3 Money-Income Causality—A Critical Review of the Literature Since *A Monetary History*

Phillip Cagan

3.1 Turns in Monetary Research

In the past three decades monetary research established a greater understanding and recognition of the role of money—a noteworthy achievement to which Anna Schwartz has been a major contributor. Earlier, in the 1930s, 1940s, and 1950s, the role of money had slipped far down the list of variables considered important in economic analysis and business commentary. Then in the 1960s, opinion began to turn. With Friedman and Schwartz's *A Monetary History of the United States, 1867–1960* (1963) leading the way, an outpouring of studies put new life into the traditional view of money as paramount. With the turn of opinion and the experience of the inflationary 1970s, few today any longer doubt the primary importance of money. Monetary economics continues to thrive on controversy, to be sure, but the difference is unmistakable: Now econometric models of the economy accord a central role to monetary variables, and business commentaries, far from ignoring monetary policy, focus on it. And the earlier barren disputes between Keynesians and quantity theorists graduated into more fruitful discussions about the proper conduct of monetary policy.

Lately, however, monetary research has turned again, and new studies claim that money has little or no effect on output and other real variables. What appeared so natural a marriage between monetarism and the theory of a stable competitive economy has produced a rather unnatural offspring—instantaneous price adjustments and rational expectations. This new view reaffirms the traditional monetary effects on

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prices but goes on to claim that changes in money affect *only* prices and perhaps only if the changes are exogenous. Other changes in money that may appear to influence real variables are dismissed as endogenous changes with no independent effects on the economy. In this view fluctuations in business activity are a real phenomenon with no monetary roots.

It is doubtful that anyone, even the practitioners of these models, firmly believes that the business cycle can be described as predominantly a real phenomenon. The supporting evidence is highly selective and limited. A less radical version of the new view admits that market prices may adjust sluggishly to monetary changes, so that *un*anticipated changes in money assumed to be exogenous do affect real variables. To what extent are monetary changes unanticipated as well as exogenous? The question is under debate. If most cyclical fluctuations in money are unanticipated and, even when endogenous, still affect prices and output, the new and older views would be compatible. But the two views interpret what is unanticipated differently. In the older view, long-run changes in monetary growth are absorbed by the price level, but all the short-run and cyclical changes play central roles in the business cycle; in modern jargon, these cyclical changes would all be unanticipated. The new view assumes, to the contrary, that the only unanticipated changes are very short movements, usually just isolated blips in the money series, and that all movements beyond one period are anticipated and immediately absorbed by prices. (Models with staggered wage contracts are an exception.) Although the length of a period in these models is usually unspecified, empirical work takes it to be one month or quarter. Nothing in the theory requires a period to be one month or quarter. But, unless one period covers the length of a business cycle, the new view and the older view clearly part company.

Much of the long-established evidence on the role of money comes from broad historical analyses. A broad historical analysis goes beyond a narrow dependence on time-series regressions. It draws on a wide-ranging examination of the institutional environment and economic events in a series of historical episodes. Statistical tests, including regression analysis, of these episodes may be run and prove useful, but they would be supplementary. Historical analysis relies on scholarship—a word on the way to losing its meaning in economics. It contrasts with the now common practice of gathering a handful of time series from a data bank and running them through a regression meat grinder.

The radical version of the new view in which money is endogenous and has no effects on the real economy is based on time-series regressions, and in particular on vector autoregressions, or VAR for short. This view and its evidence have made few converts. The Federal Re-

serve examines business conditions and decides policy, while the market watches anxiously in the firm belief that open market operations do something of utmost importance to the real side of the economy. If we accept the bulk of historical evidence as confirming important monetary effects on the real economy, contrary findings cannot be fully valid. And, if such contrary evidence is not valid, what kind of evidence in monetary research is acceptable and convincing? I want to address this wider issue about the validity of evidence from time-series regressions.

3.2 Endogeneity of the Money Supply

Central to most criticisms of evidence on monetary effects is the possible endogeneity of money. The empirical evidence that money, prices, and activity are related, now widely accepted, raises the question of the direction of influence. *A Monetary History* gives it major attention. Economic activity as well as policy decisions and institutional developments obviously affect monetary growth and fluctuations. The fact that money is significantly influenced by economic variables, however, does not itself imply the unimportance of monetary effects or justify downgrading their role. Friedman and Schwartz argued that the Fed could have prevented the decline in money in 1929–33 but failed to act. Stable monetary growth in that period would have changed the outcome of the business contraction. Even if the actual behavior of the money supply can be viewed as endogenous, it was possible for the Fed to have acted to stem the decline in money and to have alleviated the depression in output. There is an important difference between being endogenous with no independent effect and a mutual dependence in which policy can, when exercised, play a role.[1] Although those who deny monetary effects on output are surely not ignorant of this point, they continue to pay no attention to it. Regression methods foster this oversight because of their weak ability to disentangle a two-way dependence.

The issue of endogeneity has a long history in monetary controversies. It appeared in early banking theory as the commercial loan theory of credit or real bills doctrine (as named by Mints 1945), which held that if banks lent only short term to finance inventories on the way to market, the resulting quantity of bank deposits would be just right to produce a stable value of money. The attraction of the gold standard was that it produced an endogenous money supply that maintained a stable value of money in terms of gold. Much of the debate between monetarists and Keynesians turns on the endogeneity of money (Foster 1986). Thus critics of *A Monetary History* relied on endogeneity to counter the claim that money lies behind most fluctuations in activity.

Two initial prominent examples of this counterargument came in 1970 from Kaldor and Tobin.

Kaldor focused on the 1929–33 decline in the U.S. money stock, which he attributed to an independent shift towards the use of currency, because of increased payments for goods relative to assets and for labor relative to capital costs. This supposedly led to a large increase in the currency-deposit ratio which reduced the money supply. The only evidence Kaldor presented to support this explanation of the shift to currency was that the currency ratio did not return after the banking panic all the way to the low level of 1929. However, the continued high level of the currency ratio in the 1940s and 1950s can be attributed to other developments which I investigated in my work on the money supply (Cagan 1958, 1965). Kaldor might have argued with more force that the 1929–33 decline in the money stock did indeed reflect the banking panic, which in turn was produced by the contraction in business activity. If such an argument were valid, he could reach his conclusion that the money-income association simply showed the effect of income on money. A critical step in this endogeneity argument, however, requires evidence that the banking panic can be explained by the business contraction rather than other largely independent developments. Kaldor disregards all the studies of the genesis and role of banking panics in U.S. history. Business contractions do not fully explain panics. It follows that income did not cause money in these episodes, and the association reflects the reverse channel of influence.

There are two additional objections to Kaldor's type of endogeneity argument. First is the point made above: The fact that money may be endogenous does not prove or even imply that it has no reverse effect on activity. The money-income association reflects a changeable, two-way dependence. The importance of a two-way dependence is that money need not always be entirely endogenous. Policy actions can break the prevailing endogeneity, whereupon the existence of monetary effects means that they can be altered by policy to influence economic activity. Even if money were in some sense completely endogenous in 1929–33, therefore, the Fed's failure to stem the decline in money had devastating consequences for the economy.

The second point is that a two-way dependence cannot be confirmed by one observation. All we can confirm is a comovement, with indications of channels of influence possibly in both directions. Whether one or the other direction of influence dominates is never clear-cut in a single case. Friedman and Schwartz were well aware of this ambiguity, and devoted *A Monetary History* to analysis of a century of many different episodes. I also addressed the ambiguity in my book on the money supply (Cagan 1965). The comovements in money and business activity have persisted through a variety of cyclical episodes. In par-

ticular, the comovements appear as well in a group of severe cycles, some with and some without banking panics. The 1929–33 episode had a banking panic, which might appear to imply a one-way channel of influence running from a severe business contraction to panic to monetary decline. The evidence against that interpretation comes from the variety of monetary episodes. In 1914, for example, we had a banking panic but, thanks to a 1908 law authorizing the emergency issue of national bank notes, no large decline in the money stock and no severe contraction in activity ensued. Thus a single channel of influence of activity on money does not explain all the cyclical comovements in money and business. While the explanations for fluctuations in money vary, a persistent association between money and business remains: when money declines sharply, business activity also declines sharply, and not otherwise. How Kaldor and other critics could fail to grapple with this kind of evidence I can only ascribe to impatience to make an argument without examining the full range of historical evidence and without searching for interpretations that fit all of it.[2]

Tobin presented a theoretical model to demonstrate that the observed lead of monetary growth ahead of fluctuations in income does not prove causality. The model assumes that money is supplied endogenously at all times in response to changes in the demand for money.[3] Although money has no effect on income in the model, cycles in money turn out to lead those in income. This lead reverses the implication of standard models of money demand in which income affects the demand contemporaneously or with a lag and so moves ahead of cycles in a passive money supply. The reversed timing in Tobin's model occurs because of the peculiar nature of his money demand, as Friedman (1970) pointed out in his reply. Tobin's money demand, which follows convention in depending on transactions proxied by income and on financial wealth, unconventionally declines in business expansions because the usual increases in transactions demand are dominated by declines in wealth demand. Thus the wealth demand for money behaves countercyclically. How can that be? Tobin assumes, first, that in business expansions the increase in income raises tax revenues and reduces the government budget deficit and, second, that the issue of government bonds to finance the deficit falls off faster than corporate bonds are increased to finance more investment. During an expansion, therefore, the decline in the wealth demand for money produces a decline in the passive money supply ahead of income and gives the misleading appearance of causing a subsequent downturn in income—and conversely for cyclical contractions.

The timing in this model rests on fragile assumptions, however. If the government budget deficit is small, as it was for most of our history, a demand for money dependent on total financial wealth would not

produce a lead over income, even for the special case here in which the supply is entirely passive to the demand.

Tobin goes on to point out, as had Walters (1967) earlier, that if money demand depends only on permanent income, exogenous changes in the money supply will produce large immediate changes in current income, because such changes are required to change permanent income sufficiently to bring money demand into equilibrium with the new supply. Such a relationship produces leads in money ahead of income that are short relative to the leads in Tobin's preceding model in which money does not affect income. Hence the preceding no-effect model is more consistent with the evidence of a long lead than is the permanent income model, contradicting a causal implication of leads. But this hardly proves that money cannot affect income. It need only imply that, given the observed long lead of money over income, the demand for money is not determined exclusively by permanent income. The demand is very likely subject to other influences and to adjustment lags that attenuate the large immediate effect on current income. A quarter century of research on the demand for money equation confirms the role of other influences and of lags in addition to the role of permanent income.

Although Tobin's model does not illuminate the actual relation between money and income, his argument succeeded in fostering skepticism of timing leads in economic variables as evidence of a direction of influence. Skepticism certainly has its place in empirical work, but timing leads deserve a word in their defense. Granted that leads are not by themselves conclusive evidence of directions of influence, as Kaldor and Tobin maintain, dismissing leads as irrelevant goes too far when our knowledge of the economic system points to the relationship suggested by an observed lead. The everyday world of business forecasting shows little skepticism of leads and for good reason. If a lead is moderately long, it is most likely not affected by feedback and therefore is suggestive of a causal relation. Most economic as well as physical effects travel forward in time.

There are some dangers of misinterpreting the appearance of a lead, to be sure. Rates of change shift the appearance of a timing sequence, as illustrated by a sine curve in which its rate of change both leads and lags its level. Nevertheless, misleading relationships involving rates of change can be uncovered by careful examination of the data. Another problem much discussed in recent literature involves expectations. The public may anticipate future changes in a variable and affect other variables ahead of the anticipated change. Thus asset prices may change before the economic events responsible for the change occur. But even in financial markets, which are most affected by expectations, such leads are surely not very long, given the sorry state of forecasting. The implication of rational expectations that observed leads may be mis-

leading does not appear applicable to the nonfinancial sectors of the economy. A skepticism of leads based on rational expectations should not be carried too far.

3.3 Time-Series Regressions and Endogeneity

When empirical research became subservient to time-series regressions, largely since World War II, endogeneity of the variables on the right side of regressions was not thought to be a problem. Either causal sequencing was assumed by lagging the right-hand variables, or the use of annual data supposedly diminished any short-term feedback from the dependent variable. Time-series regressions came to be accepted as evidence of real-world relationships. (I take up the questionable validity of this acceptance below.)

The early study by Friedman and Meiselman (1963) called attention to the empirical importance of monetary effects on income by showing that money outperformed the driving variable of the Keynesian theory, autonomous investment. In extensions of their approach, the St. Louis equation confirmed the importance of money and the unimportance of fiscal variables (Andersen and Jordan 1968, Carlson 1986). Although fiscal variables sometimes rose above the floor of statistical insignificance, they never attained the importance they were supposed to have in the prevailing Keynesian theory. After a string of forecasting successes in the late 1960s, however, the St. Louis equation faltered in the 1970s and fell from favor. The inflationary 1970s nevertheless dramatically certified the importance of money for inflation and by implication also for output fluctuations.

The rational expectations developments in theory that rose to prominence in the late 1970s introduced new views of monetary effects. The new versions of the money-income regressions separated money into its anticipated and unanticipated components. Since the anticipated component is predictable, it must be endogenous to the economic system. This emphasis on anticipations presumes that the predictable, endogenous component of monetary growth is sizable and important.

Initial studies found that only unanticipated changes in money affect output, because prices fully absorb the anticipated changes. But then more sophisticated statistical tests reported that both components of monetary changes affect output. Apparently, empirical differences between the effects of anticipated and unanticipated monetary growth cannot be reliably established.[4]

These studies raise a question about the meaning and measurement of anticipated monetary growth. Clearly, after prices fully adjust to an increase in monetary growth, the temporary stimulation to output that occurred during the adjustment disappears. The only question concerns

how rapidly the economy adjusts. If the measure of anticipated monetary growth implies a faster adjustment than in fact occurs, the analysis will show an effect of the anticipated growth on output. But there will be some measure of anticipated monetary growth consistent with the actual pace of price adjustment so that only the residual unanticipated growth correlates with output. Controversy arises here because the new view of expectations implies much faster adjustments than most studies can verify. No doubt anticipated money has little effect on output in data measuring cycle averages, but the more recent studies show that it has such effects in quarterly and even annual data. The existence of monetary effects is not at issue here. The only issue, to describe it in the new terminology, concerns how rapidly economic behavior becomes "rational." Since behavior is "rational" for cycle averages, the issue is whether it should be labeled "irrational" for shorter time spans, thereby suggesting some kind of failure of market adjustments. I might note that, if the stochastic component of economic variables can be characterized by permanent and transitory random shocks, rational economic behavior will respond to the expected values of the permanent component by filtering out the transitory component via an "adaptive expectations" adjustment which can take some time (Brunner, Cukierman, and Meltzer 1980).

Although motivated to examine the difference between anticipated and unanticipated monetary growth, these studies can also be interpreted as introducing a procedure—formalized later in "causality" tests—to remove endogenous changes in monetary growth that reflect predictable influences on the money supply. The residual changes in money, assumed to be "unanticipated," thus show an effect on output free of spurious correlation. Since the anticipated component of monetary growth is by derivation endogenous, the finding of later studies (see note 4) that it has equal effects on output can be faulted for depending on a variable that lacks exogeneity. Indeed, if an important component of money is anticipated, its endogeneity calls into question all regression studies that claim to find monetary effects.[5]

The old argument that correlation does not imply causation, which received little attention while econometric research focused on developing more sophisticated techniques, has now become a major issue. Consider the standard St. Louis equation, which regresses changes in nominal GNP on concurrent and past changes in money and government expenditures. These variables are assumed to represent unidirectional effects on GNP. That assumption can be questioned by the likely feedback from GNP to the concurrent change in money. In an attempt to avoid this feedback, the concurrent monetary variable can be omitted from the regression, with the purpose of isolating the effect of monetary changes that precede the change in GNP and likely have

the major impact. Unfortunately, this does not necessarily eliminate the possibility of any feedback.

Feedback produced by expectations is one kind frequently discussed that may reach beyond concurrent movements. Thus policy may produce monetary changes in anticipation of future changes in GNP. Such feedback could, in principle, account for an observed correlation between GNP and earlier monetary changes. Nevertheless, this possibility seems far-fetched. Policy is generally not based on forecasts more than a quarter or two ahead. Even if this could be done accurately, the resulting relationship would not ordinarily produce a *positive* correlation with GNP, since policy is as often used to try to offset anticipated movements in aggregate demand as to reinforce them. To explain a positive feedback we must assume a channel working through the currency and reserve ratios, but it is hard to see why expectations should move these ratios ahead of developments in the economy. Feedback through expectations beyond the concurrent period, therefore, can surely be largely ignored, and in practice they usually are.

However, a potentially serious form of feedback can result from normal serial correlation in the money and GNP series. Suppose GNP affects money concurrently. The serial correlation in GNP will then transmit its concurrent feedback to monetary changes earlier and later in time. The resulting correlation with earlier monetary changes will give the appearance of their causal influence on GNP even if no such influence actually exists. To take a simple extreme example, suppose economic activity generated a concurrent cyclical fluctuation in the currency ratio and thence in the money supply. There is indeed evidence of such an effect (Cagan 1965). Cyclical fluctuations implant serial correlation in economic data. The fluctuations in economic activity will correlate with past changes in money, contaminating the evidence of the St. Louis equation. The correlation will, of course, also appear between activity and future changes in money, giving the impression that money is endogenous to past changes in activity as well.

The possible presence of this form of feedback still leaves open whether it can account for the association between money and income. When Friedman and Schwartz and I discussed this question of the direction of influence, we concluded that the money-income association could be explained only in part by the effect of activity on money, because the sources and nature of this effect varied considerably over time and could not account for the consistency of the observed association. The historical evidence indicated that only a strong monetary effect could account for such a consistent association over a long history of cycles.

Such historical analysis of the evidence has not satisfied a preference for formal statistical testing, however, and regressions have become

the accepted form of empirical analysis. The new "causality" tests emphasize the point that the existence of serial correlation and feedback compromise evidence based solely on conventional time-series regressions.

3.4 Testing for Causality

I see the motivation for the revolution in method introduced by Granger (1969) and Sims (1972) as residing in the feedback problem. To ascertain the effect of a variable that is partially endogenous, their causality tests remove the serial correlation in a pair of variables and then look for any remaining correlation between them. This "whitening" of the data eliminates the appearance of feedback that is carried by serial correlation backwards and forwards in the data. The basic idea is that only a cross correlation that survives the extraction of serial correlation in the individual series provides evidence of a direction of influence that can be identified by statistical means.

Much has been written in criticism of these tests because of the initial claim that they identified causal influences. Philosophical critics objected to the derivation of something so basic as a "cause" from unstructured statistical relationships (Zellner 1979). So at best the tests can claim only to look for exogeneity and temporal sequences. Econometric critics pointed to the problem of expectations and technical difficulties of prewhitening (Feige and Pearce 1979). As general propositions, the criticisms carry weight, but in application to the money-income relationship, we have specific knowledge to make judgments about expectations. Expectations are simply too weak and inaccurate to account for strong correlations over more than a short time horizon. As for the philosophical question of causality, economists do traditionally reach tentative conclusions from statistical time sequences about directions of influence when our theory gives a sound basis for expecting such influences.

The difficulties of prewhitening are another matter. Similar to the studies of unanticipated money, the tests of causality at first found an effect of money on output as well as prices. However, many subsequent studies reported mixed or negative results, particularly for foreign countries, apparently owing to differences in the method of removing serial correlation from the variables.[6] Although these studies are not all equal in technical sophistication and quality of scholarship, it is still not a simple matter for readers to determine the degree of validity of a particular study, much less a group of studies covering different countries and periods. Despite the number of clear indications of monetary effects the totality of this literature leaves the evidence subject to considerable doubt. As concluded by Feige and Pearce (1979, 532),

Since a variety of prefilters can be used to attain the objective of whitening regression residuals, we are left with the uncomfortable conclusion that an essentially arbitrary choice left to the discretion of individual researchers can significantly affect the nature of the economic conclusions derived from the test procedures.

3.5 The VAR Statistical Method

These causality tests disregard the possible influence of other variables on the two under examination—a major deficiency. This led Sims (1980b) to expand the number of variables included by means of a VAR method. To examine monetary effects, the VAR groups money with a set of other relevant variables, usually output, prices, interest rates, and sometimes bank credit, and regresses each one on lagged values of themselves and each other. The estimated coefficients of these regressions show the effects of the lagged values of the variables on each other, after the correlation between the lagged values of all the right-hand variables has been removed. In practice this method avoids the need for prewhitening the data because it effectively removes the serial correlation in each variable as well as the cross correlation of the right-hand variables with concurrent and past values of each other. What remains are statistically exogenous movements in the right-hand variables, attributable to events from outside the system of variables being examined. Only if these exogenous movements in money then correlate with subsequent movements in output, in which correlations of output with past values of itself and the variables other than money have also been removed, does uncontaminated evidence of a monetary effect exist.

In view of the problem of disentangling multiple influences, VAR is a legitimate and welcome attempt to deal with spurious correlation. It can help to confirm effects that are obscured by relationships among endogenous variables. Let us leave aside the various econometric objections (Leamer 1985, Cooley and LeRoy 1985). I want to raise objections of a practical nature that have received less attention. I have no quarrel with the purpose and the method, but rather take exception to the interpretation of the results. The VAR seems to me to be hopelessly unreliable and low in power to detect monetary effects of the kind we are looking for and believe, from other kinds of evidence, to exist.

A VAR test can answer two questions. First, how much effect does an exogenous disturbance in one of the variables of the system have on output? An unfortunate ambiguity arises here if concurrent disturbances in the different variables are correlated. In the absence of a theoretical structure, a sequential ordering of concurrent correlated

disturbances must be imposed arbitrarily. If we are willing to treat all of the concurrent disturbances in money as exogenous, the VAR can answer an important second question about the effect of monetary changes. This is given by the statistical significance of the lagged monetary variables in a regression of output on the lagged values of money and the other variables of the system.

In the widely noted article by Sims (1980b), money affected output in the VAR system but, in the post–World War II period, not after an interest rate was included. The interest rate in that period accounted for most of the effect on output previously attributed to money. In other studies a credit variable equals and sometimes surpasses the effect on output and prices of money. The findings of these studies are not all in mutual agreement, but the overall implication is that money responds endogenously to other economic variables and that its observed simple correlation with output and perhaps also prices may reflect a spurious correlation produced by other economic influences.[7]

3.6 Interpretation of an Interest-Rate Effect

Sims (1980b) explained the result that an interest rate knocks out the significant effect of money by extending the theory that business recessions reflect exogenous declines in the marginal product of investment: Businesses anticipate this decline in investment opportunities, which leads to a reduction in investment expenditures and eventually in output. In the meantime, before the anticipated decline in the marginal product of investment actually occurs, the decline in investment reduces the prices of new capital goods and *increases* the yield on existing capital as measured by the ratio of its still intact returns to its lower market prices. Interest rates follow this rise in capital yields. Such a rise in interest rates would, as Sims suggests, correlate negatively with the subsequent decline in output. But one wonders how much a three-month interest rate would be affected by the yield on existing capital goods. Would not the assumed decline in borrowing more likely succeed in lowering short-term interest rates?

The sequence of effects outlined by Sims puts an unbelievable weight on the ability of investors to foresee future changes in the marginal product of investment. To avoid this, alternative theories are available to account for an association between interest rates and future output. A rise in interest rates is widely thought to work to depress output. If money is not to play an active role, however, we must assume that the money supply adjusts passively to induced changes in its demand. In Sims's VAR results, money declines as interest rates rise, which he attributes to a passive response of supply to a decline in the demand for money balances induced by the rise in interest rates and fall in

output. This reasoning attributes movements along the supply curve improperly to movements along the demand curve. The subsequent fall in output could reduce monetary growth, but the earlier rise in interest rates would ordinarily increase monetary growth as banks expand to take advantage of higher rates and as the Fed partially accommodates the expansion. In any event, what then becomes of the decline in monetary growth that Friedman and Schwartz found to lead business downturns? Apparently the VAR relates the monetary decline to the concurrent rise in interest rates. To attribute that relation to a passive response of the money supply to a decline in its demand seems to me shaky. The widely accepted explanation of a *negative* relation between interest rates and money goes the other way, such that the monetary change induces the change in interest rates.

Sims does consider a possible monetary interpretation of his results, whereby interest rates reflect but precede exogenous changes in money. He conjectures that this might happen if policy changes in the monetary base have delayed effects on the money stock but affect interest rates immediately. Yet he rejects this alternative hypothesis, because the interest rate continues to dominate in his VARs even after the monetary base is substituted for the money supply.

Sims overlooked the alternative explanation subsequently pointed out by McCallum (1983). If the Fed targets interest rates, money becomes endogenous to the interest-rate target. Nevertheless, it is monetary policy, setting interest rates in response to market developments, that determines the outcome.

The historical importance of interest-rate targeting certainly raises doubts about a business cycle theory based on exogenous shocks to interest rates. But McCallum's point, while important, may not provide a full explanation. Interest-rate targets have not always determined monetary policy, particularly beyond very short-run horizons, so that many longer-run fluctuations in money have other explanations. No doubt many of these monetary fluctuations are also related through policy decisions and banking responses to market developments. Nevertheless, to attribute all or most monetary fluctuations to interest rates conflicts with other evidence. In the straightforward NBER analysis of cyclical turning points, monetary growth displays long and variable leads, while short-term interest rates have little or no leads on a positive basis over business cycle peaks (Cagan 1966). This apparent inconsistency with the VAR results calls for further study.[8]

3.7 The VAR in Practice

With the VAR results we have arrived at the anomalous situation in which the latest econometric techniques frequently find that money

does not affect activity, and perhaps not prices either, even though such effects are confidently expected in financial markets, by the monetary authorities themselves, and indeed by most economists. What is going on here? This is not a case of research ignoring some of the messy but unimportant details of reality. These results conflict with the major effects of money as widely perceived. A conflict between research results and widely perceived reality has never stopped economists, to be sure, but it should give pause for second thoughts.

The VAR literature is growing apace, and perhaps some plausible and generally accepted interpretation of these anomalous results will soon emerge. Much leeway exists for tinkering with the form of the equations. Economic research often gives birth to conflicting results which subject a line of research to controversy. But usually controversies can be understood in terms of differing hypotheses about economic behavior. For example, when money stood near the bottom of the totem pole of relative importance, nonmonetary explanations of the business cycle invoked theories of an investment accelerator, or the "animal spirits" of businessmen, or shocks to the consumption function—all capable of empirical interpretation and examination. In most of the VAR results, by contrast, one struggles in vain to decipher what they imply about economic behavior, inasmuch as the VAR method cooks the data beyond recognition.

To illustrate the VAR method I regressed real GNP on lagged values of itself, the GNP deflator, the commercial paper rate, and money, from first-quarter 1951 to second-quarter 1987. The variables are log levels, and each right-hand series is represented by eight lag terms. This is one equation of a typical four-variable VAR system to test for monetary effects. With the commercial paper rate excluded to form a three-variable system, the money terms are collectively highly significant, but with the commercial paper rate included the money terms have a much lower, though still significant, level of .035. No detrending was applied. Sims (1980b) found money to be insignificant in the latter four-variable system with monthly data using industrial production for real GNP and twelve lag terms. Stock and Watson (1987) resurrected the significance of money for the same monthly system, but for a shorter period beginning with 1960 and after detrending the data. The major differences in results depending on the data series used and on the time period covered illustrates a certain lack of robustness of VAR.

Figure 3.1 shows the residual terms of my quarterly real GNP regression with the money terms first included and then excluded. As shown by the amplitude of the residuals, the predicted values of both regressions lie mostly within 1 percent of the level of real GNP. The two residual series differ by only a small fraction of the amplitude of business cycle fluctuations. From the point of view of predicting GNP, the

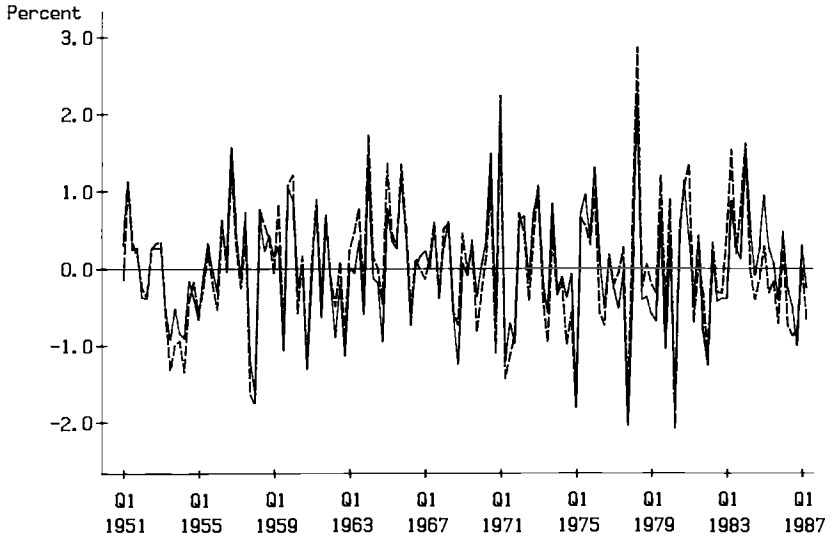

Fig. 3.1 Residual terms from regressions of real GNP on lagged values of real GNP, GNP deflator, commercial paper rate, and with M1 included (solid) and excluded (dotted), quarterly, 1951–1987 Q2. *Note:* Residuals from regressions described in fn. 9.

economic as opposed to statistical significance of including money here hardly pays its way.

What does the contribution of the money terms, buried in these computations, look like? Figure 3.2 gives their picture. It is based on the principle that the partial correlation of a dependent on an independent variable in a multiple regression is equivalent to the simple correlation between the residuals of the two variables from regressions on the other independent variables. Thus, in this case, the eight lagged monetary variables are regressed in turn on the other independent variables (including the other seven monetary variables, which excludes the one as dependent variable), and the residuals of these regressions are cumulated in a sum for each date which is weighted by the regression coefficients of the monetary terms in the full four-variable regression. The correlation of this series in figure 3.2 with the residuals of the regression excluding the money series (the dotted series in figure 3.1) is equivalent to a test of the combined significance of the eight money terms.[9]

Figure 3.2 shows the contribution of the monetary terms to real GNP in the VAR regression, as just described, and figure 3.3 shows the quarterly rates of actual monetary growth and the tendency of their fluctuations to lead business turns. The VAR by comparison attenuates

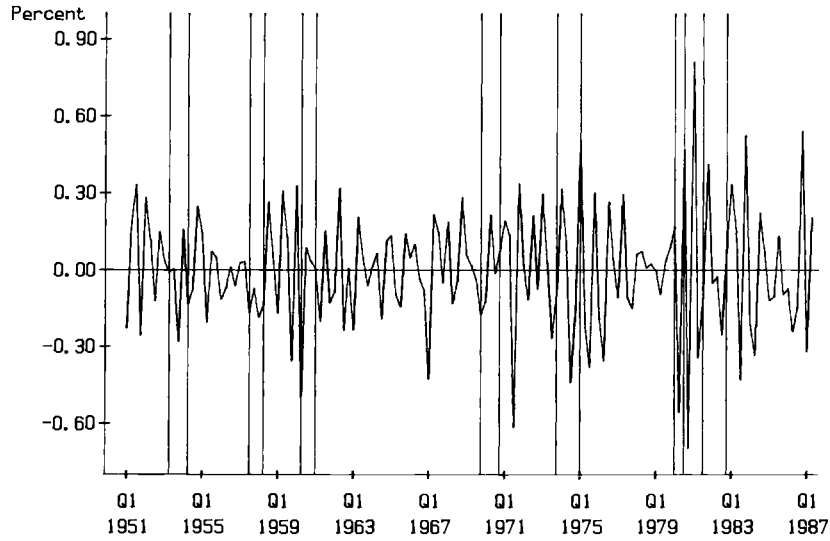

Fig. 3.2 Contribution of monetary terms to real GNP in a four-variable VAR system, quarterly, 1951–1987 Q2. *Note:* Shorter periods between vertical lines represent NBER business recessions. Weighted sum of residuals derived as explained in fn. 9.

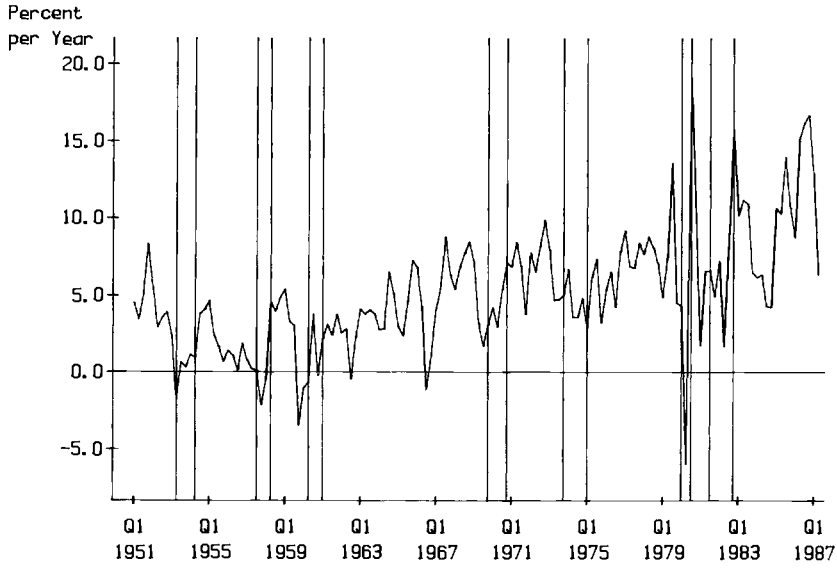

Fig. 3.3 Monetary growth (M1), quarterly, 1951–1987 Q2. *Note:* Shorter periods between vertical lines represent NBER business recessions.

and largely eliminates the cyclical fluctuations in monetary growth (and in real GNP as well) before testing for their correlation. Note the relatively small amplitude of fluctuation of the monetary contribution in figure 3.2. (The comparison of amplitudes between figures 3.2 and 3.3 is admittedly made difficult by the difference in units. Figure 3.2 shows quarterly deviations on the order of one-third of a percent, largely offsetting over each of several quarters. Figure 3.3 shows annual rates of change, with cyclical fluctuations on the order of roughly 4 percentage points, that is, 4 percent in a year.) Thus VAR looks to the noncyclical, very short-run movements in economic variables to identify their cross effects. This critically limits the evidence and poses serious problems for identifying monetary effects.

3.8 Deficiencies of the VAR Method

VAR originated as a welcome response to the largely neglected problem of spurious correlation among economic variables. But are its results trustworthy? Its application to money points up three problems that the generally voiced econometric criticisms gloss over. These are the linearity of regressions, the complex interaction between money and interest rates, and the elimination of most of the cyclical fluctuations in money. The first two are not problems confined to VARs, as will be noted.

Linearity governs all regression analyses and may often be a reasonable approximation to a moderately nonlinear reality. But for monetary effects it is not reasonable and cannot be made so by transformations to logarithms or to first differences. The limitations of linearity apply to St. Louis–type equations as well as VARs. Monetary episodes vary substantially in timing and cannot all be represented by the same values of parameters and fixed lag patterns. This seems clear from historical analysis.[10] Perhaps the variability in the timing of monetary effects can be represented by a complex dynamic system, but certainly not by a three- or four-variable VAR or by any system of equations we are now capable of specifying. Moreover, the timing varies from stage to stage of business cycles, so regressions fit to subperiods covering a few cycles do not avoid the problem. Thus a fixed lag pattern estimates a varying lag pattern as an average, which reduces the estimated correlation between money and the variables it affects. The extent of the reduction could be substantial, possibly to the point of not showing a significant effect. Money illustrates the theoretical point that a linear *in*dependence does not rule out a nonlinear dependence of some kind. (See Snowder 1984.)

Consider the variation in the lag of monetary effects in recent cyclical downturns. In 1966 and 1969 monetary growth (M1) peaked about a

half year before business activity did. The monetary peak preceded the business peak at the end of 1973 by about a year and preceded the sharp decline in output in October 1974 by almost two years (though the peak in output came earlier and was followed by a plateau). By comparison, the lag in monetary effect was just a few months at the early 1980 business peak precipitated by the imposition of credit controls, and again just a few months at the mid-1981 business peak which appeared short because the escalation of inflation had earlier reduced monetary growth in real terms. One average lag pattern does not capture the variety of these episodes. The variations in monetary relationships reflect their complexity and not changes in "monetary regimes," as that term has recently been used.

As a second problem with VARs that include an interest rate, the varying interaction between money and interest rates can hide monetary effects. Interest rates can at times influence monetary growth positively because of a response by bank lending and monetary policy, and at other times a tight or easy monetary policy affects interest rates negatively. These interactions make both money and interest rates partly endogenous. In addition, the cyclical pattern of interest rates conforms to business activity, so that the rise of rates in expansions correlates negatively with the subsequent decline in activity. If the movements in interest rates that are exogenous to monetary growth have a more systematic cyclical pattern than do the movements in monetary growth that are exogenous to interest rates, the VAR will show a closer correlation between interest rates and business activity than between money and activity. Yet this finding would give the wrong impression of the monetary process at work. And there would be no way to determine the true relationship by linear regression methods.

As a specific example take the 1969 episode. The Fed reduced monetary growth drastically beginning in April.[11] In due course a credit crunch developed in September producing sharp increases in interest rates, and business turned down in December. Given a fairly consistent relation between interest rates and business activity and the timing variability of monetary effects, the VAR analysis will find support for the role of interest rates in the 1969 episode and downplay the decline in monetary growth. But that misrepresents the paramount role of monetary policy in this episode.

A third problem with VARs is that the particular technique for dealing with spurious correlation eliminates important monetary changes. By removing all serial and cross correlations from economic series, VAR reduces them to exogenous movements and looks for correlation between these movements in each pair of series. But these exogenous movements are little more than isolated blips in the series, which in monetary growth have little effect on GNP. The financial system filters

out the effect of monetary blips. Only changes in monetary growth that are maintained for an extended period of time affect business activity. These extended changes in monetary growth, however, exhibit serial correlation and, despite their variable lags in affecting output and prices, tend to be correlated with cyclical movements in other economic variables. The VAR accordingly eliminates the correlated movements in money as endogenous to the economic system. Thus does this technique give new meaning to the old cliché of "throwing the baby out with the bath water." Only the monetary changes that have little effect on GNP survive elimination in the VAR process.

Will money be more prone to the emasculation of the VAR process than other economic variables? It is likely to be. The interrelationships of the financial system produce comovements in money and interest rates and other credit variables that appear to be more systematic than the varying effects of money on business activity. The latter effects will therefore have low power in VAR tests.

3.9 General Observations about VAR and Time-Series Regressions

Despite the above objections to VAR, the problem it addresses of endogeneity and spurious correlation cannot be waved aside. Indeed, the VAR methodology derives from the accepted treatment of endogenous independent variables in conventional time-series regressions. The conventional treatment of endogeneity in economic models has essentially assumed it away. In econometric estimation the lagging of independent variables supposedly makes them exogenous. As the VAR method indicates, this is not valid. Instrumental variables, widely used to avoid spurious correlation with the residual error term, are generally not exogenous to the system; they may reduce some spurious correlation but do not eliminate it. In reality nearly all the important effects in the economy reflect movements in variables that are basically endogenous to the system. Even monetary and fiscal policy, which are typically treated as exogenous, basically are not.[12] Their endogeneity is sometimes handled by introducing reaction functions, based on a quadratic tradeoff between desired levels of inflation, unemployment, interest rates, and exchange rates. These functions have not worked. It is not possible to describe macroeconomic behavior solely in terms of exogenous variables. The economy is essentially a closed system. No doubt the weather is exogenous, but that is no help where agriculture plays a minor role. Even the sudden increase in oil prices by OPEC in 1973 and 1979 was not entirely exogenous and, in any event, cannot by itself fully explain the subsequent economic developments. Long-run movements reflecting resource and productivity growth can perhaps be treated as largely exogenous, but not their cyclical

movements. Since regressions require exogenous independent variables, ordinary time-series regressions cannot provide valid evidence of economic effects.

As a specific example, endogeneity problems plague estimation of the money demand equation. In different studies of the standard equation a variety of specifications and explanatory variables are found to be significant. Not all of them are likely to be. Some of them are proxies for others, which means that the basic relationship cannot be identified precisely. Interest rates and real income are treated as exogenous, which supposes that changes in the money supply do not affect real income or interest rates. While possibly valid for the long run, these assumptions cannot claim validity for the short run. Some studies (Goldfeld 1973, Artis and Lewis 1976) claim comparable results whether the money demand equation treats the interest rate as a dependent or independent variable. The former is questionable, however, because real money balances have only a short-run influence on interest rates, quite different from the long-run relation, and because real income remaining on the right-hand side cannot be treated as independent of interest rates or real money balances. Furthermore, a study by Mehra (1978a) found that the interest rate and income are not exogenous to money when all are measured in nominal terms, yet the popular Koyck-lag adjustment, which gives better results when money balances are measured in nominal rather than real terms (Liang 1984, Fair 1987, but see Goldfeld and Sichel 1987), thus suffers from lack of exogeneity. Finally, when joint estimation of a supply equation takes account of the effect of interest rates on money supply as well as demand, the interest rate is either treated as an exogenous variable or is assumed to be determined by the demand and supply of money (Teigen 1964, Brunner and Meltzer 1964, Gibson 1972), ignoring the effect of investment demand. The VAR method tells us that these estimation procedures are invalid and the results highly questionable.

To be sure, any sweeping rejection of regression analysis needs qualification. While cross-section data also suffer from spurious correlations, these are often amenable to treatment. Time-series regressions sometimes give acceptable estimates of the parameters when the precise specification of an equation and the exogeneity of the independent variables can be taken for granted, as in some micro industry studies. Where interrelationships play out in a short time horizon and feedback is minimal, regressions can identify influences. Thus some work on asset price movements and relationships appear legitimate. And strict random walk hypotheses can be tested by time-series regressions.

In general, however, time-series regressions sit on a shaky foundation. Explanatory variables are employed that do not meet the statistical

criterion of exogeneity. The well-known prevalence of correlation among virtually all economic variables and the ease of finding statistical significance in almost any specification of economic equations raise warning flags. It is increasingly difficult to take the myriad runs of computer printouts seriously, except as simple descriptions of the data. Can anyone have confidence in regression methods when numerous studies of the same relationships for the same period give contradictory results? Time-series regression studies give no sign of the scientific ideal of converging toward the truth. If the call to take the "con" out of econometrics (Leamer 1983) led to a new practice of checking the robustness of regression equations to changes in specification, the result would be that few time-series regressions would stand up to a wide-ranging sensitivity analysis. Other variables can invariably be found to reduce any given partial correlation. Understandably, analyses showing lack of robustness heretofore have not appeared in articles offered to journals or accepted for publication. (See the proposal by Feige 1975.) The VAR method calls our attention to the deficiency of present practices.

The existence of endogeneity and its corollary of multicollinearity and spurious correlation has long been recognized as a problem for economics as a nonexperimental science. The early warning of Yule (1926)—"Why do we sometimes get nonsense correlations?"—has long been simply ignored, though the problem is receiving increasing attention (Granger and Newbold 1974, Leamer 1983, Lovell 1983, Los 1986). The VAR methodology tells us to dismiss any apparent effects of variables that cannot be certified as exogenous. Its solution is to isolate exogenous "shocks" to the variables. VAR depends on the exogenous movements being sufficiently strong and numerous to show up after their extraction from the original data. It can identify a relationship among economic variables if some indication of it remains after the systematic movements in the time series are removed.

But, while the VAR method can help to confirm economic effects, its results, when often negative, are not conclusive. Money has difficulty passing VAR tests, yet by all other indications it plays an important role in business fluctuations. If the movements in money identified as endogenous by VAR and extracted from the data series were instead eliminated by monetary policy, would the economy be the same? Hardly anyone thinks so. Since regression analysis cannot evaluate the effect of these monetary changes, it fails in its principal purpose.

Although skepticism of time-series regressions has become widespread, the practical consequences are widely resisted. The purpose and limitations of the VAR method argue for less dependence on macroeconomic regression fitting of all kinds, but too much capital has

been invested in econometric time-series techniques for this to happen, at least very soon. As practiced today, empirical macroeconomics could not survive without them.

Yet empirical research need not be so dependent on time-series regressions. It can proceed as it did before the computer made multiple regressions so cheap and plentiful. Economic research can reemphasize the kind of careful historical analysis that we honor Anna Schwartz for at this conference. In such research the mutual relationship among variables can be studied, and the subtle differences among historical episodes can provide clues to the channels of influence at work. The pseudo-precision of regression analysis once seemed to promise statistical tests of monetary effects, but now this appears to have been an illusion. In contrast to the questionable, conflicting, and obscure results of time-series regressions, general historical analysis presents understandable evidence and, though lacking the finality of formal statistical testing, usually converges to a consensus on the facts and often also on an interpretation of the facts. I see an indication here and there in the journals of a return to empirical studies where regressions may supplement but do not dominate a broad analysis. Anna Schwartz's monetary research (1987) will not, it may be hoped, be the last of a fine tradition of scholarship.

Notes

1. Suppose monetary changes are induced by the variable Z, so monetary effects on GNP are attributable to Z. The question is, if the effect of Z on money could be altered, would Z still have the same effect on GNP? If monetary effects exist, the answer is no, despite the fact that money is otherwise endogenous when determined by Z.

2. Kaldor carried his argument of an endogenous money supply to the amazing conclusion that money has no effect on anything, apparently including prices. He does accord a minor role to "liquidity," as described in the Radcliffe Report (1959), but not to any ordinary concept of money. In his model of the economy the price level appears to be tied to labor costs without a monetary anchor. For a similar view see Davidson and Weintraub (1973). Fortunately, that point of view no longer has much of a following.

On the importance of the range of evidence in *A Monetary History*, see Hirsch and de Marchi (1986).

3. Buiter (1984), in support of Tobin's argument, notes that endogeneity of the money supply hides its effects in the data. He concludes that changes in policy regimes are needed to validate monetary effects. Cottrell (1986) defends Kaldor with an argument also based on the endogeneity of money.

4. The emphasis on unanticipated monetary growth began with studies by Barro (1977, 1978, 1981) and Barro and Rush (1980) of U.S. data. Dutkowsky and Atesoglu (1986) verified that the model held up in postsample forecasts to

1984. Confirmatory support that only *un*anticipated changes in money affected output was provided by Leiderman (1980) for the United States, Wogin (1980) and Darrat (1985a) for Canada, Attfield, Demery, and Duck (1981a, 1981b) and Bellante, Morrell, and Zardkoohi (1982) for the United Kingdom, Darrat (1985b) for West Germany, Blejer and Fernandez (1980) for Mexico, and Attfield and Duck (1983) for a cross section of eleven countries. Kormendi and Meguire (1984) found that monetary shocks affected output in forty-seven countries while the effects faded with time, which by suggesting no effect in the long run was consistent with the Barro thesis of the neutrality of anticipated monetary changes. Similarly, Haraf (1978) found that monetary surprises affected inventories and orders before aggregate output, but appeared to have no long-term effect. Brocato (1985) verified the U.S. output effects of unanticipated money, though he ignored the anticipated component. Grossman (1979) concluded that only unanticipated changes in policy affected U.S. output, on the assumption that policy determined aggregate demand as proxied by nominal GNP. Enders and Falk (1984) found that only unanticipated money affected output for an individual industry, U.S. pork production.

Many other studies cast doubt on these results, however. Barro (1979b) could not confirm his U.S. results for three foreign countries: for Mexico, anticipated money affected output, and for Brazil and Colombia, all monetary effects were weak, though the inability to fit adequate money supply functions precluded clear conclusions. Anticipated money dominated the effect on output in Japan (Pigott 1978). Small (1979) revised Barro's measurement of unanticipated money for the United States and found its effect to be no greater than that for anticipated money, though in reply, Barro (1979a) defended his results. Froyen (1979) and Sheffrin (1979) also reported that anticipated money affected U.S. output. Darby (1983) found an extremely weak effect of unanticipated money on output for seven foreign countries and only a small effect for the United States post–World War II. In a study of the pre–World War I U.S. data under the gold standard (Rush 1985), neither monetary component affected output. Kimbrough and Koray (1984) got mixed results for Canada and negative results for the United States on output effects of unanticipated money; in addition, they could not reject an effect of anticipated money for either country. Similarly, in Darrat (1985c) deflated aggregate money dominated unanticipated money in unemployment effects for three European countries. Demery, Duck, and Musgrave (1984) could find only qualified support for unanticipated money effects on real variables for West Germany. Korteweg (1978) showed that anticipated money growth correlated with inflation in the Netherlands, but could find no effect of money on output at all. Boschen and Grossman (1982) and Boschen (1985) reported no differences in output effects of observed and unobserved money, which contradicts the supposed importance of anticipations.

Another series of studies revised Barro's estimation procedure and found the same effect for anticipated and unanticipated money. These studies of U.S. data were Cuddington (1980), Makin (1982), Gordon (1982), Mishkin (1982a, 1982b, 1983), Driscoll et al. (1983a), Merrick (1983), Carns and Lombra (1983), Sheehey (1984), Sheehan (1985), and Cecchetti (1986). Cecchetti (1987) also reported similar results for eight foreign countries, as did Darrat (1985e) for Italy, and Hoffman and Schlagenhauf (1982) for the United States and five foreign countries except for Canada, where anticipated money did *not* affect output. But Askari (1986) found that output was affected for Canada using a more complicated statistical test. In a trivariate autoregression of U.S. real GNP, prices, and money, McGee and Stasiak (1985) found that anticipated money affected real GNP in the short run, though not the long run. For post–

World War II U.K. data, Garner (1982) and Driscoll et al. (1983b) found that anticipated money affected output in the short run, as did Demery (1984) when allowance was made for expected changes in velocity; Driscoll, Mullineux, and Sen (1985) rejected the neutrality (that is, no effect on output) of anticipated money and rational expectations in a joint test; and Bean (1984) rejected neutrality for anticipated M1 but not £M3.

To avoid the endogeneity of anticipated money, Rush (1986) showed that only shocks to the monetary base affected unemployment in the United States from 1920 to 1983 (except for the 1930s), and the estimated effect of the anticipated base had the wrong sign and presumably had no real effects.

5. Nor is the interpretation of *un*anticipated money free of ambiguity. Pesaran (1982) points out that, if the money supply were determined passively by interest-rate targeting, the correlation of the derived unanticipated monetary growth with output could be interpreted as consistent with Keynesian types of nonmonetary effects on output. I find this Keynesian interpretation doubtful because the Barro (1977) money supply equation implies a negative effect of government expenditures on output. Nevertheless, possible spurious correlation opens up these results to alternative interpretations. Rush (1985) interpreted the lack of correlation between U.S. money and output in pre–World War I in his study as due to the endogeneity of money.

6. In addition to Sims's study (1972), evidence of a monetary effect was also found for the United States by Neftci and Sargent (1978), Brillembourg and Khan (1979), and Hafer (1981). Two-way effects between money and income were reported by Hsiao (1979b) in one test and only a weak relationship in a second test, and by Thornton and Batten (1985), who also found a short-run one-way effect of the monetary base, as did Mehra (1978). Paulter and Rivard (1979), however, found one-way effects of all the monetary aggregates but no effect of the base. Ciccolo (1978) reported a monetary effect for the U.S. interwar period and a weak two-way effect for the post–World War II period. For other countries: Huffman and Lothian (1980) found mixed results for the United Kingdom—two-sided effects for post–World War II (as did Mixon, Pratt, and Wallace 1980), a weak monetary effect for 1870–1914, and a strong monetary effect for 1837–70; Hsiao (1979a), supported by Osborn (1984), found two-way effects for Canada, as did Komura (1982) for Japan, Layton (1985) for Australia, and Wachter (1979) for money and prices in Chile; and unidirectional monetary effects were reported by Sharpe and Miller (1975) and Jones (1985) for Canada, von Hagen (1984) for West Germany, and by Darrat (1985d, 1986) on prices for three major OPEC, and three North African, countries.

Decidedly mixed results subject to the prefiltering method or the test used were reported by Sargent (1976), Schwert (1979), and Kang (1985) for the United States, and Kamath (1985) for India. Falls and Hill (1985) found for the United States that money "caused" prices but not output for 1972–79, and the reverse of those effects for 1979–83, apparently owing to the change in policy regime. Christiano and Ljungqvist (1988) claim that insignificant monetary effects for the United States reflect an inappropriate first-differencing of the data.

Negative results of causality tests for money were reported by Barth and Bennett (1974) for Canada, qualified by Auerbach and Rutner (1978); Feige and Pearce (1979) and Geweke (1986) for the United States; Pierce (1977) for monetary effects on U.S. retail sales; Williams, Goodhart, and Gowland (1976) for U.K. output in post–World War II, though they did find a monetary effect on prices; Mills and Wood (1978) for the U.K. gold-standard period (1870–1914); Van Hoa (1981) and Weissenberger and Thomas (1983) for West Ger-

many; and Parikh (1984) for Indonesia. Although money and prices appear to be causally independent in post–World War II Spain, C. and F. Hernández-Iglesias (1981) argue that bidirectional causality is difficult to detect without large changes as in hyperinflation.

7. An early VAR study by Sargent and Sims (1977) found that money affected unemployment in the short run, and Sims (1980a) confirmed this result for GNP in the United States but not in West Germany, where the effect was weak and in the long run nonexistent. For pre–World War I, Dwyer (1985) found a monetary effect on prices in the United States but none in the United Kingdom. Eichengreen (1983) subdivided the U.K. pre–World War I period and found effects of the monetary base after 1870 but not before. For both the United States and United Kingdom pre–World War I, Huffman and Lothian (1984) found evidence of monetary effects on real income, but also cross-country feedback on money consistent with the specie-flow mechanism. Their VAR included an interest rate, as did a trivariate test showing monetary effects on income and an interest rate for India (Laumas and Porter-Hudak 1986).

However, monetary effects often disappear when an interest rate is included and partially do so when a debt variable is included. Sims (1980b) found no effect in the United States for post–World War II data with an interest rate, nor did Hsiao (1982) for Canada. U.K. postwar output depended on monetary policy when proxied by M1 and an interest rate, but not when proxied by M1 and M3 without the interest rate (Bean 1984). For post–World War II U.S. data, Myatt (1986) reported that money does not affect prices, and Fackler (1985) that neither money nor a debt variable affects quarterly real GNP or prices, though possibly money and debt do so indirectly through the interest rate. Litterman and Weiss (1985) reported that a measure of the real interest rate is the exogenous source of changes in money, prices, and output. Eichenbaum and Singleton (1986) showed that these results could be sensitive to statistical technique: A monetary effect on real GNP was significant when they removed a linear trend from the data, but was not significant when they used first differencing. In a more elaborate model adjusted for trend, Bernanke (1986) found a significant monetary effect, as did Stock and Watson (1987) using the same data series as Sims (1980b). It is difficult to diagnose how differences in statistical procedure affect all these results.

Friedman (1983a, 1983b) initiated the VAR study of credit or debt and monetary effects. He reported that money and debt shared comparable effects on real income. McMillin and Fackler (1984) tested a range of financial aggregates including money and found that all except bank credit affected income, though there was feedback from income particularly to money. No consensus has emerged about debt. Porter and Offenbacher (1983) found the effects of money to be somewhat stronger than those of debt, but found the results to be sensitive to the measurement and ordering of the variables. King (1986) also found money stronger than a bank loans variable and the results sensitive to ordering. Bernanke (1986) used a structural model to order the variables and found bank loans and money to be of equal importance. In Granger-causality tests (Hafer 1985), both M1 and debt affected GNP, but debt made no marginal contribution after the monetary effects, and the feedback of GNP was greater on debt than on money. (In related earlier studies of policy indicators, debt and money had comparable effects [Davis 1979] and debt effects on GNP that were independent of M1 largely reflected the liquid asset component and, in the 1970s, largely M2 [Cagan 1982]).

8. A problem also arises with VAR results attributing a major exogenous influence to bank credit. Bernanke (1986) argues that shocks to the supply of

bank credit affect the amount rationed to bank-tied borrowers, who reduce spending and contract aggregate demand. If banks are unable to lend to their borrowers, however, a given total supply of credit will reach other borrowers in the economy. If the total supply declines, one reason could be a reduction in monetary growth. If the VAR removes correlated movements in money and credit, it eliminates important monetary effects on aggregate demand from consideration.

The emphasis on rationed bank credit resurrects a largely forgotten controversy of the 1950s (see Bach and Huizenga 1961).

9. The VAR test determines the level of significance of the monetary variables in the OLS regression

$$(1) \quad \log O_t = \text{const.} + \Sigma a_i \log O_{t-t} + \Sigma b_i \log M_{t-i} + \Sigma c_i \log P_{t-i} + \Sigma d_i \log R_{t-i} + \varepsilon_t$$

where O is real GNP, M money stock (M1 version), P GNP price deflator, R commercial paper rate, and ε the residual error; $a, \ldots, d$ are regression coefficients, and the summation Σ runs from $i = 1$ to 8 (eight lagged quarters).

The two series in figure 3.1 are the residual values of the regression with the money terms excluded (solid) and included (dotted).

The money series in figure 3.2 was derived as follows. Each lagged value of money in the above regression was regressed on the other independent variables. That is, eight regressions were run, one for each of the lagged log $M(k)$, $k = 1, \ldots, 8$, in (1). Each of these series was regressed on

$$(2) \quad \text{const.} + \Sigma f_i \log O_{t-i} + \Sigma g_i \log P_{t-i} + \Sigma h_i \log R_{t-i} + \Sigma j_i \log M_{t-i} + \mu_t(k)$$

where f,g,h,j are regression coefficients and the summation Σ runs from 1 to 8 quarters, except for the monetary lags which omit the lag term that corresponds to the dependent variable in each regression.

Figure 3.2 shows the sum for each quarter t of $\Sigma \hat{b}_k \mu_t(k)$ for $k = 1, \ldots, 8$ where $\hat{b}_k$ is the estimated coefficients of b_i in (1). The simple correlation of this series with the residuals from regression (1) with the money terms excluded (the dotted series in figure 3.1) is equivalent to an F-test that all $b_i = 0$ in (1).

10. The literature lacks a consensus on the length of monetary lags but indicates considerable evidence of their variability. See Rosenbaum (1985).

11. This was clearly an exogenous decision of the authorities to combat inflation, not a passive response of the money supply to a decline in interest rates or economic activity. See my discussion in Cagan (1979, esp. pp. 113–18).

12. Goldfeld and Blinder (1972) claim that treating endogenous policy variables as exogenous produces little bias in estimates of relevant economic parameters. But see Crotty (1973) and Sims (1982). It would be useful to put this claim to a general test, because I find it highly questionable as a general proposition.

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Comment

Robert H. Rasche

Another paper presented at this conference proclaimed that the "dark age of vector autoregressions" has dawned (Bordo 1987). Cagan's paper assumes the role of the Center for Disease Control or the Surgeon General. It warns there is a pernicious, communicable, even fatal threat to our profession at large during these dark days, and that RATS (*R*egression *A*nalysis of *T*ime *S*eries) spread this plague!!

Phil Cagan's review of the literature of the past two decades on money-income causality is critical, exhaustive, thoughtful, and thought-provoking. It is truly a tribute to Anna Schwartz and a masterful counterattack on the various accusations against a fundamental proposition of *A Monetary History,* namely, that there is "an influence from income to money over the business cycle, yet . . . the main influence both secularly and cyclically runs from money to income" (Bordo 1987, 5). However, the traditions and requirements for discussants set by our profession are not satisfied if I stop at this point. Moreover, as a practitioner of the time-series techniques that are the object of the Cagan counterattack, I am unwilling to run up the white flag and passively surrender.

Counterattack: Hypothesis

The fundamental approach of the paper is the juxtaposition of two competing hypotheses. Cagan's maintained hypothesis throughout his review of the money-income causality literature is: "Are time-series

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regression techniques capable of detecting and measuring the impact of exogenous monetary disturbances on real output when the available time series reflect monetary regimes that permit endogenous changes in the money stock?" My reading is that Cagan uniformly rejects this hypothesis, but rejects in favor of what? The alternative hypothesis in his analysis is "Are historical analyses capable of detecting and measuring the impact of exogenous monetary disturbances on real output when the historical monetary regimes have permitted endogenous changes in the money stock?" The major conclusion of his review is not just the rejection of the hypothesis of the adequacy of time-series regression techniques. It is the rejection of that hypothesis and the *acceptance* of the alternative hypothesis of the adequacy of historical analyses *on the basis* of the demonstrated inadequacies of the time-series techniques.

My evidence for this characterization of Cagan's argument is as follows: First, that the question of the (partial) endogeneity of monetary disturbances is not at issue is found early in the paper: "The empirical evidence that money, prices, and activity are related, now widely accepted, raises the question of the direction of influence. *A Monetary History* gives it major attention."

Second, there is the rejection of the hypothesis of the adequacy of time-series regression techniques: "Regression methods foster this oversight because of their weak ability to disentangle a two-way dependence"; "The new 'causality' tests emphasize the point that the existence of serial correlation and feedback compromise evidence based solely on conventional time-series regressions"; "The VAR seems to me to be hopelessly unreliable and low in power to detect monetary effects of the kind we are looking for and believe, from other evidence, to exist"; and "Since regression analysis cannot evaluate the effect of these monetary changes, it fails in its principal purpose."

Third, the acceptance of the alternative hypothesis of the adequacy of historical analyses: "In such [historical] research the mutual relationship among variables can be studied, and the subtle differences among historical episodes can provide clues to the channels of influence at work"; and ". . . general historical analysis presents understandable evidence and . . . usually converges to a consensus on the facts and often also on an interpretation of the facts."

Repulse: Weaknesses of the Attack on Time-Series Analysis

In my view, there are two problems that invalidate Cagan's conclusion. The first is that even if we accept the evidence presented as a conclusive demonstration that time-series regression techniques are inadequate for the task at hand, it does not follow that historical anal-

yses are adequate. It is quite possible that both research strategies are "unreliable and low in power to detect monetary effects of the kind we are looking for," as it is also possible that both approaches can reliably detect such monetary effects given the way that central banks have operated historically. Cagan asserts (or assumes) that his rejection of time-series regression analysis validates the historical approach. There is no evidence in his review of the reliability or power of the historical analysis approach.

The second problem is there are occasions when there is a circularity in the argument against time-series regression techniques. In several places in the review, it is asserted that time-series analysis fails because it does not support known propositions. A closer examination reveals that the known propositions are the conclusions of previous historical research. Thus, the conclusion that the time-series techniques are inadequate is sometimes conditional upon the adequacy of the historical research approach. Should the technique or conclusions of the previous historical analysis be faulty, then the alleged failure of the time-series analysis may be no failure at all.

An example of this circularity of reasoning is the quotation above in which VAR analysis is dismissed because it does not detect "monetary effects . . . we believe, from other evidence, to exist." The only other evidence alluded to in the entire review is the conclusions of historical analyses. Later, the conditional nature of the rejection of time-series techniques is clear: "Linearity governs all regression analyses. . . . But for monetary effects it is not reasonable. . . . Monetary episodes vary substantially in timing and cannot all be represented by the same values of parameters and fixed lagged patterns. *This seems clear from historical analysis*" (emphasis added); and "Money has difficulty passing VAR tests, *yet by all other indications* it plays an important role in business fluctuations. . . . Since regression analysis cannot evaluate the effects of these monetary changes, it fails in its principal purpose" (emphasis added).

Regroup: Some Alternative Sources of Concern Regarding Specific Time-Series Techniques

Cagan specifically addresses four different types of time-series analysis: (1) the "Post Hoc Ergo Propter Hoc" criticism of timing analysis; (2) models that attempt to distinguish the effects of "anticipated" versus"unanticipated" monetary growth; (3) bivariate "causality" models; and (4) vector autoregresions (VAR). Extensive discussions of the first and third of these approaches appear in the existing literature and, for the most part, his review summarizes the existent criticisms of these time-series methods. Most of the new criticisms

address the "anticipated" versus "unanticipated" money models and the VAR approach to time-series modeling. There are some interesting ideas in these two areas that deserve further elaboration.

The most important comment in Cagan's paper about the anticipated/unanticipated money literature is that "These studies raise a question about the meaning and measurement of anticipated monetary growth." The motivation for such models derives from a theoretical literature in which agents know the structure of the economy, possess information on the history of various "policy variables," and form expectations "rationally" based on this information set. Under these conditions, *and* if prices adjust to clear markets continually, then the conclusion follows that anticipated money growth, in the sense of the best forecast of money growth from the available information, does not affect real output.

A well-known class of models (Fischer 1977; Taylor 1979) relaxes only the assumption of market clearing and generates the result that both anticipated and unanticipated money growth have nonzero effects on real output. The different conclusion arises because in the latter models there is a difference between the best forecast of current money growth and the expectation of the inflation rate, given the available information set.

Relaxation of the assumptions on the information set in different ways would undoubtedly further muddy the waters. We need to remember that before the monumental work of *A Monetary History* and *Monetary Statistics,* data on the stock and growth rate of money in the United States were not available weekly from local newspapers (or even Federal Reserve statistical releases) at virtually no cost. *Banking and Monetary Statistics,* initially published in 1943, provides data only at semiannual intervals. As late as 1959, the "Details of Deposits and Currency" table in the *Federal Reserve Bulletin* provided data only on a last-Wednesday-of-the-month basis. As Friedman and Schwartz note in *Monetary Statistics:* "Comprehensive coverage of all banks at annual dates, did not become available until 1959, when the Federal Reserve System published its compilation of these reports in *All Bank Statistics,* and even this compilation goes back only to 1896" (1970, 212). The availability of information on economic statistics, which we so easily take for granted, is a recent phenomenon and one to which Anna Schwartz has made significant contributions.

None of this literature has progressed beyond the assumption of *known* reduced-form coefficients models. In reality, the best information that agents can possess is unbiased estimates of the true reduced-form coefficients that are subject to sampling error. Indeed, given the review that we have at hand, this is probably a heroic and inaccurate

assumption about the true state of an agent's information set. Once stochastic coefficient reduced forms are part of the information set, then the meaning and measurement of anticipated money growth becomes considerably more complex. The effects of such models on the specification error of the available empirical studies of anticipated and unanticipated money growth remain undetermined.

The major thrust of Cagan's objection to VAR analysis is that the "VAR method cooks the data beyond recognition," and that monetary effects are not adequately represented by linearity (or log-linearity). The problem with this criticism is that it comes close to arguing that, in one way or another, monetary events are unique. If this is the case, then we will never untangle the interdependence of money and income, since as Cagan is aware, "a two-way dependence cannot be confirmed by one observation."

A more substantive criticism of commonly practiced VAR analysis is that it is incapable of answering the question of what effect money has on real output under the acknowledged nature of historical monetary policymaking. Given acceptance of a two-way dependence of money and income, the residuals or innovations studied in typical VAR analyses—which Cagan feels do not demonstrate the well-known effects of money on income—are just not the appropriate residuals. Sims (1980) clearly acknowledges that after construction of the moving average representation of a VAR system by the now conventional orthogonalization approach:

> The residuals whose effects are being tracked are the residuals from a system in which contemporaneous values of other variables enter the right-hand sides of the regressions with a triangular array of coefficients. (p. 21)

The structure of such a system is a Wold causal chain, not the structure of the economy that is of concern to Cagan or to *A Monetary History*. Given the restrictions that Sims imposes on the data, the moving average representations are not unique, i.e., the economic system of concern to Cagan is not identified. *The issue* in the interpretation of the VAR results is *identification*. It would appear that a skillful practitioner can use both historical analyses and time-series analyses to shed some light on the issue of a two-way dependence between money and income. To quote Sims (1980):

> We may sometimes be able to separate endogenous and exogenous components in policy variables by careful *historical analysis*, in effect using a type of instrumental variables procedure for estimating a structural relation between policy variables and the rest of the economy. (p. 12)

The message is that more information is available to analysts than that contained in economic time series. This additional information is important and can supplement time-series analysis in ways that overcome the inherent limitations of staring myopically at the time-series entrails. This message has a much higher marginal product than one that says we must abandon time-series analysis altogether and return to the fundamental scholarship of historical analysis. Certainly it is important to deplore the mechanical manipulation of economic time series, but it is also important to recognize that historical analyses and time-series analyses are not mutually exclusive, nor are they substitutes. Indeed, the highest quality "scholarship" in our profession combines the two approaches to produce lasting contributions to the advancement of our understanding. Anna Schwartz's contributions rank with the best in this latter tradition.

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General Discussion

KOCHIN offered an alternative explanation for the dominance of interest rates in a VAR incorporating the money stock, an interest rate, and real output. He argued that, in an efficient market, interest rates absorb all the information available in the monetary series simply because interest rates are anticipating whatever information is available about future money. That information will be in the interest rate as it becomes available, which may be before it is incorporated in the money supply.

Thus, for example, if there is a projection in October 1989 of faster growth in the monetary base and higher inflation in the 1990s, and that information becomes available in October 1987, interest rates would go up during October 1987, even if the monetary base has not yet increased. So the rise in interest rates in 1987 will, according to the VAR analyst, cause the inflation of the 1990s.

POOLE, in a similar vein, pointed out that the interest rate, as a speculative price in the markets, filters out the noise in the money data. People in the markets are responding to the systematic part of the monetary influence, as well as other things. So it is not surprising that the interest rate drives out money.

CAGAN argued that the movements in interest rates are more systematically related to real GNP than is money. Otherwise they would both have an equal chance of playing that role.

MCCALLUM reiterated his 1983 explanation for Sims's 1980 finding of little influence of monetary policy innovations in a VAR containing money, interest rates, and real output. He had argued that, if the monetary authorities are using the interest rates as an instrument, you would expect the interest rate to show up as a *better* indicator of monetary policy surprises than the money stock.

He raised an objection to Cagan's argument. Interest rate targets, Cagan stated, have not always ruled monetary policy, particularly beyond short horizons. According to McCallum, that statement seems to confuse the difference between an interest rate instrument and an interest rate target—a target being an objective of policy and an instrument being something to do with operating procedures. Interest rate instruments have been in effect throughout the postwar period, which is the period of concern here. Even during the 1979–82 regime, indirect interest rate instruments were used. Furthermore, his argument presumes that this obtains only at short-run horizons, that it is only over a period of a month or six weeks that these things are fixed and held rigid.

STOCKMAN warned against rejecting a statistical technique because the results are not all uniform. He pointed out that not all applications of the VAR approaches have treated data in exactly the same way. For example, there is a substantial difference in the results that people get if they take linear time trends out of the data rather than if they take growth rates. And that can be explained because taking the growth rates of a time series amounts to applying one filter to the data, while taking linear trends out is another filter altogether, and it makes sense that using different filters leads to different results. When we plot the squared gains from these filters, we find that taking the growth rate of a time series leaves more in at the higher frequencies while taking out linear time trends leaves less in at higher frequencies and more in at

lower frequencies. The differences in the answers that economists obtain with different filters, therefore, gives us information that we could use for subsequent statistical analyses and for construction or evaluation of theories. If detrending versus taking growth rates makes a difference for the correlation of two economic variables, then we would like a theory that predicts this difference.

He then expanded on why using different filters may make a difference. He pointed out that if you plot the squared gain from these filters against frequency, you can compare the results of applying d-log filters to the data to taking out a linear time trend. In this kind of plot, the squared gain of a linear time trend is flat, except at a zero frequency. But the squared gain of the d-log filter rises, starting out lower at high frequencies and getting higher at low frequencies. The relationship between any two economic variables can differ across frequencies—there are high frequency relationships and low frequency relations. By using the d-log filter, one is looking mostly at the higher frequency relationships. By taking out a linear time trend and then looking at the detrended series, one is looking less at the higher frequency relations and more at the lower frequency relations than is the case with the d-log filter.

He argued that presumably economic theory should tell us something about whether there are some short-run relationships or long-run relationships between these variables. He described research he has done with Marianne Baxter where they found that the correlation between foreign and U.S. industrial production was about the same under pegged or floating exchange rates when they took linear time trends out of the data, while the correlations between growth rates of industrial production were lower under flexible exchange rates. That, he argued, suggests that short-run (high frequency) correlations are lower under flexible rates, while longer-run correlations are unaffected. And that might be explained by greater national monetary autonomy under floating rates, combined with a short-run, but not long-run, effect of money on industrial production.

McCallum made the point that the use of VARs is just a technique of *descriptive* statistics—that running a VAR is comparable to calculating the mean of a series of data, or calculating the standard deviation—it is just a slightly more complicated descriptive statistic. One is not going to get any understanding from any descriptive statistic unaided. It must be combined with some sort of understanding that relates the descriptive statistics to the characteristics of the system.

Brunner described a critique of Granger-causality tests in a paper by William Schwert published in the Carnegie-Rochester Conference Series (1979). Schwert argued that the tests are badly misnamed. They do not test causality but actually test incremental information. They

simply reveal whether the addition of some variables raises our information level about the future value of the dependent variable. Brunner then gave an example to show that this is very different from a causality test. Suppose we construct a quantity theory world with specific stochastic processes controlling money, velocity, and output. The optimal forecast of inflation is determined by a distributed lag on past rates of inflation. Adding money to the regression yields nothing. All the relevant information is already contained in the past rates of inflation. But this does not mean that money has no causal effect. We know by construction that money substantially influences the ongoing inflation.

CAGAN reiterated the main point of his paper—that a lot of the theoretical objections to VARs did not focus on what the real problem was, namely, that VAR looks for a very rigid relationship between money and output that does not exist. By not finding that relationship, one should not jump to the conclusion that there was no effect.

In reply to Rasche, he accepted that there are a lot of problems with general historical analysis. You have to persuade by an accumulation of evidence, interpreting different episodes. This he believes is the best we can do. By contrast, econometrics provides formal statistical tests of these propositions. In his opinion, formal statistical tests where the *t*-statistic tells you yes or no is not sufficient to determine whether you have an effect. His paper is thus a protest against such use of econometrics.

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