Real and Monetary Factors In Business Fluctuations

Axel Leijonhufvud

Introduction

Professor Yeager is a major contributor to contemporary monetary economics. With the present paper (Yeager 1986), he has given us a comprehensive statement of his views on a broad range of major issues in this field. It is, moreover, not a cautious, hedged statement but a forceful, bold, and often blunt one. He deals with three "monetary" theories of macroeconomic fluctuations while leaving "real" theories out of the discussion. In the contention between the three monetary theories, moreover, his main purpose is to reassert the claims of "monetary disequilibrium" theory over those of its two rivals, Austrian business cycle theory and New Classical theory. The term "monetary disequilibrium" theory is borrowed from Clark Warburton. It refers to orthodox monetarism à la Friedman, or Brunner and Meltzer. Yeager prefers the label not only, I think, to give Warburton his due and to emphasize the older lineage of the theory, but also to draw a sharp demarcation between it and the "monetarist equilibrium" models of the New Classical group.

In order to move on to the points that I want to discuss let me first indicate in very general terms where I stand. First, I do not believe that all past "cycles" have been caused by the same impulse, whether real or monetary. (This, moreover, is not the only difficulty I see with the notion that cycles are "repetitive occurrences" of the same phenomenon.) Second, I believe that "real" cycle hypotheses are being far too cavalierly dismissed nowadays. Third, the hypothesis that real cycles do occur helps explain how monetary cycles can occur, for without the former the real propagation of nominal impulses becomes

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The author is Professor of Economics at the University of California, Los Angeles.
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difficult to understand. Fourth, the theoretical debate is bedeviled by an ambiguity in what may be meant by "monetary impulse."

In complaining about the dismissal of "real" theories, I am less concerned about the most recently advanced hypotheses of this description—King and Plosser et al. can fend for themselves—than I am about the old one, that is, the Keynesian one. In the macroeconomic discussion of recent years, it seems to me, Keynesian theory has become the "Phantom of the Opera"—hovering around somewhere in the wings, face contorted (one imagines) by irrational expectations, accused of all manner of murderous misdeeds, but no longer allowed a role on stage. Leaving Keynesian economics out of account is a bad mistake in my opinion, although in so saying it is not the routinely vilified straw man of Keynesian theory that I want to put back in a starring role (that "bastard"—the term is not mine—always played badly).

Monetary Disturbances and Price Rigidity

Yeager's discussion is, I think, particularly good and insightful on two related matters. One is the proposition that, in recession, the generalized excess supply of goods must have as its counterpart an excess demand for money. This is a central proposition in the field of business cycle theory, the ancestry of which, Yeager shows, goes back at least to Hume and Christiernin. The other is the "logic of price stickiness," a subject with an equally honorable pre-Keynesian ancestry.

What Yeager has to say on these two matters is in every essential respect (although not in every particular) what I have taught to UCLA students since the mid-1960s—presenting it, however, very often in the context of Keynesian theory. A reader of Yeager's paper might easily, I think, come away with the impression that these two pieces of macroanalysis belong, if not exclusively to his monetary disequilibrium theory, then to the wider class of monetary business cycle theories. It is important to realize that this is not at all so.

The proposition that a decline in nominal income is an adjustment to an excess demand for money does not presume that this excess demand for money has in turn been caused by an exogenous decline (or deceleration) of the money stock. It does not presume orthodox monetarist causation. The alternative hypotheses are, of course, that some real impulse has led either to an increase in the amount of money demanded in relation to income, or to an endogenous contraction of the banking system (that is, to a reduction in the money supply). Both hypotheses figure in the account I would give of a "Keynesian" recession.

Let me reiterate at this point that I am not committed to any "single cause" theory of business fluctuations and do not look at real impulse and nominal impulse theories as mutually exclusive. I thoroughly agree with Yeager when he says that "Many episodes of association between changes in money and in business conditions defy being talked away with the 'reverse causation' argument, that is, the contention that monetary changes were mere passive responses to business fluctuations of nonmonetary origin." But unlike him, my concern with reverse causation does not end there. I think it remains important, even if the argument has been misused.

On the logic of price stickiness, Yeager stresses first that it is difficult for transactors to diagnose a generalized excess demand for money. (In this context, he makes an extremely interesting point about easy-to-diagnose coin shortages to which I return later.) But in an orthodox monetarist model that should not be so. The money demand function is stable. Changes in the money stock are presumed uncontaminated by "reverse causation" and can thus be attributed to exogenous supply factors. As long as the money stock is public information, the sign and indeed size of the excess demand for money should be perfectly easy to diagnose. (The point is well known, of course, having long since become the conventional objection to first-generation Lucasian models.)

Even if the excess demand for money is generally perceived, Yeager adds, prices are still likely to be sticky because no one may want "to move first." But in a monetarist world where prices should be proportional to the money stock, everyone would know how the new equilibrium price differs from the old price. Obviously, it is possible to lose some money by cutting prices ahead of the pack. What is absolutely certain, however, is that lagging behind the pack is disastrous. In this monetarist context, therefore, we cannot lean very heavily on the conjectural problem, although it would be unwise to dismiss it altogether (compare also Phelps 1983). If it caused a great deal of friction in the system's adjustment to nominal shocks, so that people found themselves going through large, undesirable fluctuations in activity over and over again for this reason, one might suppose that they would organize cooperative solutions to the "who's first" problem. In a hypothetical monetarist world that knows no real-impulse cycles, a particularly simple such solution is obviously available (Eden 1979): index-link all prices to the quantity of money!

Real Impulse Hypotheses

Consider, then, the class of real impulse hypotheses. The Keynesian member of the class starts with a change in the "marginal efficiency of capital," that is, a change in the perceived profitability of using present resources to augment future output. It is not altogether clear why this hypothesis, which was accepted almost without question for some decades, has fallen so completely out of favor, for the explicit arguments against it are neither novel nor convincing. Among them are the following: (1) the real impulse hypothesis leaves the positive money-income correlation unexplained; (2) if there were such a thing as a real aggregative impulse, it should show up as an inverse correlation between money prices and output; (3) reasons are lacking for supposing real disturbances on different sectors of the economy to be correlated, so the notion of aggregative real impulses is itself suspect; (4) even if occasionally real impulses were preponderantly of one sign, the resources required for some sectors to expand would have to be bid away from others, which would therefore contract. These, of course, are examples not just of pre-Keynesian but of pre-Mitchellian reasoning. (I do not intend attributing any of them to Professor Yeager.)

To meet these objections, one must recognize both that the money supply varies endogenously and that the level of activity in the system depends (even in equilibrium economics) on the real rate of return on investment. Take the latter idea first. If the perceived value-productivity of present inputs in terms of future outputs increases, while that in terms of present outputs is unchanged, it will pay to expand employment. (This, after all, is how we would explain why farmers work harder in the planting season, for instance.) The sectors first affected may expand, therefore, without forcing corresponding contractions elsewhere. The increase in output is financed by producers getting trade-credit from their suppliers and bank-credit for their increased wage-bills. Thus rising investment and employment are accompanied by an endogenous increase in the money stock.

In order for the economy not to overshoot the equilibrium adjustment to the improved intertemporal prospects in a couple of its sectors, the real rate of interest should rise to its new "natural" level. Now, what *that* level may be is difficult to diagnose! As Keynes stressed, moreover, it is not clear that securities markets participants have a strong incentive to try to figure out what real rate of interest would equate aggregate saving and investment at full employment (the level of which also depends on the interest rate), for profits are made from anticipating what is in fact going to happen and not what should happen in the best of all possible worlds. "Efficient markets," therefore, do not assure us of the right outcome. To illustrate overshooting, consider the sufficient but not necessary condition that the central bank stabilizes interest rates by giving the banking system free rein to rediscount at the old interest rate. In this case, the sectors that should expand will expand too much and will gradually begin to pull their suppliers into the expansion; consumption spending will then increase and the expansion becomes general. To make sense of Keynesian economics for ordinary business cycle purposes, one should, I think, picture this gradual spreading of the expansionary impulse as the process behind the textbook phrase "an outward shift of the marginal efficiency of capital." Certain political events, for instance, may be representable as shocks that impinge directly on the investment expectations of most sectors of the economy at the same time, but such aggregative real impulses should not be the general case.

The point about this real impulse case is the following. In the process analyzed, the money stock covaries with income for endogenous ("reverse causation") reasons, and employment covaries with money income for reasons that, to begin with at least, have nothing to do with the stickiness of money wages (but a great deal to do with the stickiness of intertemporal relative prices, that is, the interest rate). Monetary disequilibrium, as described by Yeager, is central also to this story so, in some sense, the theory still qualifies as a "monetary" cycle theory although it assumes an initial real impulse. In particular, it is possible that we might reduce such fluctuations greatly by forcing the central bank to quit stabilizing interest rates and to try instead to impose a Friedman M2–rule on the banking system. (It is also possible, however, that a policy that went far enough in this direction to succeed would also make the real supply of credit in the system so inelastic as to prevent the exploitation of many Schumpeterian growth-opportunities.)

Real versus Nominal Impulses

Suppose, for the sake of argument, that we were to conclude that all aggregative cycles were "monetary" in the sense that they would disappear if a Friedman rule could be imposed on the system. It would still be necessary to distinguish clearly between the real and the nominal impulse cases in order not to be trapped in the ambiguities of this usage of "monetary." In the orthodox monetarist case, changes in the money stock are modeled as if they were purely nominal supply impulses in a fiat standard system: in recession, the money supply is too small in relation to the price level; in boom, too large. The appropriate adjustment is to change the price level so as to obtain the desired, constant real money supply. In the Keynesian reverse causation case, however, the nominal money stock varies to satisfy changing real money demand when output and employment respond to real impulses. In this case, *watching the changes in the money stock will give basically no clue as to how to set money prices.* Any agent following the rule of setting his prices proportional to the money supply would lose all his customers in the upswing and sell out all his stock below replacement cost in recession. *It is in a system where fluctuations of this sort are commonplace that nominal impulses can have major real effects.* From where I sit, we need Keynes to save Friedman from Lucas!

Even so, transactors will not be completely helpless in gradually sorting out what kind of impulse predominates at any one time. Thus, if we could compare the effects of the two types of impulses (for, say, equal changes in money income), we should expect nominal impulses to show large price and small output changes and real impulses of the Keynesian kind to show large output and small price level changes. The short-run Phillips trade-off, in other words, is not the same for "LM-shifts" as for "IS-shifts." This is one reason for not committing oneself to a single impulse hypothesis for all cycles: it does not explain why fluctuations before and after the breakdown of Bretton Woods seem different in this respect. My inference is that real impulses (with endogenous money) predominated until the mid-1960s and that, while real impulses are still intermingled later, nominal ones predominate.

What Keynes Really Meant

There are two points from Yeager's discussion of monetary disequilibrium that I would like to take up separately. One is a matter of putting the record straight in my own (somewhat belated) defense. Yeager strengthens the impression that his analytical insights into the necessarily monetary aspect of aggregative disequilibrium and the logic of price stickiness belong to *his* tradition and not also to the Keynesian tradition when he says: "Robert Clower and Axel Leijonhufvud rediscovered it, *questionably suggesting* that it was what Keynes really meant in the *General Theory*" (italics added). He refers to a 1973 article of his own in which his charge that we had misread Keynes was somewhat counterbalanced by the generous suggestion that we should get the credit for contributing the original ideas that we attributed to Keynes. By coincidence, my co-discussant, Herschel

Grossman, raised similar questions about my interpretation of Keynes at about the same time (1972), concluding that while indirectly "Keynes helped set the stage for development of the new paradigm . . . focusing upon the interrelation of markets which fail to clear," nonetheless "[t]he most plausible *answer surely is that Keynes did not have in mind anything resembling Clower's interpretation* of the consumption function" (italics added).

Now, although "what Keynes really meant" is not at all as good and useful a question as, for instance, "could macroeconomics have evolved along a more fruitful path from the *General Theory*," it so happens that on these particular points we now do know precisely what he meant. Volume 29 of Keynes's *Collected Papers,* which appeared only in 1979, contains outlines and drafts of introductory chapters (pp. 63–102) that Keynes eventually discarded in favor of his brief and cryptic chapter 2. This material leaves absolutely no doubt whatsoever that the conceptual experiment of Keynes's analysis was exactly that which Clower and I have attributed to him.

Cooperative Solutions

The second point concerns Yeager's comment that, in the case of coin shortages, which are easier to diagnose than a general excess demand for money, people manage to find cooperative solutions that avoid propelling the economy into deflation or recession. Let me point to an even more pertinent case, namely, that of the Irish Bank strikes, the longest of which shut the banks for over six months and created a much more dramatic "shortage" of transactions media, since transfers of demand and time deposits could not be executed for the duration. The Irish found cooperative solutions also for this situation, and the effect of the general excess demand for money was a rise in transactions costs rather than a Great Depression (Murphy 1978).

The closing of the Irish banks was obviously easy to diagnose. But the point, surely, is that in the coin shortage and bank strike cases the diagnosis does not only tell us that means of payment will be in excess demand but also that people's ability to carry out their contractual obligations and to enter into new commitments is basically unaffected by whatever events brought this excess demand about. It is this, not just the evident fact of money being in excess demand, that makes people willing—up to a point—to go for the cooperative solution.

I have already made the point that in an orthodox monetarist model where changes in the money stock can be presumed uncontaminated by "reverse causation," the excess demand for money should not be difficult to diagnose. Suppose now that we have a system such as this theory assumes and that the government reduces the stock of money. Everybody knows about it. Will people react as if to a coin shortage or will they cut prices? If the excess demand for money were generally perceived as transitory, it would seem possible that people would tide themselves over with various cooperative transactions practices without either recession or deflation. If, however, it is believed to be permanent—if the government is thought to be bent on deflation—then it is no longer the case that people's ability to honor or undertake commitments is going to be unaffected. The new equilibrium, sooner or later, is going to be at a lower price level and the deflation that takes the economy there is going to redistribute wealth.

During the bank strikes, the Irish were able to get along for some time on the presumption that people were good for what they used to be good for, even though currently they might not be able to pay money. When a complex process of wealth redistribution is in train, it is not easy to know or inexpensive to learn who is a net gainer and who a net loser. The Irish presumption is then not safe. Instead of agreeing to suspend customary payment practices, people will want to insist on them being followed; keeping track of who is and who is not able to honor commitments is the very rationale for these practices. The excess demand for money will then have to work itself out through a reduction in money income.

This attempt to pursue Yeager's observation concerning coin shortages leads in a direction that, to my mind, is more Keynesian than monetarist. Cash constrained behavior is integral to Keynesian theory, as Clower and I have argued in the dispute just referred to, and the social rationale for cash constraints is therefore more apt to be a preoccupation of theorists with a Keynesian orientation. But monetary theory in general, and not only monetarist theory has had two glaring weaknesses: (1) its inability to explain whether it is the stock of coins, or M1, or M2, or some other aggregate that is the "True M" for quantity theory purposes; and (2) its failure to tell us when an excess demand for one "M" or another will lead to a small rise in transactions costs in the economy and when it will produce a Great Depression.

Austrian Business Cycle Theory

There is a bit of irony in the impatience with which Auburn's Ludwig von Mises Professor deals with Austrian business cycle theory (ABC) even if he professes to have the good of Austrian theory at heart in trying to rid it of this "embarrassing excrescence." Having also been overexposed to this theory, I tend to share Yeager's impatience, but our reasons for being critical are rather different.

Yeager argues that what is right and important in ABC is all contained in monetary disequilibrium theory and what is not so contained is either "mere details" or "unnecessarily specific." He suggests that monetarism, therefore, is superior in that it pays attention to Occam's razor. A friendlier critic might have praised ABC on the Popperian grounds of having more falsifiable content. Monetary disequilibrium theory tells us that in expansion, for example, we have an excess supply of money balancing a generalized excess demand for commodities. ABC adds predictions about the distribution of this commodity excess demand across the various markets.

My trouble with ABC is that its excess falsifiable content has been falsified. According to ABC, inflation should produce an overinvestment boom. The stagflation decade of the 1970s does not fit: it gave us inflation but no acceleration of capital accumulation and no forced saving. So one cannot accept it as a "General Theory" (if you will pardon the expression). Yet, I think there probably are historical situations that fit the theory. Consider, for instance, the historical circumstances surrounding its formulation. Austria in the 1920s had some industries built to the scale of the Austro-Hungarian empire that now faced the protectionist policies of the countries which had been their prewar markets. "Cheap credit" was an important instrument in the attempts to modernize these industries and make them competitive under the new conditions. Maintaining (rather than creating) "overinvestment" was in a sense the purpose of this policy. The eventual failure of the *Kreditanstalt* can be viewed as its appropriately Hayekian denouement.

Suppose for the sake of argument that my all-too-casual empiricism is roughly right and that ABC fits Austria in the 1920s but not the United States in the 1970s. What was the difference? Obviously, the monetary regimes were very different. After the end of its post-World War I hyperinflation, Austria was committed to the gold exchange standard. The maintenance of a fixed exchange rate constrained the domestic price level and made price expectations inelastic with respect to domestic monetary aggregates. Under these conditions, the expansion of the banking system meant an increase in the real volume of credit (and, eventually, in "really unsound" credit), and was associated with the distortion of relative prices and misallocation effects predicted by Austrian theory. The American inflation of the 1970s, in contrast, occurred in a pure fiat regime that put no convertibility obstacles in the way of a general increase in the nominal scale of all real magnitudes. If the inflation nonetheless failed to be neutral, this was mostly because of the uncertainty about its future course; with the uncertainty about future nominal values growing exponentially with distance from the present, this kind of fiat "random walk" inflation tends to discourage capital accumulation.

The "monetary impulse" in the second case is a purely *nominal* one. In the first, the expansion of the money supply (by some broad definition) is mainly a *credit* impulse. Economic theory does not predict a proportional change in the price level to be the equilibrating response in this case. Discussion between monetarists and Austrians (what there has been of it) has clearly been impeded by the desire on each side to claim general validity for its theory. Lack of clarity concerning the meaning of "monetary impulse" may have been a contributing factor.

Assessing the New Classical School

Yeager also takes on the New Classical school. I have been groping my way toward an assessment of the challenges and contributions of this group in several recent papers, some of them quite lengthy (for example, Leijonhufvud 1983). To compare opinions with Yeager also on this large subject would take me too far. When it first emerged and was still relatively homogenous in outlook, the New Classical group could be identified by three doctrines: monetarism, rational expectations, and continuous market clearing. Yeager accepts the first, says very little about the second ("probably useful in many applications"), and blasts the third with everything he's got.

With regard to the first, I find the exclusive preoccupation with purely nominal shocks of the early New Classical literature misconceived. On the second, I believe rational expectations to be the right equilibrium concept for macroeconomics. Since I have a historically episodic view of business fluctuations and doubt that they can be regarded as repetitive instances of the same event, I find the step from the general rational expectations assumption to the specific assumptions about the information sets of agents very problematic. How much one may sensibly assume economic agents to know and to understand in a specific analytical context remains a question that often cannot be settled by recipe. On the third, I tend, like Yeager, to revolt against the changed usage that defines "equilibrium" so as to append a methodological prohibition against "disequilibrium" analysis. (Is not the term itself superfluous if there are no other kinds of states?) That said, however, I am waiting to see how much of the substance of what I have called disequilibrium economics will end up being covered by the equilibrium economics of the New Classicals.

The issue, I agree with Yeager, is whether the new equilibrium economics will allow us to study the coordination of economic activities as a genuine problem. Yeager feels that an "equilibrium-always" economics precludes such study. But it is not obvious that that is so. The solution states, all of which the New Classicals call equilibria, are conditional on the information possessed by transactors. What Yeager and I would call an "equilibrating" process, for instance, can be represented as a sequence of such New Classical equilibria in which agents continually update their information sets by watching the outcome of market interactions. This is an example of a class of collective learning processes, which has traditionally and for good reasons been regarded as central to the study of economic coordination problems. The issue is whether New Classical economics is going to include or exclude the study of such learning processes. If learning by market feedback is excluded, the school has barred itself on methodological grounds from the study of an important substantive problem, and the rest of us will just have to carry on as best we might without them. If it is included, fine, but then the New Classicals will, I think, have saddled themselves with some "free parameters" after all, because the speed of learning, especially about the implications of nonrecurrent events, is hardly amenable to choice theory.

Yeager also expresses some exasperation over the emphasis on technical virtuosity that has been associated with the growing influence of this school. While I greatly admire some of the papers that set this trend, I too am frequently exasperated. Perhaps it is just the Hollywood outlook of someone who has been too long at UCLA, but it sometimes seemed to me in the 1970s that macroeconomics was going the same way as the movies: the story-lines were getting simple-minded, but the special effects ever more stupendous!

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