

## **Whatever happened to microfoundations?**

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July 2006

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## 1. Introduction

To anyone trained in contemporary macroeconomics – to the generation to whom IS–LM and AS–AD are historical curiosities, irrelevant to serious research, that need to be learned only if one has to teach undergraduates or work in government<sup>1</sup> – the title of this paper will seem strange. Microfoundations are everywhere, to such an extent that the distinction between macroeconomics and microeconomics has almost disappeared. Microeconomic theories of individual optimization are central to the main results that differentiate contemporary macroeconomics from the already-forgotten theories of the decades immediately following the Second World War. Looking back over the past half century, it is possible to discern a progressive rise in the importance macro economists have attached to microfoundations, a story running from Modigliani (1944) and Patinkin (1956) to present-day Real Business Cycle and New Keynesian theories. There is nothing to explain.

However, this Whig history hides a paradox. Much of the early literature was trying to do two things that have dropped from sight in contemporary macroeconomics. It was trying to create a macroeconomics based on heterogeneous agents – a macroeconomics in which the economy was not simply the individual agent magnified  $n$  million times. The search for microfoundations was the search for a macroeconomics that was *different* from microeconomics, not where differences were assumed away through making assumptions that were blatantly false. It was trying to develop a theory of how markets worked that corresponded to what even casual observation revealed to be important features of the real world. It was also trying to provide an explanation of how things could go wrong at the macroeconomic level because economies did not behave like the perfectly competitive world of Walrasian, or Arrow–Debreu general equilibrium theory. These two things came together in the belief that there might be co-ordination failures: that the market system might fail to co-ordinate the actions of heterogeneous individuals.

It can be argued that these ideas have been taken up in New Keynesian economics, where market imperfections, asymmetric information and other devices are used to explain why the labour market does not clear. This is correct. However, the ambitions of those searching for microfoundations were much broader than this: they were not concerned about simply providing an explanation of why labour markets failed to clear, but of how the market mechanism as a whole might fail to operate in the way predicted by the theory of perfectly

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<sup>1</sup> Cf. Krugman 2000.

competitive general equilibrium. Non-market clearing was a label used to cover a wide variety of market situations that did not fit into the Walrasian mould. However, more recently disequilibrium has become synonymous with imperfect competition. Thus although Benassy (2002:vii; emphasis added) sought to argue the need for a strategy for enriching Walrasian theory ‘to encompass nonclearing markets *and* imperfect competition’, the reaction of at least one reviewer (Hornstein 2004) is to see the disequilibrium macro part as history of economic thought.

This paper does not attempt to survey the entire literature on microfoundations, which is far too large, or to provide a comprehensive answer why that literature became less fashionable (though it will clearly discuss that).<sup>2</sup> Instead we focus on the paradox of how a literature that sought to find a more general understanding of how markets worked was seen as coming to be superseded by one that simply neglected these issues whilst referring to this literature as though they had been solved. It overlaps substantially with Michel de Vroey’s *Involuntary Unemployment*, which will be discussed in section 6. However, our concerns are not the same as his. We are concerned with a narrower episode, not with the entire history of macroeconomics since Keynes. On the other hand, though involuntary unemployment is part of the story of microfoundations, an important part of the argument we are making is that the search for microfoundations was much broader, and that to focus on unemployment is to miss some of the important things that economists who were searching for microfoundations were trying to achieve.

## 2. Disequilibrium macroeconomics

The search for microfoundations that we are concerned with here is one that took place in the early 1970s and which went under the name of ‘disequilibrium macroeconomics’.<sup>3</sup> This label is far from adequate and both terms in it have been vigorously criticized, with considerable justification: it was not an exclusively macroeconomic literature, and the meaning of disequilibrium is problematic – that, indeed, was one of the lessons that was learned during this period. However, it will be used in the absence of any better alternative. The literature centred on Barro and Grossman (1971) whose ‘general disequilibrium model’ became one of

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<sup>2</sup> For surveys and general assessments of the literature on microfoundations and non-Walrasian equilibrium, see Weintraub (1977, 1979, forthcoming), Drazen (1980), Silvestre (1992), Dow (1985), Howitt (1987), Busetto (1989), Janssen (1993, 2006), and Rizvi (1994).

<sup>3</sup> Hence the title of Backhouse and Boianovsky (2005a).

the most widely-cited articles in economics. It is commonly considered to have started with the reinterpretation of Keynesian economics made by Clower (1965) and Leijonhufvud (1968). At the other end, the late 1970s provide a suitable end-point because around that time the character of the literature began to change. Economists began to take stock – through major survey articles in prominent journals (Grandmont 1977; Weintraub 1977; Drazen 1980), one of which was followed by a book (Weintraub 1979), and an IEA conference involving a very wide range of participants (Harcourt 1977). There were articles offering simplified, teachable, explanations of the theory (e.g. Stoneman 1979); there was a potential graduate textbook available (Barro and Grossman 1976) and at least one intermediate macro textbook included an exposition of the theory (Branson (1979). It was at this time that the new classical macroeconomics began to become established, changing attitudes towards the disequilibrium macroeconomic literature, and contributing towards the situation that has now emerged in macroeconomics.

The economists who contributed to disequilibrium macroeconomics approached it from many starting points, but what they had in common was the idea that markets might settle down into an equilibrium in which supply and demand (as defined by utility and profit-maximization in perfectly competitive markets) were not equal. The argument central to the entire literature was that if markets were out of equilibrium, it was impossible for all demands to be satisfied: agents' plans would be inconsistent with each other – this was what being out of equilibrium meant. If this were the case, the Walrasian demand functions, based on agents being able to buy or sell as they wished at the prevailing prices, would be inappropriate. For example, if households could not sell as much labour as they wished, they would revise their plans to buy goods. In general, failure to buy or sell as much as desired in one market would result in revised plans in other markets (spillover effects) resulting in effective demands being different from Walrasian demands.<sup>4</sup> Given these effects, it was possible for an economy to get stuck in an equilibrium other than the Walrasian one. For example, there might be a low-level equilibrium where households could not buy consumption goods because they could not sell their labour, and firms could not employ labour because they could not sell goods that they produced.

The conventional story of the episode known as disequilibrium macroeconomics is a simple one. Keynesian economics told the story of what happened when wages were sticky and the labour market did not clear, but failed to provide proper microfoundations. In the

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<sup>4</sup> 'Notional' demands in Clower's terminology.

early 1970s economists provided a micro-economic foundation for Keynesian economics by working out how agents would behave if prices (including the price of labour) were fixed – this was disequilibrium macroeconomics. Patinkin and Clower provided the key ideas about how firms and households would behave when there was rationing, and Barro and Grossman put it all together into a complete model. As the 1970s progressed, stagflation became the dominant economic problem and so economists realised that the assumption of fixed prices did not make sense. Some economists responded by arguing that observed unemployment could be explained using market-clearing models. The result was the new classical macroeconomics, based on rejecting the central claim of disequilibrium macroeconomics – that markets did not clear. Others provided explanations of why wages did not adjust to clear the labour market (wage contracts, asymmetric information, unions' monopoly power), creating New Keynesian economics. According to this story, the progression from disequilibrium macroeconomics to New Keynesian macroeconomics is one of progress: first assume fixed prices and then move on to a microeconomic explanation of why prices are fixed.

The point we are making is that the story was much more complicated than this. The first step in this argument is to point out that disequilibrium macroeconomics comprised a large literature that was far from homogeneous. Some key elements in it are shown in Figure 1. On the left hand side is the story most familiar to macroeconomists: the sequence Lange–Patinkin– Clower/Leijonhufvud– Barro/Grossman– Malinvaud. However, even within this part of the story (told in detail in Backhouse and Boianovsky 2005a) there is great variety. There is also the work of Solow and Stiglitz (1968), who came to disequilibrium macroeconomics from a route that was different from the routes anyone else was following, and which resulted in the creation of a very detailed general disequilibrium model published three years before Barro and Grossman's.<sup>5</sup> However, alongside this were other approaches to disequilibrium macroeconomics that arose much more directly from general equilibrium analysis as formulated by Arrow and Debreu. In Figure 1 we distinguish two main strands (discussed in Backhouse and Boianovsky 2005b). The first starts with Arrow and involves the work done by Takashi Negishi and Frank Hahn analysing non-tâtonnement and monopolistic-competition models, and which can be traced to Hahn's non-Walrasian models in the mid 1970s. The other starts with Debreu, who had not only known Malinvaud (a fellow student of Maurice Allais's) and Jacques Drèze (whom he had met at the Cowles

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<sup>5</sup> This is also discussed in Backhouse and Boianovsky 2005a.

Commission in 1953), but who also supervised the PhD dissertations of two of the major figures in disequilibrium macroeconomics in the 1970s – Jean-Michel Grandmont and Jean-Pascal Benassy. This group, including Drèze and his colleagues in Louvain, and the group centred on the research group, CEPREMAP in Paris, became known as the ‘French school’. After Barro moved decisively away from disequilibrium macroeconomics in the second half of the 1970s, they were the main figures associated with disequilibrium macroeconomics, developing it in a variety of directions.

It is important to note that the list of economists involved with disequilibrium macroeconomics is anything but comprehensive. These are just some of the people and links that are most significant. Figure 1 says nothing about the many applications and extensions of the basic Barro-Grossman/Malinvand model – such as the attempts to develop an econometric implementation of the theory, to relate it to international trade or economic growth.

As we have provided a more chronological narrative elsewhere, what we do in this paper is to draw out some of the themes that are found in this literature that call into question the Whig history outlined a few paragraphs back. We do this under two very loose headings.<sup>6</sup> Section 3 discusses the ideas and motivations of those associated most closely with the well-known story of disequilibrium macroeconomics. Section 4 covers those who were trying as much to reformulate problems with general equilibrium theory as to place macroeconomics on a firmer foundation. This split is, however, made purely for convenience and is in some ways rather arbitrary.

### **3. Price adjustment, information and coordination**

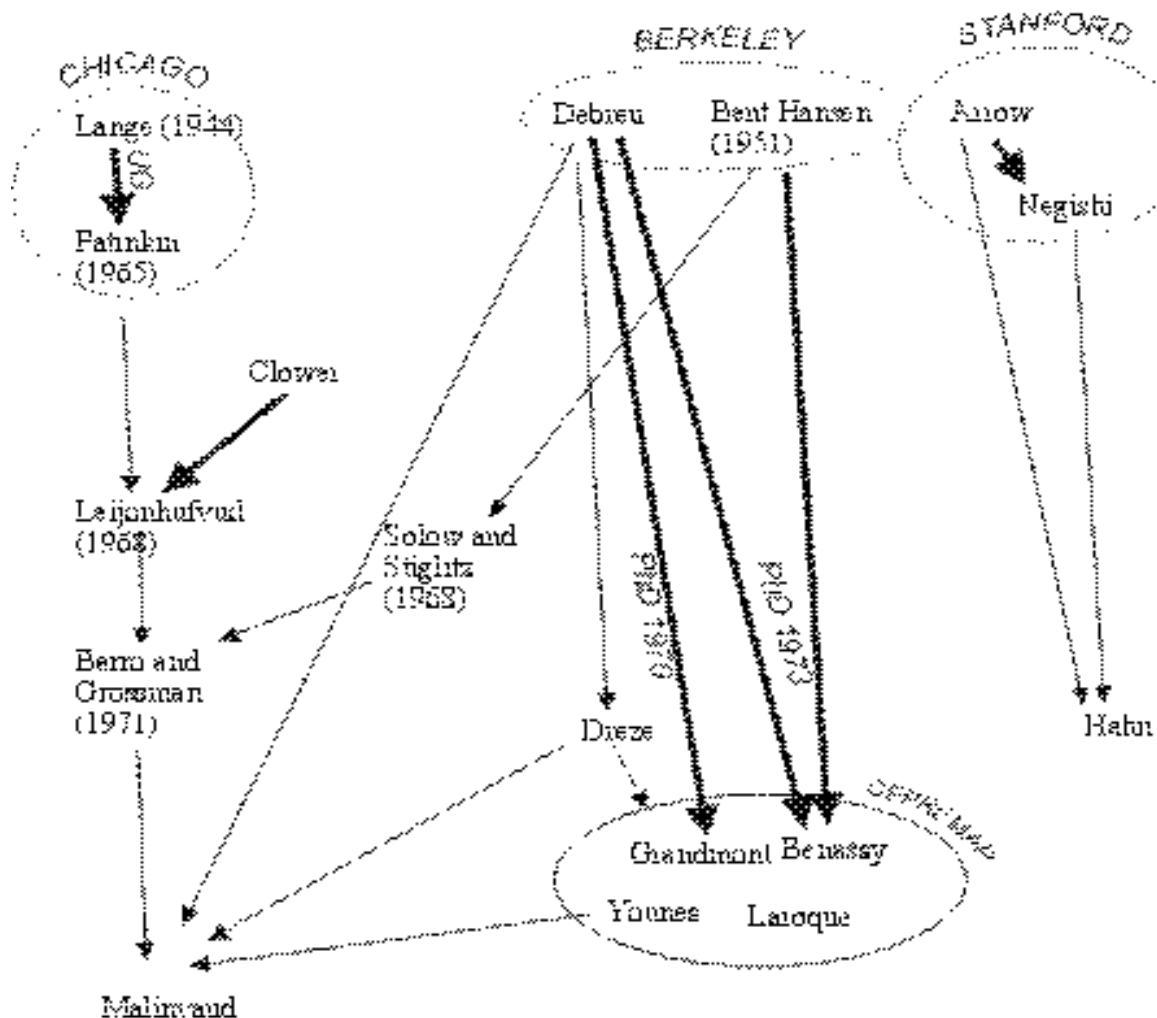
Disequilibrium macroeconomics arguably started when Patinkin, as a PhD student in Chicago in the late 1940s, took up, from Oskar Lange, the idea that Keynesian economics was about what happened if there was an inconsistency in the Walrasian general equilibrium equations that made it impossible for all markets simultaneously to be in equilibrium. At first, this was seen to be a general problem that might arise given certain sets of parameter values. However, once the real balance effect was introduced and the equilibrium was

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<sup>6</sup> The split between this corresponds approximately, though not exactly, to the split between Boianovsky and Backhouse 2005a and 2005b. Evidence for many of the claims made in Sections 3 and 4 is provided in those papers.

correctly formulated, Patinkin came to the view that equilibrium would always be possible. Inconsistency of the excess demand functions came to be seen as a disequilibrium phenomenon in a different sense of the word disequilibrium: the system was still in motion because prices had not yet converged on their equilibrium values.

Figure 1: Disequilibrium macroeconomics: key links



*Note:* Bold arrows represent pupil-teacher relationships or equivalent (e.g. Negishi was Arrow's research assistant and obtained his PhD later). Other arrows represent significant links that are discussed in Backhouse and Boianovsky (2005a and b). Stanford and Berkeley are shown as adjacent. Malinvaud is part of the French (CEPREMAP) group but his role in the story is why he is listed separately in the diagram. The bunches of arrows from Clower, Leijonhufvud and Barro and Grossman denote that virtually everyone who came afterwards was aware of their work. There are many other links that could be added, but to do so would complicated the diagram to the point of rendering it useless. The main omission is Malinvaud's role as the senior figure behind the entire CEPREMAP group.



Patinkin claimed that Keynesian economics was the economics of disequilibrium, arising when equilibrating forces failed to eliminate unemployment within a socially acceptable period of time. As part of that story he introduced the idea that unemployment should be discussed from a dynamic perspective, with both firms and workers off their respective labour demand and supply curves. The influence of output or anticipated sales was reflected not in the variables but in the form of the labour demand function, which was illustrated by his well-known kinked labour demand curve (see Boianovsky 2006). However, disequilibrium macroeconomics ‘proper’ did not begin until Clower introduced a corresponding constraint on the consumption side: his dual decision hypothesis. The significance of this was that Clower used the dual decision hypothesis to attack the notion that Walrasian excess demands were relevant for the behaviour of markets. It was effective, not notional demands (the terminology itself carried a strong message) that mattered.

Leijonhufvud, in an extraordinarily influential PhD thesis and the book derived from it (1968), took up this theme. Keynesian economics was not about the economics of wage stickiness, let alone wage rigidity: it was about the absence of any ‘Walrasian’ auctioneer.<sup>7</sup> In the absence of an auctioneer to adjust prices in response to excess demands, there was no reason why prices should obey the rules of the tâtonnement process. If there were no tâtonnement, then transactions would take place out of equilibrium and Clower-type constraints would come into operation. Leijonhufvud integrated into this discussion of disequilibrium dynamics a reinterpretation of the liquidity trap, not as a feature of a static demand-for-money function but as a dynamic phenomenon arising from inelastic expectations in a world where prices of goods and assets were changing. Keynesian economics was about coordination failure in a world where there was no auctioneer to coordinate the activities of individuals with different expectations and plans about the future. Heterogeneity of agents was crucial, because if all agents were the same, and had the same expectations, there would be no problem with coordinating saving and investment, for different agents’ plans would automatically be compatible with each other.

The model produced by Solow and Stiglitz (1968) is worth mentioning because, though it bore a family resemblance to the model published by Barro and Grossman three years later, it was not motivated either by Leijonhufvud’s concern with what happened when the auctioneer was absent or by the search for microfoundations. Instead they wanted to suggest a resolution to the ‘two Cambridges’ controversy over distribution by showing that the

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<sup>7</sup> Walrasian is put in quotation marks to avoid implying that Walras believed in such a mechanism.

Kaldor-Robinson theory of distribution prevailed at a demand-constrained equilibrium and the neoclassical theory at a supply-constrained one. Though they modelled quantity constraints and effective demands, prices were set by what was in other respects a tâtonnement process. They had so little interest in microfoundations that, though they recognised that it would be trivial to write down the microeconomic behaviour underlying their model, they did not do so. The relationships they postulated were purely macroeconomic.

It was Barro and Grossman who noticed the parallel between Clower's dual-decision hypothesis and Patinkin's analysis of labour demand and combined them to produce a formal model of how an economy they might interact. They saw such a model, which took prices as given, as providing a way of achieving their goal of a macroeconomics with rigorous microfoundations. This was their main aim: they chose Keynesian economics because, at that time, macroeconomics was Keynesian economics, so if one was to provide microfoundations, it was the Keynesian model that one had to explain. Their model went beyond traditional microeconomics in modelling agents as facing and responding to quantity as well as price signals. Their model worked with representative agents, but households and firms were distinct and their interaction resulted in macroeconomic outcomes that were distinct from what could be found at the microeconomic level alone. In their graduate textbook (1976), Barro and Grossman extended the model to price changes by postulating that prices changed in response to effective, not Walrasian, excess demands: apart from the use of effective excess demands, this was essentially tâtonnement process, simplifying away many of the complications with which Clower and above all Leijonhufvud had been concerned.

At the same time as Barro and Grossman were using quantity-constrained behaviour to produce a microfoundations of macroeconomics, focusing on getting a model of the economy as a whole, Phelps and his colleagues (1970) were pursuing the goal of microfoundations in a different way. They were exploring formal models of search and information. This produced a justification for why the labour market might not appear to be in equilibrium, even though all agents were optimizing given the information available to them. It was an alternative to the Barro-Grossman microfoundations.

#### 4. Responding to the scandal of tâtonnement

In 1959, Arrow identified what he called three scandals in general equilibrium theory: there was no integration of macroeconomics and microeconomics; general equilibrium theory did not incorporate imperfect competition; and it did not take account of the costs of making transactions. These could be summed up as the scandal of tâtonnement, for it was the tâtonnement that caused all three of these problems by implying that prices could costlessly and immediately adjust to clear markets. The challenge was taken up by Negishi and Hahn. Negishi (1961) produced a model of monopolistic competition. This did not simply involve acknowledging that markets were imperfectly competitive because of product differentiation but that imperfect competition arose as the logical consequence of abandoning the assumption that there was a *deus ex machina* who could costlessly bring markets into equilibrium. In other words, the tâtonnement could not be correct and the logical consequence of abandoning it was imperfect competition.<sup>8</sup> If agents were to set prices (and short of a *deus ex machina* prices had to be set by agents in the model) this meant they could not face perfectly elastic demand curves, for they had to be able to choose prices. There was thus a very short jump from this to the joint work by Hahn and Negishi (1962) on non-tâtonnement processes (see also Negishi 1979, chapters 5-7).

Thus when Hahn tackled non-Walrasian equilibria in the mid-1970s, he approached it as part of the project of producing a theory of general equilibrium with imperfect competition. This is easy if drastic assumptions are made, but he did not wish to make those assumptions. He modelled firms as making conjectures about the demand curves they faced, and setting prices accordingly. Given those prices a macroeconomic equilibrium could be worked out and from that it was possible to work out the consequences for firms and the information that they would receive. They would learn from this, revising their conjectures, and the process would go through another iteration. In this model, firms were not representative agents in the sense of being miniatures of the economy as a whole. Their conjectural demand curves, and hence their decisions, would depend not just on the prices they faced, but also on macroeconomic variables.

Though approaching the problem very differently, Clower also was concerned with the process of price formation. In the 1950s, he wrote a series of papers where he sought to

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<sup>8</sup> There are organised markets in the real world but these use resources and are organised by agents within the model.

provide alternatives to the tâtonnement. For example he explored a series of rules about how prices might be changed in response to expected and realised sales, the profitability of alternative pricing strategies and so on. It turned out that the models exhibited complicated dynamics, even for a single market, and he never sought to generalise these rules into a general equilibrium model of the economy as a whole. His motivation, however, had much in common with Hahn's, namely to provide an account of how markets work that could be taken seriously as a model of real-world markets.

Drèze came to disequilibrium macroeconomics from working on resource allocation under uncertainty and from research into public economics (specifically pricing rules for public enterprises). Familiarity with French planning meant that the idea of quantity signals was natural. Having noticed a multiplier process in one of the public economics models he was working on, he went on to prove existence in a model where prices were inflexible. He offered two rationales for this, though neither was developed formally. One was that there were in practice institutional rigidities – prices were not completely flexible. The other was that he was aware that price stickiness could usefully provide income insurance in a world where insurance markets were incomplete. In constructing his model, he assumed not that prices were completely rigid but that they were inflexible in one direction: constraints on price changes involved inequality constraints.

Drèze equilibrium – equilibrium with given prices – became one of the benchmarks in the literature that developed in the mid-1970s by economists centred around CEPREMAP. It was extended in many ways. Grandmont and Younès, for example, showed how a world where markets did not clear could be used to find a role for money, something that was very difficult to do in Walrasian models. In the course of this, Drèze's inequality constraints came to be replaced with fixed prices. The most elaborate model arguably came from Benassy. He assumed that agents took decisions on the basis of perceived constraints, and that these depended on the information they had about transactions in other markets.

## **5. Lost microfoundations**

Economists did not lose microfoundations altogether – it would be ridiculous to suggest that it did. However, the microfoundations that emerged were a poor reflection of the microfoundations that had been sought during the early 1970s. During the 1960s and 1970s, many of those working within the disequilibrium macroeconomics literature had recognised that the tâtonnement had serious flaws as an explanation of how markets worked – there was

a lacuna in the theoretical explanation of prices and it appeared to be empirically inaccurate – and they were seeking an alternative. Some contributors to this literature tried explicitly to model how agents would change prices when such changes were taking place in real time, impelled by the logic of the process to do this. Others had a vision of a more complex adjustment process in which prices almost inevitably failed to adjust instantaneously and were therefore, to a degree that was hard to specify, inflexible. In order to cut through the resulting mathematical complexity, many of them adopted the provisional step, when analysing interactions between agents, of taking prices as given, at least temporarily. The result was fixed-price models.

The authors who in some ways expressed least consciousness of these problems were Barro and Grossman, but it was their model – simple, easy to manipulate, and teachable – that came to sum up disequilibrium macroeconomics to outsiders. As the literature rapidly became established in the 1970s, it became acceptable to explore the properties of fixed-price equilibria, which helped to push aside some of the qualifications to and motivations for this programme. Malinvaud devoted much of his Jahnsson Lectures to providing a more rigorous and elegant microeconomic foundation for the Barro-Grossman model. But, for many, it failed to capture the vision that had motivated their work in this field. Clower and Leijonhufud considered Barro and Grossman unoriginal – they were useful allies in propagating disequilibrium macroeconomics, but little more than that. Fixed-price models failed to capture many of the insights about markets and dynamics to which they attached great importance. For members of the ‘French’ school, the fixed-price model was to an even greater extent one building block – a provisional approximation – in the construction of a more complex model. It certainly was not the more general theory of markets for which they were looking.

What happened was that disequilibrium macroeconomics came to be identified with the fixed-price model and thereby rejected. What Barro kept from the model was the aspect that least endeared it to most of those writing in the disequilibrium macroeconomics literature – the representative-agent approach to microfoundations. He abandoned fixed prices, embraced rational expectations, and by the end of the 1970s he had abandoned disequilibrium macroeconomics. His reason – that there was no reason why fix-wage contracts should be sub-optimal – ignored most of the reasons given for assuming that prices were less than completely flexible. In place of disequilibrium macroeconomics, economists embraced the Phelps programme of formal models of information. Though they modelled information

explicitly in a dynamic context, and were in that sense an advance on the disequilibrium macroeconomics literature, they failed to respond to the challenge that had motivated disequilibrium macroeconomics in that it did not provide an alternative to the tâtonnement (see, however, Howitt 1987 for the different interpretation that Phelps island parable did not rely on the tâtonnement mechanism).

The claim that high inflation rendered fixed price models irrelevant is also far from the way it was perceived at the time. The heyday of these models was precisely the time when inflation was at its peak in many countries. This economic experience was in part the motivation for disequilibrium macroeconomics. Many economists saw the events of the 1970s as evidence that markets were not in continuous Walrasian equilibrium – as evidence that there was no auctioneer who could costlessly coordinate decisions of heterogeneous buyers and sellers. They could not begin to formulate a model capable of capturing the complexity of such a world, so they resorted to dealing with the problem piecemeal, and one of the devices was to work out the consequences of markets being out of equilibrium. Malinvaud was perhaps most explicit in saying that he turned to fixed price models as a way to make sense of what was happening in the stagflation of the 1970s.

New Keynesian macroeconomics, which began to coalesce in the late 1970s and early 1980s<sup>9</sup>, took up some of the ideas that lay behind disequilibrium macroeconomics: it used limited information, monopoly and other factors provided explanations of why labour markets would not clear: to provide explanations of the apparent institutional rigidities in some markets. However its ambition was much more limited. It focused on the labour market, in a sense returning to the previous Keynesian literature. The sense that there was a need for a new microeconomics, because tâtonnement failed to offer a convincing story of how markets worked, or because markets failed to coordinate decisions about saving and investment by agents who had different expectations of the future, vanished from sight. Rather than taking disequilibrium macroeconomics a stage further, the new Keynesian macroeconomics could be presented as a reversion to a narrower programme – providing new microfoundations not for macroeconomics but for wage stickiness.

The new classical macroeconomics, of course, simply embraced tâtonnement and the Walrasian model of perfectly competitive markets that could never be out of equilibrium, using the largely unobservable device of errors in expectations to reconcile fluctuations in employment with this model. Microfoundations were limited to representative agents with

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<sup>9</sup> This is the subject of Backhouse and Boianovsky (in preparation).

rational expectations. This was microfoundations, but one that, compared with the ambitions of those behind disequilibrium macroeconomics was ‘a pretty meagre programme’.<sup>10</sup> In that sense, with the eclipse of disequilibrium macroeconomics, microfoundations were lost.

## 6. The demise of disequilibrium macroeconomics

Why did economists move away from disequilibrium macroeconomics? One explanation (Busetto 1989) is that supporters of disequilibrium macroeconomics failed to find any explanation of why prices should be inflexible.<sup>11</sup> Another one (Rizvi 1994) claims that Benassy, Hahn and Negishi made *ad hoc* assumptions (particular forms of aggregate excess demand functions) in their arguments about existence proofs. However there are perhaps deeper reasons. One has to do with a problem identified by De Vroey (2004, chapters 11-12), who has pointed out that the key assumption concerns ‘trade technology’ – the mechanisms by which trade is organised and prices and quantities are established. He draws a sharp distinction between economists working within the Walrasian and Marshallian frameworks, differentiated by whether or not there is an auctioneer to establish prices. Leijonhufvud (2006) offers a similar explanation: that Marshallian adjustment processes take place in real time, leading to radically different conceptions of equilibrium from those that occur in the Walrasian world. Though our interpretation is similar, we have chosen not to use this Walrasian/Marshallian distinction. The label Marshallian begs many questions and seems better avoided. Moreover, many of the economists who were trying seriously to develop rigorous models of how prices could be formulated in a non-Walrasian world were those most closely associated with Arrow and Debreu: they could thus be seen as arising from the heart of the modern ‘neo-Walrasian’ approach to economics.

The heart of the problem is perhaps that the architects of disequilibrium macroeconomics had a vision of a radical programme for providing macroeconomics with microfoundations that were free of the logical inconsistencies of Walrasian theory and which might explain macroeconomic phenomena, including those that were central to Keynesian economics. However, such a world was immensely complex, so approximations akin to partial equilibrium analysis were needed. To avoid the impossibility of working with a near infinite-

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<sup>10</sup> These are Hahn’s words (email to authors, 25 February 2005).

<sup>11</sup> Clearly with the partial exception of the labour market.

order difference equation (as would be required if all agents were to set prices independently) the system had to be simplified. This resulted in the apparent mixtures of Walrasian and Marshallian methods, diagnosed by De Vroey. It also meant that the simple models that disequilibrium macroeconomics produced failed to do justice to the vision out of which they emerged. They could be viewed as models in which prices were fixed, and rejected accordingly. It also meant that the microfoundations offered by disequilibrium models were, for theorists, not as rigorous as was desired. Conjectural equilibrium, for example, invited analysis in terms of game theory, and it took time before that was understood.<sup>12</sup>

The result was that, as disequilibrium macroeconomics began to be stabilized in survey articles and textbooks, the results were seen to be inconclusive. Weintraub (1977) used the literature on microfoundations (which extended significantly beyond what has been considered here) to mount a defense of general equilibrium theory.

General equilibrium analysis has, for a number of years now, gone far beyond Walrasian typologies to a consideration of many issues, like transactions structures, information costs, speculation, imperfect adjustment, and search behaviour, which are nearer to traditional macroeconomic concerns. ... Those who argue that the current theory is unrealistic fail to appreciate the attention being paid to real adjustment processes in real time. (Weintraub 1977: 19)

However, the subject was ‘not susceptible to neat packages of integrated results’, as a result of which he predicted that the short-term result would be eclecticism. On the narrower question of disequilibrium macroeconomics, he concluded:

Keynesian macroeconomics cannot be derived from any simple Walrasian microsystem. ... the question of *appropriate* microfoundations for macroeconomic theory is still an open one. (Weintraub 1977: 18)

To a certain extent, Weintraub was right: there was eclecticism in that many economists did not accept the new classical macroeconomics. However, it could equally be argued that the result was the opposite, as much of the new classical macroeconomics was accepted, even by new Keynesians and those who dissented from new classical policy conclusions. In

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<sup>12</sup> Hahn has commented that game theorists took a long time to realise that the opponent in his theory was not an individual but a group, impeding their acceptance of his ideas (email 25 February 2005).



particular, rational expectations and the belief that markets would be in equilibrium except where specific reasons were given for them not to be; there was also widespread acceptance of the representative agent model. The new classical macroeconomics resolved the dilemma identified by Weintraub not by creating better microfoundations but by abandoning Keynesian economics. The excitement of rational expectations and the debates that followed led to the neglect, at least by macroeconomists, of the issues about markets that had largely motivated disequilibrium macroeconomics. As part of the process, the microfoundations research program – in the sense of the search for microeconomics compatible with Keynesian macroeconomics - died out; microfoundations became restricted to the requirement that economic models are clearly based on the choices of agents (see also Weintraub, forthcoming).

To say this is neither to say that new classical macroeconomics took a wrong turning, or the contrary. Rather, it is simply to say that there was a change in focus. Issues that in the early 1970s had seemed important no longer seemed so. Thus, whereas invoking a *deus ex machina* to bring markets costlessly and instantaneously into equilibrium had seemed a significant logical and empirical problem to the creators of disequilibrium macroeconomics, it no longer seemed a problem to the new classical macroeconomists and many of those whom they influenced. The theory might not be a realistic account of price formation but it was an acceptable approximation in the process of creating macroeconomic problems that could shed light on policy and other problems.

## **7. Concluding remark**

What happened here is typical of the way economists reinterpret their past. The actual story is one where there was progress by some criteria and failure, or even retrogression, on other criteria. However, economists build a narrative of progress and in doing so they mis-represent the past. Even though Barro had by then moved on, this had certainly not happened by the late 1970s: however able their surveys Weintraub, Grandmont, Fitoussi and Drazen did not close the discussion in the sense that Neghishi (1962), according to Weintraub (1991) stabilized the earlier discussion of dynamics. Perhaps the subject had not yet progressed to the point where this was possible. They certainly did not mis-represent what had been achieved by disequilibrium macroeconomics. But they were beginning, perhaps inadvertently, to reformulate the issues in ways that made it easier for subsequent

economists, whose interest were different, to simplify and rewrite history.

History written by the victors; the task for the historian is not to challenge economics directly, but to correct distortions in the historical account. If correcting them causes economists to wonder whether perhaps some babies disappeared with the bathwater, that is for economists to consider.

## References

- Arrow, K. J. 1959. Towards a theory of price adjustment. In *The Allocation of Economic Resources*, ed. A. Abramovitz. Stanford: University of California Press.
- Backhouse, R. E., and M. Boianovsky. 2005a. Disequilibrium macroeconomics: an episode in the transformation of modern macroeconomics. Paper presented to Conference on History of Macroeconomics, Louvain, January 2005.
- . 2005b. Generalizing general equilibrium: from Arrow to Malinvaud. Unpublished paper.
- Barro, R. J., and H. I. Grossman. 1971. A general disequilibrium model of income and employment. *American Economic Review* 61:82-93.
- . 1976. *Money, Employment and Inflation*. Cambridge: Cambridge University Press.
- Benassy, Jean-P. 1975. Neo-Keynesian disequilibrium theory in a monetary economy. *Review of Economic Studies*. 42: 503-23.
- Benassy, Jean-P. 2002. *The Macroeconomics of Imperfect Competition and Non-Clearing Markets*. Cambridge, MA: MIT Press.
- Boianovsky, M. 2006. The making of chapters 13 and 14 of Patinkin's *Money, Interest and Prices*. *History of Political Economy* 38: 193-249.
- Branson, W. H. 1979. *Macroeconomic Theory and Policy*. 2nd ed. New York: Harper and Row.
- Busetto, F. 1989. Why the non-tatonnement line of research died out. *Economic Notes*.
- Clower, R. W. 1965. The Keynesian counterrevolution: a theoretical appraisal. In *The Theory of Interest Rates*, ed. F. H. Hahn and F. P. R. Brechling, 103-25. London: Macmillan.
- De Vroey, M. 2004. *Involuntary Unemployment: The Elusive Quest for a Theory*. London: Routledge.
- Dow, S. C. 1985. Microfoundations: a diversity of treatments. *Eastern Economic Journal*. 11: 342-60.

- Drazen, A. 1980. Recent developments in macroeconomic disequilibrium theory. *Econometrica* 86:505-16.
- Drèze, J. H. 1975. Existence of an exchange equilibrium under price rigidities. *International Economic Review*. 16:301-20.
- Grandmont, Jean-M. 1977. Temporary general equilibrium theory. *Econometrica* 45:535-72.
- Hahn, F. H., and T. Negishi. 1962. A theorem on non-tatonnement stability. *Econometrica* 33:463-69.
- Harcourt, G. C., ed. 1977. *The Microfoundations of Macroeconomics*. London: Macmillan.
- Hornstein, A. 2004. Review of Benassy (2002). *Journal of Economic Literature* XLII (June): 513-15.
- Howitt, P. 1987. Macroeconomics – relations with microeconomics. *The New Palgrave*, vol. 3. London: Macmillan.
- Janssen, M. 1993. *Microfoundations – a critical inquiry*. London: Routledge.
- . 2006. Microfoundations. *Tinbergen Institute Discussion Paper* 41. Forthcoming in *The New Palgrave Dictionary of Economics*, 2<sup>nd</sup> edition. L. Blume and S. Durlauf, editors. London: Macmillan.
- Krugman, P. 2000. How complicated does the model have to be? *Oxford Review of Economic Policy* 16 (4):33-42.
- Leijonhufvud, A. 1968. *On Keynesian Economics and the Economics of Keynes*. Oxford: Oxford University Press.
- . 2006. Keynes as a Marshallian. In *The Cambridge Companion to Keynes*, ed. R. E. Backhouse and B. W. Bateman. Cambridge: Cambridge University Press.
- Modigliani, F. 1944. Liquidity preference and the theory of interest and money. *Econometrica* 12:45-88.
- Negishi, T. 1961. Monopolistic competition and general equilibrium. *Review of Economic Studies* 28:196-201.
- . 1962. The stability of a competitive economy: a survey article. *Econometrica* 30:635-69.
- . 1979. *Microeconomic Foundations of Keynesian Macroeconomics*. Amsterdam: North-Holland.
- Patinkin, D. 1956. *Money, Interest and Prices*. Evanston, IL: Row, Peterson.
- Rizvi, S. A. 1994. The microfoundations project in general equilibrium theory. *Cambridge Journal of Economics*. 18: 357-77.
- Silvestre, J. 1992. Notes on the non-Walrasian approach to macroeconomics. In

- Macroeconomics: A Survey of Research Strategies*, ed. A. Vercelli and N. Dimitri, 87-126. Oxford: Oxford University Press.
- Solow, R. M., and J. E. Stiglitz. 1968. Output, employment and wages in the short run. *Quarterly Journal of Economics* 82 (4):537-60.
- Stoneman, P. 1979. A simple diagrammatic apparatus for the investigation of a macroeconomic model of temporary equilibria. *Economica* 46:61-66.
- Weintraub, E. R. 1977. The microfoundations of macroeconomics: a critical survey. *Journal of Economic Literature* 15:1-23.
- . 1979. *Microfoundations: the Compatibility of Microeconomics and Macroeconomics*. Cambridge: Cambridge University Press.
- . 1991. *Stabilizing Dynamics: Constructing Economic Knowledge*. Cambridge: Cambridge University Press.
- . Forthcoming. Microfoundations. In *International Encyclopedia of the Social Sciences*, 2<sup>nd</sup> edition. W. A. Darity, editor. New York: Macmillan Reference.