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Working Paper

What would Keynes have thought of rational expectations?

Diskussionsbeiträge - Serie A, No. 177

Provided in Cooperation with:

Department of Economics, University of Konstanz

Suggested Citation: Leijonhufvud, Axel (1983) : What would Keynes have thought of rational expectations?, Diskussionsbeiträge - Serie A, No. 177, Universität Konstanz, Fakultät für Wirtschaftswissenschaften und Statistik, Konstanz

This Version is available at:

<https://hdl.handle.net/10419/75153>

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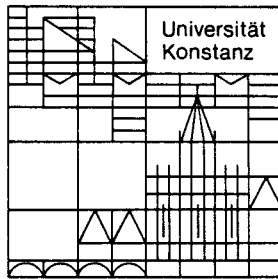
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**What Would Keynes Have Thought
of Rational Expectations?**

20. NOV. 1984 Weltwirtschaft
1161

Diskussionsbeiträge

Postfach 5560
D-7750 Konstanz

Serie A — Nr. 177
Juli 1983

22. AUG. 1983 Weltwirtschaft /

WHAT WOULD KEYNES HAVE THOUGHT OF RATIONAL EXPECTATIONS?

by

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Serie A - Nr. 177

July 1983

I. Introduction	p. 1
II. Monetarism in Three Lessons	p. 2
III. The Out-of-Focus Keynes	p. 6
IV. Expectations	p. 8
V. Prices and Quantities	p. 11
VI. Monetary Regimes	p. 16
VII. Regimes and Cycles	p. 21
VIII. Involuntary Unemployment	p. 25
IX. The 1920's and the 1980's	p. 30
X. Conclusions	p. 33

Paper to be presented at the Royal Economic Society's Keynes Centennial Celebrations, King's College, Cambridge, July 1983. Among the people not responsible for this paper, Robert Clower, Michael Darby and Carlos Daniel Heymann deserve special mention.

Serie A: Volkswirtschaftliche Beiträge

Serie B: Finanzwissenschaftliche Arbeitspapiere

Serie C: Betriebswirtschaftliche Beiträge

I. Introduction

The Keynes Centenary celebrations would be more festive if the Keynesian tradition were in intellectual good health and vigor for the occasion. Unfortunately, it is not. Unsuccessful policies and confused debates have left Keynesian economics in disarray.

In recent years, the intellectual excitement in macroeconomic theory has centered around the development of the rational expectations approach. Many economists have concluded that rational expectations spells the end of Keynesian economics -- and many more seem to fear that this is so, even while they dispute it. What has caused the most commotion, however, is not so much rational expectations per se but rather the so-called New Classical Economics. Rational Expectations is but one of the characteristic components of NCE. The other two are Monetarism and Market Clearing.

It does not seem particularly fruitful to speculate on how Keynes might have reacted to theoretical developments taking place thirty years or so after his death. Economists who still regard themselves as "Keynesians" (in some sense) will, however, have to define their positions vis-a-vis these new developments. What should we learn from this recent work? What criticisms of Keynesian economics have to be accepted? What lessons of Keynesian economics must not be abandoned? How can they most persuasively be reasserted?

The relevance of Keynes' contributions to current concerns

is best reaffirmed by providing good, clear answers to these questions. Many retorts to the New Classical Economics have been impatient outbursts, tinged with moral indignation. They have gotten us precisely nowhere. Quite generally, Keynesian economics has adapted badly to opposition. As a consequence, it is losing the battles for the best young talent in economics. In the United States, this has been true for a decade or more. To the younger generation of economists, Keynesian economics -- all of it, not just Keynes himself -- belongs to the history of economic thought.²

II. Monetarism in Three Lessons

How did Keynesian economics end up in such a sorry state? Although some of us have not conceded defeat, it is obviously a widely held view that Keynesianism was vanquished by Monetarism. James Tobin has distinguished two Monetarist creeds: Mark I and Mark II.³ It is useful, I think, to distinguish two stages in the development of his Mark I Monetarism and, correspondingly, to recognize three stages of the long controversy.

In the first stage, through the mid-1960's,⁴ the discussion concerned the Monetarist causal interpretation of money-income correlations. The stability of the demand function for a well-specified stock of "money" and the predominance of supply over money demand in the determination of that money stock were the core tenets of this Stage I Monetarism. In claiming that monetary policy would be an effective regulator of nominal income this Monetarism differed markedly from Keynesian views of that time. In almost every respect the policy doctrine

advanced by Friedman and Brunner was diametrically opposed to that of the Radcliffe Report.

In the second stage of the controversy, many Keynesians embraced the Phillips-curve and the Monetarists challenged its stability. Arguments based on the anticipation of inflation became central to the debate for the first time. Although not logically entailed by labor-market anticipation of inflation, the Natural Rate of Unemployment hypothesis was made a Monetarist doctrine. This natural rate doctrine sharpened the crowding-out arguments against fiscal stabilization policies. The Monetarists found use for the anticipated inflation model (AIM) also in accounting for the Gibson's paradox (pro-cyclical) pattern of nominal interest rates. Friedman's presidential address (1968) authoritatively summarized this Stage II Monetarism.

In the third stage, Lucas (1972) succeeded in providing a model, carefully built on rational expectations foundations, within which Friedman's (1968) conjectures about the short-run and long-run Phillips curves hold true. A breakthrough in the systematic modelling of informational assumptions, this immensely influential paper married the rational expectations approach to Stage II Monetarism from the outset. Sargent (1973) generalized the policy-ineffectiveness proposition which was then further developed by Sargent & Wallace (1976) and Barro (1976). The "New Classical Economics" gained currency as the label for this Stage III Monetarism.

The reason for distinguishing between the Stages I and II is that the former is capable of a "weak" and a "strong"

interpretation of the money-income correlation. In the strong version, exogenous changes in a purely supply-determined money stock interact with a stable money demand function to "cause" the observed movements in money income. The "weak" version allows a reciprocal influence from real income movements via real money demand to the money stock. The weak version nonetheless implies that control of the money stock will yield control of money income. Recall the oft-quoted summing up of Friedman and Schwartz's Monetary History:⁵

Mutual interaction, but with money rather clearly the senior partner in longer-run movements and in major cyclical movements, and more nearly an equal partner with money-income and prices in shorter-run and milder movements -- this is the generalization suggested by our evidence.

Sufficiently diluted, Stage I Monetarism can be made weak enough, obviously, to be stomached by almost all Keynesians, most of whom use a stable money demand function in any case. Stage II Monetarism, however, pretty much excludes this weaker interpretation. In the absence of monetary shocks, employment stays at the Natural Rate level. The permanent income corresponding to the Natural Rate of Unemployment determines the demand for real balances which is, therefore, a constant in the absence of monetary shocks. This leaves us with "money causes income", without the reciprocal influence. It is this strong version, consequently, that is carried over into Stage III New Classical Theory.

Where then did Keynesianism founder? At Stage II, obviously, on the Phillips-curve or, more generally, on the failure to incorporate inflation rate expectations in the Keynesian model.

When the American inflation picked up steam, the misbehavior of the Phillips-curve and the inflation premium in nominal interest rates became obvious for all to see. Monetarists, who had predicted these things by reasoning from the neoclassical anticipated inflation model, made enormous headway within the economics profession and without. Keynesians, who had continued to argue the usefulness of the Phillips-curve and to pooh-pooh the empirical relevance of the anticipated inflation model, lost face and lost influence.

It was a debacle. A bad enough debacle so that the profession proclaimed the long controversy a Monetarist victory and, by and large, turned its interest elsewhere. This collective reaction left a number of things muddled.

First, the Phillips-curve and Gibson's Paradox were both late-comers among the issues of the Monetarist controversy. When the verdict was rendered on the basis of the obvious significance of inflationary expectations, the original (Stage I) issues were not thereby settled. Rather they were forgotten -- or at least tabled for a number of years. I would agree with Tobin that "...the question whether money causes income or income money or both is still undecided."⁶

Second, the stable Phillips-curve had not been an integral part of earlier Keynesian theory. It was added on to that theory in the 1960's, not without opposition by some Keynesians.⁷ It is not obvious, therefore, that the destruction of this excretion by unfolding events should be regarded as tantamount to the demolition of the central structure.

Third, although the Natural Rate hypothesis is

pedagogically effective as the polar opposite to the stable Phillips trade-off hypothesis, it is not the case that empirical rejection of the latter establishes the former. Suppose that fully anticipated, purely nominal shocks have no employment effects. Other things (such as changes in the "marginal efficiency of capital") might still have such effects. The ability to anticipate inflation (or "absence of money illusion"), then, does not by itself imply some sort of strong stability of the economic system around full employment, be it "Natural" or not.

III. The Out-of-Focus Keynes

What do the Three Stages of Monetarism have to do with Keynes? How do we bring Keynes into some sort of relation with developments decades after his death?

In all the debates over Keynesian economics in the last twenty years or so, there is one Keynes that has remained curiously out of focus. This is true also of my own writings. I mean Keynes, the monetary reformer. After more than fifteen muddled years of inflation, preoccupied as we are with intractable problems of monetary stabilization, it seems natural to give some thought to the Keynes who gave so many years of his life, from Versailles to Bretton Woods, to the cause of a stable and workable international financial order.

The Phillips curve debacle coincided in time with the elimination of the last vestiges of the Bretton Woods system. The heritage of Keynes, the theorist, came to grief when the legacy of Keynes, the monetary reformer, had been squandered. Is this

just a curious coincidence? Or should we make more of it?

Keynesian theory failed to incorporate inflation expectations. Before the Great American inflation, the theory was widely accepted as an adequate guide to reality. Once the inflation picked up momentum and became both high and volatile, the Keynesian neglect of nominal expectations became fatal. But the international monetary order that Keynes had striven for should have had responsible international central bank policy by the reserve currency countries and everyone else disciplined by fixed exchange rates. In such a regime, rational agents should not have volatile nominal expectations and a theory in which they do not is appropriate to the regime.

This is a rational expectations argument. The concept of "monetary regime" figures prominently in the more recent rational expectations literature. It links expectations and institutions. It may be defined as follows: A monetary regime is a system of expectations that governs the behavior of the public and that is sustained by the consistent behavior of the policy-making authorities. Since the responses of an economy to shocks or to policy-actions depend on the public's expectations, we need, in effect, a different short-run macrotheory for each different regime.

The regime approach is a highly useful one -- certainly, one of the most useful developments to come out of the rational expectations movement so far. I suggest we use it on Keynes and ask what regimes (if any) his theory would fit and also what his opinions were of various regimes. First, we need to consider his

treatment of expectations.

IV. Expectations

It used to be one of the proud boasts of Keynesian economics that it incorporated expectations in a significant way. Sir John Hicks in his first review of the General Theory gave pride of place among the book's contributions to its treatment of expectations:⁸

If we assume given, not only the tastes and resources ordinarily assumed given in static theory, but also people's anticipations of the future, it is possible to regard demands and supplies as determined by these tastes, resources and anticipations, and prices as determined by demands and supplies. Once the missing element -- anticipations -- is added, equilibrium analysis can be used, not only in the remote stationary conditions to which many economists have found themselves driven back, but even in the real world, even in the real world in 'disequilibrium.'

This is the general method of [the General Theory]; it may be reckoned the first of Mr Keynes's discoveries.

The claim Hicks made for Keynes was that, by bringing in expectations in the right way, he had succeeded in significantly extending the scope of equilibrium analysis. This is precisely the claim now being made for Lucas, Sargent & co. although for rather different reasons. Keynes extended the use of the Marshallian equilibrium method by treating long-term investment expectations as exogenous determinants of his short-run income equilibrium. Lucas extended the use of neo-Walrasian equilibrium analysis by making short-run nominal expectations strictly endogenous again while shifting to a stochastic equilibrium concept that allows realizations to diverge from expected values.

By the early thirties, business cycle theorists had come to realize that use of the equilibrium toolbox could be strictly

justified only for stationary and perfect foresight processes.⁹ This pretty much excluded business cycles -- and there was no other toolbox. Keynes' new method successfully evaded this dilemma. Lucas' new method attempts to solve it.

That, however, is not the whole story. Keynes' innovation concerned the long-term expectations of real magnitudes, while NCE theory has dealt mainly with the short-term expectations of nominal magnitudes. Keynes, on the whole, ignored nominal expectations and the rational expectations pioneers have only recently begun to turn their attention to long-term investment expectations.

Keynes' own treatment of short-term expectations should give pause to anyone tempted to attack the NCE on the grounds that it assumes too much foresight on the part of agents:¹⁰

... it will often be safe to omit express reference to short-term expectation in view of the fact that in practice there is a large overlap between the effects on employment of the realized sale-proceeds of recent output and those of the sale-proceeds expected from current input;... etc.

The omission of "express reference" is achieved, of course, by simply equating expected and realized real income, a procedure subsequently imbedded in the Keynesian cross, in IS-LM, and thus in the entire Keynesian literature. This is "perfect foresight" such as the rational expectations people have not allowed themselves to indulge in! Keynes, I would think, should have appreciated the considerable weakening of this assumption achieved through the use of a stochastic equilibrium concept.

Long-term expectations are another story. In the early stages of the rational expectations debate the issue was the

Phillips curve and the focus, therefore, was entirely on expectations over the most immediate future only. The ability of agents to infer more-or-less correctly the immediate price-level consequences of current monetary policies were emphasized to the neglect of their inability to infer much of anything about the future nominal values that will emerge from longer sequences of discretionary policy actions.¹¹ As a consequence, this early rational expectations literature provides very little in the way of theoretical foundation for the opposition to inflationary policies (and discretionary policies in general) that also characterizes it.¹² This temporary neglect does not mean that the rational expectations approach implies negligible social costs of inflation. Nor does it mean that it somehow precludes sensible study of this problem on which Keynes held such strong views.¹³ On the contrary, progress beyond the point reached in the Tract requires, I think, careful specification of the "inflationary regime" in question -- requires, in other words, a rational expectations approach.

Nonetheless, long-term expectations pose the question of how far the endogenization of expectations can be taken. Elsewhere, I have used a distinction between "well-behaved" and "ill-behaved" expectations.¹⁴ Well-behaved expectations bear a stable relationship to the observable state variables of a macroeconomic model and can therefore be treated as fully endogenous. Expectations are "ill-behaved" if not explainable by the model. If, in addition, they are unobservable (or unmeasurable), ill-behaved expectations will spell trouble for our ability to forecast.

In these terms, Keynes' short-term expectations were (excessively) well-behaved but his long-term expectations ill-behaved in that they shifted for reasons not incorporated in the model. The rational expectations approach to this problem will, of course, be to strive for a behavior-description in which long-term investment expectations are completely endogenized.¹⁵ Keynes would presumably have raised philosophical objections to so foolhardy an attempt to harness the "dark forces of time and ignorance" with the actuarial calculus.

From the standpoint of rational expectations methodology, a refusal to attempt to endogenize all expectations is perhaps nothing but obscurantism. The Keynesian trick of explaining income movements by invoking exogenous (and perhaps also unobservable) "shifts in MEC" appears as nothing more than putting a verbal label on our quantitative ignorance. Clearly, we are better off the more success this ambitious rational expectations programme has. Meanwhile, a label for one's ignorance is a very useful thing -- if it helps remind one that one is ignorant.

V. Prices and Quantities

During the course of the Monetarist controversy it was often said that the two sides differed in their explanations of changes in nominal income but "were in the same boat" when it came to explaining the breakdown of nominal income changes into their price and quantity components. But surely the two approaches do not belong in the same boat? Throughout the entire history of modern macroeconomics, I feel, there has been

something profoundly unsatisfactory, something thoroughly befuddled, about our handling of the relationships between nominal and real magnitudes.

I have no precise diagnosis of what has been the problem. I do have a hunch about it, namely, that the trouble may stem from a failure to keep straight the differences between monetary (or nominal) and real business cycle hypotheses.

Any business cycle "story" will have, as two of its elements, first, a shock or disturbance and, second, the failure of some endogenous variable or variables to adjust appropriately to the shock. The disturbances can be nominal or real (or, of course, mixed) and so can the adjustment failures. Thus, we obtain the classificatory scheme of Table 1, where the "mixed" cases are slighted for the purposes of the present discussion.

TABLE 1 Adjustment "failures"

		Nominal	Mixed	Real
Shocks	Nominal	N/N dM \bar{w}		N/R dM $\tilde{E}(r)$
	Mixed			
	Real	R/N dMEC \bar{w}		R/R dMEC \bar{r}

A "(purely) nominal disturbance" is one that requires a scaling up or down of all nominal values for the reequilibration of the system. Thus, nominal shocks are neutral by definition. A "real shock" is one that requires some reallocation of resources and, correspondingly, changes in real relative prices. Keynes' shifting "marginal efficiency of capital" is the case we will deal with here (oil-shocks and other newfangled inventions will

be ignored). MEC shocks change perceived intertemporal opportunities and require, therefore, adjustments in intertemporal prices, i.e., in the structure of real rates of interest.

In a nominal/nominal (N/N) theory, the disturbance requires a rescaling of nominal values. A truly exogenous change in a purely supply-determined money stock might approximate such a case. If money wages (for instance) were to be inflexible -- for whatever reason -- the maladjustment would show up in changes in employment. Friedman's (1968) explanation of deviations from the natural rate of unemployment exemplifies this brand of theory.

In the diagonally opposed R/R case, the MEC shift requires a reallocation of resources between production for present and production for future consumption. (To the extent that intertemporal substitution elasticities in labor-supply vs. leisure choices are of significant magnitude, it may also call for a change in the present "natural" level of employment). If the intertemporal price structure proves inflexible, saving and investment cannot be appropriately coordinated and the maladjustment, again, shows up in changes in employment. Keynes' (1936) General Theory is, of course, of this variety.

If we could have had a Monetarist controversy of this clear-cut N/N versus R/R variety, modern macroeconomics would be more easily understandable than is now the case. That a failure of nominal values to adjust to a nominal disturbance will mean trouble is not a very complex idea. That a failure of relative prices to adjust to a real disturbance likewise spells trouble is not that much harder to grasp.

The actual discussion has seldom been that straightforward. First, the most widely accepted version of Keynesian economics combines shifts in MEC with rigidity of money wage rates. This R/N story is not at all as transparent as our first two examples. If we start with a real disturbance requiring changes in the allocation of resources and in relative prices, but not in the level of nominal values, why should rigid money wages give us trouble? At best a crucial link is missing from this story. At worst it is confused.

One of the consequences of "Keynesian" economists shifting their ground in this way was a rather confused altercation with the Monetarists over unemployment theory. In this discussion, the Monetarists -- who are obliged to invoke some nominal adjustment failure to explain how the real cycle results from nominal shocks -- were steadfast in denying any rigidity of wages, while the Keynesians -- who should have no particular use for the assumption -- eventually made it the touchstone of Keynesian doctrine.¹⁶ Probably, nothing could have done more to make wage-rigidity seem an essential Keynesian tenet than the objections to it from Karl Brunner and Milton Friedman.

At the same time, of course, Friedman assumed temporary "stickiness" of wages to explain how nominal shocks would cause temporary deviations of unemployment from its "natural rate." In constructing an equilibrium model with the properties conjectured by Friedman, Lucas transformed the temporary maladjustment into an intertemporal one. The canonical version of NCE, therefore, has nominal disturbances causing misperceptions of the real rate of return which give rise, in turn, to intertemporal substitution

adjustments in employment.¹⁷

Thus, the New Classicists have, in effect, shifted the Monetarist position from a N/N one to a N/R one. This moves the muddled conflict over unemployment theory onto the R/N to N/T off-diagonal, which frankly does not help much. It leaves us with Keynesians blaming sticky money wages confronting Monetarists blaming real return misperceptions.

The slow quadrille continues. It may be that most American Keynesians see little difference between the R/N and N/N positions. From IS-LM, one learns that both monetary and real shocks can produce changes in nominal income; it appears, then, that the point one must insist on is that changes in nominal income produce changes in real output and employment only if money wages or prices are sticky. Quite a few former Keynesians, moreover, have come to agree that it takes monetary impulses to produce aggregative movements. These people actually occupy the original Monetarist position (at N/N) but still regard themselves as quite non-Monetarist in their insistence on the inflexibility of wages; they do so with some reason since the leading younger Monetarists have vacated these premises in favor of a position (at N/R) allowing a principled insistence on market-clearing wages.

Meanwhile -- are you following me? -- doubts have arisen in the Rational Expectations camp concerning the Monetarist causation hypothesis. Indeed, Sims has moved already from a reconsideration (1980) to rejection (1983) of the monetary business cycle explanation. Here I must ask you to stand by for further developments. It is, as yet, too early to tell whether

Sims will lead the New Classical Economists to occupy the original Keynes position (at R/R), while the Old Keynesians make themselves at home in Friedman's quarters (at N/N).

This will remind you, I am sure, of that great Cambridge contemporary of Keynes, Sir Dennis Robertson (1954):

... highbrow opinion is like a hunted hare; if you stand in the same place, or nearly the same place, it can be relied upon to come round to you in a circle.

Whether Robertson was here expressing a Rational Expectation or merely voicing the autoregressive prejudice of his five lagged decades in the profession, I will not presume to judge. If, in this clockwise dance, highbrow opinion were to come back to "nearly the same place" as Keynes, it may still not be perceived as a vindication, for by now the term "Keynesian" is little more than a label for the hindmost.

The long controversy between Keynesians and Monetarists is thus a very complicated story. That acknowledged, I will proceed "as if" the basic conflict, all along, had been between a Keynesian real disturbance/real maladjustment theory and a Monetarist nominal disturbance/nominal maladjustment theory.

VI. Monetary Regimes

There are two basic but contrasting conceptions of how control of nominal values can be achieved which we may call the quantity principle and the convertibility principle, respectively. Monetary regimes may be distinguished inter alia according to how closely they approximate a system of pure quantity control or one of pure convertibility control.¹⁸

The quantity principle seeks control of the price level through control of some monetary aggregate usually referred to loosely as the "quantity of money." The logically tidiest version of such a system will be on a pure fiat standard. It requires central banking. The private sector must be prevented from creating perfect substitutes for the government controlled "money" since otherwise control of the latter might not achieve control of the general price level. Hence the system usually has government monopoly of the note-issue and more or less far-reaching governmental control of the banking system. Basically, the government decides on the quantity of money and the private sector sets the price level.

An extreme version of this regime would arise if the government, in changing the quantity of money, did so only by means of currency reforms that change the nominal value of outstanding contracts and of the real balances held by the public. (The 1958 French replacement of old by new francs is an example). In this unrealistic case, the "nominal scalar" case, the government could directly manipulate the nominal scale of all real magnitudes.

The convertibility principle, in contrast, requires the government to set the legal price of some commodity (such as gold), allows banks to produce "money" redeemable into the commodity, and lets the private non-bank sector decide the quantity of paper money and bank deposits it desires to hold. Suppose, just for a moment, that the government could set the legal nominal price of a basket of commodities, and that reedemability of money into baskets could be made operable. Such a "basket case" monetary regime would be the diametric opposite

to the "nominal scalar": the government sets the price level and the private sector determines the quantity of money.

We may thus consider a spectrum of institutional possibilities with the commodity standard regimes towards the convertibility control extreme at one end and the fiat regimes toward the quantity control end. Early banking history shows us systems relying altogether on convertibility for monetary control. The present system retains no shred of convertibility, but relies altogether on governmental quantity control. In between, we could array, in rough historical sequence, the managed gold standard, the gold exchange standard, and the Bretton Woods system in its various stages of ascendancy and decline. The historical process has not been a smooth and gradual transition from commodity to fiat standards, of course. War-time lapses into inconvertible paper were repeatedly followed by the reestablishment of regimes in which convertibility had a more or less significant role to play. With some backing and filling, the secular process has nonetheless been one away from convertibility and toward quantity control of fiat money.

Most of the historical experience relevant to the present discussion is not well represented by either of my two extreme cases. Nonetheless it is instructive to note what kind of monetary theories and monetary policy doctrines would fit these extremes. We should also ask what relationship might be established between the previous classification of business cycle theories and the present one of monetary regimes.

Obviously, the quantity control fiat standard is made for

Monetarist theory. In Friedman's theory, (particularly, Stage II) the central bank sets the quantity of money and the private sector adjusts first nominal income but ultimately only the price level. The monetary authorities can control nominal magnitudes but, in equilibrium, real ones are beyond their grasp. Attempts to control what cannot be controlled produce undesirable results. Pursuit of a low interest target, for instance, would eventually produce an explosive inflation. Monetary policy should be directed at monetary targets and the latter should not be adjusted with an eye to variables, such as employment, that are ultimately beyond nominal control. And so on.

The (unrealistic) case of "basket convertibility" would be a convenient one for Radcliffe monetary policy doctrine. The price level is set and the public rationally expects its future to be regulated by convertibility. The non-bank public's trading of real IOU's for real deposits with the banking system determines the monetary aggregates. To the extent that the central bank can affect the terms of this exchange, i.e., mainly the real rate of interest, it will have some small degree of influence on real investment, output and the real money stock, but control of the nominal scale of real magnitudes in the economy is essentially beyond its powers. Monetary policy operates within narrow limits to affect real credit conditions and liquidity. The use of interest targets does not carry any imminent danger of nominal instability in this setting where both the price level and price expectations are kept in check by convertibility.

Now, of course, not even the late 19th century gold standard resembled this "basket convertibility" regime at all closely. It

had an anchor for nominal values in the sense that price level fluctuations were constrained to those of the relative value of gold in terms of other commodities. Mean reverting price level expectations helped stabilize prices. Even so, a variable supply of new gold, a small and price-inelastic non-monetary demand and a vanishing non-bank monetary demand meant that the bounds which gold-convertibility put on the price level could be uncomfortably wide. The wider these bounds, the more room the system gave for the "Credit Cycle", as Keynes called it.¹⁹ Management of the standard, for reasons beyond merely protecting the solvency of the banking system against "drains", became increasingly desirable.

The last attempt, in the 1920's, to control nominal values through gold convertibility ended in total disaster. Convertibility for the public disappeared and was never seen again. One by one, the features of the commodity standard were removed, making central bank control increasingly important. With redeemability gone, the public could no longer protect itself against inflationary policies. Until 1971, small open economies were still disciplined to some degree by fixed exchange rates but retained a significant ability to sterilize reserve flows, particularly in periods when capital flows were restricted.²⁰

The international system of multiple fiat regimes, snakes, tunnels, dirty floats, and flexible exchange rate is far removed from the old gold standard world. But although the peoples of the Western world have had to become accustomed to the instability of nominal values and even though the correct anticipation of nominal changes is of the utmost importance in such a setting, the present system of multilateral monetary mismanagement does not closely approximate my "nominal scalar" extreme.

So, our historical experience lies well inside these extremes. But the never-ceasing theoretical debate juxtaposes two traditions of monetary analysis each of which interprets that experience as if it "essentially" belonged close to one of the extremes. Very often, moreover, the battle between Monetarism and the "New View" over the interpretation of some regime midway between the extremes is carried out in terms that suggest that the two theories are regarded as mutually exclusive so that one must be True and the other False.²¹ My own unprincipled belief is that both theories are about half true and that we can be dangerously misled if we base policy wholly on one to the total exclusion of the other.

VII. Regimes and Cycles

Turning now to business cycle theories, it is clear that those postulating purely nominal shocks are relevant only at the fiat extreme, whereas at the convertibility extreme only real shock hypotheses are admissible. The (strong) Monetarist causal chain from exogenous money shock via nominal inflexibility to real output and employment is familiar. The Keynesian chain from changes in real intertemporal prospects via real interest rate maladjustments to real income and endogenous movements in inside money, even if familiar, is out of fashion. We may sketch both an equilibrium and a "disequilibrium" version of it.

In the equilibrium version, we start with a rise (exogenous in relation to the model specified) in the future real income perceived as derivable from present factor employment in some sizeable sector of the economy. All agents are equally informed about this change in the situation and all evaluate it in the same way. The entire system responds as would Robinson Crusoe

therefore. Suppose, for the sake of argument, a high degree of intertemporal elasticity of labor/leisure substitution, so that we obtain a significant supply response to this change in the future return to present labor. This will allow a temporary equilibrium employment expansion in one sector without equal contraction elsewhere. Hence, the natural rate of unemployment is not a constant but depends on the marginal efficiency of capital. The expansion of output is financed by an expansion of bank and non-bank trade credit. As income increases more real money balances are demanded so that the additional saving matching the increased investment ends up being partly intermediated by the banking system. Investment, real interest rates, and employment all rise and the expansion of the banking system (and of non-bank credit) allows this to happen without downward pressure on money prices.

Note that this sketch follows the rational expectations equilibrium groundrules, although it is non-Monetarist. The money stock varies with income for purely endogenous reasons. Employment covaries with money income for reasons that have nothing to do with nominal misperceptions or other maladjustments of wages or prices.

The disequilibrium version is the Wicksell-Keynes story. Here it is not the case that all agents get the same information. Individual firms see improvements in the future return to present activity, but no one has an overview of what is happening to the economy-wide marginal efficiency of investment. Since, historically, the average real rate of return has not been a volatile variable, speculation stabilizes the real rate of interest and

firms adjust their rates of investment to it. When the real interest rate fails to find its "natural" level, household saving and business investment are not properly coordinated. In the upswing, (over)-expansion of credit allows investment to exceed planned saving, putting upward pressure on money prices and wages. In recession, the contraction of credit will similarly put downward pressure on prices. The cycle, therefore, would leave a Phillips-curve pattern of observations even in this system where nominal values are anchored by convertibility.

Suppose this is a serviceable description of the kind of cycle that occurs towards the end of our spectrum where convertibility more or less guarantees against the occurrence of purely nominal shocks. What then happens to the cyclical behavior of the economy as the historical trend away from convertibility control takes it towards pure quantity control? What does not happen is that the Keynesian R/R cycle fades out to be replaced by a Monetarist N/N cycle. Two things might happen. Either the quantity control is handled in such a way that shocks requiring adjustments in the nominal scale of real magnitudes do not occur; or it is mismanaged in which case a N/N cycle is superimposed on the R/R one.

Recent history presents us with about two decades of one and two of the other. What seems most interesting about the Bretton Woods regime in retrospect is that a system of expectations basically appropriate to an economy with convertible money was sustained by quantity control and with the central convertibility mechanism removed. A system of price level

expectations consistent with the convertibility principle means that people expect prices to revert to the longer term trend if and when they go above or below trend. For such expectations to be maintained when the economy is not in fact on a standard where non-discretionary, objective factors determine the trend, the central bank must, in effect, "mimic" such a standard. It does so by imposing monetary restraint above trend and applying monetary stimulus below it. The government must also maintain the faith that this pattern of behavior will be continued indefinitely. An (at least implicit) monetary constitution will be of help in this regard. For small open economies, a habit of defending a fixed exchange rate may be the way to accomplish this task -- if, that is, the reserve currency country behaves responsibly. This can be a big "if". In any case, public confidence in the indefinite maintenance of this pattern of monetary control will require budgetary policies consistent with this objective.²²

In the United States, monetary stability was maintained in this way until the mid-sixties. With the private sector firmly expecting a quite low and not very variable rate of inflation, the Federal Reserve System could affect the availability and price of "real" credit to some extent. Monetary policy could play a limited, but constructive role in attempts to stabilize employment. But the continuance of the regime required continued restraint on the part of the authorities.

The one-time Keynesian (or Radcliffe) doctrine of the "ineffectiveness" of monetary policy would seem to have served, however inadvertently, as a myth protecting the Bretton Woods regime. Like the belief in the stickiness of nominal wages, it is a doctrine fitting a true convertible standard -- where the

monetary authorities cannot play around with the nominal scalar, rational agents will not expect that adjustments in the nominal scale of contracts will be needed. When the Friedmanite doctrine that the quantity of money is an effective regulator of nominal income gradually gained acceptance, however, it was inevitable that advocates of discretionary policy would put it to use. To economists who explained unemployment by the stickiness of money wages, this Monetarist doctrine suggested that the stock of money might serve as an effective regulator of employment. If so, it was almost a moral imperative that it be used. But vigorous manipulation of the supply of outside nominal money will destroy the system of expectations that makes nominal values relatively inflexible. The Phillips curve will then start to misbehave.

VIII. Involuntary Unemployment

At the outset I noted that the New Classical Economics was made up of Rational Expectations, Monetarism, and Market Clearing. It remains to comment on the last of the three.²³

The equilibrium approach has caused more uproar among Keynesians than any other aspect of the work of Lucas, Sargent & Co. The reason is that the market clearing assumption is taken to be inconsistent with "involuntary unemployment", a concept which most Keynesians feel obliged to defend to the bitter end of their creed. Much ink has been spilt and a considerable volume of hot air expended, therefore, in criticizing or satirizing the rational expectations approach on this score. From the rational expectations side, scorn is heaped on the arbitrary fix-price constraints of "disequilibrium" theory while the concept of

excess demand is declared inoperational and the notion of "involuntary" behavior spurned as inexplicable in utility-maximizing terms.

In my opinion, however, the issue has hardly been joined, so that not much can be sensibly said about the debate as it relates to Keynes. One reason for this is that few, if any, people on either side care much about what Keynes might have meant by "involuntary unemployment" and that most proceed to use the term as if whatever associations happen to come to mind are good enough at least for polemical purposes.²⁴ The term, without a doubt, is one of the most unfortunate new coinages in the history of economics. The problem to which the term refers is not therefore nonsensical. Keynes was concerned with a systemic problem that could be defined neither in terms of individual decision situations nor in terms of interactions between buyers and sellers in a single market. His "involuntary" unemployment is the result of effective demand failures.²⁵

Two distinct effective demand failures are involved in Keynes' persistent involuntary unemployment state. One is the intertemporal (R/R) one discussed above which arises because²⁶ —

...a decision not to have dinner to-day....does not necessitate a decision to have dinner or to buy a pair of boots a week hence or a year hence or to consume any specified thing at any specified date.

Hence, it does not pay to organize all the markets for specified things at all specified dates. In their absence, it is possible to have an effective excess supply of present goods to which there corresponds a notional excess demand for future goods which is nowhere registered in a market. The other one, predicated on

the prior occurrence of the first, occurs between the spot markets for labor and for consumption goods because unemployed people without money cannot bid for consumption goods so that an effective excess supply of labor may have as its Walras' Law counterpart an ineffective excess demand for goods.

Now, this kind of situation does not have fix-price rationing as a prerequisite. Suppose atomistic markets where, every day, sellers of commodities and buyers of labor post prices and wages and buyers of commodities and sellers of labor have to decide on their demand-price and reservation-wage schedules.²⁷ These prices are set using the best information available. Suppose further that agents find a way to carry out all transactions compatible with these prior valuation decisions. "Markets clear." If, however, the system has been perturbed in some way such that not all agents are equally informed about the developing situation, these information asymmetries will make realized transactions deviate from their "equilibrium" volume (if by "equilibrium" we mean the transactions that would be consistent with plans based on some universally shared view of what the true situation is). So the "market clears" at a "disequilibrium" volume.

In the first round of a Keynesian recession, demand price schedules for capital goods shift down because expectations about their future rental values have deteriorated and the rate of interest at which expected rentals are discounted has not declined commensurably. The derived demand for labor in those industries consequently declines but suppliers of labor, who have

little reason to believe that realizeable real wages have declined throughout the entire system, keep their reservation wages up. So the market clears at reduced employment with not much change in the observed wage-level.

Now, this first round outcome may be described in various ways. To say, as I once did to my frequent regret, that "In the Keynesian macrosystem the Marshallian ranking of price- and quantity-adjustment speeds is reversed"²⁸ is too mechanical to be helpful. It reads as an open invitation to fix-price rationing modelling of the sort that pays little attention to the determination of prices. In my 1968 story, prices were not "rigid" but held up temporarily (in atomistic markets) because of speculation based on "inelastic expectations." This story does not give both sides of the labor market the same information sets. But it does not otherwise differ significantly from the way in which later rational expectations models deal with variations in employment.²⁹

In the New Classical models, however, tomorrow's another day (drawn from a distribution of pretty nice days). Tomorrow you try it again, starting from scratch. Today's decline in employment has no persistent consequences. (A reason for persistence, in fact, has to be invented).

In Clower's version of the Keynesian story, the temporary curtailment in employment means that tomorrow's consumption demand is constrained by today's realized income. The derived demand for labor in the consumption goods industries is now also affected. By a familiar route, multiplier repercussions bring the system into a state where the inability of the unemployed to

back their notional consumption demands with cash is a major reason for the persistence of unemployment. The unemployment that persists in the system for this reason Keynes called "involuntary."^{30, 31}

Now, I will agree that the theory of effective demand failures raises more questions than it answers and, also, that it has made no progress (as far as I know) for several years. But the nature of the problem that it poses should be clear. Individuals interact on the basis of incomplete information. The consequence is a price vector reflecting the incompleteness of information and a pattern of realized transactions which leaves some agents disappointed. Will this set in motion a learning process that leads to a coordinated solution? If price-adjustments were governed by notional excess demands, then neo-Walrasian stability theorems will tell us under which conditions the answer is Yes. Effective demand theory argues, I think persuasively, that there is no reason to suppose that, whatever the trial-and-error process that capitalist economies rely on, the successive trials will in fact be governed by these notional errors. Consequently, tatonnement' stability theorems are suspect.

To my knowledge, the New Classical literature contains nothing of any relevance one way or another to these issues. When "excess demand" is simply dismissed as an inoperational concept, inquiries into its "notional" or "effective" nature are somewhat discouraged. The oft-paraphrased point that "rational agents will act to exhaust perceived gains from trade" may serve very well as a pedagogical note of caution vis-à-vis certain fix-price

constructions, but as a contribution to our understanding of the stability of general equilibrium it ranks somewhere below the Law of Jean-Baptiste Say.

This debate, to repeat, has not been joined.

IX. The 1920's and the 1980's

To many people, my assigned subject is worth discussing mostly in so far as it leads up to a stand for or against Mrs. Thatcher or President Reagan. It should be developed in adversary terms: Keynes vs. Rational Expectations. Aggregate demand management in a world of sticky wages versus policy ineffectiveness in a world of neutral money. Re-inflation versus continued disinflation.

My own belief, in contrast, is that this way of seeing the issues gets neither Keynes nor the Rational Expectations people right. I do not believe it gets the alternatives currently before us right either.

There was one period, the early twenties, when Keynes had to deal with a monetary regime resembling our own, which is to say, a system of flexible exchange rates, unbalanced budgets, and unanchored fiat currencies. His main reaction to it, evidently, was that it urgently demanded Monetary Reform. The Tract was not a book about how best to muddle along from year to year within the existing system. It argued for a change of regime.

The 1920's have recently drawn the renewed interest of balance-of-payment theorists and monetary economists. In a recent paper on the problem of "Stopping Moderate Inflation", Sargent (1981) compares the methods of Poincaré and Thatcher. He criticizes Mrs. Thatcher for carrying through with disinflation without reforming

the policy regime and attributes the 1926 Poincaré "miracle" to a systematic fiscal and monetary regime change.

The diagnosis of the French situation and the precise recipe for the miracle had been given by Keynes more than two years before Poincaré' reluctantly acted on these lines. Keynes' diagnosis, item by item, was then exactly the one that Sargent has now rediscovered.³² For example:

What ... will determine the value of the franc?
First, the quantity, present and prospective, of the francs in circulation (T)he quantity of the currency, depends mainly on the loan and budgetary policies of the French Treasury.

What course should the French Treasury now take in face of the dangers surrounding them? It is soon said. First, the government must so strengthen its fiscal position that its power to control the volume of the currency is beyond doubt

Obviously, Keynes had an adequate working knowledge of that "unpleasant monetarist arithmetic"³³! A more detailed reading of Keynes and Sargent only makes the agreement between the two even more remarkable.

The Tract on Monetary Reform is a very monetarist book. Many latter-day Keynesians like to think that Keynes successfully kicked this habit soon afterward and went on to write the Treatise, which he in turn discarded as the General Theory began to take shape in his mind. But it is also possible to see this progression less as a series of radical changes in Keynes' fundamental theoretical beliefs than as reorientations of his theoretical efforts to meet changing problems.

This characteristic of Keynes' work -- that he adapted his theory to changing problems -- has often been remarked upon. Practical political economists approve; pure theorists disapprove.

Rational expectations economists might recognize in this adaptability of Keynes' something more than an engaging or irritating character quirk. Rational Expectations theory tells us that the short-run effects of particular disturbances or policy actions will depend upon the expectations of the public and, therefore, on the regime that the public believes to be in effect. We need a different applied macrotheory for each monetary regime. The lesson is that we all must, like Keynes, adapt our theories to a changing world.

The Tract denounced "instability of the standard of value" in strong, colorful terms. We do not find him retracting these opinions later. Instead, throughout the rest of his life, he strove for an international economic order that would anchor nominal values and provide "fixed" exchange rates while leaving scope for discretionary domestic policies and, in particular, giving Britain time to adjust.³⁴ His work in theory, subsequent to the Tract, assumed a regime in which the nominal scale of real magnitudes was not being manipulated. The influence of nominal expectations on behavior was correspondingly neglected. His theory did assume, of course, that one would have to face a business cycle even in the absence of nominal shocks. Absence of obstacles to money wage adjustment does not suffice to guarantee rapid convergence on the natural rate of unemployment in this theory, since intertemporal coordination failures ("saving exceeding investment") are not corrected by changes in nominal values.³⁵ The strong version of Monetarism, therefore, cannot hold in this theory.

X. Conclusions

Keynesian economics used to be the mainstream. Now, the younger generation of macrotheorists and -econometricians regard it just as a backwater, look to Monetarism for navigable channels, and find their real white water thrills in the technically demanding rapids of Rational Expectations. This aging Keynesian thinks the main channel is still where it used to be. But it obviously has silted up, is full of accumulated debris, and must be thoroughly cleared, before one can hope that it will see much traffic again.

Mainly, I suggest, the Keynesian tradition has had trouble in keeping the analysis straight on nominal versus real shocks and adjustments. This happened to surface in the squabble over the Phillips curve. But the trouble goes deeper and begins earlier. When I was a student, over twenty years ago, two of the tenets (for example) that were taught to us as "Keynesian" were (1) that unemployment was due to the rigidity of nominal wages, and (2) that monetary policy could not bring about sizeable changes in nominal income. Both propositions are basically true if we can take a framework of monetary stability as part of the (unstated) ceteris paribus conditions. Both are false as matters of "general theory". As it happens, you will be all right as long as you firmly believe both of them. Unlearning (2) while still holding on to (1) led to confusion and produced the Phillips curve debacle.

One does not revive Keynesian economics again by insisting that nominal wages are sticky or by denying that governmental money creation causes inflation. The doctrine that unemployment is produced by nominal income changes (without distinction as to their cause) interacting with sticky wages keeps pointing us in

the wrong direction, namely, toward using nominal instruments to try to bring about real change.

Keynesians should learn from Monetarism (if need be) that manipulation of the nominal money stock has strong effects on nominal income in discretionary fiat money regimes. From Rational Expectations they should learn that nominal expectations (of price-setting agents, in particular) are endogenous in regimes where the nominal scale is subject to manipulation; also that stabilization policy is better thought of in terms of the design of policy regimes with desirable overall, long run properties rather than in terms of one short-horizon policy choice at a time. But there are also fashionable things they should refuse to learn. We do not have sufficient reason to accept the strong version of Monetarism; we have reason to reject the Natural Rate of Unemployment doctrine;³⁶ and we have no reason to pay much attention to Rational Expectations denials of effective demand failures and the possibility of involuntary unemployment.

We should seek a return to a monetary order that should as far as possible minimize nominal shocks. They do us no good but cause us much harm.³⁷ A return to monetary stability -- if we can find a way -- requires us to fore swear policies that are built on the hope of exploiting temporary money-illusions, or the incomplete indexing of contracts, or other information imperfections. This includes fore swearing fiscal deficits financed by borrowing today but by money creation tomorrow.

In a world where the nominal scale were firmly anchored, business fluctuations would presumably still take place (and they would probably leave behind a record of observations looking much like a stable Phillips curve). It is conceivable that these would

be socially optimal in some sense or other, but we have no substantive reasons to give much weight to this possibility. The amplitude of these real cycles and the incidence of their social cost can be modified by policy regimes designed to have real effects on real variables: unemployment insurance, functional finance, built-in stabilizers. The lessons of Hansenian Keynesianism would come back into their own.

As in the 1920's, so in the 1980's: the times call for Monetary Reform. That will be easier said than done. Simple money growth-rules, assuming their operational feasibility, are probably too tight as constraints on systems where not only does "(nominal) money cause (nominal) income" but "(real) income also causes (real) money"³⁸. We should have no longings for the "barbarous relic". And there can be no returning to Bretton Woods. From Keynes, the monetary reformer, we get a useful suggestion on where to start:

"First, the government must so strengthen its position that its power to control the volume of the currency is beyond doubt...."

but hardly any help beyond that point. As is proper for an economist, I am thus led to a dismal conclusion -- namely, we have to start thinking for ourselves.

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FOOTNOTES

1. In the past, I have sometimes been accused of claiming knowledge of "what Keynes really meant". The present title was assigned to me by Royal (Economic Society) decree. In trying to write a paper to fit it, I have had occasional bouts of the unworthy suspicion that it was meant to goad me into some sort of spiritualist seance before witnesses. I would like to declare from the outset, therefore, that I have not been in touch with Maynard about this!
2. A recent Lucas & Sargent paper (1979) is entitled "After Keynesian Macroeconomics." (It deals, however, to a very large extent with econometric issues outside the scope of my discussion).
3. Tobin (1981). For the material in this section, see also Laidler (1981, 1982).
4. Friedman's (1956) "Restatement" through the years in which Friedman and Schwartz's (1963) Monetary History and related works by Cagan, Brunner and Meltzer were absorbed by the profession.
5. Friedman and Schwartz (1963), p. 695.
6. Tobin (1981), p. 41.
7. Cf. esp. Phelps (1968). I may also refer to my own comment, ibid.
8. Sir John Hicks (1936), reprinted as "The General Theory" a First Impression," in Hicks (1982), p. 86.
9. Cf., again, J.R. Hicks (1933), reprinted as "Equilibrium and the Cycle," in his (1982). Compare also Robert E. Lucas, Jr., (1980), reprinted in his (1981), esp. section 5.
10. Cf., Keynes (1936), pp. 50-1. My colleague, Robert Clower, reads this passage simply as assuming static expectations. Even on that reading, however, the solution states of Keynes' model will be perfect foresight equilibria.
11. Cf. Leijonhufvud (1981b, 1983a).
12. That the New Classical Economics does not provide sufficient reasons for its strong aversion to inflationary policies is a complaint often voiced by critics. Cf., e.g., Tobin, op.cit., or Hahn (1983), pp. 101ff.

13. Keynes (1971b) "Preface". It is true, of course that Keynes' thought changed and developed from the Tract on. It is also true, however, that the world of the Tract resembles our current regime of fiduciary standards and flexible exchange rates more than does the world of the Treatise or that of the General Theory. One must insist, moreover, that we do not have evidence from Keynes's later years that would indicate a change of mind on his part with regard to the consequences of inflation.
14. Leijonhufvud (1983a).
15. Early in his career, Lucas was best known for his work on investment. He returns to it, in context of a complete macromodel and from a rational expectations perspective in his (1975). For the growing interest of the rational expectations group in explaining the cyclical behavior of investment, cf., the "Introduction" to Lucas (1981) and for recent work, e.g., Kydland and Prescott (1982).
16. E.g., Okun (1981), Gordon (1981), or Solow (1980).
17. Cf., Lucas (1975) and Barro (1980).
18. I have discussed the material in this and the following sections more extensively in my (1982a, 1982b).
19. Keynes (1930).
20. For an assessment of the scope for independent monetary policies, cf., Darby, Lothian et al. (1983).
21. For the strong anti-Monetarist position, cf., Kaldor (1982).
22. See the "Unpleasant Arithmetic" of Sargent and Wallace (1981), and Keynes' advice to Poincaré quoted in Section IX below.
23. The following remarks will deal only with "involuntary unemployment" which is the aspect of the matter that has the most to do with Keynes. For a broader discussion of the equilibrium methodology of the New Classicists, cf., the last section of my (1983a) and also my (1983b).
24. Lucas (1978), for example, concludes his discussion of the concept as follows: "In summary, it does not appear possible, even in principle, to classify individual unemployed people as either voluntarily or involuntarily unemployed depending upon the characteristics of the decision problems they face." Lucas may have had in mind, perhaps, Barro and Grossman (1971), Benassy (1975), or Malinvaud (1977), etc., but his comment is simply irrelevant to Keynes' concept.

25. One should recall that the Keynesian categories of "frictional" and "voluntary" unemployment covered vast territories, and especially a number of possibilities that later-day Keynesians often like to bring into their quarrel with the rational expectations equilibrium theorists. The General Theory (1936, p. 6) briskly lumps into the voluntary category, for instance,

"unemployment due to the refusal or inability of a unit of labour, as a result of legislation or social practices or of combination for collective bargaining or of slow response to change or of mere human obstinacy, to accept a reward corresponding to the value of the product attributable to its marginal productivity."

Note especially that the "inability..." is "voluntary"!

26. Cf., General Theory, p. 210.

27. This is basically the conception from which I began in my (1968). It will not serve very far before a more structured picture of how trade is organized in the system becomes required. Cf., Robert Clower (1975).

28. Leijonhufvud (1968), p. 52.

29. An important class of rational expectations models, exemplified by Barro (1976), have what amounts to Hicksian "inelastic expectations" as a central feature. Most of the "action" in realized transactions comes from a term in the supply and demand functions which measures the difference between current and expected future price. When the expected future price fails to reflect a disturbance appropriately, the result is speculative intertemporal substitution effects that affect the price and volume of transactions in the spot markets.

Asymmetries of information between the two sides of the market are against the rules of the game that apply to this class of models, however. They occur only in the market where the central bank conducts its open market operations.

30. Cf., Clower (1965, 1967), Leijonhufvud (1968, Chapter II:3), and for second thoughts on how prevalent such effective demand failures may be, Leijonhufvud (1973).

31. If you will permit one paragraph of self-indulgence, I have this to add. In my 1968 book, my discussion of involuntary unemployment ended on this note:

One must conclude, I believe, that Keynes' theory, although obscurely expressed and doubtlessly not all that clear even in his own mind, was still in substance that to which Clower has recently given precise statement.

Although this interpretation was the only one that made sense to me, I was nonetheless conscious of having done a good deal of reading between the lines. A number of colleagues who did not agree that "one must conclude" anything of the sort poked some fun at the claim. When Volumes XIII and XIV of the Collected Writings appeared, I skimmed them solely to see whether my interpolations had been too imaginative. Somewhat to my consternation, I could not find anything that seemed relevant to the problem one way or another! In the Fall of 1974, I visited Cambridge as an Overseas Fellow of Churchill College and took the opportunity of a dinner at King's to ask my host, Lord Kahn, and also Lord Kaldor and Professor Robinson whether the Circus had not discussed Chapter 2 of the General Theory and why no background material had come to light. They did not recall any such discussions -- which left me somewhat mystified.

Some time ago, Mr. C.W.S. Torr brought to my attention that the "Tilton laundry hamper" had contained the answer. Much of Vol. XXIX is devoted to some discarded introductions to the General Theory in which "the contrast between a Co-operative and an Entrepreneur Economy" is treated as fundamental.

Keynes' "Co-operative Economy", as it turned out, was one in which labor is bartered for goods, so that the supply of labor is always an effective demand for goods. In his "Entrepreneur Economy" the Clowerian rule applies: labor buys money and money buys goods but labor does not buy goods. In the entrepreneur economy, therefore, effective demand failures are possible and so, consequently, is "involuntary unemployment".

That, I think, should settle the matter. Cf., Keynes (1979), pp. 63-102.

32. Cf. the March 1924 "Preface to the French Edition" of Keynes (1971a). Also the 1926 and 1928 commentaries reprinted as "The French Franc" in Keynes (1972).

33. Cf. Sargent and Wallace (1981).

34. Cf. Tumlr (1983). I am grateful to Tumlr for insisting in conversation that I should go back and read Keynes on the French franc.

35. Cf., "The Wicksell Connection" in Leijonhufvud (1981), esp. sections IX and X.

36. Cf., my "Wicksell Connection", loc. cit.

37. Cf., Leijonhufvud (1981b).

38. For a monetary rule allowing limited scope for discretion, cf. the discussion in Leijonhufvud (1982b).