

## THE SIGNIFICANCE OF MONETARY DISEQUILIBRIUM

*Leland B. Yeager*

### Rival Theories

Among theories of macroeconomic fluctuations that accord a major role to money, at least three rivals confront each other nowadays. One is orthodox monetarism—"the monetary disequilibrium hypothesis," as Clark Warburton has called it (1966, selection 1, and elsewhere). A second is the so-called Austrian theory of the business cycle. A third builds on notions of rational expectations and equilibrium always. What monetarism offers toward understanding and perhaps improving the world becomes clearer when one compares it with its rivals.

### Monetary Disequilibrium Theory

Fundamentally, behind the veil of money, people specialize in producing particular goods and services to exchange them for the specialized outputs of other people. Any particular output thus constitutes demand, either at once or eventually, for other (noncompeting) outputs. Since supply constitutes demand in that sense, any apparent problem of general deficiency of demand traces to impediments to exchange, which discourage producing goods to be exchanged. The impediment that most readily comes to mind hinges on the fact that goods exchange for each other not directly but through the intermediary of money or of claims to be settled in money.

As Warburton has argued (e.g., 1966, selection 1, esp. pp. 26-27), a tendency toward market-clearing inheres in the logic of market

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processes. Whenever, therefore, markets are generally and conspicuously failing to clear—when disorder is more pervasive than gluts or shortages of only particular goods or services—some exogenous disturbance must have occurred, one extensive enough to resist quick, automatic correction. It is hard to imagine what that pervasive disruption could be other than a discrepancy between actual and desired holdings of money at the prevailing price level. (It is unnecessary to worry here about just how to define “money.” A supply-demand disequilibrium for money broadly defined is very likely to entail disequilibrium in the same direction for money narrowly defined also. Financial innovations may well complicate the task of avoiding imbalance between money’s supply and demand, but that complication for policymakers is distinct from the question of diagnosis.)

A discrepancy between supply and demand is likely to develop, Warburton argued, when growth of the money supply falls short of the long-run trend. Actual shrinkage poses the simplest case. People and organizations try to conserve or replenish their shrunken money holdings by restraint in buying and greater efforts to sell goods and services and securities (Wicksell [1898] 1936, p. 40).

Since transactions are voluntary, the shorter of the demand side and the supply side sets the actual volume of transactions on each market. Transactions and production fall off, unless prices and wages promptly absorb the whole impact of the monetary disturbance. Typically they do not. Production cutbacks in response to reduced sales in some sectors of the economy spell reduced real buying power for the outputs of other sectors. Transactions in ultimate factors of production and in final consumer goods and services are far outnumbered by interfirm transactions in intermediate goods—materials, parts, equipment, structures, items traded at wholesale, and the like—and this circumstance magnifies the scope for damage from shrinkage of the routine flow of the monetary lubricant. Financial intermediation and trade in financial instruments are similarly vulnerable (Bernanke 1983).

When money is in short supply at the existing nominal price and wage level, why won’t people collaborate to economize on money and so keep their transactions, production, and employment going anyway? People do collaborate to economize on coins when they are in short supply. George Akerlof (1975) and Alan Blinder and Joseph Stiglitz (1983, pp. 299–300) suggest that the two cases offer similar incentives for collaboration. Yet they are quite different. A shortage specifically of coins is easy to recognize, and collaboration in economizing on coins works not only in the general interest but also in one’s evident personal interest (to facilitate specific transactions and

to earn goodwill). An overall shortage of money is much harder for individuals to diagnose and to palliate cooperatively in individual transactions.

The rot can snowball, especially if people react to deteriorating business and worsening uncertainty by trying to hold more money relative to other assets and to income and expenditure—if velocity falls, as it typically does in such situations. In depression or recession, what would be an excess demand for money at full employment is being suppressed by people's being too poor to "afford" more than their actual money holdings. Relief of this (suppressed) excess demand for money somehow or other—perhaps by an increase in the nominal money supply, perhaps by growth in real money balances through wage and price cuts—would bring recovery. An excess supply of money, at the other extreme, brings price inflation. The theory extends readily to deal both with stagflation and with the adverse side-effects of monetary policy to stop inflation, since an analogy holds between the stickiness of a price and wage level and the momentum of an entrenched uptrend (Yeager and associates 1981).

This doctrine, or key strands of it, goes back at least to David Hume ([1752] 1970) and sometimes was the dominant view in macroeconomics. It flourished in the United States in the early decades of the 20th century, as Warburton has reminded us (1981 and an unpublished book-length manuscript). W. H. Hutt (1963, 1974, 1979) has long expounded something similar in his own idiosyncratic terminology. Robert Clower (1965, 1967) and Axel Leijonhufvud (1968) rediscovered it, questionably (Grossman 1972; Yeager 1973) suggesting that it was what Keynes really meant in the *General Theory*. Robert Barro and Herschel Grossman (1971, 1976) developed some of its theoretical aspects. The doctrine accords well with the statistical evidence of Warburton and Milton Friedman and other monetarists.

It also accords well with narrative history. 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 the monetary changes were mere passive responses to business fluctuations of nonmonetary origin. Warburton (1962) and Friedman and Schwartz (1963) have assembled episodes from American history.

Episodes appear even in fairly exotic times and places. In several American colonies in the early 18th century (that is, even before Hume wrote), issues of new paper money apparently had their intended effect in relieving a "decay of trade" (Lester [1939] 1970, chaps. 3–5). Writing in Sweden at a time of irredeemable paper money, P. N.

Christiernin ([1761] 1971) observed that “Reduction in the circulating money supply chokes prosperity” (italicized, p. 86); and he went on to amplify that observation. Anticipating Irving Fisher, Christiernin even warned about the interaction between deflation and existing debts (pp. 91–94). From 1863 through 1865, efforts to deflate the Austrian paper gulden back to its silver parity produced a depression lasting until the Seven Weeks’ War of 1866. In the judgment of two modern Austrian economists, the war-related paper-money issues then served as a “deliverance for the entire economy” from the deflation and contributed to the “greatest boom in Austrian history.” “The experience gained from the severe economic depression in the wake of [Finance Minister] *Plener’s* contractionary measures and from the economic upswing after the expansion of the circulation in the year 1866 confirmed in increasing degree . . . the recognition of a far-reaching connection between the monetary system and the development of business conditions.”<sup>1</sup> The association between monetary and business conditions in Tsarist Russia is recognized by Haim Barkai (1969), P. A. Khromov (1950, pp. 293–94), A. F. Jakovlev (1955, pp. 388–89), and A. Shipov (1860, pp. 33–34, quoted in S. G. Strumilin 1960, p. 479) and is borne out by available statistics. Relative resistance to depression in the early 1930s by fiat-money Spain and silver-standard China and China’s subsequent suffering under the U.S. silver-purchase program illustrate monetarist theory. So do the consequences of deflation of the stock of cigarette money in a prisoner-of-war camp (Radford 1945). These episodes are cited merely as evidence bearing on a theory, not as arguments for populist monetary expansionism.

### Early Recognition of Price Stickiness

Since assuming—or recognizing—wage and price stickiness is now widely viewed as a *distinctively Keynesian trait in macro theory* (a view discussed further below), we should remember that even early monetarists invoked it. David Hume ([1752] 1970, esp. pp. 39–40) explained that monetary expansion can stimulate production only during a transition period, before prices have risen fully; and, though less clearly, he saw the corresponding point about monetary contraction. “It is easy for prices to adjust upward when the money supply increases,” observed Christiernin ([1761] 1971, p. 90), “but to get prices to fall has always been more difficult. No one reduces the price of his commodities or his labor until the lack of sales necessitates

<sup>1</sup>Quotations are from Alois Gratz, p. 254, and Reinhard Kamitz, p. 147, in their articles in Mayer (1949).

him to do so. Because of this the workers must suffer want and the industriousness of wage earners must stop before the established market price can be reduced." Henry Thornton ([1802] 1978, pp. 119–20) was also quite explicit and even noted that wages tend to adjust downward more stickily than prices.

G. Poulett Scrope (1833, pp. 214–15), under the heading "General Glut of Goods—Supposes a General Want of Money," explained that "epochs of general embarrassment and distress among the productive classes, accompanied . . . by a general glut or apparent excess of all goods in every market . . . are . . . occasioned by the force of some artificial disturbing cause or other," namely money. "[A] general glut—that is, a general fall in the prices of the mass of commodities below their producing cost—is tantamount to a rise in the general exchangeable value of money; and is a proof, not of an excessive supply of goods, but of a deficient supply of money, against which the goods have to be exchanged."

Like many other diagnosticians of disequilibrium, Scrope did not distinguish as clearly as we might wish between excessive monetary expansion or contraction, on the one hand, and general price increases or decreases on the other hand—price changes which, along with changes in quantities traded and produced, are symptoms or consequences of the monetary disturbance. These price changes *tend* to correct or forestall the monetary disequilibrium but do not and cannot occur promptly and completely enough to absorb the entire impact of the monetary change and so avoid quantity changes. By clear implication, though, Scrope does recognize the stickiness of at least those prices entering into the "producing cost" of commodities.

It was not a hallmark of classical and neoclassical economics to believe that markets always clear or that automatic market-clearing forces always quickly overpower disturbances to equilibrium. When concerned, as they usually were, with the long-run equilibrium toward which fundamental forces were driving patterns of prices and resource allocation, classical and neoclassical writers (including Ricardo, Mill, and Marshall) did abstract from the shorter-run phenomenon of monetary disequilibrium. But they recognized that such disequilibrium does occur and sometimes paid explicit attention to it (Warburton 1981 and unpublished manuscript).

Turning to early 20th-century America, we find H. J. Davenport (1913, pp. 319–20)<sup>2</sup> emphasizing the monetary nature of depression:

It remains difficult to find a market for products, simply because each producer is attempting a feat which must in the average be an

<sup>2</sup>See also Davenport (1913, pp. 291–305, 318).

impossibility—the selling of goods to others without a corresponding buying from others. . . . [T]he prevailing emphasis is upon money, not as intermediate for present purposes, but as a commodity to be kept. . . . [T]he psychology of the time stresses not the goods to be exchanged through the intermediary commodity, but the commodity itself. The halfway house becomes a house of stopping. . . . Or to put the case in still another way: the situation is one of withdrawal of a large part of the money supply at the existing level of prices; it is a change of the entire demand schedule of money against goods.

Davenport recognizes (p. 299) that the depression would be milder and shorter if prices could fall evenly all along the line. In reality, though, not all prices fall with equal speed. Wages fall only slowly and with painful struggle, and entrepreneurs may be caught in a cost-price squeeze. Existing nominal indebtedness also poses resistance to adjustment.

More generally, uneven changes in individual prices and wages amid a change in their general level, whether downward in depression or upward in inflation or stagflation, degrade the information conveyed by individual prices and in other ways add difficulties for trade and production. Nowadays, theories of “overshooting” of floating exchange rates invoke the stickiness of prices of goods and services.

### The Logic of Stickiness

In an elementary textbook already in its fifth edition in 1931 (pp. 104, 88–89), Harry Gunnison Brown explained why price reductions would not immediately absorb a contraction of money, credit, and spending. Producers, dealers, and workers do not easily see why they should accept reduced prices and wages; owners of land or buildings will not see why they should accept lower prices or rents. “[T]here are various customary notions of what are reasonable prices for various goods and reasonable wages for labor of various kinds and, furthermore, each person hopes to be able to get the old price or the old wage for what he has to sell and does not want to reduce until sure that his expenses will also be reduced.” People hesitate, holding off for standard prices, wages, and so on. The process of readjustment “may be one requiring several months or (sometimes) years, during which business is relatively inactive and ‘depression’ is said to continue.”

Brown was alluding to the who-goes-first problem. It is illegitimate to suppose that people somehow just know about monetary disequilibrium, know what pressures it is tending to exert for corrective adjustments in prices and wages generally, and promptly use this knowledge in their own pricing decisions. One cannot consistently

both suppose that the price system is a communication mechanism—a device for mobilizing and coordinating knowledge dispersed in millions of separate minds—and also suppose that people *already* have the knowledge that the system is working to convey. Businessmen do not have a quick and easy shortcut to the results of the market process. They do not have it even when the market's performance is badly impaired. Money-supply numbers are far from everything they need to know for their business decisions.

Even if an especially perceptive businessman did correctly diagnose a monetary disequilibrium and recognize what adjustments were required, what reason would he have to move first? By promptly cutting the price of his own product or service, he would be cutting its relative price, unless other people cut their prices and wages in at least the same proportion. How could he count on deep enough cuts in the prices of his inputs to spare him losses or increased losses at a reduced price of his own product? The same questions still apply even if monetary conditions and the required adjustments are widely understood. Each decisionmaker's price or wage actions still depend largely on the actual or expected actions of others. A businessman's difficulties in finding profitable customers or a worker's in finding a job are unlikely to trace wholly, and perhaps not even mainly, to his own pricing policy or wage demands.

Although this point is obvious, many people seem not to grasp its significance; so further emphasis is justified. Suppose that I and a teenage neighbor want to make a deal for him to mow my lawn. Somehow, however, lawnmowers and lawnmower rentals are priced prohibitively high. At no wage rate, then, could my neighbor and I strike an advantageous bargain. The obstacle is not one that either or both of us can remove, and our failing to remove it is no sign of irrationality. Similarly, whether a manufacturer can afford wage rates attractive to workers may well depend on land rents, interest rates, prices of materials and equipment and fuel and transport, prices charged by competitors, and prices entering into workers' cost of living.

The point of these examples is that attaining a market-clearing pattern of prices and wages is not simply a matter of bilateral negotiations between the two parties to each potential transaction. Comprehensive multilateral negotiations are infeasible or prohibitively costly; so groping towards a coordinated pattern of market-clearing prices must take place instead through decentralized, piecemeal, sequential, trial-and-error setting and revision of individual prices and wages.

The economy never reaches a state of full coordination. How close or how far away it is depends on how severe and how recent shocks have been in “wants, resources, and technology”—and monetary conditions. The impossibility of perpetual full coordination is no defect of the market system. It is an inevitable consequence, rather, of the circumstances with which any economic system must cope. One of the market system’s virtues is that it does not require or impose collective decisions. The dispersion of knowledge and the fact that certain kinds of knowledge can be used effectively only through decentralized decisions coordinated through markets and prices—rather than coordinated in some magically direct way—is one of the hard facts of reality. It forms part of the reason why monetary disturbances can be so pervasively disruptive: they overtax the knowledge-mobilizing and signaling processes of the market.

Interdependence among individual prices and wages appears in input-output tables. It appears in the attention given to production costs, the cost of living, and notions of fairness in price and wage setting. The holding of inventories (of materials and semifinished and finished products) and buildups and rundowns of inventories testify to the perceived rationality of waiting for further information rather than adjusting one’s price in response to every little change in customers’ demands.

Even in a depression, when it would be collectively rational to cut the general level of prices and wages and other costs enough to make the real money stock adequate for a full-employment volume of transactions, the individual agent may not find it rational to move first by cutting the particular price or wage for which he is responsible. He may rationally wait to see whether cuts by others, intensifying the competition he faces or reducing his production costs or his cost of living, will make it advantageous for him to *follow* with a cut of his own. The individually rational and the collectively rational may well diverge, as in the well-known example of the prisoners’ dilemma. Taking the lead in downward price and wage adjustments is in the nature of a public good, and private incentives to supply public goods are notoriously inadequate. (An analogous argument helps explain people’s reluctance to go first in breaking an entrenched uptrend in wages and prices as soon as inflationary monetary growth has been stopped.)

Because wages and prices are sticky, automatic market forces, working alone, correct a severe monetary disequilibrium only slowly and painfully. Extreme flexibility in money’s purchasing power not only is infeasible but would even be undesirable in several respects.

Many circumstances make stickiness reasonable from the standpoint of individual decisionmakers. (A theory does not deserve sneers for being eclectic if its eclecticism corresponds to reality.) The value of long-term customer-supplier and employer-worker relations and notions of implicit contract ("invisible handshake") enter into the explanation (Okun 1981). The workers foreclosed from a particular employment by too high a wage rate may well be only a minority of the candidates, victims of a seniority system or of bad breaks. The more senior or the luckier workers who remain employed are not acting against their own interest in refusing to accept wage adjustments toward a market-clearing level. For the employer, as well, the costs of obtaining and processing information may recommend judging what wage rates are appropriate by what other people are paying and receiving and by traditional differentials. If changed conditions make old rules of thumb no longer appropriate, it takes time for new rules to evolve. An employer may offer a wage higher than necessary to attract the desired number of workers so that he can screen ones of superior quality from an ample applicant pool. Considerations of morale are relevant to many jobs that involve providing informal training to one's less experienced fellow workers. Performance in this and other respects is hard to monitor, and workers may withhold it if they come to feel that they are being treated unfairly. For some goods and services as well as labor, actual or supposed correlations between price and quality may provide reasons for not relying on market-clearing by price alone (Stiglitz 1979).

More broadly, money's general purchasing power is sticky because individual prices and wages are interdependent. This interdependence is crucial to the who-goes-first problem (see also Cagan 1980, p. 829, and Schultze 1985). It intertwines with a banal but momentous fact: money, as the medium of exchange, unlike all other goods, lacks a price and a market of its own. No specific "money market" exists on which people acquire and dispose of money, nor does money have any specific price that straightforwardly comes under pressure to clear its (nonexistent) market. Money's value (strictly, the reciprocal of its value) is the average of individual prices and wages determined on myriads of distinct though interconnecting markets for individual goods and services. Adjustment of money's value has to occur through supply and demand changes on these individual markets, where these changes can affect not only prices but also quantities traded and produced. In particular, an excess demand for money will tend to deflate not only prices but also quantities—unless prices absorb the entire impact, which is unlikely for the reasons under discussion.

For nothing other than the medium of exchange—ranging from Old Masters to the nearest of near-moneys—could an excess demand be so pervasively disruptive. A nonmoney does not have a routine flow, lubricating exchanges of other things, to be disrupted in the first place. Efforts to hold more than its actual quantity cannot cause such pervasive trouble. Excess demand for a nonmoney hits its own specific market. The frustrated demand either (1) is curtailed by a rise in the thing's price (or fall in its yield) or (2) is satisfied by a response in its quantity or else (3) is diverted onto other things. No excess demand for a nonmoney can persist, unaccompanied by an excess demand for money, and yet show up as deficiency of demand for other things in general. For the medium of exchange, in contrast, excess demand is neither directly removed nor diverted. Instead, (4) the pressures of monetary disequilibrium are diffused over myriads of individual markets and prices, which renders its correction sluggish.

### Comparison with Rival Theories

We better appreciate monetary disequilibrium theory when we consider how it compares with rival theories and stands up under criticism by their adherents. Criticism from the camp of rational expectations and equilibrium-always is relatively explicit. First, though, we shall look at a rival doctrine whose criticism is rather vague, showing up as jabs at "Chicago" economics, at supposedly excessive aggregation, and at supposedly inadequate attention to the nonneutrality of monetary changes.

### The Austrian Theory of the Business Cycle

A particular theory cultivated by Ludwig von Mises and F. A. Hayek in the early 1930s is so widely expounded in speech and print by "Austrian" economists nowadays that I hardly know where to begin or end in giving citations (but see Ebeling 1978). Some economists may consider that theory too unfamiliar, outmoded, or preposterous to be worth any further attention. Still, I did not want to pass up my present opportunity to reason with its adherents. Their slant on economics has much to offer. I want to support modern Austrianism by helping rid it of an embarrassing excrescence.

Briefly, Austrian cycle theory attributes recession or depression to a preceding excessive expansion of money and credit. It does not flatly deny any possible role of their contraction during the depression; but it insists that misguided expansion has already, before the depression begins, caused the damage fated to follow. The theory, or a hard-core version of it, also suggests that resistance to contraction

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is then useless or even harmful. Depression must be dealt with early, by forestalling the unhealthy boom in which it originates.

Let us review the supposed process. Perhaps in response to political pressures for lower interest rates, the monetary authorities begin expanding bank reserves through their discount or open-market operations. Business firms find credit cheaper and more abundant. These signals suggest, incorrectly, that people have become more willing to save and so free resources for investment projects that will make greater consumption possible in the future. Accordingly, firms invest more ambitiously than before. In particular, they construct "higher-order" capital goods, goods relatively remote from the final consumer—machine-tool factories, for example, as opposed to retail stores and inventories of consumer goods. Relatively long times must elapse before resources invested in such goods ripen into goods and services for ultimate consumers. This large time element makes demands for higher-order goods relatively sensitive to interest rates. That is why credit expansion particularly stimulates their construction.

Actually—so the Austrian theory continues—the underlying realities have not changed. Resources available for long-term-oriented investment have not become more abundant. Shortages and price increases will reveal intensifying competition for resources among industries producing higher-order capital goods, lower-order (closer-to-the-consumer) capital goods, and consumer goods. This becomes particularly true as workers in the artificially stimulated industries, whose contributions to ultimate consumption are far from maturity, try to spend their increased incomes on current consumption.

Price signals, especially the interest rate, have been falsified. Sooner or later appearances must bow to reality. Shortages or increased prices of resources necessary for their completion will force abandonment of some partially completed capital-construction projects, spelling at least partial waste of the resources already embodied in them. A tightening of credit, with loans no longer so readily available and interest rates no longer so artificially low as they had become, may play a part in this return to reality; for policies of expanding money and credit could not doggedly persist without threatening unlimited inflation.

Cutting back long-term-oriented investment (and even abandoning some partially complete projects) for the reasons just mentioned means laying off workers, cancelling orders for machines and materials, and cancelling some rentals of land and buildings. The downturn is under way. In the ensuing depression, unwise projects are liquidated or restructured and the wasteful misallocation of resources begins to be undone—but painfully.

## The Appeal of the Austrian Scenario

Some such scenario understandably appeals to Austrian economists. They like to stress that money is not neutral. New money enters the economy through particular channels and only gradually works its effects on all sectors. Meanwhile, it exerts what the Austrians like to call “*Cantillon effects*” (after Richard Cantillon [1755] 1931, particularly pp. 158ff.). The new money exerts differential effects on individual prices, including the interest rate, and individual types of economic activity. Austrian economists dislike theorizing in terms of aggregates such as the general price level, total output, and total employment. They disaggregate. They practice “*methodological individualism*”; they carry their theorizing to the level of the individual business firm, worker, and consumer, investigating how the individual responds to incentives impinging on him, including changes in interest rates and other relative prices.

### What Evidence or Argument?

A theory’s appeal on quasimethodological grounds is not the same thing, however, as evidence supporting it over its rivals. The Austrian scenario of boom and downturn is hardly the only conceivable scenario. Furthermore, it does not explain and hardly even purports to explain the ensuing depression phase. Depression is a pervasive phenomenon, with customers scarce, output reduced, and jobs lost in almost all sectors of the economy. Unlike what might be said of the boom and downturn, the depression phase can hardly be portrayed as an intersectoral struggle for productive resources exacerbated by distorted signals in interest rates and other prices. Austrian economists can explain the continuing depression only lamely, mentioning maladjustments being worked out painfully over time—unless they invoke a “*secondary deflation*,” meaning monetary factors going beyond their own distinctive theory.

My chief objection to the Austrian theory, then, is that it is no more than a conceivable but incomplete scenario. Furthermore, it is an unnecessarily specific scenario: it envisages specific responses to specific price distortions created by the injection of new money, but it demonstrates neither the necessity nor the importance of those specific distortions to the downturn into the depression, let alone to the depression itself. Monetary disequilibrium theory, in contrast, can handle the phenomena of boom and depression with less specific suppositions; unlike the Austrian theory, it does not disregard Occam’s Razor.

Austrians offer little evidence for their cycle theory beyond its supposed plausibility and its coherence with their methodology. To my knowledge, the chief published exception to this statement is Charles Wainhouse's article of 1984 (evidently derived from his unpublished New York University dissertation). Using monthly data for the United States for January 1959 through June 1981, all seasonally adjusted except interest rates, Wainhouse investigates whether (1) changes in the supplies of savings and of bank credit are independent, (2) changes in the supply of bank credit lead to changes in interest rates, (3) changes in the rate of change of bank credit lead to changes in the output of producer goods, (4) the ratio of producer-goods prices to consumer-goods prices tends to rise after bank credit starts expanding, (5) prices of producer goods closest to final consumption tend to decline relative to prices of producer goods further away from final consumption after bank credit starts expanding, and (6) consumer-goods prices rise relative to producer-goods prices at the turn from boom to recession, reversing the initial shift in relative prices.

Applying Granger-causality tests and other statistical techniques to his data, Wainhouse obtains results he deems consistent with the six hypotheses mentioned. (He also states but does not test three further hypotheses associated with Austrian cycle theory.) Wainhouse does not claim to have actually validated the Austrian theory, of course, but he does suggest that his results warrant further serious study of it.

Stepping back from the details, let us consider just what Wainhouse has found true, or has failed to reject, for the United States from 1959 to 1981. Expansions of money and credit do occur, do affect interest rates, do appear to affect output of producer goods, and do appear to be followed by temporary shifts in relative prices of goods far from and near to final consumption, all of which is compatible with the Austrian theory.

Wainhouse deserves congratulations for going beyond the usual Austrian recitations and looking for actual evidence. (I sometimes get the impression that Austrians recite their favorite cycle theory as a kind of elaborate password for mutual recognition and encouragement.) Wainhouse does not offer any empirical discussion, however, of the downturn and the ensuing recession or depression. He merely finds several facts consistent with Austrian theory. But innumerable facts are consistent with almost any theory—that Bach lived before Beethoven, that Hebrew is the language of Israel, and that Mars has two moons. My point is that Wainhouse does not find, and as far as I

know did not look for, evidence that might discriminate between the Austrian theory and its rivals.

## Austrian Theory and Disequilibrium Theory

Wainhouse's statistical results are compatible, in particular, with monetary disequilibrium theory. Most obviously, both Austrian and monetarist theories recognize that expansion and contraction of money affect credit conditions. The specific Austrian scenario is not necessary to understand why demands for capital goods, particularly of higher orders, fluctuate more widely over the cycle than demands for consumer goods and for investment goods close to final consumption. Firms invest in view of prospects for profitable sale of the consumer goods and services that will ultimately result, and investment is more susceptible to postponement or hastening than is consumption. In the short and intermediate term, then, investment can exhibit a magnification of observed or anticipated fluctuations in consumption demands. In a world of uncertainty, furthermore—uncertainty exacerbated by monetary instability—hindsight will reveal some investment projects to have been unwise, some even being abandoned before their completion. The Austrian theory is not needed to account for these facts.

Monetary disequilibrium theorists put less stress than the Austrians on shifts in the interest rate and relative prices. The reason is not that they deny such shifts.<sup>3</sup> The reason, rather, is that such shifts, though crucial to the distinctively Austrian scenario, are mere details in the monetary disequilibrium account of the business cycle. Understandably the monetarists emphasize the centerpiece of their story—a disequilibrium relation between the nominal quantity of money and the general level of prices and wages.

## Rational Expectations and Equilibrium Always

The Austrians and rational expectations theorists reject traditional monetary disequilibrium theory for different reasons. The Austrians do not mind recognizing the reality of disequilibrium and sometimes even wax scornful of equilibrium theorizing, but they favor a specific scenario of intersectoral distortions tracing to manipulations of money and credit. While belief in rational expectations ("ratex" for short, as in Dean 1980) does not logically entail belief that markets always

<sup>3</sup>For documented refutation of Austrian charges that mainstream economists deny or unduly neglect relative-price effects, see Humphrey (1984).

clear or that one should at least theorize as if they did, there is no denying that the two beliefs often occur together.

Austrians and ralex/equilibrium-always theorists have one thing in common, however—strong methodological influence on their substantive doctrines. This I hope to show.

The challengers of disequilibrium theory ask why stickinesses persist and contracts go unrevised, obstructing exchanges, if people can reap gains from trade by adjusting prices and wages. They find it irrational for people to delay adjustments enabling mutually advantageous transactions to proceed (Grossman 1981, 1983).

Equilibrium-always theorists do not, then, see fluctuations in output and employment as reflecting changing degrees of disequilibrium. Robert Lucas (1980, p. 709) recommends “*equilibrium* models of business cycles. . . in which prices and quantities are taken to be always in equilibrium” and in which “the concepts of excess demands and supplies play no observational role and are identified with no observed magnitudes.” Mark Willes (1980, pp. 82, 90, 92–93), at the time president of the Federal Reserve Bank of Minneapolis, one of the citadels of the school, waxed enthusiastic about new developments in what he called classical economics, built on “the premises that individuals optimize and that markets clear.” The school believes that “the economy is best represented by a model that includes continuous equilibrium. Equilibrium modeling . . . appears able to explain unemployment and the business cycle without discarding what we know about microeconomics. . . . It is not necessary, after the new advances in classical theory, to resort to disequilibrium models in order to account for unemployment, queues, quantity rationing, or other phenomena that accompany the business cycle.”

Even Barro, one of the elaborators of disequilibrium economics in the tradition of Clower and Leijonhufvud, subsequently joined in complaining (1979, p. 58) that “the disequilibrium type of model . . . relies on a nontheory of price rigidities. . . .”

Why does he say “nontheory”? Though perhaps not often spelled out in detail, the theory is available, as this paper has been trying to show; and if it is eclectic, so be it. Anyway, lack of a theory would not mean absence of the phenomenon. Robert Solow (1980, p. 7) recalls “reading once that it is still not understood how the giraffe manages to pump an adequate blood supply all the way up to its head; but it is hard to imagine that anyone would therefore conclude that giraffes do not have long necks.”

Other critics of the ralex school have also interpreted its members as saying just what they do seem to be saying. They take the view, according to Kenneth Arrow (1980, pp. 140, 148, 150) “that all

unemployment is essentially voluntary." They "assert that all markets always clear." They work with "a model in which prices clear markets at every instant. . . ." James Dean (1980, p. 28) directs skeptical attention to "the notion that unemployment is best modeled as *voluntary*. . . most or all of the unemployed are simply making a free and voluntary choice based on the real wage available to them." An unsoftened position of their school "is essentially one of perfect competition, of instantaneously clearing markets" (Haberler 1985, p. 23). Frank Hahn ([1981] 1985, p. 105) finds the Lucasians, as he calls them, professing "the notion of involuntary unemployment to be beyond their comprehension and in some way meaningless. I confess that I sometimes hope that they may come to learn by personal experience what the notion is about." Willem Buiter (1980, p. 41) identifies "the *ad hoc* assumption of instantaneous and continuous competitive equilibrium applied so routinely to labour and commodity markets by economists of the 'New Classical School'. . . ." James Tobin (1980, p. 788) reminds his readers of two crucial ingredients in the "new classical macro models": "the assumption of continuous market-clearing equilibrium and the specification of imperfections and asymmetries in the information on which economic agents act and form expectations. The two are connected in the sense that information gaps play in the new macroeconomics very much the same role that failures of prices to clear markets play in the Keynesian tradition, by which I mean the neoclassical synthesis. . . ."

Instead of identifying disequilibrium for what it is, ratex theorists suggest that markets still clear as people react to distorted or misperceived prices. Producers or workers misperceive increases in the prices of their own products or labor as genuine increases in real or relative terms even when those increases merely accompany a *general* price inflation. Workers supply more labor (as by reducing their quits or accepting new jobs after shorter searches) because they think they are being offered increased real wage rates. Such misperceptions are likely when inflation comes unexpectedly or at an unexpectedly increased rate. In the opposite case, people cut back work or output because they mistakenly perceive general price deflation as cuts specifically in the prices of their own labor or products. Even a mere slowdown in inflation can cause contraction in this way. Mistakenly thinking that their real wages are being cut, workers may quit their jobs more readily than before and voluntarily engage in lengthier job search. Producers, similarly, may mistakenly perceive a general slowdown of price inflation as declines in the real or relative *prices of their own products* and may cut production in response. In the sense that workers and producers are still operating "on their

supply curves," equilibrium, though distorted, continues to prevail. Even this distortion would be absent if people fully expected and allowed for the underlying change in monetary policy, as self-interest would lead them to do to the extent cost-effectively possible. On this theory, fluctuations in production, employment, and price levels do not represent changes in the degree and direction of any monetary disequilibrium.<sup>4</sup>

The idea of rational expectations is probably useful in many of its applications, but the associated doctrine of equilibrium-always is just wrong as macroeconomics. It contradicts the facts of involuntary unemployment and other failures of markets to clear. It unconvincingly challenges a doctrine that has appealed to economists for over two centuries, that fits in well with microeconomic theory, and that is well supported by narrative and statistical history.

No general rule applies in all cases about what simplifying ("unrealistic") assumptions are appropriate. All depends on the particular questions being tackled. In tackling questions about the long-run effects on prices and outputs of specified changes in wants, resources, technology, and legislation, one may legitimately neglect intervening disequilibrium to get on with the analysis. But when questions of macroeconomics are at issue—essentially, questions concerning disruptions or imperfections or delays in processes working to coordinate the plans and activities of many different people—then attention properly turns to how quickly and smoothly markets respond when disturbed, to transitional stages, and to the frictions of reality.

Of course markets tend to clear; of course people act to reap gains from trade. But how quickly and effectively? When a monetary disturbance makes price adjustments necessary, how do individual transactors know just what particular adjustments would be appropriate, and what incentives do they have to go first in making them? Such information and incentives do not come to the attention of individual transactors in some magical way, outside the market. The market has work to do. Individuals see the need for price adjustments when they meet frustration in trying to carry out desired transactions at the old prices. Echoing Christiernin, quoted earlier, Charles Schultze notes (1985, pp. 11, 13) that "In a world of price and wage setters, firms and workers observe demand shocks principally in the form of

<sup>4</sup>This paragraph alludes to the Phelps-Friedman-Lucas supply function, or Lucas supply function, or "surprise" supply function, so called by Buiter (1980, p. 34 and *passim*). See also, for example, Lucas (1973). For further criticism of insistence on seeing quantity changes as occurring only in response to price changes, whether interpreted correctly or incorrectly, see Birch and others (1982).

changes in their own physical quantities—sales first and then output and employment. . . . [T]he same kind of initial signals—changes in the volume of sales—” is required for “a change in the general level of wages and prices” as for a micro reallocation of resources. Even if an exceptional individual did quickly understand the underlying disturbance and the required adjustments, he might see little advantage in adjusting his own price unless others adjusted theirs also.

Anyway, actual or incipient failure of markets to clear is necessary to convey information and incentives. When rational theorists emphasize that people will adjust prices as necessary to reap gains from trade, they should recognize that they are theorizing about market forces and signals and processes. They have no warrant for assuming that those processes work so fast as to preclude disequilibria in the form of recessions or depressions.

As Haberler has written, quoting Armen Alchian (Haberler 1985, p. 13; Alchian 1969, p. 117), “even ‘in open, unrestricted competitive markets with rational, utility maximizing individual behavior,’ substantial or, in case of a sharp decline in monetary demand (depression), ‘massive’ unemployment is possible. . . . The basic idea is that information about job opportunities is not a free good.”

John Boschen and Herschel Grossman employed both preliminary and revised data on monetary aggregates to try to distinguish between responses to anticipated or perceived and to unanticipated or unperceived components of monetary policy. They obtained results “apparently fatal to the equilibrium approach.” They find the theory of macroeconomic fluctuations in an “unsatisfactory state.” “[E]quilibrium theorizing does not provide an . . . explanation of macroeconomic fluctuations whose implications accord with the apparent facts. The business cycle, consequently, seems mysterious” (1982, pp. 329–30). One must admire the authors’ candor, yet wonder at their being mystified.

## The Curse of Methodology

How scholars got their ideas and why they keep urging them are irrelevant to whether those ideas are right or wrong. One should not dismiss ideas because of conjectured motives. But when people persist in an idea—such as a particular interpretation of macroeconomic phenomena—that abundant evidence and argument tell against and for which a well-supported alternative is available, that persistence itself arouses intellectual curiosity. Is persistence among leading scholars some sort of argument for an idea’s validity, after all, and a sign of poor judgment on the part of those who reject it? Or is its

persistence a genuine puzzle? A puzzle prompts a search for hypotheses that would explain it.

Setting forth some hunches on these questions may contribute to a dialogue among monetary disequilibrium theorists and equilibrium-always theorists. It may bear on diagnosing the current state of academic economics, including what one might call the curse of methodology. Perhaps sheer fashion has some influence on what ideas are thought acceptable.<sup>5</sup>

Recent writings by Donald McCloskey (1983, 1983, 1985) are helping make it respectable, or so I hope, to question methodological sermons (especially sermons that are insidious because pervasive and tacit), to pay attention to styles of argument, and to regard clarity and even effective rhetoric as virtues. Respectability should not demand one single approved style of modeling or evidence or argument.

To start with a specific example of apparent methodological preconception, I suspect that the Lucas supply function and the idea that sellers are responding to prices according to their supply schedules (rather than sometimes meeting frustration in nonclearing markets) trace to an overemphasis on price signals. People respond to prices, and macroeconomists who do not want to lose contact with price theory should take those responses seriously.

So far so good. That methodological view contributes, however, to the tacit but questionable idea that producers or sellers respond to prices only—rather than also to how readily they are finding customers. That view tends to preclude seeing “positions off the curves,” and positions “off” to a greater or lesser extent. Notions of pure competition lurk below the surface: the seller can sell all he wants to at the going price.

Equilibrium-always theorists seem to believe that monetary expansion, for example, and unexpected monetary expansion in particular, can have an impact on real variables only *through* price changes—unexpected and misinterpreted price changes—and not directly, as by giving sellers more customers. The rival monetary disequilibrium theory can readily interpret recovery from depression following expansion of the nominal quantity of money (or, alternatively, following expansion of the real quantity through wage and price cuts) as due to relief of an excess demand for money (strictly, relief of what would have been an excess demand at full employment).

<sup>5</sup>In an apparent allusion to this situation, Edmund Phelps (1981, p. 1065) praised Okun for courage—“courage to venture a big theoretical work, in an accessible style, on urgent questions.”

But a theorist unwilling to recognize disequilibrium in the first place has to attribute the expansion of output and employment as people's responses to prices along their supply curves.

More generally, the idea seems to be afoot that equilibrium modeling is the thing—the technically advanced thing—to be doing in macroeconomics. Lucas recommends his own brand of equilibrium economics by saying that it employs technical advances in modeling that simply were unavailable a few decades or even a few years earlier. The most important force in recent business-cycle theorizing, he writes (1980, pp. 697, 708), “consists of purely technical developments that enlarge our abilities to construct analogue economies. Here I would include both improvements in mathematical methods and improvements in computational capacity. . . . The historical reason for modeling price dynamics as responses to static excess demands goes no deeper than the observation that the theorists of that time did not know any other way to do it.”<sup>6</sup>

Mark Willes (1980, pp. 90, 92) notes that the rational expectations school builds on classical premises but has constructed models exhibiting business-cycle features “which the old classical theory couldn't handle. . . . It is not necessary, after the new advances in classical theory, to resort to disequilibrium models in order to account for . . . phenomena that accompany the business cycle.”

Also suggesting the influence of sheer commitment to a cherished theoretical tradition, Grossman writes (1983, p. 240): “The position that strict application of neoclassical maximization postulates is relevant to macroeconomic developments only in the ‘long-run’ may seem reasonable from an empirical standpoint, but it puts neoclassical economics in a defensive position. It suggests the possibility of a general inability of neoclassical economics to account for short-run economic phenomena.” Yet, despite what Grossman seems to imply, disequilibrium is not incompatible with individuals' efforts to maximize.

The idea seems to be in circulation that notions of disequilibrium betray an incomplete model. An economist who talks about disequilibrium is not really talking about failure of market mechanisms but rather, without realizing it, about his own failure as a model-builder. A related interpretation views the equilibrium-always doctrine as a methodological exhortation or heuristic rule: do not cop out by speaking

<sup>6</sup>Expressing a more “general view of the nature of economic theory,” Lucas (1980, p. 697) says that a theory “is not a collection of assertions about the behavior of the actual economy but rather an explicit set of instructions for building a parallel or analogue system—a mechanical, imitation economy.”

of disequilibrium; try to improve your model so that observed magnitudes correspond to solutions to its equations.

In mathematical models, states of affairs or patterns of economic activity are conceived of as solutions to sets of equations, as points on intersecting curves. Disequilibrium states—states represented by points off the curves, so to speak—are messy. It is methodologically unsatisfactory to allow for prices and quantities that are not at their equilibrium values but are only tending toward them at speeds specified only in *ad hoc* ways. In this connection, Lucas (1980) scorns models containing “free parameters.”

Similar remarks apply to treatment of disequilibrium processes, such as what happens when people try to increase or decrease their cash balances or how the decentralized but intertwining nature of wage and price determination makes for stickiness in the average level or trend of prices. Observation of and reasoning about such processes in the relatively nonmathematical manner in which they are most straightforwardly handled can be stigmatized as casual and loose, so they escape due attention.

Equilibrium-always theorists presumably know as well as anyone else that atomistic competition is and must be the exception rather than the rule in the real world, that sellers are typically not selling as much of their output or labor as they would like to sell at prevailing prices, that most prices and wages are consciously decided upon rather than determined impersonally (even though they are set with an eye on supply and demand), and that these circumstances, among others, make for or reveal price stickiness. But they do not know these facts officially—not in what they consider a methodologically reputable way.

They are inclined to invoke a famous slogan, reasonable enough in certain contexts and under certain interpretations, yet much abused: Willes (1980, p. 91) recites that “theories cannot be judged by the realism of their assumptions. . . .” Actually, it is necessary to distinguish at least between simplifying assumptions that abstract from facts irrelevant to the question under investigation and assumptions on which the conclusions crucially depend. In critically examining Milton Friedman’s position, Alan Musgrave (1981) makes enlightening distinctions between negligibility, domain, and heuristic assumptions.

A related bit of methodology tending to discredit notice of unmistakable realities is ritualistic insistence that scientific propositions be testable and conceptually refutable. A supposedly empirical proposition immune to being refuted by any evidence is by that very token beyond the pale of science.

Two kinds of irrefutability, however, must be distinguished. Propositions of the disreputable kind have a *built-in* immunity to adverse evidence. Their ostensible empirical character is a sham. Instead, they convey emotions or the intention to use words in special ways or to follow particular policies. Charles Peirce ([1878] 1955, pp. 30–31) gave an example: the proposition that the wafers and wine in the Mass turn into the body and blood of Christ while retaining all physical and chemical and other detectable properties of wafers and wine. Another example is the remark attributed to Father Flanagan of Boys' Town that there is no such thing as a bad boy (no matter what horrible crimes he habitually commits, he is fundamentally a good person and worthy of efforts to rehabilitate him). Still another example might be the Marxian proposition about increasing immiserization of the proletariat, with immiserization interpreted flexibly enough to accommodate any evidence.

A more respectable kind of irrefutability characterizes propositions for which empirical evidence keeps pressing itself upon us every day in such abundance that only with effort can we even imagine a world where those propositions were not true. (But if it turned out that we had been deluded, propositions hinging on our delusions would be refuted after all.) Some examples are that people act purposefully, that resources are scarce in relation to people's practically limitless wants, that more than one factor of production exists and that the law of diminishing returns holds true, that money functions and is supplied and demanded differently than all other goods, that most prices and wages are not determined impersonally and flexibly in atomistic competition, and that markets sometimes do fail to clear. No one will make a scientific reputation by discovering facts like that, of course; but it hardly follows that inescapably familiar facts are by that very token unimportant and deserving of neglect.

## The Appeal of Equilibrium Theorizing

It is unnecessary to spell out a precise and agreed definition of "equilibrium" to recognize that different and changed meanings of the word are in circulation. Traditionally, and loosely, equilibrium is said to prevail when the plans of different people are meshing in the sense that markets clear. Disequilibrium means discoordination. Market participants may have good reasons from their own points of view for not promptly initiating the price adjustments that would bring markets closer to clearing. Whether or not plans mesh does not hinge only on bilateral negotiations between the potential parties to individual transactions, for what appears acceptable to those parties

may well depend on what other parties are agreeing on or failing to agree on for other and perhaps quite different transactions. Some transactions may be falling through because they are not attractive without adjustments to prices not under the control of the parties directly involved. (Some producers may have shut down in a depression, for example, because input prices have not fallen enough for them to cover even their variable costs at a product price low enough to attract customers.) The fact that everyone is behaving rationally from his own point of view does not mean that plans are meshing and markets clearing after all. Each individual may be making the best of the circumstances confronting him—and be in equilibrium in that narrow sense—without the aggregate of such individual positions constituting a general equilibrium for the economy.

Equilibrium-always theorists nevertheless seem to be sliding into the notion that practices making sense for the parties involved constitute an equilibrium. If, for example, advantageous but tacit contracts make prices and wages inflexible in the short run, then the apparent failure of markets to clear need not count as a departure from equilibrium. If, as mentioned above, talk of disequilibrium betrays an incomplete model, then an adequately modeled state of affairs is an equilibrium. Lucas and Sargent (1978, p. 58) even appear to congratulate themselves on the “dramatic development” that the very meaning of the term “equilibrium” has undergone in recent years. Dennis Carlton (1979) also seems to use the term “equilibrium” in pretty much the changed sense noted here. Stiglitz (1979, pp. 342–43, 345) speaks of “competitive market equilibrium [without] market clearing,” “non-market-clearing equilibria,” and “equilibria in which markets do not clear.” Sargent (in Klammer 1983, pp. 67–68) expresses satisfaction with “fancier” notions of equilibrium, “much more complicated” notions of market-clearing, and “fancy new kinds of equilibrium models.” Yet destabilizing the meanings of words, subverting communication, is hardly constructive. (Compare trying to defend the Catholic interpretation of the Mass with “fancier” and “much more complicated” definitions of body and blood, ones that have undergone “dramatic development.”)

Perhaps theorists who are uncomfortable with disequilibrium and who change their conceptions of equilibrium do so because they do not recognize that equilibrium is a limiting concept, a theoretical extreme case. They do not recognize that equilibrium, like pure competition, although highly useful in theorizing as a benchmark state toward which market forces are tending, is nevertheless not actually and fully reached in the real world. They feel they must define or redefine it so they can say it exists.

Equilibrium in the sense of complete meshing of plans could not prevail outside the abstract world of pure and perfect competition or of a Walrasian (actually, *non*Walrasian) auctioneer who somehow makes everyone behave like a price-taker (and, furthermore, a world without disturbing changes in the data). Nevertheless, it still makes sense to speak of greater or lesser closeness to this limiting state. It makes sense to speak of a state of approximate equilibrium being disrupted by a change in money's supply or demand. This formulation is loose, admittedly, but as Aristotle said (1947, p. 309), "Our discussion will be adequate if it has as much clearness as the subject-matter admits of, for precision is not to be sought for alike in all discussions, any more than in all the products of the crafts."<sup>7</sup>

Monetary equilibrium or disequilibrium prevails according to whether or not total desired holdings of money equal the actual quantity at the existing purchasing power of the unit. The importance of the distinction does not hinge on anyone's being able to identify monetary equilibrium with precision. Despite real-world difficulties of maintaining or restoring monetary equilibrium, the sheer *concept* of equilibrium is, in one respect, beset with slighter difficulties for money than for an ordinary good or service. A specific national money, the actual medium of exchange, is more nearly homogeneous than an ordinary good or service. The individual transactor is a price-taker with regard to it: he must regard its purchasing power as set beyond his control, except to the utterly trivial extent that the price he may be able to set on his own product arithmetically affects money's average purchasing power. This very fact that no one sees himself as having any appreciable influence over the value of the money unit helps explain the sluggishness of the pressures working to correct a disequilibrium value.

Another hunch about the appeal of equilibrium always concerns the apparent notion—reflected in the very title of Barro's "Second Thoughts on Keynesian Economics" (1979)—that theories involving price and wage stickiness are Keynesian and therefore, to advanced thinkers, outmoded and wrong. Clower (1965) and Leijonhufvud (1968) offered their disequilibrium theories as interpretations of Keynes. Arrow (1980, p. 149) casually refers to "Disequilibrium theorists. . . stemming from Keynes. . ." Stanley Fischer (in Fischer 1980, p. 223) refers just as casually to "Keynesian disequilibrium analysis." Tobin (1980, p. 789) refers to "the Keynesian message" as dealing with disequilibrium and sluggishness of adjustment. Hahn

<sup>7</sup>On inappropriate preoccupation with being precise, compare Popper (1957, II, 19–20, 296, n. 50).

(1980, p. 137) notes "the present theoretical disillusionment with Keynes" (which, he conjectures, will be reversed). An admirably realistic discussion of nominal wage stickiness is presented by Schultze (1985), generally regarded as a prominent Keynesian.

Actually, as shown earlier, theories of stickiness and monetary disequilibrium far antedate Keynes; and it is ironic to associate those theories with him, especially since he did more than perhaps any other economist to divert attention from them. Economists have been playing musical chairs in recent years, but with doctrines and labels instead of chairs. (Leijonhufvud made some such observation in a witty talk in November 1983.) The abandonment of disequilibrium macroeconomics by players shifting into the *ratex*/equilibrium-always camp left a partial void into which former Keynesians could move, gracefully discarding their discredited doctrine while keeping their old label. As a result, the label "Keynesian" is now often applied both to nonKeynesian monetary-disequilibrium theorists and to the (former) Keynesians who have recently joined them. Observers should be more careful with doctrinal history and labels.

Mention of theories thought to be "outmoded" prompts a more general remark. Not novelty, not fashion, not even methodological fashion or technical virtuosity or suitability for academic gamesmanship should be the criterion of accepting a theory. Being venerable does not necessarily prove a theory wrong. The contrary is more plausible when human behavior is the subject matter. If observations in widely separated times and places have led many different writers to broadly the same theory, such as monetary disequilibrium theory, that fact counts *something* in its favor. The criterion should be explanatory power and conformity to fact and logic.

A final conjecture about the appeal of equilibrium always is that some theorists (e.g., Barro 1979, esp. p. 55) are sliding from (warranted) skepticism about activist government policies into (unwarranted) attribution of near-perfection to markets. Yet no human institution is perfect. The imperfection of one, the state, does not imply the perfection of another, the market. It does not imply the capacity of the market to cope quickly and painlessly even with severe shocks.

## Prospects for Theory and Policy

I want to guard against being misunderstood. I am far from condemning the *ratex*/equilibrium-always school root and branch. It offers improvements in some strands of theory, it makes sound criticisms of Keynesianism as it used to be widely taught and practiced, and it draws sensible policy implications (Lucas 1981, for example).

But those improvements and criticisms and implications can be obtained in a way that better accords with straightforward observation and theory and better maintains continuity with earlier research achievements.

Monetary disequilibrium theory stands up well in comparison with both rate/ex equilibrium-always theory and Austrian business-cycle theory. Both those rivals are suffused with methodological preconceptions. (The Austrians deserve credit, however, for facing up to facts of reality that many neoclassicals apparently regard, if they regard them at all, as embarrassing "imperfections.")

Hahn (1980, p. 37) and Dean (1980, p. 32) may well be right—Hahn in expecting reversal of disillusionment with the disequilibrium approach, Dean in judging that "macroeconomic theory's future probably lies with the Evolutionaries" (which is his term for disequilibrium theorists).

This is not to say that all issues are now settled and that monetary disequilibrium theory should henceforth be held as dogma. Like all theories about empirical reality, it is open to being modified or abandoned in the light of new evidence and argument and newly devised alternatives. I conjecture, though, that it will be fruitful to develop the theory further along lines that recognize how the forces tending spontaneously to restore a disturbed monetary equilibrium are diffused weakly over all sectors of the economy because the medium of exchange lacks a definite market and price of its own on which the pressures of imbalance between supply and demand come to a focus. Quite rationally from their own points of view, individuals behave in ways that add up, macroeconomically, to price and wage stickiness (and, in inflation or stagflation, to persistence of trends). Well-warranted skepticism about activist macroeconomic policies does not justify optimism about the capacity of markets to cope rapidly with monetary disturbances.

The reality and the severe consequences of monetary disequilibrium recommend policies to forestall it. Perhaps the old monetarist rule of steady monetary growth still would be adequate for keeping the supply of money approximately matched to the growing demand. On the other hand, perhaps prolonged disregard of monetarist advice has created complications that the steady-growth rule now could not cope with. Inflation-boosted nominal interest rates interacting with interest ceilings and reserve requirements have induced such a series of financial innovations that we no longer can be confident of how to define money, of whether the Federal Reserve could adequately manipulate its quantity, and of whether the demand-for-money function will remain stable.

The time has come to consider radically different alternatives. (The contrasts they afford with our existing system can be instructive, even if none of them is ever implemented.) One radical alternative is a version of Irving Fisher's compensated dollar (1920). Two-way convertibility between the dollar and the variable physical amount of gold always equal in actual market value to the bundle of goods and services defining a comprehensive price index would amount to indirect convertibility between money and the bundle. Under that arrangement, the whole price level would no longer have to rise or fall—painfully bucking frictions—to correct monetary disequilibrium; and the actual quantity of money would become automatically responsive to the demand for it. A different reform (Greenfield and Yeager 1983) would get the government out of the money business. The unit of account, divorced from the medium of exchange, would be defined as the value of a bundle of many goods. As under Fisher's plan, the price level would be spared pressures tending, sluggishly, to change it. The supply of media of exchange would be left to private banks and investment funds, which would respond to demands for them. These arrangements would preclude monetary disequilibrium as we have known it.

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# MONEY, REAL ACTIVITY, AND RATIONALITY

*Herschel I. Grossman*

## Money and Real Activity

Conventional wisdom interprets the empirical relation between monetary aggregates and measures of real aggregate economic activity primarily as reflecting the effect of monetary policy on real activity. A host of historical episodes apparently accord with this interpretation. It is, for example, hard to deny that disinflationary monetary policy contributed to the 1982 recession in the United States.

Some theorists, such as King and Plosser (1984), have questioned this interpretation and have developed real business cycle models that attempt to explain the observed correlations of money and real activity as solely a result of the common influences of other factors, such as disturbances to tastes, technology, and resources. These theorists, however, have not been able to identify an alternative set of impulses that does not contain disturbances to monetary aggregates and that has appropriate structural characteristics, sufficient magnitude, and requisite regularity to be responsible for the bulk of observed fluctuations in real activity. This inability to identify alternative causal factors reinforces the standard reading of history that monetary policy influences real activity.<sup>1</sup>

Given the conventional interpretation of the observed relation between money and real activity, a satisfactory theoretical and empirical analysis of macroeconomic fluctuations must account for an effect of monetary policy on real activity as well as on inflation. This account must be consistent with the following general features of the data:

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<sup>1</sup>See McCallum (1986) for a thorough critique of real business cycle models.

(1) current realizations of monetary aggregates are correlated with subsequent realizations of both real activity and inflation; (2) correlations of money with real activity are strong in the short run but weaken in the long run, whereas the correlations of money with inflation are weak in the short run but become stronger in the long run; and (3) correlations with real activity are stronger for unanticipated realizations of monetary aggregates, whereas the correlations with inflation are stronger for anticipated realizations of monetary aggregates. The main attraction of monetary disequilibrium theory, which is the useful name Leland Yeager (1986) applies to what is often called the Keynesian or non-market-clearing approach, is that it provides an explanation for the effects of monetary policy on real activity and inflation; an explanation that in its modern versions (which incorporate the natural-rate hypothesis and the rational-expectations hypothesis) seems to be broadly consistent with these three general features of the data.

An explanation for the effect of monetary policy on real activity also must satisfy criteria of logical consistency. Most important, aggregate economic activity is merely a statistical summary of a multitude of individual productive decisions, which are the same individual decisions that determine resource allocation and income distribution. Accordingly, the assumptions about economic behavior used to account for the relation between money and real activity should be consistent with the assumptions used to explain resource allocation and income distribution. Moreover, we cannot avoid this consistency requirement by asserting that macroeconomic fluctuations are a short-run phenomenon, whereas questions about resource allocation and income distribution involve the long run. In fact, economists routinely apply standard microeconomic analysis to the short run—that is, to a time horizon shorter than the typical business cycle.

## Economic Rationality and Monetary Disequilibrium

The distinguishing feature of conventional economic analysis of resource allocation and income distribution is the assumption that producers in free markets exhaust perceived opportunities for mutually advantageous exchange. Standard microeconomic analysis takes this assumption to be a corollary of the basic economic postulate of maximization. Yeager's contentions notwithstanding, the disattraction of the monetary disequilibrium theory is that, as yet, its proponents, who include most macroeconomists, have been unable to reconcile

it with the postulate of maximization and its corollary that perceived gains from trade are exhausted.

Yeager claims that the existence of coordination problems reconciles monetary disequilibrium with the postulate of maximization. He argues that, even with producers behaving as rational maximizers, *perception and coordination* of the wage and price adjustments necessary to clear markets in the face of unanticipated monetary disturbances take time. Yeager points out that "One cannot consistently both suppose that the price system is a communication mechanism—a device for mobilizing and coordinating knowledge dispersed in millions of separate minds—and also suppose that people *already* have the knowledge that the system is working to convey." This observation is correct, but it seems irrelevant for the analysis of monetary disequilibrium because the values of monetary aggregates are public information. In contrast to truly private information, the monetary aggregates are not information that the price system has to convey.

Yeager claims further that even with complete information strategic considerations would cause individual rationality to diverge from the collective rationality implicit in monetary equilibrium. Like Charles Schultze (1985), Yeager invokes the analogy of the prisoner's dilemma to argue that the unwillingness of any producer "to go first" would *inhibit wage and price adjustments*. This analysis is confusing because it seems to imply too much—namely, that wages and prices are rigid rather than merely sticky. In any event, the usefulness of the prisoner's dilemma analogy for understanding market behavior seems limited because the prisoner's dilemma relates to a hypothetical game played by a small number of agents who cannot communicate with each other during the game.

For a monopolist or *collusive oligopoly*, individual and collective optimality of wage and price adjustments obviously coincide. In a market of many imperfectly competitive producers, however, optimal individual wage and price responses to some disturbances can differ from optimal collective responses. But, observed changes in monetary aggregates are not such a disturbance. Unless price adjustments are prohibitively costly, optimal individual price setting behavior requires responding to an observed disturbance to monetary aggregates even if the individual thinks that others are ignoring the disturbance. The "initial" response, of course, might not be an equiproportionate price adjustment, but, even without rational expectations, subsequent responses culminate in an equiproportionate adjustment. Moreover, if we assume either that expectations are

rational or that price-adjustment costs are small, the theory suggests that the full adjustment is essentially instantaneous.

Yeager, again like Schultze, also refers to models of efficient long-term contracts and implicit buyer-seller understandings. This reference is puzzling, because, although these models suggest that real or relative wages and prices would be less flexible than models of spot markets imply, models of efficient contracts also suggest, if anything, that rational wage setters would fully index nominal wages and prices to observed monetary disturbances. Schultze recognizes this point, but claims that the complexity of the relation between monetary aggregates and market-clearing nominal wages precludes indexation. It is not clear, however, why this problem results in zero indexation. Even if producers cannot easily determine the optimal degree of indexation, they surely know that some positive indexation would be better than zero indexation. Yeager does not mention the currently popular models of efficiency wages, but, like the idea of efficient contracts, the idea of efficiency wages, whatever its ability to explain the equilibrium structure of real wages and employment, also has no apparent relevance for the problem of rationalizing stickiness of nominal wages and resulting monetary disequilibrium.

In the early 1970s, such theorists as Robert Lucas (1972, 1973) and Robert Barro (1976) responded to the problem of reconciling monetary disequilibrium with the postulate of maximization by utilizing advances in the theory of expectations and general economic equilibrium under incomplete information to formulate "equilibrium" models of macroeconomic fluctuations. These equilibrium models assume that all perceived gains from trade are realized and that expectations are rational, and they rely on assumed lack of information about monetary aggregates in order to generate an effect of such aggregates on real activity. In recent years, interest in these equilibrium models has waned largely because, as Yeager points out, more extensive theoretical and econometric analysis has shown these models to be unable to account for the observed relation between monetary aggregates and real activity.

The empirical problem with equilibrium models, it should be stressed, does not involve direct evidence that perceived gains from trade are actually not realized. In fact, contractual versions of equilibrium models readily account for prominent observed features of macroeconomic fluctuations that would seem inconsistent with market clearing if market clearing were narrowly interpreted in a framework of spot markets.<sup>2</sup> These observed features include lack of cor-

<sup>2</sup>See, for example, Azariadis (1978) and Grossman (1981).

relation between aggregate employment and real wage rates and the use of layoffs to effect employment separations.

The empirical rejection of equilibrium models is based on rejection of an essential testable implication of the combined assumptions that all perceived gains are realized and that expectations are rational. This implication is that disturbances to monetary aggregates affect real aggregates only to the extent that currently available information does not permit agents to infer current monetary aggregates accurately. The testable form of this implication, derived by Boschen and Grossman (1982) following the lead of Robert King (1981), is that the current innovation in real activity is uncorrelated with contemporaneous measures of current and past changes in monetary aggregates. Not surprisingly, econometric analysis of data for the United States reported by Boschen and Grossman not only unambiguously rejects this hypothesis, but also finds no correlation between the innovation in real activity and revisions in preliminary estimates of monetary aggregates, these revisions being measures of the unperceived part of monetary policy.

The early equilibrium models of Lucas and Barro obscured the problem of reconciling equilibrium assumptions with the observed relation between monetary aggregates and real activity because they abstracted from the existence of contemporaneously available monetary data. Barro himself was among the first to recognize the consequences of relaxing this abstraction. An empirical study by Barro and Hercovitz (1980) anticipated the subsequent and more formal theoretical and econometric analysis of King and of Boschen and Grossman. In an early reassessment of equilibrium theories, Barro (1981, ch. 2, p.74) wrote:

A significant weakness of the [equilibrium] approach is the dependence of some major conclusions on incomplete contemporaneous knowledge of monetary aggregates, which would presumably be observed cheaply and rapidly if such information were important. The role of incomplete current information on money in equilibrium business cycle theory parallels the use of adjustment costs to explain sticky wages and prices with an associated inefficient determination of quantities in Keynesian models. The underpinning of the two types of macroeconomic models are both vulnerable on a priori grounds.

On the same page, however, Barro is quick to emphasize that:

[D]oubts about the explanatory value for business cycles of currently available equilibrium theories do not constitute support for Keynesian disequilibrium analysis. The disequilibrium theories are essentially incomplete models that raise even larger questions about the consistency of model structure with underlying rational behavior.

It remains a fair observation that existing macroeconomic theories—including new and old approaches—provide only limited knowledge about the nature of business cycles.

Lucas also has recognized the consequences for the implications of equilibrium models of taking contemporaneous monetary information into account. In a recent lecture Lucas (1985) acknowledges that “insofar as the monetary information necessary to permit agents to correct for what are, or ought to be, units changes is public . . . then one would expect this information to be used, independent of the form of interaction among agents.” Nevertheless, Lucas still seems willing to defend abstracting from contemporaneous monetary data as an “as-if” assumption, although he apparently can only vaguely conjecture about why rational agents would ignore information that is important and freely available. In the same lecture, he offers only the thought that “it seems to me most unlikely that it would be in the private interest of individual agents to specialize their individual information systems so as to be well-equipped to adapt for units changes of monetary origin.”

### Concepts of Near Rationality

As an alternative to the formulations of equilibrium models, other theorists have reacted to the difficulty of reconciling monetary disequilibrium with the postulate of maximization by appealing, either implicitly or explicitly, to concepts of near rationality. The seminal work of Stanley Fischer (1977), incorporating rational expectations into a nonmarket-clearing framework, is an important example of this approach. Fischer’s model assumes that nominal wages are sticky. But, in order to stick as closely as possible to the idea that perceived gains from trade are exhausted, the model also assumes that these predetermined nominal wages are equal to rational expectations of market-clearing wages.

Econometric testing of these nearly rational monetary disequilibrium models with rational expectations encounters the difficult problem of realistically dating the formation of the expectations relevant for the determination of current nominal wages and current real activity. As explained in Grossman (1983), Barro’s empirical results on the relation between real activity and unanticipated monetary disturbances, summarized in Barro (1981, ch. 5), provide qualified support for Fischer’s model. In another study, Grossman and Haraf (1985), by taking advantage of the fact that wage setting in Japan is both decentralized and synchronized, were able to examine empirically some detailed implications of Fischer’s model and to

show that the model, if suitably elaborated, seems to fit the Japanese data.

More recent theoretical work by Akerlof and Yellen (1985) focuses on the possibility that near rationality can account for monetary disequilibrium. This analysis directly confronts the problem that the postulate of maximization is inconsistent with an effect of monetary policy on real activity. It poses the question of how much nonmaximizing behavior is necessary and of what form this behavior must take for the effects of monetary disturbances on real activity to have a realistic order of magnitude. Akerlof and Yellen show that minor deviations from maximization by a subset of producers, who individually suffer only second-order consequences, are sufficient to produce first-order macroeconomic effects.

These recent developments still leave us without a fully unified theoretical framework applicable to the analysis of macroeconomic fluctuations and to the analysis of resource allocation and income distribution. Economic theory in its present state has to rely on empirical regularities to identify the sets of questions for which either near rationality or full rationality are more useful "as if" assumptions.

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# 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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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 Classics.

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 Classics 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 Classics 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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