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HICKS ON TIME AND MONEY

By AXEL LEIJONHUFVUD

MODERN macroeconomic theory has been shaped to an extraordinary degree by Keynes and by Hicks. My assignment was to discuss them both, but I have found it too large for a paper. I will confine my discussion of Hicks's role to two related themes: Time and Money.

Even within these boundaries, the following attempt at an interpretation cannot be definitive.¹ Among the several reasons for this, one is germane: I know that I shall learn more from Sir John Hicks in the future. But I cannot know exactly what I shall learn next time I sit down to read or re-read him. Hence today's assessment cannot be my 'optimal' or final one. Rather than commit myself fully, I should retain a measure of 'flexibility'.²

In certain types of situations, it is rational to commit oneself fully or contingently. In others, where the future contingencies cannot be enumerated or their nature anticipated, one should retain flexibility. One difference between neo-classical and Keynesian theory is that the former tends to exclude, whereas the latter must include, situations of the second sort.³ The younger Hicks is remembered for his contributions to neo-classical economics; over the years the elder Hicks has become more insistently Keynesian in this particular sense.

Time and equilibrium

'Every economist is familiar with the accomplishments of Hicks the Younger, whether he has read him or not. That brilliant young man was supremely successful—by reformulating utility theory, by simplifying monetary theory, by interpreting Keynes and the Classics, and by reviving general equilibrium theory—in constructing the moulds into which 40 years of subsequent theoretical developments were to be cast'.⁴ It is helpful to try to see the young Hicks in historical context.

What went on at the London School in the early thirties appears in retrospect almost as important as what was going on in Cambridge. At LSE, the world of Anglo-American economics was being won over from the tradition of Ricardo and Marshall to modern neo-classical economics—or, in the terms of Hicks the Elder, from 'plutology' to 'catallactics'. If Cambridge

¹ I have made one previous attempt. My 'Monetary Theory in Hicksian Perspective' was written in 1968 but not published until 1981, at which time I was still reasonably content with the paper. Once it was in print my understanding of some of the issues began to change—as I shall explain below.

² Cf. Hicks, *The Crisis in Keynesian Economics*, ch. 2, and the antecedent Hart (1942).

³ In stressing this particular distinction between neo-classical and Keynesian theory over others, I am following G. L. S. Shackle more than my own earlier work. Cf. esp. Shackle (1972).

⁴ Quoting my own (1979) review of Hicks's *Economic Perspectives* (1977).

was sufficient unto its British self, Lionel Robbins's London School encouraged the study of the Austrian and the Lausanne schools, of the Americans and the Swedes. ('We were such "good Europeans" in London that it was Cambridge that seemed "foreign".')⁵ Robbins brought Hayek to London and assembled a stable of superbly talented junior people: R. G. D. Allen, Marian Bowley, John Hicks and Ursula Webb-Hicks, Nicholas Kaldor, Abba Lerner, Vera Smith-Lutz, Richard Sayers, and G. L. S. Shackle. Most importantly, Robbins wrote the programmatic tract that, highly controversial in its time, has long since permeated the teaching of economics to the point where its main message has become a platitude (thus depriving its author of the Nobel Prize?). His *Nature and Significance of Economic Science* argued the 'scarcity' definition of economics, a definition that fundamentally changed both the scope and the content of Marshall's subject. Robbins made rational means-ends calculation the core of economics.

It was the younger Hicks that demonstrated how this Robbins programme could be realized. The Hicks-Allen 'Reconsideration' recast demand theory in terms of rational decision theory. Hicks's simplification of monetary theory drew Money into the orbit of marginalist calculation. 'Taking step after step along a road which seemed pre-ordained as soon as one had taken the first step' in a few years time led to the 'static' parts (chs. I-VIII) of *Value and Capital*.⁶ These were the parts of Hicks's early work that, together with 'Keynes and the Classics', were to have such a profound and pervasive influence on how economics was to be taught in the United States in the era when American economics was becoming strongly predominant. Perhaps it is more accurate to say that these parts of Hicks's work were selected by the generation of American economists led by Paul Samuelson that were re-erecting the structure of economic theory using constrained optimization building blocks.

Pure decision theory, formalized as optimization subject to constraints, is essentially timeless. The choice among the foreseen outcomes of alternative actions⁷ is a purely logical calculus that does not involve time in any essential way. Thus was created a durable tension between neo-Walrasian microtheory and Keynesian macrotheory that, decades later, was to culminate in crisis.

This could hardly have been foreseen. As Robert Clower has remarked,⁸

... it was only natural for economists generally to proceed on the presumption that general equilibrium theory had no inherent limitations. . . . That any even moderately 'general' economic model should [be incapable of representing Keynesian

⁵ Cf. the 'Commentary' to *The Theory of Wages*, (1963), p. 306. 'Plutology' and 'catalactics' are discussed in Hicks, 'Revolutions' in Economics', in Spiro Latsis (ed.), (1976) reprinted in Hicks, *Classics and Moderns* (1983).

⁶ *Economic Perspectives*, pp. v-vi.

⁷ The foreseen consequences may of course be probability distributions of outcomes. This does not alter the problem.

⁸ Cf. Clower (1975), p. 134.

processes] . . . would hardly occur naturally to any but a very perverse mind. That the elaborate Neo-Walrasian model set out in Hicks' *Value and Capital* might fail [in this respect] would have seemed correspondingly incredible to any sensible person at the outset of the Neo-Walrasian Revolution.

The younger Hicks knew that Time was a problem. We find him wrestling with it in almost all the parts of his early work that did *not* become part of the American neo-classical canon. It was to become even more of a preoccupation—an unfashionable preoccupation—for Hicks the Elder.

From the first, it seems, Hicks saw it as a supreme theoretical challenge, deserving the most sustained effort, to find a mode of process analysis that would retain a role for equilibrium constructions without denying (or trivializing) change. In the early going, this amounted to finding a workable way between Walras and Pareto, on the one hand, and Knight and Hayek on the other.⁹ Thirty or forty years later, the opposed alternatives—Arrow-Debreu *v.* Shackle or Lachmann—are clearer and also further apart. In the Arrow-Debreu construction, the rational choice of each agent is defined over all dimensions of commodity–time–contingency space; the result is that all decisions are made once and for all at the origin of time. To obtain a model in which decisions are made in temporal sequence, agents must be ignorant of some of the information that is necessary in order to calculate all optimal allocations at the beginning of time. Thus Shackle poses the issue with uncompromising force: ‘. . . the theoretician is confronted with a stark choice. He can reject rationality or time.’¹⁰

The American Neo-Walrasians, from Paul Samuelson to Robert Lucas, have not seen this choice as at all difficult. In general, they have simply gone whole hog for Rationality, letting Time and Change be trampled underfoot in the philosophical muck as unfit food for economic thought. If forced (somehow) to choose, it is possible that Hicks the Younger might also have opted for rational allocation theory; Hicks the Elder almost certainly would opt for economic history. In actuality, Hicks fought fifty years to maintain a conceptual middle ground.

The issue may have come into focus at LSE precisely because all of the neo-classical schools were to some extent cultivated in the circle around Robbins and Hayek. Marshall had been aware of the problem¹¹ and had

⁹ Cf. Hicks, ‘The Formation of an Economist’, (1979*b*, p. 199), now reprinted in *id.* (1983).

¹⁰ Cf. Shackle (1972), Preface.

¹¹ Hicks, *Capital and Growth*, pp. 47–8 quotes Marshall (1928), p. 379, n. 1: ‘A theoretically perfect long period . . . will be found to involve the supposition of a stationary state of industry, in which the requirements of a future age can be anticipated an indefinite time beforehand . . . and it is to this cause more than to any other that we must attribute that simplicity and sharpness of outline, from which the economic doctrines in fashion in the first half of this century derived some of their seductive charm, as well as most of whatever tendency they may have had to lead to false practical conclusions.’ Of course, the second half of the 20th century takes a generally more permissive attitude to ‘seductive charms’ than this most eminent Victorian among economists. Shackle’s aptly titled chapter ‘Marshall’s Accommodation of Time’, in *id.* (1972), gives a sample of other remarks of Marshall’s indicating his preoccupation with the issue.

devised a method that at least partly evaded it. Hayek had worked on the construction of an equilibrium process 'in time' and had found himself forced back onto 'perfect foresight' assumptions.¹² Robbins had drawn the conclusion that 'The main postulate of the theory of dynamics is the fact that we are not certain regarding future scarcities.'¹³

As matters stood around 1930, the static toolbox of economic theory was strictly applicable only to stationary, perfect foresight processes. It was not at all clear that economic theory provided any foundation for the disciplined analysis of monetary questions or business cycles. Hicks's earliest work dramatized the predicament. In particular, his remarkable 1933 paper on 'Equilibrium and the Cycle'¹⁴ drove home a point made by Knight: that in a perfect foresight equilibrium process, people would not demand cash-balances. This spelt trouble for the most sophisticated cycle theory available at the time. What became of Hayek's notion of 'neutral money' as a criterion for maintaining macroeconomic equilibrium, if in equilibrium there could be no place for money, 'neutral' or otherwise?

The Swedish followers of Wicksell had run into similar quandaries and it was from Myrdal and Lindahl that Hicks got help with the next step.¹⁵ The next step had to be a method of describing economic processes that (a) was not confined to just 'perfect foresight' processes, and (b) still did not force the abandonment of the entire apparatus of inherited static theory. Lindahl's temporary equilibrium method¹⁶

reduced the process of change to a sequence of single periods, such that, in the interior of each, change could be neglected. . . . Everything is just the same as with the 'static' kind of process analysis . . . save for one thing: that expectations are explicitly introduced as independent variables in the determination of the single-period equilibrium.

Thus, when the *General Theory* appeared, Hicks had been working along these lines for some time. His first reaction gave pride of place to Keynes's use of a similar device: a short-run equilibrium adapting to independently

¹² Cf. Hayek (1928).

¹³ Robbins (1932), p. 79.

¹⁴ 'Gleichgewicht und Konjunktur', *Zeitschrift für Nationalökonomie*, iv (1933). This remarkably modern, historically important paper was finally translated and published in *Economic Inquiry* (Nov. 1980), thanks to its then editor, Robert Clower. It is now reprinted in Hicks (1982).

¹⁵ G. Myrdal, 'Geldtheoretisches Gleichgewicht', in F. A. Hayek, (ed.), *Beiträge zur Geldtheorie* (1933), was reviewed by Hicks in *Economica*, (Nov. 1934). The review is reprinted in Hicks (1982). G. L. S. Shackle, also a member of the Robbins circle, testifies to the great influence and importance of Myrdal's contribution in his (1967), Chapters 9 and 10. Of Lindahl's temporary equilibrium concept, Hicks first learned through personal acquaintance. He has discussed temporary equilibrium methods repeatedly, e.g., in *Value and Capital*, esp. chs. IX-X and XX-XXII, in 'Methods of Dynamic Analysis' (1956) now reprinted in Hicks (1982), and in *Capital and Growth*, ch. VI.

¹⁶ Hicks (1965), p. 60.

specified long-term expectations.¹⁷ But the kinship was not all that close. Keynes had applied the ‘methods of expectations’ to a Marshallian short period. Marshall had invented a kind of analysis (‘with some slight dynamic flavouring’¹⁸) which definitely was ‘in time’ but that left the line between statics and dynamics unclear. In *Value and Capital*, Hicks developed an alternative line of attack.

The attack starts with the famous definition of ‘Economic Dynamics’ as those parts of economic theory ‘where every quantity must be dated’.¹⁹ This was an important step. The Marshallians, for example, had not taken it.

By itself, the dating of goods only adds dimensions to the commodity space considered in ‘timeless’ statics. Studies in efficient intertemporal resource allocation following Fisher and Hicks have improved our understanding of capital, growth, and interest theory immensely. But the course of this development became quite similar to what happened to British classical theory, about which Hicks observed: ‘The more precise capital theory became, the more static it became; the study of equilibrium conditions only resulted in the study of stationary states’.²⁰ We have to substitute ‘steady’ for ‘stationary’, of course, but otherwise the conclusion holds. It is presumably for this reason that Hicks no longer favours his old static–dynamic distinction but prefers to talk of analysis that is ‘out of time’ or ‘in time’.²¹

Dating brings in future time, but it does not necessarily help in bringing in the passage of time. If the usual (stochastically) perfect knowledge assumptions are made, the end result will be the Arrow-Debreu contingency market model in which all decisions are made at the origin of time. There is no business left to transact at later dates. Money and liquidity can be forced into such a structure only by obvious artifice.

The present-day practice at this juncture is for the theorist to retire behind a smoke screen while intoning some incantation about transactions costs. Hicks, in 1939, did a bit better. What must be done is to weaken the informational assumptions of the model so as to make agents postpone at least *some* decisions ‘until they know better’.²² Hicks discussed several types of uncertainty and decided, I think correctly, that agents’ uncertainty about

¹⁷ Hicks’s 1936 *Economic Journal* review is reprinted in *Money, Interest and Wages* as ‘The General Theory: A First Impression’.

¹⁸ Surely, Hicks was thinking of Marshall when (*Value and Capital*, pp. 115–16) he declined to follow ‘the usual course of economists in the past . . . and give(s) one’s static theory some slight dynamic flavouring, (so that) it can be made to look much more directly applicable to the real world But it will still be quite incompetent to deal properly with capital and interest, or trade fluctuations, or even money . . .’

¹⁹ *Value and Capital*, p. 115.

²⁰ *Capital and Growth*, p. 47.

²¹ Cf. esp. his ‘Time in Economics’, as reprinted in (1982), e.g. p. 291: ‘(Steady State economics) . . . has encouraged economists to waste their time upon constructions that are often of great intellectual complexity but which are so much out of time, and out of history, as to be practically futile and indeed misleading. It has many bad marks to be set against it.’

²² It is for this reason that I have proposed changing the Hicksian definition of dynamics to ‘those parts of economic theory where *decisions* must be dated’. Cf. Leijonhufvud (1983*b*).

their own intentions was the most fundamental²³

... in particular, they know that they cannot foretell at all exactly what quantities they will themselves desire to buy or sell at a future period. . . and this it is, in the end, which limits the extent to which forward trading can be carried on in practice.

This argument is the bridge by which Hicks made his escape from steady-state capital theory into temporary equilibrium theory. In the temporary equilibrium theory of *Value and Capital*, time is divided into a sequence of 'weeks'. Planned demands and supplies for the week depend on current prices and expected future prices. Current prices are determined on 'Monday' and rule unchanged for the rest of the week. On 'Sunday' (we may imagine), the parameters of the equilibrium system are updated: changes in stocks are accounted for and price-expectations revised. The system is then ready for another Monday morning.

In this story, all markets cleared each Monday. Hicks understood perfectly that this assumption by itself did not preclude periods of subnormal activity in the system. The defence of the assumption that he suggested is exactly the one so strenuously insisted upon by Lucas, Barro, *et alia* almost forty years later. In Hicksian terms, if price-expectations are inelastic, a fall in current prices will induce intertemporal substitution: supplies will be shifted from this week into next.²⁴ Market-clearing, however, was equilibrium in a 'limited sense'; in the more fundamental sense of 'Equilibrium over Time', Hicks emphasized, the economic system was 'usually out of equilibrium'.²⁵

This temporary equilibrium method is thus clearly distinct both from Keynes's short-run equilibrium, on the one hand, and from the new classical equilibrium method of more recent years. It avoids some of the problems of the alternatives and deserves further exploration, therefore,²⁶ although of course, it has problems of its own. But, while Hicks has resumed the struggle

²³ *Value and Capital*, p. 137. Of course, this way out of the predicament ultimately requires us to formulate a theory of the behaviour of agents who *know* that they are likely to 'foresee their own wants incorrectly' (p. 134). This problem Hicks did not tackle in 1939. It is in his *Crisis in Keynesian Economics*, Chapter II, thirty-five years later, that we find it addressed. Decision-making by agents who know that they will know better later (but don't know, even probabilistically, what it is they will learn) will not fit naturally into the usual constrained optimization apparatus. For a comprehensive attack on the problem, cf. Ron Heiner (1983).

²⁴ Cf. *Value and Capital*, p. 131: 'There is a sense in which current supplies and current demands are always equated in competitive conditions. Stocks may indeed be left in the shops unsold; but they are unsold because people prefer to take the chance of being able to sell them at a future date rather than cut prices in order to sell them now. The tendency for the current price to fall leads to a shift in supply from present to future. An excess of supply over demand which means more than this is only possible if the price falls to zero, or if the commodity is monopolized, or if the price is conventionally fixed'.

²⁵ *Value and Capital*, loc. cit.

²⁶ It took more than 30 years for the profession to catch on to what Hicks had been up to in 1939. Grandmont's survey (1975) shows how the crisis of Keynesianism, which was in part a crisis of Keynes's method, had produced a more profound appreciation of the difficulties that the temporary equilibrium approach had been designed to address.

for a systematic ‘in time’ analysis later—and on more difficult ground even²⁷—he chose to abandon the Temporary Equilibrium approach.

Why? The Elder Hicks has given his retrospective reasons. There were problems *within* the ‘week’ and *between* ‘weeks’.²⁸

Much too much had to happen on that ‘Monday’! And... I was really at a loss how to deal with the further problem of how to string my ‘weeks’ and my ‘Mondays’ together.

Getting from one ‘week’ to the next required both a theory of capital accumulation and a theory of the revision of expectations. The first problem by itself was forbidding at the time; only the later development of modern growth theory made it manageable. Forty years have not brought us much advance on the second problem.²⁹

In his retrospective evaluation, Hicks does not point to the problems that the temporary equilibrium method would have to overcome in order to provide a ‘continuation’ theory; instead, he focuses on how the method dealt with events ‘within the week’.³⁰

... I tried to go further [than to work with *given* expectations], to allow for the effects of current transactions on expectations; supposing that these effects could (somehow) be contemporaneous with the transactions themselves. . . . That however was nonsense. . . . It does deliberate violence to the *order* in which in the real world (in *any* real world) events occur.

It was this device, this indefensible trick, which ruined the ‘dynamic’ theory of *Value and Capital*. It was this that led it back in a static, and so in a neo-classical, direction.

What an extraordinarily harsh judgement this is! Why? Because in obliterating the *sequence* in which things happen, the model comes to ignore the structure of markets. It matters, for instance, whether people commit themselves on quantities and discover their mistakes through price-change ‘surprises’ or set their prices and see their errors revealed in the behaviour

²⁷ The ‘Traverse’ problem which Hicks set himself in chapter XVI of *Capital and Growth* and analysed at length in *Capital and Time* adds a forbidding burden of capital theory to the difficulties discussed in the text.

²⁸ Cf. ‘Time in Economics’, in Hicks (1982), p. 290. In 1956, (‘Methods of Dynamic Analysis’), Hicks distinguished between the problems of *single-period theory* and those of *continuation theory*. Cf. the reprint in (1982).

²⁹ A ‘Robertson lag’ in income is yet another possible bridge from ‘week’ to ‘week’. In Leijonhufvud (1968), I tried to get to the *General Theory* by this route: I had a first period in which sales declined because sellers had inelastic price expectations and thus did not cut prices fast enough; in the next week, demand was then ‘income-constrained’ with consequent Keynesian multiplier-effects, etc. I thought at the time that I had, in effect, got over from *Value and Capital* to the *General Theory* in fairly good order and it puzzled me why Hicks had not tried this route. But Hicks had defined his temporary equilibrium in such a way as to preclude unintended shortfalls in sales. See his comments below on the ‘indefensible trick’.

³⁰ *Economic Perspectives*, p. vii. The sentence in quotes is from *Capital and Growth*, ch. VI, where the matter is also discussed. Compare also Clower (1975) and Clower and Leijonhufvud (1975).

of quantities.³¹ It matters, in Hicks's terms, whether the markets in the system are mostly of the *flex-price* or the *fix-price* variety. In this century, 'the unorganized flexprice market, the old type, is on the way out... modern markets are predominantly of the fixprice type...'³² In Hicks's view, this historical transformation is of major macroeconomic significance. The change in the predominant market form is a change in the way that impulses are propagated through the system. The harsh language becomes understandable—for, of course, Hicks sees the 'indefensible trick' still being practised all over!

IS-LM

The younger Hicks may have had somewhat different reasons for abandoning his temporary equilibrium method. One of them surely was that Keynes had come up with an alternative method of short-period analysis. It was a rough-and-ready sort of short-period method and Hicks the Younger would have realized better than anybody else how rough it was. But it seemed to be adequate to Keynes's purposes and Hicks agreed that Keynes's purposes were the supremely important ones.

Soon after his original review, Hicks returned to the *General Theory* and wrote 'Mr. Keynes and the 'Classics': A Suggested Interpretation'. The IS-LM apparatus of this immensely influential paper was not a Walrasian (or Paretian) construction but a hybrid. Keynes's macrotheory was built with Marshallian microcomponents. But the modelling idea was, as Hicks has himself explained,³³ borrowed from *Value and Capital*, where he had worked out a two-dimensional representation of the equilibrium for a Walrasian system of three markets.

The IS-LM model summarized numerous features of the *General Theory* with admirable economy and it was to serve in the deduction of numerous Keynesian comparative statics propositions that Keynes had not thought of. The model became the backbone of instruction in macroeconomics for forty years. Nonetheless, something was just a bit askew with it. In later years, Hicks has several times come back to reassess it and the uses to which it has been put. In brief, (a) he has remained fairly content with it as a synopsis of Keynes' theory;³⁴ (b) he has become less satisfied with it as a way of portraying the 'classics' and hence as a tool for isolating Keynes's contribution by IS-LM comparisons;³⁵ and (c) he has grown somewhat sceptical

³¹ 'Methods of Dynamic Analysis', section iv.

³² *Economic Perspectives*, p. xi. Cf. also *Capital and Growth*, Chapter VII. *Money, Interest, and Wages*, pp. 226–35, 296–99, 320–4.

³³ Cf. 'IS-LM: An Explanation', in Fitoussi (ed.) (1983) and also included in Hicks (1982).

³⁴ Cf. e.g. *The Crisis in Keynesian Economics*, p. 6, and 'Recollections and Documents' in *Economic Perspectives*, this paper also records Keynes's detailed and favourable reaction to the IS-LM representation of his theory.

³⁵ Cf. *Critical Essays in Monetary Theory*, p. vii: 'But as a diagnosis of the 'revolution', [IS-LM] is very unsatisfactory. It is not a bad representation of Keynes; but it does not get his predecessors (the 'Classics' as he called them) at all right.'

about it as a general purpose framework for macroeconomic analysis.³⁶ His several commentaries on IS–LM all focus on the problem of time.

From the early fifties to the mid-sixties, Hicks did not participate much in ongoing developments in economic theory.³⁷ When he returned to theoretical work full time, he was eager to learn what had been accomplished in growth theory but found himself out of sympathy with the directions taken in macroeconomics and monetary theory. The trouble was that these directions had been set by Hicks the Younger—in those parts of his work that the American economists had chosen to cultivate. Hicks's first dismaying confrontation with his own brain-children—now fully grown and so independent!—came, it appears, in 1957 when he was asked to review Patinkin's first edition. Patinkin's work had been systematically and rigorously built on the basis of the Hicks–Allen 'Reconsideration', the paper 'simplifying' monetary theory, 'Keynes and the Classics', and the first eight chapters of *Value and Capital* (together with some closely related works by Oscar Lange).³⁸ But the theoretical structure that Patinkin had erected on these foundations, Hicks thought, threatened to emasculate Keynesian economics.³⁹ Never a whole-hearted Keynesian, Hicks was nonetheless too much of a Keynesian to stand idly by under the circumstances.

Patinkin's basic model was a Walrasian general equilibrium model, built up from choice-theoretical individual experiments, via aggregation, to equilibrium market experiments. It allowed no Marshallian distinctions between short-run and long-run equilibria. It was either in 'the' equilibrium or not in equilibrium at all. Patinkin used the Hicksian technique for portraying the equilibrium of an aggregative version of the system as the intersection of two reduced forms in interest/income space. It 'looked' exactly like IS–LM—except that this version would not allow for unemployment.⁴⁰

Hicks set out to show that 'classical' and Keynesian theory 'do not overlap all the way'—that all the Keynesian furore had not been pointless. His point

³⁶ Cf. e.g., 'Time in Economics', in Hicks (1982), pp. 289–90: 'All the same, I must say that the diagram is now much less popular with me than I think it still is with many other people. It reduces the *General Theory* to equilibrium economics; it is not really *in time*. That, of course, is why it has done so well'.

³⁷ Approximately, from *A Contribution to the Theory of the Trade Cycle* (1950) to *Capital and Growth* (1965). Or, perhaps, for the duration of his tenure as Drummond Professor (1952–65). For his preoccupations during this period, cf. 'The Formation of an Economist', p. 202.

³⁸ O. Lange (1942) and (1944).

³⁹ The book, he said, was written not 'to elucidate the 'Keynesian Revolution'', but to deny that it is a revolution at all'. Cf. Hicks (1957). This judgement was not fair to Patinkin as Hicks has acknowledged. Cf. id. (1979c), n. 5.

⁴⁰ Patinkin understood, of course, that this model would produce unemployment only if one imposed the restriction of rigid (and too high) wages. He also was quite clear on the fact that Keynes had assumed neither rigid wages nor a liquidity trap. (Patinkin (1948) had in any case demonstrated already that a liquidity trap would not by itself lead to unemployment in this type of model). Consequently, he chose to deal with Keynesian unemployment informally, discussing the unemployment dynamics of the system 'off the curves' of his formal model. Cf. Patinkin (1956, Chapter 13).

of departure was the right one:⁴¹

The crucial point, as I now feel quite clear, on which the individuality of the Keynes theory depends, is the implication. . . that there are conditions in which the interest-mechanism will not work.

In the original Patinkin review, Hicks tried to show this in two ways. His first argument, however, amounted to a reassertion of the liquidity trap explanation of unemployment and Patinkin had only to repeat his demonstration of how, with flexible wages, the Pigou effect would restore full employment. Within the IS–LM context, the explanation of unemployment is thus thrown back unto the ‘rigid wages’ postulate.⁴² Hicks’s second and surviving argument attempted to clarify the relationship between Keynes and the ‘classics’ by showing how the parameters of the IS–LM model depend on the *length of period* assumed. The extent to which wages are variable, Hicks pointed out, will depend not only on the magnitude of excess demand (or supply) of labour but also on the length of time allowed for adjustment. Over a sufficiently long period, the IS-schedule should then be infinitely elastic (at the ‘natural rate’ of interest), while the speculative component disappears from money demand so that the LM-schedule becomes quite inelastic. With a shorter period, the ‘classical’ dichotomy fails, and the shorter the period the more ‘Keynesian’ the picture: IS becomes very inelastic and LM exceedingly elastic in the very short run.⁴³

This defense of Keynes (if such it was) could only focus attention on Keynes’s own treatment of time, however. Hicks’s reservations on this score (as well as those of other ‘critical readers’) went back all the way to the thirties: ‘. . . but we have agreed to suspend our doubts because of the power of the analysis which Keynes constructed on this (perhaps) shaky foundation.’⁴⁴ It could not be left at that indefinitely. In his 1974 effort to address *The Crisis in Keynesian Economics*, Hicks left the matter to one side and simply made no use of IS–LM at all. But in *The Crisis*, he advanced the theory of liquidity as flexibility as one of the needed cures for the ailing

⁴¹ Cf., ‘The “Classics” Again’, as reprinted in Hicks (1967), p. 143. My reasons for judging this to be the right point of departure are spelt out at great length in ‘The Wicksell Connection’ in Leijonhufvud (1981).

⁴² Cf. Hicks (1957), Patinkin (1959).

⁴³ Alan Coddington (1983) discusses this Hicksian analysis in somewhat more detail, pp. 68–73.

⁴⁴ Cf. *Capital and Growth*, p. 65. The particular difficulty (‘. . . now lulled to sleep by long familiarity’) mentioned in this context was that ‘[Keynes’s theory] works with a *period* which is taken to be one of equilibrium . . . and which is nonetheless identified with the Marshallian “short period”, in which capital equipment . . . remains unchanged. The second seems to require that the period should not be too long, but the first that it should not be too short; . . . It is not easy to see that there can be any length of time that will adequately satisfy both of these requirements’. (pp. 64–65). One notes that this observation would seem to threaten the legitimacy of Hicks’s accordion playing with the period in his ‘The “Classics” Again’.

Keynesian tradition. In contrast to how it emerges in static portfolio theory,⁴⁵

... liquidity is not a property of a single choice; it is a matter of a sequence of choices, a related sequence. It is concerned with the passage from the known to the unknown with the knowledge that if we wait we can have more knowledge. So it is not sufficient, in liquidity theory, to make a single dichotomy between the known and the unknown. There is a further category, of things which are unknown now, but will become known in time.

This, clearly, lends urgency to the question of how time is to be treated in Keynesian models. Immediately afterward, therefore, Hicks turned to re-examine the compromises of Keynes's method and found them, on close inspection, less and less satisfactory:⁴⁶

Keynes's theory has one leg which is *in* time, but another which is not. It is a hybrid. I am not blaming him for this; he was looking for a theory which would be effective, and he found it... but what a muddle he made for his successors!

In brief, the 'leg in time' is LM, the 'leg in equilibrium' is IS. (Clearly, this 'straddle', as Hicks called it, was a position that had to become uncomfortable with the passage of time!) Hicks' own temporary equilibrium method⁴⁷

also was divided; there was a part that was *in* time and a part that was not. But we did not divide in the same place. While Keynes had relegated the whole theory of production and prices to equilibrium economics, I tried to keep production *in* time, just leaving *prices* to be determined in an equilibrium manner.

Production will not be equilibrated in a 'week'. Hicks's 1983 'IS-LM: An explanation' carries the argument forward:⁴⁸

If one is to make sense of the IS-LM model while paying proper attention to time, one must, I think, insist on two things: (1) that the period in question is a relatively long period, a 'year' rather than a 'week'; and (2) that, because the behaviour of the economy over that 'year' is to be determined by propensities and such-like data, it must be assumed to be, in an appropriate sense, *in equilibrium*.

Product markets are in *flow* equilibrium throughout the 'year'; production plans are being carried through without disappointment or surprise; this, in Hicks's view, is how we must interpret the IS-curve. What about the LM-curve? It is a *stock*-relation and, by itself, could apply simply to a point in time. But to be consistent with the IS-construction, Hicks points out, a more restrictive equilibrium condition should be applied, namely, maintenance of stock equilibrium throughout the 'year'. Expectations and realizations must be consistent within the period. But at this point of his 1983

⁴⁵ *Crisis in Keynesian Economics*, pp. 38–9.

⁴⁶ 'Time in Economics', in Hicks (1982), pp. 288–9.

⁴⁷ *Ibid.*, p. 290.

⁴⁸ 'IS-LM: An Explanation', in Fitoussi (1983), p. 57.

argument, we are suddenly back facing the dilemma of that 1933 paper: 'Disequilibrium is the Disappointment of Expectations'—and in equilibrium processes there is no place for money! The 'Equilibrium method, applied to liquidity over a period, will not do'.⁴⁹

Within the IS–LM construction itself, therefore, we find this tension between Equilibrium and Change which I see as a *Leitmotiv* through five decades of Hicks's work. Hicks is 'quite prepared to believe that there are cases' where we are 'entitled to overlook' the potential inconsistency between the ways that the IS and the LM have been constructed. But he clearly no longer regards it as a robust tool for the analysis of almost all macroeconomic questions.⁵⁰

IS–LM served us well for so long (didn't it?). How could we not have run into obvious problems with it, if it teeters on the brink of conceptual inconsistency? IS–LM exercises produce the right answers (most of us will agree) to a large number of standard macroquestions. Yet, it produces the wrong conclusions (some of us insist) on some issues. Hicks leaves us with a general scepticism about the method which does not help us much in determining what uses are safe and what uses are not.

In an attempt to find out 'What was the Matter with IS–LM?', I came to a conclusion very similar to Hicks's judgement on the temporal equilibrium method: as with all equilibrium constructions, IS–LM ignores the *sequence* of events *within the period*. The result can be nonsense.⁵¹

IS–LM, handled as if it were a static construction... produced a nonsensical conclusion to the *Keynes and the classics* debate: namely, that Keynes had revolutionized economic theory by advancing the platitude that wages too high for full employment and rigid downwards imply persistent unemployment. It failed to capture essential elements of Keynes's theory: namely, that the typical shock is a shift in investors' expectations and that it is the failure of intertemporal prices to respond appropriately to this change in perceived intertemporal opportunities that prevents rational adaptation to the shock. The same 'as if static' method produced the conclusion that *liquidity preference versus loanable funds* was not a meaningful issue; that it does not matter whether the system is or is not potentially capable of adjusting intertemporal prices appropriately in response to changes in intertemporal opportunities.⁵²

⁴⁹ *Causality in Economics*, p. 85.

⁵⁰ Cf. 'IS–LM: An Explanation', pp. 60–2. The brief summary in the text fails, I am afraid, to do justice to the subtlety of Hicks's argument. The reader who would appraise it should consult also his *Causality in Economics*, ch. VI and VII.

⁵¹ Leijonhufvud (1983b), p. 86. But the IS–LM interpretation of Keynes still has backers who feel that the algebra cannot but lead us right. Paul Samuelson (who has, of course, advocated the sticky wages view as preserving the essentials of Keynes's theory) sees preoccupation with the model's conceptual foundations as revealing some sort of anti-mathematical obscurantism. See his Keynes centennial article in *The Economist*, 25 June, 1983.

⁵² The equivalence of the liquidity preference and loanable funds approaches to interest determination was argued by Hicks the Younger in his 1936 review of Keynes and in *Value and Capital*, ch. XII. There the argument was made in a Temporary Equilibrium context but it has been carried over to IS–LM by others. The argument is, I think, misleading—except possibly in

Ignoring sequencing becomes a source of trouble in particular in connection with *comparative statics* uses of the IS–LM model—i.e. the uses that are the stuff which macrotexts have been made of for several decades, but which Hicks did not consider in reassessing the model.

Consider, for illustrative purposes, the analysis of an increase in the supply of money in the common textbook context where the money supply is simply an exogenously fixed M . Full adjustment to this parametric disturbance requires a proportional rise in all money prices with no effect on output, employment or other real magnitudes. In an IS–LM diagram with money income on the horizontal axis, *both* schedules have shifted the same distance rightwards. In a Lucas model, if M is observable, the system goes to this position immediately. In a Friedman model (of, say, ten years ago), on the other hand, nominal income responds strongly in relatively short order, but part of this is an increase in real output and employment and full adjustment to the neutral equilibrium takes ‘longer’. In a Keynesian model (of 20 years ago?), finally, the ‘short-run’ reactions are that the interest rate falls, velocity declines, investment and employment increase a bit, while the price-level stays about the same.

All three possibilities can be demonstrated with the same basic model. How, then, do they differ? To Friedman, the Phillips-curve is vertical only over the ‘long run’, not already in the ‘short run’ as in Lucas. In Friedman’s short run, the monetary disturbance has output effects because the people temporarily miscalculate real wages. To the Keynesians, the (approximately) proportional increase in nominal income occurs only over the ‘long run’, not already in the ‘short run’ as in Friedman. In the Keynesian short run, the monetary disturbance has only weak effects on nominal income now because people fail to anticipate the effect that it must have on nominal aggregate demand sooner or later; hence the short run effects on income occur only in so far as some firms are induced by a fall in the interest rate to increase their investment even though their expectations of future nominal aggregate demand have not improved.⁵³

the context of rational expectations models; if the general equilibrium consequences of some parameter change are ‘rationally anticipated’, all markets would ‘open’ with the new equilibrium prices already ‘posted’. For such a conceptual experiment, it indeed does not make sense to ask which excess demand was responsible for the change in which price. One must (to make sense) consider instances where, once price-setters have posted prices based on their best forecasts, actual trading produces excess demands and supplies thus revealing the ‘errors’ in the forecasts. The issue of the liquidity preference versus loanable funds squabble is how this error-activated feedback control of price works in the case of the interest rate—specifically, whether the interest rate is ‘governed’ by the excess demand for money or by the excess supply for securities. To discriminate between the two hypotheses, one must then consider states of the economy which do *not* have ED for money and ES of securities (or vice versa) at the same time. In a Keynes Model, a ‘decline in MEC’ produces an example, namely, a state with an ES of commodities and a corresponding ED for securities while—at this stage of the *sequence*—the ED for money is still zero. If the loanable funds hypothesis is true, it is possible that the intertemporal prices mechanism will take care of the intertemporal coordination problem (without a recession); if the liquidity preference hypothesis is true, it is inconceivable.

⁵³ For a more careful and detailed discussion, cf. Leijonhufvud, (1983*b*), pp. 69–70, 76–80.

So, Lucas's people are assumed to know something that Friedman people do not, and Friedman people something that Keynesian people do not.⁵⁴ The temporal order of decisions matters when information is incomplete, when people have to react to situations they did not foresee and when they learn from realizations they did not anticipate. Such learning can be slow or fast or, in some cases, unnecessary.

Note how these knowledge or learning assumptions are reflected in the mechanics of manipulating the IS–LM diagram. In the Keynesian exercise, LM shifts right, IS stays put, and the short-run effects depend on the elasticities of the two reduced forms. In the Friedman case, IS also shifts, although perhaps *not quite* all the way; the elasticities then are practically irrelevant. In the Lucas case, both reduced forms shift in parallel fashion. The IS–LM modelling strategy would seem to presuppose that we have to deal with a Keynesian world of slow learners. Otherwise it does not seem to make sense to adopt the two-stage procedure of, first, deriving the two reduced forms and, second, getting the answers by shifting one and keeping the other constant. The use of IS–LM as if it were a comparative static apparatus involves the lag-assumption that one schedule shifts before the other and that there will be a well-defined 'short-run' solution halfway in the equilibrating process. This sequencing or lag structure rests on assumptions of incomplete information on the part of various agents in the model.⁵⁵

This conclusion we have derived from an illustrative case where monetarist assumptions are made about the supply of money. There is, however, also another possible interpretation of Keynesian IS–LM analysis which we will come to later.

Money and history

In the most exciting chapter of his *Critical Essays in Monetary Theory*, Hicks sought to structure two centuries of monetary writings in a simple, striking, and informative way. His 'Monetary Theory and History—An Attempt at Perspective' was critical of ahistorical monetary theorizing and insisted on the necessity of doing monetary theory in historical and institutional context. It also suggested that the history of monetary controversy could be understood as a running battle between two traditions, a 'metallic money' tradition and a 'credit money' tradition.

The 'metallic money' theorists, in Hicks's schema, focused on equilibrium propositions in their theorizing, dealt analytically with money 'as if' it were a commodity, and strove to reduce monetary policy to obedience to some 'mechanical rule'. Credit theorists, on the other hand, saw money as part of the overall system of debits and credits that extends beyond the banking system to encompass the entire economy; credit expansions and contractions

⁵⁴ This sounds suspiciously like an IQ ranking for Lucasian, Friedmanian and Keynesian economists. This Keynesian didn't mean it that way!

⁵⁵ Cf. Leijonhufvud (1983b), p. 87.

were central to their conception of the subject and so obliged them to try their luck at disequilibrium analyses; always aware that credit rests on confidence; finally, writers in this tradition saw monetary policy as an exercise in judgment of contemporary conditions. Hicks named Ricardo the patron saint of the 'metallic' tradition and gave Thornton the same status in the 'credit' school of thought. He saw the Currency School and, later, Hayek, Pigou, Rueff, and Friedman as Ricardo's followers and put the Banking School, Bagehot, Wicksell, Hawtrey, Robertson, and Keynes in line of descent from Thornton.

In insisting on the close link between monetary theory and history, Hicks thought above all of the evolution of credit markets and financial institutions: 'In a world of banks and insurance companies, money markets and stock exchanges, money is quite a different thing from what it was before these institutions came into being.'⁵⁶ The metallic money theorists (including the modern monetarists) seemed determined to ignore this historical development. Consequently, Hicks's analysis suggested, time had put an ever-increasing distance between their theory and reality.⁵⁷

The 1967 'Perspective' helps one understand what Hicks regards as the important themes running through his own contributions to monetary theory.⁵⁸ Consider, once again, what aspects of the work of Hicks the Younger came to be influential and what aspects ignored. For decades, all graduate students have learned that the modern choice-theoretical money demand function stems from his 1935 'Simplifying' paper. Most will know that Hicks already had the demand for money depending on wealth, on anticipated yields on alternative placements, and on the cost of asset transactions. Some may recall that his analysis was anything but reassuring on the stability of the function in terms of these arguments. Few (I am guessing) will remember that, in Hicks's hands, the theory immediately suggested the beginnings of a theory of financial structure, of the composition of balance sheets and of intermediation. Balance sheet equilibria, he noted,⁵⁹

⁵⁶ Cf. *Critical Essays*, p. 158.

⁵⁷ I have made a previous attempt at getting Hicks's 'Attempt at Perspective' into perspective—and pretty much failed. Cf., Leijonhufvud (1981), ch. 8. My review shows how influenced I then was by Friedman and Schwartz, Brunner and Meltzer, and particularly by their work on United States monetary history since 1929. (In 1968, American monetarists had hardly begun thinking about small, open, fixed-exchange-rate economies yet). This made me critical, for instance, of Hicks's insistence on the 'inherent instability of credit'. The piece also shows my great fascination for Hicks's daring attempt to put 200 years of tangled controversies in order; for various reasons, the way I saw it, several important writers just would not fit neatly into Hicks's scheme—but I failed completely to suggest a scheme that would do better.

⁵⁸ The main line of Hicks's work in monetary theory runs as follows: 'A Suggestion for Simplifying the Theory of Money' (1935); ch. XXIII, 'Keynes After Growth Theory' in *Capital and Growth* (1965); the three chapters on 'The Two Triads' in *Critical Essays* (1967); the chapter on 'Money, Interest and Liquidity' in *The Crisis* (1974); the 60-odd-page-long 'Monetary Experience and the Theory of Money' which is the backbone of the *Economic Perspectives* collection (1977); and 'The Foundations of Monetary Theory' in *Money, Interest and Wages* (1982).

⁵⁹ Quoted from reprint in *Critical Essays*, pp. 75–76.

[are] determined by subjective factors like anticipations, instead of objective factors like prices, [which] means that this purely theoretical study of money can never hope to reach results so tangible and precise as those which value theory in its more limited field can hope to attain. If I am right, the whole problem of applying monetary theory is largely one of deducing changes in anticipations from the changes in the objective data which call them forth. Obviously, this is not an easy task, and, above all, it is not one which can be performed in mechanical fashion.

In our textbooks, Hicks's paper is remembered for a money demand function with which any latter-day monetarist could be comfortable. But, clearly, he was in the Credit tradition from the beginning!

Moreover, it is the neglected themes of Hicks the Younger that the Elder has taken up and carried forward. The first step beyond his 1935 position, came three decades later with the sketch in *Capital and Growth* [1965] of a simple financial system, consisting of a bank, household savers, and firms:⁶⁰

Savers can hold their assets in bank money, or in securities (loans or equities) of the producing firms; . . . Firms have real assets, and they may have bank money; they have debts to the bank, and to the savers. The bank has debts owing to it from the firms; it owes debts (bank money) to the firms and to the savers.

The 'Two Triads' of 1967 introduced the classification of assets into running assets, reserve assets, and investment assets; the specific assets that served these functions would differ between the balance sheets of households, of firms, and of banks; for each type of transactor, the three classes of assets could be matched up with Keynes's Transactions, Precautionary, and Speculative motives; in Hicks's treatment, however, these three were no longer just motives for holding money but for preferring balance sheets of a certain structure. In 'Monetary Experience and the Theory of Money' [1977], the financial structure of Keynes's world was envisaged as three concentric sectors: (1) a banking 'core' with monetary liabilities and financial securities as assets; (2) a financial 'mantle' owing financial securities and holding industrial securities; and (3) an outer 'industry' owning the industrial securities and holding the (hard crust of?) the economy's productive assets (and some financial assets and money). In the 1982 'Foundations of Monetary Theory', Hicks added to this 'monocentric' credit economy model, some analysis also of a 'polycentric' world of multiple central banks (and flexible exchange rates).

What do we get out of this 'Credit' approach that a monetarist supply and demand for 'money' apparatus would not provide with less trouble? Hicks, of course, uses his financial structure model routinely in the analysis of a broad range of questions. In my view, however, the significant advantage of his approach is that it gives a better picture of the financial and monetary consequences of 'real causes': a rise in the anticipated yields on real capital will change the configuration of balance sheets desired by the business,

⁶⁰ *Capital and Growth*, pp. 284–5.

household, and banking sectors; the financing of investment will in part be intermediated by the banks; consequently, an increase in income due to a rise in marginal efficiency of capital will normally be associated not only with a rise in velocity but also with an *endogenous* increase in the money supply.

Hicks's insistence on linking monetary theory to monetary history has been echoed in recent years by rational expectations theorists who insist that we must link short-run monetary theory to *monetary regimes*. These modern writers, however, have come to their preoccupation with the conditional nature of monetary theory from an entirely different angle. Their concern has been to keep track, not of slowly evolving financial institutions and markets, but of rapidly changing nominal (price level) expectations. A 'monetary regime' may be defined as a system of expectations that governs the behaviour of the public and is sustained by the consistent behaviour of the monetary authorities.⁶¹ Since the short-run effects of particular policy-actions, for example, depend upon the expectations of the public, it follows that we need a different short-run macromodel for each monetary regime. A regime change occurs when the behaviour rules followed by the monetary authorities change. This 'regime approach' directs our attention to the history of monetary standards, viewed as methods for controlling the level of nominal prices, and to the system of nominal expectations that would (rationally) go with each such method.

Historically, we find two basic but contrasting conceptions of how price level control can be accomplished. I have labelled them the 'quantity principle' and the 'convertibility principle', respectively. Briefly (and perhaps a bit too simply) we may say that the quantity principle dictates that the government should control the 'quantity of money' while the private sector sets the price level; the convertibility principle, in contrast, dictates that the government set the nominal price of some 'standard commodity' while the private sector determines the quantity of money.⁶² The logically tidiest version of the first would be a fiat standard with flexible exchange rates, and of the second a commodity standard with 'hard money' still in circulation. Price expectations on the fiat standard are almost entirely a matter of beliefs about what the government might choose to do; price expectations on the commodity standard (conditional on the belief that the standard will be adhered to!) are almost entirely a matter of forecasting 'real' business developments.

The two contrasting systems give the extremes on a more or less continuous spectrum of monetary regimes. The last fifty-odd years have taken us from a position rather close to the commodity standard end (in 1929) all the way to the extreme fiat standard end (after 1971). We could proceed to classify macrotheories according to the segment of the regime-spectrum over which they might claim validity.

⁶¹ I have used this rather informal definition repeatedly. Cf. e.g. Leijonhufvud (1983a).

⁶² Cf. Leijonhufvud (1982) and (1983a) for rather more careful explanations.

This classification of theories according to control-regime differs from the Hicksian schema of metallic money theories *v.* credit theories and may be a useful complement to it. This may be seen, for instance, by considering how the American monetarists fit into Hicks's schema. In a metallic money world, money is a produced commodity and thus not neutral; the price level is determined (in the long run) by the cost of producing the metal; the money stock is endogenous and not subject to policy control; the 'mechanical' policy rule is to maintain the metallic standard. The 'mechanical' rule of the monetarists is to fix the growth rate of some '*M*'; it is predicated on the beliefs that '*M*' is neutral and controllable (and 'more or less' independent of endogeneous real factors); the object is to control nominal income in the short run and the price level over the longer run; fixed exchange rates are readily sacrificed to this end. When Hicks includes both Ricardo and Friedman in the same 'metallic' tradition these points of contrast are obscured (even as the contrasts between Ricardo and Friedman, on the one hand, and Thornton and the Radcliffe Report, on the other, are brought into focus). Similarly, Hicks has come to prefer Wicksell's 'pure credit' model (of an economy without 'hard money') as his vehicle for explaining the central theoretical message of the 'credit tradition'.⁶³ But to a monetarist audience, for instance, the main lesson of Wicksell's cumulative process is simply that, on a fiat standard, interest targeting of monetary policy produces *nominal* instability. A model of a system where convertibility anchors the price level—and, therefore, anchors rational price expectations as well—does a better job of fitting credit as a *real* magnitude into monetary theory. It is easier, in such a model, to show both how banking policy can influence investment and employment via the price and volume of 'real' credit and how real income movements can influence the supply of nominal money via the demand for 'real' credit.

Keynesian theory, to take a case in point, seems suited to regimes that behave as if monetary policy were constrained by the requirements of external if not also internal convertibility. The real quantity of money varies endogenously over the cycle in such regimes, nominal price level expectations should be inelastic, and the *numeraire* component of prices correspondingly sticky. This brings us back to IS–LM. Clearly, the old textbook repertory of IS–LM exercises will pass muster much better if interpreted as applying to an economy which retains some significant vestiges of convertible money systems. (An open economy with fixed exchange rates will do, for instance, as long as we are not thinking of the dominant reserve currency country). But the textbook should not have specified '*M*' as a given parameter, controlled by the central bank.⁶⁴ Under convertibility, the

⁶³ Cf. 'Monetary Experience and the Theory of Money', pp. 61–73, and 'Foundations of Monetary Theory', pp. 237, 264 ff.

⁶⁴ On which Hicks can rightly say: '... I may allow myself to point out that it was already observed in 'Mr. Keynes and the Classics' that we do not need to suppose that the curve is drawn up on the assumption of a given stock of money. It is sufficient to suppose that there is (as I said) 'a given monetary system—that up to a point, but only up to a point, monetary

monetary authorities do not have the powers to regulate nominal income assumed by Friedman or by Lucas. The Keynesian picture (of LM shifting, IS staying put) of relatively modest effectiveness of monetary policy, transmitted via the price and volume of credit, is nearer to the mark.

Keynesian theory should do fairly well, I have argued elsewhere,⁶⁵ as long as the monetary system still resembles the kind of system which Keynes strove for as a monetary reformer. Its lack of attention to inflationary expectations was on the whole appropriate to the Bretton Woods world. When the last vestiges of Bretton Woods were swept away, its neglect of inflationary expectations became a critical flaw. We should not have been so surprised!

Conclusion

In some quarters, Hicks is routinely blamed for the paths we have taken from his path-breaking early contributions. Those who do so blame him have not studied him very closely. 'One of the best reasons for studying the elder Hicks, in fact, is precisely that he is less a prisoner of the younger Hicks's constructions than are most of us.'⁶⁶ Among the lessons that Hicks the Elder would impress on us, I have tried to bring out two:⁶⁷

One must assume that the people in one's models do not know what is going to happen, and know that they do not know just what is going to happen. As in history!

Monetary theory, especially, has to be developed 'in time [with] future becoming present, and present becoming past, as time goes on'.⁶⁸ And 'it belongs to monetary history in a way that economic theory does not always belong to economic history.'⁶⁹

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authorities will prefer to create new money rather than allow interest rates to rise. Such a generalized (LM) curve will then slope upwards only gradually—the elasticity of the curve depending on the elasticity of the monetary system . . .'. Cf. *Money, Interest and Wages*, p. 328.

⁶⁵ Cf. Leijonhufvud (1983a).

⁶⁶ Leijonhufvud (1979), p. 526.

⁶⁷ *Economic Perspectives*, p. vii.

⁶⁸ *Ibid.*

⁶⁹ 'Monetary Theory and History', in Hicks (1967), p. 156. But this too is an old Hicksian theme. One finds it in his 1943 review of Charles Rist's *History of Money and Credit Theory*. Cf. Hicks (1982), pp. 132ff.

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