

WHAT WOULD KEYNES HAVE THOUGHT OF RATIONAL EXPECTATIONS?

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Discussion Paper Number 299
July 1983

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Serie A - Nr. 177

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

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

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"

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

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

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

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

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.

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

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

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

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

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

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

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

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

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.

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

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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171. GERARD GÄFGEN
Zur Beurteilung medizinischer
Fortschritte und Forschungen:
Notwendigkeit, Ansatzpunkte und
Probleme normativer Aussagen

+) Ein Verzeichnis der Beiträge bis einschl. Nr. 153 kann beim Sekretariat der Fakultät angefordert werden.

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.
5. 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.

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.

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