1987

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Citation of this paper:

Laidler, David. "The Austrians and the Stockholm School - Two Failures in the Development of Modern Macroeconomics."
Department of Economics Research Reports, 8708. London, ON: Department of Economics, University of Western Ontario (1987).

10293

ISSN:0318-725X
ISBN:0-7714-0911-7

RESEARCH REPORT 8708

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DEVELOPMENT OF MODERN MACROECONOMICS

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N6A 5C2

July 1987

THE AUSTRIANS AND THE STOCKHOLM SCHOOL - TWO FAILURES IN THE DEVELOPMENT
OF MODERN MACROECONOMICS?

by

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A paper presented at a Conference on the Economics of the Stockholm School to be held at the Grand Hotel Saltsjöbaden August 31-September 2 1987. I am grateful to Lars Jonung, Johan Myhrman and Eric Lundberg for helpful discussion of some of the issues dealt with here, and to Hans Brems, Karl-Olof Faxen, Herbert Giersch, Peter Howitt, Lars Jonung, Axel Leijonhufvud, Johan Myhrman and Paul Samuelson for helpful comments on an earlier draft of this paper. None of them is responsible for the opinions I express, though. I also gratefully acknowledge the financial support of the Social Science and Humanities Research Council of Canada.

INTRODUCTION

Nowadays Keynesian economics is the most visible legacy of the debates about macroeconomic issues that marked the 1920s and 30s. That is because Keynes and his associates were successful, in the well defined sense of laying down an agenda for research, a syllabus for instruction and a programme for the conduct of policy, which dominated the subject for three decades. In the 1920s and 30s, however, Cambridge was but one of a number of centres where potentially important developments in what we would now call macroeconomics took place. Austrian business cycle theory, developed in Vienna and at the London School of Economics, and the work of the Stockholm School on problems of "Monetary Equilibrium", attracted just as much attention as the efforts of Keynes and his associates, and there is no sign in the pre-1936 literature of how quickly and completely Keynesian economics would come to dominate the discipline.

That two bodies of work could fail as completely as those produced by the Austrians and the Stockholm School are now are commonly judged to have done, presents an interesting problem, in and of itself, and one of the aims of this paper is to investigate the nature of the failures in question.¹ It has a second purpose though. Both the Austrians and the Stockholm School were self-consciously working on problems that grew out of the work of Knut Wicksell, and indeed the very same problems. Even so, the Stockholm School had arrived, by about 1936, at a set of ideas which some commentators (e.g. Shackle 1967) claim to have anticipated to an important extent those of Keynes' *General Theory*, and which at the very least enabled them readily to understand and assimilate Keynes' thought. The Austrians on the other hand created a theory and derived from it a set of policy proposals which were

quite antithetical to what was to become Keynesian economics. The question of how two groups, in at least sporadic contact with one another, and starting in the same place, could so quickly move so far apart provides the second theme of this study.

WICKSELL

Knut Wicksell's work provides a crucial link between the Quantity Theory of Money, as it was understood in the later years of the 19th century, and modern Macroeconomics.² Wicksell himself was a quantity theorist, but he was dissatisfied with the analytic apparatus he had inherited from his Classical predecessors. In particular, he saw that Say's Law, as they understood it, precluded the existence of a supply and demand mechanism capable of moving the general price level towards whatever equilibrium value the Quantity Theory might require after a monetary disturbance. In seeking to repair this defect in the logical structure of Classical Economics he became a pioneer of that integration of monetary theory with neo-Classical value theory which has been one of the principal items on the research agenda of twentieth century economics. Wicksell applied the concepts of supply and demand to the economy's output as a whole, and argued that the general price level would move in response to a discrepancy between them. In postulating the existence of such a discrepancy, of course, he both abandoned Say's Law, as the Classical Economists had usually understood it, and made use (albeit in embryo form, and to deal with the determination of prices rather than output) of the concept of aggregate demand which was to play a central role in the Keynesian Revolution.

Wicksell's celebrated "cumulative process" was, for its creator, a supplement to the Quantity Theory, meant to elucidate the processes whereby

the price level moved from one equilibrium level to another in the presence of a modern banking system. As is well known, it postulated that the existence of a shortfall of the banks' lending rate of interest, the "money rate", from the "natural interest rate" (to be defined in a moment), would induce entrepreneurs to increase their borrowing from the banks and to bid up the prices of factors of production. The increase in factor incomes, thus induced, in turn ensured that the price level of output would also increase. Given the assumption about entrepreneurs' expectations usually made by Wicksell, namely that this period's prices were expected to prevail next period, this mechanism would work to raise prices period after period, until the discrepancy between the two interest rates was removed. It would be removed eventually, though, because a commodity currency usually lay in the background of Wicksell's analysis. Rising prices would cause an increasing demand for currency on the part of the public, the banks would lose reserves, and in response would increase their lending rate. Hence the price level would settle down at a new and, according to Wicksell, meta-stable equilibrium value.

Though this process was conceived by Wicksell as a supplement to the Quantity Theory, the quantity of money itself, which he, in common with most of his contemporaries thought of as consisting solely of what we would now call currency, played no role in the actual process of price formation. Credit granted by the banking system was the active element here. Furthermore, in some expositions of the cumulative process, (eg (1905, 1935) p. 194 et seq.) Wicksell found it convenient to analyse the operation of what was usually called a "pure credit economy" in which currency did not exist. Thus a body of analysis designed to supplement the Quantity Theory had the

potential to supercede it, for the conditions of "monetary equilibrium" had to do with the credit market operations of the banking system, and not, except indirectly in the particular case of a currency-using economy whose banks also held currency as a reserve, with the quantity of "money" and its velocity of circulation. In order fully to realise the potential of Wicksell's contribution, though, his successors had first of all to clarify the notion of "monetary equilibrium", and this proved problematic. Wicksell had attributed to it no less than three distinct characteristics. First, treating the "natural rate of interest" as the marginal product of the economy's capital stock, Wicksell had argued that monetary equilibrium would rule when the money rate of interest was equal to that natural (or, in the vocabulary of the *Lectures*, "normal") rate; second, he had suggested that equality between the economy's rates of saving and investment was a property of that same equilibrium; and third, he had argued that it would be characterised by constancy of the general price level.

In his first, and most careful exposition of the cumulative process, that of *Interest and Prices* (1898 (1936) ch. 9) Wicksell had worked with a model whose properties ensured that the abovementioned conditions were indeed simultaneously attainable. No fixed capital accumulation took place; and land and labour were available in given quantities to be hired by entrepreneurs, in order to produce, over a uniform production period, a homogeneous "corn" output, which also functioned as the wage good. Hence with output and capital consisting of the same physical units, the natural interest rate was well defined as the marginal product per unit of variable capital of the aggregate stock of variable capital. Moreover, with aggregate savings and investment always zero, output remained constant, and zero net credit creation by the banking system resulted in stable prices. Perhaps to add realism to his

analysis of the cumulative process, in later expositions (e.g. 1905 (1935) and (1907)) Wicksell placed it in the context of a growing economy with fixed capital accumulation and potentially heterogeneous output, without facing up to the following questions: would the marginal product of capital per unit of capital then remain well defined? would positive saving and investment be equilibrated by a rate of interest equal to it if it was? and would the amount of bank credit creation taking place at whatever turned out to be this critical value of the money interest rate indeed be such as to produce price stability in a growing economy?³

Some theoretical muddles mark the end of a line of research, and others a beginning. Wicksell's muddle was seminal, for it defined the theoretical starting point for the research of both the Austrians and the Stockholm School. Both groups, though, were motivated by more than a desire to sort out a theoretical difficulty. The economic instability that plagued Europe in the wake of the First World War, and which would come to plague the United States after 1929, gave their research a strong practical impetus; and though "monetary equilibrium" was first and foremost a theoretical concept, it appeared to be of considerable policy significance as well. If the conditions producing monetary equilibrium could be clarified and defined in terms of variables with readily observable real world counterparts, then, so it seemed, a successful formula for ridding the real world of economic fluctuations might be at hand. Both groups, in short, took up this concept, which in Wicksell's hands had been a tool for analysing secular movements in the price level, and transformed it into the central idea of business cycle analysis, both theoretical and policy oriented. How the two groups in question tackled the questions involved here, why their answers to them came to diverge so greatly, and how those answers failed to attain a lasting well-identified place in the body of macroeconomic knowledge, will form the subject matter of the remainder

of this essay.

THE AUSTRIANS

The adjective "Austrian" is used in a very narrow sense here to refer to that body of business cycle theory developed during the late 1920s and early 1930s from certain insights of Ludwig von Mises, particularly by Friedrich von Hayek, but also by Lionel Robbins. Thus, I am not here concerned with the contributions to Mathematical Economics that were made in Vienna in the 1920s and early 30s, with Schumpeterian business cycle analysis, or indeed with that vision of economics as dealing with an ongoing and essentially creative process of competition which marks current work in the "Austrian" tradition, work which also owes much to Mises and Hayek. The business cycle theory in question, whose *locus classicus* is Hayek's famous lectures on *Prices and Production* (1931-2) was, for a short time before the publication of the *General Theory*, the most fully developed theoretical account available of economic fluctuations, and seemed to some contemporary observers to represent a line of enquiry at least as likely to come to dominate the discipline as anything being developed at Cambridge.

The Wicksellian origins of this work were quite explicitly recognised by the Austrians, not least by Mises in his analysis of "the gratuitous nature of credit" (1924 (1953) p.352), by which he meant the problem of what forces they are that compel the rate of interest charged by the banking system to move towards ". . .the level determined by the circumstances of the capital market, i.e. the market in which present goods and future goods are exchanged for one another." (p. 352). Though Mises found Wicksell's analysis of the interaction of the money and natural (or normal) interest rates inadequate, he nevertheless conceded to him ". . .the merit of having stated the problem clearly." (p. 355). As we have seen above, the mechanism in Wicksell's analysis linking the money and normal interest rates was the effect

that any discrepancy between them would have on the price level, hence on the public's demand for commodity money, and therefore on the credit terms the banking system offered its customers. He thus created, as has already been stressed, a theory of the role of interest rates in the transition from one equilibrium price level to another. Mises, noting the crucial role played by a commodity money in restoring equilibrium between Wicksell's money and natural interest rates, and noting that this particular institution, though highly desirable, did not seem essential to the economic system, sought a deeper and more general analysis, and in so doing began the process of converting a theory of price level change into one of real economic fluctuations.

Wicksell's work was not the only starting point for the Austrians. Boehm-Bawerk's capital theory, which had deeply influenced Wicksell's first, and in many respects most thorough, exposition of the cumulative process, that of *Interest and Prices*, provided a vital component of their model, as we shall see, while the influence of Walrasian general equilibrium analysis is also much in evidence, particularly in Hayek's work. Moreover, and crucially, Austrian business cycle theory was a self-conscious application of a particular methodology of economics, one whose most accessible account is to be found in Lionel Robbins' *Nature and Significance of Economic Science* (2nd ed. 1935) but which under the influence of Carl Menger had marked Austrian work from the very outset. This method was, as is well known, individualistic, but above all rigourously deductive. Economic theory was thought of as being exclusively a matter of deriving conclusions from premises which, being self-evidently true, could yield only true implications. Empirical evidence might have its uses to illustrate the truth of conclusions gained by deductive methods, or to provide a quantitative foundation for their practical implementation, but it had no role to play in establishing their validity:

"If. . . the theory is logically sound . . . the best that statistical investigation can do is show that there still remains an unexplained residue of processes. It could never prove that the determining relationships are of a different character from those maintained by the theory." (Hayek 1929 (1932) p. 33)

For the Austrians, only conclusions rigourously derived from what we would nowadays term "maximising premises" had scientific validity, and Robbins (1935 p. 115) was surely referring to Austrian business cycle theory when he wound up his attack on "Quantitative Economics" with the remark that

". . . a few isolated thinkers, using the despised apparatus of deductive theory, have brought our knowledge of the theory of fluctuations to a point from which the fateful events of the last few years can be explained in general terms, and a complete solution of the riddle of depressions within the next few years does not seem outside the bounds of probability."

It has sometimes been noted that the revolution in deductive microeconomic theorising which Robbins and his associates at the London School of Economics had so much to do with creating in the 1930s, brought about a change in the discipline almost as important as that brought about by Keynes (and some modern commentators would, I am confident, replace "almost" with "more"). It is worth noting, therefore, that to at least one of the architects of that revolution, Austrian Business Cycle Theory appeared to be part and parcel of it.

What then was Austrian Business Cycle Theory? It began, as I have already remarked, with Mises' attempts to find a more general explanation than Wicksell had provided of the tendency of the money rate of interest to converge upon a normal value determined on the real side of the economy. In his hands, and those of Hayek, it became an explicit attempt to explain the occurrence of depressions in real income and employment. As far as Hayek was concerned, the starting point of such an explanation had to be a state of

general equilibrium characterised by full employment. He put it in the following terms in *Prices and Production* (p. 3)

"...if we want to explain economic phenomena at all, we have no means available but to build on foundations given by the concept of a tendency towards equilibrium. . . .If we are to proceed systematically, therefore, we must start with a situation which is already sufficiently explained by the general body of economic theory. And the only situation which satisfies this criterion is the situation in which all available resources are employed. The existence of unused resources must be one of the main objects of our explanation."

Or again (p.95)

"...we can gain a theoretically unexceptionable explanation of complex phenomena only by first assuming the full activity of the elementary economic interconnections as shown by the equilibrium theory, and then introducing consciously and successively, just those elements which are capable of relaxing those rigid inter-relationships." (passage originally in italics)

But though by "equilibrium theory" Hayek explicitly meant the work of "the Lausanne School of theoretical economics" (1929 (1932) p.42, fn.), the interrelationships which for him, as for Mises, needed to be relaxed if the cycle was to be explained were those which Boehm-Bawerk, rather than Walras had particularly stressed, namely those involved in allocating resources over time.⁴ The role of the rate of interest was, according to the Austrians to co-ordinate inter-temporal choices. The act of saving involved the sacrifice of current consumption in the expectation of consumption in the future. The act of investment involved devoting currently available resources to the production over time of those future goods. Moreover, in a modern economy, these two acts were usually undertaken by different sets of agents:

"Only in comparatively few cases will the people who have saved money and the people who want to use it in production be identical. In the majority of cases, therefore, the money which is directed to new uses will first have to pass into other hands. The question *who* is going to use the additional funds available for investment in producers' goods will be decided on the loan market." (1931-2(1936) p. 84)

If the loan market worked as it should, an increase in the attractiveness of saving for the sake of future consumption would lower the rate of interest, thus providing the necessary incentive to producers to adopt a more roundabout, and therefore in Austrian eyes necessarily more productive, method of production. Unfortunately, in a monetary economy, the loan market could not be relied on always to work in this way.

Like Wicksell, the Austrians took the Quantity Theory as the starting point for their monetary analysis, but they were particularly critical of the central place which that theory accorded the idea of the "general price level": the latter was a statistical artifact which corresponded to no economic variable of importance for individual maximising behaviour.⁵ Here the critical prices were relative prices, and the Quantity Theory, in focussing on the effects of monetary changes on an aggregate statistical artifact overlooked their all-important non-neutral influence on relative prices.

"Those who hold the mechanical version of the Quantity Theory. . . believe that the increase in the quantity of money must eventually lead to a uniform increase in the prices of all economic goods . . . Thorough comprehension of the mechanism by means of which the quantity of money affects the prices of commodities makes their point of view untenable. Since the increased quantity of money is received in the first place by a limited number of economic agents only and not by all, the increase in prices at first embraces only those goods that are demanded by these persons. . ." (Mises 1924 (1953) p. 140)

The rate of interest was a particular relative price, and to the extent that monetary changes influenced it, they were capable of dislocating the mechanisms co-ordinating the inter-temporal allocation of resources. This was the central insight upon which Austrian business cycle theory was built.

In Austrian analysis the equilibrium value of the rate of interest is that which equates the supply of voluntary saving in the economy to the demand for resources for investment. Any credit creation on the part of the banking system involving an increase in the money supply will be associated with a lower than equilibrium value for the rate of interest and to the extent that the newly created funds come first into the hands of firms, will lead to a demand for investment goods in excess of voluntary saving. The newly created money in question will, however, enable firms to realise their demands and to obtain resources by bidding them away from consumers. Such "forced saving", however, is the first step towards economic crisis, because ". . . incomes of wage earners will be rising in consequence of the increased amount of money available for investment by entrepreneurs. . ." while at the same time ". . . these decisions will not change the amount of consumers' goods immediately available. . ." Hayek (1931-2 (1936) p. 87). Indeed, in due course the supply of consumer goods will shrink, because a curtailment of their current production (albeit with a view to an increase in their production at some time in the future) is of the very essence of the forced saving process.⁶

Wage earners, however, will wish to divide their incomes between consumption and saving in the same proportion as before, and once money passes into their hands, it will underpin an undiminished real, but increased nominal, demand for consumption goods. The first consequence of the excess demand for consumer goods implicit here will be a rise in their relative price. This may, in turn, influence profit expectations, and for a while encourage firms to persist in borrowing from the banks. The latter, however, ". . . for obvious reasons . . . cannot continue indefinitely to extend credits. . . ." (p.89) Eventually, then, the economy's underlying savings

rate reasserts itself as a constraint on investment; but in the interim resources have been devoted to initiating investments which can no longer be seen through to fruition. Producers working on "... a process where the transition to longer roundabout processes is not yet completed when the amount of money ceases to increase. . . will have to abandon the attempt to change over to more capitalistic methods of production." (p. 60).

Resources will now have to be shifted away from more, towards less, roundabout methods of production, but unlike the preceding shift in the opposite direction, this movement cannot be smoothly accomplished. On the contrary, this "... transition to shorter processes . . . will regularly be accompanied by a crisis. . ." (p.93): capital equipment embodied in roundabout processes, and perhaps still under construction, being durable and specific in nature, cannot immediately be transferred to other uses. Thus "... a loss of capital and reduction of income [are] inevitable" (p.93). How severe is the crisis depends upon how much forced saving has preceded it. The more prolonged and vigorous the boom, the more prolonged and severe the slump, for the latter will only come to an end when redundant capital has at last been amortised and a time structure restored to production compatible with the economy's underlying saving rate.

That capital should necessarily be idle in the slump is clear, but it is less obvious why labour could not be redeployed to the short production period industries from which it had been withdrawn in the first place. This problem was certainly evident to Hayek and Robbins, and they invoked at various points downwardly rigid wages, and fixed (or nearly so) proportions in production, in order to explain why redundancy of capital would be accompanied by redundancy of labour. The explanations in question, though, are not integral to the Austrian model and have something of an ad hoc air to them. In Haberler's

words, "This . . . explanation of the depression is . . . incomplete and unsatisfactory." (1937 (1964)) p. 58. To modern students of macroeconomics, whether brought up on conventional Keynesian wisdom, or on more recent "new-Classical" analysis, this failure of Austrian Business Cycle Theory to provide a properly integrated account of unemployment must seem a grave one, but before passing judgement, the reader should recall that until 1936, no-one provided a satisfactory account of this phenomenon. Even so, the absence of a theory of unemployment did not prevent Austrian analysis being used to derive policy proposals of a sort, proposals which, despite the novelty of the theoretical analysis upon which they were based, bore more than a passing resemblance to the time honoured wisdom of English Classical economics.⁸

For most Classical Economists, not least the proponents of the 1844 Bank Charter Act, the essence of the problem of preventing slumps was seen to lie in preventing the preceding boom; or, if that was impossible, in ensuring that it was as mild as possible. Hayek stated the Austrian position in the following words in *Prices and Production* ". . . we arrive at results which only confirm the old truth that we may perhaps prevent a crisis by checking expansion in time, but that we can do nothing to get out of it before its natural end, once it has come." (p.99) In principle the boom could be prolonged by credit expansion which permitted firms to continue to outbid consumers for resources, but Lionel Robbins' (1934) warning on this matter is quite representative of Austrian views.

"Once costs have begun to rise it would require a continuous increase in the rate of increase of credit to prevent the thing coming to disaster. But that itself, as we have seen in the great post-war inflations would eventually generate panic. Sooner or later the initial errors are discovered. And then starts a reverse rush for liquidity. The Stock Exchange collapses. There is a stoppage of new issues. Production in the industries producing capital-goods slows down. The boom is at an end." (pp. 41-42)

Accelerating inflation, followed by collapse, was thus the predicted consequence of attempting to prolong the boom by prolonging the monetary impulse that had started it (or permitted it to begin) in the first place.⁹ Nor could the expansion of credit to finance consumer demand be expected to mitigate a crisis already characterised by an excess demand for consumer goods. On the contrary, as Hayek noted ". . . a relative increase in the demand for consumers' goods could only make matters worse." (1931-2 (1936) p.97). As to public works, their implementation would have consequences similar to policies designed to maintain private sector investment. Robbins, discussing contemporary American experiments along such lines warned that

"The unbalancing of the budget and the vast expenditures on public works have an inflationary tendency which may well. . . engender an inflationary boom - a boom which . . . would be likely to be followed by a deflationary collapse." (1934) p.125)

Once in a crisis, then, there was nothing to be done but await "its natural end". Better by far to avoid it by maintaining monetary equilibrium in the first place, but this would be well nigh impossible.

"The rate of interest at which, in an expanding economy, the amount of new money entering circulation is just sufficient to keep the price-level stable, is always lower than the rate which would keep the amount of available loan-capital equal to the amount simultaneously saved by the public." (Hayek 1929 (1932) p. 114) (original in italics)

For the Austrians, price level constancy was not a characteristic of monetary equilibrium. What was needed was a rate of interest which would equate saving and investment, this in turn required a constant money supply, and therefore, in a growing economy with no change in the technology of exchange, a *falling* price level.¹⁰ But to maintain a constant money supply in the presence of a modern banking system was essentially impossible.

Though Mises tended to regard the behaviour of the banking system as initiating the cycle, Hayek, who in this respect followed Wicksell, did not. For him ". . .an improvement in the expectations of profit or . . . a diminution in the rate of saving. . ." (1929 (1932) p. 147) could just as well initiate discrepancy between the money and natural interest rates (the use of the Wicksellian vocabulary here reflects Hayek's, or his translators', choice), because in the face of such changes the banks would be bound, for a while at least, to continue to lend at the pre-existing interest rate. They could do so even under a gold standard by means of the ". . .often-disputed 'creation' of deposits. . ." (p.148) because ". . .the ratio of reserves to deposits does not represent a constant magnitude, but, as experience shows, is itself variable." (p.163)¹¹

The practical implication of all this was that "So long as we make use of bank credit as a means of furthering economic development we shall have to put up with the resulting trade cycles" (p. 189), not least because any attempt to do better ". . .could only be attempted by a central monetary authority for the whole world. . ."(1931-2 (1936) p.125). To have individual central banks attempt to maintain a constant supply of money would - at least under a gold standard - interfere with world wide allocative efficiency. Hence

"It is probably an illusion to suppose that we shall ever be able entirely to eliminate industrial fluctuations by means of monetary policy. The most we may hope for is that the growing information of the public may make it easier for central banks both to follow a cautious policy during the upward swing of the cycle, and so to mitigate the following depression, and to resist the well-meaning but dangerous proposals to fight depression by 'a little inflation'." (p.125)

THE STOCKHOLM SCHOOL

Long before there was Austrian Business Cycle Theory, there existed a well established "Austrian (or Vienna) School" of economics, working out a research programme whose outlines are clearly discernible in the writings of its acknowledged founder Carl Menger; in the previous section of this paper I have drawn attention to aspects of the relationship which the cycle theory of Mises, Hayek, and Robbins bore to that broader tradition. The Stockholm School is altogether less well defined. There is no figure in the history of Swedish economics comparable to Menger, and though Wicksell, Cassel, Davidson, and their contemporaries did much to advance our subject, their contributions grew out of what sometimes seems to be a propensity to argue about almost everything, rather than from any collective attempt to seek the answers to a common set of problems using mutually agreed methods to derive the implications of an acceptable set of assumptions. Even the Swedish contemporaries of Hayek were not defined as belonging to a definite "school" until Bertil Ohlin did so in his well known (1937) commentary on the relationship between their work and the central ideas of the *General Theory*.

The casual reader of Ohlin can easily gain the impression that there had emerged in Stockholm by the mid-1930s a consensus about the theory of macroeconomic fluctuations as coherent as that to be found among any group of Keynesians or Austrians, a consensus that had in certain crucial respects anticipated Keynes' central insights into the problem of unemployment. However, matters are not quite so clearcut. A reading of the important Swedish contributions of the period reveals far less of a consensus than marks contemporary Austrian writings. If Lindahl, Lundberg, Myrdal, and indeed Ohlin himself, really did think of themselves as belonging to a "school"

before (or even after) 1937, it was a school with rather lax rules. Moreover, as Patinkin (1982) has shown, the key Keynesian insight that output changes themselves can act as an equilibrating mechanism did not play a central role in Swedish economics before 1936. Even so, the label "Stockholm School" has stuck, and it is hard to see that it would have done so if those to whom it was affixed had not held some important ideas in common. Furthermore, the speed with which Keynesian analysis was assimilated by Swedish economists, notably by Lundberg ((1937) Chs. 5, 8, 9,) after the publication of the General Theory also suggests a strong affinity between those commonly held ideas and those out of which the *General Theory* developed.

Now no-one would take seriously a suggestion that Mises, Hayek, and Robbins were prototype Keynesians. But, as was stressed at the outset of this paper, their macroeconomic analysis started from the same point as that of the Stockholm School, namely the incompatible concepts of "Monetary Equilibrium" which Wicksell had bequeathed to his successors. As was also noted earlier, these two facts surely present something of a puzzle. How could two groups of economists, starting in essentially the same place, and dealing with essentially the same problem, move so far apart so quickly? What was it about the economics of the Stockholm School that makes their work appear so Keynesian and so antithetical to that of the Austrians?

Gunnar Myrdal remarked in (1932 (1939)) that

"It is not surprising that it was the Austrians who found the connexions with Wicksell: Wicksell himself was a pupil of Boehm-Bawerk and he put his thoughts into forms and constructions based directly on Austrian habits of thought."
(p. 7)

Myrdal and his colleagues did share Austrian views on the inadequacy of the Quantity Theory, on the grounds that ". . . credit is a causal factor for the price level, but also for price relations [i.e. relative prices]. . ." so that ". . . the problem of credit requires a monetary theory which is really integrated with the central economic theory. . ." (p. 16), but otherwise their notions of what constituted a valid body of "central economic theory" were far removed from "Austrian habits of thought."¹²

Wicksell's natural interest rate was, as Myrdal correctly noted ". . . the physical marginal productivity of the roundabout process of production . . ." (p.24). This concept derived directly from Boehm-Bawerk, and became completely central to Austrian cycle theory, where the key mechanism giving rise to economic fluctuations was precisely a departure of the money interest rate from this natural level. But the relation between Wicksell and Boehm-Bawerk is most noticeable in Interest and Prices. As his thought developed to encompass a growing economy with heterogeneous output, the concept of the "natural" or "normal" rate of interest became less well defined as "... the physical marginal productivity of the roundabout process of production...". The Austrian solution to clarifying the rather confused concept of monetary equilibrium with which Wicksell had ended up involved restoring to it a much stronger element of Boehm-Bawerk's ideas about capital than he himself had utilised in his later expositions of it. Wicksell's Swedish successors, on the other hand, took it as one of their first tasks to rid his monetary theory of any reliance that remained in his later work on just these ideas.

As early as his 1929 essay on *The Place of Capital in the Theory of Price* Eric Lindahl had explicitly dealt with problems of measuring capital (cf. Lindahl (1939, p. 313 et seq.), noting the arbitrary elements inevitably

present in any treatment of such an aggregate; and by (1930) he had come to understand that

"Only under very special assumptions is it possible to conceive of a natural or real rate of interest determined purely by technical considerations, and thus independent of the price level. For this to be true it must be supposed that the productive process consists only in investing units of goods or services of the same type as the final product, the latter increasing with the passage of time alone without the co-operation of other scarce factors. . . . Under more realistic assumptions . . . the real rate of interest does not depend only on technical conditions, but also on the price situation, and cannot be regarded as existing independently of the loan rate of interest" (1939 pp.247-248)

Myrdal too understood the point (cf. (1932 (1939) p. 50 et seq.) and devoted a large part of the early sections of his "immanent criticism" of Wickseil to purging the latter's monetary theory of the concept of a natural interest rate determined independently monetary conditions.

Myrdal's approach here was, in essence, the Fisherian one, which even today dominates monetary economics, namely to recognise ". . . capital value [as] nothing else than the discounted sum of all future gross incomes minus operating costs . . ." (p. 62) and to state as an aggregate approximation to the condition for monetary equilibrium (at this point in a static economy) ". . . the condition of equality between the capital value and the cost of reproduction of existing real capital." (p. 69 Myrdal's italics). As to the case of a growing economy, a discrepancy between the value of existing capital and the cost of producing new equipment had to exist so as to create ". . . a complex of profit margins in different firms which stimulates just the amount of total investment which can be taken care of by the available capital disposal." (p. 82, italics in original.) For the Austrians the conditions of "monetary equilibrium" involved the relationship between bank lending rates and what they treated as a technically determined natural rate of

interest.¹³ For Myrdal and the other Swedes, bank lending rates were important for monetary equilibrium too, but they replaced what for the Austrians seemed to be the objective and stable technical characteristics of the production function, with a subjective and volatile factor, namely firms' expectations about future net revenue, as the other element to be considered. For those interpreters of Keynes (1936) (eg. Shackle) for whom his stress on expectations marks an essential contribution, this characteristic of the work of the Stockholm School, is for obvious reasons, of particular significance.

The Stockholm School took it for granted that the analysis of a monetary economy should be based on variables measured in nominal terms, and indeed slowness to adopt such a mode of analysis was, for Ohlin (1937 p.230), a fault of English economics in the Marshallian tradition. To anyone reasoning in terms of nominal variables, it was obvious that both relative price and price level expectations must underlie expectations about future net revenue. It was but a small step from this point to the view that price level fluctuations, *provided they were fully anticipated*, were irrelevant to other aspects of the economy's performance. Hence the behaviour of the quantity of money which was so important for the Austrians, was reduced to a matter of no theoretical significance for the Stockholm School.

Lindahl (1930 (1939)) treats the matter of price level expectations more thoroughly than anyone else among the Stockholm School, though there is ample textual evidence that Myrdal too had a firm grasp of the issues involved. (cf. e.g. 1932 (1939) p.121). Here, as with the problems of giving content to the concept of a natural interest rate Lindahl develops ideas first noted in (1929) (cf. e.g. 1939 p. 330). To begin with, he is clear that the irrelevance of perfectly foreseen price level fluctuations arises from the fact that "[a] shift in the price level that is foreseen by everybody early

enough can be taken into account in all contracts for the future" (p. 148). Such an insight, though it has obvious enough roots in the work of Marshall (1890) and Fisher (1896), was nevertheless something of a rarity in the early 1930s, but Lindahl not only stated it, but went on to point out several crucial implications that it yielded, namely: that "The monetary authority . . . cannot directly regulate the price level through its interest policy, but it can do so indirectly by influencing the primary determining factor, i.e. general anticipations" (1939 p. 149); that "A *neutral* rate of interest does not necessarily imply an unchanged price level, but rather such a development of prices as is in accordance with the expectations of the public. . . ." (p. 252); and that, because

"[i]n a community with perfect foresight, the height of the loan rate of interest depends upon the anticipated course of prices. . . [a] rate of interest that is normal in relation to a certain foreseen course of prices would . . . be abnormal in relation to other anticipated developments, even if in other respects the conditions in which it influenced the demand for and supply of saving are unaltered." (1930 (1939) p. 252)

Now the Stockholm economists did not, of course, regard the perfect foresight assumption as being of immediate practical relevance. Rather the device was used heuristically to enable them to bring out the crucial role of expectations in influencing the conditions of monetary equilibrium. They understood well enough that, if one was willing to assume perfect foresight, the social problem of co-ordinating intertemporal choices posed no special analytic difficulties. In Lindahl's (1929) words,

"Under the assumption that the future is perfectly foreseen, all prices in all the periods included in the dynamic process thus become linked together in a uniform system. The equilibrium of this system is maintained by the same laws as under stationary conditions." (1929 (1939) p. 330.)

Lindahl and his fellow Swedes, like the Austrians, saw that the really

interesting problems arose when the activities of agents, *lacking perfect foresight*, had to be co-ordinated over time.

For the Austrians prices conveyed information and incentives to agents, and, for intertemporal choices the money interest rate was the relevant price. In their theory, agents extracted the information and acted on the incentives contained therein, on the crucial but usually implicit assumption of expected price level constancy. If that exercise resulted in attempts to execute incompatible plans for saving and investment, then crisis was the inevitable result. The Stockholm School understood that a given interest rate, or term structure of interest rates (cf. particularly Lindahl (1930 (1939) part II ch.3), would convey very different information and incentives depending upon the expectations in whose light it was interpreted. They also believed that expectations could differ among agents and vary over time in response to both exogenous and endogenous variables. Hence they went on to consider a bewilderingly varied menu of possibilities, and the stress on the importance of expectations and their maleability which is one of the great strengths of the economics of the Stockholm School also became one of its great weaknesses. It prevented them from developing a simple analytic model which could yield unambiguous predictions. As Bjorn Hansson (1982) has shown their main theoretical contribution was a method of analysis capable of dealing with alternative possible "dynamic sequences" as the term came to be used, rather than any core theoretical model.

There is not space here to give an account of the efforts of the Stockholm School to create a satisfactory method of dynamic analysis, and in any event Hansson's valuable study deals with just this issue. Suffice it to sketch the main outlines of their contributions here. Starting with a notion of equilibrium every bit as Walrasian as Hayek's, they noted first, as did he, the importance of expectations about the future values of economic variables

as parameters determining their current values. In the absence of perfect foresight, they (and in particular Lindahl (1929, 1930 (1939)) went on to consider a series of Walrasian equilibria evolving over time in response to new information, but rejected this approach as unsatisfactory because it led to the explanation of the evolution of the values of endogenous variables solely in terms of the evolution of exogenous factors. What we would now call "intrinsic dynamics" seemed to the Stockholm School to be essential ingredients of any satisfactory model of economic fluctuations. The key analytic concepts which they developed to deal with them were "the unit period", and the distinction between "ex ante" and "ex post" values of variables.

"It is. . . essential for the very concept of monetary equilibrium. . . that the analysis be restricted to a particular *point* of time. . . . the dynamic problem proper concerns the development from the point of time to a second and a third and so on. . . *periods* of time are defined as the interval between two points of time. . ." Myrdal (1932 (1939) p.43, Myrdal's italics)

At the beginning of the unit period, variables take their ex ante values, and "[i]n the the ex ante calculus it is a question not of realised results but of the anticipations, calculations and plans driving the dynamic process forward." Ex post values are observed when "[l]ooking backward on a period which is finished. . .", they are the values ". . . actually realised . . .". (p. 46)

In due course it became apparent that the problem of analysing the economy's evolution over time could be divided into two sub-problems: how an ex ante ". . . tendency to disparity. . ." in variables might, within the unit period, ". . . develop into an ex post balance." (Myrdal 1932 (1939) p. 46), and how the ex ante ". . . plans and expectations . . ." with which any particular period began related to the ". . . 'realisations' of earlier

periods. . ." (Ohlin 1937, p. 222) Thus the fully developed "sequence" analysis of the Stockholm School dealt with both the evolution of economic variables within the unit period, and the development of plans and expectations between such periods.¹⁴ The voluntary-savings-investment disparity and resulting crisis studied by the Austrians is analagous in a rough and ready way to a particular treatment of the first of these problems, but, as we have seen, the Austrians never systematically integrated into their analysis the notion that the expectations and plans of agents might evolve over time as a result of the economy's behaviour. The capacity of Swedish analysis to yield unidirectional cumulative movements in economic variables of an essentially Wicksellian nature, as well as booms truncated by a crisis, stems in large measure from its treatment of the plans and expectations of agents as endogenous to an ongoing dynamic process. Different conclusions followed from different assumptions about expectations.

Lindahl's work yields an early and readily accessible illustration of this characteristic of Swedish analysis. In (1930 (1939) pp. 169 et seq.) he considered the effects of a fall in the money rate of interest from an initial equilibrium value. He noted that, as a result, "[f]actors of production will be transferred from the direct production of consumption goods to the production of capital goods. . ." This effect would, in due course, render ". . . a rise in the prices of consumption goods . . . unavoidable." (p.170). Up to this point in the analysis he told a completely Austrian story, but he then went on to show that "*[u]nder the assumption that existing prices of consumption goods are expected to continue in the future. . .*" (which was of course Wicksell's usual assumption about expectations) this would involve a rise in capital values too, and therefore a cumulative inflation ". . . since capital values are partly determined by the anticipated prices of consumption goods. . ." (Lindahl's italics).

Moreover, because the increased money incomes arising from such a process would be concentrated in the hands of entrepreneurs rather than workers or capitalists, "[t]he saving required to enable production to be more capitalistic. . ." would be forthcoming, and ". . .from the point of view of the individual. . .in large part quite voluntary". (p.173)

Thus, an experiment with an Austrian beginning need not, according to Lindahl end in crisis at all, but ultimately in a new equilibrium with a more capital intensive mode of production in place. (cf. p.181) The particular assumption made about expectations here was, however, understood to be crucial, for Lindahl went on to warn his readers that

"If we now assume alternatively that individuals, and especially entrepreneurs, expect the rising price movement to continue, the anticipation of higher prices will make longer investments . . . appear still more profitable. This will accelerate the transfer of factors from the consumers' goods industries, with the result that the rise in prices of consumption goods will proceed at an ever increasing pace. . . . [T]he rise in the price level. . .will be cumulative, until the process is brought to an end by a crisis." (1930 (1939) p. 182)

Thus, he concluded that

"The objection made by Cassel . . . to Wicksell that a new equilibrium with a higher price level must always eventually be achieved when the rate of interest has been lowered, therefore holds good only under very special conditions. . ." (p. 186)

This capacity of the analysis the Stockholm School to generate a variety of conclusions about the economy's time path when out of equilibrium, depending upon the specific assumptions made about the nature of the disequilibrium processes driving that time path was to become increasingly prominent as it developed during the 1930s.¹⁵ Although the foregoing example depends upon alternative assumptions about price expectations to generate differing conclusions, the Stockholm School did not become

over-dependent upon the analysis of expectations to generate alternative dynamic sequences. The degree of price and wage stickiness; the availability of unemployed resources; their distribution between the capital goods and consumption goods industries; their mobility between sectors; the extent to which the behaviour of agents vis-à-vis cash holding might intrude upon the functioning of the "pure credit" system upon which their monetary analysis was usually based; the length of the "unit period"; all of these could vary.¹⁶ In doing so they could radically alter the outcome of any experiment that began with a specific departure of the economy from a situation of monetary equilibrium. That celebrated passage of Myrdal's which forms the core of the case for treating him as having anticipated the central theoretical contribution of the *General Theory*:

"Let us assume the case of a downward process. The decrease of income will diminish savings less, the more consumption decreases. A shift in the distribution of incomes, however, in favour of classes which save less at the expense of those which save more, which takes place during such a process, counteracts this decrease of consumption. In the degree to which consumption is maintained by such a shift, losses need not become as big as they must otherwise be in order to bring about the bookkeeping correspondence between capital disposal and the value of real investments which is subsequently necessary. This means that the intensity of the depressive process is then not as great as it would otherwise be."
(p. 119)

when read in context, is seen to present one possible outcome of the consequences of an initial discrepancy between the ex ante plans of savers and investors, not an attempt to state a uniquely important insight into the equilibrating mechanisms at work in an economy sinking into a depression.

Though I have referred to the absence of a core model, yielding definite predictions, as a weakness in the Economics of the Stockholm School, they regarded the open-endedness of their analysis as something of a virtue. Certainly it had a well articulated methodological basis which seems to this

commentator to be closely related to the Marshallian view of economic theory as "an engine for the discovery of concrete truth". Their method was deductive, but they did not, in contrast to the Austrians, expect deductive reasoning alone to produce empirically valid and useful results. Myrdal's justification of ". . . a priori procedure . . . at present particularly in need of being emphasised in the social sciences" was that "[the] first requirement for receiving sensible answers is to have raised sensible questions" (p. 209) not that, such procedure would in and of itself yield such answers. Lundberg's (1937) attitude was similar. His *Studies in the Theory of Economic Expansion* was intended to show how ". . . we may introduce time elements in order to carry . . . static theory further . . ." but he recognised ". . . an infinite number of possibilities for dynamising the static relations" as a result of which "[t]heories of the business cycle tend . . . to follow as many lines of explanation as there are possibilities of disrupting static relations" (p. 51)

Lundberg made no claim to have made ". . . a complete, or even an "unbiased" selection among the numerous theories of money and business cycles". With respect to the theories he did choose to discuss, a ". . . choice . . . primarily . . . governed by discussions among Swedish economists during the last decade . . ." (p. 5) he claimed no more than that

"We have tried to state the underlying assumptions as clearly as possible in order to make the limitations of the conclusions evident. Against the objection to the abstract nature of these assumptions the fact should be taken into account that we have not directly aimed at explaining the actual course of events in a business cycle. . ." (p. 244)

He was willing to go no further than Myrdal in justifying theoretical work:

". . . to be able to discuss the effects of . . . [economic policy]. . . our reasoning must always be based upon the conception of a simplified economic system . . . Any statistical investigation of business cycles, however extensive, cannot diminish our need of such a general conception of economic processes if . . . fundamental questions are to be answered. (p.245)

Deductive reasoning, that is to say, was a means of asking relevant questions, not of answering them, as far as the Stockholm School were concerned, and it is hard to quarrel with such a defence of their work. The trouble is though, that economists seem to prefer economic theory to yield answers, even wrong answers, rather than questions, even interesting ones. If for a modern reader, the central contribution of the Stockholm School is hard to pin down, that is surely because their work yielded so many more questions than answers.

Be that as it may, abstract and (for its time) mathematical though the theorising of the Stockholm School was, it derived its purpose from the practical policy problems facing the Swedish economy during the inter-war depression, and that analysis was indeed used to underpin policy advice. As with their theoretical work, so too with their comments on policy, the Stockholm School were a good deal more eclectic than their Austrian contemporaries, although here too they started from very much the same place, namely ". . . the *value premise* that cyclical movements should be made less severe and the *factual premise* that this requires primarily the maintenance of the conditions of monetary equilibrium." Myrdal (1932 (1939) p. 181.) The Swedes, though, were suspicious, and Myrdal in particular downright scornful, of an approach to business cycle and monetary theory which represented ". . . a rationalisation of economic liberalism, which erects its own fatalistic, negative attitude toward planned economic control into a doctrine".¹⁷ Myrdal cited with approval a remark which he attributed to Cassel to the effect that

" . . . perhaps the whole attitude was ultimately based upon a primitive puritanism; happiness is somehow evil, something immoral, which should be accompanied by a purifying misery now and then in order that those who have experienced it may be redeemed; and so it is only proper, right and natural that after the upswing, with all its sad mistakes, bad times should follow." (p. 201)

The policy pragmatism of the Stockholm School was nevertheless far from being devoid of theoretical discipline. To begin with, they agreed that, because of the key role played by expectations in determining the economy's time path, the aim of monetary policy, which was conceived of in terms the manipulation of interest rates, ". . . should be determined and announced, and in each case the reason for the measures taken should be explained." (Lindahl (1930 (1939) p.232) More specifically, Myrdal urged that the aims of monetary policy be defined in terms of the behaviour of prices, not, though, because economic theory dictated that prices had to behave in a particular way to preserve monetary equilibrium, for we have seen that it did not. Rather he took this position on the practical grounds that "[a] publicly declared monetary policy can hardly serve its purpose if it is not stated in simple terms and in terms which are of direct importance to the anticipations of entrepreneurs." (1932 (1939) p. 193.) He explicitly rejected the idea that ". . . monetary policy should attempt to maintain the employment of the means of production at a maximum. . ." on the grounds that ". . . a monetary policy with this aim as the only standard would either lead to certain and general cumulative price movements . . . or require extensive public regulation of markets. . ." (p. 196).

The affinity of this statement to similar propositions of Hayek and Robbins is not so close as it looks at first sight. It must be read in the context of Myrdal's view that

"Maintaining a monetary equilibrium [is] a question not only of monetary policy but of economic policy as a whole, social policy and the institutions which rule the labour market, cartel legislations and all related factors. Various combinations of these heterogeneous things, more or less under political control, together with appropriate values of the standard combination of credit conditions, produce stable monetary equilibrium relations." (p.184)

Hence, the open-ended economic theory of the Stockholm School yielded a large "field of indifference", in the choice for the setting of policy instruments. Their assignment of monetary policy to the maintenance of equilibrium between saving and investment in the context of a stated policy goal of price stability was just that, an assignment of a particular instrument to a particular target.¹⁸ It did not imply any ranking on theoretical grounds of price stability above other policy goals.

Nor is "price stability" here to be interpreted as constancy of consumer prices or some other convenient index. The Stockholm School were well aware that not all prices were equally flexible, singling out money wages, particularly in the unionised sector, as likely to be sticky. In this they were early exponents of the idea, later associated with Hicks (1955), of a "labour standard" for the determination of the price level, and for Myrdal, at least, ". . . a monetary policy aimed to preserve the equilibrium relations, must, therefore, *adapt the flexible prices to the absolute level of the sticky ones.*" (Myrdal 1932 (1939) p.133 Myrdal's italics). Though the Stockholm economists stressed the role of inflation expectations when discussing nominal interest rates, they did not discuss the possibility that money wages might be influenced by expectations. Even so, they did see scope for other policy tools (Cartel policy and such) to affect the behaviour of sticky prices. Hence this injunction of Myrdal's implied no uniquely appropriate behaviour for the monetary authorities.

THE "FAILURES" OF THE AUSTRIANS AND THE STOCKHOLM SCHOOL

The "failures" of the ideas of the Stockholm School and their Austrian counterparts are as distinct in nature as the two bodies of analysis themselves, and as we shall see the differences between the "failures" are intimately related to the differences between the doctrines. Both were

superceded by what came to be known as Keynesian Economics. However, though after 1936 the major works of the Stockholm School were not to become staples of university reading lists, their principal ideas were nevertheless absorbed into what was in the following three decades the mainstream of macroeconomics. In contrast, over the same period, the contributions of their Austrian counterparts simply fell into neglect, to the point of being abandoned even by many of their own most enthusiastic exponents.

Why the Swedish literature rather than that originating in Cambridge was not chosen by economists at large as the foundation for the macroeconomics of the 1940s and subsequent decades must inevitably be a matter of conjecture.¹⁹ Suffice it here to list a few possible reasons: the relative isolation of the Swedish economists from their American and British colleagues during the war; the inability of most English and American economists to read Swedish and German, combined with their overwhelming numbers in the profession; closely related, the inability of the Swedish profession to generate a textbook through which identifiably Swedish ideas about macroeconomics could be imstilled into anyone studying economics; and also, perhaps most important of all, a factor intrinsic to the economics of the Stockholm School, its already noted reluctance (or inability) to come to sharp, easily grasped, and definite conclusions about specific problems. The contrast between the simplicity of the Keynesian message that movements in output and employment were themselves equilibrating mechanisms, a message readily summarised in a diagram that could be embossed on the cover of a textbook, and the complexity of the exercises whereby, Lundberg, say, demonstrated that almost anything could be the outcome of a dynamic sequence, is a telling one. Simple and definite messages are, for good or ill, the ones

that catch on in our subject, almost regardless, it sometimes seems, of whether or not they are misleading.

In drawing the above-mentioned contrast between the open-ended complexity of the economics of the Stockholm School and the straightforward simplicity of Keynesian economics, though, we should not lose sight of two facts. Keynesian economics did not long retain its simplicity; and much of the clarity of the Keynesian message as it was transmitted to students of economics derived from its being cast in terms of the *ex-ante-ex-post* distinction which was one of the principal conceptual contributions, indeed according to Shackle, (1967) the all important contribution, of the Stockholm School. It will be convenient to elaborate on the latter point first of all. In the *General Theory* Keynes *did* argue that saving always equalled investment, and as a result he *did* leave it unclear whether what we would now term the static multiplier summarised the equilibrium relationship between output and autonomous expenditure or a mere accounting identity.²⁰ As Ohlin (1937 p.237) put it, discussing Keynes' insistence on the equality of savings and investment as a basis for deriving the multiplier relationship,

"...either Keynes' reasoning is *ex-post*, and it explains nothing, or it is *ex-ante*, and then it is entirely wrong. There is no reason why the planned investment plus the planned consumption should be equal to the expected total income for society as a whole. . . . planned investment will differ from planned saving. . ."

For Ohlin, applying conventional Stockholm reasoning, it was precisely the inequality between planned saving and investment which "...sets in motion a process which makes realised income differ from expected income, realised savings from planned savings and realised new investments differ from the corresponding plan." (p. 64). Nevertheless, Ohlin had little time for the

simple multiplier, even correctly analysed, as an account of the outcome of that process. To begin with ". . .planned consumption. . ." depended

". . .not on . . . expected income during the first coming period only, but on what [the consumer] expects to earn over a long period in the future. If a man gets a temporary, well-paid job which gives him a much higher salary than he is used to and more than he can expect to earn later on, his standards of consumption will obviously be much affected by consideration of this latter fact. . . . Keynes' analysis on this point seems a little superficial." (p. 62)

Furthermore

"The chief reason why the multiplier theory can tell us but a little about the effects of a certain increase in investment is not its fluctuation, but the fact that it leaves out of account the reaction of a certain change in the volume of output . . . on profit expectations and the willingness to invest." (p. 240)

The Stockholm School's longstanding stress on the importance of expectations thus led to immediate doubts about the stability of the Keynesian consumption function, for reasons which, much elaborated, were later to underpin the work of Friedman (1956) as well as Modigliani and his associates (eg Modigliani and Brumberg 1954) on this relationship; and also to a proposal that Keynesian analysis needed extending by treating the willingness to invest as an endogenous variable. In the latter respect, as Lundberg was later to note in his introduction to the post-war reprint of his Studies..., the Stockholm School were among the leading precursors of the "dynamic economics" of Harrod (1949) and Hicks (1951), and of much pioneering econometric work on macroeconomic issues too.²¹

By the late 1950s there had emerged a consensus body of macroeconomic analysis, both theoretical and empirical, known as "Keynesian Economics", but of which the *General Theory* was but one important source. The best analytic

ideas of the Stockholm School were simply absorbed along with a host of other influences into that framework. Their policy analysis too became part of a similar consensus that viewed monetary policy as but one not particularly important tool among several available for achieving a variety of economic and social goals. What Lundberg has recently (1985) called the "Swedish Model" of economic and social policy was by no means a unique experiment. Rather it provides an example, albeit a rather extreme one, of a pervasive interventionism in economic and social affairs which characterised policy throughout the western world down to the beginning of the 1970s. The "failure" of the Stockholm School, that is to say, did not lie in any inability on their part to gain acceptance for a significant proportion of their ideas. Rather it lay in the fact that the discipline at large lost sight of the Swedish element in those ideas as it adopted them.²²

If the distinctive contributions of the Stockholm School were absorbed by the mainstream of macroeconomics, those of the Austrians seemed to meet a less happy fate. In the political climate of the 1930s, a body of theory which taught that the depression simply had to be waited out could hardly be expected to withstand the arrival of Keynesian analysis. The latter provided an almost irresistible justification for the ad hoc interventionist policies which the majority of economists had long advocated. Robbins was soon converted to Keynesian ideas, while Hayek, with his (1937) *Economica* essay on "Economics and Knowledge" began to move away from equilibrium theorising deriving from Walras and Boehm-Bawerk towards that vision of economic life as a perpetually evolving set of disequilibrium processes which nowadays is regarded as the hallmark of Austrian economics. One characteristic insight of Austrian analysis of the 1930s, into the relationship between what we now call

the capital-output ratio and the rate of interest, did, as Hicks (1967) noted, survive in the growth models that were so popular in the 1950s and 1960s, but those models too are out of favour now. Only Mises and a few disciples (e.g. Murray Rothbart (1975)) remained faithful to the Austrian Business Cycle Theory which has been discussed in this paper, and their work now lies on the fringes of the subject as far as the majority of practitioners are concerned.

A judgement that the failure of Austrian theory involved the wholesale and permanent rejection of its ideas would have commanded widespread assent among economists as recently as fifteen years ago. Since then, though, "new-Classical" business cycle theory has laid a strong (though still disputed) claim to be the mainstream doctrine of a new macroeconomics which: asserts the desirability of basing all economic analysis, including macroeconomic analysis, on explicitly maximising premises; proclaims the primacy of deductive reasoning in a Walrasian framework as the most fruitful path to knowledge of the workings of the economy; treats the response of individuals to misleading relative price signals about the terms of certain crucial intertemporal choices (albeit real wage signals concerning labour-leisure substitution opportunities, rather than interest rate signals about consumption-investment choices) as the key mechanism driving the business cycle; rejects the endogenously adjusting backward looking expectations of the macroeconometric dynamic systems descended from Stockholm style sequence analysis, and substitutes forward looking rational expectations; and provides the basis of policy doctrines every bit as anti-activist as those of the Austrians.

As should be apparent from a comparison of this brief summary of the business cycle analysis of Lucas, Barro, Sargent and Wallace, with that of

Mises, Hayek and Robbins, though the doctrines are by no means identical, they have several features in common. Nor is this an accident. I have already noted that Austrian Business Cycle Theory represented part of a much more broadly based attempt to establish economics as a deductive science based on individualistic maximising premises, and I have also drawn attention to the success of that attempt outside of what we now call macroeconomics. The exponents of new-Classical economics have from the outset been conscious of the tensions in the subject arising from the coexistence of "Keynesian" macroeconomics with essentially Walrasian microeconomics, and have seen their task as being to extend the microeconomics they inherited to a point at which it could supercede Keynesian economics in dealing with macro problems. The purpose of the new-Classicals, that is to say, has been precisely to succeed where the Austrian business cycle theorists of the 1920s and 30s apparently failed.

New-Classical analysis differs in certain respects, sometimes important ones to be sure, from the work of the Austrians, not least in its ability to produce a coherent account of employment fluctuations as a consequence of maximising choices. But, and for the good reasons just sketched out, its overall flavour has been very similar to that of the earlier doctrine. Indeed, this commentator has elsewhere (1982) suggested that "neo-Austrian" is a more accurate label for it than "new-Classical".²³ At this point in the development of our subject, it is hard to say how much of this work will prove to be of lasting value. If the claims of its proponents about its inherent superiority are justified, though, and if the demise of the post-war Keynesian consensus about macroeconomics proves permanent, then far from having been rejected, Austrian notions will after all have survived to be absorbed into

the mainstream of the discipline, and it will be the ideas of the Stockholm School that will be seen to have suffered wholesale abandonment.

Perhaps, however, on a longer view, the "hunted hare" (to borrow from an apt metaphor from Sir Dennis Robertson) of analytic fashion will continue to run in circles. If it does, then the "failure" of the economic ideas discussed in this essay will turn out to be, like treason, merely a matter of dates.

FOOTNOTES

¹Both Siven (1986), in describing the outcome of the Stockholm School's efforts, and Hicks (1967), in describing the disappearance of Austrian Business Cycle theory from the scene, without using the word "failure", nevertheless tell stories to which it surely could be applied. In the case of Austrian theory it might also be noted that Shackle's (1967) *Years of High Theory* which deals with "Invention and Tradition in Economic Thought 1926-39" mentions Mises not at all, Robbins once (as an opponent of the Marshallian idea of the "representative firm"), and Hayek twice (in his capacity as the editor of the German version of Myrdal's *Monetary Equilibrium!*).

²Indeed Leijonhufvud (1981) has aptly referred to a "Wicksell Connection" in the development of macroeconomics, for his influence on Keynes, though perhaps indirect, is marked. The influence is most notable in the *Treatise*, but is clearly present in the *General Theory* too, as Leijonhufvud (1967) has shown. Nor, I think can it be argued that the influence is just a matter of appearances. Richard Kahn was translating *Interest and Prices* while commenting on Keynes' drafts of the *General Theory*, and Keynes himself refereed Claassen's translation of the *Lectures* for Macmillan in 1932. See Keynes (1983) pp. 862-865.

³Wicksell's work is analysed in detail by Patinkin (1965), Laidler (1972), and Hansson (1985), who pays particular attention to the problematic nature of Wicksell's monetary equilibrium concept and its relationship to the relative vagueness of his later expositions of the "cumulative process".

⁴The extension of the Walrasian vision to an intertemporal choice framework was accomplished by Hayek in his (1928) article "Intertemporal Price Equilibrium and Movements in the Value of Money" (reprinted 1984) a piece much cited by the Stockholm School as well as the Austrians.

⁵ Indeed Mises would have no truck with the concept at all, regarding it as essentially meaningless. He nevertheless frequently discussed the influence of the quantity of money on the "value of money", thus creating considerable confusion for modern readers. However, by "value of money" Mises meant something akin to the marginal utility of the services of a unit of nominal balances, not the inverse of a price index. That marginal utility, in turn, was, to use Hicks' (1935) phrase, "the ghost" of the marginal utility of whatever commodity had served as money at some time in the past.

⁶ The concept of forced saving has a long history in monetary economics, going all the way back to Cantillon (1734). Hayek (1932) is still the definitive study of the history of this idea.

⁷ See Hayek (1939) pp. 62 et seq. for an example of such discussions.

⁸ And the Austrians were very conscious of the historical origins of their ideas. Hayek's work on the history of forced saving has already been alluded to, and his introductory essay to Henry Thornton's *Paper Credit*. . . , still the standard reference, was also a product of the 1930s. Lionel Robbins' prowess as a teacher of the history of economic thought verges on the legendary. Nor were Hayek and Robbins alone in finding support for Austrian analysis in earlier work. One of their colleagues at the LSE, T. E. (later Sir Theodore) Gregory went so far in his (1928) introduction to Tooke and Newmarch's *History of Prices* to refer to the Currency School as "the elect". The use of Calvinist vocabulary to characterise favourably the originators of an economic doctrine is of some interest in the light of Myrdal's comments quoted below, p. 29.

⁹ It should be noted explicitly that the "accelerationism" of the foregoing passage, and the many others like it that occur in Austrian writings, in no way anticipates the accelerationism of Friedman (1968) or

Phelps (1967). It stems from the argument that, to enable forced saving to continue at its original rate, the price of producer's goods must remain at a constant level relative to consumer goods. With the price level of the latter rising, this will require a steady increase in the size of the injection of nominal bank credit to sustain the relative price distortion in question. Austrian analysis paid no formal attention to the effects of inflation expectations on the time path of money wages and prices, which lie at the heart of modern theories of accelerating inflation. However, in less formal analysis, something more akin to modern accelerationism driven by expectations effects, indeed rational expectations effects, does appear. Consider the following quotation from Mises (1932):

"A nation which has experienced inflation till its final breakdown will not submit to a second experiment . . . until the memory of the previous one faded . . . Made overcautious by what they suffered, at the very outset of the inflation they would start a panic. The rise of prices would be out of all proportion to the increase in the quantity of paper money; it would anticipate the expected increase of notes." (p. 233).

¹⁰ A modern reader would argue that the rate of growth of the nominal money supply should make no difference here, provided that inflation expectations adjusted so that real variables' values would be independent of the time path of the nominal money supply. He would be right to raise this point, though a latter day Austrian could perhaps counter by arguing that anticipated deflation would distribute new real balances in the community in proportion to existing holdings and hence would be neutral, whereas adding to real balances by creating nominal balances at a constant price level would lead to their initial distribution being determined by the pattern of bank lending, and hence being non-neutral. Hayek does at one point (1931-2) (1936) p. 97 entertain the possibility of avoiding a crisis, at least in principle, by engineering a time path for bank credit that would maintain the "right"

ratio of prices between producers' and consumers' goods, but dismisses the theoretical possibility as impractical. However, anticipations about the behaviour of prices play no role in conditioning the behaviour of agents in the formal Austrian model, as has already been noted above (fn. 9). This is one of its great weaknesses, both in comparison with modern work but, as we shall see and more to the point, with the work of the Stockholm School too.

Underlying the "non-neutrality" of monetary injections discussed here was a very mechanical vision of the price formation process, in which prices are determined so as to equate the value of a flow supply of output to the flow of money expenditures directed at it. The considerable attention that Hayek pays in *Prices and Production* to the effects of vertical integration of industries on the interaction of money and prices is also related to this vision, which the Austrians seem to have inherited from English Classical Economics. See Laidler 1987 (forthcoming) for a discussion of this process of price formation as it was viewed the 1870s.

¹¹To hinge a theory of the business cycle on a variable reserve-deposit ratio, on the proclivity of banks to make loans to firms but not consumers, and on the short-run non-neutralities arising therefrom, will strike the modern reader as rendering it heavily dependent on particular institutional assumptions, but that is what Hayek did.

¹²Though not all the Stockholm School shared Myrdal's contempt for marginal utility theory. (See Myrdal (1932 (1939) p. 4) for an expression of this attitude.)

¹³I do not mean to imply here that the Austrians were innocent of the difficulties involved in the concept of an aggregate capital stock. Hayek (1941) provides ample evidence to the contrary. However, when it mattered, in the early 1930s, the Austrians did anchor their vision of monetary equilibrium

to a technically determined natural rate of interest and their vision was, therefore, deeply flawed. The Stockholm School avoided this particular trap.

¹⁴Hansson (1982) credits Hammersjold with producing the first sequence analysis in 1933, and Lindahl with having ironed out certain remaining problems with Hammersjold's work in 1934. In the early 1930s, Myrdal and Ohlin concentrated on developing the within period ex-ante-ex-post distinction, and Lindahl on developing the links between periods in terms of his influential "temporary equilibrium" concept.

¹⁵It is also worth noting that the formal analysis that underlies Lindahl's conclusions involved linking a series of temporary static equilibria with changing expectations. He did not analyse the convergence of variables from their ex ante to ex post values within the period for which the equilibrium was determined. Hence his conclusions did not come from a full "sequence analysis". In (1939) Lindahl comments explicitly that to apply a more elaborate dynamic method to these particular cases did not seem to change any conclusions. See pp. 261 et seq.

¹⁶For examples of some of these variations see, Myrdal (1932 (1939)) pp. 150-156, Lindahl (1939) pp. 164-170, 236-239. Note that Lundberg (1937) Chs. 3, provides a valuable survey of the various dynamic sequences which some of his predecessors and contemporaries, not all members of the Stockholm School, had analysed.

¹⁷Myrdal was, though, something of a methodological extremist in his own right, being particularly anxious to stress the ethical and downplay the scientific element in discussions of economic policy. For a comparison of Myrdal and Robbins views on this and related issues, see Hicks (1983).

¹⁸It is also worth noting explicitly that the Stockholm School's policy pragmatism permitted them to offer concrete advice to the government of

a particular open economy about economic policy. The contrast between this characteristic of their work and Hayek's view that policy to maintain monetary equilibrium had to be conducted on a worldwide basis is, to say the least, striking. Jonung (1981) gives an account of the practical application of economic ideas in the conduct of Swedish economic policy during the depression.

¹⁹This problem is specifically addressed by Siven (1986) and the reader familiar with this essay will recognise its influence on the arguments advanced here. The reader is warned that I do not intend here to give the impression that influences between Sweden and the rest of the world ran in only one direction during the 1930s. If Keynes did not read the Swedes in the 1920s and early 1930s, they certainly read him, and they also read Dennis Robertson whose development of business cycle theory ran parallel to Swedish work in many important respects, though he himself was unaware of this until the 1930s. Moreover, the Swedish economists paid careful attention to American, German and Austrian literature too.

²⁰And, it might be pointed out, Roy Harrod, who of all British economists remained most faithful to the *General Theory* as the source of knowledge about short run macroeconomics left just this matter equally unclear in the (1969) textbook which he based on his undergraduate lectures on monetary topics. See pp. 166 et seq.

²¹In denying the stability of the consumption function, and the usefulness of the multiplier concept, Ohlin provides powerful evidence against the claim that the Stockholm School anticipated the central analytic contributions of the *General Theory*, which hinge on just these two matters. I hasten to add that there is no implication here that that Keynes was right on these matters and Ohlin wrong. Indeed, with benefit of the last fifty years of economics, it would be easier to defend the contrary argument.

²²It is worth pointing out that Sir John Hicks, whose work has left such a deep mark on macroeconomics was, from the very outset of his own work, a careful reader of the output of the Stockholm School and that his writings, including those dating from before the publication of the *General Theory* have provided an important source of information about Swedish ideas for English speaking economists.

²³And since my (1982) commentary was written, there has emerged a new variation in new-Classical theory, namely "real" business cycle theory, stemming from the work of Kydland and Prescott (1982). The fundamental building block of their work is an aggregate production function, exogenous shocks to which are thought of as being the prime cause of the business cycle. The affinity of a model, which puts the physical conditions of production at the centre of an explanation of the cycle, to Austrian work is obvious enough. So too ought to be the flimsiness of its microeconomic foundations, given the long understood difficulties in defining an aggregate production function discussed above pp. 18-19. On this matter, see also Blaug (1985) Ch. 12.

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