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2 Anna Schwartz's Perspective on British Economic History

Forrest H. Capie and Geoffrey E. Wood

A word that constantly crops up in a description of the work in which Anna Schwartz has shared is "monumental." It is certainly a word that comes to mind when considering *Growth and Fluctuation of the British Economy, 1790–1850: An Historical, Statistical, and Theoretical Study of Britain's Economic Development,* volumes 1 and 2 (1953), Anna Schwartz's first contribution to British economic history. Coauthored with Arthur Gayer and W. W. Rostow, this work was conceived, and largely researched and written, in the 1930s. It runs to over one thousand published pages and almost as many additional pages available on microfilm. It is a testament to its depth that it is still amongst the first works turned to in any investigation of the British economy in the first half of the nineteenth century. When the book first appeared in 1953 it was accepted as by far the most thorough study of the subject. That is still true. Arthur Gayer was the senior partner in the exercise; the idea grew out of his doctoral dissertation completed at Oxford in 1930. Anna Schwartz was, however, involved in the study from its beginning (1936) and did most of the basic data collection and statistical analysis. Walt Rostow joined the team in 1939 and was responsible for most of the historical narrative in part 1 of volume 1 and the general analysis in part 1 of volume 2. It was originally planned in five volumes, but wartime delays, and probably rises in publication costs, resulted in it appearing as two volumes only, with remaining material available on microfilm.

Growth and Fluctuation unearthed, gathered, and collated every available statistical series on the British economy, and constructed

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several new ones—around two hundred series in total, all at least on an annual basis and some monthly: data on output, prices, trade, finance, labor, and other variables. It is an extraordinary work.

Amongst the most notable data contributions were new price series for domestic and imported commodities. The Gayer-Rostow-Schwartz (GRS) indices are still widely used and the names run together so easily in this connection that every student of British economic history is familiar with them.[1]

The data are subjected to the full panoply of the National Bureau techniques (put through the Bureau's "special mincing machine" as one reviewer put it, although it was not a Bureau product), and set within the framework that shaped so much work of the 1930s and 1940s: specific cycles and reference cycles are measured and chronicled. Each cycle is explored on a year-to-year basis. Interest is concentrated on cyclical fluctuations and on price movements. A distinction is made between major cycles—"cycles marked in their expansion phases by large increases in long-term investment" (1953, 33)—and minor cycles, usually associated with monetary changes, and, in the upswing, with export growth.

When the book appeared in 1953, twenty years had elapsed since its conception and more than a decade lay between its completion and its publication. In spite of that it was, as noted above, hailed as the most detailed study of the subject. Arthur Gayer died before the work was published and in an addendum to his preface, Rostow and Schwartz took the opportunity to reflect that if the work were being written in the 1950s, it might well be done differently. (The irony is that the hinted-at revised interpretation would have had a Keynesian slant.) But one good reason for leaving it as it was, they argued, was that the data and the statistical analysis both held and were worth publishing as they stood. Other researchers could then use these and draw different conclusions.

Rostow and Schwartz noted, in a brief survey of the economic history literature of the 1940s, that the general historical interpretation had not changed greatly. In economic theory, though, there had been some major developments, notably Keynes's analysis of short-period income fluctuations, and different approaches to long-run dynamic problems. The authors accepted that the changes in income analysis could well have influenced the character of their interpretation. However, the interpretation that had been written in the 1930s they left alone. In this interpretation they did not distinguish between real and nominal interest rates, and they accepted that central banks could control interest rates and that these in turn could influence private business investment spending, which in turn affected aggregate income.

Interestingly, Sir Alec Cairncross (1954), not noted for his emphasis on monetary explanations, in his review of the first edition was critical of the lack of consideration given to monetary causes:

> These minor cycles of the late 'twenties and early 'thirties seem quite plainly to have petered out because of dear money. Indeed, it may be ventured as a generalization that the surest sign of an approaching depression was a rise in the market rate of discount above the yield on consols. Throughout the entire period between 1834 and 1842 there was only a single year (1838) in which the market rate of discount averaged less than the yield on consols. How then can it be said that the money supply was ample? (p. 562)

Aside from criticisms of interpretation such as this, the work was generally enthusiastically received as a "remarkable work of collaborative scholarship" (Imlah 1953).

Evidence of the continuing demand for the book was the appearance of a new edition in 1975. Rostow and Schwartz again took the opportunity to review some of the intervening literature, though only a fraction of the great explosion in the literature impinged directly on the original themes. Interest had shifted in the 1950s and 1960s, as Rostow and Schwartz had in 1953 predicted it would, to long-run growth.

However, on the interpretation of the facts in the original volumes a gulf, described in the 1975 preface as an "amicable divergence of view," had opened up between the two authors. Anna Schwartz indicated that she had, in the light of recent theoretical and empirical research—much of it her own work with Milton Friedman—revised her view of: the role given to monetary policy, the interpretation of the behavior of interest rates, and the difference between *relative* price changes and changes in the *general* price level. She gives a succinct summary of the state of monetary theory in the mid-1970s, and drawing on empirical findings, mainly from post-1865 U.S. experience, suggests that British experience in the period 1790–1850 should not have differed greatly. However, the lack of aggregate monetary data for the period made it impossible to establish this contention, although the type of evidence already alluded to by Cairncross suggested she was correct.

The emphasis on relative prices and the cost explanation that was offered in the original study by GRS failed, she now insisted, to account for price level movements: "Changes in relative prices tell us nothing about changes in the price level" (p. xii). Rises in costs were associated with poor harvests and other difficulties in supply conditions: "These factors are highly relevant to the price of one item *relative* to the price of others. But, for movements in general prices, the cost explanation begs the question of the source of the autonomous increase or decrease in costs'' (p. xii). Thus, while the views that were offered in the original study were not attributable to Anna Schwartz alone, she wanted, in 1975, explicitly to distance herself from the cost-push explanation. Rostow, however, stated that he held to the views contained in the first edition, and did not accept the distinction between individual and general price analysis. He maintained that the monetary system played a passive role and that this was always true—and, therefore, inevitably so—for Britain in this period.

In spite of these surely substantial differences of view, the preface goes on to say that while the alternative analytical framework would not affect the validity of the basic research, it would alter ''the cast'' of the analysis, and ''would entail revision of some of the conclusions'' (p. xiii). Of course, consideration of the long-run trend in output would not be affected since real output growth is affected only by real factors.

When Anna Schwartz was summing up her reservations in the preface, she effectively threw out a challenge to others to take up the different analytical framework and reinterpret the original data. This has not yet been done in any systematic way, perhaps for reasons set out below in the discussion of business cycles.

The criticisms of the book that would be made today are understandable, and made only with hindsight. First, the data. For a work with ''growth'' in the title we would immediately think in terms of the rate of change of an aggregate measure of output. However, as yet this still does not exist for this period and discussion is limited. Furthermore, there are gaps in the data that have arisen from the change in emphasis. For example, we would invariably seek an export/output ratio in any discussion of Britain's economic experience since the external account was always important. (Though again, the lack of an aggregate output series precludes any such construction.) Secondly, advances in econometrics mean that much of the statistical analysis could now be greatly refined. (It is here that the argument for the publication of the original data reemerges. If they were readily accessible, a lot more work could be done by the young computer historians.) And thirdly, advances in theory would lead to the investigations of quite different lines of enquiry, often along lines indicated by Schwartz in her prefatory remarks to the 1975 edition.

2.1 The Business Cycle

As noted above, one of the reviewers, Cairncross (1954), suggested that money should have been given a larger role in the explanation of business cycles than it had been given by GRS. At first sight it may seem surprising that it was not, surprising not simply in the light of Schwartz's later work, but in view of the possibilities of the time. By

1914 all the principal ideas and most of the data on the trade cycle had been set out.[2] Juglar (1889) had provided the basic information and statistical analysis of time series, estimating periodicity and identifying turning points. And by the 1920s a monetary theory of the cycle was certainly a prominent explanation. For example, Hawtrey (1913) saw the business cycle as "a purely monetary phenomenon." Changes in money were a sole and sufficient cause of changes in income. Furthermore, an inspiration for GRS was Mitchell, and he had set out in his organizing survey of problems and materials (1927) the state of the main lines of explanation, including an extensive discussion of monetary factors.

However, it is important to remember that a prominent view of objective enquiry at this time demanded that the data be gathered first, that measurement should then follow, and that theory be brought to bear later—indeed, in part, was suggested by the first two.[3]

Given the closeness of this project to Mitchell (the authors talked to Mitchell and Burns frequently), it is not surprising that the essential elements of GRS's implicit model are those found in Mitchell's writings. Mitchell's views were essentially eclectic, but the general ideas are as follows.

Beginning in a depression, conditions are produced that are favorable to an upswing: costs have fallen and that allows profit margins to begin to rise, inventories are low and require boosting, banks are more willing to lend as their reserves are rising, and so on. These give rise to a cumulative increase in income, increased investment, bank lending, etc., until finally, under a "slow accumulation of stresses," this process is brought to an end and then there is a downward cumulative process. This account was one that gave the main emphasis to businessmen and their expectations, and emphasized the complexity of business conditions and the inability to see the future at all clearly.

Keynes narrowed the emphasis to a few macro variables: income, consumption, saving, and investment. Mitchell, in contrast, did not accept that the key to fluctuations was to be found in a few aggregate variables; and yet, they were not entirely opposed for, in the end, Keynes believed that incentives to invest depended "on the uncontrollable and disobedient psychology of the business world" (1936, 317). Keynes saw cycles deriving from fluctuations in investment which in turn came from fluctuations in the marginal efficiency of capital, and that depended upon changes in the rate of *expected* future returns on current investment. In booms there is an overoptimistic view of future returns and that leads to investment increasing too rapidly.

In contrast, for those like Hawtrey who stressed monetary factors, the primary actor in the process was the banking system. For Hawtrey it was the banks expanding credit that led to the increased money supply and hence increased total spending. Lower interest rates followed, inducing firms initially to expand inventories, leading to rising income and on, cumulatively, to the point where banks halted lending as their reserves ran down too low.

Work on money and the cycle has of course now been done by Anna Schwartz (in collaboration with Milton Friedman in, for example, "Money and Business Cycles" [1963a]), but for many years the role of money in business cycles was the subject of little attention. This has changed recently. There has been a flood of literature on business cycle theory.[4] Much of this theory has been rather different from that which was general, although certainly not universal, at the time Gayer, Rostow, and Schwartz were at work.[5] But GRS inclined rather, as indicated above, to the competing line in cycle theory, when they did come to theory, that derived from overinvestment. The central idea, arising from the fact that producer goods industries were more affected by the cycle, was that changes in the production of consumer goods (which came from changes in demand) gave rise to greater changes in production of producer goods: "the acceleration and magnification of derived demand." It is worth commenting here that it was changes in demand that were the source of all explanations. This had to be the case since all the variables (money, prices, output, etc.) moved in the same direction. Only changes in demand could produce this. And yet, there is a difficulty here: while this holds for a view of the nominal cycle, the whole direction of the work was cast in real terms, i.e., it was business activity and what business was doing that was of interest.

The basis of the theory in GRS lies in the long gestation period for fixed investment and the secular growth of demand. At the start of an upswing there is excess capacity. Hence (and also because a sustained rise in profits is awaited) there is a long delay before investment rises. But the catching up of demand with capacity is going on all the time. Profits start to recover, supported by the rise in exports. The latter characterized all the major upswings, but is clearly not part of the theory of the cycle.

At length, investment starts to take place. Additions to capacity are by their nature in large units, and there is a long gestation period. As all firms operate under a similar stimulus, all firms respond, taking no account of each other's actions. There is thus excess capacity, which slows investment in the *next* recovery. But that is not what brings the recession. The turning point depends not on a fall of profits resulting from the introduction of new equipment, but "on the consequences for costs of relatively full employment. . . ." (1953, 557).

Note this is not a multiplier/accelerator theory, for there need not be (although there can be) a fluctuation in final demand. Money does enter, but only through the observation that interest rates generally rose in booms.

One of the reviewers, R. C. O. Matthews (1954), raised some objections to this theory, but these objections did not hint at the developments to come. Rather they were concerned with the behavior of individual time series relative to the cycle as a whole. Of most general interest are his comments on speculation. Many of the booms were characterized by "manias" (1954, 106). Such "manias" only took hold when trade was prosperous, and the collapse of speculative bubbles then started the downturn. And as "any purely speculative movement is a highly unstable phenomenon" (p. 101), even a modest rise in interest rates would be sufficient to prick the bubble and end the boom.

Gayer, Rostow, and Schwartz place great importance on exports as starting booms. There was a strong upward trend to these (due to the expansion of world population and rising real incomes, together with a secular fall in Britain's export prices).[6] Matthews (1954) expressed some reservations about this, but concluded that the data did not permit a clear-cut conclusion. In summary, then, GRS had an accelerator-based theory of the cycle, with secular demand growth combining with investment's gestation period, the lumpiness of investment, and investors' lack of foresight, to produce a cycle.

Keynes was not solely responsible for the focus on the role of investment and saving in cyclical fluctuations. Indeed, there was a long history that embodied this approach, dating at least from 1900, and GRS can be said to have drawn at least implicitly on that. Expectations had also long played a part in business cycle theory, having been present in some form in the earliest theories. They are present in GRS in a stronger form, but it is fair to say that only after the 1950s and 1960s did they come to dominate.

The change to the kind of theory now in fashion has its origins in a suggestion by Hayek (1933) summarized in the following quotation:

> The incorporation of cyclical phenomena into the system of economic equilibrium theory, with which they are in apparent contradiction, remains the crucial problem of trade cycle theory. (p. 33n)

Robert Lucas ([1977] 1981) has sought to meet this challenge. In his 1977 paper, he set out some common characteristics of business cycles, and on the basis of these concluded that "business cycles are all alike." He then discussed modelling this regular pattern. Keynesian models of the cycle—the multiplier/accelerator is a good example—had, he argued, no role for money and had households, for arbitrary reasons, *choosing* "to supply labor at sharply irregular rates through time" (p. 218). This, as Lucas observes, is puzzling, for since the recurrent pattern of cycles allows rational forecasting, subjective probabilities can be identified with actual probabilities. Hence, quantity movements should be explained as "optimizing responses to movements in prices" (p. 222). Here Friedman and Schwartz come in. Secular movements in prices are due to secular movements in money. "This fact is as well established as any we know in aggregative economics. . . ." (p. 232). The evidence Lucas cites for this is Friedman and Schwartz (1963a).

Since money triggers price movements, money must be at the heart of the cycle. How, in view of the weak money-price link in the short run? *Because* of the weakness of that link; because the theory set out rests on the difficulty of telling relative from general price movements.

This line of attack is currently out of favor, not on theoretical grounds, but because of what is believed to be the implausible assumption that individuals cannot tell relative from absolute price changes, even a substantial time after they have occurred. McCallum (1986) notes that this assumption may be plausible for earlier periods, and suggests this contrast may help explain the fact that the amplitude of business cycles is smaller than it used to be. The problem with this interesting conjecture is that it may fit the United States, but certainly does not fit the experience of at least some other countries.

Business cycle research currently follows two main directions: real business cycles and sticky price business cycles. Real business cycle theories play down monetary influences. Money has no significance for real output and employment in the strong form of the theory. (There is also what may be called a "weak form," in which both money and technological shocks affect output.) This conclusion is reached partly by unhappiness with the Lucas theory, partly by evidence that seems to suggest no effect of money on output, and partly by the Nelson-Plosser (1982) argument that most fluctuations in aggregate variables are in the trend component, and that should be unaffected by monetary shocks. But, as McCallum (1986) says, the evidence is not persuasive. On statistical grounds McCallum rejects the Nelson-Plosser argument. Tests which claim to have ruled out any impact of money on output have ignored the money supply process. (They assume base control in a period of interest rate setting.) Hence the positive support for the real business cycle falls away and it is left depending on unhappiness with the Lucas-proposed alternative.

There is a clear link between the work of GRS and the most recent work on business cycles. GRS's study was of growth and fluctuations, and an implication was that there was great difficulty in separating trend and cycle. That difficulty was explicitly recognized in the 1950s and 1960s. Cyclical forces affect the trend, and the kind of trend that is eliminated affects the resulting fluctuations. As Robert Gordon put it, "it is better not to think of business cycles as fluctuating around any 'normal' level. . . . There is no justification for regarding the secular movements as a path of moving equilibrium, around which cyclical fluctuations take place" (1961, 256).

Nevertheless, despite this link we have come a long way from Gayer, Rostow, and Schwartz (and their reviewers and immediate successors). It should be observed, though, that in one respect they are well ahead of current developments. Real business cycle theories, driven by technological shocks, should surely have a close connection, as GRS posit, between growth and cycles. In fact real business cycle theories do not as yet do so; most business cycle models describe an economy with a stationary mean. But on the other hand, within these new models individuals cannot persist in their mistakes. The kind of repeated errors by businessmen that GRS relied on are not now allowed in formal models. Further, in many models, money has a dominant role, not the modest one they hinted it may have had at the upper turning point. It is also noteworthy that these advances in theory are advances in a special sense. We know more only in the sense that we know we know less than we once thought we did; there is no longer a widely accepted theory of the cycle around which historical research can be organized. Although Gayer, Rostow, and Schwartz did not point to this conclusion, it was led to by their demonstration of the repetitive nature of cycles.

Something that remains to be said, and has not to our knowledge been said before, is that this work deserves to stand as perhaps *the* pioneering work in British, and possibly any, econometric history. There used to be a sport of trying to identify the birth of the cliometric movement, and since much of the activity went on in the United States, Conrad and Meyer's *The Economics of Slavery in the Antebellum South* (1958) usually featured, while some British names such as Cairncross and Matthews sometimes got a mention. *Growth and Fluctuation* predates them all. It is the kind of blend of history, statistics, and economic analysis that is still aimed for by those who think of themselves as "new" economic historians. It gathers the essential data, subjects them to the most sophisticated statistical techniques available, and employs economic analysis in their interpretation. As Victor Morgan said at the time: "The present volumes certainly form one of the most solid and useful exercises in the interpretation of history by means of economic analysis that have yet appeared" (1954, 860).

2.2 Monetary Trends

Some thirty years after first working on British economic history, Anna Schwartz returned to the subject. The main product of this return was *Monetary Trends in the United States and the United Kingdom, 1875–1975* (1982), co-authored with Milton Friedman.[7] As the authors explain in their preface (pp. xxviii–xxix), a draft of the book was "submitted tentatively to an NBER reading committee" in 1966. This reading committee suggested broadening the coverage "to include the United Kingdom and perhaps other countries." Note that *Monetary Trends* was not published until 1982. Again to quote Friedman and Schwartz, "We understand how much of a start the earlier three volumes [i.e., *Monetary Statistics* (1970), *Monetary History* (1963b), and Phillip Cagan's *Determinants* (1965)] had given us for the United States analysis of this area. . . ." They express regret over the delay, and say that ". . . in retrospect we probably made a mistake in accepting the reading committee's suggestion."

That judgment is one with which it is hard to agree. *Monetary Trends* was received with considerable excitement in Britain, almost immediately manifested by a number of lengthy reviews, a conference at the Bank of England, and working papers prompted by some of these initial reactions. And perhaps most important of all, the volume raised a large number of questions about British monetary history.[8]

It is useful now to consider the questions raised in a little detail. To do so we examine first the reviews, and then the Bank of England conference and its aftermath.

2.2.1 The Reviews

In his review, David Laidler (1982) paid particular attention to the findings for the United Kingdom. He did, however, start with some perceptive remarks about the statistical method and the underlying model, remarks which are worth sketching here because they remind us of the so-called disequilibrium money tradition pioneered in this century by Clark Warburton (1950), in which Friedman and Schwartz can be interpreted as working. (We say "so-called" because a better name would focus on the distinction between short- and long-run equilibria, and would also avoid confusion with the buffer stock approach which has been called a disequilibrium money approach by Charles Goodhart. The above name does, however, seem to be the generally accepted one.) Laidler places the book in a long-established intellectual tradition. He remarks initially that *Monetary Trends* can be viewed as summing up the National Bureau's work on U.S. monetary history and opening up such work in the United Kingdom. It sums up work on the United States because many of the preliminary questions that should be answered before conclusions for that country are finally established were answered in one of the previous three volumes. It opens up work on the United Kingdom because of the ground it clears and because of the large number of subsequent questions it prompts. Before turning to these and to why they emerge, it is worth noting that Laidler, in commenting on the econometric techniques used, writes that "The econometrics *per se* never relies on anything more complex than least squares estimators. . . . The ratio of human intelligence to computer time that has gone into the production of this book is, that is to say, refreshingly high" (p. 251). This is important in that some of the initial discussions paid great attention to econometrics, and, as Laidler suggests might be the case, the outcome was far from being that a higher ratio of computer time to intelligence would unequivocably have been a good thing.

The book *is* about trends, and is to some degree concerned with establishing the long-run validity of the quantity theory's neutrality propositions. This theory contrasts with a view of the world where the demand for money is always unstable, disturbances principally originate on the real side of the economy, and the price level is either given exogenously or endogenously determined by a Phillips curve, and is, in any event, unimportant in producing equilibrium. This is in sharp contrast to the view that a major driving force in the economy is a gap between desired and actual real balances, and that this gap, although it may produce real effects, produces only transitory ones and permanently affects only the price level.

The analytical model of this book is not Keynesian—interest rates do not move to clear the money market—nor what has become known as new-classical (now a shorthand term for continuous market clearing). Rather it is one in which an excess supply of nominal (and initially real) money drives nominal income. We can for a time be off the long-run demand curve for real cash balances. Despite that, a money demand function can be estimated. The basic unit of observation in *Monetary Trends* is the cycle phase, and that is sufficiently long for transitory disturbances to work themselves out. (This is an important point, to which we turn below.) The demand for money function estimated on cycle-phase–by–cycle-phase data then shows striking stability and, indeed, a perhaps even more striking similarity between the United Kingdom and the United States.[9]

These results established, Friedman and Schwartz go on to report that they find no support for the existence of a Phillips curve or, indeed, in the United Kingdom *any* effect of money on real income. There we come to a finding that should surely generate further work. Their study demonstrates that money supply changes have little, if any, effect on real income over a cycle phase. This leaves open—or, perhaps, opens up—the question of what the effect may be within the cycle. Is there an effect? Is it stable? Or is it, perhaps, a product of particular money supply regimes where it is rational, albeit maybe ultimately wrong, to expect a stable, or at any rate a very sluggish, price level?[10]

Initial reaction to this result was not well-conceived. The result was thought to be surprising, apparently because many readers took it as saying something about what occurred *within* a cycle. In fact while, as remarked above, it does direct attention to what goes on within a cycle, it tells one about what happens *over* a cycle—quite a different matter. Viewed that way, the result should not have been a surprise. In his 1972 study, for example, Phillip Cagan found that income responded to monetary fluctuations and this offset the initial impact of money on nominal interest rates well within a cycle phase. To quote,

> The estimated pattern [of lag coefficients] indicates that monetary effects on aggregate expenditures are quite rapid. In table 7-3 the cumulative effect reaches unity six months after the initial change in monetary growth. Unity is the total long-run effect. There is overshooting, however, and the cumulative effect settles back close to unity by the eighteenth month. (pp. 110–11)

Despite this, Charles Goodhart was so surprised by the finding that he sought to replicate it over what he called the "raw" data—that is to say, the basic annual data—and found it confirmed. This *is* a striking finding. How can it be explained? It certainly requires explanation, for it is notably at variance with, for example, Attfield, Demery, and Duck's 1981 paper which found that over the years 1963–78, unanticipated money did affect output in the United Kingdom.

David Laidler, in his review of Friedman and Schwartz, advanced an explanation of the result which, although perhaps redundant to the cycle-phase finding, is certainly worth discussion when Goodhart's result is noted. His argument turned on the openness of the U.K. economy. Suppose that there is an increase in the nominal quantity of money. This rise in the money-income ratio would be expected to stimulate output. But it is prevented from doing so by a devaluation of sterling which, by the law of one price, quickly affects the U.K. price level and results in the *real* stock of money being, for all practical purposes, unaffected by the change in the nominal quantity. There would thus be a rise in the nominal quantity of money and in the price level, but no transitory rise in the real quantity of money and thus in output.

"A few observations like this could easily swamp a weak tendency for money and output to be positively correlated elsewhere in a time series. . . ." (1982, 253). This may explain the Goodhart finding, but it still leaves problems. It does not really fit the episode examined in Williamson and Wood (1976), in which it is reported that, in the particular episode studied, output growth and inflation were *both* produced by monetary expansion and preceded the ultimate devaluation. Nor does it accord with Attfield, Demery, and Duck (1981) where at any rate, unanticipated money seemed to affect output.

It is useful to turn next to the *Journal of Economic Literature* reviewers. Of the three 1982 *JEL* reviews, one (Mayer) focused on the United States, one (Hall) focused on what was not in the book, and one (Goodhart) was subtitled "A British View." It should be noted that the last was a British view written from the British central bank, for it places emphasis on institutional and operational matters which Friedman and Schwartz ignore. But it also dealt with, and raised, wider issues. Some of these overlap with those raised by Laidler (1982), but others, notably a stimulating discussion of the data, do not and are considered here first.

Friedman and Schwartz converted their data to cycle-phase averages. (This is turned to again below when we consider some econometric issues.) Goodhart (1982) suggests that although there are advantages from Friedman and Schwartz's point of view, the loss—such as the inability to use Granger-Sims techniques—outweighs the gain. This is more likely an example of the data being organized for the tests, for the method is well suited to a "disequilibrium money" approach (in the Warburton, rather than the Goodhart sense—see p. 88 above). Rather more important are the adjustments made to income and price data to take account of price controls. The importance of these lies in their novelty; no substantial study of the effect of price controls in the United Kingdom precedes Friedman and Schwartz's work. Here, however, they already have followers. Rockoff and Mills (1986) have carried out a comparative study of U.K. and U.S. wartime experience, while Capie and Wood (1988) and Capie, Pradhan, and Wood (1989) have written papers concerned with Second World War and 1960s price controls in the United Kingdom. (The 1988 study fully supports the findings of Friedman and Schwartz, but lays stress on factors additional to price controls, particularly the wartime rationing system, while the 1989 paper suggests controls to be somewhat less effective than Friedman and Schwartz find.)

Goodhart's (1982) comments on the money demand function—what interest rate or rates to use, and so forth—do not open up fresh territory, but in his examination of the money-output connection he draws attention to a whole range of issues. As noted earlier, Goodhart redid the statistical work on the money-output relationship. He did it on the "raw" data—i.e., the unsmoothed annual data—and found that the U.K. evidence is "*consistent* with the monetarist view" (p. 1546, Goodhart's italics). Now of course the result he finds has nothing to do with the "monetarist view." Insofar as such a thing can be identified, it relates to long-run results. If the long run turns out to be a year, then so be it, but it is no part of monetarism that it has to be. Nevertheless, reflecting on that finding, Goodhart raised some interesting questions.

In regressions with the rate of change of prices as the dependent variable, the main explanatory variables are contemporaneous money growth (lagged money for 1946–75) and the lagged dependent variable. But drawing from his experience in a central bank, and drawing also on some prior work of Friedman's, Goodhart suggests that interest rate setting induces procyclical money stock variation, so that *current* inflation affects *current* money growth (and current interest rates). There may, therefore, be simultaneity. Dropping current variables on these grounds then allows past output growth, as well as past money growth, to affect inflation. What are we to make of this?

Goodhart suggests that questions of endogeneity and simultaneity need to be considered, as they were for the United States in the prior volumes of this series. That recommendation is wise; some of the work is now being done, and more surely will be. But that is not all that can be said. If today's income growth causes today's money growth, then yesterday's income caused yesterday's money. Hence it may well be that the effect of yesterday's income on inflation is spurious, or rather, the result of it causing money growth. Thus inflation could well be purged of any causal impetus from output. In summary, what Charles Goodhart's arguments do is *not* refute the conclusions of Friedman and Schwartz on the impact of money on prices and output, but strengthen the case for a short-term analysis, and provide some initial hypotheses to be explored.

Goodhart also suggests that the exchange rate regime may be important; with a fixed exchange rate, money growth may respond to output growth. Goodhart says Friedman and Schwartz consider and reject this. Surely a misreading; they argue that regardless of the source of the money growth, the money growth will have subsequent effects. (This is stated particularly clearly in fn. 10, p. 319, and fn. 14, p. 325.)[11] But although a misreading, it is a potentially fruitful one, in that it directs attention to the various sources of money growth and prompts study of whether the source affects the *speed* of impact. (As Friedman 1979 suggested was possible.)

Goodhart also directs attention to the possibility that the United Kingdom has not experienced severe enough monetary fluctuations to show a "strong statistical relationship between money growth and output growth" (p. 1548).[12] He conjectures that this is due to the benevolent and efficient stabilization policy of the Bank of England. There is, however, another interpretation which he hints at, and comparison of the two is certainly worthy of serious study. The other interpretation is that this better monetary policy resulted from different institutional structures. This may be correct. The episode when money most clearly affected output in the United States was the Great Depression, when the Federal Reserve System failed to act as a lender of last resort. As

Anna Schwartz has pointed out, no such failure has occurred in the United Kingdom since 1866 (1986). The idea that large monetary fluctuations affect output while small ones do not—or, at any rate, not enough to show up in econometric results—seems to fit the facts. But if only large monetary changes affect output, why this is so is still an unanswered question.[13]

Turning next to interest rates, *Monetary Trends* contains (in addition to a masterly exposition of the interaction of money growth with interest rates), a reexamination of the Gibson paradox and a closely connected analysis of price expectations formation. As Goodhart (1982) points out, while Friedman and Schwartz provide most cogent explanations of why interest rates should not (until recently) have adapted to inflation—explanations based on the nature of the monetary standard and the temporary nature of most inflations—these explanations are better suited to explaining long rates than short rates. The behavior of short rates remains a puzzle.[14]

Before considering what *Monetary Trends* provoked at the Bank of England, two other reviews are worth noting as raising interesting points. As was observed by Friedman and Schwartz in their preface, the U.S. content of *Monetary Trends* was underpinned by an extensive body of analysis on numerous issues. One of these was the determination of the money stock. They had not carried out such a detailed preliminary study for the United Kingdom. This was taken up by Tim Congdon in a 1983 review in *The Banker* unprecedented in length for that magazine, an indication in itself of the importance attached to the book. The review is both puzzling and interesting. Its starting point is that Friedman and Schwartz "fail to recognize that the money supply is itself the result of an economic process" (p. 117). What Congdon means is that the institutional setting within which the money stock is determined differs between the United Kingdom and the United States, and that he thinks they should take account of this. His concern was not, however, with the main substance of the book, for he acknowledged that what he saw as an omission would not affect trends. Rather he argued that as the Bank of England set interest rates and supplied whatever money was demanded at that rate, Friedman and Schwartz's account of short-term money income relationships and short-run interest rate movements was likely to be wrong. Again, in a different form, a complaint which some other reviewers had made: that the book really was about trends, as its title implied.

Congdon (1983) is also to some degree misleading, for his description of the Bank's procedure is accurate only for a limited part of the period.[15] Nevertheless, he does direct attention to several important areas of research which can be built on the work in *Monetary Trends*. The first is one which has been, and is being, developed extensively by Anna Schwartz: the role of the lender of last resort, and how the central bank carries out this role. As she has described (1986), the Bank of England took on this task in the nineteenth century. After 1866, the British banking system was much more stable than the American, and there was in Britain no collapse in the money stock such as triggered the Great Depression. This demonstration that institutions do matter raises interesting and important questions. Why did the Bank of England take on the lender-of-last-resort role, and did its acceptance affect its day-to-day behavior in the money markets?

These questions have been examined in a recently completed Ph.D. thesis (Ogden 1988). The answer to the first question is not simple, for the process was gradual and seems to have been the result of numerous influences inside and outside the Bank, and of personality clashes and their resolutions; it was not a straightforward response to the recognition of a responsibility. The answer to the second question, as given by a close examination of the daily discount figures, is however a straightforward and unqualified negative.

On the question of central bank operating procedures, does it matter for the behavior of interest rates if central banks conduct monetary policy by interest rate setting? As Friedman and Schwartz have argued and demonstrated several times (in *Monetary Trends* and elsewhere), once money is in the economic system, it does its work, regardless of how it got there. It is hard to believe that this does not apply to long-term rates regardless of central bank operating procedures. But what of short-term rates? If a central bank sets a short-term rate and supplies whatever money is necessary to hold that rate, then that rate and other rates linked to it, will very likely respond differently from how they would have behaved had there been a similar amount of money supplied without pegging the rate. The short-run dynamics of short-term rates probably are affected by the central bank's money stock control procedure. Confirmation of this would be of interest in itself, and would also resolve some puzzles over the behavior of short-term rates in periods of high inflation, such as the First World War when, contrary to its pre-war procedure, the Bank did engage in interest rate stabilization.[16]

Finally, in examining issues prompted by reviews of *Monetary Trends,* we move to the stability of the money demand function Friedman and Schwartz estimate. This is considered more extensively below, but it should be remarked that in a brief review Michael Artis (1983, 461) described Friedman and Schwartz as "straining for effect" in finding a function which fitted the United Kingdom throughout their data period. He was not particularly surprised by stability of the function; he had (with Mervyn Lewis) found a function that was stable over long periods. But stability for *so* long seemed to him a puzzle. Indeed, and partly because of the role of dummies in the money demand function, there remained a suspicion that the interwar years were special and that "Keynes was generalizing from an idiosyncratic episode" (Friedman and Schwartz 1982, 622).[17] The results Friedman and Schwartz obtain for these years suggest that the intensive study of the interwar years which is now under way will not only help understanding of these years, but also help clarify whether the *General Theory* should be retitled—perhaps *A Special Theory* . . .[18]

2.2.2 The Bank of England Conference

Some institutional information is now in order. The Bank of England from time to time convenes a meeting of a "Panel of Academic Consultants." This panel comprises not an unchanging group but individuals invited to attend according to the subject being discussed. They meet, together with Bank of England and Treasury staff, to discuss two previously circulated papers on a theme chosen by the Bank. The Bank convened such a meeting to discuss *Monetary Trends,* and the two papers presented, along with a brief introduction by Robin Matthews, were published by the Bank of England in 1983.

In his introduction, Matthews made five points, all worth repeating—one because of the foresight it displayed, and the others because, extracted from the discussion, they reveal in their overlap with reviewers' remarks the homogeneity of the reactions the book provoked. Matthews drew attention to the publication delay which had made some of the results of the book confirmations rather than first demonstrations. Second, he remarked on the absence of institutional discussion (but did not, like some others, suggest why it might be important). He raised questions of causal direction in an open economy, thus agreeing with Friedman and Schwartz about the underpinning of the U.S. section. Like Artis (1983), he asked whether the observed money demand stability over such an extraordinary period was not perhaps more than was required. And, on the econometric results of one of the Bank papers, he (presciently as it turned out) hazarded the judgment that the final word was not yet in.

What did the papers have to add to that? The first paper to be discussed, and first in the Bank's publication, was by Arthur Brown (1983). All the other reactions to *Monetary Trends,* broadly speaking, accepted its main results and suggested developments that could be set against the background thus established. Brown attempted to reject these results. In doing so he paid perhaps a greater tribute to the work of Friedman and Schwartz than did any more well-disposed reviewer. For it is largely due to their work (and that of others prompted by them) that the view of the world which Brown attempts to defend seems "a bit obsolete," to borrow a phrase Laidler used in his review; and, it should be added, so far as guiding future research is concerned, guides it only to a dead end.

It is a view in which velocity is an irrelevance (p. 13–14), "cost push" is an important cause of inflation (p. 26), and the Phillips curve provides a permanent tradeoff and stable basis for policy (p. 24). British inflation is often imported (in contrast with Williamson and Wood 1976), and has little to do with money growth. There is well exemplified what has been called "adding up economics"—explaining a movement in some aggregate, e.g., national income, as due to its biggest or fastest moving component (house building, for example, p. 33, and again, p. 34, where long-term growth, which presumably has something to do with supply, "is attributable to the fact that growth depended on foreign demand"). There is even the traditional confusion between relative prices and the general price level: "All these outstanding price changes are associated with changes in foreign trade prices. . . ." (p. 35).

The conclusions of the paper are summarized as answers to a series of questions (pp. 40–43). There we find all the points noted above, together with the extraordinary statement that "Strict truth of a simple quantity theory implies that velocity is constant. . . ." Friedman and Schwartz have consigned to works on the history of thought many of the views set out by Arthur Brown. But with this statement, scope for fresh research emerges. How can a view, adamantly rejected by that distinguished quantity theorist Henry Thornton in 1802, persist in being repeated and believed over a century and a half later?

Finally, in work prompted by *Monetary Trends* we come to an econometric study. This was by David Hendry and Neil Ericsson (HE) (1983).[19] Their paper made two points: Friedman and Schwartz had used "old-fashioned" econometrics, and, when modern econometric techniques were applied to their data, a money demand function stable over their whole period cannot be found.[20] The first point is correct, but should certainly be viewed as a factual statement rather than a criticism. What surely needs to be considered is not the vintage of the techniques, but whether they are appropriate for the data and whether they give reliable results.

Setting these points aside, however, how did the econometric criticisms stand up? The answer has to be that the criticisms are not well directed. HE engaged in extensive data mining and, although claiming to reject the Friedman and Schwartz equation, in fact do not really do so. Rather, they reject an equation which omits a demographic variable and the own rate on money—which Friedman and Schwartz regard as important, and spend some pages discussing—and present an equation that uses different interest rates from those discussed and chosen with some care by Friedman and Schwartz. In other words, their assertions are not supported by their own finding.

Further, the HE claim that modern econometrics (i.e., theirs) rejected the findings of Friedman and Schwartz on the demand for money was quickly subject to a direct challenge in a paper by Sean Holly and Andrew Longbottom (1985). They wrote,

> In this paper we extend the work of HE on the demand for money and find that after all it is possible—using the methodology which HE employ—to observe a long run underlying demand for real money balances which does support the claims of Friedman and Schwartz. In particular we can find a long run demand for money relationship which is very similar to that which Friedman and Schwartz estimate using the methodology. (p. 1)

HE have not yet (1988) responded to this challenge. It is, however, notable that the methods of the HE paper are highly sensitive to minor changes in data (such as can occur, for example, when different authors have chosen different ways of linking two series to give a longer run of data), in data period, and in computing techniques. It may appear that not all advances in econometrics represent progress.

One aspect of the statistical methods used by Friedman and Schwartz has recently been discussed by Saleh Neftci (1986). His paper is a general examination of NBER business cycle methodology and the NBER practice of converting data to cycle-phase averages, which procedure Neftci regards as embodying the assumption that ". . . the state of the cycle is important even after account is taken of the relevant calendar time variables" (p. 11). If, he writes,

> a cyclical time unit can be consistently defined, . . . we can transform these time series using this newly defined time unit. This transformation of the series will eliminate some types of movements in macroeconomic data while highlighting any remaining periodicities, namely any "long cycles" and the trend component. One such procedure that uses a cyclical time unit is phase averaging (Friedman and Schwartz 1982 and HE 1984). (p. 39)

Is this phase-averaging technique appropriate, in the sense that applying it to the data gives information additional to that which can be obtained from the use of straightforward (calendar time) variables? Neftci shows that it does, under certain circumstances, given such information. He thus severely qualifies Hendry and Ericsson's strident rejection of the technique.[21] In particular, the technique not only eliminates serial correlation due to the business cycle and eliminates "measurement error" (these points are discussed by HE), it can help to capture long-run relationships. The particular long relationship that the procedure helps capture is a nondeterministic trend. Suppose, for example, we have a nondeterministic trend whose slope alternates between slow growth and fast growth, with uncertain length of each phase, then phase averaging would capture (approximately) the random movements in the trend. A crucial issue in future evaluation of the NBER procedure is thus the nature of the trend. On this point, evidence is starting to accumulate. Neftci reports some work which, tentatively, supports the nonlinear assumption (p. 45). And, addressing the question directly, Nelson and Plosser (1982) have claimed that trends are stochastic. But this work is still far from uncontroversial. Plainly, Robin Matthews's (1983) caution in summing up the import and the econometric work was well founded, and plainly, too, Friedman and Schwartz have managed to stimulate further work by econometricians as well as by monetary economists.

2.3 Conclusion

It is easy to say that Anna Schwartz, by her two, co-authored, massive volumes, and by her papers, has made a major contribution to the understanding of Britain's economic history. Summing up that contribution without injustice to part of it is harder. Nevertheless, three aspects of her work must be highlighted in conclusion.

The analytical framework now generally used is somewhat different from that in Gayer, Rostow, and Schwartz. But that volume remains unchallenged as a source of carefully constructed data for the years it covers, and its interpretations are derived with such care that, despite changing intellectual fashions, they too have to be taken very seriously by any current scholar of the period. The book's imprint on the study of eighteenth- and nineteenth-century Britain is indelible. *Monetary Trends* covers a different time period and uses a different intellectual framework, but this volume, too, will surely have an influence on all future work on British monetary history from 1870. By focusing on trends, it sets an agenda for future work—what goes on over shorter time spans—and provides a clearly delineated background to which, like studies of the period covered by Gayer, Rostow, and Schwartz, future studies must either conform or, if dissenting, do so explicitly and with caution.

But perhaps most important of all is the example provided by the method of Anna Schwartz's work. It is always clear, meticulously thorough, and in its conclusions carefully considered. Her work on British economic history is not only important to future scholars, it is an example to them.

Notes

1. The raw data are still extant, typewritten in 861 densely packed pages. It is a great pity that these pages are not in published form. They contain enormous detail but have been available only on microfilm, which is less than enticing. There must be a case for publishing these data, for to have monthly data for 1790–1850 on series such as exchange rates in six foreign centers, and the yield on consols, etc., would facilitate work in the area and stimulate further testing of hypotheses.

2. This was done by Mitchell (1913, 1927).

3. In this sense the "measurement without theory" attack on Burns and Mitchell was unfair since they never abjured theory. The Bureau had developed certain techniques in collation and measurement, and although the study by GRS was not a Bureau project, these techniques were drawn on heavily. It is interesting to note the early objectives of those who set up the Bureau, and this concentration on objective fact: "The Committee will concern itself wholly with matters of fact, and is being organized for no other obligation than to determine the facts and to publish its findings" (Fabricant 1984, 6).

4. A tribute to empirical work was paid by Robert Lucas in one of his theoretical papers on the subject. He wrote, "The features of economic time series [which he was about to describe] listed here are, curiously, both 'well known' and expensive to document in any careful and comprehensive way. A useful, substantively oriented introduction is given by Mitchell (1951), who summarizes mainly interwar U.S. experience. The basic technical reference for these methods is Burns and Mitchell (1946). U.S. monetary experience is best displayed in Friedman and Schwartz (1963). An invaluable source for earlier British series is Gayer, Rostow, and Schwartz (1953), esp. Vol. II. The phenomena documented in these sources are, of course, much more widely observed. Most can be inferred, though with some difficulty, from the estimated structure of modern econometric models. An important recent contribution is Sargent and Sims (1976), which summarizes postwar U.S. quarterly series in several suggestive ways, leading to a qualitative picture very close to that provided by Mitchell, but within an explicit stochastic framework, so that their results are replicatable and criticisable at a level at which Mitchell's are not" (Lucas 1981, 236, n. 4).

5. It is also worth remembering that Gayer was interested in monetary policy and had himself written a book in the 1930s on the subject of monetary stabilization. Apparently he never suggested that this should influence the work on the British economy.

6. Note the importance of *trend* factors in producing cycles—this looks forward to Nelson and Plosser (1982).

7. Anna Schwartz had given British academics two advance indications of what was in this volume. These were in her comment on a paper by Alan Budd et al. (1984), and in her Henry Thornton Lecture at the City University ([1980] 1988). The comments on Budd et al. set out the results on the U.K. and U.S. money demand functions that were reported in detail in *Monetary Trends*, and noted the lack of connection between money and output which was also set out in that volume. In her lecture, "A Century of British Market Interest Rates," she used the work in *Monetary Trends* to examine the impact of inflation on real and nominal interest rates. Did inflation, as Thornton conjectured, introduce a gap, equal to the inflation rate, between the real and nominal interest rates? She found that, over most of the century she looked at, support for

Thornton was not strong, but it became so toward the end of the period. She attributes the change to the increased price level variability, consequent upon the shift to a fiduciary standard, and the associated increased rewards to anticipating inflation.

8. Some indication of just how important this opening up of British monetary history was is that in our forthcoming books (Capie and Wood 1989 and 1990), we touch on a large number of previously explored topics in British monetary history, yet only some of these are on the list of subjects suggested for future study on the work of Friedman and Schwartz.

9. In abnormal times, such as the interwar years of depression and abnormal liquidity preference, dummies were necessary in the statistical work.

10. Friedman and Schwartz raise a topic of great importance for all future studies when they discuss expectations. As they point out, a forecasting procedure which turns out to be *systematically* wrong when viewed with hindsight may have been perfectly rational given the information available at the time.

11. It is worth pointing out that the results obtained in the article (cited on p. 325) are in fact due to chance. It was found that over the gold standard years in the United Kingdom, income Granger-caused money. That was, it was argued, to be expected, and some inferences were drawn from the confirmation. In fact the inferences hold, but the confirmation depended on the chance that there was no gold discovery sufficiently large to cause a gold inflow of such size as to offset in the estimates the effects of gold inflows resulting from income growth. Theory alone should have led one to the conclusion that in an open economy with a fixed exchange rate, the "causal" relationship (in the Granger sense) between money and prices would reveal nothing about causation, but would depend on the relative size and frequency of external and internal monetary shocks.

12. Goodhart at this point confuses Granger-Sims timing studies with studies of causality, an error he carefully avoids earlier.

13. This has bearing on whether rules should be contingent or noncontingent. The claims for contingent rules all rest on the assumption that *small* movements in money affect output. If that proves false, the grounds of debate are shifted rather dramatically. Bernanke's (1982) conjectures may be relevant here.

14. The puzzle may be solved by recent work, e.g., T. C. Mills (1985, 1987) and Mills and Stephenson (1986), which suggests the real rate (ex ante as well as ex post) may not be exactly constant. But such studies are as yet at a very preliminary stage.

15. See Wood (1983) for a discussion of this part of the period for which Congdon's description of Bank of England procedures is accurate. This part comprises the years from 1945 to about 1970, and also occasional episodes thereafter.

16. The behavior of the Bank in this period is described in Sayers (1976). In conjunction with Michael Bordo and Ehsan Choudhri, Anna Schwartz is currently engaged in an analytical and econometric study of the effect of interest rate setting on money stock control. A study of this question based on Canadian data has recently appeared.

17. Friedman and Schwartz, we should make clear, are referring to the experience of the United States only at this point.

18. Examples of such studies are cited in Broadberry (1985).

19. It should be remarked that their paper, which purported to be "An Econometric Appraisal of Monetary Trends," in fact dealt with only one chapter in a twelve-chapter volume.

20. There was also an important difference between the two pairs of authors on research method. HE placed complete reliance on formal econometric tests.

Economic analysis, in their implicit view of research, may suggest questions but is not qualified to comment on answers. Hypotheses stand or fall according to purely statistical criteria. Friedman and Schwartz, by contrast, explicitly regard formal statistical testing as a part—only a part—of evaluating a hypothesis. It is hard to believe that the Hendry-Ericsson approach, which essentially ignores the environment from which the data came and the reasons for examining them, is the best way to advance knowledge of the economy.

21. Neftci (1986, 41) very neatly summarizes their florid and rhetorical criticisms as follows: "For example, assume that the processes Y_t and X_t are related to each other through a relation:

$$Y_t = \Sigma\beta_s X_{t-s} + f(t) + \epsilon_t \qquad (10)$$

where $f(t)$ is (possibly) a nonlinear trend, and where ϵ_t is i.i.d.

Then, phase-averaging as described in (8) is like applying two complicated filters to Y_t and X_t. These filters will be nonlinear in the data, since the γ_k^i are selected after analyzing the observed time series Y_t and X_t and some observations are eliminated. Because of this nonlinear nature of the filter, it is generally not possible to quantify precisely the effects of phase-averaging. However, one can make the following comments:

1. The phase-averaging shown in (8) will lead to a loss of information about the system (10), since many data points would be eliminated.
2. If the original ϵ_t were white, ϵ_k^* may exhibit complicated heteroscedastic behavior.
3. More importantly, the selection of $|\gamma_x^i|$ after observing the realization of Y_t and X_t may in general introduce a correlation between X_t^* and ϵ_t^*—even where there was none originally, so that linear projections will give biased estimates of the $|\beta_s|$.
4. Because the filters applied to Y_t and X_t are different, $|\beta_s|$ would not be the same as $|\beta_s^*|$."

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Comment David Laidler

I very much enjoyed reading this paper, and was particularly pleased by the attention which Capie and Wood have paid to Anna Schwartz's earliest work on Britain, carried out with Gayer and Rostow. The very fact that this work, begun more than fifty years ago, still retains its

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importance today speaks eloquently of the lasting value of the care and discipline which have always marked Anna's contributions to our subject. These qualities are all too rare, and the empirical basis of our economic knowledge would be a good deal stronger if more of us would follow the example Anna has set throughout her distinguished career.

Fortunately for my ability to function as a discussant, my pleasure in reading this paper did not arise from finding myself in complete agreement with it. My dissent is more from particular details of the argument though, than from its broad outlines. I share Capie and Wood's (and Anna's) views on the importance of monetary factors in the business cycle, on the basic soundness of the framework for analyzing them that the quantity theory tradition provides, and on the necessity for continuously and carefully testing theoretical arguments against empirical evidence. Even so, two aspects in particular of Capie and Wood's analysis seem to me to require a little more thought before their conclusions are accepted. I do not completely share their views on the historical development of business cycle theory, or on the way in which inflationary impulses are transmitted between countries under fixed exchange rates.

Business Cycle Theory

Economists in the 1920s and 1930s would have agreed with Capie and Wood that in their era, "a monetary theory of the cycle was . . . a prominent explanation" (p. 83). However, they would not have thought that they were thereby endorsing the view that fluctuations in the money supply are the key causative factor driving cyclical fluctuations. Most economists of the interwar years believed that systematic cyclical fluctuations could occur only in an economy whose activities were coordinated by monetary exchange. That is the sense in which they believed the cycle to be a monetary phenomenon. Comparatively few, however, attributed more than a permissive (or at most exacerbative) role to monetary variables in the propagation of cyclical impulses, whose origins lay outside of the monetary sector. To give some examples: Knut Wicksell, as an empirical matter, believed that cumulative processes of the type he analyzed (and which he himself did not systematically treat as cyclical phenomena) were more likely to be set in motion by exogenous increases in the "natural" interest rate than by any change in the money rate initiated by the banking system; this view was shared by virtually all those—Hayek and the Austrians, as much as the Stockholm school—who were later to produce self-consciously Wicksellian theories of the cycle; Keynes's stress in the *General Theory* on fluctuations in "animal spirits" as a source of economic disturbance reflects a longstanding consensus of Cambridge economists on this matter; and so on.[1]

If one seeks a pre-Keynesian prototype for the cycle theory that Anna Schwartz and Milton Friedman have done so much to establish, one must look not to the general body of European business cycle theory, nor even to the work of Mitchell and Burns—though the influence of their empirical methods is clearly crucial—but to the work of Irving Fisher. His discussion of "transition periods" in chapter 4 of *The Purchasing Power of Money* (1911) deals with a cyclical process set in motion by shocks to the quantity of money, and kept in motion by other monetary factors, namely the influence of inflation expectations on nominal interest rates and their interaction with profit expectations. This work represents a line in the development of business cycle theory quite distinct from that which Capie and Wood rightly identify as running from the work of Hayek ([1929] 1932) to that of Robert E. Lucas (1977). As an admirer of Friedman and Schwartz's analysis, I wish that Capie and Wood had been more critical of this latter approach, whose fundamentally Walrasian character seems to me to render it quite incompatible with work in the Fisher-Friedman-Schwartz tradition. One or two issues bear a little thinking about before the superiority of New-Classical cycle theory is accepted.

If "money matters" at all, it surely matters for mitigating the consequences of unforeseen market events. That is one reason agents hold money as a "temporary abode of purchasing power." But causation runs two ways here. We hold money because we are ignorant, but we remain more ignorant than we need to be because our money holdings protect us from the worst consequences of that ignorance. If this conjecture has any empirical content, it implies that the last thing a monetary theory of the cycle should do is assume that all agents within the economy make full use of all the information available to the economist looking into it from the outside. Moreover, historians, of all people, should be aware that the time during which individual business executives are in a position to make important decisions seldom spans more than a couple of cycles. That is hardly long enough for them to learn from their own mistakes; and are institutional memories so well designed in the business world that we can rely on the executives having learned from the mistakes of their predecessors?

Why then should a money-using economy, inhabited by mortal men and women who face significant marginal costs of acquiring and processing information, move over real time "as if" it was populated by immortals to whom most relevant information is a free good, as is the computing power needed optimally to extract from noisy signals estimates of those few data that are missing? Why should the repetition by one generation of the errors of its predecessors not be an important source of the continuity of cyclical phenomena? The superior compatibility of New-Classical business cycle theory with the historical record needs to be demonstrated before we conclude that the theory of Friedman and Schwartz has been superseded. Its premises should be treated as testable hypotheses, not undeniable axioms.

The Supply and Demand for Money

Capie and Wood correctly identify as the central characteristic of Friedman and Schwartz's monetary model the hypothesis that there can arise a discrepancy between the quantity of money in circulation and the amount that the nonbank public is willing to hold, given what we would nowadays call its "long-run" demand for money. This hypothesis is incompatible with New-Classical theory, where flexible prices prevent such a discrepancy ever occurring, and with Keynesian analysis, where interest rate movements similarly keep the supply and demand for money in perpetual equilibrium. The consequences of such a discrepancy for expenditure flows of all sorts are the driving force in models of the cycle deriving from the quantity theory tradition. Clark Warburton's work is surely important here, as Capie and Wood note, but one does not have to work too hard to extract a similar story from *The Purchasing Power of Money,* or from some of Alfred Marshall's writings. This is not surprising because what are nowadays called "disequilibrium money" or "buffer-stock" effects reflect very much the same class of phenomena as that which a traditional quantity theorist might have labelled "cash balance mechanics."

There is, of course, more to Friedman and Schwartz's version of cash balance mechanics than the proposition that there often exists a state of affairs which can be characterized by the following inequality: $Ms \neq Md$. Capie and Wood correctly differentiate Friedman and Schwartz's product from Charles Goodhart's "buffer-stock" analysis, even though he too attaches great importance to this same inequality between the supply and demand for money.[2] Goodhart locates the source of most (or at least many) disturbances on the right-hand side of this inequality, and treats induced fluctuations in the supply of money as being crucial to absorbing their consequences. For Friedman and Schwartz the predominant causes of such inequalities are fluctuations in the supply of money, and their predominant consequences are fluctuations in the arguments of the demand function, namely interest rates, real income, and prices. Now in most cases I would take the Friedman-Schwartz view of these matters, but there is, as Capie and Wood note, one case where I do not, and that concerns the international transmission of price level shocks.

My disagreement here is important for the following reasons. In their work with cycle-phase data on the United Kingdom, Friedman and

Schwartz were able to find plenty of evidence linking money and prices, but none suggesting a chain of causation going from money through real output and employment to money, wages, and prices. Goodhart (1982a) repeated their work, using annual data, with similarly negative results. Taken at face value this would suggest that reducing (increasing) money growth in the United Kingdom leads to lower (higher) inflation with no effects on output and employment that endure long enough to show up in annual data. I simply do not believe that the recession of the early 1980s was independent of the anti-inflationary stance of Mrs. Thatcher's monetary policy; that the real aspects of the Heath-Barber boom of the early 1970s were independent of the inflationary money growth that their policies engendered; and so on. But if I do not believe these things, I have to explain why the mechanisms at work during these episodes do not appear to be generally present in United Kingdom data. That is why I find attractive the hypothesis that, on some occasions at least, inflationary impulses originating abroad, or arising from devaluations, might have disturbed domestic prices before they caused the money supply to vary, thus producing simultaneous contractions of real variables. It is also why I regret Capie and Wood's rejection of this hypothesis largely on the basis of a priori argument supported by some evidence drawn from but one episode.[3]

There is nothing theoretically novel about the mechanisms involved here. Thus Fisher (1911, 90) noted "When a single small country is under consideration, it is . . . preferable to say that the quantity of money in that country is determined by the universal price level, rather than to say that its level of prices is determined by the quantity of money within its borders."[4] Wicksell ([1905] 1935), in discussing the effects of gold inflows on domestic prices under the gold standard, suggested that ". . . this increase [in commodity prices] may even precede the arrival of the gold. . . ." (p. 197). Moreover, the effects in question do not have to be always at work to influence the results of applying regression analysis to a run of data. They only need to have been important from time to time. Nor do they have to work through commodity arbitrage. A transmission of foreign price or exchange rate shocks through domestic inflation expectations will also suffice. Nor do such shocks have to impinge on the long-run inflation rate to interfere with underlying empirical regularities. A disturbance in the inflation rate for a year or two while a new international structure of relative price levels is established could be enough to upset things. Moreover, my conjecture is supported by a certain amount of empirical evidence generated ten years or so ago by the Manchester Inflation Research Programme which Michael Parkin and I supervised.[5] I would not claim that this evidence is in any way definitive, but surely it should be followed up before the effects it seems to reveal are dismissed as irrelevant.

Thus, as their work on the monetary history of the United Kingdom progresses, I hope that Capie and Wood will keep an open mind about this question and will investigate the possibility that, when it comes to the international transmission of inflationary impulses, or the response of domestic variables to exchange rate changes, more than the price-specie-flow mechanism has sometimes been at work in generating their data. Obviously it requires the techniques of the historian, rather than the econometrician, to look into the possibly infrequent operations of other mechanisms, but no one is better able to employ those techniques in analyzing the United Kingdom experience than are Capie and Wood. Nor could there be a better tribute to Anna Schwartz than that they should follow up her pioneering research on such issues with the same care and discipline which she has always brought to such work.

Notes

1. Wicksell's views on the actual sources of price level movements are set out in *Interest and Prices* ([1898] 1936, ch. 11) where he argues that ". . . changes in the *natural rate of interest on capital* are . . . the essential cause of such movements" (p. 167, Wicksell's italics). His paper, "The Enigma of Business Cycles" (1907), which is included in the 1965 reprint of *Interest and Prices*, shows that he did not regard his cumulative process analysis as being of central importance to understanding the cycle. For an account of Austrian and later Swedish views on these matters, see Laidler (1987). Patinkin (1976) and Eshag (1963) are accessible sources of information on the development of Cambridge thought.

2. Goodhart's analysis is set out in (1982b). Other discussions of "buffer-stock" effects are to be found in Jonson (1976) and Laidler (1984).

3. I refer here to the evidence generated by Williamson and Wood (1976) on the 1967 devaluation, cited by Capie and Wood.

4. The discussion in which this passage occurs is not, however, entirely consistent with certain later passages in the *Purchasing Power of Money* that deal with inter-regional links: e.g., "The price level outside of New York City . . . affects the price level in New York City only via changes in the money in New York City. Within New York City it is the money which influences the price level, and not the price level which influences the money" (p. 172).

5. Here I would cite Cross and Laidler (1976) who showed, with evidence drawn from no fewer than nineteen fixed exchange rate open economies, that domestic inflation expectations seemed to be directly influenced by the behavior of world prices, that the influences in question were more important the more open the economy, and that exchange rate changes seemed to profoundly disturb the mechanisms at work here; and Carlson and Parkin (1975) whose analysis revealed an apparently important effect of the 1967 devaluation on British inflation expectations, and hence casts doubt on the conclusions drawn by Capie and Wood from the Williamson and Wood (1976) study about the irrelevance of such a phenomenon.

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General Discussion

Rostow recalled his collaboration, which began 48 years ago, with Anna Schwartz and Arthur Gayer on the study of the British economy from 1790 to 1850, evoking the enthusiasm of the participants in the project. He then responded to the point raised by Capie and Wood that the Gayer-Rostow-Schwartz (GRS) book did not pay sufficient attention to the monetary dimensions of the economy. According to him, the authors tried in the historical sections of the book on the financial system to weave in qualitative evidence with the limited data series they had available to them. He stressed two key differences between GRS and the modern mainstream monetarist perspective.

First, that GRS viewed the monetary sector as part of an endless, interactive process with real factors. In the historical part of the study they tried to capture how money interacted with all the other forces determining output, employment, prices, and real wages.

Second, that one consequence of regarding money and real factors as interacting endlessly and dynamically through time is that the distinction between the short period and the long period falls away. The long period becomes the accumulation of what happens in the short period. Trends—which are by no means linear in history, as Simon Kuznets and Arthur Burns demonstrated—become an ex post view of what, in fact, happened through historical time.

Rostow views Friedman and Schwartz's *A Monetary History* as really a study of how, in four respects, the authors judged money to be significant in the evolution of the American economy from 1867 on: in wars; in gold and its influence on prices; in the mechanism of cyclical downturns and deep depressions; and then, specifically, in determining the depth of the Great Depression after 1929. GRS were asking a different question: what happened to output, employment, prices, and real wages, and why?

Rostow expressed great admiration for Anna Schwartz's scientific contribution despite occasional differences with her conclusions.

Darby, in response to Laidler's comment on the Capie-Wood paper, referred to his *International Transmission of Inflation* study with Lothian, Gandolfi, Schwartz, and Stockman which found evidence that the price-specie-flow mechanism, rather than price arbitrage, was the dominant channel of international transmission.

Schwartz made the distinction between transmission under fixed exchange rates—the focus of the Darby et al. study—and flexible exchange rates.

Laidler pointed out that taking into account expectations—which is not quite the same thing as arbitrage—is important not so much for the international transmission of inflation per se, but for the issue of what different channels of transmission do to the relative timing of output, employment, and inflation changes in an open economy. He felt that the effects of a fairly weak expectations shock on the timing of changes in a few key wages and a few crucial nominal prices, in a particular cyclical upswing, could change the timing of aggregate variables relative to what is normal. In turn, this could create problems for the goodness of fit of regressions fitted to data taken from a number of cycles. He felt that evidence on the timing of cyclical variables in the domestic economy could reveal if there was a subordinate role for this mechanism.

WOOD responded to Laidler's point on whether or not commodity arbitrage could conceal the short-term impact of nominal money on real income. He argued that Laidler's suggestion that a devaluation or an exchange rate change in a country like the United Kingdom could lead to prices rising so rapidly that money did not have time to grow in real terms before prices rose, might be an explanation for what Friedman and Schwartz found in *Monetary Trends* over cycle-phase averages and what Goodhart found with annual data.

Wood mentioned further that the period covered in *Monetary Trends* encompasses more than one exchange rate regime: the gold standard, the interwar years, then Bretton Woods. That again should surely complicate the story Laidler tells.

Finally, he made the point, based on studies by Lipsey and Kravis, that price arbitrage is very strong in commodity markets but becomes progressively weaker in semifinished goods and manufactured goods markets. Thus, though commodity arbitrage may be important, it is not sufficiently important to provide the explanation of why fluctuations in the nominal quantity of money did not affect output, even transitorally, in the United Kingdom.

LAIDLER doubted that money does not have transitory effects on output in the United Kingdom, citing evidence from particular cycles when the authorities slammed on the monetary brakes, slowing down both real output and the inflation rate. On some occasions monetary contraction showed up in the behavior of the money supply; on others, because the economy was on a fixed exchange rate, in the behavior of domestic credit.

As evidence that a currency devaluation changes something in the timing of relations between inflation and unemployment, he described some of the research he and his colleagues at Manchester did in the 1970s. Initially they could not get anything to fit until they dropped the years following devaluations. Doing this, they found that traditional expectations-augmented Phillips curves, that initially performed quite poorly, improved considerably.

MELTZER raised two issues concerning the monetary theory of the 1920s. The first issue was that the Cambridge school, including Marshall, Pigou, and Keynes, were all believers in a cycle driven by waves of optimism and pessimism, rather than a monetary theory of cycles. Second, he argued that proponents of a monetary theory of cycles, as discussed by Haberler, had a totally different idea of the source of the cycle than the modern view. For many of them it was overinvestment or overconsumption, fed by something in the internal dynamics of the system, not a monetary impulse. The idea of a monetary impulse can be found mainly with Irving Fisher who emphasized gold flows. According to Meltzer, Fisher's approach was an exception. The dominant theory of the business cycle at that time starts with a real shock to consumption or investment. The banking system then furthers the expansion of output produced by the real shock.

ROSTOW described the doctrinal underpinnings of the Gayer study. It was based on a mixture of the Marshall-Pigou approach and the Continental approach with emphasis on waves of optimism and pessimism.

MELTZER amplified on his distinction between monetary and real theories of the cycle. He views Hawtrey as having a real theory in which inventories change, and the banking system finances the opportunity for firms to rebuild their inventories. By contrast, he views Wicksell, in his 1907 *Economic Journal* article, as a proponent of a monetary theory of the cycle. For Wicksell, the initiating impulse was a reduction in bank rate simultaneously by all the central banks of the world.

LAIDLER disagreed with Meltzer's interpretation of Hawtrey and Wicksell's views. Hawtrey's notion of the unspent margin was not too dissimilar to an excess supply of cash balances, granting however that one source of this discrepancy was the real side. According to him, a reading of Wicksell's *Interest and Prices* posits fluctuations in what we would call the marginal efficiency of capital as driving the economy, with the banking system moving slowly to react to such shocks. In his opinion, Irving Fisher was the father of the monetary impulse view of the cycle.

O'DRISCOLL made the point that business cycle theorists of the 1920s were more interested in analyzing the cyclical process and less interested in the issue of proximate causation.

MCCALLUM argued that pre-Keynesian cyclical theory should not be regarded as the same as what is now called real business cycle theory. An important part of Marshall's argument was that nominal wages would not adjust to shocks, so that with an unchanged stock of money, cyclical influences would come about because of changes in real wages. These changes resulted because nominal prices adjusted more rapidly than nominal wages. Thus his theory was one that mixed real shocks with a Keynesian view of the workings of the system. According to McCallum, Keynes's theory was very much a spelling out of the mechanism that was implicit in Marshall's 1887 analysis.

M. FRIEDMAN argued that all the above-mentioned predecessors had elements of a monetary theory since almost all emphasized the extent of the strain on the banking system. At the same time, none of them had a purely monetary theory. Rather they viewed the cycle as the result of waves of optimism (Pigou), of bursts of innovation (Schumpeter), or of action of the real forces that led to a reduction in real wages. But then in all of these cases—and this is where he believed Hawtrey fits in—they all spelled out the ways in which the banking system gets overtight and finally brings the boom to an end.

ROSTOW expanded on Marshall's theory of the cycle. Marshall's theory was based on his observation of the cycle which peaked in 1873 and on Mill's theory of the cycle, which in turn was based on the cycle that peaked in 1825. Both episodes were characterized by a rise in money market interest rates before the cyclical peak, suggesting to Rostow that the rise in interest rates and pressure in the money markets was a key part of the background to the crisis. Rostow then described other cycles characterized by a shock to the rate of return over cost (marginal efficiency of capital), in turn precipitating a financial crisis that occurred after the upper turning point. Thus, he argued, a sharp distinction needs to be made between the role of the monetary system in helping set the framework for the crisis—along with an increase in wages, raw material prices, and other costs—and uncertainties about the future profitability of the leading sectors during the boom.

LAIDLER and WOOD, in response to a question posed by Milton Friedman, cited instances where Irving Fisher's work was influential in the development of the Cambridge approach.

HETZEL raised the question of whether the quantity theory tradition of Irving Fisher had much influence on the treatment of the business cycle in the United States in the 1920s.

M. FRIEDMAN replied that Fisher's influence was dominant and that Wesley Mitchell paid a great deal of attention to monetary influences on the cycle in his 1913 book.

LAIDLER pointed out that the Austrian economists—Hayek, Mises, and Robbins—as well as Wicksell and Robertson, referred to themselves as quantity theorists, but that was only with respect to their treatment of the relationship between the quantity of money and the price level. According to him, they did not propound a monetary theory of the cycle.

BORDO discussed the relationship between Clark Warburton's theory of monetary disequilibrium and its historical antecedents. Aside from Irving Fisher, the American proponents of the monetary theory of the cycle are not well known today.

SCHWARTZ emphasized that many of these monetary theories basically were theories about the way the interest rate operated, and not about what happened to the quantity of money.