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## **Axel Leijonhufvud and The Quest for Micro-foundations - Some Reflections**

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*Abstract:* Axel Leijonhufvud's *On Keynesian Economics and the Economics of Keynes* (1968) was a seminal contribution to the literature on what came to be known as the micro-foundations of macro-economics, but its Marshallian approach, which involved analysing the disequilibrium dynamics of markets in which trade at non-market clearing prices would occur, was not that eventually adopted in the early 1970s. Instead, a Walrasian development of monetarism, namely new-classical macroeconomics, which combined the postulate of continuously clearing markets with the rational expectations hypothesis became dominant. Even so, Leijonhufvud's subsequent work on the costs of inflation had an important influence in establishing this phenomenon's policy importance, and, along with his earlier analysis of employment fluctuations, provides still important insights about why the relevance of now-orthodox economics might be limited to helping us understand the economy's performance within a corridor whose boundaries lie close to its full-employment-price-stability equilibrium..

Key words: macroeconomics, micro-foundations, Keynesian economics, monetarism, new-classical economics, general equilibrium, disequilibrium, auctioneer, false prices, money, unemployment, inflation.

*JEL* Classifications: B22, B31, D50, D80, D90, E12, E31.

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I think that I first met Axel Leijonhufvud when he was still a graduate student at Northwestern, and, because I had the good fortune to be Bob Clower's junior colleague during my time at Essex, I was kept well aware of the development of his ideas even before *On Keynesian Economics and the Economics of Keynes* appeared. When it was published in 1968, Harry Johnson made sure that I was an early British reader, and I am proud to say that my copy of it bears an inscription from the author, commemorating a visit he made to Manchester in 1974. That, as is evident from Laidler (1974) was at a time when his influence had begun to loosen my up-till-then rather uncritical embrace of monetarism. In short, I have been learning from Axel's work for a long time, and I have sometimes followed it too, usually from a little to the right, but with undiminished admiration over the years.

I have given this essay the sub-title "some reflections" because it is probably as much informed by (no doubt prejudiced) hindsight and (no doubt inaccurate) memory as by a careful weighing of the published record. The history of macroeconomics in the second half of the twentieth century, and of Axel Leijonhufvud's place in it, largely remains to be written, but I hope that this essay will provoke some of those who were not yet professionally active in those years, and can therefore view them dispassionately, to begin that task. When they do so, I also hope that their work will bear out at least some of the judgements offered here.

#### *Economic theory in the 1950s*

Half a century ago, those of us starting out on the serious study of economics found a great deal to perplex us. The subject, particularly as it was taught at the intermediate level and above, was theory-based, but that theory was divided into two components, the connections between which were, to say the least, obscure. *Microeconomics* dealt with the maximising behaviour of individual households and firms, how the decisions of these individuals were co-ordinated by the price mechanism, and how this mechanism might fail properly to allocate the economy's endowment of productive resources without a few well-placed government subsidies and taxes designed to change the structure of relative prices. *Macroeconomics*, on the other hand, as enshrined in the Hicks-Hansen *IS-LM* model, dealt with aggregate consequences of the behaviour of those same firms and households, arguing that these would often include a failure of that same endowment of productive resources to be fully employed without help from a steady injection of expenditure from the government that was so co-ordinated with the flow of taxes paid by the private sector as to provide an appropriate level of aggregate demand.

Microeconomics and Macroeconomics thus seemed to be telling possibly contradictory stories about how the economy as a whole worked. To be sure, efforts were made to forge a link between them: for example, Paul Samuelson's *neo-classical synthesis*, notably as expounded in the third edition of his introductory textbook (Samuelson 1955) had it that, though the market economy needed some government help designed with the aid of macroeconomics to bring about full employment, once this was achieved its further allocative functioning could safely be left to those devices which were the subject of microeconomics; and Abba Lerner's *Economics of Control* (1944) had earlier found a role for micro theory in guiding the pricing behaviour of publicly-owned enterprises in a thoroughly socialised economy whose government made maintaining full-employment a priority. But both of these attempted links had to do with the

policy applications of received economics; they skirted questions about the logical relations between the theoretical foundations of its two branches and about whether, and if so how, these could be reconciled.

By the late 1950s, IS-LM macroeconomics was beginning to take on the status of an unchallenged orthodoxy, under the label *Keynesian Economics*, and had begun to find its policy feet too.<sup>1</sup> Soon, though belatedly, it would dominate policy making even in the United States. In 1965, at the height of its influence, but in perhaps the worst call made by an eminent economist since John Stuart Mill's 1848 claims about the completeness of the theory of value (see Mill 1848, ed. Ashley, 1909, p. 436) Robert Solow (1965) would proclaim that “. . .most economists feel that short-run macroeconomic theory is pretty well in hand . . .All that is left is the trivial job of filling in the empty boxes, and that will not take more than 50 years of concentrated effort at a maximum . . .”

If we remember this (partly tongue in cheek) claim of Solow's nowadays, that is probably because only 3 years after it was made, Axel Leijonhufvud would quote it in his book *On Keynesian Economics and the Economics of Keynes* (Leijonhufvud 1968, p. 4). This work did more than any other single contribution to energise the search for a cure for the discomfort that many economists were feeling in constructing proper *micro-foundations* for macroeconomics, an endeavour that would end up pushing the IS-LM model from the centre of macroeconomics and replacing it with a new approach whose microeconomic basis was thoroughly transparent. But though what Harry Johnson (1971, repr. 1978, p.198) would refer to as Leijonhufvud's “monumental re-interpretation of [Keynes'] thought” was seminal in giving impetus to these developments, the micro-foundations that were eventually established were the very opposite of the ones he had proposed.

In what follows, I shall reflect upon how and why this came about. Specifically, I shall first describe the micro-foundations problem as it appeared about fifty years ago, and how it was then being addressed. Then I shall argue: that, even as Leijonhufvud was writing his book, the macro-orthodoxy that so disturbed him was already being undermined by *monetarism*, whose attack was, however, based more on empirical evidence than micro-theoretic considerations; that a by-product of monetarism's success was nevertheless to shift the theoretical concerns of macro-economists away from just those parts of Keynes's legacy upon which Leijonhufvud sought to build; and that, as a consequence, the search for micro-foundations that he helped set in motion was quickly diverted from his chosen path.

### *Marshallians and Walrasians*

The received economic theory whose overall structure seemed so puzzling half a century ago was the product of two intellectual upheavals in the 1930s. Both of these had happened in

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<sup>1</sup>The publication of Alvin Hansen's *A Guide to Keynes* in 1954 was surely a critical step here. The book both symbolised and cemented the dominance of the IS-LM interpretation of Keynes in the standard undergraduate curriculum.

Britain, but because of the destruction by the Nazis (along with much else) of important and distinct intellectual traditions on the continent of Europe, not to mention of the accident that English was also the language of the United States, whose universities would soon come to dominate all of economics (again along with much else), they profoundly influenced the development of the subject as a whole. The first of these was the macroeconomic revolution that surrounded the publication and interpretation of Keynes's (1936) *General Theory of Employment, Interest and Money*, and the second, less noted but just as influential, was an upheaval in microeconomics that saw continental *general equilibrium* theory, whose principal English language text was John Hicks' *Value and Capital*, largely displace Marshallian *partial equilibrium* analysis.<sup>2</sup>

These developments were incompatible with each other. General equilibrium analysis stemmed from the work of Walras and the first generation of Austrians, notably Carl Menger, but the short-run macroeconomic theory that sprang from it - Austrian business cycle theory - had failed to catch on in the 1930s, partly as a result of Keynes's success.<sup>3</sup> Partial equilibrium analysis, on the other hand, was one component of a broader Marshallian approach to economic theory that had also provided microeconomic foundations for the macroeconomics of *General Theory*, as Leijonhufvud (2006) has recently documented. Thus, the root cause of economic theory's troubles in the 1950s was that, in the 1930s, competing continental and Marshallian traditions had won one battle each, the former on the micro front and the latter on the macro, and that a third battle remained to be fought, over the micro-foundation of macroeconomics.

The most thorough exposition of the tension between Marshallian and Walrasian approaches to economics written at that time was Milton Friedman's (1953) "The methodology of positive economics". This essay argued that the main point of contrast between the two lay in the Marshallian use of economic theory as "an engine of analysis" that permitted empirically testable hypotheses about real world economic phenomena to be formulated, and the Walrasian quest for an analytic framework general enough to encompass essentially any possibility. Obviously, on this criterion, the economics of Keynes's *General Theory*, with its strong hypotheses about the stability of the consumption function, the volatility of the marginal efficiency of capital, the sensitivity of the demand for money to the rate of interest, and so on, is as thoroughly Marshallian as the general equilibrium theory of *Value and Capital* is Walrasian, and it is hardly surprising that the bodies of literature that followed on from them would prove

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<sup>2</sup>The story of general equilibrium theory's arrival in English language economics is complicated. Walras' *Elements* itself did not appear in translation until 1954 when William Jaffe's edition appeared. (Not co-incidentally, Jaffe held an appointment at Northwestern, when Leijonhufvud was a graduate student there) The main source of information about this body of theory available in English in the 1930s was the 1923 translation of Gustav Cassel's *Theory of Social Economy*, though Hicks seems to have read Pareto in the original Italian.

<sup>3</sup>This history too is beyond this paper's scope. I have discussed it at length in Laidler (1999)

hard to square with one another.

Even so, by the 1950s, the phrase “Keynesian economics” had come to refer not so much to a system built around Keynes’s own specific empirical hypotheses, but around the IS-LM model, a formal framework which could accommodate those hypotheses to be sure, and generate results that bore a reasonable resemblance to what Keynes had claimed them to imply as well. But the IS-LM framework was a general equilibrium model of sorts that could also accommodate other hypotheses which yielded very different predictions.<sup>4</sup> Though IS-LM was certainly not a model in the tradition of Walras in any strict sense, some of its exponents were beginning to deploy it in ways that any follower of Friedman would characterise as Walrasian, and it was hardly surprising that economic theorists working along such lines would begin to explore its logical relationship to traditional general equilibrium theory. That is how the search for the micro-foundations of macroeconomics, to which Leijonhufvud contributed so much, seems to have begun, and two names stand out among those who preceded him, Don Patinkin (see eg. 1956, 2<sup>nd</sup>. ed. 1965) and Robert Clower (see eg.1965).

#### *The Patinkin-Clower contribution*

The typical general equilibrium model of fifty years ago dealt with an economy with a given endowment of productive resources, inhabited by utility-maximising households and perfectly competitive profit maximising firms, and its analysis showed (among other things) that the resources in question would be fully utilised if a set of relative prices ruled in the system that rendered the decisions of each agent compatible with those of all others, this even if the information available to each agent concerned only those prices, (as well as its endowments of resources, its own tastes - if household - or the technology available to it - if a firm).

The typical IS-LM model, on the other hand, was largely devoid of explicit maximising foundations, dealt with a world in which one input, labour (or two if account was taken of an exogenously given capital stock) produced a single good. In that model the nominal wage level was constant, and agents also faced a portfolio decision which was usually reduced to one about holding a stock of nominal money (whose supply was exogenously fixed). Such a model could, and typically did, generate a solution in which some labour remained unemployed. Two salient characteristics in particular differentiated these systems, each of which, be it noted deals with the behaviour of the economy as a whole: the absence of money from the first of them, and the capacity of the second to generate unemployment.

Patinkin’s main contribution to their reconciliation was to introduce nominal money into the general equilibrium system by including *real* money balances in agents’ utility functions, and

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<sup>4</sup>And the fact that Hicks (1937) had a major role in its creation and popularization makes it tempting to speculate that there was a micro-general-equilibrium influence at work there from the beginning. Even so, the immediate inspiration for Hick’s creation of the famous diagram seems to have been his interactions with Roy Harrod and James Meade about the interpretation of the *General Theory*. On this See Warren Young (1987)

allowing a “real balance effect” driven by a modicum of price flexibility to ensure that the model generated a stable equilibrium price level. But he also showed that the logical properties of his model implied that, if unemployment was to occur, the labour market in his system must have settled at a point of market disequilibrium, *off and inside* its demand curve for labour.

Clower, on the other hand, emphasised the contrast between the behaviour relations implied by a standard Walrasian general equilibrium model, where quantities responded to prices, and a key relationship of the standard IS-LM model, in which one quantity, consumption, varied with another, income. He then argued that the latter only made sense if agents were trading at *false* prices, prices other than those compatible with general equilibrium. Specifically, he argued that, if households were unable to sell all the labour they intended at the going real wage, they would simultaneously be unable to fulfill their consumption plans, and that their actual consumption would then be constrained to vary with income. The general equilibrium model enabled *notional* demand and supply curves to be generated, but the plans implicit in them could only be accomplished if market clearing prices ruled. If they did not, then actual behaviour would be driven by *effective* demand and supply curves in which quantities figured as arguments.

Patinkin’s analysis of the labour market, and Clower’s of the goods market were complementary to one another, and implied that micro-economic foundations for IS-LM macroeconomics were to be found in the hypothesis that trading could indeed take place at non-market clearing prices, and, second, in its implication that an initial shock to the system would then set in motion quantity dynamics, an *income constrained process*, of which the Keynesian *multiplier* was the prototype, in which deviations from full employment equilibrium were amplified rather than damped.

Such interactions were, of course, amenable to explicit modelling based on maximising premises, and one product of the Patinkin-Clower enterprise was an extensive formal literature whose highlights include Barro and Grossman (1976) and Malinvaud (1977), but whose details need not concern us in this paper. Suffice it to say that the easiest way to build models in which trading takes place at false prices is to hold prices constant, and that more and more elaborate systems built upon this assumption rapidly ran into diminishing returns. The literature in question rigorously established the existence of the linkages between general equilibrium analysis and 1960s style macro-theory that the insights of Patinkin and Clower had postulated, and generalized them as well. To this extent it was important, but its significance was to help bridge an existing gap between two already well established research agendas, rather than to create a foundation for any new work.

#### *Leijonhufvud, Keynes and Marshallian microeconomics*

Leijonhufvud’s work should be seen as a search for an alternative and potentially more fruitful way forward from the Patinkin-Clower insights. He assiduously avoided the trap of reducing trading at false prices to trading at fixed prices, so his work had an immediate claim to relevance when it came to analysing the interaction of money prices and quantities over time, a problem that was attracting increasing attention as the great inflation that began the mid-1960s gathered

momentum; and crucially, his way of establishing microeconomic foundations for a macroeconomics descended from Keynes's very Marshallian *General Theory* was self-consciously to seek them, not in contemporary Walrasian microeconomics, but in the equally Marshallian microeconomics that Keynes had worked with, and from which IS-LM analysis had become detached.

This Marshallian microeconomics, though already overshadowed by its Walrasian challenger, had not quite disappeared fifty years ago. Indeed it figured prominently in Friedman's (1953) essay on "Positive Economics" already referred to above, where the main example cited of the advantages of the pragmatic Marshallian approach to economic theory was the theory of perfect competition, whose empirical content Friedman favourably contrasted with monopolistic competition, for him the epitome of Walrasian vacuousness. Nowadays, it seems odd to characterise perfect competition as Marshallian, because we are used to defining it as a state of affairs in which all agents are price takers, who respond to market clearing prices set by an entity known as the *Walrasian auctioneer*. But Friedman's view made excellent sense at a time when perfect competition's defining characteristic was still regarded as being the absence of any interdependencies among individual firms' roles in the price formation process that would rule out the use of supply and demand analysis at the level of the industry, and when the every-agent-a-price-taker assumption remained to be examined.<sup>5</sup>

Friedman was, that is to say, writing before Kenneth Arrow's (1959) observation that, if every agent was a price taker, then no-one was left to set and change prices, and therefore before the above-mentioned fictitious auctioneer became a central player in microeconomics, whose specific task was to resolve this paradox.<sup>6</sup> Leijonhufvud, on the other hand, was writing in the immediate wake of these developments, and was fully conscious that they seemed to render the Walrasian theory of competitive markets totally unhelpful for analysing real world price adjustment processes. But he was also aware that the older Marshallian conception of competition that had underlain Keynes's macroeconomics left space for prices to be adjusted without the help of an auctioneer; and he saw that modern theories of market search, such as were being developed, among others, by his colleague Armen Alchian were perhaps able to fill this space and in a way that would allow the Patinkin-Clower insights about the consequences of trading at false prices to be placed on a firmer theoretical footing.

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<sup>5</sup>The contrast between Marshall and Walras's approach to economics was much discussed in the 1950s and early 1960s, and I am far from sure that everyone who drew a line between the two did so in the same place. It would be interesting to investigate this matter further. It is also worth noting that partial equilibrium microeconomics retained a strong position in introductory textbooks long after intermediate and advanced micro-theory had been taken over by the general equilibrium approach.

<sup>6</sup>This entity seems to have got this name some time in the late 1960s, perhaps from Leijonhufvud himself. This author recalls Hirofumi Uzawa referring to the "market secretary" at about this time.



Leijonhufvud summarised the point in an *AER* article published shortly before his book, (explicitly citing Arrow 1959 and Alchian and Allen 1964)<sup>7</sup>

Walras' auctioneer is assumed to inform all traders of the prices at which all markets are going to clear. This always trustworthy information is supplied at zero cost. Traders never have to wrestle with situations in which demands and supplies do not mesh; all can plan on facing perfectly elastic demand and supply schedules without fear of ever having their trading plans disappointed. All goods are perfectly 'liquid,' their full market values being at any time instantaneously realizable. Money can be added to such models only by artifice.

Alchian has shown that the emergence of unemployed resources is a predictable consequence of a decline in demand when traders do not have perfect information on what the new market clearing price would be. The price obtainable for the services of a resource which has become "unemployed" will depend upon the costs expended in searching for the highest bidder. In this sense the resource is "illiquid". . . . Reservation price will be adjusted gradually as search continues. Meanwhile the resource remains unemployed. To this analysis one need only add that the loss of receipts from its services will constrain the owner's effective demand for other products - a feedback which provides a rationale for the multiplier-analysis of a system of atomistic ("competitive") markets. (1968b, as repr.1981, p. 6)

The account of the problems associated with finding new equilibrium prices given in the first part of this quotation is more elaborate than those that Keynes frequently offered his readers, but it does not differ in substance from them. Leijonhufvud's claims that the *Economics of Keynes* was informed by a microeconomic analysis of decentralised markets that did not rely on the auctioneer were thus surely correct, though it is less clear that Keynes was sufficiently aware of the alternative to have self-consciously rejected it.<sup>8</sup> The following passage, taken from the *Treatise on Money*, is typical of several discussions there and in the *General Theory*, of the difficulties faced by agents in such markets when prices must change to keep them cleared.

Under a socialist system the money rate of efficiency earnings of the factors of production might suddenly be altered by *fiat*. Theoretically, I suppose it might change under a

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<sup>7</sup>Alchian and Allen were, like Leijonhufvud, members of the UCLA economics department. They were probably unwise to publish important and original analysis for the first time in an introductory textbook, if they wanted to maximise its exposure among their professional colleagues..

<sup>8</sup>Among his predecessors, however, both Edgeworth and Walras had been very aware of the need to separate the process of price formation from that of exchange in general equilibrium systems which give rise to the need for this entity. On this matter, see Leijonhufvud (1968a, pp. 68 et seq.)

system of competitive individualism by an act of collective foresight on the part of entrepreneurs in anticipation of impending monetary changes, or by a *coup de main* on the part of trade unions. . . In existing circumstances, however, the most usual and important occasion of change will be the action of entrepreneurs . . . in increasing or decreasing the volume of employment which they offer at the existing rates of remuneration . . . and so bring about a raising or a lowering of these rates. (1930, I, p. 141, italics in original)”

### *Inter-temporal co-ordination*

As Leijonhufvud was at pains to argue, what mattered for setting in motion cumulative fluctuations in expenditure and employment was not that prices should be rigid, but only that should move sufficiently slowly to permit trading at non-market-clearing prices to get under way. Indeed, as the passage quoted earlier makes clear, it was an essential characteristic of his analysis that the quantitative consequences of trading at false prices would arise from the very same dynamic processes that would drive variations in those prices. It was partly on this basis that Leijonhufvud argued that fixed price IS-LM exercises not only seriously misrepresented the economics of Keynes but, more generally, were inadequate for analysing the behaviour of any market economy; but only partly. He also strongly criticised the appropriateness of the IS-LM model’s treatment of output as consisting of a single good. The distinction between consumption and investment goods was, he suggested, crucial.<sup>9</sup>

Not only did a chronic inability of the price of capital goods to find and maintain its right level relative to that of current consumption lie at the heart of Keynes’s explanation of the market economy’s inability to maintain full employment, but that explanation was also basically correct. Any shock which required that this relative price should fall to re-equilibrate the system would initially create a shortfall of the nominal demand price of capital goods from their supply price, and set in motion a cumulative contraction of output. However, the required relative price adjustment could not necessarily be accomplished by a fall in money wages (even if these were capable of rapid adjustment) because this would also cause the money price of consumption goods to fall. What was needed was a fall in the rate of interest that would cause the current demand price of capital goods to rise. But, argued Leijonhufvud, “Once the income-constrained process had been allowed to gather momentum . . . expectations would no longer be such as to sustain full employment even in conjunction with a ‘metastatically right’ interest rate” (1968a, p. 340); and so, in his view,

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<sup>9</sup>Leijonhufvud’s concern with inter-temporal allocation issues perhaps reflects his Swedish training, for it was Knut Wickell’s (1898) who had, not altogether intentionally, set in motion the shift of monetary economics’ focus away from the influence of the quantity of money on the price level towards that of the rate of interest on saving and investment. He would later write a seminal study of the influence of these ideas on early macroeconomics, namely “The Wicksell Connection”, (1981b). The possible Swedish origins of Leijonhufvud’s 1960s insights into Keynes’s role in developing the analysis of this problem is yet another important topic which lies beyond the scope of this paper.

Keynes' diagnosis of the conditions leading to a downturn in activity focussed on the relation between the money prices of non-money assets [i.e., investment goods] and the money wage rate. If this relation was out of line, . . . he put the 'blame' on too low asset values as a rule, not on too high wages. The conclusion is that deflation will help *only if it changes this relative price in the appropriate direction, i.e., only if it cures the malady that underlies the emergence of excess supply of commodities in the first place* (1968a, pp. 341-342, italics in original)

And to repeat, in Leijonhufvud's interpretation of Keynes, that malady lay in a misaligned relative price of investment and consumption goods, with a concomitant failure of market mechanisms to co-ordinate the allocation of resources over time; and cumulative output fluctuations, driven by income constrained dynamics, were the market economy's response to this failure. Obviously, a single good IS-LM model could not be used even to formulate this idea, let alone evaluate it.<sup>10</sup>

Now *Keynesian Economics and the Economics of Keynes* presented two challenges for its readers. First, as a work in the history of economic thought, it repudiated IS-LM analysis as an interpretation of Keynes's *General Theory*, and proposed an alternative version of that book's central message; and secondly, as a contribution to economic theory, it proposed the abandonment of this same IS-LM model in favour of an approach, which, being based on the analysis of trading at non-market-clearing prices, reduced then standard microeconomics to a special and not very interesting case of an altogether broader framework. In short Leijonhufvud argued that macroeconomics had gone off on the wrong track because Keynes's interpreters had failed to understand him, that the perplexing gap between the macro and micro components of then contemporary economic theory referred to earlier this paper had been a direct result of this, and that the gap in question could not be bridged without fundamental revisions to both micro and macro theory as they then stood.

A full treatment of this extraordinarily ambitious book's significance for the development of economics would have to assess both the validity of its claims about the discipline's past, and the success of its proposals for the subject's future, and there is not space here to do both. The balance of this paper will therefore deal only with the latter topic, and only certain aspects of it into the bargain.

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<sup>10</sup>The reader will note that Leijonhufvud's interpretation of the essentially dynamic nature Keynes's central message rests heavily on material that appears in Chapter 19 of the *General Theory* and plays little role elsewhere in the book. As he himself noted, however, though that message was about dynamics, "Keynes' model was static" (to which this author would add, and was not badly summarised in the IS-LM framework either, which is why so many of the *General Theory's* early readers found versions of it there. On this, see Laidler 1999, ch. 12)

### *Monetarism*

Co-incidentally, the word *monetarism* was introduced into the mainstream vocabulary of economics by Karl Brunner in (1968), the same year in which *On Keynesian Economics and the Economics of Keynes* was published, and ultimately it would be developments springing from this doctrine that would prevent Leijonhufvud's ideas having their intended impact on the future course of economic theory. Monetarism was not new in 1968, of course.<sup>11</sup> On the contrary, the appearance in March of that same year of Friedman's AEA presidential address on "The Role of Monetary Policy" (of which more below) put in place the capstone of an intellectual edifice that had been under construction at least since the publication *Studies in the Quantity Theory of Money* in 1956.

As Leijonhufvud himself would later note "By the mid-sixties, . . . macroeconomics was drawing most of its excitement from the challenge posed by . . . the 'monetarist' or 'new-quantity' theory of Friedman, Schwartz, Cagan, Brunner and Meltzer" (1976, repr. 1981, p. 316). Monetarism, however, was also an alternative and parallel expression of dissatisfaction with orthodox LS-LM macroeconomics to that represented by his own work; but where Leijonhufvud's research agenda centred on matters of economic theory, monetarism was more concerned with practical policy and the empirical evidence upon which it might be based. Initially too, these competing approaches emphasised different economic phenomena, income and employment fluctuations and inflation respectively, a factor which Harry Johnson would still argue as late as 1971 made monetarism inherently less interesting to mainstream economics.

But there was more to monetarism than the revival and refinement of the quantity theory of money as an explanation of inflation. Thomas Mayer's organised his still definitive (1975, repr. 1978) survey of the doctrine around twelve defining characteristics, three of which are particularly noteworthy in the current context, namely ". . . Belief in the inherent stability of the private sector. . . . Irrelevance of allocative detail for the explanation of short-run changes in money income, [and] . . . Focus on the price level as a whole rather than on individual prices" (p. 2). In his contribution to the symposium that Mayer's paper inspired, Benjamin Friedman (1978, p. 96, fn.3) noted in passing that the monetarist debate had not intersected with Clower and Leijonhufvud's work, and this is surely not surprising. Their emphasis on the importance of allocative detail and relative prices for understanding macroeconomic fluctuations, not to mention their insistence on the private sector's vulnerability to income constrained dynamics that tended to amplify shocks, set their work far apart from that doctrine. Nor did it have any point of contact with the characteristic of monetarism that Mayer put at the very top of his list, namely the deployment of "The quantity theory of money in the sense of the predominance of the impact of monetary factors on nominal income"

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<sup>11</sup>Karl Brunner too was a member of the UCLA department in the 1960s, and it is therefore probably no accident that his version of monetarism paid more attention to the information problems that lay at the heart of monetary economics than did Friedman (see, for example, Brunner and Meltzer 1971). The history of the UCLA department's contributions to monetary economics during this period would make a fascinating study.

As we have seen, Leijonhufvud was concerned with the logic of economic theory as it was then expounded, and as it appeared in Keynes's work; but at a time when "positive economics" was popular and "monetarism" seemed to be based on its precepts, many among his readers would be bound to judge his work not so much on the basis of its logical coherence and scholarly accuracy, as on its empirical relevance. His version of the *Economics of Keynes* was, however, firmly based on the presumption that the experience of the inter-war years in general, and of the United States in the 1930s in particular, had demonstrated that market economies were inherently unstable, and that it was the task of economic theory to discover just where their flaws lay. Hence, monetarism's assertion of the inherent stability of the private sector challenged not only IS-LM orthodoxy, but Leijonhufvud's work too, and it was supported by (among other evidence) a specific and detailed re-interpretation of the causes of the Great Depression in the United States.

The full impact on economics of chapters 7 - 9 of Friedman and Schwartz's (1963) *Monetary History of the United States*, which dealt with the 1930s, was slow to be felt.<sup>12</sup> Their immediate message about economic history was that the downturn with which the Depression had begun in 1929 had very likely been provoked by monetary tightening, and that the economy's subsequent catastrophic contraction had been caused, not as orthodoxy had it, by some exogenous collapse in the marginal efficiency of capital that monetary policy had been powerless to offset, but by colossal ineptitude on the part of the Federal Reserve; and. Leijonhufvud did in fact refer to the *Monetary History's* diagnosis of the role of monetary contraction in bringing on the initial downturn in late 1929.

"Keynes [as author of the *Treatise on Money*] would have concurred with Friedman and Schwartz in all essentials of their critique of Federal Reserve policy in this period [the late 1920s] and in attributing the onset of the Great Depression to the period of tight money preceding the actual downturn in activity, although he would, as usual, have conducted the analysis in terms of interest rates and 'credit conditions' rather than the stock of money" (1968a, p. 286)

but did not refer to what Friedman and Schwartz had to say about the Great Contraction itself in that book, nor to the broader implications of their re-interpretation of economic history for

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<sup>12</sup>As Susan Howson has impressed upon me. Note also that we are now more conscious than were readers of the 1960s of the work of some of Friedman and Schwartz's predecessors, and it is hard now to appreciate just how radical it seemed at the time, and strong was its impact. Lauchlin Currie's work, for example, had largely been forgotten, though he had published an article entitled "The failure of monetary policy to prevent the depression of 1929-1932" in the *JPE* in 1934, surely a title that tells its own story. On the reaction of American economists to the Great Depression while it was under way, See Laidler (1999, ch.9).

macroeconomic theory.<sup>13</sup>

Those implications were nevertheless of profound significance, for if the cause of the Great Contraction had been an avoidable monetary disturbance, did not that perhaps suggest that market economies which were not subject to such policy disturbances were well capable of coping with the allocation of resources over time and therefore inherently stable after all? If this was indeed the case, then the conventional interpretation of economic history that had motivated Leijonhufvud's work (and much else) was misguided, and though interesting as doctrinal history and economic theory, was it not also empirically irrelevant?

It was not until the early 1970s that these deeper implications of Friedman and Schwartz's work began to sink in among economists in general, and Leijonhufvud addressed them indirectly in his 1973 (repr. 1981) paper on "Effective Demand Failures". There he faced up to a weakness of his earlier work, namely that it seemed to make economic instability all too inevitable, and now declared that ". . .the central issue of macroeconomics is - once again - the extent to which the economy, or at least its market sectors, may properly be regarded as a self-regulating system? How well, or badly, do its 'automatic' mechanisms perform?" as a prelude to exploring the properties of the *corridor of stability* within which various mechanisms that he had earlier ignored or downplayed might be at work. These included the capacity of inventories, not least inventories of money and financial assets, to interfere with the mechanics of income constrained processes so as dampen deviations from full employment caused by various shocks.

#### *Newclassical microfoundations and Occam's razor*

This paper provoked little direct response.<sup>14</sup> By the mid-1970s, a new approach was beginning to take hold of the micro-foundations research agenda..Where Leijonhufvud's *Economics of Keynes* had investigated the non-Walrasian microeconomics of an economy that was presumed to be unstable, and had perhaps explained more instability than the world in fact displayed, *Newclassical* economics went to the opposite extreme. Building upon monetarism - indeed James Tobin (1981) would label it *Monetarism Mark 2* - it investigated the macroeconomic properties of a system in which Walrasian micro-mechanisms were presumed always to work, and which could only be disturbed by arbitrary shocks administered by erratic monetary policy.

The rise to popularity of this approach has a number of explanations. First of all it had an element of empirical plausibility. Not only had Friedman and Schwartz re-interpreted the Great Depression as a consequence of monetary policy, but by the early 1970s, memories of it were

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<sup>13</sup>Though he did refer briefly to these matters in later lectures given at the Institute for Economic Affairs in London, England in 1969 (see 1969, repr. 1981, p.42).

<sup>14</sup> Peter Howitt's (1978) paper was a notable exception. I am, however, relieved to be able to report that, along with *Information and Co-ordination* where it was reprinted, it is cited in some of my own subsequent discussions of money's "buffer-stock" role (see Laidler 1984 and 1987)

fading under the influence of a quarter century of rather stable expansion at more or less full employment; and closely related, inflation, in Harry Johnson's (1971) judgement, the policy problem to whose analysis monetarism was in any event best adapted was becoming a serious issue.

But these empirical issues were of secondary importance when compared to the influence of theoretical developments, particularly the discovery of the so-called *expectations augmented Phillips curve*. This was not an exclusively monetarist creation, for Edmund Phelps (1967) was its co-creator, but Friedman (1968) used it to help establish two quintessential monetarist propositions: namely, that the permanent inflation-unemployment trade off with which exponents of IS-LM *Keynesian Economics* were by that time routinely supplementing their analysis, was at best a short-term phenomenon, and that monetary policy's only long term effects were on the inflation rate. In other hands, moreover, though curiously not in Friedman's own, the expectations augmented curve began to fill the role of the "missing equation" that monetarist analysis had long needed to allocate the quantity-theoretic effects of money growth on money income between its real-income and price level components.

The rich literature that in the late 1960s examined potential microeconomic foundations for this relationships still awaits careful attention from historians of economic thought, but it should at least be noted here that Leijonhufvud's deployment of Armen Alchian's search theoretic analysis of non-Walrasian market processes was one candidate, but not the one that ultimately won out.<sup>15</sup> Instead Robert. E. Lucas's (1972) thoroughly Walrasian aggregate supply curve interpretation of the curve, coupled with his application to it of John Muth's *rational expectations* concept, found broad acceptance; and this was quickly cemented not just by his own subsequent work (1976) on its application to econometric policy modelling, but also by that of Thomas J. Sargent's (1973) on its implications for Friedman's natural unemployment rate concept, and of Sargent and Neil Wallace (eg. 1975) demonstration that it permitted monetarist scepticism about systematic monetary policy's ability to affect anything other than the inflation rate to be put on firm micro-theoretic foundations.

Citing subsequent papers by Lucas, Harry Johnson (1976, repr. 1978)) explained why Leijonhufvud's approach failed to catch on in the following terms ". . . It is virtually impossible to find a simple and comprehensive mathematical device for converting a general equilibrium system of mathematically formulated relationships into a fruitful technique for the study of persistent 'disequilibrium' and 'market failure'." (p. 244), and he elaborated the point in a footnote "The essential problem is that it is virtually impossible to invent a plausible mechanism that leaves the economy in disequilibrium with unexploited possibilities for profits or increased labour incomes, and at the same time specifies exactly how the economy will respond to a

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<sup>15</sup>Such a study should begin with the famous "Phelps volume" ( Phelps et al. 1970). Phelps (1974) characterised the main purpose of Leijonhufvud's book as being to establish a connection to the *General Theory* for the literature in question, surely too narrow a characterisation of its significance.

*change* in profit or labour income opportunities” (p. 244, fn) Johnson’s point was a sobering one, for it amounted to saying that to give up Walrasian foundations in order to study macroeconomic phenomena seemed also to require their abandonment when allocative issues were to be discussed, if the analytic consistency of economic theory was to be preserved, and that there was no workable alternative available to permit this shift.

The abandonment of Walrasian general equilibrium theory as a basis for the study of the economics of allocation was too large a sacrifice to contemplate, and perhaps the discipline might have chosen to live a little longer with what was by then an all too obvious inconsistency between its macro and micro branches, had not Lucas’s work seemed to render this unnecessary. Here it was not so much its theoretically compelling treatment of information processing as an exercise in maximising behaviour that mattered as its extremely attractive capacity to reconcile the co-existence of fluctuations in quantities as well as prices with continuously clearing Walrasian markets.

In the conventional *Keynesian economics* that Leijonhufvud had attacked, quantities varied in response to demand shocks because prices did not vary at all, and in his version of the *Economics of Keynes*, they varied because prices did not vary instantaneously. But both approaches simply took it for granted that, if prices were instantaneously flexible, quantities would always remain at their full employment level. In Leijonhufvud’s words “Perfect knowledge and absence of any costs connected with the act of changing price (or rate of output) would enable the traders in an atomistic market to detect and move instantaneously to the new price equilibrium following a disturbance” (1968a, p. 69). Perfect knowledge was to him synonymous with the presence of a Walrasian auctioneer in the market place, and trading at false prices the inevitable consequence of his absence.

Lucas, on the other hand kept the auctioneer in place but limited his activities. Specifically, he still let him set prices that would keep markets cleared but prevented him from informing agents about them. They had to estimate relative prices by applying knowledge of a true model of the economy in which they operated to information about the time series properties of the monetary disturbances to which it was subject (both of which they were assumed to have) and information about particular money prices culled from the markets in which they were sellers. Thus Lucas logically separated the phenomenon of limited information from the mechanics of price formation; and in so doing, he demonstrated that limited information problems that did not imply price stickiness were nevertheless sufficient to generate quantity variations even in the presence of complete price flexibility.

In short, Lucas showed that neither *Keynesian Economics* nor *the Economics of Keynes* was needed to explain what seemed to be the salient facts of macroeconomic experience, because the addition to a Walrasian general equilibrium model of the right assumptions about agents’ limited information was sufficient to do so. Lucas’s model, if it was to be taken seriously as an “as if” representation of a real world in which there was no auctioneer, amounted to arguing that markets would be kept cleared, not just by the collective foresight of entrepreneurs acting in anticipation of impending monetary changes as Keynes (1930, p. 141) had suggested when



stretching for an example, but also by those entrepreneurs' ability to take account of the errors into which imperfections in that foresight would collectively lead them. But far fetched though it was, even on an "as if" basis, it implied nevertheless that a separate macroeconomics was logically unnecessary for the explanation of output and employment fluctuations, and that Walrasian microeconomics was sufficient as a basis for all economic theory. Economists in large numbers began to reach for Occam's razor, and both IS-LM style *Keynesian Economics* and Leijonhufvud's *Economics of Keynes* were quickly cut adrift. In short, Walrasian economics seemed to have won the third and final battle with the Marshallian alternative.<sup>16</sup>

#### *A postscript on inflation and disequilibrium*

As a matter of logic, to show that it is not necessary to refer to a specific factor when explaining a phenomenon does not also demonstrate that factor's irrelevance, and as Harry Johnson (1965, p. 395) warned in his review of *The Monetary History of the United States*, "Occam's Razor is a fine principle, but there is no need to cut the throat of empirical research with it". Thus, though in the 1970s and 1980s it was very difficult to get serious attention paid to any analysis of output fluctuations that either relied on price stickiness and/or postulated information problems that did not square with the idea of rational expectations, Marshallian pragmatism turned out still to have some life left in it when it came to coping with the economics of inflation.

As we all know, it is very difficult to find a place for money in an economic model in which markets always clear, which is why work in the Newclassical tradition expended so much energy on *cash-in-advance* constraints, and *overlapping generations* models.<sup>17</sup> For many applications, perhaps this did not matter, but, hardly surprisingly, investigations of the consequences of a falling value of money that began from premises that money had no serious work to do in the first place were hardly likely to find these to be important. The best that they could do to capture the idea that inflation was costly - and everyday experience demonstrated beyond any reasonable doubt that it was, extremely so - seemed to be to follow Friedman (1969) in deploying Patinkinesque formulations of the demand for money that relied on putting real balances in the utility function, or to revert to Baumol-Tobin style models of transactions costs in asset markets; but these rather arbitrary fixes implied that the costs of inflation were merely a matter of "shoe-leather", barely worth considering when weighted against likely unemployment

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<sup>16</sup>As with Friedman and Schwartz (1963), it took some years for the full significance of Lucas's contribution to be fully appreciated. As we have seen, even so notable a contributor to the Newclassical literature as Robert J. Barro would later become was still publishing on the economics of fixed price equilibrium models as late as 1976.

<sup>17</sup>The cash in advance constraint was originated by Clower (1967), in order to highlight the ideas that, because, in a monetary economy, goods did not exchange for other goods, they were less liquid than money, and that this had consequences for the way in which markets function. These ideas are obviously closely related to those that inform Leijonhufvud's work, and it is therefore safe to say that the uses to which Newclassical economics put the cash in advance constraint were not among those that Clower had in mind.

costs of reducing it, as James Tobin (1972) was quick to point out

The basic trouble here was that, in a Walrasian framework, the costs of inflation at best could be assessed on the assumptions that it was “fully anticipated” and that markets for goods and services continued to clear. Because such an approach trivialised money in the first place, it also trivialised any disorder of the monetary system, inflation included. It is surely no accident that Leijonhufvud (1977a & b, repr. 1981), a recent exponent of a Marshallian approach to microeconomics that left space for market disequilibrium, was quick to recognise these problems, and to propose an alternative line of attack. This started from an institutionalist vision of monetary exchange that encompassed its essential role in the workings of the market economy, and enabled him to organise ideas about how inflation not only undermined money prices’ ability to transmit information and incentives to agents, but also arbitrarily redistributed the property rights on whose security the very workings of voluntary exchange depend in the first place.

There is not space here to give Leijonhufvud’s work on the costs of inflation the attention it warrants, but I suspect that a careful study of the subsequent literature on these issues, that finally led to policy makers taking them seriously enough to begin to tackle inflation in the 1980s, will show that it had a seminal influence on them<sup>18</sup>. I also suspect that a comparison of Leijonhufvud’s earlier work on the disequilibrium microeconomics of employment fluctuations with his later analysis of inflation’s capacity to disrupt the workings of market mechanisms will reveal close analytic connections between them, though Leijonhufvud himself did not stress these connections. His 1977 paper on inflation started from institutions rather than micro-theory *per se* and when he developed its ideas further in his much under-appreciated work with Daniel Heymann on *High Inflation* (1995), it was once more these factors, not to mention a great deal of empirical evidence about what actually happens in markets under such conditions that took centre stage.

Recall, furthermore, Harry Johnson’s Lucas inspired objection to Leijonhufvud’s disequilibrium dynamic reconstruction of the *Economics of Keynes*: “it is virtually impossible to invent a plausible mechanism that leaves the economy in disequilibrium with unexploited possibilities for profits or increased labour incomes, and at the same time specifies how the economy will respond to a *change* in profit or labour opportunities”. Does not Leijonhufvud’s subsequent work on inflation imply a response to this criticism along the following lines? “Quite so: that is because once disequilibrium takes hold of a monetary economy, markets stop working. If we want a world to which the special case of Walrasian general equilibrium theory can usefully be applied, we had better have policies that prevent either deflationary or inflationary shocks big enough to bring about such a state of affairs”. Let me conclude by asking whether this would not be a very pragmatic, even Marshallian, comment on the limits to Walrasian theory’s usefulness, and by expressing the hope that it might also meet the approval of the author of *On Keynesian Economics and the Economics of Keynes*?

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<sup>18</sup>For example, it is cited in Peter Howitt’s now classic 1990 paper on the costs of inflation which played a crucial role in the debates that preceded Canada’s adoption of inflation targets in 1991.

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