

## **What was lost with IS-LM\***

by

Roger E. Backhouse  
*University of Birmingham*

and

David Laidler  
*University of Western Ontario*

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### **1. Introduction**

The dominance of the IS-LM model in the macroeconomics of the third quarter of the twentieth century resulted in important ideas becoming lost from the sub-discipline, or at least pushed into its background. Some of these losses were temporary and have already been made good, but others were longer lasting. The fact that IS-LM became the basis of what James Tobin called “the trained intuition” of the great majority of economists working in the area had important effects, both positive and negative, on the subsequent evolution of macroeconomics.<sup>1</sup> Ideas that were easy to formulate as applications or extensions of this framework flourished, and lines of enquiry that were harder to fit into it atrophied. This is not to say that other approaches vanished without trace. Perry Mehrling (1998, 2001) has reminded us that Edward Shaw, who was trained before IS-LM took hold on the undergraduate and graduate curricula, kept an earlier American Institutionalist tradition alive, and Milton Friedman too preserved an developed ideas that he had learned before 1936. Though their work certainly formed part of the profession’s discourse in the 1950s and ‘60s when the influence of IS-LM was at its height, it was very much a minority taste at that time.

Many commentators have pointed out that important elements of the *General Theory* itself were lost with IS-LM, a judgement that is well summed up in Joan Robinson’s famous gibe at this framework as “bastard Keynesianism”. There is much to be said for such arguments, but the publication of the *General Theory*, and the development of IS-LM as a means of conveying what many took to be its central message, can also be seen as representing two stages in a longer process whereby a number of insights and questions that had been prominent in monetary and business cycle theory before 1936 were pushed off economists’ agenda. The ideas in question all had a common element. From the late 1930s onwards, the discipline began to lose sight of the fact that economic activity takes place in time. Neglected in IS-LM, only some of the implications of this fact have even now re-appeared, and those that have done so re-emerged only gradually, and in an intellectual climate very different from that of the 1930s.

## **2. The Keynesian revolution and th rise of static equilibrium macroeconomic theory**

The IS-LM model was the main product of the Keynesian revolution. It provided the lens through which economists, from the 1940s up to at least the 1960s, viewed not only Keynesian economics but the theories that went before. If the “classical economics” described in Keynes’s *General Theory*, the IS-M model and post-war macroeconomics textbooks had been an accurate and complete summary of the pre-1936 literature, this paper would hardly need to be written.<sup>2</sup> The story of the Keynesian Revolution and the role of IS-LM in it would be would be both simple and already well known. However, the development of macroeconomics before 1936 was both more complicated and more interesting than is generally appreciated, and the intellectual revolution that began in that year marked the climax of over a quarter of a century of intensive theorizing about the causes and structure of the business cycle. This work followed a wide

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<sup>1</sup>See Tobin (1982) as quoted by Robert Dimand (2003)

<sup>2</sup>And many history of economic thought textbooks (eg the first four editions of Blaug 1962).

variety of paths, by no means all of which led in the same direction. What happened after 1936 was that an important and rather specific sub-set of the ideas that had been developed by a large community of economists during the 1920s and 1930s were summed up and formalized, first in the *General Theory* and then in a small number of models, of which the IS-LM model was by far the most important.<sup>3</sup>

The simplest way to establish this point is to recall: first, that Keynes himself argued that his theory rested on three relationships, the propensity to consume, the marginal efficiency of capital, and the liquidity preference function, which in turn interacted to determine the level of effective demand; and second, that none of these ideas was original to the *General Theory*. The idea of a propensity to consume arose out of work in the 1920s in which economists began to separate and define the concepts of saving and investment, and Warming's (1932) deployment of the idea is identical to that used by Keynes in 1936. The marginal efficiency of capital, whose crucial feature is that the rate of return on investment is determined by forward-looking expectations, rather than by some physical productivity of capital, was simply Irving Fisher's (1907) rate of return over cost under another name. The concept of liquidity preference derived from Lavington's (1921) extension of the Cambridge theory of the demand for money, and had already been developed in detail in Keynes's *Treatise on Money*. As to the concept of effective demand, this had been deployed, under that name, by Ralph Hawtrey as early as (1913), and had played a central role in his work thereafter.

It was no accident that these concepts had evolved before 1936. Monetary theory had been shifting away from analysing the quantity of money's effects on the price level, and towards understanding the influence of the rate of interest on saving and investment, at least since the appearance of Wicksell's cumulative process analysis in (1898). Subsequent developments as different from each other as Austrian business-cycle theory and Swedish dynamic process analysis can be traced directly to this common origin, while Wicksell's work was certainly known to Irving Fisher (see 1911, pp. 59-60), and exerted a considerable and explicitly acknowledged influence on *The Treatise on Money*.<sup>4</sup>

When the *General Theory*, and a little later IS-LM, synthesized and formalized earlier developments to create a "Keynesian revolution", some ideas were simplified and others, that had once seemed important, simply dropped from sight, in a process that was far from random.

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<sup>3</sup>Other important macroeconomic models that also originated at about the same time, for example the multiplier–accelerator business-cycle model and the Harrod-Domar growth model, but, remained. in rather specialised niches, rather than becoming common knowledge among all those who studied it. We discuss this point at greater length below.

<sup>4</sup>Wicksell's earlier influence on English monetary economics is problematic. A paper setting out the essential features of his "cumulative process" appeared in the *Economic Journal* in (1907), but such important contributors as Hawtrey and Dennis Robertson did not cite him in the 1920s, and subsequently claimed that they were unaware of his work at that time.

This was not, however, a matter of some ideas having survived contact with empirical evidence and others having been discredited by it. The major factor at work was that, as economists came increasingly to use formal models to analyze macroeconomic questions, ideas that could be fitted into them flourished, while those that could not began to seem unimportant. Crucially, the IS-LM model was a comparative-static device, and this meant that a wide range of issues, related to the fact that economic activity happens in time, simply could not find a place within it. Only ideas that seemed relevant to the equilibrium features of the economic system and its comparative-static properties received careful attention from those who used IS-LM, but that was the vast majority of economists working on macroeconomic issues between the 1940s and the 1970s.

It is uncontroversial that the *General Theory* contained many arguments, about money-wage movements, the role of expectations, the interest inelasticity of investment demand, own-rates of interest, capital satiation, the falling rate of profit, and so on, that could not be brought into heart of the IS-LM model.<sup>5</sup> It selected and formalised only those parts of the book that could be fitted into a static equilibrium framework, which could in turn be manipulated to derive propositions believed to be characteristically “Keynesian” and “classical”. However, it is argued here that the loss of ideas from the broader inter-war literature on monetary economics and the business cycle, which began with the *General Theory* itself, and was accentuated by the development of IS-LM, was more important for the subsequent development of macroeconomics than the mislaying of some insights from that book during the transformation of “Keynesian economics” into IS-LM. When all is said and done, IS-LM did sum up a critical and central subset of the ideas expounded in the *General Theory*. This is why it is legitimate to refer to a Keynesian rather than a Hicksian revolution.

In *The General Theory*, Keynes not only expounded his own ideas. He also re-wrote the macroeconomics that had come before, and labelled the result “classical economics”. In the process he presented a theoretical critique of propositions that were entirely static and atemporal. In the absence of this distortion of what macroeconomics was like before the *General Theory*, Keynes’ own contribution would have been perceived very differently, and it would have been much more difficult for such early exponents of IS-LM as Harrod (1937) and Hicks (1937) to present this model as capable of capturing the fundamental issues at stake between Keynes and his predecessors. As it was, none of the functional relationships that lay at the heart of IS-LM, in either its “Keynesian” or its “classical” form, explicitly modelled issues having to do with time, nor did the framework itself permit time to be brought into the analysis of their interaction. In that model the implications of economic activity taking place in time were all in the background, and those whose intuitions were trained on its manipulation came to view the macroeconomics that had existed before 1936 as being just as amenable to characterisation in comparative static

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<sup>5</sup>Note that we are here discussing what could be incorporated into the formal geometric and algebraic structures upon which the intuitions of future generations of economists were trained. We do not intend to deny that these ideas often found a place in the verbal discussions with which the textbooks of the 1950s onwards accompanied their formal analysis.

terms as Keynes' own

Some examples will help to make clear just how many ideas were thus rendered inaccessible to a younger generation of economists. Hawtrey, following a tradition that dated back at least to the Currency-School - Banking School controversies of the 1830s and '40s, had argued that the monetary transmission mechanism over the course of the cycle involved the continual creation and destruction of credit. For him, and for Irving Fisher too, not to mention the many economists whom these two influenced, the interest rate set by the banking system was therefore considered to be a critical variable for swings in the price level, and in output too for Hawtrey, who placed increasing emphasis of wage price stickiness in the 1920s and '30s. Others, notably the Austrian and Swedish followers of Wicksell, but also Dennis Robertson, carried this line of analysis further, and stressed the capacity of interest rate swings to generate variations in output which often involved "forced saving". The Austrians in particular had argued that, in a world experiencing change, the market mechanisms that existed in a monetary economy were likely to fail in fully coordinating the choices of individual agents, notably with regard to the allocation of resources over time; this because the banking system's activities would interfere with the interest rate's capacity to induce a time-structure of production that was compatible with households' plans for future consumption. Those Swedish economists known as the "Stockholm School" focussed on the processes whereby *ex ante* disequilibria were turned into *ex post* equilibria. They therefore paid careful attention to the importance for macroeconomic behaviour of expectations and their evolution over time. And so on.

The neglect of ideas such as these under the influence of IS-LM had profound effects not only on macroeconomic theory, but also on economists' conceptions macroeconomic policy, which came to be analysed in terms of comparative static diagrams. Monetary policy, for example, began to be seen in terms of discretionary shifts in variables, designed to offset the effects of disturbances to the private sector of the economy. These disturbances were typically characterised as once and for all shifts in the investment function that underlay the IS curve, to which an *ad hoc* offsetting shift in the LM curve was the appropriate monetary response. As they became accustomed to thinking in these terms, economists began to lose sight of the idea that in the actual economy, such measures would always be deployed in the context of a specific policy regime, and that the choice of the regime itself might affect both the nature of the shocks hitting the economy, the menu of responses available to the authorities, not to mention the transmission mechanism through which the private sector would respond to policy.

It might seem paradoxical to argue that macroeconomics began to neglect time, just when economists were starting to construct formal dynamic models of economic growth and were learning how to deploy systematically the interaction of the multiplier and the accelerator to analyse the business cycle. However, though these strands of thought did not disappear from the literature under the weight of IS-LM's influence, they were marginalised. They preserved some aspects of economists' earlier concerns with economic processes that happened in time, but where those concerns had once been of the very essence of macroeconomics, they now seemed to be relevant only to the specialized topics of growth and the cycle, and these were treated as distinct from the main body of macroeconomics. By the 1960s, students would be taught about

the cycle, and some of them about growth too, but these were specialist topics that could be neglected most of the time.<sup>6</sup>

In the same way, economists never altogether forgot that consumption and investment decisions had something to do with the allocation of resources over time, and by the 1960s they were once again becoming aware that they might fruitfully be analysed, along lines pioneered by Irving Fisher (1907), as the outcome of maximizing inter-temporal choices. Thus theories of the consumption and investment functions began to acquire well articulated microeconomic foundations that involved paying explicit attention to expectations. Even so, the proper macroeconomic application of such ideas was still thought of as being the derivation of essentially static consumption and investment functions, for example a consumption function with permanent income or wealth as an argument instead of current income.<sup>7</sup> No doubt these relationships were better formulated than the ones they replaced, but the role of time in their derivation played only a minor role in their applications to macroeconomic theory. In empirical work, time became more important however, since these theories provided a rationale for the use of distributed lags in econometric estimation.

In what follows, we shall discuss four overlapping sets of problems associated with the passage of time from which IS-LM distracted attention. The first involves the simplest kind of dynamics: economic changes take time to happen, which means that variables need to be dated and that adjustment paths between equilibria need to be examined. The second arises because current decisions are always taken with reference to some time-horizon or other, which means that expectations are always relevant when maximizing behaviour is analysed. The third is significant because economic policy too involves forward-looking decisions, and this insight suggests that policy is better thought of in terms regimes and how they function, rather than as a series isolated interventions whose transmission mechanisms might be of interest. Finally, to take explicit note of the temporal dimension inherent in all aspects of economic activity draws attention to important coordination problems, having to do with the allocation of resources over time, that are invisible in a static world.

All of these matters had received widespread attention in the interwar literature, but the wholesale adoption of the static IS-LM framework from the 1940s onwards led to them falling

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<sup>6</sup>It is worth noting that, even with business cycle theory, a process of formalization and forgetting took place. Despite the bewilderingly wide variety of approaches that had existed in the inter-war literature, by the 1950s and 1960s, the phrase “business cycle theory” had become almost synonymous with multiplier-accelerator interaction and the models to which students were exposed were almost invariably ones whose properties could be understood by analysing a second- or higher-order difference equation.

<sup>7</sup>The work on the consumption function that we have in mind here is, of course, that of Friedman (1957) and Modigliani and Brumberg (1954) On investment, See, for example Dale Jorgenson (1967)

into neglect. When some of these problems were rediscovered, they were approached using theories whose own development had been profoundly affected by the dominance of IS-LM, as we shall now see; and, as we shall also see, not all of them have yet reclaimed a place on the agenda of modern macroeconomics.

### 3. The loss and rediscovery of dynamics

IS-M is a static model in the simplest sense of the term. Variables are not dated and lags are not modelled formally. They are arguably implicit in those stories about the stability of the IS-LM equilibrium that Victoria Chick (1973) felicitously labelled “the pseudo-dynamics of IS-LM”, but even here the dynamic structure of the world plays no role in determining the equilibrium towards which the economy converges, in the delineation of differences between alternative theoretical positions, or in the analysis of policy choices. This is a major reason why wage rigidity seemed to play a crucial role in explaining persistent unemployment, and also, perhaps why monetary theory and the theory of inflation became estranged from one another in the 1950s. Economist knew, of course, that wages and prices did change, but in an IS-LM framework such change had to be viewed, formally speaking, as an exogenous factor shifting the LM curve, not as an endogenous response to anything, even to variations in the nominal quantity of money, which themselves also appear simply as exogenous influences that shift the LM curve..

There is no doubt that the static nature of IS-LM constitutes a considerable simplification of the analysis presented Keynes’s *General Theory*. As an example, consider Keynes’s discussion (in chapter 19) of how cuts in money wages might raise employment. Some of the mechanisms he describes there can be fitted into a static equilibrium framework (the effects of lower money wages on the demand for money and on distribution) but others make sense only in a dynamic setting (the reduction of present money wages relative to money wages expected in future; the possibility that a general reduction in wages might be mistaken for a reduction specific to individual firms). Even so, not too much should be made of these dynamic elements in Keynes’s account in the current context, because, although he made many remarks that could be (and in some cases were later) turned into dynamic models, the emphasis of the *General Theory* was nevertheless on unemployment as an *equilibrium* phenomenon. Dynamic accounts of how money wages might affect employment were only a little more integrated into Keynes’s formal analysis than they later were into IS-LM.

Far more significant for the development of macroeconomics than IS-LM’s neglect of the dynamic element in Keynes’s thought is the way in which Keynes himself systematically neglected dynamic factors that had been discussed in previous explanations of unemployment. This was a feature of the *General Theory* remarked upon by Ohlin (1937, pp. 235–6):

Keynes’s theoretical system ... is equally ‘old fashioned’ in the second respect which characterizes recent economic theory – namely, the attempt to break away from an explanation of economic events by means of orthodox equilibrium constructions. No other analysis of trade fluctuations in recent years – with the possible exception of the Mises–Hayek school – follows such conservative lines in this respect. In fact, Keynes is

much more an ‘equilibrium theorist’ than such economists as Cassel and, I think, Marshall. (Ohlin, 1937, pp. 235–6).

Another way of making the same point is to observe that though the 1920s in particular generated a substantial literature on the microeconomics of unemployment that used Marshallian comparative statics as its principal tool, in the pre-1936 macroeconomics literature, with some notable exceptions such as Pigou (1933), aggregate employment was analysed specifically in the context of the cycle, with shocks, lags and dynamic adjustment processes being the order of the day.<sup>8</sup> As Ohlin pointed out, such ideas were pervasive in macroeconomics before 1936 – so pervasive that it is possible to provide no more than a few illustrations of the point in this paper.

The clearest example of the use of explicitly dynamic analysis before 1936 was to be found in the work of the Stockholm School, which formed the basis of Ohlin’s critique of Keynes. As Bjorn Hansson (1982) has shown, this group developed an explicit method, based on the idea of a succession of “unit periods”, in which each period began with agents having plans based on newly formed expectations about the outcome of executing them, and ended with the economy in some new situation which was the outcome of market processes set in motion by the incompatibility of those plans, and in which expectations had been reformulated too in the light of experience. They applied this method to the construction of a wide variety of what they called “model sequences”, many of which involved downward spirals in economic activity at whose very heart lay rising unemployment. This is not the place to discuss the vexed question of the extent to which some of this work anticipated the Keynesian multiplier process, but it should be noted that, in IS-LM, it is the limit to which such processes move, rather than the time path they follow to get there, which is emphasized.

The Swedes may have taken explicitly dynamic analysis further than anyone else, but they were far from alone in putting it at the centre of their endeavours. Ralph Hawtrey, one of the most influential cycle theorists of the 1920s, made dynamics central to his explanation of how unemployment emerged (Backhouse, 1985, pp. 184–6; Laidler 1999, pp. 112–18). Though he started his analysis from the quantity theory of money, he nevertheless criticized it for emphasizing an equilibrium relationship between money and prices that did not apply when the money supply was changing. Rather than analysing the demand and supply of money as static schedules, therefore, he emphasized the continual creation and repayment of credit (bank deposits) as banks and firms interacted over time. New loans were taken out in order to be spent, generating income in a process that involved purchasing power returning to the banks to cancel old loans and constantly being returned to circulation as new loans were created.

Explicit, albeit informal, dynamics were central to Hawtrey’s story of how a cycle emerged: the process of credit creation was unstable; there were lags between credit creation and spending; between the credit-financed investment in inventories and the production of goods to meet that investment; and, at turning points in the cycle, between variations in output and money

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<sup>8</sup>See Laidler (1999, Ch. 7) for a brief survey of this literature



wages, the non-bank public's demand for currency, and the banking system's interest rate response. Hawtrey relied on wage and price stickiness to explain why fluctuations in effective demand brought about by swings in the money supply caused fluctuations in employment. Because he did not allow for the possibility that velocity might vary systematically with the nominal interest rate, he argued that expansionary fiscal policy not financed by money creation would be ineffective in countering unemployment, and this particular idea can, *ex post*, be captured in a vertical LM curve special case of IS-LM.<sup>9</sup> Hawtrey himself, however, always explained fluctuations in effective demand as manifestations of the workings of an inherently dynamic system, and these essential features of his analysis could not be captured by this model. Virtually all of the subtlety of Hawtrey's story lay in its systematic analysis of what can be thought of as the factors that would keep the LM curve moving back and forth in an ongoing cycle, and such swings can only be treated as exogenous shifts in the IS-LM model.

Robertson's analysis of the cycle was also intrinsically dynamic, and it may, in the 1920s, have provided some inspiration to the Swedes as they began their own work. In *Industrial Fluctuation* (1915) he proposed a real theory of the cycle. Innovations could start an upswing in business activity which, because of the lag between investment and the resulting increase in output, would eventually lead to a surplus of specific capital goods and a shortage of saving. This emphasis on real dynamics was part of a tradition going back at least to Marx, about which Robertson seems to have learned from his reading of Aftalion (1913) and Tugan-Baranowsky (1913). When he later (eg. Robertson 1926) extended his analysis to take account of monetary factors, he continued to pay close attention to dynamics. Because consumers build up cash balances by spending less than they receive as income, an increase in the transactions demand for money that arises in the process of a real expansion will cause expenditure to lag behind income, with consequent effects on the time path of aggregate demand. This effect, which is also relevant to the process whereby the economy adjusts to an increase in the money supply, is missing from the standard static IS-LM model, though it was later revealed by the Phillips machine, whose very nature prevented questions about the economy's time path between equilibria from being evaded. (Leeson 2000, pp. 45, 48–9).

Economists of the IS-LM generations were, of course, aware of time-lags. In the 1950s and 1960s it was part of an economist's specialised training to learn about the various lags that might be brought in when creating a theory of the cycle or an empirical model. There were also formal dynamic analyses of the stability of the equilibria that underlay the comparative statics of income determination, such as are present in Patinkin's *Money, Interest and Prices* (1956, 1965). Though it was somewhat broader, Patinkin's model shared many features with IS-LM, but in his work dynamics remained peripheral to what was perceived to be the core of macroeconomics. It described explicit adjustment paths around static equilibria, but it was the properties of the latter which remained central to the theory. Patinkin analysed a limited range of stock-flow links and the *tâtonnement* process, and, at its heart, his system remained comparative static. The lags that had played such an important role in pre-Keynesian theory were largely absent from it, and when

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<sup>9</sup>As indeed Hicks noted in 1937, fn 4, with its oblique reference to the "Treasury view".

Patinkin went so far as to conclude that, formally speaking, unemployment was a disequilibrium phenomenon when viewed in the context of the system in question, he was considered by many Keynesians to have sold out to ‘classical economics’.<sup>10</sup>

The extent to which dynamics had become marginalised and effectively forgotten by a profession accustomed to thinking in terms of IS-LM is well illustrated by the dispute between Friedman and the Keynesians in the 1960s and early 1970s. In his attempt to reinstate the quantity theory of money, Friedman, who had been trained before the *General Theory* or IS-LM came upon the scene, chose to focus on the dynamic effects of monetary changes on money income. He refused to play the static game laid down by ‘Keynes and the classics’ and the “neoclassical synthesis”, and he was accused by most of his colleagues, not of having an erroneous theory, but of having no theory at all to justify his claims about the important role played by the quantity of money in the economy. He has failed, or so it was said, to provide a satisfactory account of the transmission mechanism whereby money had its effects.

Rather late in the day, Friedman tried to meet this criticism. In his ‘Theoretical framework for monetary analysis’ (Gordon, 1974), he deployed an IS-LM framework to explain the differences among Keynesian economics, the simple quantity theory, and the sophisticated quantity theory which he espoused. The first two were described in purely static terms, but the sophisticated quantity theory was described as involving a lagged adjustment of nominal income to changes in the money supply, and as being short of one equation, which was needed to determine how nominal income variations were broken down over time between output and the price level. This may have been an inadequate summary of the pre - IS-LM literature on inflation and the cycle that had started from the quantity theory, containing as it did no explanation of the form such lags should take, or why they should take that form, but it could be seen as echoing ideas from the earlier literature. And, to be fair to Friedman, a more elaborate exposition of his views on the dynamics of the interaction of money, real income and prices had already been presented by Friedman and Schwartz (1963b).<sup>11</sup>

Nevertheless, to economists trained on IS-LM, where the central problem was the determination of a static equilibrium, Friedman had either revealed himself as having the same theory as them, if they concentrated on the first part of his 1974 framework, and hence as having nothing to teach them, or he had yet again failed to answer their questions about the theoretical basis for his claims. What they seemed to want from him was a model that differed from IS-LM

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<sup>10</sup>The reader’s attention is drawn to our use of inverted commas here. The classical economics that these critics had in mind would more accurately be described as Keynes’s caricature of that body of doctrine.

<sup>11</sup>This aspect of Friedman’s work is described in more detail by Bordo and Schwartz (2003) and Nelson (2003). Both papers stress the inadequacy, from a monetarist standpoint, of IS-LM based accounts of monetary policy’s transmission mechanisms that run exclusively through a narrow range of market interest rates,

but remained comparative static in nature, but what they got was an account of monetary dynamics that had much in common with the inter-war analysis of the interactions among money, inflation, the cycle and crucially, expectations, that IS-LM had temporarily superceded.

The centrality of dynamics for macroeconomics was arguably ‘rediscovered’ by the majority of economists working on macro theory only later in the 1970s when they became concerned with inflation and had to confront the abovementioned interactions. For example, they were forced to deal with the co-existence of high inflation and high interest rates and turned to the Fisher effect to do so. This effect, a commonplace of the pre-1936 literature, relied on the influence of inflation expectations on nominal interest rates, and the influence of actual inflation on those expectations, and both of these factors were absent from the static monetary theory that underlay the LM curve. Once the importance of the Fisher effect was recognised, it became clear that a real rate of interest was relevant to the IS curve, but a nominal rate to the LM curve; and because inflation expectations thus had to become part of the IS-LM model, it also became harder to preserve it as a fundamentally static construction.<sup>12</sup>

However, the real break-through in dynamizing IS-LM came when it was supplemented by a Phillips curve augmented with endogenous inflation expectations. The original Phillips curve, which had the advantage that it could be reinterpreted as deriving from the familiar *tatônnement* process (Lipsey, 1960), had sometimes been used ‘to close’ IS-LM models from the 1960s onwards (Lipsey 2000, pp. 61–3). In this form, however, it was a dynamic adjunct to an essentially static system, and indeed, with benefit of hindsight, it amounted to little more than a particular way of dealing with the price level changes that underlay Patinkin’s real balance effect. By the 1970s, though, the co-existence of rising unemployment with high inflation began to pose an empirical problem that, it turned out, could be addressed using Friedman (1968) and Phelps’s (1967) extensions of the Phillips curve to accommodate endogenous inflation expectations and the idea of an equilibrium unemployment rate whose value was independent of the conduct of monetary policy. This relationship was quickly recognised as providing one solution to Friedman’s “missing equation” problem (though Friedman himself seems to have remained sceptical about this application), and its addition to the IS-LM model turned out to be sufficient to render the whole system properly dynamic.

#### **4. The loss and rediscovery of inter-temporal choice and expectations**

Decisions to save and invest are clearly about the allocation of resources over time, for they concern the timing of consumption and production. However, this inter-temporal dimension of aggregate demand was neglected in IS-LM analysis, and also in the *General Theory*. The marginal propensity to consume was presented by Keynes as ‘a fundamental psychological law’ whose foundations, however, he did not investigate, and in practice he treated it as a behavioral regularity that could be justified empirically. Similarly, though all economists understood that the marginal efficiency of capital depended on expectations, and though Keynes himself,

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<sup>12</sup>Needless to say, Friedman’s dynamic framework had always had ample room for the Fisher effect.

following Pigou (eg.1929) and Lavington (1922) had stressed this factor, under the label “animal spirits” and had emphasized the influence on those spirits of moods of pessimism and optimism, the marginal efficiency of capital was all too often treated in subsequent literature as easily derived from a static production function exhibiting diminishing marginal productivity of capital.

Issues in capital theory that had seemed of great importance in the 1920s and 1930s, when economists were accustomed to treating matters of inter-temporal choice as central, were reduced to side issues, or at least to the status of exogenous shift parameters lying behind the IS curve. Though there were debates about capital theory in the 1950s, these arose not in the context of core short-run macroeconomics but in the context of growth theory, and were largely concerned with the properties of steady state growth paths. Even in this context, formal modelling of inter-temporal choice in consumption was minority taste, most growth models using, despite Ramsey’s example, exogenous propensities to save to anchor their analysis.

Economics rediscovered the analysis of expectations and their bearing upon inter-temporal choice in two stages, the first of which involved attempts to seek better theoretical bases for the individual relationships that underlay the IS curve, and to refine their econometric formulations. The inter-temporal dimension of consumption decisions received a new emphasis with the work of Friedman, and Modigliani and Brumberg in the 1950s. They showed, building on an essentially Fisherian (1907) foundation that it was not possible to understand consumption behavior without taking this matter into account. However, though the permanent income hypothesis and its life-cycle variant became a part of the established curriculum in the 1960s, along with theories of investment that paid attention to adjustment costs, their foundations in the explicit modeling of inter-temporal choice remained peripheral. If one wanted to explain consumption, or to construct a macro-econometric model, the permanent-income - life-cycle theory (or, perhaps more accurately, a lagged dependent variable) was certainly needed, but all too often this requirement seemed to be satisfied by making consumption depend, not on current income alone, but on current income and a geometrically weighted average of lagged incomes. Policy analysis using IS-LM-based econometric models did not seem to require more than this. The same applied to investment: the static IS-LM model required no more than an investment function that sloped downwards with respect to the rate of interest, and in its econometric implementation the addition of a few exogenous shift parameters and, once more, a suitable pattern of distributed lags seemed to suffice to cope with its dynamics. Theories of inter-temporal choice might imply a more complicated investment function, but inasmuch as these did not seem to be needed to improve the performance of econometric relationships, they remained in the background.

An important corollary of this neglect of the explicit analysis of inter-temporal choice was a relative neglect of theorizing about expectations. In the late 1920s, Myrdal and Lindahl (see Lindahl 1929) had begun to struggle with the idea that the current configuration of an economy inhabited by forward looking agents would have to depend upon their expectations about its future configuration. But purely forward-looking expectations seemed impossible to handle, so, as the 1930s progressed, these Swedish economists and their colleagues began to investigate the endogenous evolution of backward looking expectations as an integral part of the dynamic

method whose salient characteristics we have discussed above. Even this approach, however, which had also been Robertson's, led to clumsy and unwieldy analysis. Keynes's "solution" to this problem had been to force expectations into only two categories: long-term, relevant to investment decisions, which he treated as exogenous, and short-term, relevant to firms' output decisions, which he assumed were always correct (c.f. Kregel 1976). This in turn enabled him to reduce the macroeconomic modelling of processes in which expectations were fundamental to a matter of comparative statics. Perhaps Keynes was well aware of what he was doing here, but those who came after him were not always as careful, so his approach to modelling expectations was a crucial step towards their being pushed dangerously far into the background.

Even Hicks, whose concern with expectations in *Value and Capital* (1939) surely derived from his knowledge of the Swedish literature, was one of those who extracted a set of equations from the *General Theory* in which expectations were completely in the background. To be sure, the location of his curves, and shifts of them, could be interpreted as reflecting particular expectations, and changes therein, but there was no necessity to do this.

Until the late 1960s, none of this seemed to present a problem, and even when the Phillips curve was introduced, and economists became aware that wage and price changes might depend on expectations of inflation, this factor was not treated as central to the analysis (e.g. Samuelson and Solow 1960). Indeed Lipsey (2000) has argued that, when he tried to model expectations empirically, he could find no sign of their being important in the data. To cite another example, the general disequilibrium macroeconomics that Clower and Leijonhufvud espoused at the end of the 1960s might have provided the basis of analysis that focussed on the effects of *expected* constraints on sales, perhaps developing into an approach akin to Robertsonian or Swedish dynamics, but it did not: instead, largely in other hands, it analysed the constraints imposed on *current* decisions by *realized* sales and purchases and became an essentially static elaboration of the micro-foundations of a fixed price macro-model that bore more than a passing resemblance to IS-LM.<sup>13</sup>

It was, therefore, left to Friedman (1968) to draw attention to the crucial importance of expectations in macroeconomics, using arguments that were in due course developed by Lucas (eg. 1972) and others. The rediscovery of the role of expectations in macroeconomics then took its second step. Instead of modelling their role in particular relationships in isolation from the system as a whole, as had been done from the 1950s onwards, the New-classicals stressed that agents' decisions about all aspects of their own behaviour would be conditioned by expectations about how the system as a whole was likely to evolve. The analysis of expectations then became linked to the exploration of the macroeconomic implications of assuming the existence of continuously-clearing competitive markets, and "rational expectations" economics came to signify, for many economists, a theoretical package in which the rational expectations hypothesis

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<sup>13</sup>See especially Barro and Grossman (1976) and Malinvaud (1977). For their applications of this analysis to the derivation of the comparative statics of the rate of interest, output and employment, See Barro and Grossman Ch. 3, and Malinvaud, Lecture 2.

itself was only one, and arguably not the most important, component.

## 5. The loss and rediscovery of policy regimes

Alongside the neglect of inter-temporal choice and forward-looking behavior in IS-LM macroeconomics went a specific view of economic fluctuations and economic policy to deal with them. IS-LM encapsulates first, a particular interpretation of what is important about fluctuations, namely, that they originate in real variables, notably investment, and second, a specific view about how to deal with them, namely, by deploying *ad hoc* fiscal and monetary responses. This vision of the macroeconomy is a descendent of that strand in the older literature, associated in particular with Lavington and Pigou, which focused on the role of errors of optimism and pessimism as sources of the cycle, and its policy implications can be regarded as spelling out some of the details of the particular activist policy agenda that usually went with this approach in the 1920s. However, when formulated in IS-LM terms, the significance of the time element in policy design that permeated the earlier literature was lost.

The point here is *not* that policy has to take account of lags and dynamic effects on the stability of the economy's response to policy measures, for there was surprisingly little said about such problems in the pre-*General Theory* literature, even by such exponents of explicit dynamics as Robertson or the Stockholm School; it was left to Phillips (1954) and Friedman (1953) to take up these issues in a serious way in their early and crucial contributions to the re-introduction of explicit dynamics into macroeconomic analysis.<sup>14</sup> Rather, it is that economic activity takes place over time, that shocks are unpredictable and, crucially, that monetary stability over time, including but hardly confined to price level stability, is vital for the efficient working of a market economy.

This way of posing policy issues permeated the literature of the 1920s and 1930s. It inevitably pointed to the importance of questions about the appropriate policy regime and whether, given the 'right' policy regime, a market economy displays inherent stability in the longer run. Keynes himself paid constant attention to such questions. They were central to his analysis of the choice between domestic price level stability and the fixity of the exchange rate in the *Tract on Monetary Reform*, played a role in his analysis of Britain's return to gold in "The Economic Consequences of Mr. Churchill" (1925), lay behind his emphasis on the role of the long rate of interest as an influence on investment behaviour in the *Treatise on Money*, and even appeared in those passages of the *General Theory* that expressed doubts about the desirability of encouraging wage-price flexibility as a means of improving the economy's performance. It is also the case that policy rules were an important element in the American literature, following the establishment of the Federal Reserve System in 1913. Unlike the Bank of England, the Fed. could not draw on any long-established tradition to inform its policy actions and give them

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<sup>14</sup>Unlike Friedman, Phillips did not owe his dynamic vision of macroeconomic issues to a training in economics that pre-dated IS-LM, but to training as an electrical engineer that had conditioned him to think about the problems inherent in trying to control systems in which responses of variables are subject to delays and lags. On all this, see Leeson (2000).

credibility with private sector agents, and economists both inside and outside of the system were fully conscious of the problems this posed. The monetary policy rule proposed in Friedman's *Program for Monetary Stability* (1960) falls squarely into a tradition that runs from Irving Fisher's frequent proposals in the 1920s that the Federal Reserve be subjected to a price stability rule, through Henry Simons' (1933, 1936) advocacy of monetary policy rules.<sup>15</sup>

These proposals were one important element in a lively and continuous debate throughout the period. Pitted against the idea of a price level rule, whose incompatibility with the maintenance of the gold standard Fisher proposed to remedy with his "compensated dollar" scheme, which would see the gold content of the currency varying with the price of gold relative to that of goods in general, were two alternatives. The first of these supplemented a commitment to the gold standard with a version of the "real bills doctrine" that derived from the work of the 19<sup>th</sup> century British Banking School, by way of the work of Fisher's leading intellectual opponent, J. Laurence Laughlin (eg. 1903).<sup>16</sup> The latter doctrine had it that, if the Federal Reserve system followed the practice of providing credit elastically to a banking system that made only good quality short-term commercial loans, it would be doing all that it could to stabilise the monetary system, and elements of it were actually written into the Federal Reserve Act in whose preparation Laughlin's student Henry Parker Willis played a crucial role. The second alternative to a price stability regime was one in which "credit control", what nowadays would be called discretionary stabilisation policy, was to be cautiously undertaken, again against the background of a firm commitment to the gold standard. American advocates of this approach, such as Allyn Young (1927) were heavily influenced by Ralph Hawtrey. Despite the fact that his monetary explanation of the cycle is closest to Friedman's among the inter-war writers, Hawtrey was perhaps the period's leading advocate of such a regime, and even, as the principal author of the 1923 Genoa Resolutions, saw a reformed gold-standard as forming the basis for its implementation on an international scale.<sup>17</sup>

Though the price stability rule attracted considerable academic support, it made little headway in political or policy making circles, where opinion became divided between the other

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<sup>15</sup>The readers' attention is drawn to the fact that the word "rule" was, and still is, used in two distinct senses in the literature we are here discussing: first, to describe a stable rule of thumb that might be adopted by policy makers as the basis for a systematic policy reaction function, and second to describe a legislated rule imposed upon them. In this sense, a "Taylor rule" is by no means an alternative to a "Friedman rule".

<sup>16</sup>Laughlin was the founding head of the Economics Department at the University of Chicago. Fisher's celebrated exposition of the quantity theory, *The Purchasing Power of Money* (1911) may be read as an extended critique of his monetary theory.

<sup>17</sup>Keynes' advocacy of a heavily managed floating exchange rate, coupled with a commitment to domestic price stability, as set out in the *Tract* is a direct response to the Genoa Resolutions, whose origins in Hawtrey's work go back to his pre-world-war 1 book *Good and Bad Trade* (1913)

two alternatives. As is well known, this division of opinion also appeared within the Federal Reserve System, and, in the view of Friedman and Schwartz (1963a) helped to ensure that the monetary policy to the onset of the Great Depression was sluggish, timid and ultimately ineffective.

This paper is not the place to debate the latter point, however. What is important here is that, before IS-LM, monetary policy debates were about alternative regimes, and not just in the US as, for example, Robertson's remarkable (1928) paper on "Theories of Banking Policy" makes so clear. After IS-LM, they were concerned with the likely effectiveness of specific measures that might be taken in the context of an atrophied version of the "credit control" option, and the possibility that fiscal policy, conceived of in terms of *ad hoc* changes in taxes and government expenditures designed to shift the IS curve, might be more reliable. It was only as explicit dynamics began to come back into the picture that the idea of consistent-over-time policies conceived of within a specific regime began to re-emerge as a central concern. At first this re-emergence took the form of discussions of built-in stabilisers and later came to be cast in terms of stochastic versions of the IS-LM framework<sup>18</sup>. After the establishment of rational expectations as a critical element in economic thinking, questions about policy credibility became central, and merged with Friedman's earlier (1960) advocacy of rule-guided policy, to generate the still lively contemporary literature that revolves around inflation targeting and supra-national monetary unions as alternative monetary orders.

Even so, contemporary policy debates in open economies are still frequently cast in terms of the choice between flexible and fixed exchange rates, a habit inherited from the style of short-run open-economy macroeconomics surveyed by Darity and Young (2003). From a longer run perspective, this is deeply flawed approach, since a flexible exchange rate is merely a permissive device that makes it possible for the authorities to choose among a wide variety of domestically oriented policy regimes, and it has no intrinsic merits or drawbacks that exist independently of the regime that is in fact selected. Before IS-LM, economists knew better. For example Marshall (1887) and Keynes (1923) posed the policy choice as lying between domestic monetary stability and exchange rate stability, in the full understanding that to choose the former would require that the exchange rate be permitted to vary. Economists have still not quite recovered a full intuitive grasp of the fact that this is the appropriate way to approach these issues.

## **6. The loss and aborted rediscovery of inter-temporal coordination failures**

When contemporary economists think of the allocation of resources over time, they probably think first of Irving Fisher's (1907) diagram showing how the rate of interest might coordinate the inter-temporal choices of consumers and producers. For Fisher, moreover, the cycle was a "dance of the dollar", and he believed that, were it not for monetary instability originating in the banking system and manifesting itself in price level fluctuations, the allocation of resources over time would present no special problems and that there would be no business cycle. Hawtrey

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<sup>18</sup>See, for example Poole (1970) for a crucial paper dealing with alternative monetary rules within such a framework.



differed from Fisher in understanding that wage and price stickiness could translate the “inherent instability of credit” directly into output fluctuations without major price level swings, but he too largely took it for granted that variations in the rate of interest would suffice to co-ordinate saving and investment at whatever level of output was ruling.

However, Fisher and Hawtrey were in the minority in holding these opinions, for others saw the matter very differently. Cambridge economists, such as Pigou and Lavington, were convinced that markets failed to coordinate saving and investment decisions because of the ubiquity of error among investors. The monetary system might have trouble mediating between them and savers when prices fluctuated, but even if such disturbances were eliminated, output instability would still occur. Pigou (1929, p. 219) speculated that a monetary policy that successfully stabilized the price level would no more than halve the amplitude of fluctuations. Dennis Robertson (1926) went further, arguing that some fluctuations – those that represented the effects of an uneven pattern of technical progress – were “appropriate”, and saw the task of monetary policy as being to aid and abet these, even at the cost of occasional bouts of inflation, while preventing them being overlaid by “inappropriate” fluctuations that would involve investment running ahead of savings as a result of the same type of errors that his Cambridge colleagues emphasized, and fed by forced saving made possible by too much credit creation.

The clearest exponents of the view that inter-temporal coordination failures lay at the root of macroeconomic instability were undoubtedly the so-called Austrians – Mises, Hayek, Robbins et al. For them, the crucial role of the rate of interest in a market economy was to harmonize the inter-temporal choices of producers and consumers. In their view any net creation of the credit by the banking system in an already fully-employed economy, even if it did not provoke price level changes, would lead to trouble.<sup>19</sup> Firms would be encouraged by such an absence of “monetary neutrality” to invest in processes of production that were longer than was appropriate given households’ preferences vis-à-vis the time path to be followed by consumption. This, they believed, was what happened in the United States in the 1920s, and they had advanced this argument even before the onset of the depression. With non-neutral money, the resulting excess of investment over voluntary saving was financed by forced saving, and as soon as the authorities attempted to restore monetary policy to a neutral stance, the associated boom would become unsustainable. As a result of this imbalance between the time structures of production and consumption, the ensuing crisis would see long processes of production being closed down. Because time was required to start up new, shorter, processes, there would inevitably be a period of unemployment whose severity and duration would depend on the length and strength of the preceding expansion, which in turn would have determined the extent of over-investment in inappropriately long processes of production.

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<sup>19</sup>The full-employment assumption is crucial here, but the Austrians made it as a matter of methodological principle. In order to explain unemployment one had to start without it and show how it came about. The Stockholm School in particular were less rigid about this matter, and a willingness to assume unemployment to begin with, and then to ask the simpler question of what factors might cause its level to change, underlay many of their model sequences. One sympathetic modern commentator, Roger Garrison (2001) suggests that the crucial role played by the full employment assumption in Austrian theory means that it should be regarded as a theory of the unsustainable boom, rather than a complete account of the cycle.

With the eclipse of Austrian economics in the 1930s, a key perspective on the importance of economy's supply side in economic fluctuations was lost. Keynes's polemical criticism of *Prices and Production* (1931) occurred during the course of a debate with Hayek that had been prompted by the latter's (1931-32) review of *The Treatise on Money*. This criticism showed that Keynes (1931, pp. 394–6) did not believe either that the Austrian theory of capital was acceptable, or that such a theory was needed to explain output fluctuations and unemployment. He emphasized the effects of discrepancies between the Wicksellian market and natural rates of interest on savings and investment, just as did the Austrian, but, unlike them, only to the extent that they affected the level of aggregate demand in what we would now call the short-run. Keynes simply ignored the fact that the volume of current investment had implications for the size of the capital stock, and hence for economy's capacity to produce goods and services for the future. Crucially this neglect also led him to overlook another fact: namely, that mistaken investment decisions made in the present had the capacity to disrupt future equilibria between supply and demand, perhaps at the level of the economy as a whole, as the Austrians argued, but even in particular sectors of the economy, as his Cambridge colleague Robertson had frequently insisted.

In the *General Theory*, Keynes moved even further from these views. The rate of interest was there said to depend on liquidity preference, to be determined on the margin between money and bonds as the outcome of a decision about how wealth was to be held. Keynes emphatically denied that it was a price with a crucial role to play in co-ordinating saving and investment decisions, a matter about which Robertson among others would, hardly co-incidentally, take him strongly to task. This denial, along with Keynes's unfortunate use of a terminology in which saving and investment were always identically equal to one another, and along with his downplaying of price changes, inevitably led to ideas of forced saving being pushed firmly aside, and with them any consideration of the possibility that the current mistakes in investment decisions might lay up problems for the future on the economy's supply side.

As the post-*General Theory* debate evolved, economists became aware of the Swedish *ex ante* - *ex post* terminology, and this might have, but did not, create scope for further exploration of such ideas. Instead, IS-LM focused attention on the properties of an equilibrium in which saving and investment were equal. Not only this, but saving was assumed to depend on a 'fundamental psychological law' rather than being the outcome of a utility maximizing inter-temporal choice, and investment was largely determined by 'animal spirits' rather than derived from any theory of capital that involved the maximization of the present value of a stream of future returns. Paradoxically, Keynes himself would use ideas about forced saving, apparently derived from his 1920s collaboration with Robertson, to great effect in *How to Pay for the War*, and this work exerted considerable, albeit indirect, influence on Milton Friedman who took it up in the work on the "Inflationary Gap" from which many of his later ideas on monetary theory would evolve.<sup>20</sup> But mainstream macroeconomists ignored this development, either because they

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<sup>20</sup>The crucial evidence on this matter is to be found in a little known (1942) paper by Friedman, in which he praises the methods of coping with the inflationary pressures inherent in war-time spending adopted in the 1941 UK budget, apparently unaware of Keynes's influence on this document. The monetary analysis that was added to his discussion of this approach in the (1953) version of paper on the "Inflationary Gap" is essentially that deployed by Keynes (1923)

were unaware of it, or, more likely, because it could not be fitted into an analytic framework dominated by IS-LM.

That is perhaps why, in the late 1940s and 1950s, when economists began to re-discover the inter-temporal dimensions of consumption and investment theory, they did not also immediately recover the idea that unemployment might be the result of an inter-temporal coordination failure that had the capacity to disrupt the economy's supply side. This idea was partially resurrected by Leijonhufvud (1968) who argued that unemployment in Keynes's own analysis should be thought of as representing such a disequilibrium associated with relative price distortions, which in turn arose from the inelastic expectations that were a natural feature of markets characterized by limited information. These ideas, which Leijonhufvud attributed to the *General Theory*, are all to be found in the pre-1936 literature, as his (1981) paper on "The Wicksell Connection" would in due course begin to make clear.<sup>21</sup>

However, despite Leijonhufvud's urging the importance of seeing unemployment as the outcome of an inter-temporal co-ordination failure, and despite the large immediate impact that his work had on his colleagues, his insights became bracketed with Clower's (1965) dual-decision hypothesis and the concept of quantity constraints, and began to be viewed as part of the 'general disequilibrium' approach to macroeconomics. In the hands of economists such as Barro and Grossman and Malinvaud, the dynamic side of such arguments was downplayed, as we have remarked above. But by the time this had happened, Lucas and his associates had already begun to provide an alternative set of micro-foundations for macroeconomics that, in stark contrast to Leijonhufvud's, insisted that markets always cleared. As we know, for good or ill, it was this New-classical approach that caught on, and mainstream economics nowadays treats the co-ordination of choices made by savers and investors about the allocation of resources over time (like the co-ordination of all other choices) as a fact whose consequences are to be investigated, rather than as a problem in need of analysis. Thus, what had been a matter of central importance in inter-war discussions of the consequences of economic activity taking place in time is at the present time probably more deeply buried than it has ever been, notwithstanding the efforts of a few commentators, for example Roger Garrison (2002), to draw attention to its continuing significance.

## **7. The significance of the losses**

Many economists and some philosophers of science share what could legitimately be called a Panglossian view of the losses that accompany the move towards more formal models. Kitcher (1993), for example, has argued, in the context of natural science, that although science may drop ideas that cannot be expressed with the required degree of rigor, if the problems are

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to analyse inflation as a tax, and by Robertson (1926), under the label "induced lacking", to show how a process of forced saving might, in some circumstances, be transformed into one of voluntary saving as agents attempted to adapt their money holdings to rising prices.

<sup>21</sup>The main difference between our argument and Leijonhufvud's is that whereas he sees the loss of insights about inter-temporal co-ordination as occurring between *The General Theory* and IS-LM, we see the *General Theory* itself, along with IS-LM, as an important contribution to the process whereby these ideas about time were suppressed.

important, scientists will eventually return to them. When they do so, the problems will be analyzed in greater depth than would previously have been possible, and the result is progress. Referring to the transitions from Aristotelian to Newtonian physics, and from Priestley's chemistry to that of Lavoisier, Kitcher has said:

The losses (if any) were vague insights that could not be articulated at that stage in the development of science; the gains, in both instances, were correct explanatory schemata that generated significant, *tractable*, questions, and the process of addressing these questions ultimately led to a recapturing of what was lost. (Kitcher, 1993, p. 117)

This view of progress is similar to the one articulated by Krugman (1995) in the context of theories of location and economic development. He argues that economists ignored theories of location other than that of Thünen, because they all relied on forms of increasing returns that they were not able to model formally. Krugman also suggests that they turned their back on the 'high' development theories developed by Rosenstein-Rodan, Hirschman, et al during the 1950s for very similar reasons. In both cases economists chose to confine their work to what could be modeled formally, even though this meant ignoring what were believed to be important aspects of reality, but once they found the techniques that enabled them to deal with increasing returns, some of the insights that they had set aside in order to indulge in formal modeling were regained.

In similar vein, it could be suggested that the loss of the time dimension of economic activity and all that went with it in the move to IS-LM was only temporary. Thus, we have seen that, as modeling strategies developed and the range of techniques available to economists increased, dynamic problems were rediscovered and in due course understood better than would ever have been possible using the methods available to economists such as Hawtrey, Myrdal, Robertson, etc. It is indeed now technically possible to construct models in which inter-temporal choice is central, expectations are truly forward-looking, in which policy changes have dynamic effects on the economy, and in which it is natural to analyze policy problems in terms of choices among alternative regimes, and the creation of formal models along these lines was undoubtedly beyond the mathematical capacities of the economists of the inter-war years.

However, to argue along these lines is to beg some important questions – in particular about the extent to which post-IS-LM discussions have recovered not just the approaches but also the insights yielded by pre-IS-LM theories. In particular, as we have seen, though contemporary macroeconomic models do allow us to address some of the questions that concerned writers in the 1920s and 1930s, they nevertheless rest on preconceptions about the problems that the area should deal with that differ markedly from those of this earlier period, preconceptions which seem to rule out even thinking about certain other issues. In particular, where IS-LM analysis merely distracted attention from the possibility that certain phenomena, such as systematic failures in inter-temporal co-ordination, might be important, newer approaches seem to rule out the analysis of such failures as a matter of analytic principle. Attractive though it is, therefore, there is at least one serious flaw in the Panglossian argument; namely, that it is possible that more than one route to theoretical progress in macroeconomics was available, and could have taken, after the 1930s, and that a different route or perhaps routes, could have led to radically different destinations.

When at last the importance of time was rediscovered in the 1970s economists had come to

take for granted general competitive equilibrium as a framework for economic analysis.<sup>22</sup> When markets are assumed to be competitive, and the only ‘imperfections’ permitted to affect them arise from agents’ failures to anticipate changes that cannot be forecast, it is perhaps inevitable that they will also appear to operate efficiently and natural to take it for granted that systematic co-ordination failures will not occur. In contrast, it is possible to conceive of a route whereby, starting from Swedish dynamic analysis, naïve of competitive equilibrium theory, economists might have explored lines of inquiry about which we can currently say little. In a world such as the Swedes analyzed, co-ordination failures were common events, and had economists persevered in investigating such a world, we might know much more about them than we did in the 1930s. The point here is not to argue that this would inevitably have happened – counterfactuals are always hypothetical. - but that it could conceivably have done so. This possibility is enough to warn us of the dangers inherent in presuming, as a methodological principle, that losses do not matter.

There is the further argument to be made here, particularly important for macroeconomic theory which, whether its academic practitioners like it or not, is always available to be pressed into service by policy makers. Because policy problems require attention as and when they arise, it is not always possible to wait for theory to be fully developed before applying it, and whatever is at hand is what is going to be used. For example, we have pointed out above that the dominance of IS-LM led to the temporary loss of what had once been commonplace insights about the monetary nature of inflation and about the intrinsically dynamic nature of inflation that stems from the role that expectations play in determining its course. *Even though* macroeconomics had largely regained its understanding of money and inflation by the mid-1970s, so that these were not permanent losses of scientific knowledge, that still leaves two decades during which policy-makers used the ‘naïve’ IS-LM model to guide them, possibly into making some of the mistakes that created the inflation of the 1970s and 1980s. That inflation had serious consequences for the economic well-being of agents in numerous countries, and it is arguable that policy makers with intuitions informed by some serious study of Fisher and Hawtrey (or even Ricardo and Thornton), in addition to IS-LM, would have had a better chance of avoiding at least its worst aspects.

Whilst some dismiss this example as a one-off phenomenon (as did Lucas and Sargent in their (1978) indictment of Keynesian economics) it seems more plausible that the danger is a recurrent one. Consider, for example the monetary problems faced by the Japanese economy in the last decade. As Mauro Boianovsky’s contribution to this conference accurately records, it is taken for granted by many commentators that Japan has encountered a “liquidity trap”. This is hardly surprising, because within IS-LM, the interest rate is the only channel through which monetary policy can work, and the liquidity trap - in the form of an interest elasticity of demand for money that approaches infinity at some low level- is the only financial market factor that can block this channel. But, as Boianowski also notes, students of Hawtrey would know that a “credit deadlock” - a situation in which money creation is inhibited by the unwillingness of pessimistic firms to borrow from the banks at any interest rate - can also render monetary policy ineffective. They would also know, however, that sufficiently aggressive open market operations or money financed budget deficits could be used to break such a deadlock, and that if, as

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<sup>22</sup>Game theory, which in any case came later, does not alter the conclusions significantly.

Hawtrey believed (and as Nelson reminds us monetarists still believe) money creation can have effects through a much wider range of channels than market interest rates, this might be enough to revive a depressed economy. The fact that none of this has been tried in Japan, and that monetary policy was declared impotent there the moment short interest rates began to meet their zero lower bound, suggest that we might here have another example of serious policy consequences flowing from the loss of an idea that didn't fit into IS-LM.<sup>23</sup>

Finally, we come back to the question of inter-temporal co-ordination failures, whose analysis has been ruled out by methodological fiat in modern macro-economics. Recent financial crises in many countries (including the collapse of the Japanese “bubble economy” in 1991) have ended not just with stock market crashes and bank failures, but with the sudden emergence of large stocks of unwanted capital goods - so called “see-through buildings” or, more recently large stocks of unwanted computing equipment and fibre-optic cable, and so on - and these are precisely the symptoms that the Austrians and Dennis Robertson highlighted as consequences of forced saving financed by excessive credit growth. But modern macroeconomics seems to leave policy-makers without any insight into the problems that these phenomena create. In a few years' time, then, we may look back on the late twentieth century with a sense of incredulity similar to that with which Lucas and Sargent viewed the Keynesian era. We may wonder how economists could ever have thought that differences between agents would not matter for the workings of the economy, and how they could have failed to see that coordination failures are endemic in complex systems.

## **8. Concluding Comment**

In view of these considerations, it is hard to portray the development of macroeconomics after 1936 as involving steady progress at the cost of no significant losses. A more accurate verdict on of what went on would be that the sub-discipline's formalization involved desirable gains to be sure, but regrettable losses as well as (c.f. Backhouse 1998). This in turn raises the question of whether it is important for economics to find ways of ensuring that its practitioners retain familiarity with older ways of thinking as new ideas develop. The discipline must certainly remain open to new ideas, but also, to a much greater extent than currently seems to be the case, it needs to retain a healthy skepticism about the extent to which these describe all relevant aspects of the macroeconomic world better than do older approaches. The evidence from the development of macroeconomics during the era of IS-LM suggests that a failure to keep track of already existing ideas extracted a price from the discipline, and one that it continues to pay.

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<sup>23</sup>Brunner and Meltzer's (eg.1993) long-standing insistence that the IS-LM model contains an inadequate account of what they termed “the money supply process”, referred to by Bordo and Schwartz in their paper for this conference, is highly relevant here, since it underlay their efforts explicitly to analyse the same set of interactions among the central bank, the commercial banks and the non-bank public that underlay Hawtrey's analysis. However, their attempt to force these interactions into the formal comparative static framework that monetary economics inherited from IS-LM, rather than into an informal dynamic framework such as that used by Hawtrey himself made their analysis hard for their readers to appreciate.

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