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Episodes in a Century of Macroeconomics

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In this chapter, I give an alternative view to those of Blanchard (2000a) and Woodford (1999) of the history of macroeconomics over the century just past. One cannot attempt to tell the whole story in the space of a chapter. So my account will be episodic. I like to think of the history of thought as a decision tree with the current generation of economists occupying the highest twigs, not all of which will grow into sturdy branches bearing ample fruit. Down below in the tree are a number of more or less important forks, where our predecessors have decided to go one way rather than another. My intention is to revisit some of those forks, to try to understand how people saw the issues at the time and why the majority chose the path that they took, and then ask how the choices made look in hindsight. If we want to learn from the history of thought, I believe, this is the way we have to go about it.

WICKSELL AND FISHER

One hundred years ago, economists had some quantitative information on money, interest and prices, and on very little else. So the story begins with monetary theory and the two great names of Irving Fisher and Knut Wicksell. Fisher and Wicksell shared a passionate concern for distributive justice that is no longer in much evidence among macro-economists. Their shared conviction that changes in the price level had arbitrary and unfair distributive effects motivated their work on monetary theory. The objective was to learn how the price level could be stabilized.¹

¹ In Fisher's later years, of course, his attention shifted to the question of how to offset the distributive consequences of price level changes through his *compensated dollar* scheme.

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Beyond these commonalities, however, the differences between the two are stark. Fisher's quantity theory was basically one of *outside* money, *exogenously* determined. Wicksell's (1898) model of the "pure credit economy" was one with only *inside* money, *endogenously* determined. For several decades, Wicksell's heritage, through the Austrian, Swedish, and Cambridge schools, had by far the stronger influence on theoretical work,² although throughout the 1920s, Fisher in the United States and Gustav Cassel in Europe had more say in matters of monetary policy.

The Wicksell connection got lost in the Neoclassical Synthesis confusion from the 1950s onwards, whereas essentially Fisherian ideas experienced a great renaissance in the monetarism of Milton Friedman, Karl Brunner, and Allan Meltzer. I will return to that later.

Wicksell will make a comeback.³ For the last 30 years or so, monetary economists have been almost exclusively preoccupied with outside money inflations driven by government deficits. The thrust of Wicksell's theory, of course, was that in systems where the gearing between outside money and inside credit is not very tight, the private sector can cause trouble all by itself. The number of bubbles that have burst in recent years around the world – and those still building – should bring him back to mind. Meanwhile, the erosion of effective reserve requirements and of government monopolies of currency production is making the gearing of inside to outside money steadily less reliable (Leijonhufvud 1997).

CAPITAL THEORY

In passing, let us note that Fisher and Wicksell also mark another fork in the doctrine historical decision tree. Wicksell was a proponent of Böhm–Bawerkian capital theory. Despite the persistent attempts by American Austrians to keep it alive, this tradition in capital theory has been all but completely eclipsed in the current vogue for intertemporal general equilibrium theory – the bloodlines of which go straight back to Irving Fisher.⁴

² Cf. Leijonhufvud (1981)

³ Michael Woodford's recent book (2003) makes this less of a bold prediction than only a year or two ago.

⁴ The revival of Fisher's interest theory is to be very largely credited to Jack Hirshleifer (1958, 1970).

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Fisher did not have a capital theory in a proper sense (and neither does IGE theory), but he showed how a theory of intertemporal prices could be built on choice-theoretical foundations, assuming smooth convexity of production sets in all dimensions. The reason that Austrian capital theory has not managed to survive in the latter half of the twentieth century, I surmise, is that nonconvexity is absolutely central to it. It is in essence, a translation of Smithian division of labor theory into a sequential temporal context and the *productivity of increased roundaboutness* is Smithian increasing returns in a somewhat new dress. And increasing returns will not fit in Fisherian IGE theory.

THE METAMORPHOSIS OF NEOCLASSICISM

Before proceeding with macro issues, we need to pay some attention to microfoundations because the evolution of macroeconomics in the second half of the twentieth century, as I see it, has to a large extent been driven by a metamorphosis of our understanding of what might constitute appropriate such foundations. This metamorphosis of common understanding has almost escaped notice, not only because it has been slow and gradual but also because it has taken place at a conceptual level below formalized economic theory.

Optimality at individual and competitive equilibrium at aggregate levels are the hallmarks of what we mean by neoclassical theory today. But the meanings attached to “*optimality*” or “*maximization*,” to “*equilibrium*” and to “*competition*” are context dependent and the conceptual context is today utterly different from what it was 100 years ago. Neoclassical theory, as is true of so many things, *ain’t what it used to be*.

In the early decades of the twentieth century, all economists distinguished between *statics* and *dynamics*. By “*dynamics*,” they did *not* mean *intertemporal* choices or equilibria but instead the adaptive *processes* that were thought to converge on the states analyzed in *static* theory. The early neoclassicals conceived of both *optima* and *equilibria* as point attractors of individual and collective adaptive processes, respectively. Statics referred to the parts of theory that could be formalized – more or less. In contrast, individual adaptive learning and market equilibrating processes were loosely sketched at best. Not much of

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substance was known about them and, in any case, next to nothing was known about a mathematics suitable to formalize them. Nonetheless, it is a grave mistake to identify the original neo-classical theories with just the static models. One should recognize instead the technical limitations that prevented generations of theorists from even attempting to formalize the dynamics underlying the propositions of static theory. In so saying, however, I do not intend to suggest that modern neoclassical theory differs from the older theory mainly in having overcome these technical difficulties. It is more accurate to say that very little has been done to address the unfinished business of the older neoclassical theory. By and large, it remains unfinished.

The conceptual issues that divide old and modern neoclassical theory are both numerous and important. The transformation of how we have come to understand neoclassical theory began, as I see it, when instead of interpreting various optimality conditions as characterizing the attractor of some loosely described adaptive process, we began to think of *constrained optimization as the way in which economic subjects make decisions* (Leijonhufvud, 2004b). If observed behavior is to be interpreted as reflecting optimal choices, one is forced to assume that economic agents know their opportunity sets in all potentially relevant dimensions. If this is true for all, the system must be in equilibrium always. Generalizing to intertemporal choice and choice under uncertainty – and the consequent equilibria – comes easy. The end point of this evolution has agents calculate optima over infinite dimensional spaces and coordinating their choices perfectly – with or without the help of markets. The train of logic departs from a picture that economists of a hundred years ago would recognize as familiar – Robinson Crusoe allocating effort between fish and bananas, say – but barrels along at uncomfortable speed, picking up loads of subscripts on the way, into a fantasy land where the assumptions made about what people are able to know, to forecast and to calculate would leave them utterly bewildered and incredulous.

Rather than belabor this metamorphosis of neoclassical economics further, let me simply reproduce a table that I have used on a couple of previous occasions (e.g. Leijonhufvud, 1999). Here I use “Classical” for the older neoclassical tradition in order to stress its link to the *magnificent dynamics* (Baumol, 1951) of the British Classical School.

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	Classical	Modern
Objective of theory	Laws of motion of the system	Principles of efficient allocation
Individual motivation	Maximize utility or profit (intent)	Maximize utility or profit (performance)
Individual behavior	Adaptive, “Procedural rationality” (often gradient climbing)	Optimizing choice, “Substantive rationality”
Behavior and Time	Backward-looking causal	Forward-looking teleological
Cognitive competence	Capable of learning, well-adapted “locally”	“Unbounded”
Role of institutions	Essential in guiding behavior; making behavior of others predictable	Problematic: Why use money? Why do firms exist?
Equilibrium concept	Constancy of observed behavior (point attractor)	Mutual consistency of plans

MARSHALL’S AGENTS

I like to use Marshall as my main illustration of the points just made. He fits here because Marshall plus Wicksell will bring me to Keynes.

Book V of Marshall’s *Principles* is usually regarded as the core of his contribution. That is debatable but what is not debatable is that it survived the longest. Yet, I believe it is most often misunderstood as a seriously flawed version of “Modern” theory. It is more accurately understood as an early attempt at what we today label *agent-based economics*. Recall that Marshall worked with individual demand-price and supply-price schedules. (This is why he drew his diagrams with quantity on the horizontal axis, etc.) From this, it is obvious that he did not build from choice-theoretical optimization. Instead, the demand-price and supply-price schedules give rise to simple decision-rules that I like to refer to as “Marshall’s Laws of Motion.”

For consumers: if demand-price exceeds market price, increase consumption; in the opposite case, cut back.

For producers: if supply-price exceeds the price realized, reduce output; in the opposite case, expand.⁵

⁵ And for the capitalist: if the rate of profit exceeds the normal, add to capacity, etc. But I will confine the discussion here to the short run.

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And we should imagine a similar rule for price-setters:

For middlemen: if inventory turnover slows down, reduce prices to both the customers and suppliers; in the opposite case, raise them.

Please note for future reference that, if all this is going on at once, God only knows what kind of messy nonlinear dynamics would result. Marshall tamed the dynamics by assuming a strong ranking of adjustment speeds that gave him a tidy ordering of market day, short-run and long-run equilibria. These equilibria, of course, have no counterparts in choice-theoretically based modern constructions.

For now, the point is this: up through the 1920s, virtually all economists were convinced that *as long as everyone obeyed Marshall's Laws of Motion, it was a foregone conclusion that an economic system would go to a full employment attractor*. The intellectual situation was, in this important respect, the same as the one we have returned to in the last couple of decades, namely, the general belief that only "rigidities" or "inflexibilities" of some sort can explain departures from general equilibrium.

Keynes began to doubt this presumption already while writing the *Treatise on Money* and became increasingly convinced that it had to be wrong as the Great Depression deepened.

KEYNES

Wicksell had been critical, scornful even, of the attempts by others to use his "cumulative process" in the explanation of business cycles. His reason, I would infer, was that the "neutral equilibrium" of the price level could hardly apply to real magnitudes.⁶ It had to be a purely nominal phenomenon.

In the *Treatise*, Keynes used Wicksell's natural vs. market rate concepts. He traced the problems of Britain in the late 1920s to a market rate in excess of natural rate. As in Wicksell, this leads to a cumulative deflation. Like everyone else, then and now, he noted that wages are sticky so that deflationary pressure is apt to produce unemployment. But this is a common sense appendage and not a part of his model which can only handle full employment.

⁶ Recall that he used a cylinder resting on a plane as his illustration of "neutral equilibrium." Displaced, it would have no tendency to return to its prior equilibrium.

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However, in a rather odd passage, known as the “banana parable” (Barens 1989), he toys with the (not very convincing) idea that real saving exceeding real investment might produce a cumulative process ending in total collapse of output. Subsequently, and eventually with some help from Richard Kahn, he worked out how the multiplier process would converge and not implode. It would converge on a state of less than full employment.

Could this state be an equilibrium?⁷ Keynes knew that if the economy stayed (somehow) at full employment, accumulating pressure on the market rate would eventually have to bring it into line with the natural rate. He also knew that if the two rates were equal, so that investment equaled saving out of full employment income, wage flexibility would guarantee the achievement of full employment. But if the intertemporal and the labor market coordination failures occurred together, general obedience to Marshall’s Laws of Motion did not seem to guarantee convergence to the full employment equilibrium. The sequence of events is important: (1) declining prospective returns make investment fall below full employment saving; (2) liquidity preference prevents the interest rate from falling enough to close the gap;⁸ (3) real output and income fall until saving equals investment. At this point, the real interest rate is too high, but the excess demand for “bonds” is zero, so there is no market pressure for it to change. At the same time, wages are roughly what they should be in general equilibrium – neither labor’s productivity, nor the outside money stock has declined – but unemployment is putting downward pressure on wages. The price that is “wrong” does not change, whereas the price that is “right” tends away from that level. So Keynes concludes that wage stickiness is actually good for you in this situation since a high degree of flexibility would set in motion a Wicksellian deflation that might wreck the financial system without restoring full employment.

It is finding this instability of the full employment state that motivates and justifies Keynes’s claim to a more *general theory*. The adaptive dynamics of his theory are more general than his predecessors. And it

⁷ Equilibrium *nota bene* in a Marshallian, obviously not Walrasian, sense.

⁸ In the *Treatise*, Keynes had a nice analysis of liquidity preference showing how the decline in expected return to capital causes ripples of substitutions all along the term structure. At the shortest end, the banks do not find returns attractive enough to relend all the funds that flow back in repayment of loans as production declines. Thus the process shows an induced decline in the stock of *inside* money, the counterpart of which is an increased demand for *outside* money as the banks act to bolster their own liquidity positions. (Note that here, there is a very good reason to prefer the term “liquidity preference” to the ambiguous “demand for money.”) If Keynes had retained this analysis in the *General Theory*, we would have been saved quite a bit of later confusion!

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makes it possible to understand mass unemployment as resulting from causes other than rigidities or waves of contagious laziness.

Why doesn't the system "automatically" straighten itself out? Because, Keynes maintained, saving is not an effective demand for future consumer goods and the offer of unemployed labor to work is not an effective demand for present consumer goods.⁹ *Effective* excess demands summed across all markets do not necessarily sum to zero. In this sense, therefore, Say's Law does not hold: *Supply cannot be counted on to create its own demand*. So, from the rejection of Say's Law, stems the original rationale for stabilization policy, understood as aggregate demand management.

But Keynes did not get everything right and besides he was not generally well-understood. But before taking note of some of the confusions and their consequences for later developments, we had better stand back and take stock a bit.

HOW ECONOMISTS UNDERSTAND THE WORLD THEN AND NOW

At about mid century, the experience of the Great Depression and the intellectual influence of Keynes (and others) had combined to instill in most economists a worldview¹⁰ which saw the private sector as unstable, riddled with market failures, and prone to fluctuations amplified by multiplier and accelerator effects. But most economists also believed that a benevolent, competent, and solvent government, dependable in its democratic role as the honest agent of the electorate, could stabilize the economy and ameliorate most market failures.

Fifty years later, mainstream economists believe that the economy maintains itself on an intertemporal equilibrium path. As long as they are not disturbed by stupid policies, markets will take care of all coordination problems. The coordinated timepath may be somewhat less than ideal. If labor market "inflexibilities" are tolerated, the natural rate of

⁹ An "effective demand failure" occurs when some category of transactions assumed to be feasible in general equilibrium models cannot be carried through. In addition to the two types of such failures prominent in the *General Theory*, a third type of effective demand failure may occur when, in a financial system loaded down with bad loans, the promise of revenues from future output cannot be used to exert effective demand for the present resources required to produce that future output. The traditional Keynesian deficit-spending policy was designed to relieve the liquidity-constraints on households. Japanese experience in the 1990s would seem to indicate that this is not the right policy in the case of an effective demand failure of this third type.

¹⁰ Here I am paraphrasing my paper in the Fitoussi Festschrift (Leijonhufvud, 2004a).

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unemployment may be quite high. And governments are seen as prone to excessive deficits and inflationary finance, as constitutionally time-inconsistent, and as addicted to playing the unstable Phillips-curve for no good reason and to no good effect. Macropolicy no longer means the active management of aggregate demand, but rather the political art of finding ways to constrain governments.

The main task for the history of the economic thought of the second half of the twentieth century must surely be to explain this 180-degree turn in the worldview of the representative economist. That will not be accomplished here. In particular, it is not possible to do justice to the macro-econometric work which has given us a far better and richer quantitative picture of the complex system under study. But some of the conceptual elements in this radical transformation can be readily identified.

ON KEYNES'S PROBLEMS AND PROBLEMS WITH KEYNES

To begin with, let us consider the flaws in the Economics of Keynes and subsequently in what became Keynesian Economics.

First, one should perhaps mention some beliefs that Keynes held that present-day economists (at least in the *first world*) have little reason to share. Among them (1) the belief inherited from Ricardo, Marx, and Marshall that the accumulation of capital would sooner or later depress the marginal efficiency of capital to near zero.¹¹ Also (2) the conviction that the modern economy has a chronic tendency to save more than could be profitably invested.¹² Furthermore (3) the view that workers live more or less hand-to-mouth, so that consumption is simply a function of current income. This last proposition may have had some verisimilitude in the early part of the twentieth century which, obviously, it has since lost. The second and the third together may, I think, explain why the *General Theory* fails to emphasize sufficiently the financial crash as the key factor in the Depression.¹³

¹¹ This is the proposition that Allan Meltzer in his book on Keynes (1988) saw as central to Keynes's theory.

¹² For the last couple of decades American economists have had little reason to worry about too much household saving. Keynes, one may surmise, might have attributed the low rate of U.S. unemployment in the 1990s to the great decline in the American saving rate rather than to labor market flexibility.

¹³ In various papers written between the *Treatise* and the *General Theory*, Keynes was both clear and forceful on the role of financial factors in the depression. Hyman Minsky developed these elements in Keynes's thought much further (e.g., Minsky [1975]).

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All that said, Keynes had one thing right that is most often missing from modern macro. He knew that macroeconomic catastrophies still happen and that, when they do, they threaten the social order.

What is more interesting than Keynes's outdated beliefs are the technical difficulties that he faced. Recall that combining Marshall's Laws of Motion for price and output in an isolated market results in a nonlinear process. Marshall "tamed" it by assuming that one could take output as "constant" while price adjusted. The justification for this was tenuous at best by the 1930s.¹⁴ Consider then, a system of multiple and interdependent such markets with varying lag structures, etc. A complex, multi-dimensional, nonlinear nightmare! Is it likely that one might find a sensible and useful *static* model for such a multi-market Marshallian dynamic system? This would require finding a partition of the endogenous variables such that when the slow-moving variables of the one set are "frozen," the others can reasonably be assumed to converge rapidly to a point-attractor. Furthermore, this partition should be reasonably stable so that it would not have to be changed each time the question posed to the model is changed. It is not obvious that the real world would always oblige the economist by conforming to these requirements.

Moreover, this was not the only technical difficulty. Keynes's explanation of "involuntary unemployment" involved, as we have seen, prices diverging from their general equilibrium values in a particular way. To explain why the system did not tend strongly towards the full employment state, Keynes did not only have to model the structure of relative prices but needed also to keep track of the flow-of-funds through the system since workers (at least) were liquidity constrained.¹⁵ Keynes thought he eventually had a static ("equilibrium") model that would satisfy these requirements.¹⁶ And IS–LM seemed to him at first and to countless others later, a fully adequate formal representation of the theory.¹⁷

¹⁴ John Hicks (1965, Chapter 5; 1989, Chapter 3) used to note that this might have been a plausible ranking when Marshall first began working in economics but that it will not fit the later "fix-price" markets for branded commodities.

¹⁵ The "hydraulic" element of Keynesian economics to use Coddington's term.

¹⁶ Hicks who had wrestled with this kind of problem along the lines learned from Erik Lindahl was impressed as evidenced by his original review of the *General Theory* (Hicks, 1936).

¹⁷ Hicks who was, of course, one of the co-inventors of IS–LM came to conclude many, many years later that the model was deeply flawed because the "partitioning" did not make sense. The length of the "periods" relevant to the equilibration of IS and of LM could not be the same, even approximately (see Hicks, 1983).