

***The Impact of Keynes on
Economics in the 20th Century***

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Mr Keynes and the Moderns ¹

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1. Classical vs. modern theory

For great minds, the greatest headaches come after death.' Thus, I am told, reads the first sentence of the Introduction to the Serbo-Croatian translation of my book *On Keynesian Economics and the Economics of Keynes*. I'm afraid it may be much the most interesting thing ever said about it. The Introduction went on to explain that, as in the case of Marx (of course!), Keynes's writings had given rise to an immense posthumous literature of debate, criticism and controversy. Both these collective headaches have pretty much subsided by now. Hardly anybody under the age of 50 seems to feel them at all. Perhaps my generation will take the Keynesian headache into the grave with us!

The Keynes debate is now 60 years old. For decades, we debated what Keynes had done to the classics. It was generally agreed that his *General Theory* had wrought a 'revolution' in macroeconomic thinking but no general agreement was ever reached on what specific idea or ideas made it so revolutionary -- even though the 'Keynes and the classics' debate eventually spawned a literature so vast that it can no longer be surveyed.

It seems time to discuss what the moderns have done to

Keynes.² To the generations that come to the *General Theory* today, it is harder than ever to understand. Not that attempts to understand it are encouraged. Many prominent economists at present consider Keynes's work so deeply flawed, riddled with error, even, that it need no longer be studied. Indeed, the opinion is widespread that the Keynesian revolution in macroeconomic policy was an unfortunate interlude that has now been overcome.

The intellectual obstacles to understanding Keynes are greater today than previously, and greater the more up-to-date is the training of the reader. The evolution of economic theory of recent decades has taken us away from the tradition to which Keynes belonged. The conceptual foundations of the *General Theory* have become alien.

Keynes belongs in that great tradition of adaptive and evolutionary theory that stems from the British Classical School. This classical tradition must be distinguished from the modern one, whose hallmarks are optimizing choice and equilibrium. Admittedly, this terminology is a bit unusual: the new classicals are denied the 'classical' label and put into the modern category, where they have to cohabit with new Keynesians and sundry others whose company they usually shun, while Keynes himself becomes the last of the great classical theorists. His *General Theory* is properly understood as a generalization of classical theory. It is misunderstood - it *cannot* be understood - as a special case of modern theory. This last proposition - that it cannot be understood that way - is one for which

we do not lack evidence.

British classical theory sought to deduce the *laws of motion* of society (see Table 2.1). The basic behavioural propositions are couched as verbal differential equations of the type: 'The population will grow as long as the real wage exceeds subsistence' or 'Capitalists will continue to accumulate as long as the rate of profit is positive'. It is 'magnificent dynamics', as Baumol termed it long ago (1951). Behaviour is adaptive. The proposition that 'agents maximize utility or profit', for example, states an assumption about motivation, not realization. Transactors are thought of as striving to 'climb utility mountains', not as lolling forever on their peaks. They are not immune to backsliding. Firms may age, get comfortable and inefficient and go under.

Table 2.1 The two traditions

	<i>Classical</i>	<i>Modern</i>
Objective of theory	Laws of motion of the system	Principles of efficient Allocation
<i>Characteristics</i>		
Individual motivation	Maximize utility or profit (intent)	Maximize utility or profit (performance)
Individual behaviour	Adaptive (often gradient-climbing)	Optimizing choice
Cognitive competence	Capable of learning	'Unbounded'
Role of institutions	Well adapted 'locally' Essential in guiding behaviour, making	Problematic: why use money?

	behaviour of others predictable	Why do firms exist?
Equilibrium concept	Constancy (point-attractor)	Mutual consistency of plans
<i>Classical examples:</i>	Marx, Marshall, Keynes	
<i>Modern examples:</i>	Arrow-Debreu or Lucas	
<i>A foot in each camp:</i>	Hicks, Samuelson	

In a social context, gradient climbing is a trial-and-error (feedback) process -- the individual acts or, rather, he interacts with others, evaluates the outcome, and then tries to improve on past performance. This feedback-governed process is guided by the social and legal rules under which people interact. What the decision maker learns depends on the nature of prevailing institutions and market organizations. Institutions shape the fitness landscape in which agents move and they will not do equally well independent of the rules that govern their interactions. That their cognitive competence has its limitations is taken a matter of course in classical theory. The actors on the classical stage *learn as they go* and become pretty smart in contexts with which they are familiar. But they depend on the guidance of the 'invisible hand'. Market outcomes convey new information to some participants -- and occasionally to all participants -- and make them adapt their behaviour to their changed understanding of the environment. The

information required for (local) maximization is produced and discovered in the course of market interaction. This type of theory is 'dynamic' in the sense that the order in which things happen and, in particular, the sequencing of decisions matter. The act of one agent provides the information on which another will act. The market process is 'in time'.

Modern theory focuses on the logical principles of efficient allocation. Behaviour is optimizing *ex ante*. If in classical theory, agents learn as they go, here they know it all and know it in advance, that is, they know all that they can know and need know in order to deduce all utility-relevant consequences of alternative courses of action. Consequently, they are *not dependent on information flowing through the communication channels of the market system*. The only 'new' information reaching them is in the nature of the outcome of lottery tickets, but this will not change their plans because the odds of all lotteries are known and unchanged. Their cognitive competence we characterize as 'unbounded rationality' and consequently the statement that they maximize utility or profit is taken to be a proposition about realized performance, not just intention or ambition. In this theory, institutions become problematic rather than essential. What are they doing there? Why do people use money? The answers are lamentably unpersuasive. Why do firms exist? Must be some kind of market imperfection ...³

2. The growing influence of modernism

Marx, Marshall and Keynes are examples of theorists in the classical tradition. Arrow, Debreu and Lucas are obvious examples of moderns in my sense. We sometimes label

them as neo-Walrasians but, although the analytical line of development from Walras is clear, it is not so clear that Walras himself was an 'early modern'. He surely thought of himself as concerned with the laws of motion of economic systems. Certainly, that other great Lausanne School protagonist, Pareto, was in the classical tradition. His consumers 'climb utility mountains' and can be depended upon to be 'rational' only in contexts with which they have had much experience and where action-cause and utility-effect are linked in a simple and direct manner.

None the less, the germ of modernism is in Walras. In successive editions of the *Elements*, he sharpened the assumptions surrounding the *tatonnement*, prohibiting 'false trades' and introducing the conditional *bons*,⁴ in order to avoid hysteresis in price formation and thus to guarantee that the initially planned, optimal trades would remain feasible. He may have thought of this as a relatively harmless way to clean up a somewhat messy technical detail. But this insistence on market participants being able to plan on the basis of parametric prices and on plans being reconciled in advance of any commitments became, not a detail, but the architectural design of the central modern edifice.

Among modern forerunners, Slutsky (1915) deserves to be singled out for having formulated optimal choice theory in clearly modern terms. But the growth of modern influence really starts with the pair of articles by Hicks and Allen (1934) that prompted the rediscovery of Slutsky. From that point onward, the modern manner of

constructing models spread gradually. But the balance between classical and modern modes of theorizing shifted quite slowly. The modern mode of 'thinking theory' only became totally dominant with a generation of economists whose training consisted almost exclusively in the learning of modern models.⁵

Classical modes of thinking theory and the modern manner of building models coexisted for decades without causing pronounced intellectual discomfort. Hicks and Samuelson were the great teachers of my generation. Both of them kept a foot in each camp. The first half of Samuelson's *Foundations* (1947) was thoroughly modern, the second half classical. Many years later, in his Nobel Lecture, Samuelson switched gear with the same mental ease, dropping optimization midway in the paper only to take up the accelerator as an 'example of a dynamical system that can in no useful way be related to a maximum problem' (1970, p. 13). Keynes's consumption multiplier could have served as another example.

The case of Hicks is more interesting. Part I of *Value and Capital* built the *atemporal* general equilibrium model in a purely modern manner. The rest of the book attempted a hybrid construction. Hicks started down the road of modern intertemporal general equilibrium constructions by introducing future-dated goods. But he shied away from closing the model with an intertemporal general equilibrium condition that would force all decisions to be made and to be reconciled at the origin of time. Instead, he followed Erik Lindahl in conceiving of the economy as in the course of time passing through a sequence of temporary equilibria, in

which all actually existing markets clear but with results that make transactors update their expectations and revise their plans. This temporary equilibrium method was an attempt to find room for classically adaptive, learn-as-you-go behaviour within the modern general equilibrium frame. The modern chapters (I-VIII) of that work had enormous influence, the hybrid construction very little.

Elsewhere I have written at length about Hicks's increasing intellectual discomfort with the tension between modern and classical ways of thinking and his struggles later in life with theories that are 'out of time' versus those that are 'in time' and capable of accommodating history (Leijonhufvud, 1984, 1994). Hicks may have been virtually alone in the importance that he came to attach to these issues. For most economists, this one-foot-in-each camp stance between classical and modern ways of thinking remained perfectly comfortable up through the 1960s and into the 1970s. This ambivalence is the intellectual background against which one has to understand the postwar debates over Keynes's contribution.

The common core of micro-theory, the price theory that was taught everywhere, was really atemporal. In retrospect, this was an important aspect of the situation. These static models had two more or less interchangeable interpretations. The conditions of rest for Marshall's adaptive agents were indistinguishable from the optimality conditions of Walras. In one interpretation the solution was seen as the point-attractor of a dynamic process of interaction among adaptive agents. In the other

it showed the simultaneous satisfaction of multiple optimizing transactions plans. Teachers would move back and forth between the two interpretations with the greatest of ease, using the one to add intuitive appeal to the other.

For someone with Paul Samuelson's mathematical equipment, it was obvious that the two ways of interpreting static models were not really interchangeable in this way. But they *ought to be complementary*. The particular 'correspondence principle' that Samuelson proposed was soon shown not to hold in general. But from a historical perspective, this is not the important point about it. The point, rather, is the motivation that Samuelson gave for asserting a correspondence principle in the first place, namely, that for the (comparative) static equilibria of modern construction to be '*meaningful*', they must first be shown to be the attractors of some classical dynamic process.

3. The Keynesian debate in the 1960s

All of the above is steeped in the wisdom of hindsight. Or in hindsight, in any case. This is not how I understood matters while working on my dissertation in the 1960s. I was very much troubled by the problematic relationship between the microeconomics that I had been taught and the macroeconomics that I had been taught. In micro, as I have just explained, the distinction between classical and modern approaches was generally sloughed over. But if you wanted to think of the system as a whole, there was only one way to go - to general equilibrium theory. The macroeconomics was supposed to be 'Keynesian' but was in fact already half modern, for all the new components that had been inserted in

the structure were of modern construction -- the Modigliani-Brumberg life-cycle theory of saving, Jorgensen's investment function, Tobin-Markowitz portfolio theory, and so on. The IS-LM model, in terms of which the discussion was couched, was another Hicksian hybrid.

Clower was the first one to see the great rift that was opening up in the general fabric of economic theory through its progressive 'modernization' and he succeeded admirably in dramatizing it for all to see in his 'Keynesian counter-revolution' paper (Clower, 1965). He was the first to see the rift, I think, because much of his work up to that time had dealt with stock-flow models and price and quantity dynamics in more general models, which is to say, he had in fact been working in a classical vein.⁶

I arrived at the gulf between Keynesian macro and Walrasian micro from a completely different direction. Like so many others, I wanted to understand what made the Great Depression so much worse and so much more dangerous to the Western world than ordinary recessions. I wrote a proposal for my dissertation work which described a deviation-amplifying *process* in which an initial decline of prices raised the real value of debt contracts to a level which neither debtors nor creditors would willingly have chosen; subsequent attempts by creditors to collect and by debtors to pay off debt would increase the excess supply of goods and services; the price level would then fall further, and so on. It was a year and a half before I learned (from David Meiselman)

of the debt-deflation theory of Irving Fisher. The illusion of originality was shattered, but by then I was already totally preoccupied with another question, namely 'Why was the debt-deflation hypothesis *impossible* to accommodate in any of the macro-models that I had studied?' What assumptions about the knowledge possessed by agents would be necessary to validate the usual, not to say universal, procedure of consolidating the balance sheet of the private sector? Stocks of capital could not generally be worth the same independently of who controlled them or cumulative debt crises would seem impossible, for example. From there, I was led on to question all the information assumptions of Walrasian models -- which came to include the 'auctioneer' and the prereconciliation of plans. From this standpoint, not much of what I had learned in graduate school held water. But Keynes, I found, made sense -- as at a deeper level did Hayek (who was definitely not read in graduate schools at that time).

There are not many obvious traces of the debt-deflation problem in my 1968 book,⁷ but my particular path to Keynes explains why I discuss information assumptions so much and why I also differ from Clower in regarding the intertemporal coordination issue (the saving-investment business) as the core problem in Keynes as well as central to macroeconomics in general.⁸

Neither Clower nor I set out to interpret Keynes. We were concerned with the situation in economic theory in the 1960s, not with what it had been in the 1930s. The issues of the 1960s eventually disappeared from discussion, of course, as the new classical victors resolved, in effect, that the conceptual

tension between classical macro and modern micro could be entirely removed by agreeing to build macroeconomics according to thoroughly modern rules. What remains, then, is history of thought and, judged as such, my book is pretty uneven.

4. Keynes as a Marshallian

My failure to see the issues clearly in classical versus modern terms was both a strength and a weakness of *On Keynesian Economics*.⁹ It was a strength in the short run, because it meant that I addressed the intellectual situation as many other economists came to experience it at that time. So, the book was a success. The weakness shows up in various parts of the argument (some of which I shall come to) but had its most important consequence in that the way in which I (and Robert Clower) couched the 'micro-foundations of macro' problem was in some degree responsible for the attempts by Barro and Grossman, Benassy, Malinvaud, Frank Fisher and others to construct 'Keynesian' models on neo-Walrasian optimizing foundations. This did not seem a promising way to go,¹⁰ and I took no part in this development. The more recent 'new Keynesian' development is another attempt to make Keynesian theory palatable to the modern taste.

Keynes's price theory was Marshallian ('classical'), not Walrasian ('modern'). When Joan Robinson read my book which she was about to review, she sent me a sequence of short notes reacting to this and that. One of these, as I recall, asked in essence: 'Why do

you bother with all this Walrasian stuff?' To which I counter-attacked by telling her that it was necessary to put Keynes's argument in some understandable relation to the theoretical framework of one's readers and that she and the Cambridge Keynesians failed to have an impact on most American economists (that is, the 'modern' ones) because they did not go to the trouble to do so.¹¹ But, in fact, I was only then beginning to understand how deep the differences between the two approaches ran. By the time I was invited to give the Marshall Lectures in 1974 I did understand, and chose to devote them entirely to these issues.¹²

So, the classical economics from which Keynes waged his 'struggle to escape' was Marshallian, not Walrasian. When I confessed this in print (Leijonhufvud, 1974a), Paul Davidson concluded that I had more or less recanted my interpretation of Keynes. Actually, while I did not do justice to the theory that was Keynes's point of departure, I still think that I understood the *General Theory* correctly in all essentials and that explaining that theory (as far as possible) in 'moderns' terms was a worthwhile thing to do. So I do not concede very much. Yet, I cannot deny that it is easier - more natural - to 'get to' Keynes from a classical adaptive/evolutionary starting-point than from a modern optimizing/equilibrium one. So let me sketch how that would go, starting from a Marshallian conception of a single market in isolation.

Marshall's agents are adaptive, not *ex ante* optimizers. At any one point in time, most of them will be 'in motion', in the process of adjusting. Households are increasing their purchases wherever their demand prices (calculated at their

realized income) exceed the market price, cutting back where market price has risen above their demand price. Producers are expanding output when the market price exceeds their supply price, contracting in the opposite case. Middlemen traders are adjusting prices upward when sales are depleting their stocks, downward when deliveries pile up unsold in inventory. And so on.

A modern reader realizes at once that the system of interacting agents thus described might very easily exhibit complex, perhaps chaotic dynamics. Like his British classical predecessors, Marshall could not deal analytically with systems of partial differential equations but had to assume, as an article of faith, that the processes investigated would go directly to some well-defined point-attractor. But Marshall had a trick that allowed him to deal with problems of a good deal more practical relevance than the long-run stationary state (of Carlyle's 'dismal science'). The trick was the ranking of adjustment speeds: let prices move qualitatively faster than output rates, and output rates faster than the adjustment of physical stocks. The result is a hierarchy of point-attractors: the market-day, short-run and long-run equilibria.

The Marshallian equilibrium concepts are defined by the constancy of some *observable* (realized) variable, not by the mutual consistency of all plans. The market-day equilibrium condition has the rate of change of price equal to zero, *conditional* on output being held constant. The short-run equilibrium has the rate of change of

industry (aggregate) output equal to zero, conditional on stocks of capital stocks held constant.

So here is one respect in which it is easier to understand Keynes by starting from Marshall. In Walrasian models, 'unemployment equilibrium' is a contradiction in terms since the equilibrium concept requires the consistency of all trading intentions. Unemployment theory has to be disequilibrium theory. From a Marshallian standpoint, there is no riddle. A state of the economy such that the rate of unemployment at that point in time has a zero time-derivative qualifies as an unemployment equilibrium. It may be conditional on the wage rate 'held constant', but this does not mean 'rigid wages' any more than Marshall's supply is 'rigid' because it is 'held constant' on the market day; it merely means that it is adjusting qualitatively slower than employment.

Adaptive agents act on feedback, which is to say on realized magnitudes. Marshall's consumer spends income already earned. Demands are based on realized income. Clower's 'dual-decision' hypothesis, which fits so ill in general equilibrium theory, would have been, for Keynes, the natural way to think of household behaviour.

When Marshall's competitive industry is in short-run equilibrium, his representative firm is at rest, its supply price equal to the market price. But this should be interpreted in *ex post* terms: the representative firm will not intend to change its rate of output when the marginal cost *incurred* equals the price that was *realized* in the market. This adaptive conceptualization of the behaviour of a firm with no market power frees the analyst from much contrived baggage. Marshall's competitive firm does not face a horizontal demand

curve *ex ante*, so its equilibrium does not require assuming a continuum of agents, homogenous products, centralized price formation, or prohibition of 'false' trading. The theory of imperfect competition was invented to escape from these assumptions. It attracted much interest in the 1930s, not least in Cambridge. But Keynes did not need it and made no use of it.

The Marshallian market does not have an exogenous 'auctioneer' setting a unique price at which one side or the other is 'rationed' when it isn't right. The collective trial-and-error process whereby the market 'grope' for the equilibrium will normally involve disappointments of the expectations of some participants. But the market process is not some intermittently interrupted *tatonnement* during which only short-side-dominated realizations would be observed. Marshallian firms have to make output decisions without knowing what price the output will eventually fetch. When they produce a larger-than-equilibrium output, they find themselves on the 'long side' of both the output and the labour-input markets.

So, 'getting rid of the auctioneer' ¹³ was (obviously) a big part of my own 'struggle to escape' from the neo-Walrasians, but it cannot have been a problem that Keynes had with his own classical mentor, Marshall.

5. A generalization of classical theory

Against this Marshallian background, we may now

picture Keynes's situation as follows.

He wanted to understand the emergence of persistent, large-scale unemployment and what could be done about it. The desired explanation should run in terms of all agents obeying simple, understandable, robust 'directional' rules of adaptation: 'If marginal cost is not covered by the price received, reduce output and employment'; 'If the rate of sales is lower than desired, reduce the asking price', and so on. These behavioural rules should apply to *all* agents, including workers. Thus, reservation wages should respond to unemployment in the same way as all other prices respond to excess supply.

If some agents did not behave according to these rules of competitive behaviour, or were prevented from doing so, it was obvious -- and, consequently, not very interesting -- that the system might fail to home in on an attractor where all markets cleared. Hence Keynes eliminated all such *intentional* obstacles to adjustment from his inquiry, putting them into the category of 'voluntary' unemployment (*General Theory*, pp. 6-10). That left 'involuntary unemployment' (*ibid.*, pp. 15-16) as the *explanandum*. This choice of terminology has proved to be extremely unfortunate, of course, since the notion of 'involuntariness' is basically incomprehensible within choice-based modern theory. But Keynes could not have anticipated what terminology economists would come to favour decades later.

In my 1968 book, I argued not only that Keynes dispensed with the auctioneer but also, to my frequent regret, that he 'reversed Marshall's ranking of the adjustment speeds' of price and output. To a graduate student in the early 1960s (or at

least to this one) that seemed an understandable way of getting from the auction-market environment of modern theory to something with a closer resemblance to Keynesian economics.¹⁴ But Keynes did not need to modify Marshall in this respect. When Marshall's firm decides whether to expand, contract or stay put, it takes its marginal-cost schedule and, therefore, the price of labour, as given. So in Marshall, as in Keynes, wages adjust less rapidly than output rates and prices either as fast or faster than output rates. Both Marshall, throughout his life, and Keynes, after the stabilization of the post-First World War inflations, presumed a monetary regime that would make expectations of nominal stability rational, so neither made much of the distinction between nominal and real wages in this context.

The new problem was to analyse how a dynamic system of multiple markets would move. It is at this level that we find the radical discovery of Keynes that entirely changed the then prevailing presumptions about the self-regulating capabilities of a 'free' market system, namely, 'effective-demand failures': an excess supply in one market does not necessarily have a counterpart in an excess demand elsewhere. Hence, the contractionary impulse in one part of the system need not be offset by an expansionary stimulus elsewhere.

The feedback of realizations that guides the behaviour of adaptive agents is shaped by the institutions within which these people interact (Table 2.1). In a financially mature capitalist economy, saving normally

does not take the form of capital accumulation by the saver. Nor does the saver place a forward order for consumer goods. Estimates of future demand are thus not firmly grounded and investment expectations may, therefore, show occasional instability. Financial intermediaries and capital markets intermediate between savers and investors, so that central bankers and bears or bulls may interfere with the intertemporal coordination of consumption and capital accumulation. Thus the first potential communication failure of the *General Theory* is that neither 'a fresh act of saving' (*General Theory*, pp. 210ff.) nor a decline in investment will create an effective excess demand for future-dated goods to which the system will adapt (see Table 2.2).

Table 2.2 Effective-demand failure: intertemporal

Assume $r > r^*$ so that $S(X^*, r) > I(r)$

	Present-dated goods	Future-dated Goods	Adjustment tendency
Walrasian market structure	Excess supply	Excess demand ('notional')	$drl/dt < 0$
Keynesian market structure	Excess supply ('effective')	Zero	$drl/dt = 0$

The saving-investment analysis of aggregate demand, which used to be taught as the core of Keynesian economics in its early days, is best done in *real* terms: If X^* is 'full

employment output', and $S(X^*) > I$, the result is an aggregate-demand deficiency at full employment. The economy will respond to such a demand deficiency by output rather than price-level adjustment. The *real* rate of interest (that is, the relevant intertemporal *relative price*) has failed to coordinate intertemporal allocation decisions in the economy. When saving exceeds investment at the natural rate of unemployment, the market real rate of interest also exceeds the natural real rate of interest. Behind this failure of real interest rates to adjust so as to coordinate intertemporal decisions will be the decisions in some sectors of the economy to build up or to restore liquidity positions. The induced decline in real income will proceed to the point where the amount of saving attempted no longer exceeds ongoing investment. At that point, there will be no excess demand for future-dated goods to exert pressure on the interest rate to fall. Thus, the intertemporal disequilibrium may persist.¹⁵ Of all prices in Keynes's theory, the (long-term) rate of interest is the slowest adjusting: 'It may fluctuate for *decades* about a level which is chronically too high' (*General Theory*, p. 204, italics added).

In the belated Volume XXIX of Keynes's *Collected Writings*, one finds drafts of introductions to the *General Theory* which Keynes later abandoned.¹⁶ These drafts provide much the most convincing evidence that Keynes understood very clearly that these institutional assumptions were crucial in determining what basin of attraction the adaptive multi-market system might settle into - that it is not enough to know that the system being

analysed has n goods and m transactors (Leijonhufvud, 1968a, p. 398). He contrasts four hypothetical systems, of which the ones of most interest are called the 'Cooperative' and the 'Entrepreneurial' economy, respectively. In the Cooperative economy, the supply of labour is an offer to exchange labour services for the firm's output. (One imagines that the firm produces a broad basket of consumption goods.) The cooperative firm tries to maximize the amount of output it can sell after labour has received its share. In such an economy, 'involuntary unemployment' cannot occur since the offer of labour constitutes an effective demand for output. In the entrepreneurial economy, firms specialize in production and maximize expected profit, the supply of labour is a demand for money wages, and money has to be earned before effective demand for wage goods can be exercised. This is the institutional setting in which involuntary unemployment may occur (Leijonhufvud 1988, 1996b).

Assume (Table 2.3) that the money wage is at the level that would prevail if the economy were in general equilibrium: $w = w^*$. But $r > r^*$, so investment is less than required to provide full employment at that wage. Workers laid off from the capital-goods-producing industries will have to reduce their consumption spending, causing a secondary reduction in employment. This is Keynes's multiplier process. If the offer of labour were an effective excess demand for consumer goods, this would not happen. Instead, the demand price for consumer goods would hold up even as the money wage would fall, so that real wages would decline, stimulating employment. With the institutions of the Keynesian 'Entrepreneur' economy, labour cannot bargain directly for

real wages (*General Theory*, esp. chapter 19, appendix). Instead, labour services have to be sold for money before that money can be used to exert effective demand for consumer goods. In the model of the *General Theory*, competition drives down consumer goods prices even further than wages as employment contracts. Hence, real wages would actually tend to rise in recession.¹⁷

Table 2.3 Effective-demand failure: 'involuntary unemployment'

Assume $r > r^*$ (as in Table 2.2) while $w = w^*$. Assume also that persons unemployed are without liquid assets or ready credit.

	Consumer goods	Labour services	Tendency Adjustments
Walrasian market Structure	Excess demand ('notional')	Excess supply	$d(w/p)/dt < 0$
Keynesian market Structure	zero	Excess supply ('effective')	$d(w/p)/dt > 0$

In Keynes's theory, therefore, the ongoing mutual adaptation of agents in a money-using system can, under certain conditions,¹⁸ lead to effective-demand failures. These cause the adaptive market processes to go off course and produce that unintended outcome of social interaction which Keynes termed 'involuntary unemployment'.

6. How the Moderns do away with Keynes

Why is this theory so seemingly incomprehensible to economists with modernist schooling? The brief answer is that in learning to perfect the construction of equilibrium models based on explicit optimizing foundations, economists have also unlearned the mental habits of adaptive and evolutionary analysis.

The peaceful coexistence, on almost equal terms, of classical and modern modes of theorizing, which prevailed up through the 1960s, did not last much beyond that. Why modern theory came to displace classical theory almost altogether is too big and complicated a story to be attempted here. Some of the reasons are fairly obvious, however.

First, it was gradually discovered that no simple and general 'correspondence' between adaptive and optimizing models could be demonstrated. The simple faith of the classical tradition that everything converges to point-attractors was unfounded. Economists were not equipped to handle complex adaptive systems like the multi-market economy Keynes had outlined. Even the lower-dimensional models of Richard Goodwin, for example, produced dynamical complexities that went to the limits of what could be done with analytical methods. Even assuming adaptive behaviours of the most simple-minded nature would lead straight into a discouraging morass of technical complications and dynamic possibilities -- and if the analyst managed to get through that, his results would then be open to the objection that people aren't as stupid as all that! The quest for general equilibrium stability theorems of more empirical relevance than the

tâtonnement gross substitution case was eventually just abandoned.

On the other side, the intertemporal generalization of modern theory was a critical development. It has greatly advanced our understanding of capital theory and finance theory over recent decades. Macroeconomists outside the new classical fold, who may chafe at the Arrow-Debreu model as a straitjacket when it comes to analyze processes, have probably forgotten what these two fields were like before this framework was available. Where the adaptive systems approach seemed to promise 'nothing but trouble', this generalization of theory built on optimizing behaviour produced an almost endless series of good problems for new generations of technically trained economists to work on.

But generalization to intertemporal optimizing had undesirable consequences as well. The logic of optimizing choice is essentially timeless. It does, however, require *all* the utility-relevant consequences of alternative decisions to be taken into account. When the step from an atemporal to a temporal context is taken, this comes to mean all possible alternative futures for all time. There is no palatable way of truncating the time-horizon over which the agent is supposed to plan or of otherwise reducing the dimensionality of the space he must consider. To have a determinate choice problem, the analyst must specify the agent's opportunity set in all the relevant dimensions, which means attributing to the agent knowledge of all the corresponding information. Thus,

intertemporal optimization constantly forces the economist to make information assumptions which are unreasonable.

The information required for the individual optimization problem to have a solution includes the equilibrium prices for all future (contingent) markets. *Everyone's choices have to be reconciled before anyone's choice can be made!* The modelling strategy, as John Hicks has put it (1977, p. vii), does 'deliberate violence to *the order* in which in the real world (in *any* real world) events occur'.

It is really the order in which *decisions* are made that is the crux. It is a question of whether in making his own decision A can know what B has decided to do so that he, A, can adapt to it. This is why, in discussing the treatment of time in the works of Hicks, I suggested that it would be a good idea to change his famous definition of dynamic analysis as dealing with problems where '*goods* have to be dated' to read problems 'where *decisions* have to be dated' (Leijonhufvud, 1984, p. 30 n). In Arrow-Debreu models, as everyone knows,¹⁹ there is no such *order* in which decisions are made. They are all made at the origin of time, presumably in the wake of some sort of recursive *tâtonnement*²⁰ to find the equilibrium prices for all markets, present and future.

Rational expectations is the modern defence against the Hicksian accusation of doing 'deliberate violence' to the order in which things have to occur. In rational expectations models, agents are supposed to have no need for the information that would be produced by market interaction because past experience enables them to predict what it will be. No need to learn as you go, therefore. In fact, it does not matter any more whether or not the intertemporal

markets exist to transmit price information. For those that do not exist, we simply substitute the rationally expected prices. And the learning of these rational expectations themselves is by convention relegated to the past as transient dynamics that (one presumes) have long since died down.

The Keynesian effective-demand failures are *communication failures* occurring in a system where people adapt their behaviour to the information they currently receive. When these communication failures occur, the complex adaptive system moves into socially unattractive basins of attraction. In intertemporal general equilibrium models, this kind of ongoing adaptation to the behaviour of others does not take place. Consequently, potential failures in information transmission are of no relevance and need not be studied.

In short, the moderns have done away with Keynes.

7. The relevance of Keynes today

Does it matter?

For the younger generation of macroeconomists nowadays, *not* understanding Keynes seems a necessary, if not sufficient, condition for professional advancement. Explaining Keynes as the fast great master of a classical tradition of dynamic models built from very simple adaptive behaviour postulates, as done above, will not change many minds, moreover -- for 'irrational' adaptive behaviour is exactly what they thought was

wrong with Keynesian economics in the first place. Are they missing something of practical relevance?

There is one story that is today widely believed to encapsulate all you need to know about the history of macroeconomic thought. It runs approximately as follows:

- Keynesian economics was all about rigid nominal wages and the stability of the Phillips curve;
- these tenets of Keynesianism were based on behaviour postulates, which meant that people were irrational and persisted in making costly errors even in simple and transparent situations;
- these propositions were what undergirded Keynesian beliefs in the usefulness of macro-policy to regulate real activity;
- Muth had shown that people who learned not to repeat the same mistakes would have rational expectations, and Lucas demonstrated the implications of rational expectations for the Phillips curve;
- the fact that inflation has obviously shifted Phillips curves everywhere proves conclusively that Keynesian economics was fundamentally flawed and that new classical economics is correct.

Now, there are various things wrong with this story as history of thought. Keynesian economics flourished for some 25 years without any Phillips curves. It was a late-arriving excrescence. Moreover, Phillips himself did not think that the patterns he had found in the data would persist under inflationary conditions. The econometrics of Phillips curves were shaky from the start and the 'curve' never had an

understandable theoretical foundation. So there were many disbelievers before the rational expectations revolution (for example, Leijonhufvud, 1968a). But there is no purpose in taking up that cudgel.

The point, rather, is that something got lost in the Phillips curve debate. Recall that Friedman, in his attack on the supposed Phillips trade-off, postulated the existence of a 'natural rate' of unemployment, the level of which was determined basically by 'frictions' in the labour market. He then proceeded to argue that the observed pattern of 'Phillips points' could be explained as departures from the natural rate caused by accelerations and decelerations of the money stock and hence of the price level. Lucas's strategy was the same except that in his version the deviations from the natural rate were caused by 'unanticipated money'.

Note the assumption that has been smuggled into the theory: in the absence of monetary shocks, or else as soon as money wages have had time to adjust to past monetary shocks, the economy will settle down to a unique level of unemployment determined solely by supply-side frictions! A great many economists discuss unemployment problems today 'as if' the inflationary instability of the Phillips curve had provided conclusive empirical confirmation of this belief about the world.

It should be obvious what is wrong here. Suppose, for simplicity, a system with rational expectations and 'super-neutral' money, so that the system of nominal values is basically 'orthogonal' to real magnitudes. Suppose that the volume of unemployment would show

some variation over time even in the absence of exogenous nominal impulses (anticipated or unanticipated). This would leave a pattern of points in Phillips space. In the hypothetical world of super-neutrality, this entire pattern of Phillips points, whatever it might look like, would be displaced vertically by anticipated inflation. The space would fill up with 'vertical Phillips curves'. To assert that fully anticipated inflation collapses the scatter to a single, unique and stable vertical Phillips curve is a completely different proposition.

What is wrong with it from a Keynesian standpoint? I recall what was probably my very first lecture in macroeconomics from Professor Erik Lundberg in Stockholm 40 years ago. Lundberg drew a then-familiar version of the Keynesian cross, showing a horizontal investment schedule and an upward-sloping saving-income relation intersecting *below* 'full employment' income. He then taught us the usual arguments for taking the saving = investment condition as determining the level of real income and employment. What was not normally said in this connection back then was that the proposition that 'saving exceeds investment at full employment' has the Wicksellian translation that 'the market rate exceeds the natural rate of interest'. In other words, *intertemporal prices are not right for the system to be in intertemporal equilibrium*.

Seen from the standpoint of Keynes's theory, therefore, the natural rate of unemployment doctrine is founded on the implicit assumption that we are dealing with a system that is always in intertemporal equilibrium. Now, this is in fact the direction in which new classical monetary theory (and later real business cycle theory) has developed. But it is a fairly

large step from the proposition that people will learn to make good forecasts of inflation one period ahead (if the inflation is generated by a stationary stochastic process) to the assertion that we live in a world of perfect intertemporal coordination. Certainly, the instability of Phillips curves does not by itself lend any support to this daring notion.

Much of the discussion of European unemployment today seems predicated on the notion that any unemployment rate that has lasted for more than a year or so must perforce be 'natural'. And that is supposed to mean that all that can be done about it is to exhort workers to be more 'flexible' and to help them along by deregulating labour markets. Even in the absence of any sign that additional flexibility is having a noticeable desirable effect, this remains the refrain.

It may well be that the governments of most European countries have allowed their finances to deteriorate to the point where any attempt to stimulate aggregate demand will only cause inflation and exchange depreciation without any noticeable effect on real output and employment. But that is a very different proposition from the belief that economic science has 'discovered' that aggregate-demand management is and always was a chimerical idea.²¹

Notes

1. Invited Lecture at the Meetings of the European Society for the History of Economic Thought, Marseilles, 28 February 1997. I thank Jean-Paul Fitoussi for helpful discussions and Ingo Barends for

saving me from error on a point where I should have known better.

2. The reader should be warned that I carry a dull axe that could stand some grinding. I made my own professional debut (1968) with a book on Keynes midway in this 60-year span and my conception of the economics of Keynes has suffered much the same fate as other versions of Keynesian economics in this modern age. Very shortly before this lecture, I came across Roger Backhouse's book, *Interpreting Macroeconomics* (1995). I opened it at random. And there on that random page (p. 207) was the Rise and Fall of Leijonhufvud in black and white -- a graph showing a very hump-shaped time-profile of citations contrasted to the monotonic rise of Muth's!

3. The firm-as-market-failure theory overlooks the fact that the markets are themselves created and operated by firms (Clower, 1994, pp. 811-12, 1995). And rather than coming into existence to *avoid* transactions costs, market-making firms *reduce* transaction costs to levels that would be prohibitive for most participants in their absence (Demsetz, 1997).

4. Cf. Walker (1987).

5. The contemporary tendency to use 'theory' and 'model' as interchangeable terms is to be deplored, in my view. A theory is a 'provisional system of beliefs' (about the so-called 'real world'); a model is a 'formal representation' of some parts or aspects of such a system of beliefs. When formal criteria of model construction start to dominate the evolution of beliefs, we are heading for trouble. Cf. Leijonhufvud (1997).

6. I discuss Clower's contribution at some length in Leijonhufvud (1996a).

7. I did think, however, that debt deflation ought to have been in the *General Theory* in so far as the book sought to explain the Great Depression. This was part of the motivation for my 1973 paper on the 'corridor hypotheses', which argued that only in the wake of great credit crashes would the self-equilibrating forces of the market system be as weak as Keynes had portrayed them.

8. Cf. esp. 'The Wicksell Connection' in Leijonhufvud (1981).

9. I did perceive the conceptual tension within Keynesian macroeconomics and made note of the 'uneasy truce' between the orthodox Keynesians and the neoclassical synthesizers. But I did not see their disagreements in clear classical vs modern terms, perhaps because I had my own

disagreements with the Cambridge (UK) group.

10. See the fine paper by Busetto (1995) on why this line of research 'died out' (leaving only a minor posthumous headache).

11. I believe she actually took my point to heart. Her next book was *Freedom and Necessity* (1970) which did confront neo-Walrasian theory at some length.

12. Cf. my 'Maximization and Marshall' (1974b). For reasons that I shall not give here, these lectures were never published, although they were circulated fairly widely. The first lecture was a lengthy critique of the optimization paradigm, in particular as applied to 'in time' processes involving sequential decisions (as macroeconomics should do). I had thus come to the conviction that the optimization paradigm was not a possible 'micro-foundation for macro' just at the time of the new classical breakthrough and hence became a sideline spectator more than a participant in all the ensuing revolutionary excitement.

The second Marshall lecture was a painstaking discussion of how the construction of Marshallian models differs from that of neo-Walrasian ones. I have finally returned to this topic in a recent companion piece to the present paper, 'A tale of two traditions' (1996c).

13. I'm afraid that I may be the one responsible for this 'anthropomorphication' of Walras's hypothetical market process. In my 1967 article, 'Keynes and the Keynesians', I wanted to dramatize the contention that (modern) general equilibrium theory was *cheating* on the obligation to explain how the information required for the orderly coordination of activities was generated and communicated. Clerk Maxwell's famous thought-experiment in physics came to mind and I introduced Walras's auctioneer as my counterpart to Maxwell's demon.

14. The same thought occurred to Solow and Stiglitz. It is one ingredient of their paper (1968).

15. A general deflation of wages and prices is not likely to help coordinate intertemporal decisions. For a more extensive analysis, see my 'Wicksell Connection' in Leijonhufvud (1981).

16. Barends (1990) quotes Wittgenstein: 'Er muss sozusagen die

Leiter wegwerfen, nachdem er auf ihr hinaufgestiegen ist'. (He must so to speak throw away the ladder once he has used it to climb up).

17. This particular property of the *General Theory* model Keynes soon recanted in response to the empirically based criticisms of Dunlop and Tarshis. Cf. Keynes (1939).

18. The conditions are less general than Keynes believed at the time. Cf. Leijonhufvud (1973), reprinted in idem (1981).

19. Or, more precisely, 'as, with the passage of time, everyone has learned'.

20. What 'sort of *tâtonnement*' is a bit of a riddle, however, since the problem is known to be uncomputable.

21. What might Keynes have said about the French policy of defending the strong franc with (until recently) high real rates of interest? 'Déjà vu', perhaps? Remember his opinion of 'The Economic Consequences of Mr. Churchill'!

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