BOOK REVIEWS

Did Monetary Forces Cause the Great Depression?

*A Review Essay by Arthur E. Gandolfi and James R. Lothian**

Did Monetary Forces Cause the Great Depression?, by Peter Temin. New York: W. W. Norton, 1976 vi +201 pp. $8.95 (cloth); $3.95 (paper).

"Given the magnitude and importance of this event [the Great Depression], it is surprising," states Peter Temin, "how little we know about its causes" (pp. xi, xii). In particular, he says, we have no explanation of why the Depression became so severe. Temin reviews the two major competing explanations, which he calls the "money hypothesis" and the "spending hypothesis."

Temin associates the money hypothesis almost exclusively with the account Friedman and Schwartz give in [5, chap. 7]. In their view, it was the banking panics of the 1930s and the subsequent failure of the Federal Reserve to counteract the contractionary monetary effects of those episodes that accounted for the depth and duration of the Depression. The spending hypothesis is the term Temin gives to the explanations advanced by economists working within the Keynesian income-expenditure tradition, who emphasized decreases in various forms of autonomous expenditures as the cause of the Depression. Temin finds both the money and conventional spending explanations wanting and goes on to develop one of his own. It is a variant of the spending hypothesis that identifies an autonomous shift of the consumption function as the crucial factor.

What's wrong with Friedman and Schwartz's explanation, according to Temin, is that it assumes what should have been tested—the direction of causation between money and income. Friedman and Schwartz, he says, only considered the channels running from money to income and totally disregarded those running in the opposite direction. Hence, Temin states, their interpretation of the Depression remains an untested hypothesis. He therefore attempts to confront their theory with data. When he does, he finds no evidence that money played an active role during 1930 and the first half of 1931, the period he views as crucial for explaining the severity of the Depression. The banking panic of 1930 was mainly induced as he sees it; and the fall in the money stock that took place thereafter was the result of a fall in demand for money. As evidence he cites the failure of either real balances to decrease or short-term interest rates to increase, as he maintains they should have if changes in the supply of money had been the initiating factor in the monetary decline.

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Temin dismisses the Fed's posture in the face of such a decline in the money stock as not germane to the question of causation.

Traditional versions of the spending hypothesis fare little better under Temin's scrutiny. He discusses the general explanations given by Alvin Hansen, Robert A. Gordon, Joseph Schumpeter, Thomas Wilson, and Keynes himself. He then proceeds to the econometric models of the period constructed by Ben Bolch and John Pilgrim, Lawrence Klein, Jan Tinbergen, and John B. Kirkwood. He concludes that "none of these models can be considered to have proved its point, in the sense of showing that the view of the Depression embodied in it is superior to the alternatives" (p. 53). His reasoning is that all of the models, like the explanations of Keynes et al., are merely descriptive, each showing that the movements in the type of expenditures that it assumed had generated the Depression were consistent with that particular hypothesis. Proponents of the spending hypothesis—econometricians and noneconometricians alike—are, according to Temin, guilty of the same type of question begging as Friedman and Schwartz. None really tested alternative explanations.

Peter Temin's book is, therefore, an extremely ambitious undertaking. It involves a reexamination of one of the most interesting and frightening episodes of modern economic history. It calls into question the most widely accepted explanations of that event. Having been published in the immediate aftermath of the severe 1974-75 worldwide recession, which has rekindled interest in the causes of depression, the book has already generated much discussion. Solely on these grounds one might ordinarily consider the book important and a success. However, given its significant flaws such a judgment is unwarranted.

To begin with, Temin seriously misreads Friedman and Schwartz and neglects other important related work on depressions. The result is a straw man money hypothesis that differs grossly from the generally accepted monetary explanation of the Depression.

Another shortcoming of the book is Temin's questionable interpretation of causation and the role of counterfactuals. These methodological problems by themselves are enough to severely limit his conclusions. What limits them further, and indeed in most important instances reverses them, are the theoretical and statistical errors that he makes. Three pieces of evidence are crucial in his analysis. Two—the behavior of the consumption function and of real balances—we find to be completely without substance; and the third—the path of short-term interest rates—is subject to a completely different interpretation than the one Temin offers.

1. Temin, Friedman and Schwartz, and the Stock of Money

Temin provides his own explanation of what happened to the stock of money during the onset of the Depression. "Income and production fell from 1929 to 1933 for nonmonetary reasons. Since the demand for money is a function of income, the demand for money fell also. To equilibrate the money market, either interest rates, the stock of money, or both, had to fall. And since the supply of money was partly a function of the interest rate, this movement down along the supply curve of money meant a decrease in both" (p. 27). He adds, "In the absence of banking crises, the decline in the stock of money would have been 'caused' primarily by a fall in the deposit-reserve ratio. In the presence of banking panics, the deposit-currency ratio fell too." (p. 27). Because Temin's alternative explanation of what happened to the money stock and his critique of the Friedman and Schwartz hypothesis are integral to his reinterpretation of the Depression we consider them together.

The focal point of Temin's objections to Friedman and Schwartz's account is the framework they use throughout their *Monetary History* to analyze changes in the nominal stock of money. As they stated it, the nominal stock of money and its three proximate determinants—high-powered money and the ratios of bank deposits to bank reserves and to currency held by the public—were related algebraically as:

$$M = \frac{D/R\,(1 + D/C)}{D/R + D/C} H,$$

where D is deposits, C currency, R reserves, H high-powered money (the sum of C and R), and M money (the sum of C and D).

Temin objects to this framework, because he claims it implicitly assumes that the stock of money is determined by supply factors alone, completely independent of demand. And therefore, he says, it assumes that the lines of causation only run from money to economic activity, and not the reverse. He arrives at this conclusion in the following way. The above equation, he states, is an identity, but one that may be useful because it separates the influence of the monetary authorities, banks, and the nonbank public on the stock of money. Where the problem arises is that such a distinction appears to him to be at variance with the usual breakdown in economics between supply and demand. He asks whether this equation fits in on the supply or the demand side and concludes it must be on the supply side. Since the monetary authorities and the banks are suppliers of money, inclusion of H and D/R in the equation places it on the supply side. But, since the nonbank public (holders of money) influences the level of D/C, he finds that ratio harder to categorize. He looks at other work by Friedman, but sees no mention of the deposit-currency ratio as being related to the demand for money. He therefore concludes that Friedman and Schwartz viewed the determinants of that ratio as part of the money supply process. Hence, he says, "they clearly assumed that the stock of money was determined by supply alone" (p. 18).

The problem with all of this, however, is that Temin is wrong both in his theorizing and in his textual exegesis. He is correct in believing that Friedman and Schwartz sometimes treat the stock of money as an exogenous supply phenomenon. But that is not their general assumption nor is it in any sense embodied in their proximate determinants framework. On the contrary, in both sections of the *Monetary History* that discuss the use of this framework for analyzing changes in the money stock, they detail the various factors that could affect each of the three proximate determinants [5, pp. 50-53, 784-88]. Many of these obviously enter the demand-for-money function, so the potential for interaction between the supply and demand side is clearly recognized; and this is the whole point of analyzing the money stock in terms of its proximate determinants—to facilitate the investigation of demand and supply interactions and thus ultimately of the direction of causation. (In addition to [5] see [1, 2].) It is curious that Temin ignores these discussions; it is equally curious that he ignores the numerous statements in their related work such as [4, 6, 7] and in Cagan's *Determinants and Effects of Changes in the Stock of Money, 1895-1960* [2], a companion volume to [5]. All of this work points to the theoretical existence, and at times, empirical importance of such interactions.

One reason for Temin's misinterpretation of Friedman and Schwartz may be due to the difficulty he has in reconciling their analysis with conventional analysis of supply and demand. But that difficulty stems from a confusion on his part rather than theirs. Friedman and Schwartz, he says, treat changes in the demand for money as only affecting price (the interest rate) and not quantity (the stock of money). He is wrong. To see this, suppose we recast the example Temin uses to highlight that apparent contradiction. Let the demand for money increase, while the nominal stock of money remains unchanged. At each level of interest rates individuals will now want to hold a higher quantity of real money balances. As they attempt to do so they will devote less of their income to accumulating other assets and thus drive their prices down and their implicit yields up. This is the short-run effect. As the initial impact of a shift in the demand for money widens, prices of consumption goods and the rental prices of the services from assets other than money will fall, and the stock of real balances will increase. Hence, interest rates and implicit yields in this simplest case will begin to fall back towards their initial levels. The long-run equilibrium will be one in which both the quantity of money, measured in real terms, and the price of money, measured as the reciprocal of the price level, have risen. The usual price-theoretic outcome holds. Appearances to the contrary result from not distinguishing between nominal and real money balances and between the interest rate, the rental price of money, and the reciprocal of the price level, its asset price.

Another reason Temin misunderstands Friedman and Schwartz results from his failure to grasp the importance they attach to monetary policy. The major conclusion reached by Friedman and Schwartz was that the Depression need not have become so severe. Had the Federal Reserve increased high-powered money sufficiently to offset the decline in the deposit-currency ratio, they said, the Depression would not have reached the proportions it did. Their answer to the question of why the Great Depression was great was that policy was inept.

To Temin this is no answer at all, as he makes clear early in his narrative: "The negative argument that macroeconomic policies were not used is hardly the same as the positive argument that these policies would, in fact, have been effective. This latter assertion is difficult, most likely even impossible, to prove, and the debate about it revolves on questions about the structure of the economy rather than the occurrence of specific historical events." (p. 7).

Temin's attempt to divorce questions of causation from questions of policy not only removes much of the common ground for discussion between him and Friedman and Schwartz, but also prevents him from formulating the appropriate scientific question. Questions of causation in a simultaneous system as complex as the economy require the aid of models that explicitly deal with counterfactual events such as alternative policies. The use of counterfactual statements is inherent in the scientific investigations in areas where controlled experiments are impossible (see [17]).

Temin apparently feels he can avoid such statements by looking at the interwar period alone, his idea presumably being that the other things that could affect real income—policy actions and the structure of the economy—are invariant across the three interwar business cycles. Three episodes are not a large sample, which is a problem in and of itself. But suppose he had sufficient degrees of freedom and that policy were in fact constant. Even then, Temin's causative factor—the autonomous shift in consumption—could only explain why so severe a depression occurred when it did within the interwar period. It would say nothing about why it occurred in that period and not in some other one. In this context the word "cause" would have only a very limited meaning. It would be like speaking of the cause of an elevator's crash purely in terms of the law of falling bodies, first having ruled out any discussions about faulty equipment. The explanation in this sense would be correct, but it would not tell anything about how to avoid future crashes.

Moreover, we would question his assumption that policy was the same during those three contractions. Certainly the introduction of deposit insurance in 1934 affected the conditions surrounding the holding of deposits in such a way that the 1937-38 contraction was fundamentally different from the earlier ones. In addition we find it difficult to maintain that monetary policy less broadly defined was the same across those cycles. In 1920-21 high-powered money largely paralleled growth in M_2, and in 1937-38 the contraction in output was preceded by a substantial increase in required reserves. By contrast, in 1929-33 growth in M_2 underwent a number of sudden sharp falls, whereas high-powered money growth remained fairly constant and reserve requirements were kept unchanged by statute.

Temin's alternative explanation of the fall in the stock of money, that it was the result rather than the cause of the Depression, implies that the banking panic of 1930 was itself induced and had no active role in promoting the business decline. Because his analysis is of this solitary episode the explanation can neither be confirmed nor refuted. And if we look beyond this one observation, we find that his conclusion that the 1930 banking panic was induced is at variance with the evidence drawn from the broader historical experience.[1] Cagan, from his analysis of banking panics, concluded that since panics generally appeared in the early stages of a cyclical decline, the contractions themselves could *not* have been their major cause [2, pp. 223-29, 265-68]. And in his broader analysis of cyclical movements in the stock of money, he tentatively concluded that for the few available observations of severe contractions

> The evidence just reviewed makes no sense if monetary developments are assumed to have played a minor role. Without that assumption, each piece falls in place: panics made ordinary business contractions severe when they led to substantial decline in the rate of monetary growth, and not otherwise. Substantial decline in this rate, by itself with no panic, could and has produced severe business contractions. The variety of reasons for decline in monetary

[1] As part of his general investigation of the banking panic of 1930, Temin examines the particular case of the Bank of United States. He argues that its closing was an isolated event with little subsequent repercussions, because, he says the Bank of United States was essentially unsound and its failure directly related to what he terms the "fraudulent" practices of its officers. Consequently, deposit holders did not view its closing as typical and hence did not extrapolate its experience to the banking community at large. In his eyes, it therefore played no causative role in promoting the panic.

This conclusion appears to us to stand on shaky ground. Deposit holders, who were seeing other banks fail, now saw a fairly large New York bank permitted to close. Why they should view this closing as a completely isolated event and not as a harbinger of the future posture of both the government and the banking sectors is unclear. Even if the Bank of United States were somehow or other unsound, why should depositors in other banks believe that this situation was unique to the Bank of United States? The very nature of a financial panic is that the public believes that institutions are not as sound as they appear.

> growth during severe depressions rules out a single cause and rules out, in particular, a sharp fall in business activity as the main reason for the associated decline in monetary growth. The evidence is therefore consistent with and, taken as a whole, impressively favors emphasis on the decline in the rate of monetary growth as the main reason some business contractions, regardless of what may have initiated them, became severe. [2, p. 267]

Cagan's interpretation also calls into question Temin's assertion that the deposit-reserve ratio would have declined even if the deposit-currency ratio had not fallen. The behavior of the deposit-reserve ratio during severe contractions led Cagan to conclude that the larger part of its changes were related to panics and not the result of the cyclical contractions themselves [2, pp. 219-33]. And this is the real question: whether a panic independently produces fluctuations in these ratios that are of overriding importance. It is not whether some movements in the ratios are induced by the cyclical fall in income and interest rates [2, pp. 134-43].

2. *Interest Rates*

Temin's analysis of the behavior of interest rates is of crucial importance to his argument against the monetary explanation of the Depression. His major conclusion is that movements in interest rates indicate that monetary forces could not have played an active role in 1930, the year he considers of prime importance. His argument is unsatisfactory.

The IS-LM framework he sets out earlier in chapter 4 provides the theoretical underpinning of this analysis. Temin argues that given the usual upward-sloping LM curve and a downward-sloping IS curve, a fall in income that is due to a fall in the supply of money will increase interest rates. Correspondingly, a fall in income initiated by some other factors, like autonomous expenditures, will decrease interest rates and presumably the supply of money. In this case a fall in the stock of money would be a consequence rather than a cause of the cyclical decline in income. The direction of interest rate movements is Temin's touchstone for evaluating the role of monetary factors. Nevertheless, he qualifies this basic IS-LM framework in several respects. He states that the rise in interest rates following a money supply decrease would be temporary. He finds that interest rates did rise in 1929, following the decline in monetary growth starting in late 1928. Though this evidence favors a monetary explanation of the initial cyclical downturn, he raises the question of why the Great Depression was so severe given that the increase in interest rates in 1929 was about the same as that observed during the reference cycle contraction of 1920-21. He argues, therefore, that the severity of the Great Depression must be explained with data after 1929. And when he looked at interest rates in 1930, he observed no upward deviation of interest rates from their downward trend, which he presumed was related to falling income. Therefore, according to Temin, monetary factors could not have aggravated the situation in 1930.

We find this argument unconvincing. The basic fact is that interest rates tend to be procyclical. They rise during expansions and fall during contractions. There are factors such as price expectations and real income growth that help produce this cyclical pattern of interest rates and may mask the impact of monetary changes. Temin tries to handle these factors, but he never does so satisfactorily. He dismisses the role of price deflation on short rates in 1930 as empirically insignificant, and tries to account for the effect of negative real income growth by examining the trend in interest rates. In our view, the latter approach is too simplistic. And since wholesale prices fell by over 12 percent from August 1929 to August 1930, we find it implausible that a price decline of such magnitude did not generate expectations of a continued fall in prices in the short term.

In addition, Temin's treatment of the relationship between real income and interest rates is very puzzling in light of his initial theoretical discussion. If, as Temin argues, falling income would cut desired investment and hence interest rates, the IS-LM framework he posits is irrelevant. It offers no basis for a test, since the IS curve could as easily slope upwards as downwards once income is allowed to influence investment. In the case where the IS curve has a positive slope, a leftward shift in the LM curve as money decreased would be consistent with a fall in interest rates.

3. Real Balances

Temin presents one other major argument against the money hypothesis: "The money hypothesis asserts that the decrease in the stock of money caused the Depression, but how can that be if the real stock of money—which figures in the demand function for money—did not fall? Why in other words, should the level of real expenditures and hence of employment have been lower in, say, 1931 than in 1929 since the real stock of money was larger by all of the measures shown in Table 23?" (p. 142).

Temin's argument is invalid for a number of reasons. First he confuses desired and actual real balances. If actual equals desired, then the annual movements in real balances, which Temin discusses, tell us nothing about the presence or absence of monetary stimulation, but only about the shape of the demand curve. Real balances are ultimately an endogenous variable that may move in various ways over the business cycle depending on the relative movements of its determinants. If the demand for real balances depends positively on real income and negatively on the rate of interest, a constant (or rising) level of real balances coinciding with a fall in real income would require a compensating fall in the rate of interest for money holders to remain on their demand functions. Hence Temin's observation that annual real balances were constant or rising while real income and interest rates declined does not provide us with any additional information. He simply cannot reconcile a fall in interest rates with an autonomous decline in the nominal stock of money.

To look at this question more fully, we have used annual data for the periods 1900-1929 and 1900-1941 to estimate a demand-for-money function. Our purpose is to determine whether the same factors that explain movements in real balances in the earlier period also explain them during the Depression. Another way for desired real balances to rise during a contraction—aside from the effect of interest rate movements—is for the demand for money to be a function of permanent income and prices. To take account of this possibility, we have used a general form of the demand function that allows both permanent and measured variables to operate but does not prejudge their importance.[2]

[2]The inclusion of permanent prices in this demand equation follows Friedman's [4] use of this variable as the deflator for desired real balances. We began the estimation period in 1900 to try to eliminate any problems that secular changes in velocity due to increased financial sophistication might have caused. We ended it in 1941 to avoid the effect of the introduction of price controls on velocity.

Below are the results of estimating this equation over the period 1900–1929.[3] The estimated coefficients for the longer period are virtually identical, none differing by more than 0.04 from the ones presented.

$$\underset{}{\ln M/P} = \underset{(19.3)}{-0.84} + \underset{(41.8)}{1.42 \ln y_p} + \underset{(0.5)}{0.08 \ln (y/y_p)} - \underset{(5.6)}{0.41 \ln (P/P_p)} - \underset{(3.8)}{0.11\, r}, \qquad (1)$$

$\bar{R}^2 = 0.985$
$SEE = 0.0257$
D-W = 1.25
(Absolute value of T-statistic in parentheses)

where M is per capita M_2, y is real income as measured by GNP, P is the GNP deflator, r is the commercial paper rate, and the subscript p denotes permanent values of the variables.[4]

These results are inconsistent with Temin's contention that the increase in real balances from 1929 to 1931 is evidence against a monetary interpretation of the Depression. Actual real balances, as we have defined them, rose by 4.1 percent from 1929 to 1931 and then fell by 12.7 percent between 1931 and 1933. The rise in real balances predicted by our equation between 1929 and 1931 is 4.2 percent, almost identical to the actual rise. The 12.8 percent decline predicted for the latter period is similarly close to the actual decline. Hence, we would argue that the initial rise and subsequent fall were due to changes in the determinants of the demand for real balances and that movements in real balances are a poor measure of the degree of monetary ease or restraint. The close correspondence between actual and predicted movements in real balances, moreover, is not peculiar to these years alone. The closeness of fit can be seen from an analysis of the residuals. The root-

[3] If permanent rather than measured income is the appropriate scale variable the coefficient of ln (y/y_p) would be zero and if permanent rather than measured prices is the appropriate deflator for desired money balances the coefficient of ln (P/P_p) would equal minus one. If, on the other hand, measured prices is the correct deflator, the coefficient of ln (P/P_p) would equal zero. Our estimate of the coefficients presented in equation (1) indicate that we cannot reject the hypothesis that our definition of permanent income is the appropriate scale variable, since the coefficient of ln (y/y_p) is insignificantly different from zero. Our results for permanent prices are less clearcut. Since the coefficient of ln (P/P_p) is significantly less than zero, the use of measured prices alone is incorrect. But because it is significantly less than minus one, measured prices provide additional information not contained in our series for permanent prices. Some lag on prices seems appropriate in determining desired real balances, but our proxy for permanent prices probably adjusts to current changes in the price level too slowly. The use of current prices alone underestimates the rise in velocity during World War I and the fall in velocity during the Depression.

Since the Durbin-Watson statistic for both estimation periods was low, we reestimated the money demand equations to correct for first-order autocorrelation of the error terms. These results are basically consistent with the OLS estimates presented in the text and do not alter any of our conclusions.

[4] The logs of permanent income and prices were computed as the geometrically declining weighted averages of the logs of their current and past actual values with an initial weight in both cases of 0.33.

Data for M_2 came from [7]; for population from [6]; for the 4- to 6-month commercial paper rate from [14], for the period up until 1935, and from various Federal Reserve *Bulletins* thereafter; and for GNP in constant prices, the GNP deflator and total consumption in constant prices, which we use below, from [11].

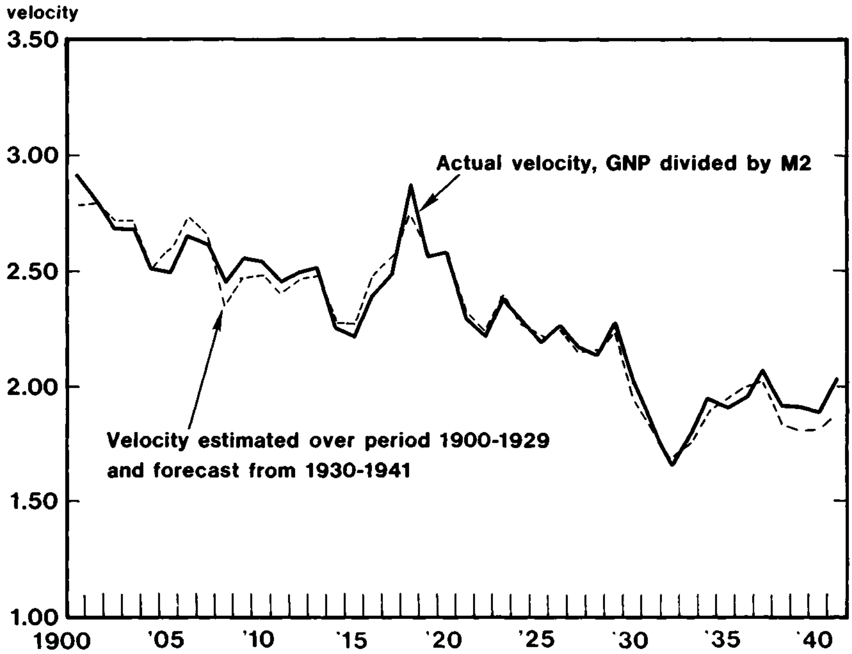

Fig. 1. Actual and Estimated Velocity (1900-1941).

mean-squared error for the period of fit (1900-1929) is 0.0235, whereas for the years 1930-33 it is 0.0285. Another way of illustrating how well estimated tracks actual real balances is to convert them into actual and estimated levels of velocity by using actual real per capita GNP. As the chart indicates, the movements in velocity calculated from our real balance equation, tracks the actual movements very closely over the entire period, including the Depression. Since our equations can account for movements in the demand for money during this period, the rise in real balances in the early thirties does not disturb us as much as it does Temin.

The other criticism we have of Temin's use of real balances is its disregard of the dynamics of income change. Temin is right when he says that "falling prices and lower prices are not the same thing" (p. 143), but he does not seem to realize the full implication of that statement. A 20 percent fall in the stock of money accompanied by a 20 percent fall in prices will not necessarily leave real output unaffected, even though real balances remain constant. If a fall in prices, whatever its cause, is not fully expected, it will have an adverse effect on aggregate supply.[5] The supply effect of unanticipated price changes has in recent years become central to explanations of how changes in economic policy affect real output (besides [12] see [15]); it is distressing that Temin ignores this literature. In fact, there is no discussion in the book as to how the fall in nominal demand that occurred in the

[5]This is a result of an aggregate supply function such as the one presented in [12], in which output depends on the price of output relative to the expected price of inputs. Because of imperfect information on input prices an unexpected fall in all prices will be perceived by suppliers as a relative fall in output prices.

Depression, whatever its cause, was divided between changes in output and changes in prices. This neglect seriously limits the usefulness of the book, since it is precisely in this area that a reexamination of the Depression is most needed.

4. The Spending Hypothesis

Temin's version of the "spending hypothesis" is a departure from the conventional analysis. Instead of attributing the autonomous fall in spending to a decline in investment, he argues that it occurred in the area where most Keynesians would least suspect, personal consumption. His evidence for this proposition is that consumption declined in 1930 by far more than is consistent with either the decline in income or the behavior of consumption spending during the other two interwar depressions.

Temin estimates a number of simple consumption functions for both total and nondurable consumption from annual data for the period 1919–41 and then analyzes the residuals from these equations. The principal form used is a regression of nominal consumption spending on nominal current disposable income and nominal wealth. He finds that actual consumption was above the predicted level in the depression years of 1921 and 1938, but below it in 1930. He, therefore, regards 1930 as unusual.

Since a decline from a very high positive residual to a low positive residual would in Temin's framework represent a deflationary reduction in the autonomous component of consumption, he also compares the yearly first difference in residuals for 1921, 1930, and 1938. Averaging the results obtained from different data, Temin finds a year-to-year change in residuals for total consumption spending of $6.1 billion in 1921, -$1.43 billion in 1930, and $1.81 billion in 1938. On the assumption that overprediction of the decline in consumption is the norm for this type of consumption function during depressions, Temin estimates the autonomous change in spending as the difference between the change in the residuals for 1930 and the average of the changes for 1921 and 1938. He calculates this autonomous fall in consumption as being $5 billion, two-thirds of the total fall in consumption between 1929 and 1930.

There are several problems with Temin's analysis, not the least of which is his willingness to interpret residuals from very simple consumption functions as almost conclusive evidence of an autonomous fall in consumption. Given the limited number of consumption functions Temin estimates, he is brave indeed to conclude "the fall in consumption must be regarded as truly autonomous, which in this case means also unexplained" (p. 83). The use of the adverb "must" is particularly unjustified, since Temin's results do not support his conclusions—as we shall proceed to demonstrate.

Temin's conclusion that consumption behavior in 1930 was aberrant hinges on the assumption that the large *positive* residuals he observes in the other interwar depressions, especially that of 1921, are normal in depression years. To test this assumption we estimated a consumption function for the longer period 1889-1941, which gave us twice as many severe contractions to appraise as in Temin's sample. The formulation of the consumption function we used was one that is roughly consistent with the permanent income hypothesis.

We regressed the log of real per capita total consumption on the log of real per-capita permanent income and the log of transitory income, which we defined as simply the difference between the logs of measured and of permanent income. As in [3], we used transitory income in addition to permanent income to try to reduce the effect of having purchases of durable goods, as opposed to their flow of services, included in our definition of total consumption. The reason is that purchases of durable goods are more cyclical than their service flows, and hence will be more dependent on transitory than permanent income. The estimated equation was[6]

$$\ln c = \underset{(21.5)}{-0.244} + \underset{(51.4)}{1.12} \ln y_p + \underset{(4.08)}{0.37} \ln (y/y_p). \tag{2}$$

$\bar{R}^2 = 0.98$
$SEE = 0.0359$
D-W = 0.64

As in Temin's equations, the Durbin-Watson statistic is very low. His approach, which we only followed for the sake of comparability, was to examine the year-to-year change in the residuals rather than their levels. The average absolute value of the change in residuals for this period was 2.25 percent, whereas for 1930 it was only -1.5 percent. On this basis alone, 1930 is far from unique. The change in the residual for 1938 was -1.3 percent; 1921 had, as Temin reported, a very large positive change of 6.6 percent. The average value of the change in residuals for the five severe contractions, other than the Great Depression, was positive but less than 1 percent, which hardly shows that overprediction of the fall in consumption is a normal feature of depressions. And even this may be misleading, dominated as it is by the very large positive value for 1921.

Viewed from a different perspective 1930 again seems rather ordinary. The negative value for 1930 is one of the smaller negative changes we found. Of the twenty-seven negative changes in residuals, fifteen are larger than 1930. In fact, the figure for 1925 was negative and four times as large as in 1930, which prompts the question of why if a negative residual in 1930 was sufficient to precipitate a severe depression an even worse one did not occur in 1925.

The same thing appears to be the case when Temin's own equations are examined in greater depth.[7] Thomas Mayer [11] has reestimated Temin's consumption equations and various alternative specifications for the interwar period. His results show that large negative residuals were hardly unique to the Great Depression. In addition, Mayer reestimated Temin's consumption equations with and without a correction for autocorrelation of residuals. In no case did he find the residual or change in residuals for the year 1930 to be significant.

[6]We used GNP rather than disposable income because of data limitations. And unlike Temin, we used real per capita measures of consumption and income.

[7]Temin's results are also quite sensitive to the specification of the consumption function and the period of analysis. In an appendix Temin estimates a permanent income consumption function. This equation underpredicts the fall in total consumption in all three of the interwar depressions. In fact, if we compare the change in residuals for 1930 with the average of the change in residuals for 1921 and 1938, we would have to conclude, following Temin's logic, that there was an autonomous increase in consumption spending in 1930.

Even if the evidence supported Temin's claim that an autonomous fall in consumption precipitated the Great Depression, one could not conclude that the fall in consumption "caused" the Depression. The question would remain why a shift in spending caused a severe depression in the 1930s, but not in other years in which such shifts occurred. In other words, what had happened to the structure of the economy in the 1930s, and in no other era, to make the economy react so violently to a random shift in spending?

5. *Conclusion*

Before we had read Temin's book our own feelings were that a monetary explanation of the Depression answered many, though not all, of the interesting questions that the economic turmoil of that era posed (see [8, 9]). It would not have surprised us, though, to find out that nonmonetary factors played a part throughout major depressions. Nor would we have been startled to see evidence indicating that many a lesser depression was greatly influenced by nonmonetary phenomena. In short, we viewed the explanations of the Depression that either assumed that velocity was a will-o-the wisp or that monetary policy was like pushing on a string as largely disproven, but we would have ruled out little else.

The very framework of Temin's book, however, precludes the possibility of adding to our understanding of additional factors that contributed to the magnitude of the Great Depression. He presents the "money" and "spending" hypotheses in their most extreme and mutually exclusive forms. But neither his evidence for the one nor against the other holds up. Hence the book has made no contribution to the question of what caused the Depression to be so great. Nor does it shed much light on aspects of the Depression that remain puzzling. The interaction between monetary and real forces is one such area; the division of nominal income between prices and real income is another. Both require further study.

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Housing and the Money Market, by Roger Starr. New York: Basic Books, 1975. vi + 250 pp.

In recent years, books attempting to clarify complex economic and financial issues in nontechnical language for the general public have been published with increased frequency. But little attention has been devoted to describing and evaluating the system by which housing activity is financed. This is surprising, considering the fact that expenditures on shelter often represent the single largest financial commitment undertaken by a consumer over his life cycle.

In *Housing and the Money Market,* Roger Starr has gone far to fill this vacuum by analyzing the basic financial framework within which housing markets function. The book is divided into three parts:

1. *Fundamentals:* a broad discussion of the relationships between various financial and nonfinancial resources and housing production in the United States. Separate chapters in this section are devoted to discussions of the relative roles of mortgage financing, equity, existing housing stock, physical inputs of housing production (land, labor, materials, etc.) and the money and banking system.

2. *The Lending Institutions:* a description of the major private and local govern-