

KEYNESIANISM, MONETARISM AND RATIONAL EXPECTATIONS:

SOME REFLECTIONS AND CONJECTURES

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Working Paper #231
February 1981

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To what extent is Keynesianism discredited? Is there anything left? Did Monetarism score a total victory? Must Rational Expectations make New Classical economists of us all?

Every teacher of macroeconomics has to wrestle with these questions -- hoping against hope that some new cataclysm will not let some fantastic supply-side doctrine or whatever sweep the field before he's been able to sort through the rubble of what once he knew. I am going to sort some of my rubble. The object of the exercise is to make some guesses at how the seemingly still usable pieces might fit together.

My starting points are as follows. Keynesianism foundered on the Phillips-curve or, more generally, on the failure to incorporate inflation rate expectations in the model. The inflation, which revealed this critical fault for all to see, was in considerable measure the product of "playing the Phillips-curve" policies. But the stable Phillips tradeoff was not an

*I have profited from discussions with Carlos Daniel Heymann, and from comments on earlier drafts by Earlene Craver-Leijonhufvud.

integral part of Keynesian theory.¹ Its removal, therefore, should not be (rationally) expected to demolish the whole structure.

Monetarism made enormous headway in the economics profession and with the public when the misbehavior of the Phillips-curve and the inflation premium in nominal interest rates became obvious for all to see. And few observers could continue to doubt the strong link between nominal income and money stock as the Great American inflation went on and on and on. The Monetarist "victory"² was impressive enough that the profession's interest turned elsewhere. But it was not total.³ The Phillips-curve and Gibson's Paradox both were late-coming issues to the Monetarist controversy. The original issues were not settled but rather more or less forgotten. Chief among these was the old backbone of contention, namely, the hypothesis that the money stock is exogenously determined so that the correlation between money and nominal income can be interpreted as a causal one-way street.

In the contest between Keynesians and Monetarists over the Phillips curve and Gibson's Paradox, the neoclassical anticipated inflation model (AIM) played a pivotal role. The Monetarists pushed it; most Keynesians were reluctant to grant it empirical relevance. The Monetarists were seen to have

¹I know, of course, that to some people Keynesianism means little else than Phillips-curve stability, but ask indulgence in using my own definition of the term.

²"Victory" and "defeat" are terms which belong, perhaps, on the sports pages rather than to the history of science. Here, however, no epistemological meaning but only sociology of knowledge connotations are intended: to "win" means to attract the best new, young talent. In this sense, the Monetarism of Friedman and Brunner "won" over American Keynesianism only to "lose" soon afterwards to the New Classical Economics of Lucas and Sargent.

³Rejection of the stable Phillips-curve does not suffice to establish the Natural Rate of Unemployment hypothesis. Cf. Leijonhufvud, Information and Coordination (New York, 1981), pp. 182-87.

been empirically more nearly right on the issues where **this** model had provided their theoretical ammunition.

The AIM was again pivotal when the Rational Expectations group shouldered the Monetarist aide. The full logical implications of the model for the Monetarist position were not entirely welcome: If exogenous money supply changes are known to be the only aggregative shocks worth worrying about, and if exogenous money is neutral, then anticipated (recognized) money supply changes will not have any real effects. Thus, rational expectations methodology applied to the Monetarist position produces New Classical economics. This development has put the Monetary theory of Nominal Income within an (unanticipated) inch of conversion into a short run neutrality proposition that could not explain cyclical fluctuations in real magnitudes and hence could not give a plausible account of the Monetary History of the United States, from (say) 1867 to 1960.

This brief recapitulation of recent controversies suggests an agenda of four items:

- I. The treatment of expectations in macromodels.
- II. The anticipated inflation model.
- III. The forgotten issues of the monetarist controversy.
- IV. Equilibrium or disequilibrium theory?

I. Expectations

Consider how the expectations business looks from the standpoint of politicians and civil servants who have to take some measure of responsibility

for macroeconomic policies and their consequences.⁴ The disarray among macroeconomists is apparent to them. Whose advice do they rely on?

From what they are told, the role of expectations in macroeconomics must be the crux. On the one hand, they have the "Old Keynesian" macroeconomics that once looked so solid and reliable, that had very little to say about expectations -- and that now, apparently, is thoroughly discredited for its lack of attention to such ephemeral matters. On the other hand, they have the "New Classical" economics that looks so paradoxical and speculative, that has very little to say except about expectations -- and that now, obviously, gets all the attention from economists. In between, they have the already "Middle-aged Monetarism" that used rational expectations arguments to undermine the one-time Keynesian belief in a stable Phillip tradeoff -- but that balks at the new Rational Expectations doctrine that fully anticipated money stock policy is totally ineffective. The "Old" advise that monetary policy alone is no way to cure inflation; the "Middle-aged" have it that only monetary policy will do but the safe way is slow and gradual; the "New" urge a quick, clean, indubitable end to inflationary money growth.

For policymakers who have been around for that long, the heyday of Keynesianism must seem like the good old days. Those were the days when macromodels disgorged policy options in the form of readily understandable quantitative predictions: If you do a, GNP will rise by x dollars per annum, employment grow by y percent, and prices go up by z percent. And so on. Nowadays, economists tell them that the effects can be this or that depending

⁴From here on I borrow heavily from my response to Joint Economic Committee Questions on the Role of Expectations in Economics, June 1981. Cf. the Joint Economic Committee volume, Expectations and the Economy, Government Printing Office, Washington, D.C., December 1981.

upon the state of expectations. Unless one can ascertain (in some quantitative manner) what the state of expectations is or will be, therefore, it would seem that one cannot know what it is that one is doing.

Unfortunately, measures of expectations do not inspire trust. Their unreliability (or unavailability) makes direct tests of all the novel propositions about the influence of expectations difficult (or impossible). So, again, whom are policy-makers to believe? And, if they cannot know what they are doing, how are they to choose from the alternative policies that different factions clamor for?

They may hope to escape from this predicament in various ways. The first hope, perhaps, is that there will be many instances where expectations will not matter after all. The second would be that in most of the remaining cases, economists will be able to measure expectations so that their influence can be taken into account.

Vain hopes. In macroeconomics, expectations always matter. Sensible policy judgments cannot be made at all if their influence is ignored. They cannot be accurately measured for econometric purposes. Significant progress on their measurement, moreover, is unlikely.⁵ For present purposes we may as well think of them as unobservable.

There is a third possible avenue of escape from the expectations predicament. Expectations might be "well-behaved" (let's call it).

⁵This is not to say that expectations cannot be made the subject of very worthwhile research. Cf., in particular, Lars Jonung, "Perceived and Expected Rates of Inflation in Sweden," American Economic Review, December 1981.

We may be able to obtain good data on short-term inflation expectations. What we theoretically require, however, is the entire "term-structure" of inflation rate expectations relevant to multiperiod decisions to be made currently. For some purposes, moreover, some measure of the dispersion over agents (period by period into the future) of these expectations is also needed. It is this task that seems to me hopeless.

Expectations are well-behaved if linked in a stable manner to the system of observable variables.

In the simplest imaginable case (Figure 1A), we would have one-to-one correspondence between the unobservable, E, and the contemporaneously observable aspects, S, of the state of the system. The unobservability of expectations then would not matter. It would not prevent us from developing reliable macromodels. (If the world generally were to conform to this simplest case, of course, the proposition that behavior is governed by expectations would be a moot point of Austrian philosophy, supported by no empirical evidence other than introspection.)

A problem first arises when each vector of observable state variables may be combined with any one of many unobservable states of expectation. With one state of expectation, a policy of demand stimulation might, for example, reduce unemployment; with another, it might produce nothing but inflation. So it matters. One may then resort to the past history of observables for the clues that will differentiate one state of expectation from another (Figure 1B). The search is for a one-to-one correspondence between sequences of observable states and states of expectations. If that hope is fulfilled, reliable prediction is again possible.

In Keynesian theory, (long-term) investment expectations are not necessarily well-behaved and Keynesian macromodels had, in fact, not much success in predicting investment. But for the rest, both Keynesians and Monetarists -- following Koyck and Cagan⁶ -- did pretty well by assuming expectations to be well-behaved in this manner. Well enough so that the "old"

⁶L.M. Koyck, Distributed Lags and Investment Analysis, Amsterdam 1954, and P. Cagan, "The Monetary Dynamics of Hyperinflation," in M. Friedman, ed., Studies in the Quantity Theory of Money, Chicago 1956.

macroeconomics in its time did not need to trouble us much by hedging all its predictions on the unobservable state of expectations. Its time ran out with the arrival of the Great American Inflation.

It is the Great Inflation of the last 15 years that has destroyed faith in macroeconomics. By the same token, it is not expectations in general that has been the problem here but specifically expectations about inflation. Why should expectations about the future of the price level give us more trouble, more serious trouble, than expectations about other things?

It is tempting to jump to the conclusion that, perhaps, price-level expectations are ill-behaved, i.e., not related in any stable manner to observables. Then the system can in principle not be modelled in a reliable way and making policy is simply and unavoidably a dangerous business. But it cannot be true in general that price expectations are ill-behaved. If that were the case, we would not have had to wait until the decade of the 1970s to discover that we were in trouble.

Inflation expectations in the 1970's were not well-behaved in either of the two ways that the older macroeconomics habitually sought to rely on. Clearly, people did not form their expectations about the future of the price level just from observing the present state of the economy. Extrapolating from the immediate past, as we know by now, would have been irrational. If these linkages to the present and the past will not do, there is only one way left: Assume that people's expectations match the macromodel's predictions about the future (Figure 1C).

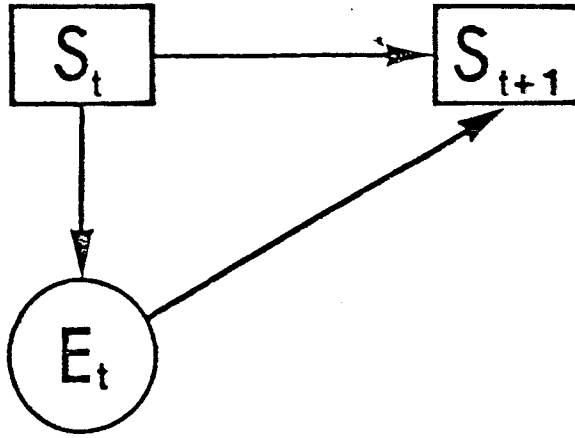


Fig. 1a

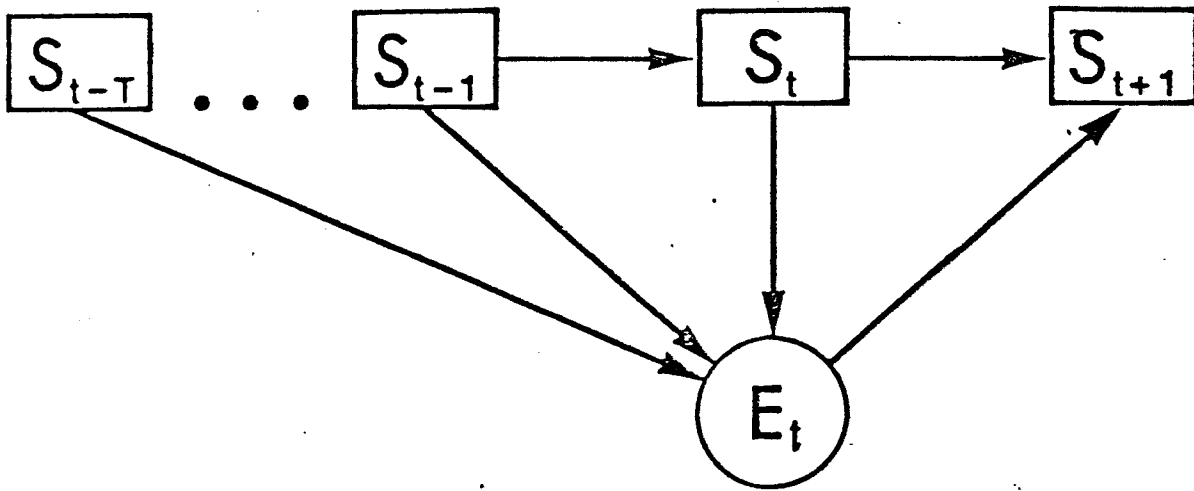


Fig. 1b

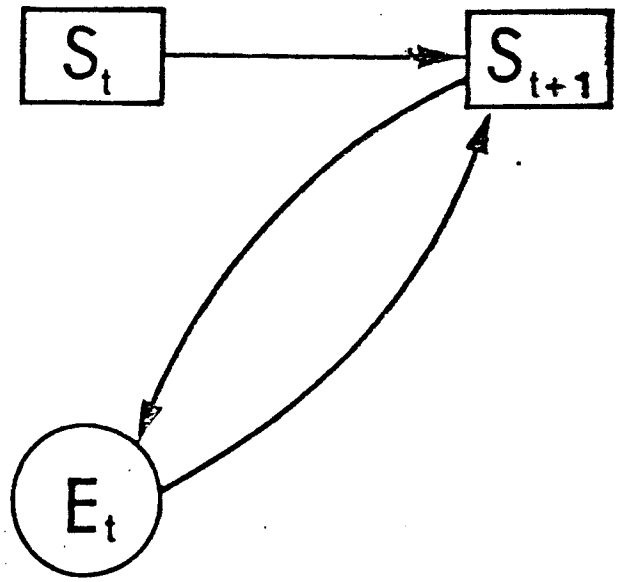


Fig.1c

The costs of not accepting this Rational Expectations development are apparent.⁷ If we have to give up on making expectations well-behaved, we must either start faking expectations measures or else condemn ourselves to an indefinite future of doubletalk: "The effect will be either this or that..." Of course, disassociating oneself from macroeconomics is also a way out and one that many colleagues have found the most attractive of late.

When would this last-ditch method to make expectations behave be needed? If monetary policy was itself well-behaved so that base money creation is some stable function of present and past observables, agents will be able to base their short-term price level expectations on the history of those variables. So this rational expectations twist becomes important when monetary policy is ill-behaved, i.e., when it is not predictable on the basis of past performance.

What limits to rational forecasting should be built into our macromodels to fit such conditions? What is it that people can and cannot know about the future of inflation? We have two clues. First, if the outside modeller must treat the state of expectations as unobservable, then inside agents will not be able to observe each others' expectations. Second, if the system poses a difficult predicament to policymakers (who can call on all the modellers for advice), it must be worse still for agents who have to cope with the added uncertainty of not knowing what actions will be taken by policymakers who do not know what they are doing.

⁷The novelty of this solution is sometimes exaggerated: this is how Keynes dealt with short-term (sales and real income) expectations in the General Theory.

Phelps' paper for this conference begins the exploitation of the first clue. I have been trying to get some mileage out of the second.⁸

II. The Anticipated Inflation Model

Let us define a monetary regime as a system of expectations that governs the behavior of the public and that is sustained by the consistent behavior of the policy-making authorities. The reaction of the public to any particular policy action (such as a change in the growth rate of base money) will depend upon the regime that is believed to be in effect. Consequently, each regime requires its own applied macrotheory; models that do reasonably well for one regime may break down badly for its successor. We can choose among the different possible monetary regimes by choosing behavior rules for the fiscal and monetary authorities.

What consistent behavior on the part of the policy making authorities would sustain the expectations assumed in the anticipated inflation model? If the public unanimously predicts a particular constant inflation rate (to continue indefinitely), then it must be because the authorities are bound to produce it. Rational agents will not anticipate a result that no one is even trying to bring about. The model presupposes a believable precommitment by the government to create money at precisely the pace required to produce the anticipated inflation rate. The authorities operate, in effect, under a most rigid monetary discipline, having foresworn all discretionary options to "make policy" in the future in light of the then existing conditions.

⁸Cf., my "Theories of Stagflation," Revue de l'Association Francaise de Finance, Vol. I, No. 2, December 1980, and "Inflation and Economic Performance," paper delivered at Pacific Institute conference on "Inflation or Deflation?" San Francisco, November 20-21, 1981.

If we define a monetary constitution as a set of rules, binding in the short run,⁹ that specify the conditions under which (base) money will be created or destroyed, then the anticipated inflation model presumes an exceedingly restrictive monetary constitution to be in effect. All meaningful constitutions must put limits on the exercise of discretionary authority. This one, however, eliminates discretion altogether.

The representation of the inflation expectations of the public by a single number, the anticipated inflation rate, seems reasonable in this instance. Individual agents who expect a higher or lower inflation rate will be taught the errors of their ways in a most systematic and pedagogical manner by suffering losses in the market place. Individual expected inflation rates should converge, therefore, on the constitutionally dictated rate. If the regime is operated with great precision, the variance of each agent's subjective expectations should also be small. Obviously, expectations are exceedingly well-behaved under this regime.

Now, if all of this be true, it doth follow that the only welfare costs of inflation are the trivial ones associated with a tax on money balances.¹⁰ Contrary to a widespread opinion among economists, this kind of inflation can be stopped at even smaller cost. If, initially, "greenbacks" are depreciating in real purchasing power by $k\%$ a year, we simply create a new "blueback" currency and make it, by law, appreciate relative to greenbacks by $k\%$ a year. Bluebacks grow in their legal capacity to extinguish greenback debt at

⁹Consider, for example, a constitutional law that can only be changed or amended by majority votes in two consecutive sessions of the legislative body, the two sessions to be separated by a general election. The U.S. Constitution is too difficult to amend, I would think, for the purpose here discussed.

¹⁰M.J. Bailey, "The Welfare Costs of Inflationary Finance," Journal of Political Economy, April 1956.

this $k\%$ rate. This currency reform does not force any redistribution of wealth. All outstanding contracts will be discharged in exactly the real terms originally anticipated by the parties. Since the inflation tax on greenbacks is $k\%$ and zero on bluebacks, the former will disappear from circulation. People will demand larger real balances of bluebacks than they held of greenbacks. To the extent, therefore, that the steady-state demand for money is interest-elastic, the monetary authorities must allow for the creation of a larger initial stock of bluebacks than the stock of greenbacks that is being retired, so as to avoid the emergence of deflationary pressure on the blue price level. If this aspect of the transition is only handled right, there should be not the slightest blip in the unemployment rate. We end up, painlessly, with a zero inflation rate in the blueback currency.

The anticipated inflation model implies that the social problem that we have agonized over for 15 years could be gone by Monday. That implication is false. So the model, for all its pedagogical virtues, makes bad theory. This conclusion is of some consequence because of the model's pivotal role in the macrotheoretical controversies of the 1970s.

For want of a better name, I have called the present American monetary regime the "Random Walk Monetary Standard" (RWMS). This is a somewhat halting metaphor, not a technical description, but it will have to do.

Under the RWMS, the authorities decide one period at a time whether to accelerate, keep constant, or decelerate the rate of money stock growth. Only current economic conditions and immediate political pressures enter into the decision. Future money growth rates are left to the future; they will be chosen at the last minute on the basis of what seems most pleasant and convenient to whoever happens then to be in charge.

In contrast to a Friedman rule (for example) which is unconditional, money creation in this regime is conditional on a great but unspecified number of things, most of which cannot be predicted very far in advance. Consider the term-structure of an individual agent's inflation rate expectations. How could he predict the rate of money growth in the first quarter of 1992? Suppose he already has a forecast for the last quarter of 1991. That given, he needs to predict the acceleration or deceleration chosen by the authorities in the next quarter. But he does not know what the economic conditions and the political situation will be, nor what people will be in charge, nor even what economic theories they will believe in. For dates this distant, therefore, he may as well regard money growth rates (and inflation rates) as picked by a random device.¹¹ The individual's expected price-level for 1991 is a subjective forecast entertained with very little confidence. It is likely to be an ill-behaved expectation, i.e., one not itself predictable from current and past values of the observable variables of some macromodel.

Since there is no scientific method to forecast price levels in the relatively distant future, the distribution over all agents of individual subjective expectations is likely to show very considerable dispersion. Period by period, policy will shower profits on those who most nearly behaved as if they had anticipated it and losses on those who failed so to behave. But the profits and losses produced by frequent turnarounds in monetary policy will not teach people to make better ten-year forecasts. Consequently, individual long-term forecasts will not converge. For brevity, we will refer to this as an incoherent state of long-term nominal expectations.

¹¹He cannot be confident, however, that drawings from this urn will be made at fixed intervals. Moreover, the distribution in the urn is presumably not stationary.

As we consider forecasts over successively shorter terms, more and more of the determinants of monetary policy will begin to seem predictable to the transactor. Such predictions -- about who will be in office, what economic theory he will favor, and what conditions he will face, five, three, two, one year hence -- will vary among agents and so will the models by which they generate price level forecasts from such predictions. Nonetheless, the state of expectations should become more coherent and more nearly well-behaved for shorter and shorter time-horizons. In the very shortest end, behavior might approximate that of the New Classical model: agents watch contemporaneous base money growth and, in so far as they believe the state of expectations to be coherent,¹² let it affect their pricing decisions but not their immediate output and employment decisions.

This does not mean that RW monetary mismanagement has "neutral" consequences. On the contrary, the regime will reduce productivity, discourage productivity growth, and lower the rate of capital accumulation. Longer term commitments will be the most adversely affected. RW inflation will also exacerbate social tensions and undermine popular confidence in inherited political institutions and social arrangements. But these costs and consequences of random walk inflation I argue elsewhere.¹³

None of the three types of theory under discussion has done much to further our understanding of the costs and consequences of the Great

¹²I.e., in so far as they believe that others generate their inflation expectations as they themselves do.

¹³"Inflation and Economic Performance," *op. cit.*, and "Costs and Consequences of Inflation," in G.C. Harcourt, ed., The Microeconomic Foundations of Macroeconomics, London, 1977, reprinted as Chapter Nine of Leijonhufvud, Information and Coordination, New York, 1981.

Inflation. But, of the three, the Keynesian has clearly been the most grievously wrong about inflation. It was wrong about the "ineffectiveness" of money stock policy in changing nominal income, wrong about nominal interest rates, and culpably wrong in supporting the "playing-the-Phillips-curve" policies that led to our present lamentable regime. In each respect, Monetarism was the better guide in an era of developing monetary instability. As the economy learned to adapt, finally, to the incessant discretionary manipulation of a fiat money totally without anchor, the lags in response of prices and nominal short-term rates shortened. In this respect, the Rational Expectations version of the Monetarist model came to look better. At the same time, however, the state of longer-term nominal expectations became increasingly incoherent and ill-behaved with the result that neither agents nor modellers can rationally expect to do very well at predicting the relationships between money, prices, and real activities.

It is not possible to have a macroeconomic science that can predict well in all possible worlds. If expectations are unobservable (or unmeasurable) and ill-behaved, macroeconomics will predict badly, policymakers will not be able to precalculate the effects of what they are doing, and agents will not have well-founded ideas of what policies to expect or of each others' expectations. It is possible, however, to change the world to fit (more nearly) what macroeconomics can do -- and, incidentally, make its inhabitants better off in the process.

This is accomplished, of course, by putting constitutional constraints on monetary policy. Some social institutions exist because they rationally solve problems of conjectural interdependence that cannot be left simply to the rational expectations of individual agents. The right-hand driving rule saves a lot of speculation on average opinion every morning before you hit the

freeway. It also saves those who did not get it right. A monetary standard is another example. It provides a target on which individual expectations can converge. As the state of expectations coheres around the time path implied by the standard, individual agents can plan on the basis of the justified supposition that average expectation is not far off from their own. Economic activities will be more efficiently coordinated as a consequence.¹⁴

Suppose then that we would have a presently unanticipated return to monetary stability -- as much stability, for instance, as the Bretton Woods regime at one time supplied. How would we rate the various macrotheories in such a setting? The judgments of their relative merits given above make the 1970's (as experienced in the United States) the exclusive test of a macrotheory's worth. To do so is very much in tenor with the literature of recent years. But the issues on which Monetarism vanquished Keynesianism only to be outflanked by the New Classical Economics are rather defused by the supposition of a return to stable money. Would our assessment of Keynesian theory be more favorable if we see it in the context of a framework for monetary stability such as might have been constructed by J.M. Keynes (to pick a largely irrelevant name not altogether at random)?

At this point, I am at the end of my reflections. Here start the conjectures.

III. The Forgotten Issues of the Monetarist Controversy

Keynesian theory failed to incorporate inflation expectations. In a world of high and volatile inflation this is a fatal flaw. But for a long

¹⁴Compare the discussion of "coordination games" in Andrew Schotter, The Economic Theory of Social Institutions, New York, 1981. Cf. also Thomas Sowell, Knowledge and Decisions, New York, 1980.

time, before the Great American inflation got under way, a majority of economists found the theory an adequate guide to reality. Events did not reveal the flaw. But how could this be so if the disturbances causing business fluctuations were predominantly of a nominal nature? Can the monetarist hypothesis be the general explanation of fluctuations in a world where the Phillips curve maintains the appearance of stability?

When we come to reconsider the macrotheory of constitutional monetary regimes, the "real" theory of business fluctuations will, I conjecture, make a comeback. The hypothesis of Keynes and most pre-Keynesian business cycle theorists that medium and long term real expectations tend to misbehave will regain, I think, a measure of qualified acceptance. Two subsidiary conjectures: (i) the interpretation of correlations between M_2 and income for such regimes will shift back to a renewed emphasis on the endogeneity of inside money while, correspondingly, (ii) Gibson's Paradox will once more be seen as a procyclical pattern of real interest rates reflecting underlying movements in profit expectations. These are the matters on which I think the "Old" Keynesians were more nearly right than they are currently given credit for.

The Great Inflation has held the Truths of Monetarism before our eyes for some 15 years without a break. But several developments in economics during this same period have actually weakened the case for a "Middle-aged" Monetarist interpretation of nominal income fluctuations in stable monetary regimes. One has been the rationally expectant Youth movement's stress on deficient information about changes in outside money as the (implausible) source of business fluctuations in Monetarist theory. Another has been the Monetarist balance of payments model for small, open economies with its demonstration of the endogeneity of money stocks for at least $n-1$ countries in

a fixed exchange rate world. A third, perhaps, has been the apparent difficulties that some Central Banks have had in hitting their money stock targets with any consistency.¹⁵

Some motivation for these conjectures need to be sketched in conclusion. We should consider a monetary constitution that has much smaller long-term price-level uncertainty than the present RW regime but comparable short-term uncertainty. For concreteness, think of a managed gold exchange standard where, over the longer term, the price level is determined by the requirement to maintain fixed exchange rates, but where, in the short run, the Central Bank can expand or contract by letting its reserves vary. Another possibility is a money stock rule with a band within which the Central Bank is allowed discretion.¹⁶

In this constitutional (MC) system, agents will know the longer-term, "permanent" trend (or level) of prices.¹⁷ They will have inelastic expectations about this permanent price level. The Fisher premium in long-term nominal interest rates, if any, should be constant. Knowing the price trend, MC agents will be able to distinguish periods of "high" and periods of "low" prices. When such periods occur, they will expect reversion to the constitutional mean. Now, suppose for the sake of argument that this system exhibits "cyclical" alternations of "high" and "low" prices and that real

¹⁵For some reason, however, early converts to money stock targets do not seem to have as much difficulty as the last central banks to profess abandonment of interest rate targets.

¹⁶I have in mind a rule specifying a constitutional level of the money supply for each date after the constitution goes into effect and a band-width of $x\% \pm$ around this target. A growth rate rule with a band defined as $z\%$ plus/minus the constitutional rate is a different possibility.

¹⁷And, if this trend is only implied rather than stated in the constitution, they could use some autoregressive scheme to learn it.

activity levels fluctuate correspondingly.¹⁸ How do we explain it? Well, it depends. We are back with the half-forgotten issues of the original Monetarist controversy, before inflation expectations became the center of it.

The Monetarist and the Keynesian hypotheses regarding the cause of business fluctuations need not, of course, be mutually exclusive. It is useful, however, to proceed to begin with as if they were.

Take first the Monetarist hypothesis that the fluctuations in nominal GNP are caused by changes in the exogenous money stock. For simplicity, suppose we have a Central Bank that sets up a sine-wave in base money around the path that will maintain the constitutional price trend over the longer run. This is a purely nominal, systematic disturbance to the economy. It is difficult to see why the system should not learn to find the rational expectations equilibrium path: in each period, nominal prices should be proportional to the base, and the short-term nominal interest rate should reflect the anticipated price change 'til next period. The short term rate should trace out its own sine-wave around a constant long-term nominal interest rate. But then output and employment should not react to this monetary "policy," so the real cycle is left unexplained.

The nominal disturbance may, of course, be less predictable than in the sine-wave example. But agents do not need to predict it accurately, if they

¹⁸It is tempting to go on to say that prices must be "sticky" since they move with less amplitude than nominal income and that this "explains" the real income movements. But the implied suggestion that prices (and/or wages) "should" move proportionally to nominal income is misleading when the shock is not a purely nominal one. Nor does it help much to substitute speculation on the basis of "inelastic price-expectations" for involuntary constraints on price setters -- as in Leijonhufvud (1968), Ch. II. Note that, while rational agents in the MC system should have inelastic expectations about the price level over the longer run, it is not obvious that the monetary stability contributed by the constitution sketched above would instill inelastic price expectations over the short run.

can be quite confident that movements in the nominal base are exogenously caused. As pointed out by Ben Eden,¹⁹ they could simply index-link all contracts to the base so as to obtain "real" transactions prices from which nominal disturbances have been purged.

The class of hypotheses that combine exogenous monetary impulses with the inability of agents to disentangle real from nominal disturbances to explain the cycle seem to me implausible, therefore. In this monetarist world with a constitution, short-term nominal expectations can be kept on track by simply watching the money-supply and long-term nominal expectations are kept well-behaved by the constitution.

Second, then, consider a "real" cycle hypothesis. In order to have a clean-cut opposite extreme to the purely monetarist theory, suppose that the monetary base obeys a Friedman constitution (but that we still have business fluctuations, although perhaps of "moderate" amplitude). All agents know, therefore, that no disturbances emanate from the Central Bank. But investment expectations are not necessarily well-behaved. Forward markets for the goods that will be produced with the capital goods to be bought today are missing. (Tired old refrain?) It is possible, therefore, for agents individually to invest on the basis of expectations that are inconsistent with aggregate ongoing investment.

To keep the story close to those in the recent equilibrium cycle literature, where agents usually are pure price-takers in all markets, we may redo the "shifting marginal efficiency of capital" story into one in which

¹⁹Ben Eden, "The Nominal System: Linkage to the Quantity of Money or to Nominal Income," Revue Economique, January 1979.

producers mistake the future relative price of their products.²⁰ In the boom, the majority of producers think that the price of their product is going to go up relative to the general price level²¹ so that, by producing today for sale tomorrow, they can earn a return (intramarginally) higher than the real rate of interest. In recession, most producers make the opposite error.

The perception of higher profits in prospect may be caused, initially, by innovations in certain industries, by government spending, or by political events abroad, etc. (Since the process never duplicates itself exactly at the individual "island" level, transactors will not learn never to be fooled again.) The producers who first start betting on improved real profits for themselves will bid for more inputs. The expansion of the industries first affected improves business conditions in general. Prices edge up as real supply inelasticities begin to make themselves felt. The increase in the volume of transactions is financed, we may imagine, primarily through an all-around expansion of trade-credit. Bank credit could be a critical component of this credit expansion but is not necessarily of predominant quantitative importance. In any case, the money supply expands endogenously.

The upswing will peak for several reasons. Real interest rates will creep up as the banking system runs out of excess reserves. As the stock of non-bank trade credit outstanding grows in relation to sales, the rate of credit growth tapers off. Rising money prices will make the present employment of "marginal" factors seem increasingly costly in relation to prospects of future revenues (at constitutional prices). But, mainly,

²⁰This suggestion is due to C. D. Heymann.

²¹Some subset may be optimistic instead that they are going to cut their real costs.

producers discover that not everyone's real terms-of-trade can improve relative to everyone else's. As this starts to dawn on more and more people, prices that are high in relation to the constitutional mean will tempt inventory liquidation. When this gets under way, sales revenues will be used to reduce accounts payable rather than to maintain production. Credit outstanding contracts and the inside money stock falls as the recession develops.

This, of course, is simply an attempt to paraphrase the kind of story told innumerable times in the Keynesian and pre-Keynesian business cycle literature.

Repeated episodes of this sort, in the MC setting, might generate observations such that an impressively stable Phillips Curve could be fitted to them. For the sake of argument we might suppose that alternating periods of high prices/low unemployment and low prices/high unemployment produces a Phillips scatter with all points virtually on a line.²² If so, that curve still does not hold out the promise of a permanent policy trade-off. It is not exploitable because the points on the locus are not stationary equilibria; instead, these historical observations record states in which the expectations held are bound to be revised in the light of outcomes. An attempt to exploit the apparent stable trade-off will, if pursued far enough, destroy the monetary constitution without gaining its object of permanently lowering unemployment. Once the MC system is swept away, the stable Phillips curve disappears. But we have been through that, I think.

In a Trade Cycle of this kind, discretionary fiscal and monetary policy could possibly have a useful role in trying to reduce its amplitude and to

²²A series of counter-clockwise loops is more likely, actually.

prompt the upswing. If this is correct, my further conjecture is that such policy will be maximally effective when fully anticipated. Note, however, that we are presupposing continued adherence to a monetary constitution that regulates the monetary base and, therefore, the price level over the medium and longer run. Monetary policy in such a setting is reduced to short-term credit policy and one expects it, naturally, to use interest rate targets. Its "effectiveness" is likely to be limited.

For simplicity we have dealt with the Monetarist and the "real" (or Keynesian) hypotheses as if they were mutually exclusive. In a purely Monetarist case, where real aggregative disturbances are known not to occur, rational agents need only watch the appropriate monetary aggregate and price their wares proportionally to stay out of trouble. In a purely Keynesian case, where exogenous monetary shocks are ruled out, monetary aggregates move with more general movements in real credit; credit, moreover, expands and contracts with real output. Pricing proportionally to bank credit will not do; the safe strategy is to revise prices in response to market excess demand.

The two types of disturbances are not, of course, mutually exclusive but may be present at the same time. When, in addition, they interact, transactors will have a difficult time sorting nominal from real shocks. Suppose, for example, that the Central Bank is in the habit of supplying an "elastic currency," i.e., that it participates in the all-around expansions and contractions of credit so that the base will move pro-cyclically also in Keynesian processes. This, I believe, creates an information problem for the private sector that may give a more plausible reason for short-run non-neutrality of money than does the "islands" story. When the base is seen to expand, is it an extension of "real credit" by the Central Bank (that will reduce real rates of interest)? Or is it a nominal scaling-up of all values

in the system, so that one's prices should be marked up proportionally? Uncertainty on this score could produce stickiness of nominal prices in the face of monetary expansion.

The relationship between money and credit in the business cycle, however, is a big and difficult topic which cannot be pursued farther here.

IV. Equilibrium or Disequilibrium Theory?

One last question: Suppose the conjecture is right that the Keynesian "real" disturbance hypothesis is due to be readmitted on at least a coequal basis with the Monetarist "nominal" shock hypothesis. Would this have any implications for current squabbles concerning the merits of equilibrium vs. those of disequilibrium approaches to business cycle theory? I don't pretend to have a firm answer to this question. But it ought not to be evaded altogether.

Monetarist theorists have, on the whole, rested content with the equilibrium method. In the older, Friedman version, monetary disturbances may cause temporary deviations from the natural rate of unemployment. In the newer, Lucas version, changes in unemployment are interpreted as movements between temporary equilibria. In either one, the coordination of economic activities is taken on faith. In Keynesian economics, it is problematic.²³ The problem, moreover, is essential in the Keynesian view of business fluctuations -- an integral, not an optional part of the inquiry.

The most heated discussion of the issue has centered on the "clearing" of labor markets. The "New Classicists" have made the (telling) point that fix-

²³In cruder versions of Keynesianism, admittedly, coordination of activities is not problematic either -- just impossible. An article of a more pessimistic faith.

price assumptions imply that agents allow perceived gains from trade to go unexploited; if, in contrast, one assumes all perceived gains to be exhausted, the implication is that the labor market "clears". At this point, the discussion easily gets derailed into unproductive arguments about the "voluntary" or "involuntary" nature of unemployment. That thicket had better be avoided on this occasion.

"Speculative" pricing, based on temporarily given information sets, seems to me also preferable to more or less arbitrary fix-price constraints as the theoretical rationale for short-run wage "stickiness". So far, so good. But the matter does not end at this point. Price flexibility in this qualified sense does not by itself guarantee that the time-path of the economy will be a sequence of temporary Walrasian equilibria. A Keynesian theorist would proceed to consider how the trades actually realized at the speculatively set prices this period might affect the feasible set of trades for next period. In the standard example, reservation wages are set too high so that labor's realized income is reduced with further consequences for consumption. And so forth.

What eliminates such income-constrained state-sequences from the New Classical theory is not just the assumption that fix-price constraints are absent. The assumption that all agents have the same information sets is just as crucial. If all agents in a market receive the same news and evaluate it using the same theory, they will all agree on what change in price is indicated. The volume of transactions will not be affected by disagreements among transactors. Taking the old Keynesian example again, if the news causes revisions in the demand-price and supply-price of labor of equal magnitude, neither side will be surprised by, or disappointed in, the volume of transactions realized.

It is not easy to come up with a context in which the assumption of universally shared information sets seems more reasonable than in the monetarist case of purely nominal shocks. Even so, it eliminates the Friedmanian temporary deviations from the natural rate of unemployment. These departures from equilibrium come about because firms learn of changes in the inflation rate before workers do, so that there is a transitory information assymetry between the two sides of the market. Once workers catch on, the information assymetry vanishes, and employment returns to the natural rate. The New Classical assumption does not allow the assymetry to develop.

The labor market focus of the debate over the new equilibrium business cycle theory has been unfortunate. Preoccupation with the stickiness of money wages comes naturally to monetarists: if you believe that all aggregative shocks are purely nominal, the failure of nominal prices to adjust appropriately must be the key to the explanation of unemployment. Keynesians do not believe that exogenous nominal shocks are the only disturbances, or even typically the disturbances, to worry about. Traditionally, they have been concerned about the intertemporal coordination of saving and investment decisions. In the latest act of the solemn, farcical muddle that is modern macroeconomics, we have been treated to a spectacular bout in unemployment theory featuring, in one corner, New Classicists blaming the failure of nominal interest rates to adjust to changes in money and, in the other, Old Keynesians blaming the rigidity of nominal wages in the face of changes in real intertemporal opportunities.

The Keynesian case for a disequilibrium approach is best considered in an intertemporal context. The real rate of return on investment is not just some given constant. We have to presuppose that political events and technological developments, for instance, can change the real returns in prospect for broad

sectors of the economy. A change in the returns perceived to be in prospect, in the unfashionable terminology of Keynes, causes a change in the marginal efficiency of capital (MEC). Such changes are real shocks -- "real" in the sense that system adjustments to them require changes in the allocation of resources and in the relative price vector and not just some scaling up or down of nominal values. We now have (at least) the following possibilities:

A) There is nothing in all this for macroeconomists to talk about. Real disturbances cause reallocations of resources between sectors but no movements in macroeconomic aggregates -- unless monetary shocks are also involved. In the latter case, the macroeconomic effects are all attributable to the latter.

B) A real equilibrium business cycle: utilization of both labor force and manufacturing capacity fluctuates but does so optimally in response to correctly perceived changes in the real rate of return. The labor market is continuously in equilibrium. People choose to work more when the real rate on savings is high, ceteris paribus -- intertemporal substitution a 'la Lucas. In this case, we assume that everybody has the same information and the same theory. They act, therefore, on the basis of mutually consistent beliefs²⁴ and these beliefs are correct. The rate of capital accumulation fluctuates but saving equals investment (ex ante) throughout.

C) A real business cycle that is a sequence of temporary equilibria. Mutually consistent beliefs again, but we allow for the possibility that what everyone believes will still be wrong. Again, saving equals investment in every period. When expectations are found to have been inaccurate, both

²⁴No great matter of principle hinges upon it, but I happen to prefer this terminology (i.e., consistent or inconsistent beliefs). Cf. my "The Wicksell Connection: Variations on a Theme," in Information and Coordination, op. cit.

savers and investors revise their expectations to the same extent and at the same time ("between innings").

D) A Wicksell-Keynes disequilibrium cycle wherein the market real rate of interest fails to coordinate saving and investment decisions appropriately. In cyclical expansions, investment tends to exceed saving (market rate below natural rate); in contractions, these inequalities are reversed. There are any number of variations on this theme.²⁵ All have in common the assumption that the expectations of entrepreneurs taken collectively are inconsistent with those held in the financial markets.

Of these four possibilities, (A) is the only one that is fully consistent with the New Classical economics. New Classical theory is made up, however, from Monetarist theory and rational expectations method. (B) and (C) are non-Monetarist, but are clearly compatible with the rational expectations equilibrium approach to modelling. (B) is probably the most appropriate benchmark for discussion of "real" cycle theories.²⁶ A formal version of (C)

²⁵The collection of variations discussed in "The Wicksell Connection," op. cit., is not by any means complete, but should suffice to try the patience of all but dedicated antiquarians with the general idea. Note also that the broad-brush taxonomy painted in the text will be judged incomplete by (E) Cambridge Keynesians who do not believe the interest rate can equilibrate saving and investment; (F) by Old-Time Keynesians who do not think the equilibrium exists; and (G) by post-Keynesians who do not think the equilibrium can be defined. We have already passed over the Modern-Muddled Keynesians who think Keynesian theory has nothing to do with saving and investment but has sticky wages as its analytical fulcrum. There are more kinds of Keynesians than one can shake a stick at! Believe me — I've tried.

²⁶My "Wicksell Connection" uses (A) as the benchmark. It would have been more relevant to current debates if I had used (B).

would be a real counterpart to Lucas' monetarist equilibrium cycle model.²⁷
The problem comes down to the various versions of (D).

Whether (D) can be modelled using a rational expectations equilibrium approach depends upon how strigently the latter is defined. Phelps' "islands" parable, formalized by Lucas in a paper that has been central to the entire rational expectations development, should be adaptable to the representation of such Keynesian processes.²⁸ Each producer is his own island in regard to his expectations of future profit from present investment. There are no archipelago-wide (futures) markets to ensure the consistency of these expectations with the plans of consumers and with aggregate on-going investment. If the MEC on the home island falls below what the producer thinks is obtainable elsewhere, he will cut back on investment and pile up his retained earnings in liquid form, etc. From there the Keynesian story develops as usual.²⁹

If, however, by "equilibrium approach" we mean modelling the economy "as if" it behaved like an Arrow-Debreu contingency market general equilibrium system,³⁰ it is clear that Keynesian processes must fall outside its purview. An Arrow-Debreu economy works like a "clockwork" going through the

²⁷Robert E. Lucas, Jr., "An Equilibrium Model of the Business Cycle," Journal of Political Economy, December 1979, reprinted in his Studies in Business-Cycle Theory, Cambridge, Massachusetts, 1981.

²⁸E. S. Phelps, "Introduction," in E. S. Phelps, et al., Microeconomic Foundations of Employment and Inflation Theory, New York, 1970, pp. 6-7. R. E. Lucas, Jr., "Expectations and the Neutrality of Money," Journal of Political Economy, April 1972.

²⁹Cf., Leijonhufvud, op. cit., pp. 197-99.

³⁰This is the position taken by Lucas more recently in his "Methods and Problems in Business Cycle Theory," Journal of Money, Credit, and Banking, November 1980, Part 2, also repr. in his Studies in Business-cycle Theory, op. cit.

markovian motions of a system in which all allocation decisions were made and reconciled at the beginning of time. Agents may have trouble predicting states of "nature,"³¹ but they have no trouble with each other. Coordination of activities, given the state of nature, is not a problem. All agents have the same information on the probability distributions for future states of nature. There is no room, in this framework, for the inconsistencies of belief or expectation that are essential in Keynesian theory.

It is, perhaps, necessary in conclusion to insist that the incompatibility of the rational expectations equilibrium method and the Keynesian hypothesis is totally irrelevant to the scientific appraisal of the latter. It merely indicates the limitations that the unanimity of beliefs postulate builds into the method.³²

³¹Monetarist cycle theory can be cast in this frame only, it would appear, by classifying central bankers as "ravages of nature" rather than as open market "traders".

³²The argument that the agents will act so as to exhaust apparent gains from trade that the New Classicists have used against fix-price modellers is simply irrelevant in the intertemporal context chosen here as the appropriate one for discussing the Keynesian case. With incomplete intertemporal markets the interactions required to generate information about, and exploit, these potential gains from trade do not take place.