

DISCUSSION PAPER NO. 330
MONETARIST INTERPRETATIONS OF THE GREAT DEPRESSION:
A REJOINDER*

by
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November 30, 1978

* This research has been supported by the National Science Foundation. Conversations with Allan Miltzer were helpful in the development of this rejoinder.

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The primary contributions of our paper are to broaden the discussion of the Depression beyond the behavior of U.S. money and income in the 1929-31 period--the issue with which Temin and his critics are mainly concerned--and to argue (with Haberler) that "explanations which run in terms of one single cause . . . should be regarded with suspicion." Unlike the recent papers that limit their attention to the role of money and other factors as determinants of aggregate demand shifts, considerable attention in our paper is devoted to the nature of aggregate supply behavior. Finally, unlike others who concentrate exclusively on the relation of money, income, and prices in the U.S. alone, we base some of our conclusions on a comparison of the U.S. with an aggregate of six European countries.

We find that both extreme monetarist and nonmonetarist interpretations of the decade of the 1930's are unsatisfactory and leave interesting features of the data unexplained. The extreme monetarist interpretation suffers from its inability to explain the severity of the initial collapse in income in 1929-31, the steady weakening in the correlation between money and income as the 1930s progressed, the failure of monetary factors to explain the nature

and timing of the 1938-41 recovery, and the weakness of the self-correcting mechanism of price flexibility. But extreme nonmonetarist explanations suffer as well by failing to place sufficient emphasis on the causal role of money in the collapse of U.S. nominal income after the fall of 1931, on the effect of more expansionary monetary policies in Europe as a central explanation of the relatively milder contraction that occurred there, on the close association between money and income in the 1937-38 recession, and on the inability of the Phillips-curve approach to explain price behavior during the decade of the 1930s.

Given the wide range of topics covered in our paper, we naturally did not expect the comments of our discussants to be in agreement on every point. Thus we are gratified at the wide range of central issues on which Meltzer appears to be in substantial agreement. Most of Meltzer's area of disagreement involve interesting problems of interpretation on questions that have no final and definitive answers. Lothian's comments appear at first glance to contain substantive evidence that contradicts some of our main conclusions, but upon further examination his evidence appears to be fully consistent with ours. Finally, Temin's comments contain a brief section that harshly condemns our interpretation of his own position but fails to alter our view that Temin's position is both extreme and unsupported by the evidence he examines.

Methodology and Extreme Positions

Although Meltzer's point of departure is his basic agreement with our rejection of extreme, single-cause interpretations of the Depression, both

Temin and Lothian are unhappy with our four-way categorization of views, albeit for opposite reasons. Temin resists being type-cast as an anti-monetarist extremist, whereas Lothian is uncomfortable with our middle-ground refusal to accept simple, single-cause explanations and instead wants to be told "exactly what to conclude."

Temin's objection begins with our interpretation of a quote from his book that we cited to support our categorization of his position as "extreme nonmonetarist." We emphasized Temin's denial of "any effective deflationary pressure from the banking system" between October 1929 and September 1931—^{1/}

^{1/} Incidentally, we must object to Meltzer's inaccurate characterization of Temin's view: "In Temin (1976) he argues that there is no evidence that U.S. monetary policy was an independent cause of the 1930's depression." On the contrary Temin is quite consistent, both in his book and in his comment here, in accepting a primal causal role for monetary policy beginning in September 1931.

Temin prefers to add emphasis to his own words as follows: "There is no evidence of any deflationary pressure . . ." Whatever the emphasis, Temin's absolute denial of "any" contractionary effect remains and seems to us prima facie evidence of any extremist approach to economic analysis.

But let us accept Temin's emphasis on the words "no evidence". What evidence is needed to convince oneself that the bank failures must have had some contractionary influence? Nowhere does Temin deny any element in the following list:

- (1) Some banks failed in late 1930'.
- (2) Deposits vanish when a bank fails.
- (3) Some individuals are forced to reduce spending when their bank deposits disappear.^{2/}

^{2/} The effect of liquidity constraints on individual consumption decisions is analyzed in Chapter 2 of the book by Barro and Grossman on our reference list.

- (4) Aggregate nominal expenditure declines when some individuals are forced to reduce their spending.

At least one step in this chain of reasoning must be denied for Temin to claim that there was no deflationary pressure exerted by bank failures, and yet Temin provides no such denial. This is one basic element in our criticism of Temin's position.

The second basic element in our criticism is the failure of Temin's own evidence to support his extreme claim. Here Temin asserts that our analysis is "hopelessly confused" and that we "ride roughshod" over the distinction between counterfactual and descriptive history. Temin apparently believes that his own "historical statement" that explains "the events that actually happened" is immune from the test of logical consistency that we applied. Yet Temin's historical analysis of the evidence remains unconvincing in its denial of any role for bank failures, because every fact cited by him is logically consistent with a model in which the destruction of money reduces aggregate expenditure.

We believe that the difference between our analysis and Temin's can be clarified in terms of his IS-LM model by writing down two quite different statements:

(1) In the absence of any leftward shift in the IS curve, the observed behavior of nominal risk-free interest rates and the real money supply is inconsistent with the hypothesis of a contractionary effect of the reduction in the money supply.

(2) Given the acceptance of a leftward shift in the IS curve (that could have stemmed from either real spending shifts or deflationary expectations), the observed behavior of nominal risk-free interest rates and the real money supply are completely consistent with the hypothesis that any decline in the money supply has a net contractionary impact on nominal spending.

Temin's evidence, both in his book and in his comment here, consists of an examination of the behavior of nominal risk-free interest rates and of the real money supply. Our position is that this evidence is compatible with either statement (1) or (2) and thus, for anyone (including Meltzer's present comment and the Friedman-Schwartz book) willing to accept the relevance of nonmonetary shocks, provides no evidence at all that the bank failures had no contractionary effect. Temin's claim that we reached "almost precisely the conclusions that I did by entirely independent means" refers to our acceptance of the leftward shift of IS involved in statement (2), our denial of the absence of a leftward shift required for the acceptance of statement (1), and to our Granger simulations that attribute three-quarters of the decline in nominal income in 1929-31 to nonmonetary factors.

But our agreement that nonmonetary factors played a major role in 1929-31 merely supports statement (2) above and does not imply any endorsement of Temin's extreme claims of a zero effect of bank failures or of his own method of analysis. The facts we examined support a role for contradictory monetary policy, because; (a) monetary growth did decelerate in 1929, (b) there was an impact of lagged money on income in the 1920-28 period, and that therefore (c) there was a contractionary influence of the effect of lagged money sufficient to explain one-quarter of the drop of nominal income in 1929-31. Twenty-five percent is closer to zero percent than to one hundred percent, and to this extent we are closer to Temin than to Schwartz or Lothian. But twenty-five percent is not the same as zero percent, and thus we must disassociate ourselves with the extreme denial of any contractionary effect of the 1930 bank failures that Temin so relentlessly pursues and erroneously attributes to us when he states that the conclusion of "no evidence of any contractionary pressure is ... reaffirmed both here and in Gordon and Wilcox's paper."

The problem with many of Lothian's comments is that they evince a preference for definitive monocausal statements and an unwillingness to accept our basic conclusion that both monetary and nonmonetary factors were important. Our paper left Lothian "not sure exactly what to conclude". Our framework "precludes any firm conclusions." Further, in his unwillingness to accept our middle ground between monetary and nonmonetary extremes, Lothian quite inaccurately claims that in our eyes "non-monetary forces . . . explain virtually everything before 1931."^{3/}

3/ In each of these three quotes, the emphasis added is ours.

Most of Lothian's reluctance to accept our conclusion involves matters of degree rather than kind, that is, the choice of words used in describing the balance between monetary and nonmonetary factors. Our position is best characterized as stating that there were several important episodes during the 1929-41 interval. The lagged behavior of the money supply explains some portion of variations in income during all phases of this twelve year period, probably a minority of the variance during the intervals 1929-31 and 1938-41, and probably a majority during 1931-38. Nonmonetary factors explained a majority (but not all) of the variance of nominal income during 1929-31 and 1938-40. Lothian's evaluation makes no distinction between intervals and insists that "the degree of emphasis does matter" in distinguishing whether money was a "prime mover" in the Depression or explains "at most a minor part." Because he makes no distinction between intervals, and refuses to accept the intermediate view that both monetary and nonmonetary factors were important, in varying degrees at different times, he simply misses the main thrust of our conclusion that both monetary and nonmonetary factors were "prime movers."

Money and Income

In our discussion of Temin's evidence on the behavior of interest rates and the real money supply, we argued that a leftward shift in the IS curve was required to explain the evidence. The importance of nonmonetary shifts

in spending as a cause of the leftward IS shift can be denied only if one were willing to claim that the IS shift was caused by the emergence of deflationary expectations. At least during the first three quarters of the contraction neither the CPI nor interpolated GNP deflator dropped outside of the range observed in the 1923-29 period, a fact that made us dubious of the potential importance of deflationary expectations between late 1929 and mid-1930, however important they might have been later. Even if agents looked back to 1920 or 1902, or to the interwar experience in Britain, as Lothian suggests they should have done, there was no evidence available as of mid-1930 that would have led them to view the first three quarters as a prelude to an unprecedented deflation, rather than a short one-year recession of the duration of, say, the 1907-08 or 1920-21 episodes.

The evidence on money and income to which Lothian devotes most of his attention, however, does not involve price expectations but rather the relation between nominal income, current money, and lagged money on an annual basis over the longer period 1893-1928.^{4/} Unfortunately, all of Lothian's

^{4/} Lothian implies that our quarterly data are "plagued" by the use of interpolated income data incapable of making "fine distinctions." Yet the crucial issue is to disentangle the uni-directional causation from money to income from the simultaneous two-directional causation revealed in annual data. It was precisely to avoid claims that our interpolation procedures introduced measurement error that we also ran the identical tests on published monthly money and industrial production data (our Table 2) and obtained results similar to those in the quarterly data used for the simulations.

results listed in his Table I are completely useless, because of his mistake of including current money in an annual equation that attempts to explain movements in current nominal income. If there is any feedback from income to money, whether within the contemporaneous month or quarter or year, the coefficients on current money in his income equation are biased upward, perhaps by a large amount, and the Lothian simulation results are biased towards the conclusion that money fully explains the drop in income during 1929-33. Put another way, without any evidence that there was no feedback from income to money within the current year (and he provides no such evidence), Lothian's results in Table I can be interpreted as saying no more than that "income fell because money fell because income fell".^{5/}

^{5/} Lothian's remark (p. 11) that transitory income can explain the behavior of consumption and housing is, of course, plagued by the same simultaneity problem, since consumption and housing expenditure are part of transitory income!

Our quarterly regressions of income on money and money on income also lead Meltzer to raise a question of interpretation. In his comments on the regressions with money as dependent variable in our Table 5, he objects to our conclusion that "the dominance of the contemporaneous correlation in the decade of the 1930s adds plausibility to the reverse feedback hypothesis that the reflex effect of business on money was a primary determinant of shifts

in the money supply." Instead, Meltzer argues, "reverse causation is not impossible, but it is implausible that the relation of base money or money to income is mainly the result of reverse causation in the thirties."

Here the first problem is semantic. When we say that a factor, say reverse causation, was "a primary determinant," we do not mean that there were no other primary determinants. For instance, we explicitly recognize the primary role of gold inflows in expanding both base money and the money supply during 1938-40. The contemporaneous correlation between money and income, even in monthly data, makes it impossible to rule out a role for reverse feedback. To go further and argue that reverse feedback must have occurred, one leaves the realm of hard evidence and must rely instead on conjecture. One conjecture is that a causal link from money to income should have taken longer than one quarter or one month to occur, based on econometric evidence from postwar quarterly models, leaving reverse feedback as a plausible explanation of the strong contemporaneous feedback observed in the 1930s.

Meltzer raises legitimate questions about the channel by which reverse feedback might have occurred. One channel he fails to discuss is the increase in the cash-holding ratio during 1931-33, and then the subsequent reduction in the ratio as the economy recovered during 1933-35. If declining income and prices help to induce bank failures, and bank failures help induce changes in the currency ratio, then there is a "reverse causation" link from income to money that is independent of open-market operations or gold flows and that will cause an upward simultaneous-equations bias in any attempt (like Lothian's Table I) to include current money in an equation explaining

the behavior of income.

Nonmonetary Factors Operating during 1929-31 and 1938-40

Meltzer's main disagreement is with our claim that the majority of the decline in nominal spending in 1929-31 and of the recovery in 1938-41 was due to nonmonetary factors. Regarding 1938-41, we emphasize the sluggish behavior of investment during 1938-40 and the contrasting buoyancy of the fiscal expansion after mid-1940 as nonmonetary factors explaining why nominal income grew at such different rates during the 1938-41 period when the growth of M2 was rapid and relatively constant. Meltzer does not really disagree with our analysis but rather goes beyond it by linking the sluggish recovery of investment between 1938-40 to pessimistic anticipations induced by anti-business New Deal measures. It is possible to admit that Meltzer's factors are plausible as a partial explanation and consistent with monetarist emphasis on the actual harm done by government policy. But Meltzer does not actually provide any evidence that would sort out the role of anti-business government measures. Pessimistic anticipations and the 1938-40 slump in stock prices, in particular, may well have been a symptom of the sluggish recovery of spending rather than its cause. Sluggish income growth and "bearish anticipations" tend to go together and presumably interact. To use a modern analogy, would Meltzer accept the hypothesis that sluggish U.S. income growth during the 1973-76 period was a result of anti-business measures introduced by Presidents Nixon and Ford, on the basis of the pathetic performance of the Standard and Poor's average during 1973-76?

Meltzer raises two objections to our discussion of the behavior of

housing and stock prices in the late 1920's. First, the decline in household formation began in the mid-1920's; thus it is difficult to accept that the adjustment to this factor did not take place in the 1920's. We agree and in our paper pointed to the rapid decline in housing investment that occurred in 1928 and 1929. Our argument is not that the adjustment had not already begun, but that the decline in household formation required a continuous downward movement in the ratio of housing investment to GNP during 1926-31 that aggravated the decline in income that occurred during 1929-31.

Further, Meltzer argues that the decline in stock prices in late 1929 was not an autonomous event, because industrial production had already fallen by 2.5 percent before the stock market collapsed in late October. He objects to statements made about the decline in economic variables from the 1929 peak, and claims that the 1929 expansion and 1930 collapse need to be explained together. Here Meltzer's position is close to ours, when we argue that the 1929 boom in consumption and the stock market was superimposed on an economy weakened by the collapse in housing investment. To the extent that the stock market boom of 1928-29 was a bubble that had to end, because stock values could not be sustained for long at triple their 1923 values, both we and Meltzer agree that the 1929-30 boom and collapse were part of the same essentially nonmonetary phenomenon. As the discussion of our paper points out, however, monetary growth was rapid in 1928 and then came to a halt in early 1929, thus aggravating the boom and collapse in both output and the stock market and requiring the conclusion that the timing of the 1929-30 episode was partially monetary in origin.

Prices and Output

Lothian's first criticism of our aggregate supply analysis starts from the following equation relating the cyclical component of output to price shocks ($P_t - P_t^*$):

$$y_{ct} = \alpha + \beta y_{ct-1} + \lambda(P_t - P_t^*).$$

Our statistical analysis demonstrates that deviations of real output from trend during the 1930's cannot be explained as responding only to price "surprises" unless the expected price level totally failed to adjust downward to the deflation that had actually occurred. Otherwise, with the price level almost constant after 1934, where were the "surprises" needed in the Lucas theory to explain the low level of output and high level of unemployment? Lothian introduces his equation in order to point to a high level of β , the serial correlation of the output deviations, as another possible explanation of the long duration of the Depression. But here Lothian misses the point of recent critiques of the Lucas approach. A drop in nominal income, as occurred in the 1930's, must be divided by definition between a drop in real output and a drop in the price level. If the drop in real output persists, as when β is high, then this means by definition that downward price adjustment is sluggish in the face of low output. But if price adjustment is sluggish, we are in the disequilibrium world of Barro and Grossman, and the Lucas model of price surprises simply does not apply, because individual economic agents are forced off their

notional supply curves. Thus Lothian's own reinterpretation of our results reinforces our critique of the Lucas equilibrium aggregate-supply approach to the analysis of economic fluctuations.

Finally, Lothian's comparisons of the U.S. and U.K., while they put the evidence together in an interesting way, do not contribute any new insights that would cause us to change the main conclusions of our paper. First, Lothian's middle column labelled "Contraction" lists figures for nominal income, money, and velocity, showing that in each episode the decline in nominal income was associated with a larger decline in velocity than in money, supporting our emphasis on the relevance of nonmonetary factors. Second, the decline in nominal income mainly took the form of declining prices in 1920-21 in both countries and in 1927-34 in the U.K., whereas the decline in real output was greater than that in prices in 1927-34 for the U.S. This merely points to the same puzzle with which we ended our paper: the mystery is not why prices rose so much after 1933, a phenomenon that the U.S. and Europe shared in common despite the greater amount of government price-raising legislation in the U.S., but rather why the price level was relatively less flexible downwards in the U.S. than in Europe during 1929-33.