

Disequilibrium macroeconomics: an episode in the transformation of modern macroeconomics

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1. Introduction

Around 1970, economists became interested in a class of models that sought to explain unemployment and related phenomena by starting from the assumption that markets were not in equilibrium with the implication, so it was believed, that this would result in supply and demand functions very different from those of conventional theory. The most prominent paper in this literature, ‘A general disequilibrium model of income and employment’ by Robert J. Barro and Herschel I. Grossman (1971), was for a long time the most widely-cited paper ever published in the *American Economic Review*.¹ However, by the end of the 1980s, disequilibrium macroeconomics had largely disappeared from view: approaches influenced by the New Classical Macroeconomics (including Real Business Cycle theory and New Keynesian Economics) had become dominant. As described by Peter Howitt (1990, p. 10), “for a brief period in the early 1970s”, disequilibrium macro was “the hottest topic in macroeconomics”, a situation that “soon changed dramatically”. The present paper is an investigation of where disequilibrium macroeconomics came from and what happened to it, offering a new perspective on the reasons for its sudden, if incomplete disappearance, which as pointed out by Howitt, are “varied and complex”. We discuss in what extent participants in that research program either became attracted to other approaches or stopped advancing the field of disequilibrium macro significantly.

In some accounts of the history of macroeconomics in this period, disequilibrium macroeconomics has disappeared completely. Many textbooks are silent on it. One such is Blanchard (2003, pp. 572-81), which even presents a history of macroeconomics in which disequilibrium macroeconomics is not mentioned, jumping straight from ‘the neoclassical synthesis’ to ‘the rational expectations critique’ and ‘modern developments’. The story is one of progress: in the wake of Keynes’s *General Theory* (1936), a new synthesis had emerged and during the 1950s and 1960s, there was ‘progress on all fronts’. Keynesians and monetarists did not agree on policy but, despite this, ‘macroeconomics around 1970 looked like a successful and mature field. ... Most debates were framed within a common intellectual framework’ (Blanchard 2003, p. 575). This consensus was then shattered by the events of the 1970s and the idea of rational expectations. This however, led to a wave of new research which resulted in the emergence of a new synthesis, centered on rational expectations, that was accepted even by New Keynesian economists who emphasized the importance of various market imperfections. It is a story on which the end result (represented by his book) is consensus on fundamentals.

In more detailed histories of recent macroeconomic theory, disequilibrium macroeconomics is typically presented in the following terms (see e.g. Blanchard 2000, pp. 1386-87; Blaug 1997, pp. 672-73, 685-87; Mankiw 1990, pp. 1655-56). Economists realized that conventional theories, based on perfect price flexibility could not provide an adequate explanation of Keynesian unemployment and they explored the implications of wages and prices being sticky: the result of price stickiness is that markets do not clear, creating spillover effects in other markets. Disequilibrium could imply a demand multiplier under conditions of Keynesian excess supply in both goods and labour markets, or a supply multiplier with generalized excess demand in those markets, depending on the price vector. Such results were “tantalizing” in the macroeconomics of the 1970s (Blanchard, p. 1386). However, there was the problem of which scenario was more likely, which could only be settled by a theory of price formation. As suggested by Gregory Mankiw (1990, p. 1655), general disequilibrium models ‘*a la* Barro-Grossman do

¹*Current Contents* 13, March 28, 1988, p. 16.

not fit easily into the history of post 1970 macroeconomics. In contrast with most of macroeconomic theories put forward after that, they were not directly aimed at correcting the flaws that provoked the breakdown of the consensus that prevailed until the early 1970s'. More recently, Mankiw (2001, p. C49, n. 1) has claimed that New Keynesian theories started in the mid 1980s should be interpreted as "explaining why the [excess supply] regime in general disequilibrium models is the normal case". In the same vein, Huw Dixon (1997, pp. 176-79) has argued that the essential insights of disequilibrium macro about firm rationing in the output market are based on the notion that price exceeds marginal cost, as later developed in the imperfect competition New Keynesian models. New Keynesian economics then enters as a natural development from this, in that it provides explanations of why prices are sticky and does not simply assume it. Again, the story is one of progress.²

Both versions of what happened to macroeconomics around 1970 – neglecting disequilibrium macroeconomics and presenting it as a primitive forerunner of New Keynesian Economics – miss important elements of the history and hence distort it. Though one received far more attention than the other (for reasons that are discussed later) the story hinges around two papers, Solow and Stiglitz (1968) and Barro and Grossman (1971), each of which presented a disequilibrium model. Though these models were similar, and were almost universally portrayed as building on the insights of earlier work by Don Patinkin (1956/1965), Robert W. Clower (1965) and Axel Leijonhufvud (1968), it is claimed here that this work represented the coming together (or arguably the collision) of at least four distinct research programs. The differences between these research programs help explain why the authors of disequilibrium macroeconomics moved in different directions in the 1970s, most of them moving away from the type of model with which disequilibrium macroeconomics had come to be associated (that of Barro and Grossman 1971). One of the new directions, adopted by Clower and Leijonhufvud after the mid 1970s, was the quest for an account of market organization different from the initial Walrasian *tâtonnement* set-up now deemed incompatible with the attempted explanation of involuntary unemployment phenomena (see Howitt 1979, and de Vroey's detailed discussion in part IV of his 2004 book). The result was that, by the late 1970s, there remained few who wished to defend disequilibrium macroeconomics against the New Classical onslaught.

The focus of this paper is on one very narrowly defined episode that forms part of a much larger story. The literature discussed so far is exclusively macroeconomic, but similar issues were being raised in the context of microeconomic, general equilibrium theory: some theorists were developing models with rationing, and others were exploring the implications of imperfect competition for general equilibrium. However, though they were influenced by the literature discussed here, there was little influence in the other direction. Patinkin, for example, has admitted that he did not attend to the details of the work of Dreze or Benassy,³ and even that he was not mathematically competent to judge it.⁴ It therefore makes sense to discuss this literature first, postponing discussion of the general disequilibrium literature to another paper (Backhouse and Boianovsky, in preparation). The treatment of the disequilibrium economics as a chronological sequence of "macro" and "micro" formulations can be also found in surveys of that literature produced in the late 1970s (see e.g. Weintraub 1979; Drazen 1980). These two types of theory were brought together in Edmond Malinvaud's *The Theory of Unemployment Reconsidered* (1977). Though seen by many as an elegant exposition of the type of model previously presented by Barro and Grossman, its bringing together these two approaches is sufficiently important to take it as marking the end of one episode in the history of disequilibrium macroeconomics and the beginning of another.

A few features of the disequilibrium macroeconomics literature are illustrated in Figures 1 and 2. Figure 1 shows citations of Leijonhufvud (1968) from its publication to the present day. This shows clearly the fluctuating interest in the subject. Figure 2 shows more citation counts for a wider range of papers for a more limited period. This shows clearly, that by the 1980s, Leijonhufvud and Malinvaud were the most cited authors in this literature, and that citations fell dramatically at the end of the 1980s. By 2000, if Patinkin is excluded (as being cited for reasons other than disequilibrium macroeconomics)

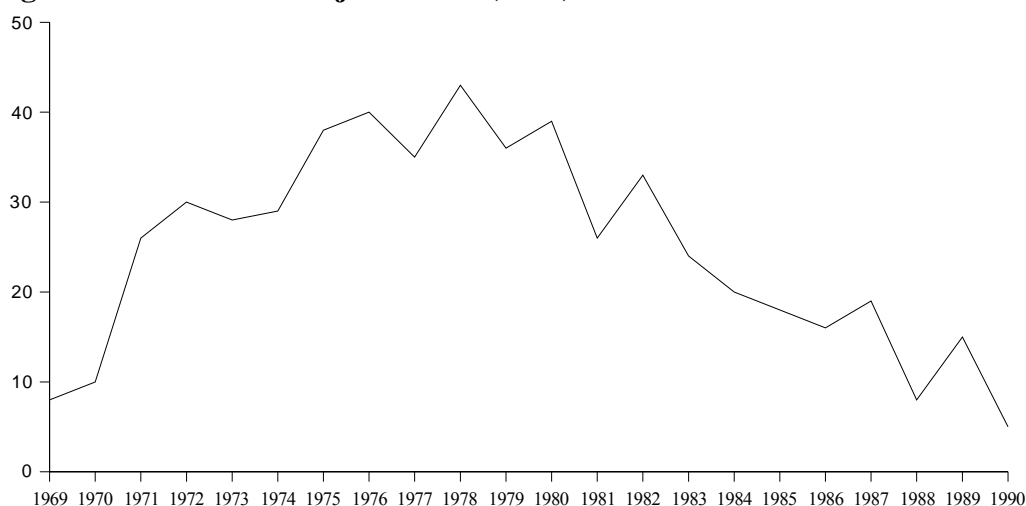
²Backhouse (1995) presented one account in this vein. This paper can be seen either as a criticism or a development from that chapter.

³Letter to Solow, July 2, 1989.

⁴Letter to Grandmont, June 12, 1987.

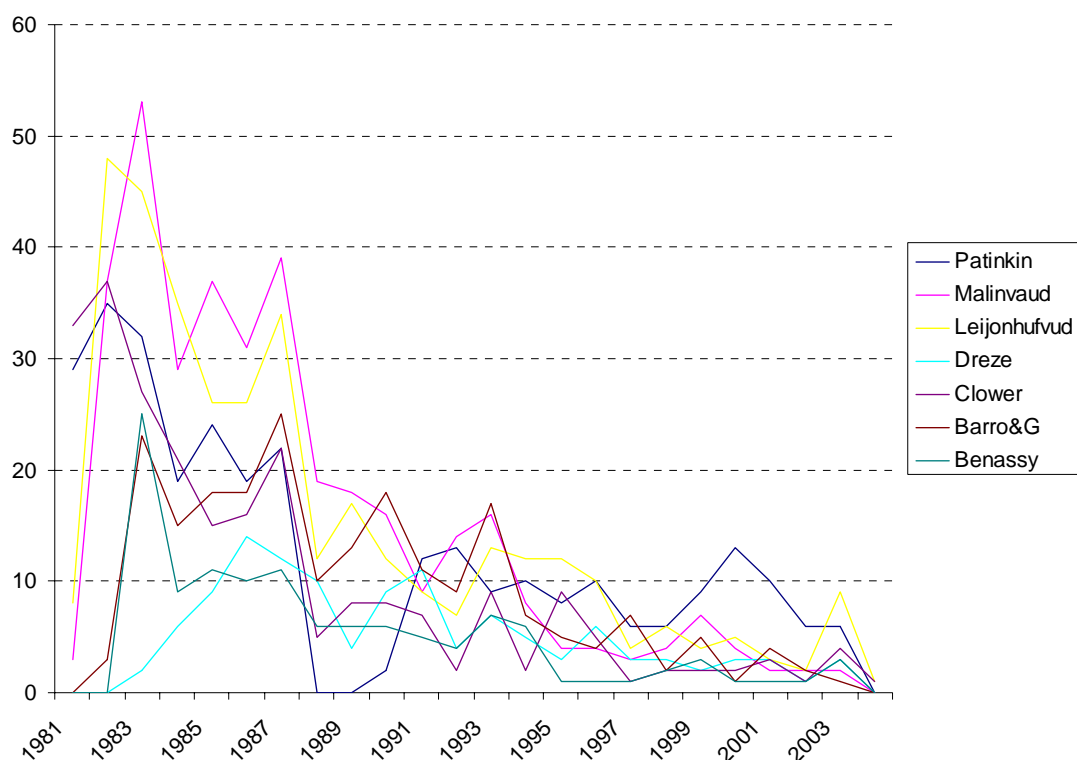
citations were at a very low level indeed, though not zero.

Figure 1: Citations of Leijonhufvud (1968)



Note: Taken from Backhouse (1995). Derived from manual count of printed Social Science Citation Index and not identical to data in Figure 2.

Figure 2: Citations of leading works on disequilibrium macroeconomics



Notes: Online Social Science Citation Index. Apologies for lack of clarity in the lines. Patinkin includes references to MI&P that do not refer to DM. 1981 figures are probably incomplete due to the way articles are reported in the SSCI.

The paper starts, in Section 2, with an account of those economists whose work is considered to have laid the foundations of disequilibrium macroeconomics in the 1950s and 1960s, Patinkin, Clower and Leijonhufvud. Their intellectual trajectories are described and the differences between them, in particular between Patinkin and the others, are clarified. Section 3 then proceeds to the central models, and places them in the context of their authors' concerns, which were not the same as those of the forerunners. This then leads, in Section 4, to the routes that this group of economists took during the 1970s – routes that took most of them away from disequilibrium macroeconomics. Section 5 describes the aftermath, outlining the career of disequilibrium macroeconomics after 1977 in no more detail than is necessary to see the significance of this episode. Finally, some conclusions are drawn in Section 6.

2. Three research programs, 1956-68

Patinkin, inconsistency and unemployment

Money, Interest and Prices is most well-known for its integration of monetary theory with the theory of value through the real balance effect. This has nothing specifically to do with disequilibrium macroeconomics, most of the book dealing with situations where markets clear. However, in Chapter 13, in which Don Patinkin (b.1922, d.1995) applied his model to involuntary unemployment, he derived this by considering a fall in the demand for commodities in a world where prices and wages did not respond immediately (Patinkin 1956, pp. 214-24; 1965, pp. 318-28). In this case firms will find that they cannot sell all the commodities they wish to sell and as a result they will reduce their demand for labor below the amount given by the labor demand curve, which is constructed on the assumption that they can sell all they wish as the prevailing prices. Reductions in real wages will not cause them to increase their demand for labor, with the result that the classical mechanism for eliminating unemployment will not work. The argument was illustrated diagrammatically by a change in the usual labour demand curve, which now features a kink at the employment level determined by effective demand (p. 316).

Although this is only one chapter in Patinkin's book, and was developed less rigorously than the parts dealing with equilibrium, there is strong evidence that he considered this the most important part of the book. In contrast with the real balance effect, introduced only in Patinkin (1948) the discussion of disequilibrium represents the outcome of a research program going back to his PhD dissertation in Chicago (Patinkin 1947). Patinkin had been taught by Oskar Lange, and under his influence he had tried to explain Keynesian unemployment in terms of an inconsistency in the equations for general equilibrium. That part of the dissertation came out, in revised form, in Patinkin's 1949 *Economic Journal* article, but it was only in the 1956 book that Patinkin developed the argument that firms are off their labour demand curves under unemployment conditions (see Boianovsky 2002a, 2006; Rubin 2002)..

Patinkin's program, therefore, was to explain unemployment in a world where prices and wages responded slowly to changes in supply and demand; firms would absorb some of the impact of changes in demand by allowing their inventories to rise or fall, but they would be unable to absorb all changes in this way. He focused on firms' decision making and spillover effects from commodity markets to the labor market. The formal theory related to the polar cases where prices and wages were either completely flexible or completely rigid; he offered verbal account of the intermediate case but 'did not succeed in integrating the intuition with formal economic analysis',⁵ this being the reason why there was no mathematical appendix to the relevant sections (2 and 3) of Chapter 13.⁶ In a long footnote, Patinkin (1965, p. 323, n. 9) admitted that his suggested solution to the puzzle of the influence of the firm's output on its labour-input is beset by a "basic analytic problem". Although the kink in the labour demand curve makes sense from the point of view of the economy as a whole, "by definition of perfect competition this kink cannot be taken into account by any individual firm" (see also Boianovsky 2002a, 2006).

Clower and disequilibrium price dynamics

Like Patinkin, Robert Clower (b. 1926) was convinced that Keynesian economics had to be understood in terms of disequilibrium. However he placed more emphasis on dynamics. He has described himself as having an iconoclastic frame of mind that inclined him towards doubts about orthodox economics (Clower 1984, pp. 259-60). This took the form of a concern with 'how markets work, seeing 'no special virtue in the excess-demand adjustment rules of established theory', for he 'knew too much about economic organization to imagine that the prices of more than a handful of commodities were determined on a day-to-day basis by impersonal market forces' (Clower 1984, p. 260).⁷ His first 'significant' publication (Clower and Bushaw 1954) was on stock-flow analysis, a dynamic problem. Clower

⁵Letter Patinkin to Leijonhufvud, Feb 12, 1974.

⁶At a 1987 conference in Perugia, Patinkin said that he did not know how to write an appendix (see Boianovsky 2004)

⁷It may be significant that much of his early career involved applied work and Visiting African countries. See de Antoni (1999) and Costa (2002) for an overview of Clower's life and contributions

continued his work on dynamics during the late 1950s, when he produced a series of papers on the process of price determination.⁸ In a paper prepared for a staff seminar on November 16, 1956, ‘Toward a theory of unified market mechanisms’, Clower sought to develop a theory based on the following premises: (1) buyers tend to purchase from the seller who offers the lowest price; (2) each seller in a given market tends to vary his current sale price when actual sales differs from current sales offers; (3) Sales offers vary when either estimated marginal profit is not zero or actual sales are not equal to current sales offers.⁹

In another paper dating from this period, Clower wrote that there was a common conceptual problem underlying theories of monopoly, oligopoly and pure competition: ‘to formulate self-contained, logically coherent, and intuitively satisfying description of the determination of the output, price and sales of each seller’ (1956/58, p. 1; these words are underlined in the original).¹⁰ He went on to provide a dynamic, period analysis of how prices and quantities changed¹¹. He assumed that sellers set asking prices (p_i) and offered goods for sale on a take-it-or-leave-it basis, and that planned sales (x_i) would typically diverge from actual sales (y_i), deriving a model comprising $3n$ first-order difference equations, where n was the number of commodities. Some aspects of this were conventional, such as the n equations relating changes in asking prices to the differences between planned and actual sales. Less conventional was his equation relating the change in actual sales to both ‘unsatisfied market demand’ and ‘the differences prevailing among individual asking prices at the beginning of period t ’ (Clower 1956/58, p. 5):

$$y_i(t) = y_i(t-1) = A_i \{D[P(t)] - S(t-1)\} + \sum_j B_{ij} \{p_j(t) - p_i(t)\}$$

where $P(t) \equiv \text{Min} [p_1(t), \dots, p_n(t)]$.

This equation is useful, not for the details, but because it shows the way Clower was searching for an alternative to the conventional tatonnement process.

The well-known ‘Keynesian counter-revolution’ paper (Clower 1965) was written in 1962, for a conference on the role of money in general equilibrium theory organized by the International Economic Association near Paris. Unable to think of anything new to say on the topic, recalling an earlier exchange between Hicks (1957) and Patinkin (1959) he turned his attention to Keynes and general equilibrium, and wrote the paper in 10 days. This posited the dual-decision hypothesis to explain why demands for commodities would differ from those given by the Walrasian equations (which Clower termed ‘notional’ demands, distinguished from the ‘effective’ demands of Keynesian theory). The paper can clearly be seen as a contribution to the literature in which Patinkin was involved. In correspondence of 3 March 1962 with Patinkin, Clower referred to “the ideas adumbrated in the second half of your book on disequilibrium systems”. He regarded that as “still the weakest part of your entire structure, and the weaknesses arise from undue concentration on the equilibrium properties of household models in Part I. I will say no more on that here, since your concentration on consumer equilibrium saves me all kinds of time to concentrate on consumer disequilibrium – an area in which I am currently specializing, with what I think are interesting results.” The rest of the letter makes clear that Clower’s contribution to the 1962 conference was prompted by what he perceived as some analytical problems of Patinkin’s macroeconomic model, although he did not refer to Patinkin’s treatment of demand for labour in the correspondence or in the paper. In particular, Clower pointed out that “you cannot legitimately put income into your demand functions in Part II [“Macroeconomics”] of your book, if you suppose that individuals earn income from inside the system – for then income is not an independent variable.” That was the heart of the matter, for “you do not in fact have income coming from heaven in Part II, nor could you and still talk about employment and unemployment. But if you want to stick to utility analysis so be it.” Clower (1965, pp. 111-12) would repeat that piece of criticism in the article. Like the published version of his previous paper, the 1962 piece contains no discussion of Clower’s theories about price dynamics. However, to understand Clower’s subsequent intellectual trajectory, it is important to locate it against the background of his earlier work on disequilibrium dynamics.

⁸Clower Papers, Duke University, Box 4.

⁹Clower Papers, Duke University, Box 4.

¹⁰The typescript is undated apart from a handwritten annotation, presumably by Clower, ‘1956 – Draft 1958’.

¹¹It is tempting to speculate that his analysis of market periods was influenced by his former supervisor, John Hicks.

In his role as a discussant of Clower's paper at the 1962 IEA conference, Patinkin agreed with what he saw as the "basic message" of the paper, that is, the interpretation of the Keynesian system in terms of a "disequilibrium model" (Brechling 1965, "Summary record of the debate", p. 301). The difference between the two approaches is made clear by Clower's result that there is no pressure to reduce prices in unemployment, for the constrained demand for goods is equal to planned supply of goods at prevailing price and income levels. In contrast with Patinkin, Clower followed Keynes up in assuming that the goods market always clear (cf. Brechling 1965, p. 304). The main question investigated by Clower (1965) was what excess demands drive prices. If prices respond to effective excess demands instead of notional demands, as claimed by Clower (1965, p. 123), the dynamic behaviour of the economy will differ from that predicted by the Walrasian tâtonnement. In particular, under the assumption that effective demand depends on realized rather than notional income, there would be no pressure on wages and prices to move in opposite directions and for real wages to fall under unemployment, since Walras's Law does not hold for the demand functions described by the dual decision hypothesis.

Convergence to full employment through changes in real wages was still possible, but the mechanism could not be illuminated by classical theory. Clower's model illustrated Keynes (1936, ch. 19) concern that money-wage cuts have a direct negative impact on aggregate demand via reduced realized income. The model, however, could not account for the indirect effect through interest rates (discussed by Keynes) or real balance effects (discussed by Patinkin), since money was not introduced. Money was mentioned by Clower in the debate that followed his presentation, when he discussed the possibility that the reduction in demand resulting from excess supply of labour could bring about higher prices to the extent that it resulted in lessened demand for an asset rather than a reduction in demand for commodities (Brechling 1965, p. 305). However, Clower insisted that the essential characteristics of the dual decision hypothesis would come out more clearly if money demand was not introduced at the outset. He would change his mind a few years later, though, when he put forward a model of monetary transactions featuring the so-called "Clower (or cash-in-advance) constraint" (Clower 1967; see also Boianovsky 2002b).

Leijonhufvud and coordination failures

On Keynesian Economics and the Economics of Keynes (1968) was Axel Leijonhufvud's (b. 1933) PhD thesis, written for Northwestern University. The process of writing the dissertation took place between 1964 and 1967, after Leijonhufvud left Northwestern to take an appointment at UCLA, where he received advice from Clower. However, though he had read the German version of Clower (1965), the thesis made little use of Clower's dual decision hypothesis, the more extensive discussion of this being introduced only when the thesis was rewritten into the book. It was at this stage that he was strongly influenced by Clower and Alchian. Despite earlier efforts by Patinkin (1956) and Clower (1965), it was Leijonhufvud's 1968 book that succeeded in stimulating interest in macroeconomic analysis under non-market clearing conditions (cf. Barro and Grossman 1976, p. xi). Its theme is that 'Keynesian economics' as it existed in the 1960s failed to do justice to the concerns of Keynes, who was concerned not with wage stickiness but with a much deeper problem of system-wide coordination failure. Part of his concern was with doctrinal history: with demonstrating that economists had got Keynes systematically wrong. However, his concerns went significantly beyond doctrinal history. He recognized that he was reading between the lines of Keynes's *General Theory* (though nowhere near to the extent that some commentators suggested)¹² and that in places he knowingly sought to extend Keynes ideas.

Leijonhufvud made his approach very clear in a letter to Patinkin on June 15, 1974:

I thought I was discussing contemporary theory issues ("contemporary" meaning early '60's—when I did my graduate work and not doctrine-history. In theory I had been taught general equilibrium models on the one hand and Keynesian models (by Modigliani) on the other; I was struck mostly by the incompatibilities, offended by the "Neoclassical synthesis", and saw no palatable way out. It was in searching for a way out from this cul-de-sac that I eventually started to "backtrack" into doctrine-

¹²Grossman (1972) and Coddington (1976); see Leijonhufvud (2000, pp. 24-7) for comments on their criticisms.

history, trying to identify, as it were, the forks in the road where the major decisions on what conceptual experiment to pursue were made.

The big point made by Leijonhufvud in his book was that Keynesian economics was about how an economy would behave if there were no Walrasian auctioneer to bring markets into equilibrium. An important part of that story was Clower's dual-decision hypothesis: in the absence of an auctioneer prices will fail to clear markets, trade will take place at disequilibrium prices, commodity demands will diverge from Walrasian demands, and as a result activities may fail to be co-ordinated. However, there was much more than this. Reflecting a long-standing interest in aggregation problems, on which he had written working papers in 1961-2,¹³ before he had started his PhD dissertation work, Leijonhufvud argued that economists had missed much of what Keynes was trying to say through failing to see that the way he aggregated financial and real assets was different from the conventional way. Keynes, Leijonhufvud claimed, treated bonds and capital goods as a single aggregate, and he did not aggregate consumer goods and capital goods (Leijonhufvud 1968, pp. 135ff). The result was that, for Keynes, the rate of interest was a long term rate and was the relative price of real assets as much as the relative price of two financial assets.¹⁴

This opened the way for Leijonhufvud to argue that the heart of the *General Theory* lay in an inter-temporal coordination failure. The rate of interest is the relative price of current and future consumption and Leijonhufvud claimed that Keynes's theory of liquidity preference amounted to a theory of why the rate of interest would fail to coordinate economic activities. If, because of speculative activity, the rate of interest were too high, the result would be a level of investment that was lower than the amount that consumers wished to save.

Although Clower's influence was important, it should not be overemphasized. As pointed by Leijonhufvud in correspondence of 15 June 1974 with Patinkin, "although my degree was from Northwestern, Clower was in Liberia and England during my stay there and I only got to lay emphasis on his work after I came to UCLA - so there was not the direct connection there that most people tend to assume." Indeed, Clower wrote to Leijonhufvud in 4 October 1964 that "I am returning the copy of Chapter III with some marginal comments. I found the discussion as a whole very rewarding. I must say that I had never thought particularly about the possibility of connecting the Keynesian type of adjustment with the sort of short-run quantity adjustments discussed by Marshall - but your discussion of the matter convinces me that there is something in that way." Clower was referring, of course, to Leijonhufvud's well-known claim about the reversal of the velocity of Marshallian price and quantity adjustments in the short-run.

Leijonhufvud's message, therefore, was that in the absence of an auctioneer to ensure that appropriate prices prevail, there will be systematic coordination failures, involving goods, labour and asset markets. These coordination failures arise not from rigidities, or any simple barrier to price adjustments but from features that are inherent in any decentralized, dynamic economy - in other words, in any economy in which the auctioneer was absent. In an interview given to Brian Snowdon in 17 May 2002, Leijonhufvud explained that the 1968 book "is essentially about the kind of information questions that do not occur in neoclassical Walrasian general equilibrium models. The issues I was dealing with had to do with how information and communication flow in the system so as to enable a coordinated solution to be achieved." The book was also influenced by Alchian's (1969) analysis of price formation with costly information, which Leijonhufvud read in the form of an UCLA working paper (see Howitt 2002).

3. Modeling macroeconomic disequilibrium, 1968-71

Solow and Stiglitz and the dynamics of income distribution

Joseph Stiglitz (b. 1943) was a graduate student at MIT at the time when interest in growth theory was at its peak, the 'Two Cambridges controversy', in which Robert Solow (b. 1924) was one of the major

¹³Leijonhufvud Papers, Duke University, Box 1, contains working papers on the aggregation of interests (September 1961) and 'Coalitions and the aggregation problem' (January 1962).

¹⁴Leijonhufvud Papers, Duke University, Box 1 contains early discussions of the rate of interest with Burstein and William Jaffe. This included a paper on Boehm-Bawerk.

participants, arguably reaching its climax in 1968 (see Harcourt 1972).¹⁵ It involved disputes about the measurement of capital and about income distribution: whether the distribution of income was determined by marginal productivity (the neoclassical view held at MIT) or by the rate of capital accumulation and the capitalists' propensity to save (the view of Joan Robinson and Nicholas Kaldor in Cambridge).¹⁶ At MIT and during a year at Cambridge, Stiglitz worked on problems of economic growth, capital accumulation and the dynamics of the distribution of income, which formed the basis for his PhD thesis. Solow (for whom Stiglitz had once worked as a research assistant at MIT) had also been working on economic growth, capital theory and distribution, and starting with a visit to Cambridge (England) in 1963-4, had been arguing with Kaldor over income distribution. He wanted to work out the difference between the two theories of distribution.¹⁷ This was the background to their jointly written paper 'Output, employment and wages in the short run' (Solow and Stiglitz 1968).

The purpose of the paper was to clarify the relation between the marginal productivity theory of income distribution and the 'Cambridge' theory of income distribution (Solow and Stiglitz 1968, p. 537; Stiglitz 1993, p. 151). They started from the insight that in marginal productivity theory, the real wage adjusted to clear the labor market, but that in the Cambridge theory its function was to clear the commodity market. The model had several characteristics that marked it out as disequilibrium macroeconomics. The change in employment, $N' = dN/dt$, was given by:

$$N' = \theta (F^{-1}(\min(Y^S, Y^D)) - N).$$

Y^S is the output that maximizes profit given the real wage. Y^D is investment plus consumption out of wages and profits, a higher propensity to save being associated with the latter than the former (the hallmark of the Cambridge theory). Output was also associated with a short-side condition, though without the lag structure they associated with employment:

$$Y = \min(Y^*, Y^D),$$

where Y^* was the momentary supply of output given by $F(N)$. The other key component of the model was price and wage adjustment equations

$$p'/p = h(Y^D/Y^S) + j w'/w \quad j \leq 1,$$

and

$$w'/w = h(N/N^S) + kp'/p \quad k \leq 1.$$

They solved the model by working out stationary loci for employment and showed that there could be a wide variety of stable equilibria could exist (as well as unstable equilibria), depending on the parameter values. From this their conclusions about theories of income distribution followed.

Solow and Stiglitz (1968, pp. 542-43) were fully aware of the "Patinkin problem" discussed in section 2 above – that is, the problematic conciliation of excess supply and perfect competition in the goods market. Under those conditions, price exceeds marginal cost, which encourages each producer to attempt to "increase his profits by selling more at the going price and, as a perfect competitor, he ought to succeed. But all producers together can sell no more than Y^D for the going real wage." Solow and Stiglitz's way out of the difficulty was to "assume like Patinkin that, despite the excess of price over marginal cost, producers in the aggregate are restrained from increasing output beyond $Y = Y^D$ by the *force majeure* of effective demand. Under conditions of aggregate excess supply, however, there may be downward pressure on prices."

Barro and Grossman and the search for microfoundations

The model that brought these four research programs together and, more than any other, was responsible for leading economists to take disequilibrium macroeconomics seriously, was Barro and Grossman (1971). In graduate school, at Harvard, Robert Barro (b. 1944) became influenced by Clower (1965) and

¹⁵See Stiglitz, 1993, 2002.

¹⁶The precise formulation of the theory was the subject of controversy. This formulation is Pasinetti's. See Harcourt (1972) for more detail. In the remainder of this paragraph, Cambridge refers to the English city.

¹⁷Letter from Solow, July 12, 2004.

Patinkin (1965, chapter 13), noting that they seemed complementary ‘in terms of how excess supply spills over from one market to another’ (Snowdon and Vane 1994, pp. 269-70). Together with Herschel Grossman (b. 1939, d. 2004), a colleague at Brown University, he used this to provide a choice-theoretic foundation for Keynesian economics to replace the ‘embarrassingly weak choice-theoretic’ basis that he found in ‘post-Keynesian’ macroeconomics (Barro and Grossman 1971, p. 82). Barro’s reason for tackling Keynesian economics was not that he considered himself a Keynesian, but that at the time ‘it seemed to be the only game in town in terms of macroeconomics’ and he wanted to understand it (Snowdon and Vane 1994, p. 269).

They started their model by outlining Patinkin’s analysis of the labor market, though augmenting his diagram by actually drawing a labor demand curve that was constrained by realized sales of output when output was less than the quantity that firms wished to sell. They then developed a parallel analysis of demand for commodities, based on utility maximization and Clower’s dual-decision hypothesis. The result was a pair of diagrams in which demand curves for labor and commodities were truncated, each constrained by realized sales in the other market. From there they went on to analyze the various configurations of the economy that could arise, taking the wage rate and the price level as parameters. A further diagram offered a version of the ‘Keynesian cross’, in which a demand function intersected a supply function, that was clearly a generalization of the conventional 45-degree line diagram of elementary textbooks. They appeared to have demonstrated how their more rigorous choice-theoretic model could explain Keynesian economics.

All the research programs that fed into disequilibrium macroeconomics came together in Barro and Grossman’s article, and it was they who noted the parallels. The view that Patinkin and Clower were both addressing trading at nonmarket clearing prices was first put forward by Grossman (1969). This was something that Clower and Patinkin had missed. Barro and Grossman also cited Leijonhufvud (1968) as having pointed out the inadequacy of Keynesian economics to represent the economics of Keynes – an inadequacy that their model was to put right – although, as pointed out in Grossman 1972 as a criticism of Leijonhufvud 1968, it was incorrect to suggest that disequilibrium macroeconomics could be found in Keynes 1936. In a footnote, Barro and Grossman (1971, p. 84, n. 6) acknowledged that Solow and Stiglitz had presented a ‘somewhat similar’ analysis, though emphasizing different questions. They pointed to three differences, the first (and arguably most important) being that they did not discuss the choice-theoretic basis of their theory. Given Barro’s and Grossman’s aims, this was a significant difference. They also noted that the model of Solow and Stiglitz did not determine the absolute price level, whereas theirs did; and that the lagged adjustment of employment was a complication that ‘obscure[d] what would seem to be essential in the intermarket effects of disequilibrium’ (ibid). These comments are significant because they point to the difference between the two research programs. Unlike Barro and Grossman, Solow and Stiglitz approached the problem from the standpoint of growth and dynamics. Explaining the price level was not their goal and in any case, rather than simply postulate, with Barro and Grossman, that household utility depend on real money balances, they had decided to postpone treatment of the monetary sector to a subsequent paper.

It was only after Barro and Grossman 1971 that Patinkin understood the connection between his and Clower’s model (see Patinkin 1995). As Patinkin implied in correspondence of February 12 1974 with Leijonhufvud, he felt vindicated by Barro and Grossman (1971) and especially Grossman (1972), who gave his chapter 13 the acknowledgment he had never got before. Leijonhufvud (1968) cited Patinkin, but referred to his treatment of real balance effect and his price-theoretic approach, not to chapter 13. On June 15, 1974, he wrote to Patinkin: ‘I read your book, very thoroughly, as a graduate student & could not, then, understand or appreciate Chapter 13.’ He went on to offer an explanation of why he did not pay more attention to it: ‘As you note the message of 13 did not “fit the modl”-- and at the time I was trying very hard exactly to learn the “modl”’. In the same letter he explained that he did not see the link between Patinkin’s work and his own, even after Barro and Grossman. “The full story is even more odd: Even when Barro & Grossman linked your Chapter 13 with Clower’s paper, I refused to see a connection (and wrote G. to that effect).”¹⁸ Leijonhufvud’s attitude to Barro and Grossman’s article was explained in an exchange of letters with Errol Glustoff. Glustoff (January 14, 1972) had asked him, ‘I do not see what is

¹⁸Presumably ‘G’ stands for Grossman.

original about this paper. That is, what is it that they have done which you didn't do, aside from giving Patinkin more credit?' Leijonhuvud (January 22, 1972) wrote in reply, 'I agree that there is nothing very original about the Barro-Grossman paper.' Leijonhufvud's printed reaction to Barro-Grossman came out in his 1973 piece on "Effective Demand Failures" (see next section). As he explained to James Witte in letter of 26 January 1973, the 1973 paper "also takes exception to the Barro-Grossman interpretation – although rather diplomatically and discretely since we don't have allies to waste, exactly."

4. Four ways forward after 1971

Continuing in disequilibrium macroeconomics – Solow and Patinkin

A useful metaphor for what happened in disequilibrium macroeconomics around 1970 is that several research programs collided, bouncing in different directions.¹⁹ Like balls on a pool table (or snooker table) after a collision balls may continue in the same or different directions. Of the seven economists involved, only Solow maintained a clear commitment to disequilibrium macroeconomics as it emerged in the early 1970s.

After the Barro-Grossman book, and even more so after Edmond Malinvaud's little book [1977], I was one of the very few Americans who took that disequilibrium-macro line of thought seriously. (Barro and Grossman were not among them!) I used to teach it regularly in our basic graduate macro course; I never came across anyone else who did.²⁰

Solow (1979, p. 345) presented the disequilibrium macroeconomics as a way forward, augmenting it with a long explanation of why prices and wages might be sticky.²¹ As late as 2000, Solow was alone in defending disequilibrium macro in America. "I hold the minority view that the fixed-price models of Malinvaud (1977) and Benassy(1986) and others were never given a fair trail by American economists" (Solow 2000, p. 152). He suggested that the absence of a serious account of how prices eventually move, plus related ambiguities about the concept of effective demand, were behind the negative verdict. However, with the exception of the disequilibrium model he adapted from Solow and Stiglitz (1968) and Malinvaud (1977) to investigate the consequences of supply shocks (see Solow 1980), he did not try to develop the analysis of non-clearing markets. In response to the question of why he and Stiglitz did not do this, Solow responded that it was probably not an answerable question. It was almost tautological to say that they were both doing things that seemed more interesting and more exciting, though he concluded, 'Of course if the 1968 paper had got other economists excited, I'm sure that we would have kept working at those disequilibrium-type problems. As I've said, I still think it's a real unsolved problem'.²² The nature of the "unsolved problem" was mentioned in Solow's reaction to Patinkin's new 1989 preface.

I always rather liked that stuff [Benassy et al] and taught it when it was being ignored in American universities. Solow-Stiglitz (1968) lives on only in acknowledgments from Benassy and Grandmont, probably because we were concerned only with macro and not at all with micro-foundations... What version of "excess supply" creates effective pressure on wages and prices? The Benassy version is not very satisfactory, but nothing better has come along. It's a genuine problem for disequilibrium macroeconomics (letter of June 14 1989 to Patinkin).

Like Solow, Patinkin ceased to be actively involved in research into disequilibrium macroeconomics. His work on applied economics relevant to Israel took much of his time, and he moved more and more into the history of Keynesian economics and the Chicago school (see Mehrling 2002; Backhouse 2002). However his commitment to keeping *Money, Interest and Prices* in print, a task that took considerable effort,²³ and available to be used as a graduate textbook, and his clear statements about the importance of

¹⁹The analogy is obviously far from perfect as there are numerous external influences and non linearities which, one hopes, are absent from the motion of balls on a pool or snooker table.

²⁰Letter from Solow, July 12, 2004.

²¹A literature to which he made other contributions.

²²Ibid.

²³ See also Solow's interview in Klammer (1983), where he argued that, despite its problems, the disequilibrium approach was

Chapter 13, suggest that he remained sympathetic towards disequilibrium macroeconomics. In correspondence with Leijonhufvud of 12 February 1974, Patinkin informed that “one of the things I want to get around to shortly... is to examine the recent developments in the theory of unemployment (namely, the work of Clower and yourself, and the literature to which that has given rise) in order to see if it provides an answer to the difficulties noted in the aforementioned footnote in my book.” As mentioned in section 2 above, Patinkin (1965, p. 323, n. 9) had been unable to explain why firms unable to sell all they want cannot find buyers by reducing their relative prices rather than accepting as given an absolute sales constraint, a problem he identified as the main stumbling block in the field of disequilibrium macro (see also Howitt, 1990, pp. 10-11, n. 12). A couple of years later, Patinkin drafted a “research proposal” entitled “A Reexamination of Keynesian Economics” (July 1976, pp. 3-4), which listed the “theory of unemployment” as one of the items in the agenda.

However, with the exception of his entry on “Walras’ Law” in the *New Palgrave*, Patinkin did not succeed in his endeavors. The development of the fixed-price literature (Dreze, Benassy, Malinvaud) did not help things either, as pointed out by Patinkin at the proceedings of the 1987 Perugia conference. “I looked at the development of the literature, what is called fixed-price and quantity restraints, as an attempt to deal with those problems, although frankly I still feel that some of the basic problems still remain.” The core of the problem was economic, not mathematical: “You can develop a whole complicated mathematical theory of quantity restraints, but in most of the literature I don’t see the person going back to ask why the quantity restraint exist. Which was what bothered me and continues to bother me.” The notion that disequilibrium macroeconomics seemingly violates the assumption of rational economic behaviour was, according to Patinkin’s 1989 (p. xx) new introduction, behind its rejection by New Classical macroeconomics in the late 1970s. That situation changed with the arrival of New Keynesian economics in the 1980s, which “on the basis of various assumptions, has rationalized the seemingly irrational.” Patinkin had in mind the assumption of imperfect competition plus efficiency-wage theory, and referred to Stanley Fischer’s - with whom he kept in close contact - 1988 survey.²⁴

Looking elsewhere for microfoundations – Barro and Grossman

The turning point in the history of disequilibrium macroeconomics in the United States may be represented by the session “An appraisal of the non-market clearing paradigm” that took place at the 1979 meetings of the American Economic Association and was published in the *American Economic Review* (May 1979, papers and proceedings). The participants, who all announced the end of the disequilibrium approach, were Barro, Grossman and Howitt. While Barro and Grossman had been the main protagonists of disequilibrium macro in the US, Howitt was at the time in close contact with Leijonhufvud and, especially, Clower.

For Barro, disequilibrium macroeconomics was primarily about providing a microeconomic foundation for Keynesian economics. Until the mid 1970s he worked on disequilibrium macroeconomics, doing further work with Grossman on Keynesian economics and the theory of consumption (Barro and Grossman 1974²⁵ and 1977²⁶) and publishing a graduate textbook, *Money, Employment and Inflation* (1976), in which they extended their original model to encompass price dynamics and expectations. However, alongside this work, Barro began to work on other problems, using different types of model. His most influential paper from this period, ‘Are government bonds net wealth?’ (1974) owed nothing to the disequilibrium approach. In ‘Rational expectations and the role of monetary policy’ (1976), he built on the work of Friedman, Lucas, Sargent and Wallace (c.f. pp. 1-2). The appearance of this work alongside his continuing work on disequilibrium macroeconomics with Grossman suggests that he was simply using different models for different problems. This is consistent with the approach adopted in his

the only alternative to Walrasian equilibrium.

²⁴In correspondence with James Tobin of January 1 1992, Patinkin wrote that “I am still happy to claim paternity” for the disequilibrium approach. “You seem to think that it assumes price rigidity, and that is particularly true of the French literature on disequilibrium macroeconomics. But in accordance with my consistent emphasis on the fact that the analysis of the *General Theory* does not depend on price rigidity, that is not the way I developed this idea in chapters 13-14 of my book.”

²⁵Written in 1972-3.

²⁶Initially presented at a conference in 1974.

intermediate textbook, *Macroeconomics* 1987 which, although designed as an exposition of New Classical economics, included a chapter on the “Keynesian theory of business cycles fluctuations” (Barro 1987, chapter 18). In that chapter Barro argued that Keynesian macroeconomics, as far as it goes (and it does not go very far), must be interpreted in terms of the fixed-price approach put forward in his 1971 article with Grossman. The change in his approach, in terms of his published output, appears to come in 1976. In his survey of monetary theory, written jointly with Stanley Fischer (1976, p. 149), it was argued that the role of money generating disequilibrium could not be explained by models in which prices were exogenous. They observed that the outstanding problem in disequilibrium theory was the working out of a joint theory of price and quantity determination. By the time he wrote ‘Long term contracting, sticky prices, and monetary policy’ (Barro 1977) his target was the long-term contracting models that sought to explain why wages would be rigid, leading to disequilibrium in the labour market.

Wage/price stickiness per se, according to Barro (1977, p. 315), was not the fundamental feature of Keynesian models. The crucial element – which marks Keynesian economics as non-market clearing analysis – “is the nonexecution of some *perceived* mutually advantageous trades.” Keynesian economics does not explain why agents should be constrained to trade at a wage-price vector that does not equate quantities supplied and demanded, which is related to the “Patinkin problem” discussed above. Barro (idem) suggested that the distinction between the non-market clearing (e.g. Barro-Grossman 1971) and the market-clearing (e.g. Lucas 1972) models is that while the former focuses on changes in the amount of perceived mutually advantageous trades that are not executed, the latter investigates fluctuations in the size of the gap between perceived and actual mutually advantageous trades as influenced by imperfect information. Interestingly enough, he concluded from that that “the two approaches are not mutually exclusive”, which explains why the disequilibrium approach continued to play a role in Barro’s macroeconomics, even if a minor one.

Likewise, Grossman (1979) understood the non-market clearing and the incomplete information paradigms as alternative formulations of non-Walrasian fluctuations in aggregate employment. The inability of disequilibrium macro to provide a convincing rationale for persistent restriction on transactions (a problem already noticed in Barro-Grossman 1976, p. 6) prompted Grossman to move towards the assumption that business cycles are caused instead by the effect of incomplete information on the incorrect perception of potential gains from trade in market clearing models. Grossman found necessary to augment Lucas’ misperception model with a theory of implicit contracts for shifting risk in labor markets (see Grossman 1978). Implicit contract models are consistent with market clearing (since layoffs do not imply a failure to realize perceived gains of trade) and, moreover, are able to account for the predicted effect of aggregate demand on employment and the observed stickiness of real wages without imposing unrealistic restrictions on worker utility functions as in the original Lucas’ model (Grossman 1979, pp. 66-67). However, Grossman’s econometric testing of equilibrium macroeconomics would soon raise second thoughts about the empirical validity of market-clearing models of economic fluctuations (see Boschen and Grossman 1982). The inability of Lucas’ model to explain the data was, according to Grossman (1983, 1987) the main factor behind the “remarkable survival of non-market clearing assumptions”. The phrase “non-market clearing” was not restricted to disequilibrium macro à la Barro-Grossman (1971, 1976), but included any theory that assumed widespread failure of economic agents to realize perceived opportunities to gain from trade, such as the rational expectation model with money-wage stickiness advanced by Fischer (1977). Although such models – like the early disequilibrium models - violate the postulate of maximization, they were seen by Grossman (1983, p. 237) as potentially reconcilable with neoclassical analysis in that they assume that the objective of wage and price setting agents is to satisfy market-clearing conditions. In the 1990s Grossman changed his research agenda and moved away from macroeconomic theory.

Repairing to Marshall – Clower and Leijonhufvud

From 1971, Clower and Leijonhufvud, whose careers had previously been separate, were together at UCLA and their research programs converged.²⁷ They wrote a number of joint articles and planned in

²⁷They diverged later as Leijonhufvud, younger than Clower, moved into new fields, but that would take us beyond the current

1972 a textbook together - entitled "The coordination of economic activities" - though it was never written. Three closely related themes characterize their work during this period, all of which took them gradually away from the disequilibrium macroeconomics with which their names had become associated.

They began to focus more on money. Leijonhufvud suggests that by the time Barro and Grossman (1971) was published, they had already moved on. In a letter to Erroll Glustoff, he wrote that he and Clower had stopped talking about sales constraints, in favour of cash constraints.²⁸ This was in recognition of the fact that, in a monetary economy people hold stocks of cash and need not respond to sales constraints by immediately reducing their purchases. In the same letter, Leijonhufvud criticized Grossman for neglecting stocks. These ideas were incorporated into Leijonhufvud (1973), which may be regarded as his last contribution to the field of disequilibrium macroeconomics per se. The 1973 article introduced the notion that the economic system is stable within a certain range of normal circumstances, called "corridor". Within the corridor, a decline in aggregate demand is dampened by wage and price adjustment. This happens because the presence of buffer stocks of liquid assets, the inelasticity of permanent income expectations and the accumulation of inventories contribute to smooth out the potential deviation-amplifying multiplier process. Things are different, however, if the initial downfall of aggregate demand is large and persistent. Effective demand failures will become prominent, with sellers reacting to the low effective demands constrained by current incomes in a monetary economy instead of notional demands (see also Howitt 1990, p. 35). Grossman (1974) rejected the corridor concept and criticized Leijonhufvud for viewing quantity and price adjustments always as alternatives. In a long, undated letter to Grossman, Clower tried to persuade him not to submit the paper for publication, for it "may suggest to readers that there is a skirmish going on within the ranks of disequilibrium theorists which, in turn, may induce a feeling amongst people presently moving into this area that the whole field is so confused as to be hardly worth bothering about." At the same time, Clower had realised that his earlier (1967) characterization of a monetary economy did not make sense (in it, money was more constraining than barter, because it ruled out possible transactions) and was trying to develop a better theory of how transactions took place (cf. Clower 1984, p. 267). He wrote that some years after the counter-revolution paper, he introduced the dual-decision hypothesis into an otherwise conventional model of price dynamics, only to discover that it yielded implausible results (Clower 1984, 265-6).

More important than this, both Clower and Leijonhufvud came to realise that Keynes's theory was built on a Marshallian framework, not the Walrasian one in which they had been working in the 1960s. Thus although Leijonhufvud conceded that he had been in part responsible for the disequilibrium macroeconomics literature, constructing 'Keynesian' models on 'neo-Walrasian optimizing foundations ... did not seem a promising way to go and [he] took no part in this development' (Leijonhufvud 1998, p. 175). By 1974 he was arguing that Keynesian economics had to be understood in a Marshallian framework.²⁹ This involved rejecting some of his 1968 arguments, most notably that concerning Keynes's reversal of Marshall's ranking of price and quantity adjustment speeds. He blamed this error on his having been misled into adopting a Walrasian perspective (Leijonhufvud 1974, p. 169; cf. 1968, pp. 20-5; see also the discussion in Harcourt 1977, p. 91).

Following directly from this perspective, Clower (1984, p. 263) moved into what he chose to call general process analysis. This involved moving away from thinking of markets in terms of impersonal forces of supply and demand, and analysing them in terms of interactions between trading institutions. The concern with the organization of markets is already clear in Leijonhufvud 1973. It is central to Howitt's (1979) criticism of the early disequilibrium macro literature in the *AER* symposium, where he referred to his joint work with Clower (see Clower and Howitt 1978) on that. In Leijonhufvud's case, the change led him to computational economics, which he later pursued in both UCLA and Trento. While Leijonhufvud's research program led him to develop further the intertemporal coordination theme of the "Wicksellian Connection" (see Leijonhufvud 1981), Clower's (1988) search for a "Marshall Connection" led him to charge Keynes (1936, chapter 3) of "fraud" in the formulation of the theory of effective demand. According to Clower (1994), Keynes' Marshallian model cannot account for sales constraints

story.

²⁸Letter from Leijonhufvud to Glustoff, January 22, 1972,

²⁹Leijonhufvud (1974) and his unpublished Marshall Lectures in the same year. See Leijonhufvud (1998), p. 175.

because it assumes (perfectly competitive) “thick” markets, as opposed to (imperfectly competitive) “thin” markets (see also Marcuzzo 1999). Hence, Clower (1994) agrees with Barro (1994) that non-clearance of the output market remains as the *sine qua non* of Keynesian macroeconomics.

From disequilibrium macroeconomics towards New Keynesian Economics – Stiglitz

For Stiglitz, his paper with Solow was one element in a much broader research program centred on dynamics modeling and increasingly, from the late 1960s, centred on problems related to information and market imperfections. For example, in a series of papers around 1970, he worked out the implications of bankruptcy for the Modigliani-Miller theorem which in the late 1960s had become the standard framework for thinking about the value of the firm. In the mid 1970s, such ideas were applied to the labour market explaining wages and labor turnover. This led, later, into models of commodity, labor and financial markets, the common feature of which was that, in the presence of information problems, markets would not behave like the perfectly competitive markets of economic theory.

Stiglitz’s main contribution to disequilibrium macroeconomics after the Solow-Stiglitz 1968 paper was the joint edition with Costas Azariadis of a special issue of the *Quarterly Journal of Economics* on ‘Implicit Contracts and Fixed Price Equilibria’ (Azariadis and Stiglitz 1983), which featured a fixed-price model with rational expectations (Neary and Stiglitz 1983). While Grossman (1979) had seen implicit contracts as an alternative to the disequilibrium approach, Azariadis and Stiglitz (p. 1) hoped that the microfoundations of the fixed-price method would be strengthened by bringing together contributions from both fields. Stiglitz’s own contribution to the *QJE* special issue was not, however, related to rationing in the labour market, but to the modeling of quantity constraints in the commodity markets. Neary and Stiglitz (1983) introduce the notion that current quantity constraints are caused by the expectation, justified or not, of future quantity constraints, which generates “bootstrap” properties. They extend Solow-Stiglitz, Barro-Grossman and Malinvaud to a two-period temporary equilibrium model in order to show, *inter alia*, that under “rational constraint expectation” (that is, perfect foresight of future constraints) the effectiveness of government policy is enhanced instead of diminished.

While in Azariadis and Stiglitz (1983, p. 2) the notion of quantity-constrained equilibrium is still described as a “very interesting concept”, in the New Keynesian manifesto written a few years later by Greenwald and Stiglitz (1987, p. 121), the fixed-price assumption “fails to square with evidence” and “cries out for theoretical justification”. Though New Keynesian Economics could be seen, and has been seen, as a natural development from disequilibrium macroeconomics, its foundations are different and, whatever the precise relationship of the two approaches, it marks a distinct episode in macroeconomic theory.³⁰

5. Malinvaud and the integration of the disequilibrium literature

The event we have taken to mark the end of this episode is the publication of Edmond Malinvaud’s (b. 1923) *The Theory of Unemployment Reconsidered* (1977). This slim volume contained the text of three lectures. The first, ‘Elements for a theory’ provided a detailed account of the microeconomics of disequilibrium, addressing issues such as how goods might be rationed if sales constraints of the type that dominated disequilibrium macroeconomics were in operation, and of how constraints might be perceived by agents. In his second chapter, Malinvaud provided a model of the short run. He simplified the model drastically, linearizing functional forms wherever possible, to present an elegant model of macroeconomic equilibrium with rationing. One reason for the success of this model was probably an innovative diagram, in wage-price space, which showed which combinations of the exogenous variables (wage rate and the price level) would lead to classical unemployment (excess demand for goods and excess supply of labour), Keynesian unemployment (excess supply in both markets) and repressed inflation (excess demand in both markets). The Walrasian equilibrium was a single point, and three lines divided the space into these three regions. The diagram proved useful in explaining why economies were

³⁰The transition from disequilibrium macroeconomics to New Keynesian Economics is the subject of another paper, not yet completed.

more likely to exhibit Keynesian than classical unemployment (fluctuations in nominal wages and prices were significantly greater than in real wages) and it helped analyse a variety of policy regimes. Malinvaud's three regimes, and their reconciliation within the same model of the classical and Keynesian approaches, have been considered as one of the main analytical results of disequilibrium macroeconomics (Mankiw 1990; Grandmont 1989; Fitoussi 1983). The final lecture extended the model to issues to do with the medium run.

Before the mid 1970s, Malinvaud was well-known for his path-breaking contributions to capital accumulation and resource allocation, econometrics, and for a highly successful graduate microeconomics textbook (Malinvaud 1953, 1966, 1972). Nevertheless, as recalled in an interview given to Alan Krueger (2003, pp. 189-90), he used to teach macroeconomics at the Institut National de la Statistique et des Etudes Economiques (INSEE) in Paris since 1957, which originated his later contributions to disequilibrium analysis. In 1962 Malinvaud (1965) took part in the IEA conference where Clower's (1965) paper was first presented, but could not appreciate the implications of Clower's approach at the time (see Brechling's record of the debates, p. 305). As recalled by Malinvaud in the same interview, he was searching in the 1960s for an explanation of unemployment phenomena as a result not just of depressions in aggregate demand, but also of business profitability (Krueger 2003, p. 191).

When I saw the work that was done on fixed price general equilibrium by people like Barro, Benassy, Grandmont, Grossman, Laroque and Younes, I realized that it provided precisely what I was up to, namely a model to explain the respective roles of wage push shocks and aggregate demand shocks on changes in employment. This is what I tried to explain in my 1977 monograph.

Instead of referring to his framework as "disequilibrium macroeconomics" à la Barro-Grossman, Malinvaud (1977, chapter 1) described it as "fixed-price general-equilibrium analysis", with the dynamic adjustments explicitly out of the picture. This differs also from Clower-Leijonhufvud's notion that non-clearing markets states have self-reinforcing tendencies ("deviation-amplifying"). As pointed out by Alan Coddington (1983, p. 33) Clower (1965) had discussed the consequences of being in disequilibrium and the difficulties of getting out of that state, not how an economy gets into disequilibrium in the first place. This contrasts with Malinvaud (1977), where other arrangements (rationing of goods) fulfill the function that relative prices would otherwise carry out and take the pressure off relative prices (Coddington, *ibid*). Malinvaud (chapter 3) applied his fixed-price model to discuss the impact of changes in exogenous variables, including productivity, terms of trade and the price of oil. In particular, Malinvaud claimed that the model was able to account for the stagflation episode of the 1970s, as confirmed by its influence on Bruno and Sachs's (1985) *Economics of Worldwide Stagflation*.

After the 1977 book, Malinvaud continued for a few years to contribute to fixed-price general equilibrium analysis, especially in connection with the notion of "classical unemployment" and the modeling of the effects of supply shocks (Malinvaud 1980, 1982). It was at that time that Malinvaud published in French a macroeconomic textbook, revised and translated in three volumes 1998. Volume two included a chapter on "unemployment and price rigidity" (Malinvaud 1998, pp. 879-910), which reproduced the basic ideas of the 1977 book. Hence, although Malinvaud has continued to explain unemployment along the lines of his fixed-price model, he has not developed it beyond the ideas introduced in the mid 1970s. The difficulty of advancing the theory of disequilibrium unemployment was perceived as extending beyond the problems associated with the introduction of endogenous prices and imperfect competition. Although disequilibrium theory could not, in Malinvaud's interpretation, be called a failure, his reading suggests that research in the field had probably reached a dead end after the early 1980s.

6. Conclusions

During the 1970s, the disequilibrium macroeconomics literature was extended in many directions. Early on, Flemming (1973) showed that it could be used to explain otherwise puzzling features of consumption behaviour, and that it offered an alternative to the dominant permanent-income theory. There also developed a substantial literature on growth models in which markets did not clear, at least in the short run (see, for example, Rose 1973; Stein 1978). The perceived merit of these models was that they made it

possible to take account of money more effectively than did equilibrium growth models.

After Malinvaud's book, applications of disequilibrium macroeconomics continued apace. Muellbauer and Portes (1978) extended the model to take better account of inventories and inter-temporal choices, producing a new diagrammatic representation of possible equilibria. Dixit (1978) used an extension of Malinvaud's model to analyse the balance of trade. Malinvaud's model soon displaced that of Barro and Grossman as the most generally accepted starting point for disequilibrium macroeconomic analysis. Neary (1981) used Malinvaud's model to discuss technological unemployment. There was also, in parallel to this literature but not interacting with it significantly, a microeconomic literature on disequilibrium, in which to which the most prominent contributors were Drèze, Grandmont and Benassy.³¹

However, though this literature flourished for a while, it became much less popular during the 1980s, as indicated by the sharp fall in citations of Barro and Grossman, Malinvaud and other core references (see Figure 1). Given that disequilibrium macroeconomics was much stronger in Europe (perhaps because of European unemployment experience during the 1980s, perhaps for other reasons) this decline came even sooner in the United States. By the end of the 1980s disequilibrium macroeconomics had become virtually invisible within macroeconomics as a whole. The new classical macroeconomics was challenged not by disequilibrium macroeconomics but by the New Keynesian Economics. Why was this? It is possible to do little more than speculate on this, but we offer some possibilities.

The obvious reason, standard in the literature, is that the economic events of the 1970s rendered disequilibrium macroeconomics completely implausible. Not only was it unable to rival the Friedman-Phelps-Lucas analysis of inflation and unemployment, but the underlying assumption that prices and wages were sticky seemed implausible in a world where inflation was running at over 10% per annum and controlling prices was the major problem facing the authorities. On top of that, despite Malinvaud's effort, disequilibrium macroeconomics was seen by many as having failed to provide new insights into problems of aggregate supply, brought into prominence by the oil shocks and the productivity slowdown after 1973. It came to be seen as a specialized theory appropriate for situations such as existed in the centrally planned economies where prices were institutionally fixed, and imbalances of supply and demand were a chronic problem (cf. Grossman 1979).

However, the narrative presented here suggests that there may be more to the story than this. For a brief period, economists spoke of disequilibrium macroeconomics as though it were a coherent, unified research program. As we have shown, this was not the case, its architects following programs that were radically different from each other. After 1971, not only did they continue to move in different directions, but virtually all of them abandoned disequilibrium macroeconomics. Clower and Leijonhufvud followed paths that were so much at variance with generally accepted canons of good practice that, even when it surfaced in the mainstream journals, it could easily be ignored. Barro's renunciation of disequilibrium macroeconomics was particularly prominent because his work on government debt and monetary policy became very widely known. Furthermore, when he switched to the New Classical Macroeconomics, he did so because he believed it provided better microfoundations for macroeconomics than did disequilibrium macroeconomics. As for those who remained sympathetic, though they may have continued to teach it, they failed to make major contributions. The result was that, though others worked on the subject, none of those with whom disequilibrium macroeconomics was primarily associated developed it further.

This account points to another important aspects of disequilibrium macroeconomics. Throughout, there was a conflict between two interpretations. In one, disequilibrium macroeconomics was seen, very generally, as macroeconomics without the auctioneer (Leijonhufvud) or general process analysis (Clower). Markets simply did not adjust instantaneously but prices and outputs were determined by a range of unknown and probably complex rules, where market prices were determined by specific trading institutions and where problems of uncertainty and limited information were pervasive. However, though such a world could be grasped intuitively, modeling required something simpler. When Clower and Leijonhufvud focused on the fact that in such a world, prices would fail to adjust instantaneously to clear

³¹This is the subject of another paper, not yet completed.

markets, causing agents to face constraints additional to those of standard price theory, this vision of the world became translated into fix-price models, culminating in those of Barro, Grossman and, later, Malinvaud. Disequilibrium macroeconomics came to be seen, not as a general theory of market processes but as a generalization of the neoclassical synthesis's interpretation of Keynesian unemployment: instead of assuming rigid or sticky wages, all prices and wages were taken as given (see e.g. Drazen 1980, Gale 1983).

This illustrates the problem of vision running ahead of technique. Patinkin could not formalize what he considered the most important part of his book. Malinvaud could not advance the model beyond the 1977 fixed-price formulation. Clower realised that both his early price-adjustment models and his dual-decision hypothesis failed to do justice to his intuitions about the world.³² Leijonhufvud, perhaps more than anyone else, sought to offer an intuitive explanation of a world that he could not capture in formal theory. Economists responded to such visions in different ways. Solow and Stiglitz developed a model that abstracted from issues they could not handle but which enabled them to analyse the dynamics in which they were interested. However, a more common reaction was to focus on the formal models and to confine their theorizing to what could be said rigorously. Founding macroeconomics on rigorous utility-maximizing microfoundations was important. This was, no doubt, the reason why Patinkin's Chapter 13 was neglected. It was a major reason why it was Barro and Grossman's static, atemporal version of disequilibrium macroeconomics triumphed over the then dynamic version offered by Solow and Stiglitz. It was the reason why Barro and Grossman moved away from disequilibrium macroeconomics to the new classical macroeconomics. It was only when, in the late 1970s and in the 1980s, that Stiglitz, Mankiw, Taylor and others began to work out models that were as firmly grounded in individual optimizing models as those of the New Classical Macroeconomics – accepting the New Classical methodology – that the New Keynesian Economics became firmly established. Though New Keynesian Economics formalized many ideas with which the founders of disequilibrium macroeconomics had been concerned, for Clower and Leijonhufvud it represented a taming of their ideas just as Barro and Grossman had done a decade earlier, and they rejected it. Solow and Patinkin (and perhaps Stiglitz)³³ occupied an intermediate position, finding important insights in New Keynesian and other formal models of how markets might work, but retaining an willingness to think that disequilibrium macroeconomics might still be useful as a way of representing intuitive ideas about markets that could not be captured within formal models.

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³²The doubts he expressed about the economic case for his mathematical models can easily be interpreted this way.

³³Stiglitz (2003) suggests that he sees the implications of information for the way we should think about markets as far more radical than one might deduce from the New Keynesian literature.

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