

THE WICKSELL CONNECTION:

Variations on a Theme

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I. Introduction

The theory of the interest rate mechanism is the center of the confusion in modern macroeconomics. Not all issues in contention originate here. But the inconclusive quarrels -- the ill-focused, frustrating ones that drag on because the contending parties cannot agree what the issue is -- largely do stem from this source.

This essay seeks to clarify the relationships between some of the major schools in modern macroeconomics by tracing the development of the theory of the (real) interest rate mechanism. It claims to get to the bottom of the Monetarist controversy and to the origin of the Two Cambridges controversy. My own position has differed from that of the Cambridge Keynesians as well as from that of the Neoclassical Keynesians and will be reasserted here as a third Keynesian (of sorts) alternative to Monetarism.¹ Some aspects of recent work on Rational Expectations will also be considered.

The following family tree of major 20th century macroeconomists helps outline the argument of the paper (Diagram 1). It shows the theories using the saving-investment approach, starting with Wicksell, as an off-shoot from the ancient and honorable Quantity theory mainstem.²

Until Friedman revived the Quantity theory, the saving-investment approaches dominated the field in this century. All Keynesians, of whatever description, belong to this branch. The Stockholm School and the Austrians also descend from the Wicksell Connection.

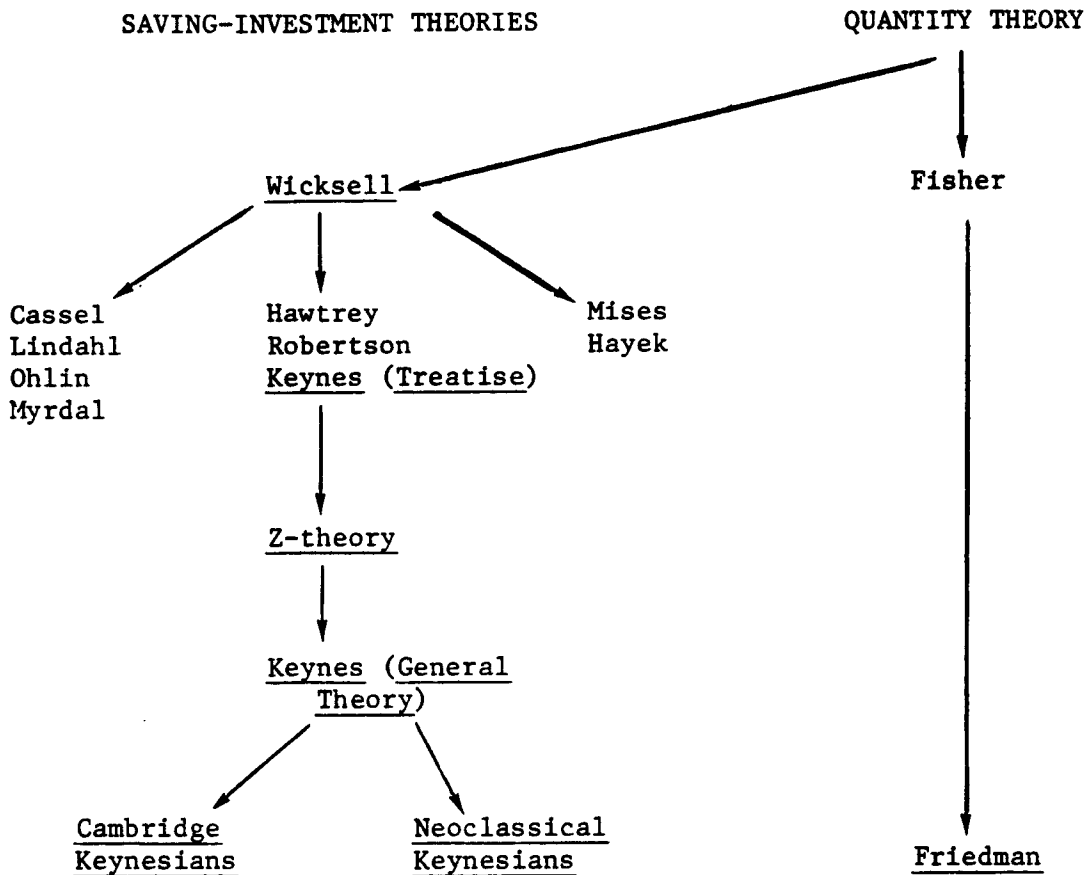


DIAGRAM 1

In Wicksell's theory of the cumulative process, the maladjustment of the interest rate -- the discrepancy between the market rate and the natural rate -- is the central idea. It is also the idea that motivates the analysis of changes in the price-level (or in nominal income) in terms of saving and investment. It is a simple but fundamental point. Use of the saving-investment approach to income fluctuations is predicated on the hypothesis that the interest rate mechanism fails to coordinate saving and investment decisions appropriately. This is where all the Wicksell Connection theories differ from Monetarism. In Monetarist variants of the Quantity theory, saving and investment have to do

with the allocation of output but nothing to do with the determination of aggregate income or the price level. This is so because Monetarist theory assumes that the interest rate mechanism can be relied upon to coordinate the intertemporal decisions of households and of firms.

Some twenty years of IS-LM exercises and applied econometrics failed to isolate this point as fundamental to the Monetarist controversy.³ Some twenty years of mathematical modelling similarly failed to spotlight incompatible ideas about the interest rate mechanism as being at the root of the Two Cambridges controversy. In order to clarify these matters--and, at the same time, explain why they have not been more obvious all along -- we will trace Wicksell's theme through a number of analytical variations.

The original idea is simple. In allocation theory, we learn that household saving decisions and entrepreneurial investment decisions are to be coordinated by the interest rate mechanism. In money and banking, we learn that "the" interest rate is governed by the supply and demand of securities (or of "credit"). Imagine a situation where the interest rate cannot do both jobs at once, i.e., in which that level of real interest that equates the supply and demand for securities does not serve to equate saving and investment. What could be the Causes of such a maladjustment? What might be its Consequences?

In the generation following Wicksell, we find suites on his theme composed by the Swedish, by the Austrian, and by the Cambridge schools. Before the General Theory, it was the dominant theme in monetary and business cycle theory as an imposing parade of names will testify: Cassel, Lindahl, Ohlin, and Myrdal; Mises and Hayek; Hawtrey, Robertson, and Keynes were among those who put the theme squarely in the center of major

works of theirs. After the General Theory, however, the theme is no longer prominent. It was abandoned by monetary economists and left to antiquarians. So what happened?

What happened, essentially, was that Keynes so obfuscated the interest rate mechanism that the later Keynesian literature almost entirely lost track of Wicksell's theme. The basic idea remains central in the General Theory. (Its middle name was "Interest", after all). In Keynes' last variation, however, the theme comes in a guise that has proven the almost perfect disguise. The failure to recognize its presence and role has proved productive of much later misunderstanding and confusion.

To see what happened, we take the theme through four variations.⁴ Numbers 1, 2, and 4 are, respectively, Wicksell, the Treatise on Money, and the General Theory. Number 3 is an analytical interpolation between the Treatise and the General Theory. It may be described either as "the Treatise plus quantity-adjustments" or as "the General Theory minus the Liquidity Preference theory of interest." (For a less cumbersome label, it is designated as "Z-theory" in Diagram 1).⁵

Although it is a doctrine-historical fiction, this Z-theory is (at the very least) useful in enabling us to judge how much of Keynes' "revolutionary" theory of unemployment is independent of the Liquidity Preference (LP) theory of interest and precisely what properties of Keynesian models derive from it. It is my own position that Z-theory incorporates all of Keynes' contribution that should be preserved and developed; that the LP hypothesis should have been rejected from the start; and that, failing this, propositions derivative from it ought systematically, if belatedly, to be rooted out of modern macroeconomics.

Keynes' obfuscation of interest theory inheres in his LP hypothesis but stems from his insistence on the saving-investment equality as an identity. If saving and investment are always equal, they cannot govern the rate of interest, nor can the interest rate possibly serve to coordinate saving and investment decisions. Hence the LP theory: money demand and supply govern the interest rate.

The elimination of the Loanable Funds (LF) mechanism and the impossibility of saving and investment decisions being coordinated by the interest rate in a system from which it is totally absent are the original propositions from which later Cambridge Keynesian positions on growth theory and related matters logically develop.

The denial of the LF mechanism makes nonsense of the very notion of a "natural rate" of interest. The Wicksellian theme is lost. The affirmation of the LP theory contradicts the dynamic hypothesis that is fundamental to Quantity Theories, namely that the excess demand for money governs the price level. The rationale for the saving-investment approach and the relationship to Quantity theories are both confounded.

The Neoclassical Keynesians have never managed to clear up the resulting muddle. This murkiness on critical issues of what was for so long clearly the majority doctrine has befuddled friends and foes alike in the Monetarist controversy. Most obviously, perhaps, the failure to grasp the role of the Wicksellian maladjustment of interest rate in Keynes' theory of unemployment has caused the Keynesians more difficulty than necessary in marking out for themselves a theoretically justifiable answer to the Natural Rate of Unemployment doctrine. A "Keynesian" answer to this doctrine would be

that unemployment will not converge to its natural level unless the interest rate goes to its natural level--and that the latter condition will not always be fulfilled. Instead, American non-Monetarists have tended to take up positions that were either largely irrelevant (the interest-elasticity of transactions demand for cash) or indefensible (the stable Phillips curve).

Another consequence of the muddle is the at least occasional failure of Keynesians to come to Monetarist conclusions under the appropriate conditions. Whenever the market rate of interest keeps to its natural level --and, surely, they do not always diverge?--the Keynesian model should reduce to a Monetarist one.⁶ In failing to bring this out, Keynesians have allowed the simplest and most important lessons of monetary experience to come to be regarded as quintessentially Monetarist insights.

II. Benchmark

In the "long run" of price theory, all adjustments have taken place. Originally, the distinctive characteristic of the "short run" was that stocks need not be fully adjusted but that net investment or disinvestment may be going on. For the rest, all activities were "equilibrated." More recently, however, new short run notions have begun to proliferate in the literature to fit various and sundry cases of incomplete adjustment being investigated.

In macroeconomics, a full adjustment benchmark different from the traditional long run is more appropriate and more useful. Call it full information. In full information macroeconomics, we study states of an economy in which agents have managed to learn all that can be (profitably) learned about their environment and about each others' behavior. Such states should be equilibria in the sense suggested by Hahn, i.e., market interaction will not teach agents anything that significantly alters their beliefs.⁷

To develop an entirely satisfactory concept of full information will not be a simple matter. The desired combination of precision and of realism (of a sort) is not easily achieved. The objective, however, will at least be clear. We want on the one hand to put a safe distance between full information macroeconomics and notions of costless information and perfect foresight. On the other hand, we strive to retain a role for equilibrium constructions. The objective itself is controversial. Certain authors--e.g., Kaldor, Lachmann, Robinson, Shackle--argue that a realistic appreciation of the role of ignorance in the human condition must preclude the use of equilibrium models. However that may be, this paper cannot do without.

We postulate that a feasible equilibrium growth path will "exist." The main purpose of full information macroeconomics, however, is merely to define the adjustments that must take place if the system is to adapt fully following some disturbance. Use of such constructions does not commit us to the belief that the system will always or normally adapt smoothly and rapidly no matter what the disturbance; nor does it force us to preclude the very rapid convergence of the economy on full information equilibrium in certain cases--as in Rational Expectations models. The notion of a full information state should not be invariably associated with either the short run or the long.

To justify, to define precisely, and to derive the analytical properties of a full information model cannot be attempted here. Instead, we simply propose a number of model properties that are to serve as the full adjustment benchmarks for the purposes of this paper:

- 1) Labor supply and derived labor demand determine output, employment, and the real wage rate;
- 2) money supply and money demand determine the price level; and
- 3) saving and investment determine the rate of capital accumulation and the interest rate.

In full information comparative statics, moreover, it should be true that monetary shocks have no real effects and that real shocks have no monetary consequences.⁸ The first of these generalizations simply asserts the neutrality of money. The second is more controversial in that it denies significant interest-elasticity to the steady state (non-speculative) excess demand for cash balances. So here we are out on that limb onto which Professor Friedman is always urged but that he has steadfastly refused! In textbook parlance: a vertical IM curve. Some comments on the

matter are in order.

In standard theory, if the economy is required to traverse from one growth-path to another, it must also change its price-level. As an example, consider the case that Keynes thought to be the main economic problem facing the civilized world in his day, namely, a non-transitory decline in the marginal efficiency of capital. The rational response for the system transfers resources from capital goods producing to consumption industries and has the economy settling down at full employment on a lower growth-path with a lower rate of profit. At lower rates of profit and interest, however, transactors supposedly demand larger real cash balances per unit output. Consequently, the system's adjustment will require, ceteris paribus, a reduction in money prices and wages.

The notion that the interest-elasticity of the transactions demand for cash should prevent the economy from traversing without also deflating (or inflating) runs counter to my intuition. It does not seem right. So saying, however, will not get one around the models of Baumol, Tobin, and Patinkin⁹, all of which imply that the interest-elasticity should be significant. Nor will intuition dispose of the empirical evidence.¹⁰

For a counterargument try the following. Consider Patinkin's model. His agents hold real balances to insure themselves against the personal consequences of default. The lower the rate of interest the more such insurance they invest in. Here the penalty cost consequent upon default is treated as a constant. But is not this arbitrary? Instead of bankruptcy as the typical consequence of not having cash when a bill is presented for payment, the representative agent may envisage incurring certain costs

connected with a delay of payment. A delay of payment is not a default (yet). What should be the appropriate penalty for it? For concreteness, suppose the creditor is entitled to triple damages. That is three times the interest on the sum due over the period of the delay. If that is the structure of the choice, the interest-elasticity of transactions (and precautionary) demand should be zero--for permanent changes in the level of the interest rate.¹¹

The point of all this is not to deny the relationship between velocity and interest rates over the business cycle but rather to shift the emphasis in the interpretation of the time-series evidence from theoretical steady-state to non-steady state properties of money demand. In particular, cyclical variations in real cash balance demand per unit output will be attributed to speculative demand or to transitory changes in "flexibility preference."¹² The observations that account for the interest-elasticity, for example, of Latané's equation, may not belong on the steady-state transactions demand for money functions. Instead, we suggest, they are associated with departures from full information time-paths.

For the full information equilibria of the model suggested above, we assume that agents have consistent beliefs about current and future market determined magnitudes; that these beliefs are ~~realizable~~ in that they are consistent with resource and technology constraints; and that all individual and mutual adjustments of behavior to this knowledge have been completed. Relative to the benchmark time-paths provided by such a model, we may then distinguish two types of coordination failures. Each corresponds to a distinct approach or emphasis in modern macrotheory.

One is the "spanner-in-the-works" malfunction¹³ for which we may imagine that agents actually do have full information (so that all the nuts and bolts

of standard theory can be used) but are prevented from acting on what they know. The obstacle to appropriate adjustment may be coercion or past commitment to a (possibly implicit) contract or to a particular structure of physical capital. Coordination failures of this sort may well be characterized as equilibria. This approach to macro theory will be altogether ignored in this paper.

The other main type of coordination failure, of course, comprises incomplete information states of the system. While on a taxonomic course, we may again distinguish two kinds of such states. In the first kind, agents have mutually consistent but incorrect beliefs: they all believe the same thing and they are all wrong. Consistent beliefs should produce temporary equilibria.¹⁴ In the second kind, agents are acting on inconsistent beliefs. Such states will be called disequilibria in what follows.¹⁵

As in my previous work, this second incomplete information approach is the one pursued here. We will apply it to the interpretation of a number of inherited macrotheories. But first some anachronistic preliminaries.

III. Double Cross

The macroeconomics we actually teach, of course, bears no clear relationship to full information macro (FIM). Instead of the static properties of the FIM model, the beginning student may be given one or the other of two crosses to bear. Call them "Model A" and "Model T". Model A uses the Keynesian saving-investment cross to determine nominal income; Model T employs a given money stock and a "Cambridge k " money demand to the same purpose.

In either case, the student has had a switch pulled on him before he even got started. In our FIM model, saving and investment have nothing to do with the level of income, whether real or nominal; and money supply and demand determine the price level and not real income or the product of the two. Full information macro admittedly has little direct bearing on the problems that motivate the study of macroeconomics. But why does "relevant" macroeconomics start off with this surreptitious¹⁶ double switch of crosses? Since A and T differ from FIM, the answer should be that certain information failures are taken for granted -- so much so, in fact, that they are built into elementary models as inescapable features of the real world.

The Monetarist Controversy started off, in effect, with a confrontation between Models A and T. No economist alive will confess to a belief in either one. Nonetheless, all the Round I issues are produced by putting these simplistic constructions on collision course: the relative "stability" of the consumption-income and the money-income relations; the appropriate empirical components to be included in "autonomous expenditure" or in the "money supply" respectively; the "autonomy" of investment and the "exogeneity" of the money stock; the effectiveness of fiscal and monetary policy actions and the predictability of their consequences.

These direct statistical contests between A and T did not serve to focus attention on the question of how A and T, respectively, depart from a full information model. The information failures implicit in the debate were not brought to light. Yet, the discord largely stems from this level. To clarify the theoretical issues, we obviously need to define the information problems that are presumed to be ever-present -- or ignored as implausible -- by each side. For Model T, we have the diagnosis recently made familiar by Lucas, Barro, Sargent and Wallace et. al.: anticipated changes in money affect prices, but unanticipated ones affect real income. In the case of Model A, as later sections will show, the corresponding diagnosis runs: whereas (in FIM) recognized changes in the realizeable rate of profit affect only the interest rate and the growth path, unrecognized changes will affect money income and (in A) all such changes go completely unrecognized.

From A or T, our student graduates to IS-LM. On this loftier plane, it is no easier to see what is going on. In our FIM model, real and monetary phenomena were independent of one another. Then the switch again: In IS-LM, as usually taught, real disturbances have monetary consequences and vice versa -- unless extreme assumptions are made about the elasticities of IS and LM. Is it plausible that this interdependence also stems from information failures of some sort?

Start back with A and T, with propositions fundamental to each. Money income, in Model A, will decline (say) if and only if intended saving exceeds intended investment (so that we have an excess supply of commodities). In Model T, money income will decline if and only if the prevailing state is one of excess demand for money. So far the two are consistent. When money income is falling, we should have both an ES of commodities and an ED for money.

But we have two contrasting hypotheses about causation. In A, a decline in investment produces the ES of commodities. In T, a reduction in the money supply produces the ED for money. If we scrutinize the A story with the suspicious eyes of a T-believer and then let an A-believer have his turn with the T story, we obtain two questions about "transmission".

Q.1: Why should real disturbances be expected to cause an excess demand for money and thus a change in the nominal income level (and, if money prices and/or wages are inflexible, a change in activity levels)? Empirically, do they? Always, sometimes, or never?

The Keynesian answer is that they always do. Monetarists think "never" is the more plausible conjecture.

Q.2: Why should monetary disturbances be expected to cause saving and investment intentions to diverge (and thus to change nominal income, etc.)? Empirically, do they? Always, sometimes, or never?

In the schools stemming from Wicksell it is presumed that they generally do.¹⁷ In modern monetarism it is presumed that they generally do not. Poles apart on this spectrum of views, one finds the Austrians and the Chicago monetarists. Among Wicksell's intellectual descendants, the Austrian business-cycle theorists were particularly insistent that monetary impulses must disrupt the coordination of saving and investment decisions and shove the system off its equilibrium growth path.¹⁸ Among the monetarists, Friedman has most strongly argued the view that the banking system cannot, except very transitorily, affect the real rate of interest on which saving and investment depend.¹⁹ The later Rational Expectations Monetarism does not rely on the interest rate as part of the transmission mechanism at all.

If we link Models A and T by "the" rate of interest, the resulting IS-LM construction will suggest what has by now for decades been the standard textbook answers to Questions 1 and 2. Consider the shocks that typically produce deflationary pressure.

A.1: A decline in investment demand shifts the IS-schedule left; the decline in investment is associated with a decline in loanable funds demand; this reduces the rate of interest; at the lower rate of interest, the amount of money supplied will fall short of the amount demanded at the initial income level.²⁰

A.2: A reduction in the money supply shifts the LM-schedule left; the excess demand for money is associated with an excess demand for loanable funds;²¹ this drives up the rate of interest, so that intended investment now falls short of planned saving at the initial income level.

So IS-LM gives us a handle of sorts on the interaction of real and monetary phenomena: the "real" disturbance leads to a "monetary disequilibrium"; and the "monetary" impulse changes the rate of real capital accumulation. The exercises suggest, moreover, that the elasticities of IS and LM are crucial to the strength of the interaction. Round II of the Monetarist Controversy -- in which the Round I issues were transplanted into the IS-LM frame and Crowding Out and Gibson's Paradox added -- pursued the notion that the issues could be narrowed down to the values of these elasticities.²² The discussion tended to presume, moreover, that the elasticities were stable properties of the system, that the results of time-series regressions gave information on these steady-state elasticities, and that qualitative results from a priori static choice theory had a bearing on the issues in that they sufficed to exclude extreme values.

In this elasticities view of the controversy, there are two extremist possibilities. The "fiscalist" extreme would postulate a vertical IS and a horizontal LM; the monetarist extreme a horizontal IS and a vertical LM. Putting it this way tends to suggest that, surely, all moderate men of sound judgment will take a position somewhere in the middle -- although leaning a bit toward one extreme or the other will be permitted without prejudice to one's reputation for reasonableness. The trouble is that this moderate position implies that real impulses always must affect income and that monetary impulses always must put a wedge between the plans of savers and investors -- i.e., that the "rational" FIM adjustments can never happen. And going to the monetarist extremes is hardly more palatable for, in context, the resulting model answers "never" to the empirical part of Questions 1 and 2 above. In either case, "sometimes" is the empirical possibility that is being excluded by construction.

Note that if it were to be the case that these short-run interactions of real and monetary phenomena are due to incomplete information on the part of agents, then the elasticities view is seen to be seriously misleading on several counts. What the response to a particular impulse will be then depends upon the state of information and not just on steady-state behavioral parameters. What counts is the extent to which the nature and extent of the shock is recognized or unrecognized, anticipated or unanticipated, perceived as permanent or as transitory. The same impulse should not call forth exactly the same responses over and over again.²³ In this setting, also, it makes little sense to ask whether fiscal or monetary policy is more "effective" or to measure their relative effectiveness by time-series regression. The effectiveness of a given set of policy measures depends on the nature and seriousness of the disequilibrium one is trying to correct.²⁴

In Round III of the controversy, the monetarists shifted the focus away from the elasticities and towards the "stability" of the system. IS-LM hardly lends itself to analysis of conflicting beliefs about the strength of equilibrating tendencies, speeds of convergence and the like.²⁵ The framework does not even help to explain why macroeconomists should divide into opposing camps over the Phillips-curve in the same way as they did over the issues of Rounds I and II. Why should belief in the autonomy of investment, the predominance of real disturbances, the stability of the consumption function, a significant interest-elasticity of money demand, and the effectiveness of fiscal policy prejudice you in favor of the notion of a stable (even if expectations-augmented) Phillips-curve? What does the exogeneity of some "M", predominance of monetary shocks, stability of velocity, and effectiveness of monetary policy have to do with convergence to a natural rate of unemployment? There is, as we shall see,²⁶ a fairly straightforward answer to these queries. But IS-LM does not suggest what it is.

It is instructive to consider how our simple IS-LM analysis would have to be amended to accommodate a theory that allows the answer to Questions 1 and 2 to be "Sometimes". These amendments will suggest, in the first case, that the elasticities may be variable rather than stable and, in the second, that the elasticities may on occasion be altogether irrelevant. Those observations pertain to the mere mechanics of so adjusting the model that it will accommodate a different theoretical conception. It is more to the point to note that, in both cases, these mutations of the simple model are forced by changes in the assumptions we make about the information possessed by transactors.

The first case is straightforward. The possibility of "sometimes" having the marginal efficiency of investment change, as in FIM, without significant change in the velocity of money and in money income can be incorporated, for example, by postulating a speculative money demand function that does not depend on the absolute level of the interest rate, \underline{r} , but only on the difference between \underline{r} and the perceived "normal" rate, r^* :

$$M^d = f(Y, \underline{r} - r^*)$$

In the thus modified model, if the change in MEC is correctly perceived and seen to be permanent, the normal rate will be adjusted accordingly, and the adjustment of the whole structure of yields can take place without creating an excess demand (or supply) of money.

In the second case, the possibility of "sometimes" seeing the money supply and nominal income change without concomitant changes in the interest rate and rate of capital accumulation can be accommodated if we allow both the reduced forms to shift together and at the same time. The usual classroom practice of shifting one schedule while keeping the other constant rests on the strong assumption that a disturbance that shifts, for example, the IS-curve will have the ED (or ES) for commodities matched by an ES (or ED) for bonds with zero impact on the ED for money. Similarly, when LM shifts the implicit matching excess demand is in the bond market with no direct impact on commodities.²⁷ But it is of course entirely possible to have disturbances that directly create an ED for commodities with a corresponding ES for money or vice versa. These cases have both IS and LM shifting.

This question of the excess demand distribution at impact of various shocks illustrates a more general problem. One of the stock complaints

about IS-LM is that it is "too static." But it may well be that it causes us more problems because it is not as static as it seems. IS-LM contains a number of built-in assumptions about the sequence in which things happen even as its simultaneous equation form gives the impression that the temporal order of adjustments is irrelevant. As with any period-model, of course, there is first the distinction between the things that happen within the short run and those that will take place "later". With many Keynesian constructions of this genre, one should recognize the further category of the adjustments that "never" happen -- such as a return to full employment. What is perhaps less obvious is that the sequence of events within the model's period is neither arbitrary nor irrelevant. Take the same example: When LM is shifted, holding IS fixed, so as to create an excess supply of money, it is implied that it takes a reduction in the rate of interest before we get our excess demand for commodities and the rise in nominal income.

The temporal order of events is analytically significant when (some) transactors have to act on incomplete information. Conversely, models with built-in sequences contain important, though often implicit, assumptions about "who knows what when."

A more detailed IS-LM story of the once-over money injection would run through the following course of events: (i) assume that the initial state is an FIM equilibrium; (ii) the banking system expands, creating an ES of money and an ED for securities; (iii) interest rates decline until the securities ED is zero; demand prices for assets rise relative to their rental values and relative to their initial supply prices; investment thus exceeds saving so that the state of the economy at this stage is one of money ES and commodity ED; (iv) nominal income rises; some part of

this rise takes the form of an increase in real output and employment;
(v) any overshooting of output and employment is discovered and corrected and prices increase further -- so that we end up in a new FIM state with the monetary impulse having affected only nominal magnitudes.²⁸

Once the sequence is spelled out in this way, it is obvious that the analysis definitely assumes incomplete information.²⁹ It also becomes apparent that IS-LM is a cumbersome, inappropriate frame for representing theories that make different assumptions about the knowledge possessed by transactors and, consequently, about the time-phasing of events.

In the above sequence, incomplete information is implied at two points: first, where the real interest rate and the relative price of assets and their services change away from their FIM values; second, where activity levels and not just nominal values rise. In Friedman's Theory of Nominal Income, the first of these is minimized, but the second admitted. In a Rational Expectations version of the once-over money injection, the entire sequence implicit in the IS-LM analysis is short-circuited -- we jump directly to the end-point where both schedules have shifted.

If agents were to have full information to begin with, an excess demand for commodities at the old prices and interest rate emerges as soon as the intentions of the Central Bank are known. The increased supply of credit by the banking system is offset by increased demand on the part of non-bank transactors anticipating a rise in prices. The rate of interest does not move and never plays any role in the "transmission" of the monetary impulse.³⁰ In the standard exercise, by the same token, the lowering of the rate of interest serves to cajole those who do not know what is going on into nonetheless increasing their spending. When agents do know, there is

no "transmission problem" and the notion of a "transmission mechanism" becomes somewhat meaningless. Fully informed agents have no need for a price-mechanism to inform them about what is happening. Prices merely reflect what they already know.

Does this sort of thing apply also to the other standard IS-LM exercises? It does. Take the shift of marginal efficiency of capital (MEC) once more. For this once, to be in accord with the usual textbook version, assume a non-speculative money demand function which nonetheless has significant interest-elasticity. In this instance, we draw the diagram with real income on the horizontal axis. So: (i) an initial FIM state; (ii) the MEC declines creating an ES of commodities and a corresponding ED for bonds; (iii) the interest rate declines until the ED for bonds is zero so that we have an ES of commodities and an ED for money; (iv) nominal income falls; some part of this decline takes the form of a decline in output and employment if wages are inflexible; (v) if the wage inflexibility is a temporary rather than permanent phenomenon, the ES of labor drives money wages down; prices fall, shifting LM rightwards until the real rate of interest is low enough for investment to equal saving at the full employment level of real income. Here, again, we have an initial FIM state disrupted by one of the reduced forms shifting; after several intervening stages, the second reduced form also shifts so as to complete the "rational" adjustment to a new FIM state. And again it is clear that, possessed with sufficient information, transactors would short-circuit the whole sequence.

In these two examples we have made certain that the initial as well as the terminal state of the process are full information equilibria. In actual IS-LM practice, of course, such sequences are often truncated both

fore and aft. The process neither begins from nor ends in a FIM state. It may not end there, for example, because wage-rigidity is assumed so that the last stage of our processes "never" occurs. If full adjustment to a disturbance never comes about, it makes no sense to assume that the initial state is such an equilibrium either. Whether or not full adjustment will ever occur is not, at this juncture, the point at issue. The point, rather, is that without a full information equilibrium to refer to we cannot define precisely what the coordination failure is that is supposed to be present. The inability to do so, in turn, is bound to bedevil our attempts to explain precisely why the coordination failure exists and persists.

Discussing the most simple-minded rather than the most sophisticated version of a model is the cheap way to produce an unfavorable verdict. It should be admitted, therefore, that most of the short-comings of the simple-minded IS-LM can be remedied.³¹ But we shall not set out on the tack of trying to clarify recent controversies through bigger and better IS-LM's. That approach has been tried and it has a bad track record. IS-LM provided the groundrules for the Keynes and the Classics debate -- and obedience to the rules produced the wrong conclusion.³² It has not been an at all helpful vehicle for the Monetarist debate.

To recapitulate: We began with the question of why real disturbances should have monetary effects and vice versa when these interdependencies do not occur in FIM theory. Answers of a sort were obtained by linking the two primitive A and T constructs by the rate of interest, while noting that choice-theoretical arguments strongly suggest both that commodity ED in A and that money ED in T should depend on the rate of interest. The resulting IS-LM construction has interdependence both ways because it will not allow the rate of interest to adjust appropriately "in the short run."

Monetary disturbances have real (allocative) effects because the interest rate changes -- when in FIM it should not. Real disturbances have monetary consequences because the rate of interest does not change far enough. In either instance, it is the maladjustment of the interest rate that is supposed to provide the link. These maladjustments, moreover, are to be ascribed to incomplete information. The IS-LM short-run "equilibria" are, in effect, transitional states in a sequential process that should eventually produce a new FIM state (unless, of course, there is a spanner in the works). If we go through the exercise of assuming full information, we find that these transitional states are skipped.

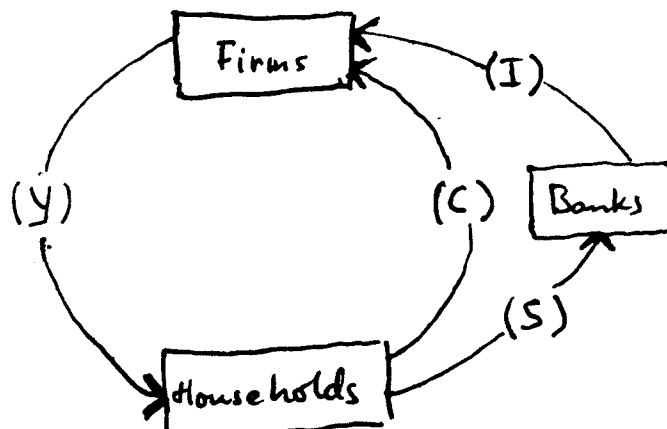
So, we can reassert our introductory contention that the maladjustment of the interest rate is a problem central to modern controversies. And we may now add that IS-LM is more a hinder than a help in coming to grips with it. We will abandon IS-LM, then, in favor of a more explicit sequence analysis that will make it easier to keep track of how -- and why -- the economy is supposed to diverge from its full information time-path. For the next several sections, Monetarism is left to one side as we trace the development of saving-investment approaches, beginning at the beginning, with Knut Wicksell.

IV. Wicksell

The point of departure is the Quantity Theory of Money. Wicksell regarded it as the only solid foundation for monetary theory and saw his own contribution as a development of the Quantity Theory.³³ With Quantity Theory, however, he understood simply the equilibrium proposition that prices will be proportional to the money stock in the long run and not the modern monetarist proposition that exogenous changes in base money drive nominal income and prices. Comparative static propositions about the invariance of real magnitudes to proportional changes in nominal values are of limited use in applied monetary analysis, however. To make the Quantity Theory useful, Wicksell thought, one needs to understand both what sets in motion a movement of money and prices from one level to another and the dynamic interequilibrium process itself.³⁴ Having conceived of his theoretical objective in such terms, Wicksell had to come up with "disequilibrium" analysis of some sort to meet it. His Cumulative Process was it. It is worth keeping in mind that this is how the Saving Investment approaches originally came to branch off from the Quantity Theory trunk.

How did Saving and Investment come to figure in a more "dynamic" Quantity Theory? The basic theoretical conception is simple enough. It combines our Models A and T in a particular manner. Model A is based on the "circular flow" notion. The Gestalt is of an economy consisting of two sectors: households (savers) and business firms (investors). The interaction of these two sets of agents determines income. Model T is a stock-flow model. Again, there is but one basic sectoral distinction -- between suppliers (the banking system) and demanders of money. Their interaction determines money income.

In Wicksell's basic image, the bewildering complex of interactions in the economy is reduced to a readily intelligible pattern of three sectors. The banking system is placed between the household sector and the business sector in the "circular flow". Household savings flow into, business investment finance flows out of banks.³⁵ Here, banks are perceived in the first instance as loan intermediaries between the household and business sectors, rather than as money suppliers. In the short run, this theory of nominal income determination will focus on changes in the flow of bank-intermediated credit rather than in the stock of money.³⁶ By lending more to the business sector than flows in as savings from the household sector, the banking system will cause the circular flow to expand. By lending less, they will make it contract. When nominal income is rising, investment exceeds saving by the net addition to loanable funds injected by banks. When nominal income is falling, banks let loanable funds "leak out" so that savings exceed investment. In income equilibrium, saving should equal investment; this requires that banks do no more and no less than intermediate the desired savings of the household sector. When they behave themselves "neutrally" the excess demand for final goods at the prevailing level of money prices should be zero.



The road from theoretical conception to analytical model is strewn with restrictive assumptions. A couple of the more important ones are institutional and time-bound and deserve notice here. For example, bank-financed consumer credit is ignored as of no quantitative significance. Bank injections or leakages of purchasing power into or out of the circular flow augment or curtail the spending of firms, not that of households. This, one may assume, was appropriate enough for Wicksell's time. Similarly, the banking system is assumed to dominate the outside financing of business investment -- the organized securities markets are ignored. For the economies that Wicksell had some familiarity with -- mainly Sweden and Germany around the turn of the century -- this was defensible as a "stylized fact." Again, the focus on bank credit rather than on the banking system's monetary liabilities should presumably be seen against the contemporary institutional background. Currency was the predominant means of payment in Sweden in Wicksell's time; the volume of checking deposits was relatively insignificant; the central bank did not yet have a monopoly on the note issue; banks were not constrained by reserve requirements calculated on their monetary liabilities on the later American pattern. Consequently, Wicksell defined "money" as currency, and treated this stock of currency as determined, at least to a first approximation, by the demand of the non-bank public. Wicksell's "money" varies endogenously in his cumulative process, it does not play a significant causal role.

For the analysis of the cumulative process, it will be convenient to have our theoretical terms and the relations between them defined in such a way that the following propositions hold:

i) the circular flow of money income and expenditures will expand if and only if there is an excess demand for commodities;

ii) "investment exceeds saving" implies "excess demand for commodities" and conversely;

iii) investment will exceed saving if and only if the banking system lengthens its balance sheet at a rate in excess of that which would just suffice to intermediate household saving;

iv) the economy will be on its real equilibrium growth path (capital accumulation path) if and only if savings equals investment.

v) the value of the interest rate that equates saving and investment at full employment is termed the "natural" rate.

It would seem that all sorts of things could happen that would violate one or more of these. The theory, presumably, says that these eventualities are either improbable or of little consequence. Nonetheless, what restrictive assumptions are we making to rule them out? The following may not be exhaustive but will illustrate what is required.

First, note that proposition iii) requires that expansion or contraction of bank credit be associated with any increase or decrease of nominal income. Hence, we should "rule out" any changes in the money volume of the circular flow in which the banking system does not play a part. Thus, we assume that changes in the spending flow associated with hoarding or dishoarding of cash already in the non-bank sectors by either the household or the business sector "may be ignored." Similarly, we ignore the possibility of changes in the aggregate money value of the demand for commodities associated with all-around expansion or contraction of non-bank trade-credit. Second, we want to ensure that any discrepancy between saving and investment be associated with a corresponding discrepancy between

household sector supply of loanable funds and business sector demand for loanable funds. To that purpose we assume that business sector financial saving may be ignored and that firms do not finance investment out of retained earnings. Also, that households do not use any part of non-consumed income for direct investment. Third, we define "saving" as the household sector's "desired non-consumption" of current output, and "investment" as the business sector's desired capital accumulation out of current output. Saving and investment, thus defined, have to be equal at full employment output for the system to be in intertemporal equilibrium.

This is quite a lot to build into the language! One had better not get too used to this brand of saving-investment analysis, obviously.

The central concepts of Wicksell's analytical apparatus are, of course, the market rate and the natural rate of interest. The terms are names for two values of the same variable.³⁷ The market rate denotes the actually observed value of the rate of interest, the natural rate the hypothetical value that the interest rate would take if and when the system is in equilibrium.

Equilibrium is used here in two senses at the same time. Their tight linkage is a result of the construction of Wicksell's model that we have outlined; in other analytical settings we might well want to keep them distinct. First, the equality of the market rate with the natural rate is a condition of "monetary equilibrium" in the sense of the system remaining at a stable price level. Second, it is also a condition for maintaining "real equilibrium" in the sense of an allocation of current resources between consumption and investment such as to be consistent at the same time with the intertemporal consumption preferences of households

and with the intertemporal production possibilities perceived by firms.³⁸ The two are tightly linked in the sense that a divergence of market from natural rate will upset both at once and neither can be upset except by such a divergence occurring.

It is worth being a bit pedantic about this. Decades of Keynesian hegemony have accustomed us so much to one saving-investment approach that we almost accept it as the natural language for analyzing changes in the income level. The equality of saving and investment is a condition for constancy of the aggregate price level in this model because of propositions (i) and (ii) above. But we forced these to be "true" (as properties of one saving-investment approach language) by making numerous restrictive assumptions. The result of making (i), (ii), and (iii) "true" is an analytical language³⁹ that would be highly artificial and inconvenient to use for anyone who believes that, empirically, hoarding and dishoarding by non-bank sectors (i.e., variable velocity) and changes in the money stock are the most frequent and significant "causes" of changes of nominal income, and that neither cause need necessarily distort the intertemporal allocation of resources.⁴⁰ The saving-investment approach language is really designed for someone who believes to begin with that "real" disturbances are responsible for observed income fluctuations.

For the description of the cumulative process itself it hardly matters, however, whether it is set off by "real" or by "monetary" disturbances. We start from some historically given equilibrium -- saving equals investment at full employment and with stable prices. A Wicksellian "monetary" disturbance would have to be in accord with (iii) above, i.e., it is triggered by the banking system moving the market rate away from an

unchanged natural rate -- neither the preferences of savers, nor the investment prospects perceived by entrepreneurs have changed, so no reallocation of resources is called for. In the case of real disturbances, we start with a shift of either the investment or of the savings function. The allocation of resources between consumption and capital accumulation should change if the system is to adapt appropriately. Coordination of the intertemporal allocation of resources by consumers and by producers requires a change in the interest rate, i.e., the natural rate moves. If the market rate is prevented by the banking system from keeping up with the change in the hypothetical natural rate, the cumulative process starts. But the description of the process is the same whether the banking system plays the active or a permissive role in it.

From the taxonomy of possibilities, we take the one that Wickseil himself concentrated on. In Fig. 1, we have saving and investment schedules represented in real terms. Initially, we are in equilibrium

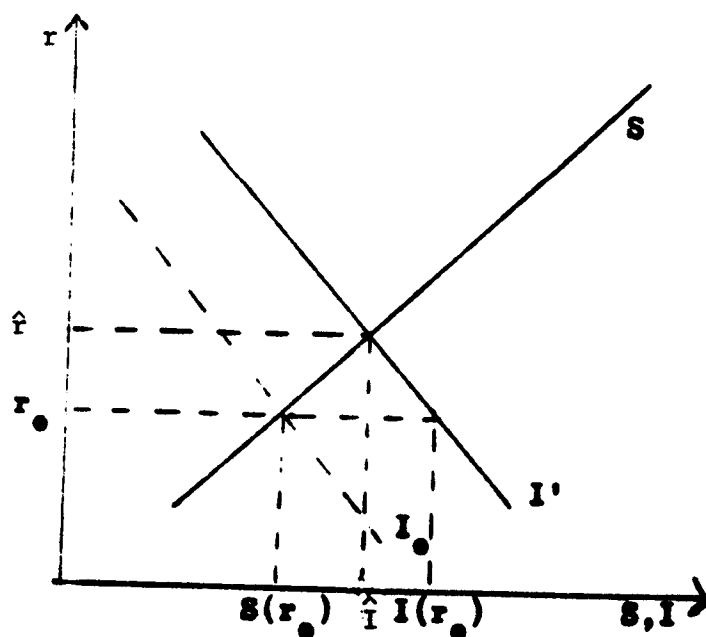


Figure 1

with the interest rate at r_0 . Assume an upward shift in the investment schedule from I_0 to I' . The impact-effect of this disturbance is to create an excess demand for commodities measured by the investment-saving gap, $I'(r_0) - S(r_0)$. To this there corresponds an increase in the demand for loanable funds of the same magnitude.⁴¹

To avoid inflation and, at the same time, obtain the proper allocation of resources, we require the interest rate to move to its new natural value, \hat{r} . A rise of the rate from r_0 to \hat{r} would decrease the demand for consumption goods by $S(\hat{r}) - S(r_0)$ and that for capital goods by $I'(r_0) - I'(\hat{r})$; together these adjustments of the spending intentions of households and firms would just suffice to eliminate the excess demand for commodities, and consequently the associated inflationary pressure, opened up at impact by the postulated disturbance. At the saving-investment equilibrium corresponding to \hat{r} we would also have the economy on the new and higher growth-path appropriate to the now more favorable collective judgment of firms about the terms on which society as a whole can convert present into future goods.

The market rate of interest is governed, not by the investment-saving discrepancy, but by the excess demand for loanable funds. When this excess demand is zero, the rate will not move. Now, by the assumptions discussed earlier, we have ensured that saving equals the supply of loanable funds by households and investment the loanable funds demand by the business sector. If there were to be no other components to the excess demand for loanable funds than these two, therefore, the interest rate would be driven to its natural level at which saving and investment intentions are consistent and compatible with retained price stability. But, of course, the

banking system is also in the loanable funds market -- in Wicksell, in fact, it makes the market. We will get exactly the desired adjustment of the economy if and only if the banking system sticks to the "neutral" policy of just intermediating household saving without generating any net injection (or leakage) of loanable funds on its own.

In the cumulative process, of course, the banking system does not stay "neutral." To avoid complicating the diagram, we may assume the extreme case where the banks -- usually with the connivance of the Central Bank, presumably -- simply accommodate all of the loanable funds demand, lending on all "good paper" that comes their way, at the initial rate of interest. The credit market then "clears" at what is, from a general equilibrium standpoint, a disequilibrium level of interest rate, $r_0 < \hat{r}$. The excess demand for commodities, represented by the investment-saving gap at this market rate, will then drive the price-level up.

In this process the consolidated balance sheet of the banking system will be lengthening at a rate in excess of what would be the equilibrium real growth-rate of the economy. This, of course, can just as well be looked at from the standpoint of the liabilities side of that balance sheet. There we would find an acceleration in the growth-rate of "money"; it should show up independently of whether we have chosen to define "money" as currency, as M_1 , or as M_2 . The expansion of the money stock is endogenously determined in the process, yet the process is entirely consistent with observed velocity being constant. The link of Wicksell's work to the pre-Monetarist Quantity Theory would, indeed, be more obvious if we added in an explicit "Cambridge- k " demand function for currency. Then, as nominal prices go up, non-bank agents will choose to hold a

corresponding fraction of the growing liabilities of the banking system on hand as cash.

In the Wicksell process, two sets of agents are acting on incorrect or incomplete information. The banks fail to recognize (the full extent of) the rise in the realizable real rate of profit. Entrepreneurs (and other non-bank transactors) fail to foresee the rate of inflation that the policy of the banking system implies.⁴² The process would go on cumulatively as long as the market rate is kept below natural rate. The only way to make sure that it will not proceed indefinitely is to see to it that a banking system acting in this way will necessarily find itself running out of reserves. As long as non-bank agents do not learn to anticipate the inflation, and as long as the real determinants of saving and investment behavior do not change, each successive period would start with an excess demand for commodities in real terms equal to $I'(r_0) = S(r_0)$. In nominal terms, of course, the gap keeps growing. If inflationary (rational) expectations take hold before the banks mend their ways, things get worse: the inflation accelerates⁴³ and a move of the (real) market rate into equality with the (real) natural rate would now merely stop the acceleration but not suffice to end the inflation. The Fisherine complications of expected inflation will, however, be dodged here and in what follows.

V. Keynes: Treatise

From his early book on Indian Currency (1913) to the Tract on Monetary Reform (1923), Keynes remained a Quantity Theorist in the Marshallian Cambridge tradition mainly concerned with questions of how to regulate the supply of money, how to stabilize the monetary liabilities of a fractional reserve banking system against inflows and outflows of international reserves, and how to structure the international monetary system so as to minimize such problems. Much of the Treatise on Money (1930) deals with these same problems.⁴⁴ In fact, the Treatise as a whole is very much a work still recognizably in the Quantity Theory tradition, despite its emphasis on problems of the short run. Here, however, these older themes are left aside to focus on the novel ideas of the work: the first Keynes variation on Wicksell's theme.

The problem now is deflation rather than inflation. As with Wicksell, we have to conceive of the system as initially in an equilibrium state with stable prices, etc. Here we have to take particular note of the initially existing stock of money for in our analysis of the basic process we are just going "to leave it there" -- taking the banking system out of the picture. In Keynes' version, the main character of Wicksell's morality play is written out of the plot!

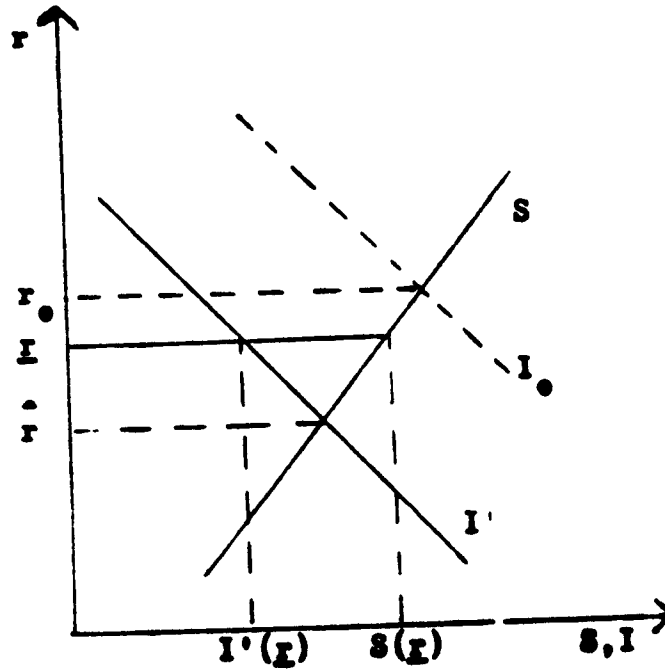
To Keynes, familiar with the City of London, "the" interest rate is determined, not by the banking system, but on the Exchange. It is a good example of how monetary theory is adapted to changing institutional circumstances. In Wicksell, the market rate was governed by the excess flow demand for loanable funds. In the Treatise, analysis of interest determination is a stock-flow problem -- a matter not just of the financing of

new investment but also of trading in the outstanding stock of "old" securities.

We now consider the consequences of a decline in the marginal efficiency of capital (using Keynes' later terminology) from I_0 to I' in Fig. 2. At impact, we get an excess supply of commodities at the old interest rate measured by $S(r_0) - I'(r_0)$. To this there corresponds an excess flow demand for "new" securities. Households intend to accumulate securities at the rate $S(r_0)$, while the business sector intends to float securities only at the rate $I'(r_0)$. This puts upward pressure on securities prices and the market rate of interest starts to decline. The banks, we assume, stay put and do not intervene to confuse the communication about intertemporal tastes and opportunities between households and firms. But speculators on the Exchange do. Having experienced for too long rates of yield in the neighbourhood of r_0 , the market turns predominantly bearish as securities prices edge up. At \underline{r} , we suppose, bears are selling off "old" securities to savers at a rate that exactly makes up the gap $S(\underline{r}) - I'(\underline{r})$ and are absorbing "idle" cash out of the "active circulation" at the corresponding flow rate. The securities market now "clears"; from a general equilibrium standpoint, the interest rate rests at a disequilibrium level with no tendency to adjust further. The incomplete interest rate adjustment leaves an excess supply of commodities, matched by an excess flow demand for cash, both measured by the gap at \underline{r} -- with a consequent deflationary pressure on nominal income.

Wicksellian business cycle theorists of that time were apt to acknowledge that when, with the market rate above natural rate, nominal income declines, real output and employment reactions must be expected to occur. Keynes was no exception.⁴⁵ But in so doing they were informally realistic rather than

formally analytical. Output reactions could not be handled by Wicksellians--whether Swedish, British, or Austrian--as of 1930. If we want to draw clear-cut comparisons between the two variations on our theme, therefore, we had better take them literally and consider the process that is being explained in each case to be one of pure deflation at the historically given full employment position.



First, the obvious contrast. Wicksell's would be a deflation associated with a declining stock of money -- it drains out of the non-bank sector. Possibly, observed velocity could be constant. Keynes' deflation shows declining velocity -- cash is going into the "hoards" of bearish speculators. Possibly, the observed money stock could be constant. Actually, both men would presumably have been "reasonable" about it and acknowledged that both the money stock and its velocity should be expected to decline in a "real world" deflation. For our present purposes, however, allowing such informal realism will make us miss the point: that the behavior of the banking system

is edging out of the focus of Keynes' developing analytical scheme. By the time we get to the General Theory, it is out of the picture altogether.

Second, the disequilibrium of both theories stems from the divergence of beliefs about economic prospects between two sets of agents. Keynes' bear-speculators disagree with the appraisal of entrepreneurs.⁴⁶ So, in effect, do Wicksell's banks. In either model, if all agents acted on consistent beliefs, there would be no problem -- not even if they were all wrong.⁴⁷ Agents acting on inconsistent beliefs is what disequilibrium means. Who then is wrong and who, if anybody, is right? I think it is in the spirit of both Wicksell and Keynes to suppose that, in the early stages of a nominal income decline at least, the entrepreneurs are roughly right. One might argue this supposition à la Hayek. The individual entrepreneur will have more and better information about the particular circumstances relevant to the prospects of his firm and his branch of industry than will speculators in general.

Third, then, beliefs -- even if qualified beliefs -- in the "automaticity" of a private enterprise market system will rest, in part, on the presumption that the working of the price system will either teach agents acting on the wrong beliefs to mend their ways or else deprive them of the wherewithal influence the course of events in the large.

This presumption is by and large still supported by the two disequilibrium theories so far considered -- although they suggest that it may take time. But as long as the system deflates while remaining at full employment, Wicksell's banks would have to continue period after period to let net deposit growth plus repayments of old loans run ahead of their new lending. It is an unprofitable policy and the cumulative process would also cumulate the pressures

on the banks to reduce their rates. Keynes' speculators would have to continue, period after period, to sell off income-earning assets from their portfolios and replace them with cash.

In either case, then, the disequilibrium strain in the economy is brought home on those responsible -- and in such a way as to induce them to revise that price which is "wrong" in the "right" direction. And the longer they persist in the behavior inimical to the coordination of activities in the system, the more expensive it gets.

VI. Interpolation: Z-theory

The big difference, it is generally agreed,⁴⁸ between the Treatise and the General Theory is that, in the latter, the system responds to deflationary pressure with a contraction of output and employment. Let us proceed first as if that was the only difference. Suppose then that the deflationary pressure on nominal income comes about exactly as in the Treatise. Incorporation of the output and employment response into the analysis will change the story profoundly in two respects:

1) Real income falls from the full employment level, \hat{X}_0 , until saving equals investment at a real income of \underline{X} . In terms of the diagram, the saving schedule shifts over until it intersects I' at \underline{r} (Fig. 3). This market rate, ex hypothesi, exceeds the natural rate, \hat{r} , i.e., the interest rate that would prevail on the system's equilibrium growth-path. As in the Treatise and in Wicksell, although the price is "wrong", this market "clears." But unlike Wicksell and the Treatise, we no longer have an excess flow supply of loanable funds whose accumulating time-integral progressively distorts the balance-sheets of banks and/or bear-speculators. With saving and investment equal, Keynes' speculators need no longer sell off more securities every period to maintain the market rate above the natural level. In this variation on the theme, the system is not generating pressure towards the appropriate adjustment on the right point.

2) In the labor market, we have instead unemployment at the going money wage rate. Note that, if the interest rate had gone to its natural rate level before any quantity-reactions set in, the labor market would have cleared at this money wage. The relationship between the money stock and the money wage rate is still the same as in the earlier full employment

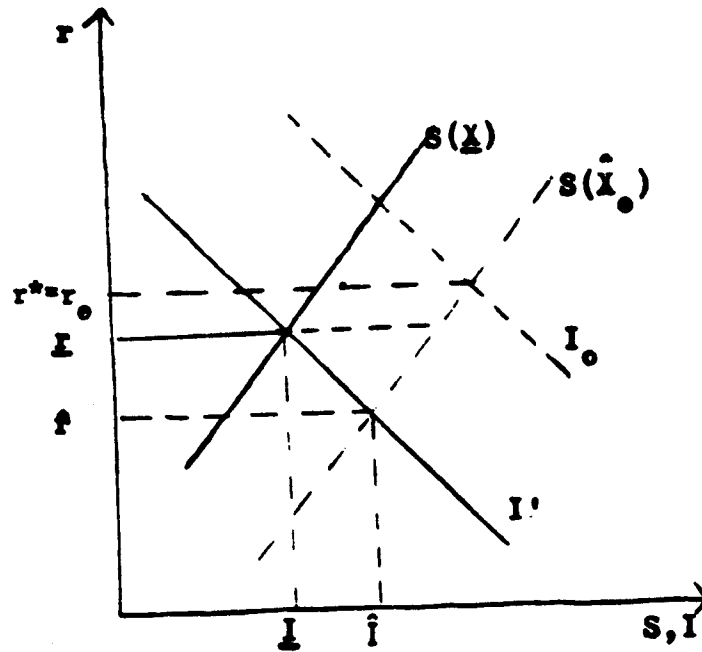


Figure 3

situation. The adjustment of the system to a decline of the marginal efficiency of capital does not per se require a general lowering of money wage rates.⁴⁹ In relation to the price vector that the system would have exhibited, had it managed to stick to its equilibrium path, money wages are not "too high" for full employment — they are (roughly) at the right level. Yet, in this market, there is heavy excess supply pressure on the price.

That is not the end of the trouble, of course: In this market, where the price is where we want it, excess supply persists and downward adjustment of the price will not remove it.⁵⁰

This diagnosis of the early recession phase of a developing depression, and the dynamic sequence postulated to lead up to it, are central to Keynes' case. His deep depression theory, which adds in the induced deterioration of entrepreneurial expectations about future demand, is the basis for his pessimism over the effectiveness of monetary policy and

for his advocacy of fiscal policy measures; but the deep depression analysis is derivative in relation to this variation on the Wicksellian theme.

Keynes' fundamental contention that a competitive, private enterprise market economy (with all its prices "flexible") may fail to home in automatically on its equilibrium time-path stems from the contemplation of states like the one just sketched: the interest rate is wrong, but that market "clears" (without "punishment", so to speak, of those responsible); the money wage is right, but large-scale unemployment prevails and persists and even the willingness of labor to reduce the money wage will not help. The system's "automatic" adjustment tendencies, presumed in pre-Keynesian analysis to be self-regulatory, are working to change prices that are right and leaving those we need to have changed alone; the response of price to excess supply of labor does not bring about a meshing of quantities in that market.

All of this was missed in the earlier Wicksellian literature. It is instructive to note why. In that literature -- of which the Treatise is a good example -- the task of explaining business cycles was approached in two stages. At the first stage, one concentrated on the causes of price level movements. The saving-investment theory presented at this first stage was relatively formal and, although in verbal dress, came pretty close to what today would be recognized as "modelling". At the second stage, one then gave informal, ad hoc recognition to "frictions", i.e., to the stylized fact that an economy under deflationary (or inflationary) pressure will in the short run respond with output and employment adjustments and not only with pure price adjustments. This amounts to changing the interpretation of the Stage 1 saving-investment model, treating it as a theory of changes in nominal income, leaving it to Stage 2 to determine --

as if it was a separable question -- how much of the nominal income change is a real output change. This procedure presumes, in effect, that allowing real income to vary should not significantly affect the Stage 1 analysis. This overlooks the dependence of real savings (loanable funds supply) on real income.

It was precisely his discovery that this two-stage approach would not do that prompted Keynes to tear up the Treatise and spend the next six years rewriting it -- with the quantity adjustments worked out explicitly within the short-run period. We get a measure of the extent to which we have lost track of what Keynes was about, therefore, when in the context of a controversy over what distinguishes Keynesian economics from other doctrines, today's macroeconomists revert to the two-stage approach that was in general use in the 1920's. After some forty years, the two-stage approach reappears in the Monetarist controversy and is used, without objection, by both sides: nominal income changes are predicted or analyzed first and then some Phillips-curve notion is brought in to decide their breakdown into price- and quantity-adjustments.⁵¹

The lesson that Z-theory adds to what can be learned from the pre-Keynesian business cycle literature is that one should not analyze the characteristic Wicksellian interest maladjustment and its consequences as if it does not matter whether the system maintains itself at full employment or not. At full employment, there will be unrelenting pressures towards correction of any "unnatural" rate of interest -- but otherwise not. Neither can one analyze unemployment and its persistence as if it does not matter whether the market rate equals the natural rate or not. With the interest rate at the right level, market forces should make unemployment converge on its "natural" rate -- but otherwise not.⁵²

In Section III, we asked why macroeconomists should split into Keynesians and Monetarists over the Phillips-curve in the same way as they had previously done over the issues defined by Model A vs. Model T and by IS-LM. The answer is now clear. Monetarists believe that interest maladjustments are unlikely to be of much consequence; hence the coordination of saving and investment is not a serious problem; consequently, flexible wages should serve to coordinate labor supply and demand; the speed of convergence on the natural rate of unemployment is merely a matter of "frictions" the strength of which raises empirical but not theoretical questions. Keynesians believe that flexible wages would not ensure convergence on full employment. They seem to have forgotten why, however.

VII. The General Theory: The Liquidity Preference Theory of Interest

Our Z-theory "interpolation" between the Treatise and the General Theory is still recognizable -- indeed, unmistakable -- as a variation on Wicksell's theme. Without the maladjustment of the interest rate, there is no story to tell. But the General Theory is not just the Treatise with output variable.⁵³ There are other developments between the two works. And, for the Wicksellian theme, they add up to the nearly perfect cover-up. The main one, to which the others are subsidiary, is the Liquidity Preference theory of interest.

In Wicksell, the excess flow demand for loanable funds governs⁵⁴ the interest rate. The Treatise has a stock-flow treatment where the interest rate must clear a market that trades not only in new issues but also in outstanding securities. In the General Theory, Keynes goes to a pure stock-analysis of interest determination. This last step would be retrogressive in itself.⁵⁵ But, on top of that, it is the excess stock demand, not for securities, but for money that -- he argues -- governs the interest rate. Not an intuitively appealing doctrine! How did he get stuck with it?⁵⁶

The trouble starts with "Savings equal Investment". Keynes used both terms, in Marshallian fashion, to denote observable quantities (rather than planned or desired magnitudes -- as would be the Walrasian fashion). He went on to show that, adopting certain not unreasonable terminological conventions, ex post saving had to be identically equal to ex post investment. Having absorbed that point, take a "fresh look" at Wicksell's image of the circular flow: If household saving and business investment are necessarily and therefore continuously equal, then it would seem that the

banking system cannot possibly be doing anything else but simply serving as an obedient go-between. So, we can just as well erase it and adopt the simplified picture of the circular flow that goes with the Keynesian cross.⁵⁷

If saving and investment are continuously equal, the rate of interest cannot possibly be governed by any difference between them. The possibility of a corresponding excess flow demand for loanable funds has then also been defined away. The loanable funds interest mechanism is gutted. So, the flow part of the Treatise's stock-flow analysis should be erased. The speculative element remaining from it now has to make do as a complete interest theory. Loanable Funds are out; Liquidity Preference is in.

This Liquidity Preference theory, moreover, can be nothing but a "bootstrap" theory, as D. H. Robertson termed it.⁵⁸ Beliefs about where the rate will be tomorrow determine where it will be today. But in deciding where it is likely to be tomorrow, speculators have no reason to contemplate "real forces" of Productivity and Thrift. In a system lacking the loanable funds mechanism, there can be no tendency, however weak or fitful in its operation, for the interest rate to converge on the natural rate consistent with the equilibrium growth path. It is an obvious but important property of our Z-theory that the market rate will track the natural rate unless banks or speculators intervene to "fix" it at some other level. The General Theory emphatically denies any such basic tendency -- whether in the short or in the long run -- for the interest rate to find its FIM value. And it makes no sense, in context, to ask what the rate would be unless speculators "fixed" it.⁵⁹

So, we arrive at the short-hand characterization of how the General Theory corrects the "errors" of the Classics: Saving and Investment determine Income, not the Rate of Interest.⁶⁰ The Money Supply and Liquidity Preference determine the Interest Rate, not Income.

With these last two propositions, any intelligible connection to mainstream Quantity Theory abruptly disappears. In the earlier variations on the Wicksellian theme the relationship to the Quantity Theory was obvious. Wicksell used the saving-investment approach to explain how a real disturbance may trigger an expansion of the banking system and hence a rise in prices together with growth of the currency stock. Keynes, in the Treatise, used it to show how a real disturbance may reduce velocity and thus put an economy under deflationary pressure. A Quantity Theorist of non-monetarist persuasion, used to organizing his analysis of price level changes in terms of factors affecting money supply and money demand, would have no trouble understanding the use of the saving-investment approach in these Wicksell variations. But in the General Theory, a decline in investment demand will reduce the rate at which money is being spent for goods without, apparently, having at all affected the excess demand for money (whether by decreasing the supply or increasing the demand). And, indeed, money supply and money demand are emphatically said to have nothing to do with expansions and contractions of the circular flow (of money), except insofar as they have an indirect influence via the rate of interest on the saving-investment balance.

The other idea, lost sight of in the firecracker string of brilliant paradoxes ignited by the "insight" that the equality of saving and investment is an identity, is that of the maladjustment of the interest rate as

the root of the trouble. It is clear enough that the interest rate is "too high" in the simple sense that, were it only lower, investment and hence employment would be higher. But the same could be said of the solution values for all the other endogenous variables as well -- they would have to be different to be consistent with full employment. That the emergence of the gap between market and natural rate is a crucial link in the causal story of how the persistent unemployment state develops is almost totally obscured. The bootstrap theory of interest erases the natural rate notion and so leaves no equilibrium reference mark by which to define the "divergence" of the market rate.

The trivial last touch to the cover-up is terminological:

I am no longer of the opinion that the concept of a 'natural rate' of interest, which previously seemed to me a most promising idea, has anything very useful or significant to contribute to our analysis. It is merely the rate of interest which will preserve the status quo; and, in general, we have no pre-dominant interest in the status quo as such.⁶¹

The reasons for getting rid of the Wicksellian terminology should be obvious in the context of this section. Keynes' wording of the disposal of interest mainly because it reinforces the impression one gets from his controversy with Hayek, namely, that he had given very little thought to problems of capital and growth theory.⁶² Wicksell's status quo was always a state on an equilibrium growth path. There are then two options for someone who wants to free Wicksellian theorizing from its dependence on the assumption that "yesterday was equilibrium". One is to tie the notion of the natural rate to the preservation of yesterday's income and employment level. The other, of course, is to identify it as the (FIM) rate of interest consistent with equilibrium growth -- and, therefore,

with the allocation of resources between consumption and capital goods producing industries which, at full employment, will entail equilibrium growth. Keynes, we notice, took the conceptually trivial option.

So, in this fourth and last variation, Wicksell's rabbit has been stuffed back into Keynes' hat, not to be seen again. An astonishing slight of mind!

VIII. Cambridge Keynesians

Liquidity Preference versus Loanable Funds marks a critical fork in the road for professed followers of Keynes. D.H. Robertson turned back to a personal view very much like Z-theory. Joan Robinson and Richard Kahn, like the Master, took the Liquidity Preference turn, soon to go beyond where Keynes left off. The Neoclassical Keynesians -- going flat out in their brand-new IS-LM machine -- failed to see any fork and ended up in the bog between the roads.

Robinson and Kahn in their writings on the matter⁶³ make clear that the crucial aspect of the LP theory is not some assumed property of a comparative static model but an hypothesis about a sequence in a dynamic-historical theory. A stock-flow dimensional argument⁶⁴ can, moreover, be utilized to free the hypothesis from too obvious a reliance on the ex post saving-investment identity.

The sequence (in my paraphrase) goes as follows. Assume an initial equilibrium. Suppose some event such as will reduce the marginal efficiency of capital. At impact, saving now exceeds intended investment at the initial levels of income and interest. This saving-investment gap, if it is maintained, can have no effect on the rate of interest. In the very short run, stock supplies and stock demands determine asset prices and yields; the excess supply of loanable funds, being a flow, is of no consequence; Liquidity Preference and the stock of money determine the rate of interest; since neither was affected by the shock to MEC, the interest rate remains constant at this stage.⁶⁵ The interest rate, consequently, cannot play any part whatsoever in closing the saving-investment gap opened up by the postulated disturbance; it has to be closed altogether through income

adjustment. Next, then, the required fall in income will "release" some stock of transactions balances;⁶⁶ these "excess balances" drive down the rate of interest to the level required for them to be voluntarily held given the prevailing state of Liquidity Preference.⁶⁷

This Liquidity Preference sequence is crucial. Clearly, any theory that incorporates it will attach a probability of zero to a successful traverse from one growth-path to another. The economy cannot do it for the simple reason that the only price mechanism that might do it never gets into play to coordinate saving and investment decisions.

In this theory, furthermore, "Saving equals Investment" simply because investment must "cause" savings to a like amount. In the closed economy, investment is always self-financing.⁶⁸ Saving, on the other hand, is a "passive" variable of no causal significance.⁶⁹ Saving-behavior, consequently, has nothing to do with determining capital accumulation and growth -- except insofar as higher saving propensities give you lower output and employment for any given volume of investment (basically, the "Paradox of Thrift").⁷⁰ The theory of capital accumulation, therefore, has to focus altogether on the business sector side since the households are out of it. The result is a fable in which "animal spirits" and technology determine growth. As a trivial corollary, various neoclassical fables, in which saving "causes" capital accumulation, rather than the other way around, are seen to be specious and not fit to conjure with. From this Cambridge perspective, Neoclassical Keynesians are seen to be prone to heresy because of their uncertain grasp of the fundamental dogma that "Investment causes saving but saving does not cause investment".⁷¹ And the Monetarists, of course, are the very heathen, still in the grasp of the ancient superstition

that the excess demand for money governs money income while saving and investment govern the interest rate rather than the other way around.

On the controverted issues of Keynesian theory, finally, the Cambridge Keynesians have always understood quite clearly that, however disguised, our Z-theory argument remains imbedded in the General Theory. Consequently, they reject all the main elements of the so-called Neoclassical Synthesis, i.e., the Liquidity Trap and/or rigid wages interpretation of Keynes' unemployment theory, and the notion that the Pigou-effect "saves the logic" of pre-Keynesian doctrines. Their adherence to the LP theory of interest, however, precludes them from reasserting Keynes' argument against the Synthesizers in the unabashedly Wicksellian terms used here.⁷²

Adherence to the LP theory of interest also explains, one assumes, why Cambridge Keynesians since the death of Keynes have showed no further interest to speak of in monetary theory. The LP theory deprives the rate of interest of its role in coordinating saving and investment; without that role, the interest rate is a variable of little, if any, significance; and money supply and demand, in Cambridge Keynesian theory, have no other job but to govern this uninteresting variable.⁷³ Monetary theory is seen as a dead-end street.

There is some irony here. Keynes worked his way to a theory of the malfunctions of a capitalist economy through a maze of monetary and financial complications. To the Cambridge school his preoccupation with such questions has come to seem an embarrassing concession to pre-revolutionary ways of thinking. Kalecki, who never entertained the notion that the system might work and who saw little need to explain why financial markets might not coordinate saving and investment, becomes the more logical figurehead.

This is especially so since Keynes shorn of his contributions to monetary theory would not amount to more than a minor pre-Kaleckian. As an economic scientist, Keynes was nothing much, if not a monetary theorist.

IX. Neoclassical Keynesianism

Where are all the Keynesians now? Was there ever such a school? I for one would feel cheated if the answer were: No.⁷⁴ Who could doubt that there was an American Keynesian school back in the days of Heller, Ackley, and Okun at the Council? How could one write the history of economics over the last forty years without it?

Only a few years ago, it was said: "We are all Keynesians now." Today, one asks: Who were they? What did they believe?

There obviously was an American Keynesian school in the days of Alvin Hansen's famous Harvard seminar and for some time thereafter. One cannot off-hand date its demise. But it is doubtful that anyone who has gained prominence in the profession and is now under the age of 40⁷⁵ would accept the label "Keynesian" for himself. So we know the school is done for.

Over the intervening decades, most of the major names in economics and almost all the most admired technical innovators occupied a macroeconomic position in some sense in the center between the Cambridge Keynesians on the one side and the Monetarists on the other. This macroeconomic center has been routinely called "Keynesian" in the United States. But, on the issues of this paper, it is curiously difficult to say what the centrist position has become or what is "Keynesian" about it. The topnotch people in the center can only be described as "eclectic" in theory and "pragmatic" in policy. Those terms, however, are descriptive of judicious temperament rather than expressive of a shared and coherent system of beliefs. On occasion, Robert Solow or James Tobin will make it his business to explain where Joan Robinson or Milton Friedman go too far. But this is not enough --

unless "anti-Monetarist" is to become the residual definition of "Keynesian".
Would there be no Keynesianism any longer except for Milton Friedman?⁷⁶

This American Keynesianism also exists at another level -- that of teaching and textbooks. Here one can find the Paradox of Thrift surviving and a continuing emphasis on the saving-investment problem.⁷⁸ The trouble with the textbook tradition is that one does not know whether anyone of consequence takes what it says seriously.

It is clear, I think, that Keynesianism has had no Champion in the United States for quite some time. There have been no leading figures concerned to maintain a semblance of intellectual coherence in Keynesian macroeconomics. Nor has there been a Keynesian "research programme" for perhaps twenty years. The best people in the center have been neo-Walrasian all along. Some of them have used the designation "Keynesian" as a flag of convenience when sailing in the muddy waters of short-run stabilization policy. As such, it is recognized as signalling belief in the usefulness of discretionary fiscal policy, etc. But its use entails approximately the commitment to the integrity and development of Keynesian theory that the world's shipowners harbor for the peace and prosperity of Panama.

Well, no one was obliged to keep the Hansen school running. Nor could one argue that our intellectual resources would have been better allocated if, say, Paul Samuelson or Lawrence Klein had taken time out for the purpose. The point is simply that we must recognize American Keynesianism for what it is: a tribe of no chiefs, all injuns.⁷⁹

The trouble is that this makes Neoclassical Keynesianism difficult to get a grip on. Obviously, it has drifted far away from the Hansenian verities. But it is difficult to say where American Keynesianism has ended up.

Critical changes in doctrine have come to pass virtually without controversy, or without a clear verdict on whatever controversy did take place. It is as if change were almost inadvertent -- or as if nobody cared. The accumulated result is an intellectual mushiness that defies attempts at sharp definition.

Most astoundingly, Neoclassical Keynesianism has ended up in a position that has proven difficult to demarcate from Monetarism in any significant way. But -- how could that be? When Warburton stated the essentials of the Monetarist position in the 1940's,⁸⁰ the Keynesians were too sure of themselves to take his challenge seriously. Nonetheless, it is perfectly obvious what kind of arguments they would have employed at that time, had they deigned to answer him: "saving and investment decisions are made by different people; the interest rate cannot coordinate them; saving and investment determine income and employment; flexible wages will not restore full employment, if saving exceeds investment at full employment income" ... and so on. A later generation of Monetarists has proven more difficult than Warburton to ignore. But in the battle against their growing influence, these saving-investment arguments have played virtually no role at all.⁸¹

The theory held by the dwindling center is some sort of "Keynesian" theory minus the saving-investment problem! This is most readily apparent in employment theory. In the course of the interminable Phillips Curve controversy, the supposedly Keynesian centrists have used almost every conceivable argument against the natural rate of unemployment theory except the original Keynesian one -- namely, that the economy will not home in automatically on full employment unless the interest rate is such that saving will not exceed investment at full employment real income.

Keynes without the saving-investment problem is like Marshall without supply and demand or Adam Smith without the division of labor. But the Neoclassical Keynesians traded in Keynes' hypothesis of intertemporal coordination failure for the old pre-Keynesian hypothesis of rigid wages as the central idea of their unemployment theory. In the process, the Wicksell Connection was entirely lost from sight.

The elimination of the Wicksellian idea of the maladjustment of the interest rate with the consequent saving-investment problem and the adoption, instead, of the rigid wages explanation of unemployment were both enshrined in the so-called Neoclassical Synthesis, that fake Keynes and the Classics compromise which was preached for decades by practically everybody -- and in the end not defended by anybody.⁸² The analytical vehicle for the Synthesis was the Hicks-Hansen IS-LM model. The full story of how American Keynesianism ended up in this position would make a long and tangled tale. Here, we simply note some of the steps on the way.

The troubles with keeping track of the Wicksellian theme in its Keynesian guises and disguises go far back in time. The original "Saving-equals-Investment" debate did not reach a clearcut collective verdict. As Lipsey has recently shown, confusion persists -- and is tolerated -- on aspects of this issue to the present day.⁸³ The IS-LM framework did not lend itself too well to a sharp characterization of the question whether the excess demand for bonds or the excess demand for money governs the interest rate. It was concluded that the distinction between the Loanable Funds and Liquidity Preference hypotheses was probably either pointless or misleading and that, in either case, the issue could safely be left unresolved. Correspondingly, Hansen found, Keynes' insistence that saving

and investment determine income while money stock and liquidity preference determine the rate of interest (rather than the other way around) makes no sense once you realize that, in IS-LM, everything simultaneously determines everything.⁸⁴ Hansen's failure to understand the issue became of great importance, for his influence was such that all the issues separating Z-theory from Cambridge Keynesianism were left unresolved among his followers. A contributing factor in this malign neglect was the doctrine of the interest-inelasticity of both saving and investment which held sway among Keynesians for two decades or so. It made the problems of interest theory into trivia questions: What does it matter which excess demand governs the interest rate, if movements in the rate could not in any event coordinate saving and investment?

The interest-inelasticity doctrine did not last but, in the meantime, the Pigou-effect came eventually to draw attention altogether away from intertemporal coordination failure as a cause of unemployment.⁸⁵ Saving tending to exceed investment at full employment income could not be the trouble in a system with flexible prices; a sufficiently large increase in real cash balances could reduce saving to the extent required; one might imagine a deflation of such magnitude as to bring this requisite real balance effect about. This argument left rigid wages as the remaining explanation of unemployment.⁸⁶

To American Keynesians in the fifties, this return to the Classical explanation of unemployment cannot have seemed a very important concession to pre-Keynesian (or anti-Keynesian) thinking. But it stored up trouble for the future. To the next generation of Keynesians it was to make a riddle of stagflation: How explain it except by now postulating the rigidity of the

rate of change (and, perhaps, higher derivatives) of money wages? And it paved the way for a far more comprehensive return to Classical doctrines than the pragmatic centrists were, in fact, prepared to accept.

Other developments in the fifties were part of the general drift towards Monetarism and, eventually, the New Classical Economics. The speculative demand for money, for example, tended to disappear from view once Baumol and Tobin had provided rationales for interest-elasticity of the transactions demand for cash. The idea that conflicting beliefs about the future were at the root of the economy's disequilibrium was buried deeper and deeper. The substitution of an interest-elastic transactions demand for speculative demand also left a stable and predictable velocity function which, given that the Keynesians usually treated the money stock as exogenously fixed, reduced Keynesian monetary theory to a Variable Velocity Quantity Theory. From that position, in turn, it became difficult to see that the controversy with the monetarists could possibly be about anything else than the interest-elasticity of money demand -- although why one should make that number the occasion for such a deeply felt schism is hard to fathom.

Neoclassical theory was also being put into the IS-LM framework in piecemeal fashion as production functions, new consumption and investment functions were deduced from neo-Walrasian microtheory and replaced the original constructions. Interest rates settling at the "wrong" level, in Wicksell-Keynes fashion, could have been a problem in the formulation of the intertemporal aspects of these new IS-LM elements but does not seem to have been widely discussed as such. More indicative still, the sixties saw the Harrod-Domar and Hicks-Goodwin models go back into the library stacks as their place in Graduate Reserve was absorbed by the burgeoning literature on neoclassical growth.

How far this neoclassical conversion went was, perhaps, not always realised because, with rigid wages (and interest-elastic money demand), the IS-LM model will still produce unemployment and a rationale for fiscal policy, etc., even if otherwise all its pieces are Classical. What it takes, however, is a wage rate that is exogenously fixed, which is to say a model without operative supply-and-demand mechanisms in its labor markets.⁸⁷ Keynes did not have wages "rigid" in this sense. Rather, his model had the money wage rate historically given in the short run, but market determined over the longer run. These are the same labor-market assumptions as in Friedman's model in which temporary wage-inflexibility allows for temporary deviations from the natural rate of unemployment but longer run market forces make unemployment converge on the natural rate.

In the 1940's, a macrotheory with wage rates exogenously determined (by non-economic factors) and unresponsive to market conditions was acceptable to many economists and perhaps to most labor economists. As the Human Capital approach later encroached more and more on an area where institutional explanations had been the rule and made progress with explaining relative earnings as market determined, the denial of an operative labor market mechanism eventually became an untenable assumption. By the early sixties, in addition, the stable Phillips curve had become, somehow, a characteristically "Keynesian" doctrine. The most plausible seeming rationale offered for its shape was that the rate of change of money wages was a function of the excess demand for labor, i.e., that the system does have an operative labor market mechanism.⁸⁸ The resolution to this was to admit a labor market mechanism, but one that works rather slowly.

Now, someone whose macrobeliefs consist of neoclassical growth, variable velocity monetarism, and unemployment caused by lags in wage-adjustment should not fight Milton Friedman, but join him. But that conclusion, of course, the Neoclassical Keynesians have refused to draw. One may appreciate their steadfast resolve to rather fight than switch. But it is not at all clear that their version of Keynesianism gives them a leg to stand on.

This branch of Keynesianism managed, somehow, completely to evade the issue of the interest mechanism. Does it or does it not work to coordinate the intertemporal decisions of households and firms? The theory does not say.

There are two clearcut answers: no and yes. The choice is between Mrs. Robinson and Friedman. Both occupy coherently formulated theoretical positions so that the implications of the choice is clear. Side with Mrs. Robinson and you give up predictable velocity and neoclassical growth-models (and the neo-Walrasian theory they belong to). Side with Friedman and you accept Monetarism and the vertical Phillips curve.⁸⁹ The Neoclassical Keynesians would like to avoid both Scylla and Charybdis. But since they have failed to see the interest rate issues as central and have not understood that Keynesian theory and Keynes' contention are rooted in Wicksell's theme, they have not managed to chart a safe course between them.

The remaining, unexplored possibility of steering clear of both Cambridge and Chicago is that the interest rate mechanism (contra Mrs. Robinson) is capable of coordinating saving and investment but (contra Friedman) cannot be depended upon to do so always. This means a Loanable

Funds theory with either speculation and/or the Central Bank capable of preventing or holding up adjustments of the real interest rate that is required for the equilibration of the system. Add temporary wage-inflexibility with consequent short-run quantity-adjustments and we are back with Z-theory.

Unless the real rate of interest goes to its natural level, unemployment will not home in on its natural level. This is true also of a system with an operative labor market mechanism. If and when the system fails to coordinate intertemporal activities by adjustment of intertemporal prices, the "flexing" of money wage rates in response to excess supply of labor will not guarantee a return to full employment. This is ground on which the Keynesians could base their opposition to the Monetarist doctrines of strong stability of the system in its real variables, the "vertical" Phillips curve at the natural rate of unemployment and to the associated strictures against discretionary policy actions.⁹⁰

It should be realised, however, that Z-theory will not salvage all the positions that Keynesians have assumed in their attempts to halt the spreading influence of Monetarism. In particular, it will not serve as a basis for resurrecting the accursed notion of a stable, downward-sloping (even if expectations augmented) Phillips curve. It suggests, rather, why we should not run regression lines through a Phillips scatter diagram. Such a diagram should be read as composed of data points drawn from different samples. If we were to label the points according to the diagnosis of the historical state to which they correspond, we might have the following:

1) FF-points (Fisher-Friedman) for states of the economy when there was no saving-investment problem. These should either be close to the

natural rate of unemployment or else be moving towards it relatively rapidly. "Automatic" convergence on full employment will not be prevented by saving-investment "gaps". Monetarist theory and policy prescriptions apply to this sample.

ii) WK-points (Wicksell-Keynes) for states of the labor market that reflect maladjustments of interest rates and consequent saving-investment problems. These points should be off the natural unemployment rate and found to be moving towards it only sluggishly, if at all.⁹¹ This sample of historical states would qualify for aggregate demand management.

iii) A few K-points (Keynes) from the early 1930's reflecting general Effective Demand Failures (outside the corridor).

iv) Perhaps some modern WF-points (Wicksell-Fisher) for certain stagflation situations.

Z-theory should recommend itself as a theoretical home base for Keynesians also in that, with a little help from Irving Fisher, it suggests a straightforward hypothesis to account for stagflation. Such business fluctuations as do take place in a policy regime providing monetary stability will not be eliminated by expected inflation (nor will they be ameliorated by erratic stop-go inflation). To the extent that changes in entrepreneurial expectations about the prospective real return to new investment produce fluctuations in activity levels, these can just as well take place around an average inflation rate of 10 or 20 percent. The simultaneous occurrence of inflation and unemployment (or even rising inflation and growing unemployment) will not appear as a riddle once one is freed from a theory that can explain unemployment only by invoking downward rigidity of wages.

Whether Z-theory as a last ditch against Monetarism will hold much water, one should not prejudge. One thing is quite clear: if it does not, all of Keynesian economics will have to be judged a big mistake.

X. Friedman

Over the course of the Monetarist debate, Milton Friedman has consistently maintained that the differences between his position and that of the Keynesians are empirical and not theoretical. From the other side, Franco Modigliani, for one, concurs.⁹² But the theoretical framework they both use has failed to point out the maladjustment of the interest rate as an idea that is central to all the various non-monetarist theories. There is an important theoretical issue separating Keynesian and Monetarist theory.

Monetarism evolved in opposition to Keynesianism. Consequently, the aspects emphasized as distinctive to the monetarist position have been determined by what has commonly been understood to be the Keynesian one. The Neoclassical Keynesians abandoned or lost track of the Wicksellian theme. So Friedman has not been pressed to argue his own views on interest theory. Nor has he seen a need to do so for his understanding of Keynes and Keynesian theory is largely the conventional one.

In this section, we will first attempt to piece together a picture of Friedman's theory of the real interest rate. Second, we will comment on his interpretation of Keynes.

Sir John Hicks has distinguished between a Thornton tradition and a Ricardo tradition in monetary theory.⁹³ Thornton adumbrated Wicksell and Keynes. Friedman is placed in line of descent from Ricardo. In marking his departures from the Classics, Keynes quoted Ricardo:

The interest of money is not regulated by the rate at which the Bank will lend ... but by the rate of profit which can be made by the employment of capital, and which is totally independent of the quantity or of the value of money.... The applications to the Bank for money, then, depend on the comparison between the rate of profits that may be made by the employment of it,

and the rate at which they are willing to lend it. If they charge less than the market rate of interest, there⁹⁴ will be no amount of money which they might not lend;...

Keynes criticized this Ricardo passage for its Classical ^{1/4}full employment presumption -- Ricardo had derived the proposition that the rate of profit uniquely determines the interest rate by assuming the system to be at full employment. My impression of Friedman's thought over the years is that he starts by assuming Ricardo's conclusion and from it deduces Ricardo's assumption, i.e., he assumes that the market real rate will equal the natural real rate and concludes that an operative labor market mechanism will make employment converge on its natural rate.

Friedman's favorite fable of capital and growth, apparently, is that of Frank Knight.⁹⁵ In a Knightian world, there is no difficulty knowing what the real rate of interest is and must be: you just check the Crusonia-plant to see how fast it tends to grow. In a von Neumann world of multiple commodities, it takes some calculation. But in neither system can there be any mistaking what the real rate is. It is independent of employment and of saving-propensities. It makes no sense to assume that anyone would speculate against it being what it is. The real rate is ascertainable without engaging in monetary calculations. The banking system cannot by its policies depress the market real rate below its natural level for any appreciable length of time. The problem that entrepreneurs have is forecasting changes in the money price level. In the Fisher-equation, the real rate is known; uncertainty and information problems attach to the prospective rate of inflation. In interpreting Tooke's stylized fact (i.e., Gibson's Paradox) one need not worry about the possibility that the time-series of nominal interest rates might reflect, in part, significant discrepancies

between the market real and the natural real rate. Instead, movements of nominal interest rates can be taken to reflect changes in the expected rate of change of the price level against the background of a steady real rate. The real sector of the economy is strongly stable and can only be disrupted by serious mismanagement of money.

This all fits. Knight's Crusonia-fable need not be taken literally, however. Instead, we should imagine a world for which it would make a serviceable metaphor: An economy with stable saving propensities and technological progress proceeding at a more or less steady pace; one in which entrepreneurs collectively are not given to "waves" of pessimism and optimism about future demand, but where competition tends with time to weed out both the overly pessimistic and the optimistic, and so on.

Friedman like so many others filters Keynes and Keynesian theory through the IS-LM model and, consequently, ends up where everyone else ends up: bogged down in the Neoclassical Synthesis, which is to say, with the conclusion that exogenous fixity of money wages was Keynes' explanation of unemployment. His discussion is notable for a sophisticated treatment of Keynes' demand for money function and for its sweeping endorsement of the Pigou-effect.

Friedman correctly specifies Keynes' money demand function as $M/P = ky + f(r-r^*, r^*)$, where y is real income, r the market rate and r^* the "rate of interest expected to prevail." The latter we may refer to as the "normal rate."⁹⁶ Friedman then distinguishes between long-run and short-run "absolute liquidity preference." In the long-run, we should have $r = r^*$, so that only the absolute level of the interest rate enters into the steady state money demand function. If, then, this long-run function becomes

infinitely elastic at some very low rate of interest, we have "long-run absolute liquidity preference." This is the old Liquidity Trap idea. Friedman's "short-run liquidity preference" is Keynes' speculative demand, the interest-elasticity of which Friedman exaggerates by calling it "absolute."⁹⁷ He proceeds to insist on the "key role" of absolute liquidity preference in Keynes' theory:

Time and again when Keynes must face up to precisely what it is that prevents a full-employment equilibrium, his final line of defense is absolute liquidity preference.... I do not see how anyone could read through these quotations and come to any other conclusion than that his 'special twist' was highly elastic liquidity preference...⁹⁸

One notes that Friedman makes no use of the distinction previously drawn between long-run and short-run absolute liquidity preference in this passage. The distinction is in fact critical, although it may be more appropriately put as one between full information and incomplete information analysis.

1) The Liquidity Trap Case. The problem here is the one that Pigou termed "Keynes' Day of Judgment." It is a problem in long run, full information comparative statics. A state of virtual capital saturation is postulated, so that no opportunities for net investment at a significantly positive rate of return exist; yet, we assume, the community wishes to accumulate more wealth. The "absoluteness" of liquidity preference is an essential part of the predicament: the interest rate cannot be depressed further so as to stimulate investment and close the saving-investment gap.⁹⁹

The original Pigou-effect analysis is the right answer to this problem. The rational solution is for the community to accumulate non-productive wealth and this can be done by allowing the real value of cash balances to grow through a declining price level. If, instead, unemployment results, rigidity of wages and/or prices should be blamed.

2) The Case of Misinformed Speculation. The speculative demand for money is not a problem in either the long run or the short run if full information can be assumed. In a full information state, speculators will regard the natural rate as normal, so that we have $\hat{r} = r^*$. In the Keynes-case, however, speculators have failed to recognize the full extent of a shift in the natural rate and are assumed to take positions against the required adjustment of market rate. We then have, as in Figure 3 (Section VI above), $r^* > \underline{r} > \hat{r}$.

That speculative liquidity preference be "absolute" is not really to the point here. It would merely mean that $r^* = \underline{r}$. What counts, of course, is the Wicksellian theme: $\underline{r} > \hat{r}$.

In such a situation, savers, speculators, and entrepreneurs are acting on inconsistent beliefs about future prospects. This, moreover, is reflected in "incorrect" relative prices. When market rate, \underline{r} , differs from the natural rate, \hat{r} , intertemporal prices are wrong; in general, we expect that, as a consequence, the ratio of durable goods demand prices to consumer goods prices will also be wrong.

The Pigou-effect is not a solution to this problem. A change in the absolute level of prices is not the rational response to disequilibrium relative prices. A large deflation would only add the problem of a large error in the money wage level to an already confused system state.

In stressing "absolute liquidity preference" as a key feature of Keynes' theory, Friedman, obviously, is within an inch of coming up with the Wicksellian theme as the difference between himself and his opponents. Yet he does not follow up with a discussion of interest rate ~~maladjustments~~, saving-investment gaps, etc. As previously noted, he is content even to

leave the saving-investment sector "as unfinished business" in the model he is using to discuss the relationship between Monetarist and Keynesian theories. There appear to be two reasons for this. One is that he does not take seriously the possibility of a significant divergence between the normal and the natural rate. It is not clear whether, in neglecting it, he lays more stress on the assumption that the natural rate is unlikely to change or on the assumption that it will be generally known when it does change. That he does regard $r^* \neq \hat{r}$ as implausible is most evident from the postulate that he incorporates in his own theory:

A nominal market interest rate equal to the anticipated real rate plus the anticipated rate of change of prices, kept at that level by speculators with firmly held anticipations.¹⁰⁰

This, indeed, is absolute liquidity preference (and, consequently, a stronger assumption than Keynes employed). These speculators will keep $\underline{r} = r^*$. This, then, is a model of a world that would come apart into the worst possible Wicksell-Keynes shambles if, unbeknownst to these speculators, the natural real rate of interest declined.

The other reason, I think, is that Friedman feels that the Pigou-effect conclusively demonstrated that "there is no fundamental 'flaw in the price system' that makes unemployment the natural outcome of a fully operative market mechanism." Keynes' contention, therefore, was a definite "error (which) consisted in neglecting the role of wealth in the consumption function -- or, stated differently, in neglecting the existence of a desired stock of wealth as a goal motivating savings."¹⁰¹ But Friedman's own analysis of the Pigou-effect deals only with the Trap Case.¹⁰² It does not, therefore, dispose of the type of saving-investment problem considered in this essay.

Indeed, as applied to a Wicksell-Keynes (Z-theory) saving-investment gap the usual Pigou-effect analysis is in error -- and it is in error precisely because it treats real balances as if they were wealth.

For simplicity, let us presuppose that the system's equilibrium path is unique. (This surely is in a "Classical" spirit in any case). Given a money stock of M , let the unique equilibrium values for the current period be \hat{r} , \hat{p} , \hat{w} , \hat{I} , ... etc. In Keynes' early recession unemployment state (Figure 3), saving out of a depressed level of real income, $S(\underline{X})$, equals investment, \underline{I} , at a market rate that exceeds the natural rate, $r > \hat{r}$. In this state $w = \hat{w}$. To get back onto the equilibrium path, the system must move market rate back into conformity with the natural rate. The Pigou-effect, of course, does not do this.¹⁰³ The argument asserts, instead, that a horrendous, all-around deflation, "balanced" so as not to change relative prices, will increase real balances to such an extent that consumption increases, with the saving-income ratio declining, until saving out of full employment income just equals investment -- at an unchanged level of market rate still in excess of natural rate. At the point (r, \underline{I}) the economy is allocating more of its full employment output to consumption and less to capital accumulation than is, by hypothesis, consistent with being on its equilibrium growth path. Savers, one may suggest, are fooling themselves. They are supposed to regard the increased real value of their cash-balances as added "wealth." But the system has not added to its productive capacity. Unless satisfied to retire in their old age on a diet of "liquidity services", households will discover that this Pigou-addition to wealth cannot be cashed in for added future consumption.

It should matter, one would think, whether we have full employment at, say, a 4% rate of interest with the money wage index at 100 or full employment at 5% interest with the wage index at .01 (or whatever). The alternative is to maintain that the full employment state brought about via the Pigou effect is also an equilibrium. In that case, the system has an infinity of "equilibria". For, with a given nominal money stock, to any value of the interest rate there will correspond a possible price level that makes the real value of the money stock exactly what is required to adjust full employment saving to the investment forthcoming at this interest rate.

So, here is our choice: Either the full employment state brought about by the Pigou-effect is not an equilibrium but instead a state where, even though employment is temporarily full, the system is more disorganized than ever. Or the system has an infinity of equilibria -- any growth-rate can be an equilibrium path.

The standard treatment of the matter by Patinkin and others fails to reveal the issue. Yet, there is no error in the way these authors handle their models. It may be, then, that this tells us that something may be wrong with these models. If so, what needs revision? I will end by putting it as a question: Does the problem, perhaps, go all the way back to Hicks' "Suggestion for Simplifying Monetary Theory?"¹⁰⁴ It is a bit frightening to think that it might, for that could imperil the whole vast tradition of monetary theory that has been built up from the Hicksian Suggestion. Yet, it is from that early paper, that the later literature stems which treats real balances as household "wealth" and, in postulating saving as wealth-adjustment behavior, fails to distinguish between household "wealth" to which there does or does not correspond, on the production side of the economy, the physical capacity to produce future consumer goods.

XI. A Restatement

I turn at last to the group known euphoniously as the Leijonhufvudians. Since the views of this querulous faction have insinuated themselves all over this essay, they had better be made explicit. This group is all chief and no injuns which makes it easier.

In the present era of interminable monetary mismanagement, one almost feels like apologizing for a continued interest in the problems of Keynesian economics. All the Wicksell Connection theorists had one presumption in common, namely, the notion that the "Dark Forces of Time and Ignorance", as Keynes put it, will make intertemporal economic activities the most difficult to coordinate and that the saving-investment problem, therefore, was the likely place to start in looking for the key to macroeconomic instability. Over the last fifteen years or so, however, the Forces cloaked by Darkness are not the future wishes of customers or innovations of competitors, but primarily the so-called monetary policies that emerge from the confused and unprincipled interaction of the U.S. President, Congress, and Federal Reserve System. I believe it probable enough, however, that we would experience some investment fluctuations even in the unlikely event of a return to monetary stability, so that it remains worthwhile to try to understand the Wicksell Connection theories correctly. More to the point (for those who would insist on contemporary relevance) it is not likely that monetary instability has made the coordination of intertemporal activities any easier.¹⁰⁵ That said, this final section too will be confined to Wicksell Connection problems.

Like the Cambridge Keynesians, and in opposition to the Neoclassical Keynesians and Milton Friedman (in an alliance that ought not to be consecrated),

I reject the Neoclassical Synthesis lock, stock, and barrel. I have spelled out my reasons more than once. Pace Friedman, they are still good.

Unlike the Cambridge Keynesians, I do not accept the Liquidity Preference theory of interest of Keynes' General Theory or any of the "lemmas" that flow from it. I believe it to be theoretically unsound, empirically false, and practically dangerous. My views on the interest mechanism consist of a basic D.H. Robertson Loanable Fund theory,¹⁰⁶ complicated, when needed, by Keynesian bear/bull speculation (as in the Treatise) and/or Fisherian inflation rate speculation. In the context of the preceding discussion, this means that Z-theory is as far as I will keep company with Keynes in the development of his thought. I accept only half of the Keynesian Revolution.

What are the purely theoretical consequences of taking this position?

Keynes believed that the contemporary problem that the economy was failing to solve was that of "traversing" from a higher to a lower growth path. His revolutionary theory develops in the analysis and re-analysis of this case. One should not fall into the mental habit of regarding this as the "typical" problem in macrotheory -- I do not think it is -- but it will be convenient to continue to focus on it here.

The immediate consequence of retaining a basic Loanable funds position is clear from Section VI: the rate of interest will go to the new "natural" level, and thus equate full employment saving and investment, unless bearish speculators (with or without the connivance of the Central Bank) intervene to prevent it.¹⁰⁷ Consequently, it is possible for the interest rate mechanism to coordinate saving and investment decisions. The least we can conclude from this is that the equilibrium path of a neoclassical growth-model can legitimately be used as a notional reference path, i.e.,

that it makes sense to compare disequilibrium states with the notional equilibrium path in trying to diagnose what has gone "wrong."¹⁰⁸ But the point goes farther: if the system will find its way to the "natural" rate of interest, unless there is strongly preponderant speculation against it, then it is no longer true that speculators are just playing Musical Chairs and forecasting Beauty Contest winners. There may be a lot of that going on, but there is now a Natural Beauty in the contest; the individual speculator has an interest in learning to recognize her -- and should not bet against her unless he is pretty sure the game is rigged.¹⁰⁹ Speculation against the level of interest consistent with Productivity and Thrift should not be the normal case.

Hence we regain a concept of saving as something more than an anti-social refusal to spend. It matters that it is also a supply of loanable funds. Higher saving propensities should normally entail more rapid growth of the Wealth of Nations, not higher unemployment. It makes sense for governments bent on growth to encourage saving. And so on. We can make a clean break with one of the most dangerous and harmful confusions ever taught as accepted economic doctrine.

Next, note carefully, that the implications of the "normal case" are Monetarist. When the securities markets keep track of the natural real rate of interest we are in a monetarist world. Velocity should not be "unstable", monetary policy does not "push on strings", stable money stock growth is to be favored -- and unemployment converges on its natural rate. I cannot see any reasons whatsoever why "Keynesians" should resist those conclusions. The conditions assumed are precisely those under which Keynes the Revolutionary fades out and the Author of the Tract and Architect of Bretton Woods come back in.

If the theory here proposed might be challenged by those who remember the Great Depression for implying that convergence onto an equilibrium growth-path is possible, the question nowadays -- in the first flush of Rational Expectations -- is apt to be different: Does the analysis leave any room for wrong-headed speculation? Is it likely that the Keynesian case will ever occur? My own predilection is to turn the question around: Suppose it does happen that the economy shows serious problems with coordinating activities. What are the methodological alternatives to explanations that stress inconsistent beliefs not reconciled in time by market processes.¹¹⁰

How plausible this Keynesian "disagreement" between entrepreneurs and speculators strikes one as being depends on what fables of capital and growth one deems most instructive. Like the Austrians, and more lately the Cambridge Keynesians, but unlike Knightians, I would emphasize the heterogeneity of capital goods and the subjectivity of entrepreneurial demand expectations. There can be no "uniform rate of profit" inferable from production sector relations alone that could be calculated ex ante by speculators as a guide to the equilibrium rate of real interest. The notion of entrepreneurs collectively "disagreeing" with the securities market about prospects I have put in oversimplified (and perhaps somewhat misleading) terms. Entrepreneurial pessimism or optimism cannot be conceptualized as fluctuations in some agreed-upon "number", such as "the" internal rate of return. Rather, whatever coherence in investment activities is achieved comes about through each entrepreneur taking the interest rate established in the market as the appropriate opportunity cost of financial capital and adjusting his rate of investment so as to obtain a subjective efficiency rate at the margin equal to the market rate of interest. A world of this sort does, I think, give enough play to the "Dark Forces of Time and Ignorance" that one cannot easily dismiss the Keynes' case.

The story can be told in terms that modern readers may find more congenial. Whether more congenial or not, comparison of the two variants is instructive. I have throughout used the language of the older business cycle literature in which the separate economic functions or activities are attached to separate actors in an anthropomorphological style of discourse that goes back, I suppose, at least to British Classical distribution theory. Older business cycle theories were written like *Commedia dell'Arte* plays; the plot could be varied but the stock characters had to be the same: entrepreneurs, bankers, speculators, consumers and laborers. I like this language for obvious reasons; I also prefer it for a heuristic reason -- in analyzing a state of the system, in which activities are not smoothly coordinated, this language permits one to dramatize the inconsistencies of such a situation either as conflicting beliefs or else as conflicts between the stock characters.

But naturally, the theory can be told in a different language. So, let each entrepreneur be a speculator and all speculation be done by entrepreneurs. Then assume some event that lowers the MEC "in general". Each entrepreneur perceives that profit prospects for his firm have deteriorated so that the marginal efficiency of investment schedule has declined for the kind of machine that he invests in. He knows that he is not alone in thinking "business is bad" but, given the heterogeneity of capital in the economy, he does not know how much of the decline that he perceives is truly general. Taking \underline{r} (Figure 3) as the plausible opportunity cost of financial capital, he cuts back his rate of investment in new machinery to the point where MEI equals this rate. With others acting in the same way, retained earnings pile up in firms while \underline{r} becomes the market rate of interest. This, the reader will recognize, is the nowadays most fashionable

search theory of the unemployment of financial capital. The general decline in the rate of profit that the system can earn at full employment is $r_0 - \hat{r}$. What the theory says (roughly) is that it is only the unanticipated or, better, unrecognized part, $r - \hat{r}$, of this real disturbance that has an effect on aggregate real income and employment. That is as it should be.

But how could it last? And why would not falling wages and prices help? General deflation will not help unless or until the Keynes' effect eliminates the discrepancy $r - \hat{r}$. That is standard. But why should not r simply come down to \hat{r} as entrepreneur-speculators learn that no better placements are opening up? It will; and this should put the system back into equilibrium unless the disappointment of short-term sales expectations induces a further decline in the subjective MEC schedule. As induced ("accelerator") decline in MEC will produce a state where the interest rate is now right but the MEC schedule too low for this to restore full employment. This transition from one type of disequilibrium to another puts the system into a state where discretionary fiscal spending is the preferred policy.¹¹¹

Does advocacy of discretionary fiscal policy in this setting rest on the implicit assumption that the government "knows best" -- and, in particular, knows better than the entrepreneurs? No. Each entrepreneur, if he is "rational", knows that were he alone to expand output he would suffer losses while most of the gains due to the increased employment at his plant would redound to others. He also understands that if all firms were to expand at the same time they could all be better off.¹¹² The problem is how to organize this collective action. The government is there, already organized to do it.

So much for a Revised Standard Version of the General Theory (with the Liquidity Preference theory omitted as apocryphal).

We have in effect, two versions (or two stages) of this Keynesian case where the coordination problem is that of effecting a traverse. In the first using the old-fashioned language, the entrepreneurs "are right and the speculators wrong; in the next, the speculators are right and the entrepreneurs collectively wrong in acting on pessimistic sales forecasts. But the challenge of changing the growth-path should not come up all that often. Are all other income fluctuations to be ascribed to "monetary" causes?

There is one more possibility that is, I think, of interest. In this case, we will have the speculators being right all along, since nothing will have happened to require the system to settle for a lower growth path; the entrepreneurs, however, are not "disagreeing" in this instance but are, rather, hesitant and uncertain. Consider a "pause" along the equilibrium growth path. By a pause, I mean a situation where the representative firm chooses to postpone investment. This may occur for many reasons. Temporary uncertainty induced by political events is an example. But even in the closest approximation imaginable to a process of constant exponential and proportional growth of all activities, there should be more than enough untidy Schumpeterian economic development to make firms "pause" for reassurance now and then before they resume putting more eggs into the same old baskets. A recession of this type is due to a temporary increase in "flexibility preference" in the Hart-Hicks sense.¹¹³ The postponement of new "fixed" commitments until the situation clarifies has its counterpart in a transitory increase in demand for money and for "liquidity" in general. Long rates of interest stay up, but short rates and observed velocity decline.¹¹⁴ I would expect this story to fit minor recessions rather better than the Keynes' story. It seems moreover, to bear a reasonable family resemblance to Lucas' way of telling the story of a minor recession.¹¹⁵

Finally, an economy may get into discoordinated states much "worse" and much more difficult of correction than the ones discussed above. This essay has concentrated altogether on the economy's behavior "inside the corridor" (as I have termed it elsewhere). For a discussion of "outside of corridor" pathology, I must refer the reader to an earlier companion-piece of this paper.¹¹⁶

Footnotes

1/ In this essay, "Monetarism" will refer to the pre-Rational Expectations position of Milton Friedman. Cf., e.g., Friedman (1968) and Gordon (1973). "Cambridge Keynesians" will refer to Joan Robinson and Richard Kahn. For reasons to be discussed later I do not have a name or names to typify Neoclassical Keynesianism. The term should be taken to refer, somewhat loosely, to those Keynesians who, in contrast to the Cambridge Keynesians, accepted the so-called Neoclassical Synthesis.

2/ Or, from one perspective, I suppose, as a bunch of suckers that should be pruned back to allow the mainstem to bloom more abundantly.

3/ None of the authors in Gordon (1973) or in Mayer (1978) have a clear statement of it, for example.

4/ Telling this tale allows me to correct and clarify earlier work. My On Keynesian Economics and the Economics of Keynes (1968) stressed two clusters of ideas. One was the "multiplier" process, the analysis of which starts from assuming quantity-adjustments, rather than pure price adjustments, as the immediate response to deflationary pressure. The other cluster of ideas belong to Wicksell's theme.

The "multiplier" part of the book has received much more attention than the rest. For this, admittedly, the writing rather than the reading is to blame. Nonetheless, neither set of ideas is of much "revolutionary" significance without the other. It is the combination of the two that produces a challenge to the "Classics" (or Neoclassics or Monetarists).

In "Effective Demand Failures", [Leijonhufvud (1973)] I gave my second thoughts on multiplier matters. This paper is a belated companion piece. They belong together.

5/ Z-theory is on the whole what I presented as the "Economics of Keynes" in 1968. In so doing, I was not taking the Liquidity Preference theory of interest quite seriously but opted to stick with what I still regard as the much superior interest theory of the Treatise. But the Liquidity Preference theory, whatever its theoretical deficiencies, has been historically important. Many of the weaknesses of "Keynesian economics" really stem from it. I failed to give it its historical due.

6/ Although some of the Keynesian models dealt with here have Velocity "unstable", all treat Money as a definite, meaningful aggregate, just as do Quantity Theories. Keynesian models crossbred with the Hawtrey-Radcliffe strain (or the Gurley-Shaw strain) have been put arbitrarily beyond our purview. The consequences of so doing are a bit uncomfortable to the author here and there, but the issues involved would only encumber the paper to little purpose.

7/ Cf., F.H. Hahn (1973).

8/ The only "real" shocks considered here are those that shift the investment or saving functions.

While they are not needed for the purposes of the paper, a few other properties will help suggest the flavor of full information constructions: (4) government resource absorption will crowd out private sector resource use; (5) future tax liabilities are fully discounted in calculations of private net worth; and (6) the Fisher equation for the relation between real and nominal interest rates holds.

When the term "real interest rate" is used in this paper, it means simply the Fisher-deflated money rate of interest, i.e., the money rate minus the expected rate of inflation. In particular, use of the term does not mean that we deny or "assume away" the multiplicity of commodity own rates. For the most part, we will deal with problems where the distinction between real and nominal rates is not required.

9/ W.J. Baumol (1952), J. Tobin (1956), D. Patinkin (1956). Note however that Clower and Howitt do not find an unambiguous sign for the interest-elasticity of transactions demand in their more general inventory theoretic model. R.W. Clower and P. Howitt (1978).

10/ E.g., H. Latané, (1954). For a survey, cf., D.E.W. Laidler (1969).

11/ The option of not paying promptly is not incorporated in the decision-calculus of either Baumol's or Tobin's transactor. Putting it in, I conjecture, will not do the trick as long as the brokerage fee is not also proportional to the interest rate. One would have to rely on Clower and Howitt instead.

12/ On flexibility preference, cf. A.G. Hart (1942). Sir John Hicks (1974) Lecture II, explains the distinction between flexibility and liquidity in the sense of Tobin-Markowitz portfolio theory and develops flexibility preference theory in exactly the context most germane to the concerns of this paper.

13/ Cf. Leijonhufvud (1968), p. 395.

14/ Cf. J.R. Hicks, (1965), Chapter VI, and J.M. Grandmont (1975).

15/ Use of "inconsistent beliefs" rather than the more fashionable "asymmetric information sets" is deliberate. The intention is to suggest transactors acting at cross-purposes. "Asymmetric information" is coming to be associated (I think) mostly with equilibria where the information advantage on one side of the market persists.

16/ Most textbooks keep the student in the dark about these (presumably pedagogical) goings-on.

17/ The exception would be those Keynesians who at one time maintained that monetary policy was ineffective because of liquidity traps and/or interest inelasticity of saving and investment.

18/ F. A. Hayek (1931) and (1933), and G. P. O'Driscoll (1977).

19/ M. Friedman (1968) and in R. J. Gordon, ed., (1973), pp. 35ff. and p. 54.

20/ The process just described we will call the Loanable Funds Sequence. For the Liquidity Preference Sequence, cf. Section VIII below.

21/ It may be pedagogically preferable to refer to the ED for loanable funds as an ES for "bonds" instead since this makes it clear that Say's Principle is adhered to at each step.

22/ Cf., for instance, Tobin in Gordon, ed., (1973), p. 77, or Bronfenbrenner in Mayer (1978), p. 49, n. 5, or B. M. Friedman, ibid., pp. 94-95.

Milton Friedman has emphatically denied that the elasticity of LM is at issue. Cf., Gordon, op. cit., p. 142. At the same time his use of what is basically an IS-LM structure in presenting his own theory, and his oft-repeated insistence that no theoretical issues but only questions of empirical magnitudes within this shared theoretical frame separate him from his opponents, have apparently fortified others in their belief that (whatever he says) this elasticity must be crucial. Furthermore, Friedman has himself played around with elasticities, for example in advancing the notion of a horizontal IS curve. Cf., his Comments in J. L. Stein (1976), p. 311.

23/ Cf., e.g., R. E. Lucas, Jr., (1976).

24/ Cf., Leijonhufvud (1968b) and (1973).

25/ Cf. Leijonhufvud (1976), pp. 70-71.

26/ Cf., below, Section VI.

27/ Without this strong assumption, moreover, IS-LM does not make sense as a modelling strategy. Analyzing income determination in terms of these two particular reduced forms is illuminating and convenient only insofar as this assumption is approximately justified. It is not just that IS-LM happens to direct attention away from direct interactions of money and commodity excess demands. It is meant to do so, being predicated on the judgment that the direct link is seldom of appreciable significance.

The early Keynesian literature managed with some frequency almost to make a riddle of how money could affect aggregate demand ... in a system where the circular flow of income, after all, shows money being spent for goods and factor-services (and not just for bonds).

28/ In the classroom, the story should be accompanied by visual aids. There are two ways of doing it. In one, we put real income on the horizontal axis, show LM shifting out at stage (i) and then have it shifting back at stage (v). In the other -- used below -- nominal income is on the axis, LM shifts out at (i) and IS follows suit in stages (iv) and (v). If teacher passes too quickly from (i) to (v), however, the diagrammatics will tend to look strangely superfluous. To repay the student for his investment in learning to construct the two reduced form schedules, etc., one might pause in the middle to single out a "short run solution" for special attention -- preferably at early stage (iv) where LM has shifted and IS is "only just about" to move. The elasticities of IS and LM might figure prominently in analyzing this intermediate position whereas, once we reach (v), they no longer have anything to do with the result.

29/ ...and that it is these ignorance assumptions and not the steady-state interest-elasticities of money demand or investment, etc., that count in explaining the positions of the system at stages intermediate between FIM states.

30/ Back in the days when the effectiveness of monetary policy was still in debate, Monetarists used to explain that it was more powerful in Monetarist theory than in Keynesian theory because "a change in the rate of interest" had a much broader interpretation in the former; correspondingly the transmission mechanism was conceived of as much more robust and effective than in Keynesian theory, etc. The latest generation of Monetarists have little use for the interest rate.

31/ Perhaps this accounts for the model's staying-power? Nothing is as successful in economics as a construct all shot through with remediable shortcomings.

32/ The conclusion was that Keynes produced a revolution in economic theory by postulating wages too high for full employment and rigid downwards as the explanation of unemployment. That Keynes' contribution to economic theory can be thus summarized is disputed in Leijonhufvud (1968) and (1969).

33/ Cf., e.g., Wicksell (1935), Vol. II, pp. 141 ff.

34/ Wicksell (1935), Vol. II, pp. 159 ff.

35/ It is instructive to note that, although more primitive, Model A is not earlier but later than Wicksell. The conception outlined in the text is the original one. The idea, pioneered by Wicksell, that the saving-investment approach is a useful way to analyze movements in nominal income stems from it. The Keynesian cross is a degenerate version resulting from letting the Banking system fade out of Wicksell's model.

36/ Milton Friedman and Anna Schwartz (1963) have a very clear statement distinguishing sharply between their own "monetary" (i.e., money stock) theory and those older theories of business cycles that used to be called "monetary" but would more accurately, they note, be called "credit theories." The distinction is sharp for a purpose -- Friedman and Schwartz disassociate their own theory from the entire class of "credit" theories. This class would include Wicksellian theories. Cf. also Friedman in Gordon, ed., op. cit., p.p. 146-148.

37/ Not to be confused with Fisher's nominal and real rates of interest which are names for two distinct variables, related through his well-known equation.

Wicksell's analysis does not deal with expected rates of inflation. It can be generalized to incorporate the Fisher idea. But this paper will dodge the issues connected with expected inflations.

38/ Warning! This is anachronistically put in terms of the much later literature on neoclassical growth. Draining the Bohm-Bawerkian capital theory from Wicksell will no doubt seem offensively impious to some, but I do not want to burden this paper also with those complexities.

39/ Note that Proposition (iv) is hardly self-evident either. From it and (v) we can obtain familiar-sounding phrases: "the interest rate will equate saving and investment in equilibrium", etc. But in a monetary model with futures markets only for money, the interest rate clears the bond market, the price for newly produced capital goods clears that market, and "saving" defined as "non-consumption" is inclusive of "hoarding". And in the context of a Fisherine non-monetary 2-period model, where the interest rate (or rather the discount factor) does clear the intertemporal consumption goods market, the concepts of "saving" and "investment" are pretty artificial. Cf. Hirshleifer (1970), pp. 36-37.

40/ Note that in neoclassical models of balanced inflationary growth we have (with the help of the Fisher-equation) "real" equilibrium without "monetary equilibrium" in Wicksell's sense. In these models, the tight link has been loosened.

41/ This already simplifies Wicksell by more simultaneity and less sequencing than the original tale. This simplified version will suit our further purposes better however.

For purposes of doctrine history, a more accurate rendition should go approximately as follows: (1) Producers borrow enough money to finance $I'(r_0)$ at last period's prices -- which they expect to continue. (2) The banks now close (hence the system will not "explode" today). (3) The labor market opens and producers use the proceeds of last period's sale of final goods plus the newly borrowed money to bid for labor. Money wages rise, falsifying the expectations mentioned in (1). (4) Production takes place and, finally, households bid for consumer goods, raising their prices.

The advantage of such a sequential tale is that it brings out very clearly that, precisely because all mutual adjustments are not assumed to have taken place simultaneously, some agents must find their plans defeated by events in the course of the dynamic process. The disadvantage, as the later literature in this vein abundantly demonstrates, is that too many such tales can be told, all equally reasonable or unreasonable, with slight variations in the sequencing.

42/ Watching "M" -- if statistics were available -- would not be of much help in forming rational expectations. In a world like Wicksell's, the money stock would be a lagging indicator. The growth rate of M is not driving the cumulative process.

43/ Wicksell (1936), pp. 96-97. David Laidler drew my attention to this passage.

44/ And so did the Bretton Woods negotiations.

45/ Wicksell was an exception. Wicksell never applied his Cumulative Process analysis to short run business cycle problems and was rather scornful of Cassel who did. Cf. Cassel (1928).

46/ This is simplistically put, even too much so perhaps. But the point is obscured by a more circumspect statement and it is important that the gist of it be grasped. We will return to the matter in Section XI below.

47/ I.e., there would be no problem within the short-run period, in the sense that, with everyone acting on consistent beliefs, there should be a temporary equilibrium that would coordinate their activities. If they are wrong about "productivity" (or some other aspect of the State of Nature), however, this should be revealed -- at least to some people -- by the outcome of the coordinated actions of the short run.

If it is only revealed to some people and not to all, then in the second period we are back in the situation discussed in the text.

48/ Cf., the proceedings of the symposium on the writing of the General Theory edited by D. Patinkin and J.C. Leith (1977). The main paper by Patinkin sets "the systematic equilibrating role that changes in output -- and consequent changes in saving -- play in the General Theory"¹¹ as the theme for the conference.

I find two things amiss with the discussion of this theme by Patinkin and the other participants. First, no answer emerges to the questions: Why does the switch from price-level to output adjustment amount to anything more than the Treatise with "frictions"? Wherein does the "Revolution" inhere? Second, the part of Keynes' new theory that explains why the rate of interest is not the variable that equilibrates saving and investment hardly gets mentioned. But, in explaining what went on (and what went wrong) between the Treatise and the General Theory, it is almost as important as the part that asserts that output is the variable that does equilibrate saving and investment.

49/ It is perfectly reasonable for labor to hold out for the wages that would be earned in equilibrium. Their expectations are "rational" -- and how are they to know that entrepreneurs and speculators are about to act on inconsistent beliefs? What information would workers have on which they could base their short-run reservation wage? They know (i) what they earned yesterday (ii) that their physical productivity is unimpaired today, and possibly (iii) that the money stock has not decreased. It is true (iv) that their value productivity in terms of future goods has gone down -- since we postulate a decline in MEC -- but if the interest rate falls correspondingly (as it should) their discounted marginal value product should be (roughly) the same.

So, to repeat an old refrain: If this is the exercise, why complain about "less than perfect flexibility of wages". They should be sticky.

50/ One should be careful to note what Keynes' theory maintains about "wage flexibility": Perfect flexibility could prevent unemployment from developing to begin with -- but it will not remove it once you have it.

Wage "stickiness" in the first short-run "period" is obviously a critical assumption -- since otherwise we stay in the realm of Wicksellian full employment deflations (as in the Treatise). If unemployment develops initially, and if the unemployed are income-constrained, then it will not help to have reservation wages give way to adverse experience and start an accelerating decline....The reader will recognize where this leads: Keynes' pari passu argument, the Keynes-effect possibility and of course, that false, frail reed: the Pigou-effect.

51/ Cf., for example, Milton Friedman's "Theory of Nominal Income", reprinted in Gordon, ed., (1973). Friedman, however, is no more responsible for re-initiating the practice than any number of other writers. Note that none of his Keynesian critics in the Gordon volume -- or elsewhere? -- object to it.

52/ Flexible wages should restore full employment unless the wrong interest rate prevents equilibrium of investment with saving out of full employment income. Conversely falling money wages and money prices will not restore full employment if relative prices are wrong and the deflation is "balanced". For a detailed discussion, cf., Leijonhufvud (1968), Chapter V.

The statement in the text presumes a stage of the recession where entrepreneurial demand expectations underlying investment demand have not yet been undermined by the recession experience. Cf., e.g., Leijonhufvud (1968b), for a discussion of the deep depression case.

53/ Even if that, on the whole, was what I presented as the "Economics of Keynes" in my 1968 book.

54/ In general equilibrium statics, one is often reminded, "everything determines everything." Common usage dictates how "determine" is to be used. The price of peanuts is "determined" by the simultaneous solution of the entire set of excess demand equations. If the time derivative of the price of peanuts depends only on the excess demand for peanuts, this excess demand will be said to "govern" the price.

55/ Various modern models of financial asset prices do the same thing, however, that is impose zero excess stock demands as the conditions determining the structure of asset-prices and yields without worrying about flow rates of accumulation and new issues in the "short run." A similar stock-flow muddle pops up in numerous places in the literature on investment functions where it is argued that the demand-price for capital goods is independent of -- or even, cannot be dependent on -- the rate of accumulation. So we have not made much progress -- especially since Keynes was not prone to the last-mentioned error.

56/ The answer, or most of it, is in General Theory, Chs. 6, 7, 13 and 14.

57/ The 45°-degree identity Keynesian cross, of course -- not Model A. Model A, it will be recalled, admitted statements of the type "When $S > I$, there is an excess supply of commodities, and income will fall." We can't have that here!

58/ As far as I can understand, Robertson was consistently right on every aspect of the interest rate controversy between himself and Keynes and Keynes' Cambridge followers from "Mr. Keynes and the Rate of Interest" onward. Hicks has collected all the most relevant pieces in Robertson (1966).

59/ Nor does it make sense, in context, to argue as follows: "The most stable, and the least easily shifted, element in our contemporary economy has been hitherto, and may prove to be in future, the minimum rate of interest acceptable to the generality of wealth owners." (General Theory, p. 309). Either this empirical belief, or the bootstrap theory -- or both -- should be false. In a Treatise world, the formation of strong beliefs about the "normal" level of long rate is plausible. In the bootstrap world it is not plausible at all. Cf., Leijonhufvud (1968), p. 240n.

60/ That saving and investment determine the rate of interest "is what I myself was brought up on and what I taught for many years to others" (General Theory, p. 175). Upbringing aside, what Keynes should have taught is that loanable funds supply and demand govern the rate of interest and that it takes numerous restrictive assumptions to make saving and investment correspond to LF supply and demand, respectively. He could then have proceeded, with justice, to argue that this "Classical" doctrine did need to be amended to take securities market speculation into account as organized exchange markets grew in importance.

61/ General Theory, p. 243. The "natural rate" is, indeed, of no use as a reference point in a Keynesian deep depression. Once the failure of sales-expectations to be realized during the recession has induced a further decline of the (subjective) marginal efficiency of capital, unemployment will persist even if we have both $w = \hat{w}$ and $r = \hat{r}$. Cf., e.g., Leijonhufvud (1968b). The general equilibrium values for prices are significant only insofar as they reflect full information.

62/ Keynes' reaction to the overinvestment theory of Hayek's Prices and Production was, simply, that while overinvestment in the past might have been regrettable, he could not see that it should cause any problems in the present; the only result would be to leave us with more capital in the present -- and so much the better off for it. But here Keynes is, so to speak, a dead duck in shallow water. His argument reveals, of course, an aggregative concept of capital on his part that would hardly be tolerated in Cambridge of later days.

63/ Cf., esp. J. Robinson (1951), and R. Kahn (1954).

64/ H.G. Johnson (1951-2) surveys the discussion inside Cambridge.

65/ An alternative argument stresses Keynes' so-called Finance Motive. As far as I can understand, it only introduces a lag in the adjustments of output and loanable funds flows without otherwise affecting the sequence discussed.

66/ Agents discover that they have "too much cash" only after income has fallen; it is not the case, here, that requirements for transactions balances are scaled down already when spending-intentions (by entrepreneurs) are cut back. When people decrease their rate of spending of money on goods -- so says the LP theory -- the resulting increase in the ratio of their cash balances to their income is unintended and unanticipated.

67/ As noted, Quantity Theorists will have a hard time. It is not that an excess demand for money causes nominal income to decline here -- it is that falling income causes an excess supply of money. Cf., Leijonhufvud, (1968), pp. 30-31, 182-83.

68/ Go ahead and invest -- the savings cannot help but be there. It is a notion that will make Economic Development seem almost too easy.

69/ Part of the problem here, actually, is the conventional definition of saving. Keynes was happy to find general agreement on saving meaning non-consumed income and dispensed, therefore, with any detailed discussion of the concept. (General Theory, Chs. 6 and 7). But "non-consumed income" can be (at least) directly invested, placed in securities or intermediaries, or hoarded. It is one of the more common muddles of macroeconomics that we do not know what an "increase in non-consumption means" for the state of excess demands in these various markets.

70/ Harrod also shared the LP theory of interest and thus the position that growth is not the result of the coordination of saving and investment decisions. He proposed instead the question: What rate of capital accumulation, if entrepreneurs happened to set out on it, would be sustained by the resulting aggregate demand? It will be recalled that the saving propensity enters into the formula answer. Saving, however, enters into the theory, not as a demand for securities and other stores of wealth, but as "non-spending", i.e., a leakage from the aggregate demand that alone can keep animal spirits up. The Paradox of Thrift mechanism is built into the model and it is it that makes the warranted path a knife edge. The Hicks-Goodwin cycle models of the 1950's also had this property.

71/ Cf. J. Robinson (1937), Chapter II. This book is especially helpful in getting a picture of the original Cambridge Keynesian position. Note that the preface to the second edition (1969) leaves unmodified those aspects of the theory that concern us here.

72/ Professor Robinson would, I think, reject the use of neoclassical growth theory even for the limited purpose of constructing "notional reference paths" with which actually postulated system motions might be compared in order to see what has gone wrong. (This is, of course, the method used throughout this article). A statement of the type "The system has got stuck at a rate of interest higher than the rate consistent with coordinating saving and investment decisions on a full employment growth path" would be judged meaningless by a Cambridge Keynesian since the basis for the comparison is a state which we -- if we adhere to the strict LP theory of interest -- cannot imagine the real world system to produce under any circumstances.

73/ The price level, for example, is governed by wage-push.

74/ When I wrote On Keynesian Economics and the Economics of Keynes, I did not identify who these "Keynesians" were. I could not come up with any major names to whom I felt one could with confidence attribute belief in the cluster of propositions I was dealing with. Having turned a quick reputation on making critical noises about the "Keynesians", I do not really want to press the case for their never having existed. In any case, my readers did not seem to share my vague unease about this -- it is one of the things on which I cannot remember being challenged.

75/ 45? Do I hear a 50?

76/ The latest serious attempt to give a positive definition to "Keynesianism" may indeed be that of Friedman in his Monetary Framework. It is as if he was seeing the imposing enemy that he has battled for twenty-odd years turning into a windmill before his very eyes -- a process one must try to reverse!

77/ Into the 10th ed. of Samuelson's Principles, for instance. The Paradox is, of course, implicit -- even when not asserted -- everywhere that the Keynesian cross is employed.

78/ Cf., e.g., Gardner Ackley (1961), Chs. VII-IX. One of the strong points of Ackley's book is that he is so clear on the Wicksellian antecedents of Keynes' saving-investment problem.

79/ ...and them skipping the old reservation in ever growing numbers!

80/ E.g., Clark Warburton (1946).

81/ And when Milton Friedman, in a paper specifically intended to define the issues between Keynesians and himself, is content simply to leave "the saving-investment sector as unfinished business", his critics do not object or find it worth discussing. Cf., Friedman in Gordon, ed., (1973), p. 40.

82/ ...except, of all people, Milton Friedman (in Gordon, ed., op. cit.).

83/ R.G. Lipsey (1972).

84/ Cf., A. H. Hansen (1949), Chapter 5, and (1953), Chapter 7. In Hansen's reading Keynes' interest theory was "indeterminate" -- money supply and demand could not determine the interest rate, as Keynes would have it, but only give you the LM curve, etc., This way of looking at it misses the issue of which excess demand governs the interest rate.

One is reminded of Hansen's indeterminacy charge by Friedman's more recent argument that Keynes' theory suffered from a "missing equation" -- and should be completed by adding an exogenously determined price level. Keynes' theory, like the others discussed in Sections IV through VIII above, was of the dynamic-historical variety. In describing the state of the system at some point in the sequential process, such theories make use of information about the system's initial (historical) state. Static models do not use historical information, of course, but have to have equations for all endogenous variables. Reading a dynamic-historical theory on the presumption that it is static, therefore, is apt to lead to the mistaken impression that it lacks equations and is indeterminate.

85/ Patinkin (1956) also preferred working with a single aggregate demand function with income and interest rate as its arguments rather than with separate saving and investment functions. This practice does not draw attention to the role of the rate of interest in coordinating intertemporal activities and will not put the spotlight on its possible failures to play that role.

The emphasis by so many Keynesian writers on the national income accounting definitions of saving and investment is not helpful either. The equality of saving and investment bears no necessary relation to the equilibrium growth path and the natural rate of interest when the variables are defined as to include "unplanned" or "undesired" components.

86/ For an appraisal of the Pigou-effect argument in the context of Keynes' theory, cf. Leijonhufvud (1968), pp. 315-353. From a somewhat different angle the matter is again taken up in the next section below.

87/ If, in the normal case, current sales were the immediately binding constraint on demand, temporary price-inflexibilities would suffice to produce effective demand failures as in the fix-price models, for example, of Barro and Grossman (1971) and Malinvaud (1977). Reasons for not treating this as the normal case are discussed in Leijonhufvud (1973). In the present paper, attention is confined to "within-corridor" problems.

88/ This should have precluded treating the Phillips curve as a convenient appendage to the textbook IS-LM model which explains unemployment as the consequence of the lack of an operative labor market mechanism. It did not. This is an example of what I mean by saying that intellectual coherence was not maintained in mainstream macroeconomics.

89/ Here, I exclude the option of (a) assuming that interest rate maladjustments do not arise but (b) that wages are exogenously "fixed", so that (c) the (in any case ill-founded) notion of a stable relation between the rate of change of money wages and unemployment must be rejected.

90/ The policy-implications for states of the system of the type considered in Z-theory, and for states developing out of them due to the induced deterioration of sales-expectations, are discussed in Leijonhufvud (1968), Chapter VI, and (1968b).

91/ Accelerator-effects might cause subsequent observations to diverge even farther.

92/ Cf., Modigliani (1977), p. 9.

93/ J.R. Hicks (1967), Chapter 9.

94/ J.M. Keynes (1936), p. 190, quoted from Ricardo's Principles of Political Economy.

95/ The reader should realize that my "impressions" here amount to rather loose speculation for which, ordinarily, I would have to be prepared to apologize. I am sure, however, that Milton Friedman will not object to my proceeding "as if" they were true as long as the implications check out.

In Friedman's provisional Price Theory (1962), the chapter of capital and interest was altogether Knightian. But the fully worked out version (1976) has a stock-flow analysis which, although the discussion retains some Knightian touches, implies that the IS-curve should in general be downward sloping. But then again we have the mention of a possibly horizontal IS in Stein, ed., op. cit., p. 311, and also, for instance, Friedman's nod to Knight and von Neumann in Gordon, ed., (1973), p. 37n.

96/ The "normal rate" here will mean, then, the long rate of interest that speculators have come to regard as "normal". Unfortunately, Wicksell has in some contexts used "normal" for his "natural" rate, but the latter has become so much the accepted label for Wicksell's concept in English that reintroducing the "normal rate" for a different concept should not cause confusion.

97/ Friedman in Gordon, ed., op. cit., p. 24 notes correctly that the elasticity will be larger the more homogenous the expectations of different holders of money and the more firmly they are held. He then goes on to assume "Let there be a substantial body of holders of money who have the same expectation and who hold that expectation firmly, and f will become perfectly elastic."

Compare Keynes (1936), p. 172: "It is interesting that the stability of the system and its sensitiveness to changes in the quantity of money should be so dependent on the existence of a variety of opinion about

what is uncertain. Best of all that we should know the future. But if not, then, if we are to control the activity of the economic system by changing the quantity of money, it is important that opinions should differ. Thus this method of control is more precarious in the United States where everyone tends to hold the same opinion at the same time..."

In England where everyone thinks for himself (and most people, therefore, arrive at the wrong conclusion), the monetary authority--Keynes is saying--can count on fooling some of the people all of the time. In the U.S., the possibility that the mob might catch rational expectations is a threat to the powers of the Central Bank. Given the success of the U.S. authorities over the last 15 years in fooling all of the people most of the time, however, why envy the Bank of England?

98/ Friedman, op. cit., p. 169. This is argued contra Patinkin. I find myself unwittingly caught in the middle and trampled into the footnotes underneath as these two giants of monetary economics clash over this issue. Patinkin (in Gordon, op. cit., p. 129) chides Friedman for favorably citing my 1968 book and then not paying heed to my rejection of "interpretations of Keynes that are based on the liquidity trap." Friedman replies that he thinks my analysis "justifies a different conclusion about the role of absolute liquidity preference than the one Leijonhufvud reaches (that it plays no important role)..." (op. cit., p. 177).

My actual position has been and remains the following. (i) By "Liquidity Trap" I mean precisely what Friedman terms "long-run absolute liquidity preference." I maintain that it plays no role in Keynes' theory. (ii) I also maintain that the speculative demand for money does play a key role in Keynes' theory. I do not think Friedman's "short-run absolute liquidity preference" is an improved label for it, in part because there is no reason for insisting on it being "absolute". I do not think that Patinkin agrees with the importance that I attach to the speculative demand in my interpretation of Keynes.

99/ As noted by Patinkin, (1965), Chap. XIV:3, however, it is misleading to treat this lower limit to the range of variation of the interest rate as if it was a property of the money demand function.

100/ Friedman, op. cit., p. 42, italics added.

101/ Friedman, op. cit., p. 16.

102/ It is found in Friedman (1976), pp. 313-21.

103/ The Keynes-effect, of course, does do it. But that is still a very strange way, indeed, for the system to effect the necessary traverse:
A. Leijonhufvud, (1968), Chapter V:1-2.

104/ J.R. Hicks (1935).

105/ A. Leijonhufvud (1977).

106/Correspondingly, of course, I side with Robertson in the controversy with Keynes -- and later with the Cambridge Keynesians -- that began soon after the publication of the Treatise. Cf., my (1968), p. 24n., pp. 28-31.

107/I am ignoring, obviously, the interest-elasticity of the non-speculative demand for cash-balances. Compare Section II.

108/I am anxious to make this point since it is the "method" I have been using in everything I have written on Keynesian subjects.

109/Keynes may well have been right that the marginal efficiency of capital was falling in Britain through the 1920s. It is not an unreasonable supposition. Recall, however, that the beauty contest was rigged from 1926 on -- the Bank of England was defending an overvalued pound with high Bank Rate. It seems doubtful that either aspect of that historical situation deserves being encapsulated into some IS-LM story of a "typical" recession.

It is important also to be clear that British unemployment after 1926 obviously could have been cured by wage and price deflation (by improving the trade balance and removing the need to defend the pound). Consequently, "rigid wages" are pertinent to that historical situation. Benjamin and Kochin have recently found that Britain between 1921 and 26 had saddled itself with a very ill-designed unemployment insurance system that could not help but keep reservation wages up. Cf. Dan Benjamin and Levis Kochin (1979).

110/Note that the classroom rigid wages explanation of unemployment presumably is of this kind: workers and employers are acting on inconsistent beliefs about what level of real wages the state of the economy warrants.

111 All of this is done in more detail, albeit in the *Commedia dell'Arte* manner, in my (1968b) and also (1968) Chapter VI.

112 The reader will recognize this as a Prisoner's Dilemma case for fiscal policy.

113 Cf., A.G. Hart, (1942), and J.R. Hicks (1976), Chapter 2.

114 Note that the low short rate/low velocity data points, that would be generated in this way, do not belong on the steady state money demand function. Hence my suspicion, indicated above, that the usual Latane-type time-series estimates get too high a value for the interest-elasticity of the transactions demand for cash.

115 R.E. Lucas, Jr., (197).

116 Leijonhufvud (1973).

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