce aggregate income, and hence reduce the saving of others. While this can safely be ignored by any individual, it is ‘nonsense to neglect it when we come to aggregate demand. This is the vital difference between the theory of the economic behaviour of the aggregate and the theory of the behaviour of the individual unit, in which we assume that changes in the individual’s own demand do not affect his income’ (Keynes 1936, p. 85).

Keynes does not return to this question until chapter 21 (‘The Theory of Prices’), where he distinguishes the ‘Theory of Value’ from the ‘Theory of Money and Prices’, and complains that they appear to be entirely unrelated: ‘we are lost in a haze where nothing is clear and everything is possible. We have all of us become used to finding ourselves sometimes on the one side of the moon and sometimes on the other, without knowing what route or journey connects them, related, apparently, after the fashion of our waking and our dreaming lives’ (*ibid.*, p. 292). But this is a mystery of Keynes’s own making, as his earlier discussion of individuals and aggregates should have suggested to

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<sup>8</sup> According to Rod O’Donnell (1989, p. 127-8), this principle was enunciated by G.E. Moore in the context of ethics, but there is no indication in the Edgeworth obituary that Keynes intended it to be restricted to ethical questions.

him. His analysis of the means of ‘escape from this double life’ (*ibid.*, p. 293) is also much less clear than it might have been, conflating in a single paragraph the micro-macro distinction (which at one point he seems to deny), the contrast between a stationary and a growing economy, and the difference between stationary and shifting equilibria (which he regards as equivalent to that between a monetary and a non-monetary economy). ‘The right dichotomy’, he suggests, ‘is between the Theory of the Individual Industry or Firm and of the rewards and the distribution between different uses of a *given* quantity of resources on the one hand, and the Theory of Output and Employment *as a whole* on the other hand’ (*ibid.*, p. 293; original stress). But this is not a dichotomy at all: the opposite of ‘whole’ is ‘part’, not ‘given’ or ‘constant’. The same lack of clarity is found in Keynes’s (1936) prefaces to the German and Japanese editions of the *General Theory*, and it is not entirely absent from his restatement of ‘The general theory of employment’ published by the *Quarterly Journal of Economics* early in 1937.<sup>9</sup>

Not until the preface to the French edition, written in February 1939, does Keynes achieve the clarity of the chapter 7 statement quoted above:

I have called my theory a *general* theory. I mean by this that I am chiefly concerned with the behaviour of the economic system as a whole, – with aggregate incomes, aggregate profits, aggregate employment, aggregate investment, aggregate saving rather than with the incomes, profits, output, employment, investment and saving of particular industries, firms or individuals.

And I argue that important mistakes have been made through extending to the

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<sup>9</sup> Though it is striking just how often Keynes uses the phrase ‘as a whole’: three times in one paragraph in chapter 21 (Keynes 1936, p. 293), and again three times in one paragraph of the *QJE* article (Keynes 1937, p. 219).

system as a whole conclusions which have been correctly arrived at in respect of a part of it taken in isolation. (Keynes 1939, p. xxxii)

Here, at least, Keynes uses ‘general’ as synonymous with ‘aggregate’ (or ‘macro’), in contrast to ‘particular’ (or ‘micro’).<sup>10</sup> His denial of the possibility of microreduction in economics could hardly be more explicit.

Some authorities on Keynes have attributed his position on this question to a broader philosophical commitment on his part. Keynes was an organicist, they maintain, and therefore hostile to atomism, as his previously-cited remarks in the Edgeworth obituary indicate; this is a constant theme in Togati (1998). According to Anna Carabelli, pre-Keynesian or ‘classical’ theory relied on ‘the premises of logical independence’ derived from Newtonian physics, which allowed it ‘to speak of economic individuals, of industries and of systems *in isolation* from each other’. Keynes, however, repudiated these premises: ‘one of the leading methodological elements in Keynes’s assault on the classical theory was provided by the concept of organic interdependence which grew directly from his approach to probability theory’, and ‘underlay his positive contribution to the study of the economy; it is the basic notion in the discipline of macroeconomics’ (Carabelli 1994, p. 142; original stress). As Ben Fine and Dimitris Milonakis suggest, Keynes’s approach has ‘a holistic and organicist outlook, both at odds with the individualistic overtones of neoclassical economics and more in line with some writers in the classical (in our sense of the word) tradition, and with the Historical Schools and

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<sup>10</sup> Elsewhere ‘general’ is taken to mean ‘encompassing’ and is contrasted with ‘special’, as in the preface to the English edition, where he defines his own analysis as ‘a more general theory, which includes the classical theory with which we are familiar, as a special case’ (Keynes 1936, p. vii). This is a quite different usage from that in the French preface.

American institutionalism’ (Milonakis and Fine 2009, p. 275). Contrary to what Sardoni and (perhaps) also Hoover and Watkins believe, Keynes could never have accepted the dogma of microfoundations, whether they be of a neoclassical or a non-neoclassical variety.<sup>11</sup>

### **III The First Two Decades (1933-1953)**

According to Hal Varian (1987), the first references to ‘micro’ and ‘macro’ in the economics literature come in a 1933 paper by Ragnar Frisch, who distinguishes ‘macrodynamic analysis’ from ‘microdynamic analysis’. Actually there are two papers, one published in a *Festschrift* for Gustav Cassel (Frisch 1933) and the other, ‘Some Problems in Economic Macrodynamics’, presented at the inaugural meeting of the Econometric Society in Leyden and summarized by Jakob Marschak (1934). There is no hint of microfoundations in either paper<sup>12</sup>, and no indication that the question was raised in the discussion of Frisch’s conference paper: ‘Microdynamics are concerned with particular markets, enterprises, etc., while macrodynamics relate to the economic system as a whole’ (Marschak 1934, p. 189); that is all. Questions from Tjalling Koopmans, Michał Kalecki, F. Divisia and Henry Schultz are briefly reported by Marschak (*ibid.*, p. 192), but they have no bearing on the issue of microfoundations.

The next (non-) event came in 1936, a few months after the appearance (in February) of Keynes’s *General Theory* (in which the words ‘micro’ and ‘macro’ are regrettably absent). This was the publication of James Meade’s *Introduction to Economic*

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<sup>11</sup> A slightly different interpretation is offered by O’Donnell, for whom Keynes’s position was ‘pluralist. The principle of atomism or methodological individualism was applicable in some situations, and the principle of organic unity in others’ (O’Donnell 1989, p. 177). The atomist principle was – presumably – *not* applicable to macroeconomics.

<sup>12</sup> As is acknowledged, rather grudgingly, in the case of the 1933 paper, by Hoover (2010, pp. 10-11).

*Analysis and Policy*, which was the very first Keynesian textbook – or, more precisely, the first textbook with a Keynesian component. Part I, ‘Unemployment’, sets out a simplified version of the *General Theory*, with an emphasis on policy, while section V, ‘International problems’, goes some way to filling the most significant gap in Keynes’s own macroeconomic analysis. In between, there are three sections on microeconomics, dealing with product markets, income distribution and the supply of the factors of production. If any single theorist might have been expected to advocate microfoundations it was surely Meade, a lifelong enthusiast for neoclassical economic theory, even when it was plainly wrong (as was the case with the theory of capital). Although macro and micro themes are interwoven throughout the book, there is no suggestion that one is foundational for the other. Significantly, in the chapter in part I devoted to ‘direct control of expenditure on consumption’, Meade (1936, pp. 49-60) does not invoke utility maximization or any other aspect of the neoclassical theory of consumer behaviour. He remained a neoclassical Keynesian for the remainder of his (very long and productive) life, without ever expressing - as far as I am aware – any opinion on the microfoundations dogma.

Three years later, another neoclassical Keynesian did precisely this. In *Value and Capital*, J.R. Hicks neatly begged the entire question in a passage in the chapter entitled ‘The temporary equilibrium of the whole system’ that deserves to be quoted in full:

It is one of the most exciting characteristics of the method of analysis we are pursuing in this book that it enables us to pass over, with scarcely any transition, from the little problems involved in detailed study of the of the behaviour of a

single firm, or single individual, to the great issues of the prosperity or adversity, even life or death, of a whole economic system. The transition is made by using *the simple principle*, already familiar to us in statics, *that the behaviour of a group of individuals, or group of firms, obeys the same laws as the behaviour of a single unit*. If a particular change in price (other prices being constant) can be shown to increase the demand for a certain commodity on the part of a representative individual, then it must increase the demand for that commodity on the part of all individuals similarly situated. (We have learnt to mark out, by our ‘income effects’, the differences in the situations of those persons who appear as buyers, and those who appear as sellers, in the relevant markets). The laws of market behaviour, which we have laboriously elaborated for those tenuous creatures, the representative individual and the representative firm, thus become revealed ‘in their own dimensions like themselves’ as laws of the behaviour of great groups of economic units, from which we can readily evolve the laws of their interconnexions [*sic*], the laws of the behaviour of prices, the laws of the working of the whole system. (Hicks 1939, p. 245; stress added)

This is not quite the origin of the term ‘representative agent’, but it is very close.<sup>13</sup> Hicks takes for granted the validity of his ‘simple principle’, which entails that the whole *is* the sum of the parts, and thereby rules out fallacies of composition, emergent properties – and any need for a ‘theory of output as a whole’.

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<sup>13</sup> A JSTOR search for the period 1900-1939 yielded very many ‘representatives’, and a very large number of ‘agents’, but no instance where the two words were used together.

The book was reviewed by several eminent theorists, at least three of them sympathetic to Keynes,<sup>14</sup> but only one of them questioned this aspect of Hicks's analysis. This was Oskar Morgenstern, who in an extended footnote to his 33-page review drew attention to Hicks's elimination of the concepts of 'income' and 'saving' from the discussion of economic dynamics. 'Among the paradoxical consequences of abolishing the notion of income', Morgenstern noted, 'would be that the entire multiplier analysis – so dear to modern economists – would break down, since there would be no income to split between consumption and saving. In the same manner there would be no room left for propensities to consume and to save. It is rather surprising that Cambridge has not yet protested against this implicit attack' (Morgenstern 1941, p. 392n; see also *ibid.*, p. 393).

Again according to Varian (1987, p. 462), the first use of 'micro-economic' and 'macro-economic' (adjectives, with hyphens), was by the Dutch econometrician P. de Wolff (1941) in a short note in the *Economic Journal*. As Varian observes, 'de Wolff's note is concerned with what we now call the "aggregation problem": - how to move from the theory of the individual consuming unit to the behaviour of aggregate consumption'. Five years later what came to be known as the 'aggregation debate' in econometrics began in earnest with a series of papers, and a book, by Lawrence Klein. In the first of his articles, Klein voiced what would later become a familiar complaint:

Many of the newly-constructed mathematical models of economic systems, especially the business-cycle theories, are very loosely related to the behavior of individual households or firms which must form the basis of all theories of

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<sup>14</sup> Kenneth Boulding (1939), Roy Harrod (1939), and Abba Lerner (1940). Fritz Machlup (1939, p. 277) described it as 'a great book', while Bernard F. Haley (1939, p.560) asked 'How much of this is good for anything?', given that Hicks did not discuss applications of his theoretical analysis.

economic behavior....These aggregative theories have often been criticized on the grounds that they mislead us by taking attention away from basic individual behavior. (Klein 1946, p. 93)

Klein's fifteen-page article is, however, devoted to a mathematical analysis of aggregation conditions, since 'the problem of bridging the gap between the traditional theories based on individual behavior and the theories based on community or class behavior is, to a large extent, a problem of proper measurement' (*ibid.*, 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 judgments about such important magnitudes as aggregate employment, output, consumption, investment' (*ibid.*, p. 108). There is no suggestion of microfoundations here.

In his book, *The Keynesian Revolution*, published in the following year, Klein does, however, advocate microfoundations in everything but name. He argues that Keynes's consumption function can be derived '[f]rom the accepted theories of consumer behavior' (Klein 1947, p. 58), while the investment function 'is based on the most classically accepted doctrine of profit maximization'. 'Again', Klein suggests, 'it seems best to develop a treatment from the behavior of an individual unit following an optimal principle, and then to derive the aggregative relationship for the economy as a whole' (*ibid.*, p. 62; cf. *ibid.*, pp. 68, 192-9). Looking back, almost half a century later, Klein

showed himself now to be somewhat ambivalent about the case for microfoundations,<sup>15</sup> but in 1947 he was a strong supporter.

So, too, was Tjalling Koopmans, whose famous attack on ‘Measurement Without Theory’, directed against institutional economists in general and Wesley Clair Mitchell in particular, included a complaint that their ultra-empiricism ‘eliminates all benefits...that might be received from economic theory – by which I mean in this context the theoretical analysis of the aggregate effects of assumed patterns of economic behavior of groups of individuals’ (Koopmans 1947, p. 164). The young Rutledge Vining came to Mitchell’s defence, providing a vigorous assertion of the autonomy of the macroeconomic domain:

I believe that in our discussions of trade fluctuations, national and international, we deal with the behavior of an entity that is not a simple aggregate of the economizing units of traditional theoretical economics. I think that we need not take for granted that the behavior and functioning of this entity can be exhaustively explained in terms of the motivational behavior of individuals who are particles of the whole. (Vining 1949a, p. 79)

In his reply Koopmans yielded some ground (Koopmans 1949, especially pp. 85-6), but Vining was left unmoved; ‘There is more to the developing house than can be learned from the bricks’ (Vining 1949b, p. 92).

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<sup>15</sup> ‘There should be microeconomic foundations for macroeconomics, but important parts of macroeconomics have no counterpart in microeconomics, and it is not always possible to obtain satisfactory microeconomic foundations for every aspect of macroeconomics. It may not necessarily be a good research strategy to develop macroeconomics by a summation of microeconomic propositions...’ (Klein 1993, p. 35).

The controversy did Vining's career no good,<sup>16</sup> and Koopmans's career no harm, but it cannot be said that the aggregation controversy left a very deep imprint either on macroeconomic theory or on the practice of econometrics.<sup>17</sup> A decade later, Maurice Peston did launch a stinging attack on the very notion of microfoundations: 'The difficulty arises from the acceptance of the doctrine that micro-theory is fundamental theory and all other theory must be built up from it' (Peston 1959, p. 60). On the contrary, 'economic theory at any level of aggregation exists on a par with economic theory at any other level...macrotheory and microtheory are seen to be complements not substitutes' (*ibid.*, pp. 59, 61). He concluded that the 'aggregation problem as understood at present...acts as a straightjacket on economic theory at all levels' (*ibid.*, p. 63). Peston's paper seems to have had little or no impact. Five years after his article appeared, H.A. John Green's authoritative book on *Aggregation in Economic Analysis* was careful to focus on technical issues and steer clear of methodological controversy (Green 1964).

The aggregation debate had begun back in 1946, the year in which a young Milton Friedman published a 19-page paper directing 'methodological criticisms' – the sub-title of the article – at Oskar Lange's *Price Flexibility and Employment* (Lange 1945). This would have been a perfect opportunity for Friedman to criticize Keynesian macroeconomics for its lack of microfoundations. But there is not the slightest hint of such a critique. Instead Friedman objects to Lange's 'casual empiricism, invalid use of inverse probability, introduction of factors external to the theoretical system, and the use

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<sup>16</sup> See Mirowski (1989) for a gripping account of the rather dirty politics of the Vining-Koopmans affair.

<sup>17</sup> It receives only a brief mention in Mary Morgan's well-received history of econometrics (Morgan 1990, pp. 202-5). See also Qin (1993, pp. 183-8) on contemporary econometricians' inferiority complex with respect to mainstream economic theorists

of only some of the implications of a formal model that has others that are unrealistic'. Lange had attempted to 'escape the shackles of formalism' by resorting to 'illogical devices and specious reasoning' (Friedman 1946, p. 331). Friedman had earlier praised Jan Tinbergen's (1939) pioneering macroeconometric study of U.S. business cycles, again without querying the absence of microeconomics and even praising Wesley Mitchell (Friedman 1940). In fact Friedman was never an enthusiast for the microfoundations dogma. His influential work on the consumption function, published as a book in 1957, did begin with an analysis of individual decision-making, yielding an inter-temporal utility-maximising equation that he described as 'a cornerstone' of his permanent income theory of the consumption function (Friedman 1957, p. 14). ('A cornerstone, be it noted; most buildings have at least four). But he makes very little of its methodological implications, being interested instead in the theoretical and policy implications: the marginal propensity to consume was lower, the multiplier much smaller, and the capitalist economy much more stable, than Keynesians would expect (*ibid.*, p. 238). The issue was the appropriate 'specification of the dynamic characteristics of the economic system itself' (Friedman and Becker 1957, p. 73), not the existence or otherwise of microfoundations.

Two decades later Friedman finally put his views on this issue beyond doubt when he listed four 'important empirical differences of judgement' in the theory of money. One he summarized as 'Build up or down':

A second dichotomy...is between the view that the best approach to analyzing aggregate change is from the top down – that is, analyzing the behaviour of

aggregates *and then perhaps supplementing that analysis* with an examination of the determinants of how the aggregates is divided among components – or from the bottom up – that is, analyzing directly each component and constructing the aggregate as a sum of components...Keynes, *like me*, was clearly in the top-down category. (Friedman 1976, p. 316; stress added)

This was entirely accurate, both as a statement about Keynes and as a reflection on his own past practice.

In economics the publishing event of 1947 was the appearance of Paul Samuelson's *Foundations*. Interviewed nearly half a century later about his early theoretical development, Samuelson claimed to have been 'worried about the micro foundations'. As he told David Colander and Harry Landreth, 'I worried most of all in my Chicago years and the first part of the time I was at Harvard. You know, I was like a tuna: the Keynesian system had to land me, and I was fighting every inch of the line' (Samuelson 1996, p. 161). But there is absolutely no evidence of this struggle in the *Foundations*, where the same sets of mathematical principles are applied first (and principally) to microeconomic questions and then (much more briefly) to macroeconomics, without any suggestion whatsoever that the former might be the foundation of the latter (see Samuelson 1947, pp. 276-83 on the Keynesian system, and pp. 335-42 on the business cycle). As Wade Hands has noted, in the context of the *Foundations*, 'there is no maximization (from individuals or any other "agents") going on anywhere in the Keynesian economics (or other "business cycle" models) of the 1940s' (Hands 2008, p. 472; see also Togati 1998, chapter 8). As Samuelson subsequently wrote:

‘No one associates a Keynesian system with a maximizing single mind or even to [*sic*] an as-if pretend maximizing system’ (Samuelson 1998, p. 1384). He himself had, in fact, acknowledged this later in the interview with Colander and Landreth: ‘I decided that life was more fruitful not worrying about it. You weren’t making any progress on it. Moreover, the search today for micro foundations for macro does not have a rich set of results’ (Samuelson 1996, p. 162). Yes, indeed.

Two years later James Duesenberry worried quite explicitly about the ‘somewhat schizophrenic attitude’ taken by economists toward the theory of consumption (Duesenberry 1949, p. 1). He is sometimes counted as a pioneer in establishing microfoundations for the Keynesian consumption function,<sup>18</sup> which he described as ‘a special case of the general theory of consumer behaviour’ (*ibid.*, p. 1). Duesenberry’s first principle for empirical research was defined accordingly: ‘every hypothesis ought to be stated in terms of the behaviour of individual firms or households, even when we are only interested in aggregate results’ (*ibid.*, p. 75). But what he actually does is to use macroeconomic evidence – the long-run constancy of the average propensity to consume in the United States in the face of a substantial increase in real incomes – to *criticize* the established neoclassical theory of consumer behaviour and to advocate an alternative to it, in which relative (not absolute) income is crucial, and individual decisions are not reversible. This is a profoundly subversive exercise,<sup>19</sup> which probably explains why it never caught on, despite the considerable evidence in its favour (on which see Barba and Pivetti 2009). Whatever Duesenberry’s own intentions, his book is *not* consistent with the microfoundations dogma, which requires the uncritical use of neoclassical

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<sup>18</sup> For example by Hynes (1998, p. 39) and Hoover (2010, pp. 28-32).

<sup>19</sup> As indicated by his approving references to Thorstein Veblen (Duesenberry 1949, pp. 14-15).

microeconomic theory to interpret macroeconomic reality. What he does, in fact, is the exact opposite of this.

One of his reviewers was Kenneth Arrow, whose Walrasian interests might have been expected to make him sympathise with the case for microfoundations. In fact Arrow's principal objection to Duesenberry is quite different: 'the most striking methodological deficiency in the entire work is its failure to set up explicitly a model of the economy against which to study the empirical data'. It is a *macroeconomic* model that Arrow has in mind, for he continues by complaining that 'nothing whatever is said about the other parts of the economy, such as the investment function and supply factors in general' (Arrow 1950, p. 910). In the following year, writing on 'mathematical models in the social sciences', Arrow took a different line, attacking Rutledge Vining's position in the aggregation controversy: 'As Koopmans points out, a full characterization of each individual's behavior logically implies a knowledge of group behaviour; there is nothing left out...one methodological principle emerges clearly: in order to have a useful theory of relations among aggregates, it is necessary that they be defined in a manner derived from the theory of individual behaviour' (Arrow 1951, pp. 133-4). But this is not really microfoundations, since the position attributed to Koopmans is tautological, and the question of the *definition* of aggregates does not go to the heart of the matter.

In any case, Arrow's endorsement of microfoundations, if such it is, offers only 'in principle' support. In practice, he suggests:

...there is something to be said for at least the possibility of a collective basis for social theorizing, if not taken too literally. Much of our casual observation and

much of our better statistical information relate to groups rather than individuals. We may therefore be led to generalize and form a theory whose subject is the total behavior of a group. So long as it is understood that such a theory is really a resultant of certain as yet unanalyzed laws of individual behavior, no harm will be done, and the greater convenience of empirical analysis on groups may be highly beneficial. (Arrow, 1951, p. 133)

Arrow's ambivalence was again apparent sixteen years later when he complained that 'Samuelson has not addressed himself to one of the major scandals of price theory, the relation between microeconomics and macroeconomics' (Arrow 1967, p. 734). General equilibrium theory could be used to explain 'certain dramatic historical episodes', he maintained, including the highly successful postwar recovery of the Western countries. 'On the other hand, the Great Depression and the problems of the developing countries remind us dramatically that something beyond, but including, neoclassical theory is needed' (*ibid.*, p. 735). What this 'something' might be, Arrow does not say.<sup>20</sup>

#### **IV Modigliani to Machlup (1954-1963)**

In 1954 Franco Modigliani published his life-cycle hypothesis, using 'the well developed tools of marginal utility analysis' to reconcile time-series and cross-sectional evidence on the relationship between consumption and income (Modigliani 1954, p. 389). 'The results of our labor', he concluded,

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<sup>20</sup> In his 1994 Richard T. Ely lecture, however, he argued that information is 'an irreducibly social category in the explanatory apparatus of economics', and that 'individual behaviour is always mediated by social relations. These are as much a part of the description of reality as is individual behavior' (Arrow 1994, pp. 1, 5; see also Arrow 1986). The discussion of Keynes in Arrow and Hahn (1971) is briefly discussed in section V below.

basically confirm the propositions put forward by Keynes in *The General Theory*. At the same time, we take some satisfaction in having been able to tie this aspect of his analysis into the mainstream of economic theory by replacing his mysterious psychological law with the principle that men are disposed, as a rule and on the average, to be forward-looking animals. (*ibid.*, p. 430)

A tie, of course, is not a foundation. In his autobiography Modigliani does use the latter term, albeit with no ‘micro’ prefix (Modigliani 2001, p. 53). But there is also a hint of emergent properties, or perhaps a suggestion of the ‘multiple realisation’ problem that has engaged the attention of philosophers writing about microreduction. Among the six ‘main innovations’ of the life-cycle hypothesis, he writes, is the fact that ‘[t]he national saving rate is not simply the result of the different thrift of its citizens, in the sense that different national saving rates are consistent with identical individual saving behavior during the life cycle’ (*ibid.*, pp. 80-1). Modigliani does not elaborate, but this statement would be very difficult to reconcile with any version of the microfoundations dogma.

The year 1956 is especially important in this story, since it was then that the term ‘microeconomic foundations’ made its first appearance in print (Hoover 2010, p. 7). The culprit was Sidney Weintraub, who would soon become a pioneering Post Keynesian dissenter but was still in 1956 a respectable mainstream Keynesian. The term is not given a particularly prominent place. It appears once, in ‘note 2’ of the appendix to his article (on the macroeconomic theory of wages), in the context of an attempt to derive Keynes’s *Z-N* function (the aggregate supply curve). Note that he is not dealing with individual

consumers, or even individual firms: Weintraub's 'aggregation procedure' is one 'whereby industry supply concepts are transformed into notions of aggregate supply' (Weintraub 1956, p. 854). This, then, is not a microfoundations project at all in the post-1970, New Classical, sense. Weintraub is also responsible for the first recorded use of the term 'micro-foundations' (with hyphen), which appears in the title of a 1957 article, but nowhere else in this paper, where it is given no methodological significance whatsoever (Weintraub 1957). If Hicks, Klein and Modigliani had the concept of microfoundations without the word, Weintraub had the word, without the concept.

Earlier in 1956, Don Patinkin's influential *Money, Interest, and Prices* was published. This is sometimes regarded as a milestone in the development of the microfoundations dogma, again without the use of the word (e.g. by Hoover 2010, p. 22). This interpretation is, however, very difficult to justify. In fact Patinkin seems rather to have anticipated the failure of the general equilibrium variant of the microfoundations project (on which see Shubik 1975, Kirman 1992, Togati 1998 and especially Rizvi 1994). It is true that, in introducing his macroeconomic model, Patinkin claims that each of his aggregate demand and aggregate functions 'is built up from the individual demand and supply functions of the relevant individual goods' (Patinkin 1956, p. 199). Almost immediately he makes an important concession 'There is, however, no pretense of showing how this process of aggregation is actually carried out' (*ibid.*, p. 200). Earlier in the book he hints at the problems posed by emergent properties, and the consequent danger of fallacies of composition, pointing to 'a clear lack of analogy between individual and market excess-demand functions...the effect of, say, an increase in the total wealth of the economy on the amount of market excess demand for a good cannot be

determined until the distribution of this increase among the members of the economy is first specified' (*ibid.*, p. 33; cf. pp. 181, 183).<sup>21</sup> Earlier still, Patinkin had distinguished 'individual-experiments' from 'market-experiments', on the grounds that 'the unit of investigation is different' (*ibid.*, p. 12); there is no suggestion that any one such unit is the foundation of any other.

A much stronger claim to membership of the fledgling microfoundations club in the 1950s can be established for James Tobin, which is ironic given his subsequent opposition to the dogma and its anti-Keynesian implications. In 1958, however, Tobin was quite emphatic on the appropriate way to establish the liquidity preference (Keynesian money demand) schedule: 'This aggregative function must be derived from some assumption regarding the behavior of the decision making units of the economy' (Tobin 1958, p. 65), which he proceeds to do by modeling the decisions of individual investors.<sup>22</sup> By 1970 – that is, before the microfoundations dogma had begun to take hold – Tobin was taking a different line: 'Macroeconomics concerns the determinants of the performance of entire economies', he wrote in an introductory textbook, and therefore deals in economy-wide aggregates. 'The basic assumption is that this can be done without much attention to the constituents of the aggregates, that is, to the behavior and fortunes of particular households, business firms, industries, or regions', even though 'the relationships among aggregate variables...are intended to be consistent with theoretical and empirical knowledge of the behavior of individual economic units and particular

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<sup>21</sup> Three years later the decidedly non-Walrasian British economist George Richardson made a similar point, noting that while 'a sufficient condition for a group, or for a system as a whole, to be in equilibrium, is for all the component units individually to be in equilibrium', this was not necessary, 'for stability in the aggregates (e.g., levels of output,) by which we describe the system might be maintained without stability in the contributions of individual units' (Richardson 1959, p. 224).

<sup>22</sup> These individuals whose behaviour he analyses are not, however, representative agents; they differ in their attitudes towards risk and the 'critical' rate of interest at which they choose to hold their entire portfolio in cash.

markets'. The aggregate consumption and production functions 'are intended to be consistent with', and 'should conform with', what is known about the behaviour of individual consumers and firms (Tobin 1970, pp. 44-5). Almost no-one would deny this; 'consistency' and 'conformity' between A and B are much weaker requirements than A's being 'derived from' B.

At a high-powered 1960 multi-disciplinary conference, the proceedings of which were published two years later, Kenneth Arrow organized a 'symposium on macro- and microeconomics'. It was opened by Abba Lerner, who took an unusual line: 'the dichotomy of economics into micro- and macroeconomics is itself much too violent an oversimplification', he maintained:

Rather than to classify different parts of economics as belonging to the one kingdom or the other, it is much more useful to arrange them according to the *degree* to which they abstract from parts of the economic system that are not under direct study. Instead of a dichotomy we then have a spectrum...' (Lerner 1962, p. 475).

As an example, he cited the relationship between money wages and employment, 'where the difficulties seem to stem from a bilateral over-concentration on the extreme cases instead of a consideration of the whole spectrum' (*ibid.*, p. 480). At the microeconomic end, small money wage cuts will leave prices unaffected and lead to increased employment. 'The illegitimate application of this position to the macroeconomic end of the spectrum', where falling money wages would reduce the price level, could not be

justified (*ibid.*, p. 481). Unfortunately there is no record of the discussion of Lerner's paper, and therefore no way of knowing whether his treatment of the 'problem of integrating microeconomics with macroeconomics', which 'may very well lead to their ultimate synthesis', met with general approval.<sup>23</sup> It may well have influenced Gardner Ackley, whose very popular intermediate textbook appeared in the following year with an entire chapter (XX) devoted to 'Macroeconomics and Microeconomics'. 'The relationship between macroeconomics and theories of individual behavior', he suggested, 'is a two-way street. On the one hand, microeconomic theory should provide the building blocks for our aggregate theories. But macroeconomics may also contribute to microeconomic understanding'. The theorist may thus 'proceed in either direction' (Ackley 1961, p. 570). The metaphor – 'building blocks' – points to microfoundations, but this is evidently not where Ackley wishes to go.

In the same symposium in 1960 the Cambridge economist Richard Stone complained that macroeconomic theories 'tend to be relatively superficial and to ignore any optimizing principle which might be supposed to lie at the roots of human endeavour' (Stone 1962, p. 494). Alternatively put: 'In order to replace the *ad hoc* relationships which figure so largely in macro-economics, the optimizing model tries to penetrate more deeply into the springs of human action' (*ibid.*, p. 505). Stone's choice of metaphors is interesting: 'roots' and 'springs' are not 'foundations'. So is his choice of optimizing model, which is that of Ramsey (1928); as he notes, the Ramsey model has 'attracted remarkably little attention among economists' (*ibid.*, p. 497).<sup>24</sup> Stone notes

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<sup>23</sup> Kenneth Arrow was unable to remember any salient details, perhaps not surprisingly at a distance of half a century (personal communication, 30 April 2009).

<sup>24</sup> I confirmed this statement with a JSTOR search, which revealed only 17 references to Frank Ramsey in the economics journal literature between 1940 and 1960, not all of them to his 1928 paper and none of

that it is a planning model, which he uses to solve the problem posed by the Indian planner, P.C. Mahalanobis: what proportion of investment should be devoted to expanding capacity in the investment-goods industry? This is a *social* problem, and there is no suggestion in Stone's paper that the Ramsey model might have a bearing on individual behaviour, still less that it might become the microfoundation for the macroeconomic theory of an unplanned capitalist economy. Indeed, Stone concludes by suggesting that his analysis

...should be enough to illustrate the unity of the social sciences. The limitations of pure economics as opposed to empirical economists are now becoming generally recognized, and it is high time economists took seriously the psychological and social postulates of their theory instead of squandering so much energy in preserving its purity, which beyond a certain age [stage?] is just another word for sterility. (*ibid.*, p.505)

It was probably just as well that this was delivered at Stanford; even in 1960, it would not have gone down well in Chicago.

There was a small flurry of references to 'microfoundations' in the journals around this time, but almost all were bibliographical citations of Weintraub's 1957 article (Hoover 2010, p. 7). Without using the word, however, Dale Jorgenson (1963) was beginning a lifelong project to provide neoclassical microfoundations for the theory of business investment, while Fritz Machlup was unique in defending the principle on

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them very substantial (though an exception might be made for Samuelson and Solow (1956), which is on capital theory).

explicitly methodological grounds. Number nine in his list of ‘twenty main theses’ governing the relations between ‘macro- and micro-economics’ was this: ‘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’ (Machlup 1963, p. 140). Earlier Machlup had acknowledged that

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

Machlup provides no evidence to support this final statement. But he can claim to have been the first in the economics literature to assert the need for microfoundations as a methodological principle.

### **V From Machlup to Phelps, and Beyond (1963-1975)**

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. It almost certainly had no impact on Robert Clower’s celebrated attack on what he termed J.R. Hicks’s ‘Keynesian counter-revolution’, in which Clower concluded that ‘orthodox price theory may be regarded as a special case of Keynesian economics, valid only in conditions of full employment’ (Clower 1965 (1984), p. 55).<sup>25</sup> This can be interpreted as a contribution to the microfoundations debate, but only if Walrasian general equilibrium theory is seen – as it is by Hoover, but not by me – as a form of ‘non-reductive microfoundations’. Hoover (2010, pp. 22-3) discusses Clower’s 1965 paper, but he overlooks a more relevant article that appeared two years later with the title, ‘A Reconsideration of the Microfoundations of Monetary Theory’.<sup>26</sup> Here Clower argued (convincingly) that Walrasian general equilibrium theory was ‘formally equivalent to the Classical conception of a barter economy’, and claimed (less convincingly) to have proposed ‘a reformulation of established microeconomic analysis suitable as a foundation for explicit analysis of the working of a money economy’. The latter, he conceded, was ‘obviously unfinished’ (Clower 1967, p. 8). It was no nearer completion in 1971, when Arrow and Frank Hahn concluded their *General Competitive Analysis* with a chapter on ‘The Keynesian Model’. Walrasian theory was not suited to the analysis of a monetary economy, they conceded. ‘This chapter can be taken as no more than a beginning of taking seriously the Keynes of the chapters on expectations and the functions of money’ (Arrow and Hahn 1971, p. 369).

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<sup>25</sup> The paper was presented at a conference in 1962 and first published in the following year in a German translation (Clower 1965 (1984), p. 34n).

<sup>26</sup> This illustrates the perils of computer-based literature searches. The paper was published in the *Western Economic Journal* (since renamed *Economic Inquiry*), which is not included in JSTOR.

If there really was a general equilibrium variant of the microfoundations project,<sup>27</sup> it had not got very far by the early 1970s.

Returning to the chronology after this brief detour, there were a couple of rather casual allusions to microfoundations in relatively obscure papers in 1966. Summarising his doctoral thesis on the term structure of interest rates in one page in the *Journal of Finance*, Liang-Shing Fan (1966, p. 131) criticised ‘the lack of proper micro-foundations’ in existing interest rate theory. In a three-page article in the *American Economic Review*, 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. In the following year William R. Bryan and Willard T. Carleton published a paper in *Econometrica* modeling the behaviour of individual banks, ‘with the purpose of improving the micro foundations of the theory of monetary dynamics’ (Bryan and Carleton 1967, p. 321); the term appears in the abstract only, and nowhere in the body of the paper. These were leading journals, but the references to microfoundations were both insubstantial and devoid of any methodological discussion. The word was in the air, but not much more.

A significant landmark occurred in 1969, with the publication of E.S. Phelps’s paper, ‘The New Microeconomics in Inflation and Employment Theory’, which had been presented at the American Economic Association’s meeting in the previous December. ‘It is notorious’, Phelps began, ‘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.’ He ended the

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<sup>27</sup> As argued by Hoover (2010, pp. 18-24). As already stated, I do not agree that the ‘general-equilibrium program’ was a genuine microreduction project, but space does not allow this important point to be developed here.

opening paragraph thus: ‘It seems clear that macroeconomics needs a microeconomic foundation’. Although the article ‘sticks doggedly to the neoclassical postulates of lifetime expected utility maximization and net worth maximization’, Phelps’s intention is ‘to buttress Keynes’ rather than to undermine him (Phelps 1969, p. 147).<sup>28</sup> The remainder of the article is somewhat eclectic, citing Lange, Sidney Winter and even Sidney Weintraub (but not Ramsay). The evolutionary economist Winter was co-author with Phelps of a chapter in the edited book, *Microeconomic Foundations of Employment and Inflation Theory* for which Phelps’s article was a sort of curtain-raiser. The book, too, was quite ecumenical, and has more to say about firms (and even unions) than about utility-maximizing individual consumers (Phelps *et al.* 1970).

Phelps’s article, and his book, were widely read and frequently cited in the early 1970s, and there is some evidence that the term ‘microfoundations’ was being used more generally than it had been previously. One theorist who in the early 1970s seems to have been untouched by it was, rather surprisingly, Robert Lucas. There is no reference to microfoundations in the – admittedly very skimpy - index to his first volume of collected papers (Lucas 1981), and the term is absent from his major articles from the period 1969-1973.<sup>29</sup> But the monetarist Karl Brunner was already describing recent developments in portfolio analysis as having been ‘exploited to provide a micro-foundation for monetary analysis’ (Brunner 1971, p. 167), and the neoclassical economists Duncan Foley and Miguel Sidrauski felt obliged to apologise for not having provided any microfoundations in some sections of their text on monetary and fiscal policy and growth (Foley and

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<sup>28</sup> It is worth noting that a buttress is *not* a foundation.

<sup>29</sup> As late as 1973 he was unable even to remember the correct title of Phelps’s book, to which he had contributed, missing out the crucial reference to ‘foundations’ and citing it as *Micro-economics of Inflation and Employment Theory* (Lucas 1973, p. 334)! Lucas’s concerns in the early 1970s seem to have been econometric, not methodological.

Sidrauski 1971, p. 4). This apology, the significance of which was picked up by a perceptive reviewer (Hester 1972, p. 1186), was the very first of its kind;<sup>30</sup> it would not be the last.

## **VI Disorder at the Border: S'Agaro, 1975**

An indication of the state of the debate on microfoundations in the mid-1970s, such as it was, can be inferred from the proceedings of the International Economic Association conference on 'The Microeconomic Foundations of Macroeconomics', held in the Catalan resort town of S'Agaro in 1975. As the chair of the programme committee Geoff Harcourt reported, Hicks was the 'basic architect and guiding light of the conference' (Harcourt 1977, p. 2). It was a much more ecumenical affair than would be possible today. In addition to Harcourt and Hicks, the other four members of the programme committee were Takishi Negichi, a Walrasian; Luigi Spaventa, a Sraffian; Erich Streissler, an Austrian by nationality and by conviction; and the Old Keynesian, James Tobin. Participation was by invitation, and included Walrasians (Edmond Malinvaud and three of his acolytes); econometricians with an interest in aggregation problems (Green, Koopmans); Post Keynesians (Tom Asimakopulos, Paul Davidson); Sraffians and other radicals (Pierangelo Garegnani, Mario Nuti, Ed Nell); historians of economic thought (Susan Howson, Donald Moggridge); and an impressive collection of luminaries not otherwise classified (Charles Goodhart, Ursula Hicks, Axel Leijonhufvud, Michael Parkin, Austin Robinson, Martin Shubik, Joseph Stiglitz). Invited but not attending were Tobin, Christopher Bliss, Dale Jorgenson and E.S. Phelps; Hahn was not there, but he did

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<sup>30</sup> Three decades later Foley himself, by now an eminent if rather idiosyncratic heterodox economist, felt obliged to repeat the apology (Foley 2003, p. 30).

send his paper (*ibid.*, p. 1). The absence of Jorgenson and Phelps was especially unfortunate, given their strong support for microfoundations.

One remarkable omission is evident: no philosopher of science or economic methodologist was invited to S'Agaro, no papers were presented on the methodological issues raised by microfoundations, and (so far as I have been able to ascertain from the conference volume) no mention was made at any point of methodological individualism, emergent properties, microreduction or any other related philosophical concepts. The majority of the participants seem to have taken the desirability of microfoundations for granted, and when the dogma was challenged it was in an unsystematic, almost in an incidental, manner. What little consideration of methodological questions that did take place seems to have come at the very end of the conference, in the 'final discussion session' (*ibid.*, pp. 378-80, 391-4). An honourable exception should be made for Streissler, whose paper does refer to the 'contrast between individual design and social consequences', recognized by both Hayek and Marx, which makes it possible that 'macroeconomic consequences differ qualitatively and totally from what is planned microeconomically'. From this criticism of 'a naïve derivation of macroeconomic consequences from microeconomic phenomena', he draws the conclusion that 'it might be better to start out with a system analytically wholly macroeconomic from the very beginning' (*ibid.*, p. 105). Towards the end of his paper Streissler invokes Occam's razor to cast further doubt on the need for microfoundations (*ibid.*, pp. 123-4)

Apart from Streissler's contribution, Harcourt's description of Koopmans's paper as 'peripheral to the central objective of the conference' (*ibid.*, p. 8) could be applied to most of them. It is small wonder that Harcourt himself drew attention to the 'air of

general dissatisfaction that pervades the contributions to the final discussion session' (*ibid.*, p. 21), in which Ursula Hicks commented that 'little progress had been made towards the objective of identifying the general relations between microeconomics and macroeconomics' (*ibid.*, p. 395). It is difficult to agree with Harcourt's more optimistic conclusion: 'views in the profession may now be converging together from very different starting points, at least in regard to analytical methods' (*ibid.*, p. 22). There is very little evidence of this in the conference volume.

A second lesson of S'Agaro is that in 1975 'microfoundations' meant Walrasian general equilibrium theory, not representative agents with rational expectations maximizing lifetime utility and making bequests to grandchildren. Judging by the index, the only participant to refer to Frank Ramsay was Koopmans, in what Harcourt described as his 'peripheral' contribution (*ibid.*, p. 150). Third, and perhaps most revealing, is the complete absence of party lines on the whole question. The need for microfoundations was explicitly endorsed by the Post Keynesian, Davidson, though he insisted that they should be of a non-Walrasian nature (*ibid.*, pp. 313, 391-2). It was explicitly denied by Nell, an eclectic radical with Post Keynesian affinities, who observed that the very title of the conference begged the question (*ibid.*, p. 392); by the Walrasian, Younès (*ibid.*, pp. 378-9); and by the Sraffian, Spaventa, who referred (most unusually at this conference) to fallacies of composition (*ibid.*, p. 137). Doubts were also expressed by Negishi, another Walrasian, who raised the prospect of providing macrofoundations for microeconomics (*ibid.*, p. 142). At the end of the conference Negishi neatly – and perhaps unconsciously – subverted the foundational metaphor again by calling for the provision of 'a bridge or at least some stepping stones' between micro and macro theory (*ibid.*, p. 380). (No-one but

a lunatic would use stepping stones as a foundation for anything!). Most of the participants, including the Post Keynesians Asimakopulos and Harcourt, and the quasi-Sraffian, Nuti, simply ignored the methodological issues altogether.

## VII Conclusion

The microfoundations dogma *could* have become established at any time after 1870, when the marginal revolution symbolised a shift in the focus of mainstream economics towards the study of individual behaviour and away from the macroeconomic questions of growth and distribution.<sup>31</sup> In fact it did not emerge then, or indeed (as we have seen) for another century. This is a real historical puzzle. Why in the 1970s, and not very much earlier? Why then, and not in the late 1930s, in reaction to the *General Theory*?

As we have seen, some eminent neoclassical theorists were asserting the microfoundations dogma well before 1970, without using the term; the most obvious example is Hicks. Many others, who might have been expected to do so, did not: Meade, Samuelson, Patinkin and Friedman are cases in point. One or two notable economists who might have been expected *not* to sympathise with the case for microfoundations in fact did so, at least some of the time: Klein and Tobin are in this third category, along with Sidney Weintraub, who invented the term. It is difficult to see what can explain these differences. *Physics envy* will not do: Paul Samuelson was easily the best mathematician of all those named above, but the need for rigour did not push him towards microfoundations. *Politics* does not really help, even though the need to establish

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<sup>31</sup> I owe this point to Robin Pope. With hindsight it seems rather obvious, but it was not obvious at the time (we discussed these issues at the 12<sup>th</sup> Conference of the Research Network 'Macroeconomics and Macroeconomic Policies' in Berlin in October 2008).

policy ineffectiveness certainly was a factor in the methodological position taken in the late 1970s by the New Classicals.<sup>32</sup> Klein was a neo-Marxist in the late 1940s, and Friedman was already a classical liberal, yet on microfoundations they took the exact opposite of the positions that one would have predicted for them. Not even *pro-* or *anti-Keynesianism* (loosely related to politics, of course) will do the job: compare Meade with Tobin.

There are, then, some perplexing questions for the period under review, to which I have no answers. And there is another – arguably much more important – question, which is why the microfoundations dogma should have triumphed so comprehensively, against all logic, in the quarter-century after 1975. But that will have to wait for another paper.

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<sup>32</sup> As Colin Rogers reminded me at the 2008 Sydney conference of the Society of Heterodox Economists.

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