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4 Understanding 1929–1933

Nearly half a century after August 1929 the debate has resumed about what produced the unusual length and extreme severity of the ensuing 43-month contraction. No new facts about that business contraction have become available that have led to revision of earlier judgments. Rather, new hands have imposed new, or reimposed old, patterns on the known facts. So we must ask whether the new hands do explain the known facts better than did earlier investigators or whether their explanations must be rejected because they do not fit the full set of known facts.

In the '30s, '40s, and '50s, the prevailing explanation of 1929–33 was essentially modeled on Keynesian income-expenditure lines. A collapse in investment as a result of earlier overinvestment, the stock market crash, and the subsequent revision of expectations induced through the multiplier process a steep decline in output and employment. The collapse in investment was consistent with a procyclical movement in interest rates and velocity. The revision of expectations in turn set off a demand for liquidity that could not be met. The attempt to meet it, however, forced widespread liquidation of bank loans with a resultant decline in the value of private claims and debts, leading to the failure of nonfinancial corporations, to bank insolvency, and to runs on banks. Try as the Federal Reserve System might, its easy money policies—

For the Granger causality tests and the preparation of tables 4.1 and 4.2 I am indebted to Thomas J. Sargent. Milton Friedman, who read the first draft of this paper, suggested that I include the Granger test results and the chart of the inverse of the price level. I have also restructured the paper on his advice. My indebtedness to him in a more general sense should be obvious. I have also benefited from comments by Arthur E. Gandolfi and James R. Lothian. R. A. Gordon gave me detailed comments, which I acknowledge, as I also do the views expressed by the discussant, Peter H. Lindert. I am grateful to Linda Dunn for preparing the figures and tables other than 4.1 and 4.2.

as evidenced by the decline in short-term interest rates until the summer of 1931—did not stabilize the economy.

In the Keynesian story, the quantity of money as such played no important part in the explanation of 1929–33. The story was faithful to the prevailing belief that the importance of money can be measured by the behavior of interest rates, and since econometric tests that included an interest rate revealed no significant effect of the variable, that seemed to dispose of the need to consider the behavior of money.

Fifteen years ago when Milton Friedman and I reviewed the facts about 1929–33, we did, of course, have at hand a new monthly series on the quantity of money outstanding. Earlier estimates by Currie (1935) and Warburton (1945), however, had measured the extent of the decline in the quantity of money over the contraction, so we did not discover the fact of sharp decline. What we did illuminate was the process by which the decline in the quantity of money was produced. This shed a new light on the course of the contraction. There were distinct stages of the contraction—it was not all of one piece.[1] The stages we noted included: (1) The period prior to the first banking panic—that is, August 1929 to October 1930. This period encompassed the stock market crash in October 1929, to which the Federal Reserve responded by a short-lived increase in the quantity of money. Subsequently, an earlier decline in the quantity of money was resumed, but there was no attempt by banks to liquidate loans or by depositors to shift from deposits to currency. During this interval, the contraction would have been defined as severe relative to earlier ones. (2) The first banking panic, covering the final quarter of 1930, when the real economy markedly worsened. (3) The first quarter of 1931, when signs of revival were nipped upon the onset of a second banking crisis in March 1931. (4) The last half of 1931, when the response of the Federal Reserve to Britain's departure from gold was accompanied by another outbreak of banking panic and a substantial deepening of the real decline that persisted through the first quarter of 1932. (5) The second quarter of 1932, when the Federal Reserve undertook open-market purchases, following which there was a widespread revival in the real economy in the summer and fall. (6) The final six months of the contraction, when problems with the banks spread, the real economy turned downward again, and the contraction ended with a collapse of financial markets.

Thus, after the 1929 peak in business, five negative shocks in turn destabilized the economy: the stock market crash and four episodes of banking panic—in the final quarter of 1930, from March to June 1931; from August to the end of the year (the response to the currency crisis abroad); and a final outbreak of panic in the last quarter of 1932, culminating in the Bank Holiday of March 1933. There was at least

one positive shock—the open-market purchases of March–July 1932—and possibly a second, if we count the short-lived open-market purchase following the stock market crash. In our analysis, we distinguished the contraction in general from the banking and liquidity crises that punctuated its course. Our main theme was that the effect of whatever economic forces produced the contraction was magnified by the unprecedented decline in the quantity of money resulting from the banking crises. Our ancillary judgment was that the Federal Reserve System could have prevented the monetary consequences of the banking crises but failed to do so.

Two published studies by Kindleberger (1973) and Temin (1976) have recently challenged the interpretation of 1929–33 in *A Monetary History,* and an unpublished study by Abramovitz incidentally also offers a dissenting opinion.

Kindleberger's focus is on the world economic system in the interwar period. He attributes an important role, in propagating the world depression, to rising stocks and falling prices of world primary products after 1925 and to maladjustments inherited from World War I.[2] He believes that the contraction in the United States was initiated by a decline in housing—that it started as a mild contraction and was transformed into a depression by the stock market crash. In his view, during the initial phase of the depression, money was abundant and cheap, but the spread between interest rates on high-grade and low-grade assets proves that an increase in the quantity of money by itself would not have been helpful, that "one also had to improve credit-worthiness by improving the outlook" (p. 138). He traces the intensification of the depression to repercussions of the initial world contraction on the economies of the countries producing primary products, not to developments in the United States, and emphasizes the halt in international lending to peripheral countries. Given their limited gold and foreign exchange reserves, they were forced to sell their primary products for whatever they would bring. Tariff increases and quota restrictions by the industrial countries exacerbated the problems of the primary-producing countries and reduced world trade. On his reading, falling security prices and commodity prices, for which Kindleberger assigns no special responsibility to the United States, made banking systems everywhere vulnerable and led to the financial crisis of 1931. He argues that the open-market purchases by the Federal Reserve in 1932 did little to relieve the squeeze on the economy outside financial markets and that only two means were available to achieve world recovery: (1) simultaneous programs of government spending in all countries, and (2) simultaneous devaluations to create gold profits that would be available for spending. Conspicuous by its absence is any discussion of monetary expansion and the liberation of monetary policy made pos-

sible by floating exchange rates in the 1930s, a regime Kindleberger deplores.

Peter Temin's story about the contraction concentrates on the United States during the period beginning with the stock market crash through the end of 1930. It seems that it was not a disturbance in investment behavior but an unexplained decline in consumption and a decline in exports as a result of world agricultural depression that produced a decline in U.S. income. This set off a decline in the demand for money to which the supply of money passively adjusted. Action by the Federal Reserve to meet the demand for increased currency holdings, as depositors attempted to convert deposits into currency in the several rounds of bank runs that characterized the contraction, or to provide increased reserves to banks would not, in his view, have prevented a decline in the quantity of money. Had there been no endogenous bank failures that served to reduce the quantity of money, the deposit-reserve ratio would have declined in response to the fall in short-term interest rates, and the decline in the quantity of money would simply have occurred through a different route. Temin does not deny that action by the Federal Reserve to restrict monetary growth may have played a role in initiating the economic downturn in 1929. What he disputes is whether the Federal Reserve engaged in such restrictive monetary action subsequently, until the Federal Reserve's sharp rises in discount rates in response to gold outflows in September 1931 when Britain abandoned gold. I defer until a later point the specific points on which he challenges *A Monetary History.*

Abramovitz's perspective is much broader than cyclical developments in 1929–33. In his unpublished study, he attempts to introduce the behavior of money into his analysis of U.S. long swings, which hitherto had concentrated on their real aspects. The usual model of long swings views waves in real income as an interaction between real expenditures on the one hand and growth of stocks of labor and capital on the other, each explained by real variables. In his current work, Abramovitz finds that nominal income growth parallels real income growth and proceeds to partition the swings in nominal income growth into its monetary elements—money-stock growth and velocity change—and, in turn, money-stock growth into its components. He proposes a model in which nominal income growth and its handmaiden, money-stock growth, are governed by the growth rate of the sum of current merchandise exports and net capital imports. U.S. factors affecting immigration, internal migration, railroad profits, the demand for urban buildings, territorial settlement, and other real matters, on this view, were important through their effects on the growth of merchandise exports and net capital imports. He then applies this hypothesis to the long swing centering on 1929–33 and suggests that the great declines

in merchandise exports both after World War I and after 1929 limited the scope for Federal Reserve action. He regards the great declines of nominal income as inevitable, short of implausibly drastic accelerations in the creation of Federal Reserve credit or in the high-powered money multiplier sufficiently large to offset the declines in the sum of merchandise exports and capital movements.[3]

Underlying these three reevaluations of old facts is the view that income changes dominated money changes during the interwar period. A test of that view is now possible and is presented in the first section. I consider the appropriate measure of monetary stringency in the second section. The reciprocal of the price level in the interwar period counters what Temin regards as the decisive evidence on the price of money as measured by short-term interest rates.[4] I then comment in section 4.3 on the decline-in-spending explanations, including Temin's version, and in section 4.4 on the explanations stressing international factors, referring not only to Kindleberger and Abramovitz but also to recent work on the Great Depression by Haberler, Meltzer, and Brunner and Meltzer, among others. I conclude in section 4.5 with some summary observations about cyclical experience with particular reference to 1929–33. In an appendix, I take up explicit criticisms of the explanation offered in *A Monetary History.*

4.1 Money and Income: A Test of Causality

The three reevaluations of the interwar years implicitly or explicitly regard the direction of change between income and money as running from income to money. Temin says his purpose is to discriminate between the "spending hypothesis" and the "money hypothesis," with which he identifies *A Monetary History,* as an explanation of 1929–33. As he states the money hypothesis, for Friedman and Schwartz (1963*a*):

> Either changes in the stock of money caused income to change, or vice versa. The resolution was equally simple. The stock of money was determined by a variety of forces independent of the level of income . . . and the direction of causation therefore must be from money to income, not the other way. (P. 14)

Temin cites as the source of this passage our article on "Money and Business Cycles." Let me therefore quote from it:

> The key question at issue is not whether the direction of influence is wholly from money to business or wholly from business to money; it is whether the influence running from money to business is significant, in the sense that it can account for a substantial fraction of the fluctuations in economic activity. If the answer is affirmative, then one can speak of a monetary theory of business cycles or—

more precisely—of the need to assign money an important role in a full theory of business cycles. The reflex influence of business on money, the existence of which is not in doubt in light of the factual evidence summarized above, would then become part of the partly self-generating mechanism whereby monetary disturbances are transmitted. . . . As noted above, Cagan shows that the public's decisions about the proportion in which it divides its money balances between currency and deposits is an important link in the feedback mechanism whereby changes in business affect the stock of money. (1963*b*, pp. 49–50)

Whatever our view was, it clearly cannot be described as one-way causation. Since we wrote, there have been important advances in the statistical analysis of the interdependence between two series. One test of the existence and direction of causality between two series in the sense of Granger (1969) is reported here.[5] According to Granger, "We say that Y_t is *causing* X_t if we are better able to predict X_t using all available [past] information than if the information apart from [past] Y_t had been used" (p. 428). The statistical test of this formulation, using the method of least squares, is to estimate the linear regression of X_t on lagged X's and lagged Y's as

$$(1) \qquad X_t = \sum_{j=1}^{m} \hat{\alpha}_j X_{t-j} + \sum_{j=1}^{n} \hat{\beta}_j Y_{t-j},$$

where the $\hat{\alpha}_j$'s and $\hat{\beta}_j$'s are least-squares estimates. On the null hypothesis that Y does *not* cause X, the parent parameters $j, j = 1, \ldots, n$, equal zero. The null hypothesis, with current income on the left-hand side (X_t) and money on the right-hand side (Y), is that income is not *caused* by money, in which case the Y variable (money) will have zero coefficients. Alternatively, with current money on the left-hand side (X_t), the null hypothesis is that money is not caused by income, in which case the Y variable (income) will have zero coefficients.

For money, the variable I used was monthly M_2. The choice of an income variable for the interwar years is limited. The monthly personal-income series first becomes available beginning 1929. A proxy for income—bank debits to deposit accounts at 140 centers excluding New York City—is available for the period beginning 1919.[6] The equations fitted, including a constant, a residual term, a trend term, and alternatives with and without seasonal dummies, were the following:

$$(2) \qquad \widehat{\text{MONSUP}}_t = \sum_{j=1}^{m} \hat{a}_j \ \text{MONSUP}_{t-j} + \sum_{j=1}^{n} \hat{b} \ \text{PERINC}_{t-j},$$

$$(3) \qquad \widehat{\text{MONSUP}}_t = \sum_{j=1}^{m} \hat{a}_j \ \text{MONSUP}_{t-j} + \sum_{j=1}^{n} \hat{c} \ \text{BKDED}_{t-j},$$

$$(4)\quad \hat{\text{PERINC}}_t = \sum_{j=1}^{m} \hat{a}_j \text{ PERINC}_{t-j} + \sum_{j=1}^{n} \hat{K} \text{ MONSUP}_{t-j},$$

$$(5)\quad \hat{\text{BKDED}}_t = \sum_{j=1}^{m} \hat{a}_j \text{ BKDED}_{t-j} + \sum_{j=1}^{n} \hat{m} \text{ MONSUP}_{t-j}.$$

Table 4.1, covering the shorter period beginning 1929, with personal income as the income variable, and table 4.2, covering the longer period beginning 1919, with bank debits to deposit accounts as the income variable, report the probability of obtaining a value of F greater than that actually obtained if the null hypothesis is valid [Prob $(F > f)$]. If this probability is low, it indicates that the null hypothesis is implausible and can be rejected. Over the shorter period, one cannot reject exo-

Table 4.1 **Granger Causality Test Results, 1929–39**

Reg. No.	X_t	Y_t	Prob $(F > f)$		F-Statistic	NOBS	Lags (min)
(1)	MONSUP	PERINCN	0.8851	~	$F(24,47)$	108	(24,24)
(2)	PERINC	MONSUP	0.3767		$F(24,47)$	108	(24,24)
(3)	MONSUP	PERINCN	0.8027	~	$F(18,65)$	114	(18,18)
(4)	PERINC	MONSUP	0.3971		$F(18,65)$	114	(18,18)
(5)	*MONSUP	PERINCS	0.6334	~	$F(24,58)$	108	(24,24)
(6)	*PERINCS	MONSUP	0.3488		$F(24,58)$	108	(24,24)
(7)	*MONSUP	PERINCS	0.7685	~	$F(18,76)$	114	(18,18)
(8)	*PERINCS	MONSUP	0.5034		$F(18,76)$	114	(18,18)
(9)	MONSUP2	PERINCN	0.9850	~	$F(24,47)$	108	(24,24)
(10)	PERINCN	MONSUP2	0.2501		$F(24,47)$	108	(24,24)
(11)	MONSUP2	PERINCN	0.7807	~	$F(18,65)$	114	(18,18)
(12)	PERINCN	MONSUP2	0.0771		$F(18,65)$	114	(18,18)
(13)	*MONSUP2	PERINCS	0.6130	~	$F(24,58)$	108	(24,24)
(14)	*PERINCS	MONSUP2	0.6508		$F(24,58)$	108	(24,24)
(15)	*MONSUP2	PERINCS	0.8030	~	$F(18,76)$	114	(18,18)
(16)	*PERINCS	MONSUP2	0.2760		$F(18,76)$	114	(18,18)

Sources: MONSUP = M_2, from Friedman and Schwartz 1970, table 1, col. 9. MONSUP2 = M_2, as above, adjusted for exclusion of deposits in unlicensed banks, March 1933–May 1935, by applying the ratio of licensed and unlicensed bank to licensed bank deposits (Friedman and Schwartz 1963a, table 15, cols. 4, 2). PERINC = Personal income, OBE from Moore 1961, 2: 139.

Note: All regressions include a constant and linear trend, and regressions (5)–(8) and (13)–(16) (shown with asterisk), seasonal dummies also. Regressions are of the form

$$X_t = \sum_{i=1}^{m} \alpha_i X_{t-i} + \sum_{i=1}^{n} \beta_i Y_{t-1} + \text{residual}.$$

Table reports marginal significance level of F-statistic pertinent for testing null hypothesis $\beta_1 = \beta_2 = \ldots = \beta_n = 0$, which is the null hypothesis "Y fails to Granger-cause X." Where f is the calculated value of the pertinent F-statistic, the marginal significance level is defined as prob $[F > f]$ under the null hypothesis.

Table 4.2 Granger Causality Test Results, 1919–39

Reg. No.	X_t	Y_t	$(F > f)$		F-Statistic	NOBS	Lags (min)
(1)	MONSUP	BKDED	0.2685	~	$F(36,131)$	216	(36,36)
(2)	BKDED	MONSUP	0.0007		$F(36,131)$	216	(36,36)
(3)	MONSUP	BKDED	0.4401	~	$F(24,167)$	228	(24,24)
(4)	BKDED	MONSUP	0.0008	~	$F(24,167)$	228	(24,24)
(5)	*MONSUP	BKDEDS	0.2410	~	$F(18,185)$	234	(18,18)
(6)	*BKDEDS	MONSUP	0.00003	~	$F(18,185)$	234	(18,18)
(7)	MONSUP2	BKDED	0.7318	~	$F(36,131)$	216	(36,36)
(8)	BKDED	MONSUP2	0.0031	~	$F(36,131)$	216	(36,36)
(9)	MONSUP2	BKDED	0.9634	~	$F(24,167)$	228	(24,24)
(10)	BKDED	MONSUP2	0.0011	~	$F(24,167)$	228	(24,24)
(11)	*MONSUP2	BKDEDS	0.7854	~	$F(18,185)$	234	(18,18)
(12)	*BKDEDS	MONSUP2	0.0002	~	$F(18,185)$	234	(18,18)

Source: BKDED = Bank debits to deposit accounts, except interbank accounts, 140 centers (excluding New York City), from U.S. Board of Governors of the Federal Reserve System 1943, pp. 236–37.

Note: See note to table 4.1. Here, regressions (5)–(6) and (11)–(12) (shown with asterisks) include seasonal dummies.

geneity in either direction, though the probability of exceeding the observed f is, with one exception [(13), (14)], uniformly lower for those equations testing the influence of money on income than for those testing the reverse relation.

For the longer period, the situation is very different: not one of six regressions yields any evidence that income had a significant influence on money—the lowest of the probabilities associated with the observed f is 0.24, which means that in at least one time in four, chance alone would yield as strong an influence of income on money as that observed. In sharp contrast, every one of the six regressions testing the influence of money on income yields a far stronger relation than could be expected by chance if money really had no influence on income. The least-favorable regression yields a probability of 0.003 for the computed f, which means that a relation this strong would occur by chance less than 3 times in 1,000.

So far as these results go, then, for the interwar years as a whole, they clearly support unidirectional causality running from money to income. The reverse hypothesis that Temin, Kindleberger, and Abramovitz appear to embrace receives no support at all. For the shorter period, the results are not inconsistent with the passage I quoted above from "Money and Business Cycles"—mutual interdependence of money and income, with money the senior partner; but neither do they give it much support. Perhaps the only conclusion they support is that eight

years[7] is too short a period to give very much evidence on direction of causation, given the large random element in the month-to-month movements of both money and personal income.[8]

4.2 Measuring Monetary Stringency

Temin has revived the Keynesian view that the importance of money can be measured by the behavior of interest rates. In Keynesian analyses, the measure of monetary stringency is a rise in interest rates. The interest rate is regarded as "the price of money." In quantity theory analyses, the price of money is $1/P$, the inverse of the price level. In the former case, stringency is reflected in credit markets. In the latter case, money is an asset, actual and desired holdings of which are adjusted through prices, so that stringency is reflected in a rise in the reciprocal of prices.

Figure 4.1 plots the reciprocal of the U.S. wholesale price index, monthly, 1919–39. It is of some interest that every monetary event of significance during these two decades is mirrored in the movements of the price of money. Moreover, the reaction of prices to each monetary event is either observed in the coincident month or within five months of the event. The monetary events in the three deep contraction phases of the interwar period—1920–21, 1929–33, and 1937–38— are marked by vertical lines on the figure and identified by number above it. Table

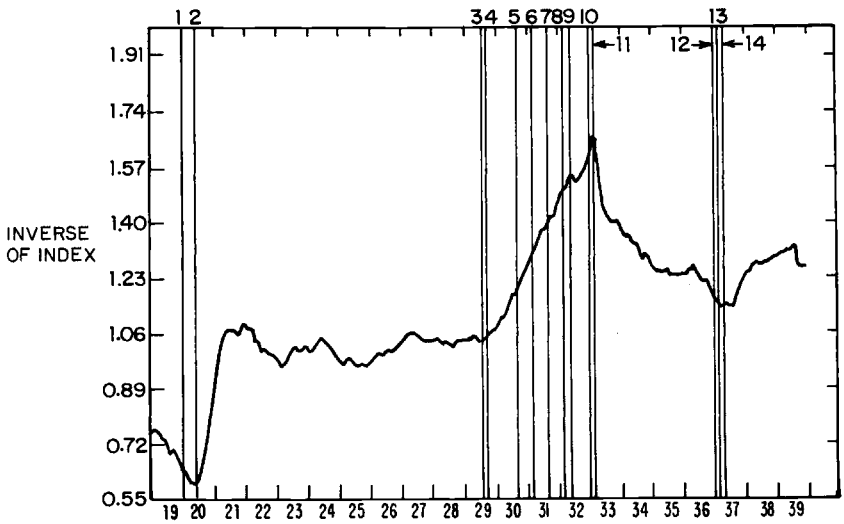

Fig. 4.1 Inverse of U.S. wholesale price index, 1919–29 (1926 = 1.0). To identify the numbered monetary events on the figure, see table 4.3. *Source:* U.S. Bureau of the Census 1949, p. 344.

4.3 shows the lag in months in the price response to the monetary events listed.[9]

Figure 4.1 refutes the allegation that Temin makes that there is no evidence of monetary stringency in 1930 and 1931 before Britain abandoned gold. I might also have entered dates of monetary events between 1921 and 1929 but refrained from doing so, because during this period the Reserve System attempted to anticipate business movements, so the monetary events and the reaction of the price of money could be regarded as the common result of movements in autonomous spending or whatever third other force produces the cycle. For the three deep contractions of the interwar years, however, the Federal Reserve took policy steps that cannot be regarded as necessary consequences of contemporary changes in business activity. The restrictive actions were followed, with brief lags, by sharp declines in the quantity of money and sharp rises in the inverse of the price level. The one important expansive action—the 1932 open-market purchases—was followed three months later by an end to large monthly declines in the quantity of money, and four months later by the largest decline in the inverse of the price level during the whole 1929–33 period.

Table 4.3 **Monetary Events Reflected in Inverse of U.S. Wholesale Price Index in Three Interwar Deep Cyclical Contractions**

Cyclical Contraction		Date of Monetary Event		Lag (in months) of Initial Response of $1/P$
1920–21	1.	1/20	Rise in discount rate	+5
	2(*a*).	6/20	Final rise in discount rate	
	(*b*).	6/20	Peak in M_2	+1
1929–33	3.	8/29	Rise in discount rate	+1
	4.	10/29	Stock market crash	0
	5.	10/30	Onset of first banking crisis	0
	6.	3/31	Onset of second banking crisis	+1
	7.	9/31	Britain leaves gold	0
	8.	3/32	Onset of open-market purchases	+4
	9.	6/32	Last of large monthly declines in M_2	+1
	10.	1/33	Final banking crisis	+1
	11.	3/33	Bank Holiday	0
1937–38	12.	1/37	Announcement of final rise in reserve requirements	+4
	13.	3/37	Peak in M_2	+2
	14.	5/37	Effective date of final rise in reserve requirements	0[a]

[a] +5 for date of onset of rapid rise in 1/P.

As it happens, we had never before made this particular use of the inverse of the price level, yet we had earlier dated the numbered monetary events, so this comparison is fresh and unbiased evidence. Frankly, we were surprised at how uniform the connection was.

4.3 Decline in Spending

I now turn to the detailed profile of the 1929–33 contraction provided by different investigators. Temin himself (1976) has reviewed critically earlier versions of what he terms the "spending hypothesis," the class of explanations that account for the severity of the Great Contraction by the collapse of one or another category of expenditure. Temin finds that the data do not support a fall in autonomous investment—the leading candidate in the Keynesian approach.

Temin reviews[10] the versions of the spending hypothesis associated with the names of Alvin Hansen, R. A. Gordon, Joseph Schumpeter, Thomas Wilson, and Keynes himself and econometric models of the interwar period constructed by Ben Bolch and John Pilgrim, Lawrence Klein, Jan Tinbergen, and John B. Kirkwood. He concludes that all of these versions are unacceptable because they rest on untested assumptions. He also reviews the long-swing or Kuznets cycle hypothesis. He asserts that it assumes that only a large shock—presumably, World War I—could have produced a large cycle in income. He then dismisses it because it cannot explain why the World War I shock did not generate a downturn until a decade later. He leaves open the possibility, but is half-hearted in offering it, that the stock market crash and the fall in construction might have been the channels through which a mild downturn was converted into a severe one.

In part, then, Temin provides a critique of both Kindleberger's and Abramovitz's approaches to 1929–33. His own version of the spending hypothesis, however, turns out to be equally vulnerable. According to him, two unexplained developments in 1930 changed the nature of the downturn. The minor one was a decline of approximately $1 billion in constant prices in American exports as a result of "the deepening world agricultural depression" and "European troubles independent of the United States. Events outside the United States therefore exerted a deflationary impact within this country" (p. 68).

The major unexplained development, according to Temin, was an autonomous fall in consumption in 1930 (see table 4.4). He specifically rejects an explanation of the fall in consumption as reflecting the effect on wealth of the stock market crash, on the ground that the wealth effect was too small. In this sense he regards the fall as autonomous and unexplained.

Temin's conclusion that there was such a fall in consumption is based on regressions of total and nondurable nominal consumption spending

Table 4.4 Changes in Macroeconomic Variables in Three Periods

	1920	1921	Change (in Percent or Percentage Points)	1929	1930	Change (in Percent or Percentage Points)	1937	1938	Change (in Percent or Percentage Points)
GNP, current prices (billion $)	91.5	69.6	−23.9	103.1	90.4	−12.3	90.4	84.7	− 6.3
constant (1958) prices	140.0	127.8	− 8.7	203.6	183.5	− 9.9	203.2	192.9	− 5.1
Personal consumption expenditures				139.6	130.4	− 6.6	143.1	140.2	− 2.0
Gross private domestic investment				36.9	28.0	−24.1	24.5	19.4	− 20.8
Net exports of goods and services				1.5	1.4	− 6.7	− .7	1.9	+371.4
Government purchases of goods and services				22.0	24.3	+ 9.5	30.8	33.9	+ 10.1
Implicit price index (1958 = 100)	65.4	54.5	−16.7	50.6	49.3	− 2.6	44.5	43.9	− 1.3
Unemployment rate	5.2	11.7	+ 6.5	3.2	8.7	+ 5.5	14.3	19.0	+ 4.7
							(9.2)	(12.5)	(+ 3.3)
	1919	1921		1928	1930		1936	1938	
GNP, current prices (billion $)	84.0	69.6	−17.1	97.0	90.4	− 6.8	82.5	84.7	+ 2.7
constant (1958) prices	146.4	127.8	−12.7	190.9	183.5	− 3.9	193.0	192.9	− 0.1
Implicit price index (1958 = 100)	57.4	54.5	− 5.1	50.8	49.3	− 3.0	42.7	43.9	+ 2.8
Unemployment rate	1.4	11.7	+10.3	4.2	8.7	+ 4.5	16.9	19.0	+ 2.1
							(10.1)	(12.5)	(+ 2.4)

Sources: U.S. Bureau of the Census 1975, series F-1, F-3, F-5, F-48, F-53, F-63, F-66, D-86. Figures in parentheses are Darby's (1976) estimates of unemployment rate.

on nominal current disposable income and nominal wealth for the period 1919–41. Actual consumption expenditures were above the level predicted by these regressions for 1921 and 1938 but below the predicted level for 1930. Hence Temin concludes that 1930 was unusual since consumption expenditures declined by more than would have been predicted from the associated decline in income and from the behavior of consumption expenditures in the other interwar major contraction years.[11]

Temin also compares yearly first differences in residuals from his regressions for 1921, 1930, and 1938 on the ground that a movement from a high positive to a low positive residual indicates a deflationary effect on the autonomous component of consumption. The year-to-year changes in residuals based on the regression were \$6.1 billion in 1921, −\$1.43 billion in 1930, and \$1.81 billion in 1938. He then averages the changes in residuals for 1921 and 1938, subtracts the change in residuals in 1930, and concludes:

> On the assumption that the [average] overprediction of decline in consumption shown for 1921 and 1938 is the norm for this function in depression years, the predicted decline in consumption was \$5 billion too low in 1930. (P. 72)

This is his current price estimate of the autonomous fall in consumption between 1929 and 1930, two-thirds of the total fall. In constant prices, the estimate is \$3 billion of the actual fall in real consumption expenditures of \$4.3 billion. Thus, according to Temin, before the onset of the October 1930 runs on banks, the economy was set on a course of deep contraction by the combined autonomous fall of \$1 billion in exports and \$3 billion in consumption expenditures.

Questions immediately arise with regard to these estimates. With respect to exports, the obvious question is whether the decline in U.S. exports was attributable to events abroad independent of the U.S. cyclical contraction. I shall deal with this question when I turn to explanations stressing international factors.

Temin's estimate of the autonomous decline in consumption expenditures raises questions of an entirely different order. In the first place, the consumption functions he fits are so crude by standards of the present state of the art that it is hard to take them seriously. Second, his assertion that the positive residuals he observes for 1921 and 1938 from this questionable regression are the norm for interwar contractions is strictly an obiter dictum. He gives no basis for regarding 1921 and 1938 as in some unspecified sense "normal" contraction years. But unless this is granted, he has no basis for regarding his small negative residual for 1930 as abnormal.

Thomas Mayer (1978*a*) in a recent paper has reestimated Temin's equations for the interwar years and has found larger negative residuals

in other years of the period. Why did the negative residual in 1930 cause a deep contraction but the larger negative residual in 1925, for example, not cause one? This is ad hoc economics without qualification.

Gandolfi and Lothian (1977), in a review of Temin's book, fit a more sophisticated consumption function for the longer period 1899–1941. They regressed the log of real per capita total consumption on the log of real per capita permanent income and the log of transitory income, defined as the difference between the logs of measured and permanent income. The inclusion of the transitory income variable is designed to reduce the effect of purchase of durable goods, as opposed to their flow of services, as a component of total consumption expenditures. Since purchases of durable goods are more cyclical than their service flows, they are more dependent on transitory than on permanent income. For the sake of comparison with Temin's results, Gandolfi and Lothian also examined year-to-year changes in residuals as well as their levels. They found 1930 far from unique. Of the five severe contractions other than 1930 (1894, 1896, 1908, 1921, and 1938), only 1921 had a very large positive residual. Overprediction of the fall in consumption expenditures, then, is hardly a normal feature of deep contractions. The negative value for 1930 is not abnormal by comparison with all years, not simply deep contraction years. Of 41 changes in residuals, 14 are positive, 27 negative, of which 15 are larger than 1930. Of all 41 changes, 25 are larger in absolute value, 15 smaller. Why is it that these 15 did not produce a violent reaction in economic activity, while the 1930 shift did?

Temin combines his hypothesis that there was an autonomous decline in (nominal and real) spending relative to output with the hypothesis that the demand for (nominal) money was falling more rapidly than the quantity of money during 1930 and the first three quarters of 1931. The decline in the quantity of money itself he regards as a movement along a stable supply-of-money function in response to a downward shift in the demand function. For logical rigor, this statement needs to be supplemented by a more precise specification of the arguments of the demand and supply functions—changes in which would equate quantity demanded with quantity supplied.

For the demand for nominal money, Temin would presumably include as arguments the price level (or, the inverse of the price level), real income, and interest rates. Assume, now, with Temin that for fixed values of the arguments, the nominal quantity of money demanded fell relative to the quantity supplied. To eliminate the putative excess supply of money (that is, make money holders willing to hold it), prices would have to rise—but they fell, which exacerbated the excess supply; real income likewise would have to rise, but it fell, again exacerbating the excess supply. The one remaining possibility is that interest rates would have had to fall, which they did. As a formal matter of monetary

theory, then, Temin treats the whole process as an autonomous *decline* in liquidity preference reinforced by declines in prices and in real income produced by other (spending) equations in the system, and wholly countered by lower interest rates—in other words, a large-scale shift of flow demand from goods to securities, and of stock demand from money to securities. But then, why no stock market boom? As Allan Meltzer (1976) points out, perhaps the temporary rally in the market in the spring of 1930 is consistent with Temin's construction; so also, of course, is the decline in interest rates in 1930, but hardly anything else is. Indeed, put this way, the notion of a sharp *decline* in liquidity preference is hard to square with a severe contraction accompanied by increasing industrial bankruptcies and unprecedented failures of banks.

With respect to the supply function, under the gold standard in effect in 1930 and 1931, both declining prices and declining real income would be expected to produce a gold inflow (which they did) and thereby increase the nominal quantity of money. Here too, therefore, Temin must treat the decline in interest rates as a sufficiently powerful force reducing the nominal quantity of money to have overcome the opposite influences of the other two variables. And apparently he does, since he argues that in the absence of bank failures the same decline in the quantity of money would have occurred through a decline in deposit-reserve ratios.

Temin presents no independent evidence of such great sensitivity of either demand for or supply of money to interest rates as would be required for his explanation. The general conclusion of most studies is that quantity of money supplied is largely insensitive to the interest rate (see Cagan 1965; Fand 1967; Rasche 1972) and that the quantity of money demanded is only moderately sensitive to the interest rate, displaying an elasticity a good deal less than unity with respect to long-term rates.

The various versions of the spending hypothesis—one of which attributes the Great Contraction to an autonomous decline in investment, another to an autonomous decline in housing, a third to an autonomous decline in consumption and exports—cast doubt on the value for cyclical analysis of the Keynesian distinction among investment, consumption, and net exports. The authors of the various versions write as if the cycle is necessarily propagated by the component of GNP that first reaches a peak or that is most volatile. Is this more than the most vulgar post hoc–propter hoc reasoning? If not, where is the evidence? The particular category of expenditure that first reaches a peak may not be the trigger of the downturn but the first to respond to a common influence on all expenditures, and similarly the variability of housing or investment or of exports is not an indicator of causal dominance,

as Temin, for example, assumes in analyzing the German national product in constant prices, 1924–29 (p. 156).

One other general comment needs to be made about Keynesian models. It is difficult to understand how the experience of 1929–33 could have spawned the notion of the need for the replacement of the classical assumption of a price-adjustment by a quantity-adjustment system of movement toward equilibrium. One key aspect of the contraction was that the decline in nominal income was divided almost equally between a fall in quantities and in prices. Real income fell by more then one-third, implicit prices by more than one-quarter. Why quantities changed as they did in response to price changes should be the goal of analysis. A model in real terms to explain a contraction in which price declines were so prominent is bound to serve imperfectly the cause of historical understanding.[12]

Finally, no decline-in-spending model has ever been able to explain the detailed development of the contraction.

4.4 International Explanations

The main problem with Kindleberger's account of the world in depression is his assumption that, because the contraction was worldwide in scope once it got under way, it therefore did not originate in the United States. The U.S. share in world trade, world capital and financial markets, and the world's stock of gold has been sufficiently large since World War I to give the United States the capacity to initiate worldwide movements and not merely to react to them. Of course, once having initiated a worldwide disturbance, it would in turn be subject to reflex influences.

From 1923 on, the Federal Reserve sterilized much of the gold inflow into the United States, preventing the kind of expansionary effect on the stock of money and thence on prices that would have occurred under the prewar gold standard. Instead, the system sought, and to a large measure achieved, stable economic growth with falling wholesale prices. This achievement was largely at the expense of economic stability in Great Britain and the peripheral countries tied to sterling. Britain's return to gold in 1925 at a parity that overvalued sterling would have caused less difficulty for Britain if prices in the United States had risen instead of falling thereafter.

Similarly, any problems of agricultural depression in the peripheral countries before 1929 were not independent of U.S. policy. For the contraction itself, the record is equally clear. The stock market boom, which is said to have drained funds from the rest of the world, and the stock market crash occurred in the United States. The downward movement in the U.S. money stock, including the sequence of bank failures,

was not the consequence of influences from abroad. The gold inflow into the United States (during the first two years of the contraction) to which reference will be made below is further evidence that other countries were being forced to adapt to the U.S. monetary policies rather than the reverse. The decline in U.S. lending abroad and the protectionist Smoot-Hawley Tariff Act were clearly U.S. actions that destabilized the world financial system.[13]

The United States was no pitiful, helpless giant on whom the rest of the world inflicted the Great Contraction. It is true that when the pound and other currencies were cut loose from gold in 1931, the U.S. trade balance was adversely affected and speculative pressure on the dollar developed. These devaluations, however, were themselves the reflex consequences of the U.S. contraction, and even so, their subsequent effects were not crippling, given the size of the U.S. economy.

This point is also relevant to both Temin's assumption that the decline in U.S. exports was independent of U.S. actions and to Abramovitz's analysis. Temin never alludes to the monetary standard of fixed exchange rates that enforced a worldwide decline in income and prices after 1929.[14] The central role of the United States in the worldwide scope of the contraction is attested to by the balance of payments.[15] If the decline in income in the rest of the world was being transmitted to the United States, we should have observed a balance-of-payments deficit in the United States, leading to a gold outflow. However, the U.S. gold stock rose by nearly $200 million from the annual average of 1929 to that of 1930. From August 1929 to August 1931, the gold stock rose by over $600 million. The gold inflow strongly suggests that any decline in U.S. exports to the rest of the world was attributable to the effects on the rest of the world of contraction here.

Likewise, Abramovitz, by assuming that there were forces making for a major decline in the dollar value of U.S. exports independently of U.S. monetary actions, is able to conclude that there was a significant constraint on the power of the Federal Reserve to sustain the growth of the money stock. In fact, however, there were no such forces. The gold inflows contradict the assumption that the initiating force was a serious decline in the dollar value of our export market independent of what was happening in the United States. The other exogenous factor for Abramovitz's analysis—net capital imports—is also an item that was crucially determined by events within the United States. In the 1930s the decline in U.S. capital exports may have been exogenous in the sense that the state of the capital market abroad and the prospective yields on investment in various foreign countries discouraged capital exports. But equally the volume of saving available for capital export relative to the volume of investment demand at home were important endogenous elements. Internal developments in the United States enormously affected U.S. capital exports.

Is the monetary approach to the balance of payments helpful in understanding 1929–33? The theory asserts that the active element in the balance-of-payments adjustment process is the equalization of the quantity of money demanded with the quantity of money supplied. Flows of specie are interpreted as responses to changes in demand for or domestic supply of money in various countries. A reduction in the public's demand for goods and securities leads to reduced imports and expanded exports on the goods side and to higher interest rates and capital imports on the securities side. The current account or the capital account or both move into surplus. Although the law of one price has frequently been associated with the monetary approach, some adherents allow for significant slippage between the rate of change of domestic prices and of world prices. Similarly, some adherents also accommodate interest rate differentials between domestic and foreign assets in their versions of the monetary approach.

Are the gold flows and price movements, 1929–33, consistent with the monetary approach to the balance of payments? As already noted, from August 1929 to August 1931, there was a gold inflow of $600 million. An increase in the demand for money in the United States relative to other countries, or a decrease in the supply of money in the United States relative to other countries, or any combination would be required by the monetary approach to account for the inflow. Such a change in the relative demand or supply of money would be manifested in a decline in U.S. wholesale prices relative to those in the rest of the world—either along with the inflow or as an intermediate step in producing the inflow. If changes in wholesale prices shown in table 4.5 for various countries are reliable, the 1929–31 decline in U.S. prices was steeper than in France and Germany, but not in the other countries. From the time Britain cut the pound loose from gold in September 1931 until July 1932, the United States had a gold outflow of $1 billion, absorbed principally by France, Belgium, Switzerland, and the Netherlands. The outflow would be interpreted as a relative increase in the demand for or decrease in the supply of money in those countries. The data on wholesale prices in the United States and France confirm an only slightly steeper rate of price decline in France than in the United States during this interval. The return flow of gold to the United States until the climactic weeks before the Bank Holiday in March 1933 restored the U.S. gold stock so that it was only $80 million lower than at the cyclical peak in business in 1929. Again, a relative increase in the demand for money in the United States and a steeper rate of price decline in the United States than abroad would be consistent with the inflow. This seems to be the case from September 1932 to February 1933.

It does not seem to me that the discussion of the international setting of 1929–33 as set forth in *A Monetary History* requires modification.

Table 4.5 **Percentage Changes in Wholesale Prices at Annual Rates for Various Countries, 1929–33**

From	To	Annual Rates of Change						
		U.S.	France	Japan	Canada	U.K.	Germany	Italy
Aug. 1929	Mar. 1933	−13.1	−11.9	− 5.8	−11.8	− 9.2	−11.6	−12.3
Aug. 1929	Sept. 1930	−12.2	− 6.7	−22.3	−16.0	−14.9	−10.8	−14.1
Sept. 1930	Sept. 1931	−17.0	−16.2	−13.6	−16.9	−15.2	−12.3	−16.2
Sept. 1931	June 1932	−13.0	−14.3	− 3.0	− 6.5	− 1.5	−16.2	− 9.5
June 1932	Sept. 1932	+ 6.5	−11.5	+53.7	− 3.1	+16.0	− 4.6	+ 4.0
Sept. 1932	Feb. 1933	−17.6	− 5.3	+16.8	− 8.5	− 7.6	−10.0	−11.5

Sources: U.S.: U.S. Bureau of the Census, 1949, p. 344. France: Librarie de Recueil Sirey 1937, table 11. Japan, Canada, U.K., Germany, Italy: League of Nations 1929–33, table 10. The U.K. index was constructed by the Board of Trade; the Italian index is labeled "Bachi."

Countries within the British orbit along with Britain were depressed during the '20s while the rest of the world prospered, partly thanks to U.S. capital exports. When the U.S. capital flow declined in 1928 and virtually ceased in the succeeding years, the economic position of the formerly recipient countries deteriorated. The gold exchange standard made the international financial system vulnerable. Given the attachment to fixed exchange rates, there was no way for other countries to insulate themselves from the effects of U.S. contraction. Deflation in the United States forced an adjustment on the rest of the world reflected partly in the gold inflows to the United States, partly in internal deflation necessary to avoid or reduce further gold flows. We exported deflation and depression to the rest of the world. Even though deflation abroad then reacted unfavorably on the United States, much leeway still remained for U.S. policy.

4.5 Understanding 1929–33

For Temin, 1929–33 was characterized by the absence of two equilibrating factors: a decline in real wages and a strong real-balance effect. Real wages in manufacturing, as the quotient of nominal wages divided by wholesale prices, were higher on both hourly and weekly bases in 1930, 1931, and 1932, and also on an hourly basis in 1933. Real wages in manufacturing, as the quotient of nominal wages divided by consumer prices, were higher on an hourly basis in every year except 1932 but lower in every year on a weekly basis. From the hourly wholesale price deflated series, Temin concludes that the marginal physical productivity of labor rose as employment fell, which is consistent with the classical theory of factor substitution: lowering the wage rate further might have avoided unemployment. The weekly consumer price deflated series shows, however, that this was a vain hope, since lower wages decreased the level of demand. He adds that if the real hourly wage series deflated by consumer prices is more accurate than the wholesale price deflated series, then no part of the classical theory is accurate.

I do not believe, however, that we can gain an understanding of 1929–33 by assigning a central role to real wages. Further, by dismissing the evidence of the hourly wholesale price deflated series, Temin fails to see a link between it and the aborted recoveries that Mitchell and Burns (1936) noted in 1930, 1931, and 1932.

The second equilibrating factor that Temin alleges was absent in 1929–33 was a strong real-balance effect. He defines that effect, however, as relating to the stock of money or the stock of money plus other financial assets. Yet the proper measure of the real-balance effect is the effect of the change in the net indebtedness of the government

sector—that is, the sum of noninterest- and interest-bearing government liabilities. The nominal value of currency plus government debt increased 27 percent from 1929 to 1933; the real value increased 62 percent. This may be described as a strong increase in real balances, whatever the strength of the effect on spending for consumption.

In Temin's account, an unexplained change in spending set the economy on its downward slide. No monetary change could stop the downward slide. An increase in the supply of money would not help, since the public had an excess supply of money. Things could get worse, as they did, when in September 1931 the Federal Reserve for the first time since 1929 in his view exerted a deflationary effect on the economy. Temin's analytical structure is a throwback to the Keynesian position of the quarter century after 1933. It has no theoretical explanation of the price level. It makes no distinction between nominal and real magnitudes. It presumes that no evidence exists on the relation of monetary change to income change. It ignores recent theoretical developments.

The period 1929–33 began as a cyclical contraction much like others, this time in response to the immoderate concern of the Federal Reserve Board about speculation in the stock market. Application of the theory of stock values as affected by expectations of the growth of earnings now suggests, as Irving Fisher believed, that marked overvaluation of stocks was not general (Sirkin 1975). Had high employment and economic growth continued, prices in the stock market could have been maintained. In the event, restriction of the growth of money from 1928 on produced a peak in business and some months later the stock market crash. A temporary increase in the money stock in October 1929 eased the effect of the shock of the crash. This may account for the increase in output recorded in early 1930 as a lagged effect of monetary growth.

The economy was thus subjected to two sharp shocks: the initial restrictive money growth and then the collapse of stock prices. Still, what followed suggests an adjustment that moved the economy toward equilibrium, but not for long. It is not hard to explain why an unanticipated decline in aggregate demand will lead employers to hire fewer workers at each real wage rate as perceived by them and will lead workers to refuse offers of work at lower nominal wages on the basis of unchanged anticipations. Along rational-expectations lines, however, employers and workers will in time revise their anticipations in accordance with the change in opportunities. If the Federal Reserve had maintained the initial moderate rate of decline in the money stock, presumably the economy after a time would have adjusted to this condition. But this is not what happened. The screw was tightened again and again, until 1932, and unanticipated change in each case required a new period of adjustment. To add to the problem, leading government officials and industrialists exhorted employers to maintain wage rates

and share employment, which must have contributed to shorter average work weeks and higher layoffs.

Still, one must acknowledge the resilience of the economy after the first shocks in 1929 and the first banking crisis at the end of 1930. In early 1931, some industries with relatively smaller price declines revived. Again, the adjustment was aborted by a second round of banking failures, subsequently compounded by the Federal Reserve's reaction to gold losses, in the autumn of 1931. The favorable shock in April 1932, when the Federal Reserve System finally began an open-market purchase program in response to congressional pressure, produced a positive reaction in the economy. Prices began to move upward and output increased. The adjustment was short-lived. The purchase program ended in early August, and the political campaign spawned rumors about the condition of banks the Reconstruction Finance Corporation (RFC) had aided. The consequence was a series of runs that ended with the shutdown of all banks as the new administration took office. The economy was at its lowest ebb. Yet vigorous growth was not precluded during the expansion phase that followed.

A far more satisfactory explanation of 1929–33 than Temin's is, therefore, that a series of negative shocks, monetary in origin, reduced real output and the demand for labor and shifted the demand for securities to short-term instruments and high-grade, long-term securities. Destroy a banking system, and the real economy will grind to a halt. There are no unexplained changes in spending that serve as deus ex machina. The presence of equilibrating forces is attested to by the interludes during the course of the contraction when real output increased. The behavior of the economy was determined by public policies. Different policies would have resulted in different behavior.

Appendix
Dissents from the Views in a Monetary History

Temin rejects the account in *A Monetary History* of the way an initial mild decline in the money stock from 1929 to 1930, accompanying a decline in Federal Reserve credit outstanding, was converted into a sharp decline by a wave of bank failures beginning in late 1930. I shall discuss in turn five items in Temin's catalog of dissent: (1) the money-stock identity, (2) the behavior of high-powered money, (3) the behavior of interest rates, (4) the price of deflation and the behavior of real money balances, (5) the role of bank failures; and I will comment finally

(6) on his and others' approaches to monetary policy during the contraction.

4.A.1 The Money-Stock Identity

In *A Monetary History,* we used an identity that relates money broadly defined to three proximate determinants: high-powered money, the deposit-reserve ratio, and the deposit-currency ratio. The three proximate determinants reflect, respectively, the behavior of the monetary authorities (in the United States, the Treasury and the Federal Reserve System), the commercial banks, and the public. The monetary authorities provide high-powered money—the sum of reserves and currency—that the banks and public divide between themselves in light of the factors influencing the two sets of ratios. The deposit-reserve ratio is affected by legal reserve requirements, banks' expectations of currency movements into and out of their vaults, and interest rates. The deposit-currency ratio is affected by interest rates, income, and the public's preference for holding coin and currency. The ratios clearly reflect demand factors that interact with the supply of high-powered money. The argument of *A Monetary History,* as already noted, is that the Federal Reserve System through its control of the issue of high-powered money can offset any undesired change by the other actors in some short run, and hence the system plays a dominant role in the control of the quantity of money.

Temin believes he has isolated a fatal error in *A Monetary History,* because the identity suggests to him that the stock of money is determined by supply factors alone, instead of being joined with a demand equation to determine equilibrium supply in the market for money. Temin writes:

> Consider the stock of bonds. The size of the stock is the product of past decisions about corporate and government finance. It is fixed at any moment of time by these previous supply decisions. If the demand for bonds shifts, it will not change the number of bonds in existence immediately; it will change their price. In the short run, therefore, the quantity of bonds is determined by the supply, and the price is determined by the demand. In the longer run, the price will be a function of both the supply and demand working through a recursive relationship. Friedman and Schwartz employed the short-run part of this argument; they appear to have rejected the long-run part. (P. 18)

According to Temin, we treat changes in the demand for money as affecting only the price of money, meaning the interest rate, and not quantity, the equilibrium stock of money. This is standard Keynesian

doctrine, in which the price of money is defined as the interest rate rather than the reciprocal of the price level.

The problem with Temin's analysis, as with much Keynesian analysis, is the assumption that the price level is predetermined and the resulting failure to treat the price level as a variable that helps to equate nominal demand for money with the nominal supply of money or, alternatively, to enable any level of real balances demanded to be attained for any level of nominal balances. Temin's failure to recognize the importance of the distinction between nominal and real magnitudes leads him to stress instead the distinction between long run and short run, but this distinction is not highly relevant to the determination of the stock of money. Undoubtedly, different forces exert different influences on the behavioral patterns underlying the proximate determinants in the short and long run. But in both runs, it is the behavioral patterns underlying the proximate determinants that determine the size of the nominal quantity of money outstanding. The demand forces emanating from the public that affect the nominal quantity of money are those that have to do with the forms among which they choose to distribute their nominal (or real) assets—the fraction they choose to hold in real assets, securities of various kinds, bank deposits of various kinds, and high-powered money. These demand forces interact with the supply conditions of high-powered money, and of various forms of deposits or securities, to determine the nominal quantity of money. The demand for real money balances interacts with the nominal quantity of money to determine the price level. Of course, this is an oversimplified statement. A more sophisticated statement would assert that all of these variables are determined simultaneously and that some of the variables that enter into the demand for real money balances may also enter into the functions that determine the distribution of the total balance sheet among various forms of assets. But the important point is fully brought out by the simplified picture: to leave price expectations out of the picture in the short run from 1929 on is to leave out a major part of the picture—both for monetary analysis and for income analysis. As the public adjusts discrepancies between its actual real money balances and desired real money balances, nominal income is altered and the breakdown into prices and output is determined.

Temin alleges that the supply of money in our specification is "determined by forces independent of income and interest rates" (p. 19). Yet we specifically note that the deposit ratios are functions of the interest rate, among other variables (contrary to Temin's discussion, which suggests that we do not include it) and that the deposit-currency ratio is a function also of income. He is right that we regard banking panics as "far and away the most important single determinant" (p. 20) of the ratios, not only in the early 1930s, but also during other panic

episodes. How do we know this? By studying the pattern of behavior in these ratios during panic episodes. The early 1930s do not stand alone. We have evidence on the behavior of the ratios in all the post–Civil War panics in the United States. They tell a uniform story of a shift from deposits to currency by the public once the economy is engulfed in panic and of a belated attempt by banks to increase reserves relative to their deposits once the panic subsided.

We have evidence also from Canadian experience in 1929–33. The percentage fall in Canadian nominal income over these years was about the same as in U.S. nominal income, yet the percentage fall in the Canadian stock of money was considerably smaller. The reason is that Canada was spared the ordeal of bank failures. There was no shift from deposits to currency in Canada comparable to that in the United States, and so there was no effect from this proximate determinant in producing a decline in the stock of money. There was no decline in the "quality" of deposits comparable to that in the United States because of a loss of confidence in banks, and hence there was less of a decline in the demand for real money balances in Canada. That is why a smaller decline in the quantity of money was consistent with almost the same decline in income and prices. The sharp decline in Canadian income and prices occurred because Canada kept its exchange rate with the United States fixed until September 1931 and then maintained its exchange rate at a new level involving a smaller depreciation than that undergone by the pound sterling. For Canada, it is entirely appropriate to regard the quantity of money as adapting in large measure to movement in income and prices, rather than as an exogenous force. It was the tail. The United States was the dog.

4.A.2 The Behavior of High-Powered Money

The decline in the quantity of money from August 1929 to October 1930, before the first banking panic, did not result from any weakness of the private economy. The decline was entirely the result of a decline in Federal Reserve credit outstanding. There were no problems with the banking structure, no attempted liquidation of loans by banks, no attempt by depositors to shift from deposits to currency that contributed to reducing the quantity of money. In fact, the banks were reducing reserves relative to deposits, and the public was increasing its deposits relative to currency—enough to offset half the decline in Federal Reserve credit.

Temin counters that a decrease in bank discounts at the Fed, in response to the decline in market interest rates, and not any failure of the Fed, was responsible for the decline in Federal Reserve credit outstanding. Bank borrowings declined from a peak of $1,096 million

in July 1929 to $189 million by September 1930. Total bank reserves fell about $40 million. Temin does not allude to the punitive attitude of the system toward member bank borrowing, hence bypasses the reason there was little incentive for them to increase rediscounting, absent any panic, when the Reserve Banks lowered discount rates—"dramatically," according to him (p. 21)—and he takes the absolute amount of discounting to be "low."[16]

In fact, the discount rate was not reduced uniformly at all Reserve Banks. By mid-1930, New York had reduced its rate in six steps from 6 to 2.5 percent, while at other Reserve Banks the rate had gone from 5 to 4 and 3.5 percent. By the end of 1930, the New York rate stood at 2 percent, the rates at two other banks at 3 percent, and at the remaining nine at 3.5 percent. The discount rate fell less than the commercial paper rate even in New York; a lot less, in other districts. The spread between the commercial paper rate and the discount rate at New York was a shade lower in 1930 than in 1929; at other Reserve Banks, much lower. Of course, under the lash of runs by the public, the banks did increase their borrowings—from $189 million in September 1930 to $338 million in December. But this increase in Reserve Bank credit outstanding was smaller than the increase in the public's currency holdings.

Temin's general Keynesian tendency to treat interest rates as the crucial monetary variable leads him astray in evaluating both the role of the Fed and our views about its responsibility. For example, he writes that the Federal Reserve "could have offset changes in interest rates by changing the discount rate, and it could have avoided the banking panics by changing its procedures" (p. 20). That is not our view. We put major emphasis, not on discount rate changes or on "procedures," but on Federal Reserve control of high-powered money, or bank reserves, through open-market operations.

From our view, the crucial question is whether the Federal Reserve was powerless to engage in open-market purchases to restore the level of its credit outstanding, given that, until the first banking panic, the banks, for whatever reason, were not willing to come to the discount window. Temin's discussion of the system's behavior is ambiguous, to say the least:

> No one disputes that the Fed has the power to undertake open-market operations. And most people agree that these actions have effects on the economy. But very few of the monetary changes in the early 1930's were the results of conscious decisions to undertake open-market operations. Friedman and Schwartz argued that the decline in the stock of money in 1930 was the result of a fall in discounts at the Fed in response to a fall in market rates not fully duplicated by the discount rate, and that the fall in 1931 was due to a decline in

> the two deposit ratios produced by the banking crises. These events are not the same as open-market purchases. (P. 25)

These events clearly are the opposite of open-market purchases, which would have increased Reserve credit outstanding and high-powered money. They are precisely the kind of events that conventional central-bank wisdom would regard as requiring open-market purchases in order to offset their effects. Temin objects that we imply that "all changes in the stock of money were the results of actions by the Federal Reserve" (p. 25). They are the results of actions or inactions by the Federal Reserve. In 1930 before the panic condition developed at the end of the year, the Federal Reserve could have readily reversed the decline in Federal Reserve credit outstanding. Temin evades the central issue of why they did not do so.

Temin makes much of the fact that high-powered money on an annual average continued to increase except in 1930. Hence, he argues, there was no restraint on the supply of money. High-powered money grew after the first banking crisis, not because member bank reserves were expanding, but because the public's currency holdings began to climb in the usual shift of its preferences toward currency as an aftermath of the banking crisis. By August 1931, the public's currency holdings had increased by $583 million over its holdings in October 1930, but high-powered money was only $558 million higher. High-powered money growth, barely adequate to meet the public's growing distrust of bank deposits, had contractionary effects on the reserve position of the banking system—hardly impressive evidence of monetary ease.

4.A.3 The Behavior of Interest Rates

According to Temin, the money hypothesis fails its most important test because there is no evidence in interest rates of monetary stringency at the end of 1930 as the result of bank failures. Temin has not examined the data for earlier panics, but it is true that short-term rates in those episodes did rise during the weeks of panic, and we do not observe a comparable rise during the weeks of the first banking crisis in the last quarter of 1930 or of the second banking crisis from March to June 1931.[17] The failure of short-term rates to rise, however, is not necessarily inconsistent with the presence of monetary stringency—both because monetary stringency might be reflected in prices rather than in interest rates and because other factors were simultaneously impinging on short-term rates. In particular, the failure of short-term rates to rise may have reflected, first, declines in the 1920s in the supply of short-term instruments issued by both private borrowers and the government and, second, the special role of the commercial banks as

demanders of these short-term instruments. There are two markets to consider, the commercial paper market and the market for short-term government securities.

The commercial paper market today is a different market from the one that existed in the 1920s through the Great Contraction.[18] In today's market, the finance companies are the dominant borrowers. In the 1920s, corporate enterprises in textiles, foodstuffs, metals, and leather were the main borrowers. There was a dramatic decline in the market from 1924 to 1933, interrupted by a brief expansion from the stock market crash to April 1930. Outstandings fell from a peak of $925 million in October 1924 to a low of $265 million in September 1929, largely because firms that had formerly borrowed in the commercial paper market found it more advantageous to float stocks and bonds. The stock market crash and the reduction in commercial paper rates relative to bank lending rates led to a rise in outstandings in April 1930 to $553 million. Thereafter, the volume declined to $358 million in December 1930 and $275 million in August 1931.

Currently, nonfinancial corporations are the main holders of commercial paper. In the 1920s through the Great Contraction, the banks were virtually the sole buyers of commercial paper, with country banks the mainstay of the market. From the member bank call date of December 31, 1930, through the September 29, 1931, call date, member bank holdings of commercial paper ranged from 102 to 141 percent of the reported total amount outstanding, the excess over the reported amount outstanding rising steadily over the interval covered. The explanation for the excess is that the banks purchased paper from dealers other than those reporting to the Federal Reserve Bank of New York and is one indication of the strength of member bank demand for commercial paper.

The chief advantage of commercial paper to member banks apart from its yield was its eligibility for rediscount at the Reserve Banks. This advantage gained in importance during a panic, so that, from the demand side, a panic, rather than putting pressure on commercial paper rates, to some extent relieved the pressure. Instead of selling commercial paper, banks increased borrowings using commercial paper as collateral to meet depositors' demand for currency. As we have seen, bills discounted rose in the last quarter of 1930 and again in June 1931, the culmination of the second banking panic. With limited supply and persistent demand, the failure of commercial paper rates to rise during the panic in no way contradicts the money hypothesis.

With respect to the government securities market, the reduction of the public debt, dating from 1919, continued through December 1930. This constituted an increase in the supply of loanable funds, thereby reducing the interest rate that would clear the market at any given price

level. The increase in the public debt was small through August 1931, so the influence on the supply of loanable funds and hence on the upward pressure on interest rates from this source must likewise have been small (see table 4.A.1).

As is true for later years also, we lack adequate data on the maturity distribution of the debt, 1929–31. Treasury bills—first issued in December 1929—and certificates of indebtedness had a maturity of less than one year when issued; Treasury notes, of three to five years; and bonds, of more than five years. When purchased or held, however, the remaining maturity might be quite different from the original maturity. So the distribution of security holdings among the indicated categories is only a rough index of their distribution by maturity.

Of the reduced total of the public debt, through December 1930, less than 10 percent of member bank holdings were in less than one-year maturities when issued; four-fifths were in long-term bonds (see table 4.A.2). Their holdings amounted to less than one-quarter of the bills and certificates outside the Federal Reserve from the October 1929 call date through the June 1930 call date, rose to three-tenths at the September 1930 call date when the first banking panic had not yet erupted, and then to three-eighths by the call date in December after the Bank of United States had been suspended. In 1931, the composition of member bank holdings of government securities shifted toward the short-term when issued, probably reflecting a shift in the composition

Table 4.A.1 **U.S. Federal Government Interest-bearing Debt Outstanding, Various Months, 1929–31 (in Millions of Dollars)**

Situation as of Last Day of:	Bonds	Treasury Notes	Certificates of Indebtedness	Treasury Bills	Total
June 1929	12,124	2,254	1,640	—	16,018
Aug. 1929	12,126	2,781	1,620	—	16,527
Oct. 1929	12,115	2,649	1,658	—	16,422
Dec. 1929	12,110	2,513	1,306	100	16,029
Mar. 1930	12,112	2,570	1,385	56	16,123
June 1930	12,112	2,390	1,264	156	15,922
Sept. 1930	12,113	2,345	1,247	120	15,825
Oct. 1930	12,113	2,345	1,247	223	15,928
Nov. 1930	12,113	2,343	1,247	230	15,933
Dec. 1930	12,113	2,342	1,192	128	15,775
Mar. 1931	12,709	1,129	2,228	214	16,280
June 1931	13,531	621	1,924	445	16,520
Aug. 1931	13,536	644	1,883	523	16,586
Sept. 1931	14,336	644	1,545	524	17,049
Dec. 1931	14,298	795	1,860	576	17,529

Source: U.S. Treasury Department 1929–31.

Table 4.A.2 **Chief Kinds of U.S. Government Direct Obligations Held by Member Banks, Member Bank Call Dates, 1929–31**

	Member Bank Holdings (Millions of Dollars)				Percent of Total Member Bank Holdings in:			Holdings as Percent of Total Amounts Outside FR Banks			
	Total (1)	Bills and Certificates (2)	Notes (3)	Bonds (4)	Bills and Certificates (5)	Notes (6)	Bonds (7)	Total (8)	Bills and Certificates (9)	Notes (10)	Bonds (11)
1929											
June 29	4,155	446	704	3,005	10.8	16.9	72.3	26.3	28.8	32.6	24.8
Oct. 4	4,022	365	665	2,992	9.1	16.5	74.4	25.0	24.0	26.5	24.8
Dec. 31	3,863	249	520	3,094	6.4	13.5	80.1	24.9	21.0	22.6	25.7
1930											
Mar. 27	4,085	273	524	3,288	6.7	12.8	80.5	26.3	23.4	21.9	27.5
June 30	4,061	259	463	3,340	6.4	11.4	82.2	26.5	22.6	21.0	27.9
Sept. 24	4,095	334	418	3,343	8.2	10.2	81.6	26.9	30.7	19.4	27.9
Dec. 31	4,125	369	485	3,271	8.9	11.8	79.3	27.4	37.6	22.9	27.4
1931											
Mar. 25	5,002	899	332	3,771	18.0	6.6	75.4	31.9	42.2	30.1	30.3
June 30	5,343	901	403	4,039	16.9	7.5	75.6	33.7	44.5	67.8	30.5
Sept. 29	5,564	914	371	4,279	16.4	6.7	76.9	34.1	54.2	60.5	30.5
Dec. 31	5,319	679	441	4,199	12.8	8.3	78.9	31.8	33.7	57.9	30.1

Sources, by columns: (1): Sum of cols. (2)–(4). (2)–(4): U.S. Board of Governors of the Federal Reserve System 1943, p. 77. (8): Holdings of the Federal Reserve Banks were deducted from the total of the three kinds of debt outstanding (ibid., pp. 332, 343, 375, 509–10); col. (1) was expressed as a percentage of the difference. (9)–(11): Procedure similar to that for col. (8), except that no breakdown of Federal Reserve holdings was available except at Dec. 31; the percentage distribution of the three kinds of debt was assumed the same at other dates in each year as on the following Dec. 31.

of outstandings, but also reinforcing the growing concentration of bank demand on these issues, already manifested in December 1930. Bank holdings of short-term governments were more than 50 percent of outstandings in September 1931, and 60 percent of outstanding medium-term governments.

Mayer (1978*b*) asks whether one would not expect "the demand for other assets to decline, and hence their yields to rise" (p. 140) when bank failures reduced the money supply. But the short-term assets for which we have yield information, primarily commercial paper and short-term governments, are not those for which demand declined. The banks dominated these markets, and with good reason. After the experience of two banking panics, the remaining banks chose to acquire assets with assured convertibility into cash sums at need and at short notice.

Short-term governments did not experience the unremitting declines in rates that characterized the commercial paper market in 1930. Continuous monthly data are available only for the yields on three- to six-month Treasury notes and certificates (see figure 4.A.1). (There were only five issues of Treasury bills that year, so there are quotations only on new offerings.) As the chart in figure 4.A.1 shows, small increases in yields occurred during three months in 1930—5 basis points in April, 27 in September, and 8 in December, the month the Bank of United States failed.

Given these conditions in both the commercial paper and short-term government markets until Britain cut loose from gold on September 21, 1931, why did the commercial paper rate rise from 2 percent, during the week ending October 3, to 4.13, during the week ending November 14, after which it continued an uninterrupted decline to the week ending with the Bank Holiday—its low point during the contraction of 1.38 percent? Why did the average rate on new issues of Treasury bills, which reached a low of 0.485 percent in July 1931, rise steadily thereafter to 3.253 percent in December 1931? Outstandings of commercial paper continued to decline to the end of the year, but outstanding Treasury bills rose somewhat. It is clear from the pattern of short-term rates of private instruments that the rate rise followed the increase in the discount rate at New York on October 9, from 1.5 to 2.5 percent, and on October 16, to 3.5 percent. In this instance, the Federal Reserve led the market. In the discount rate reductions from November 1929 to May 1931, it followed the market. In the short-term government market, an added factor contributing to the rise in interest rates may have been the increase in Treasury notes.

Bills discounted had been rising from July 1931, when discounts averaged $169 million, to $282 million in September. They then rose to $614 million in October, peaking at $848 million in February 1932.

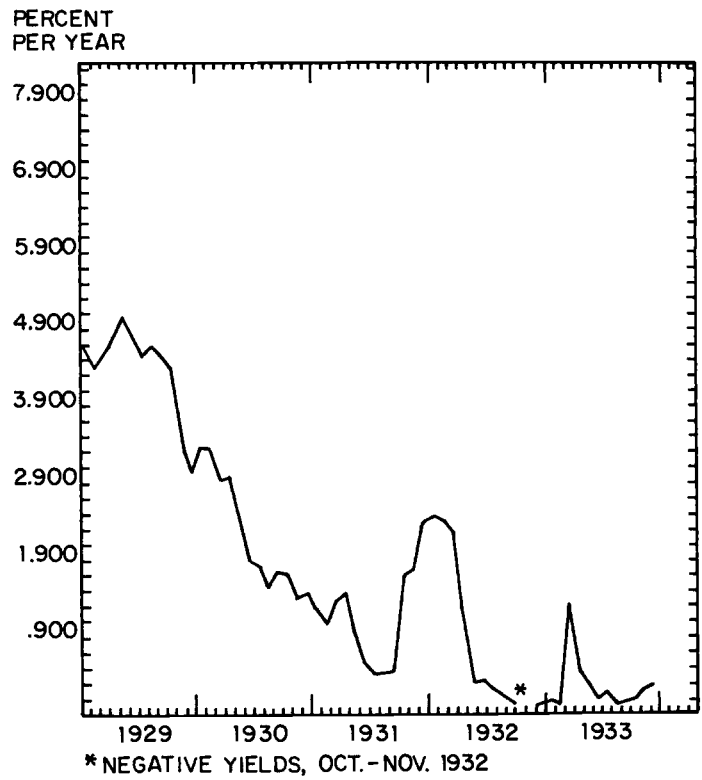

Fig. 4.A.1 Yields on three- to six-month treasury notes and certificates, 1919–33. *Source:* U.S. Board of Governors of the Federal Reserve System 1943, p. 460.

In addition to the increase in their indebtedness, member banks lost $74 million in reserves between July and September 1930 and a further $426 million between September 1931 and February 1932.

Interest rate behavior is not, then, inconsistent with monetary stringency both before and after September 1931. The pattern of short-term interest rate declines before September 1931 reflected firm demand by commercial banks for commercial paper and short-term government securities and a generally declining supply of these instruments. When the Federal Reserve increased discount rates sharply in October 1931, it led market rates, pulling them up, whereas its earlier discount rate reductions followed market rate declines.

Temin, however, is right in arguing that short-term interest rates are the ones to examine because they most nearly resemble holding-period yields. For long-term rates, only yields to maturity are available, al-

though investors make plans on the basis of holding-period yields. Temin therefore regards long-term rates as unsuitable for analysis because they are complicated for the years 1928–31 by the growing risk of default for some bonds and the rising price of risk.

In *A Monetary History,* we noted that while both long- and short-term interest rates had been declining before the first banking crisis, a widening spread began to emerge, synchronous with the first crisis, between yields to maturity on lower-grade corporate bonds and on government bonds as yields on corporate bonds rose sharply and yields on government bonds continued to fall. Temin says "this suggestion will not stand up" (p. 105) because bond prices began to fall well before the panic, and only the prices of lower-grade bonds fell; the prices of high-grade corporate and government bonds stayed roughly constant.

The point of Temin's insistence that the value of bank portfolios declined well before the bank panic of 1930 is that the price decline of bonds was not a result of the liquidity scramble but rather a cause. He argues that bonds were being moved from one quality class to another so that movements in the Baa rate do not show the change in the price of banks' portfolios. The price decline in any actual bond portfolio in the 1930s was the result of both the decline in the price of a given quality class and the decline in the quality ratings of the bonds in the portfolio. The yields on the fixed sample of bonds that Temin constructed for December and June dates 1928–31 rise continuously and far exceed the yields on Baa bonds.

No one disputes that bond prices were depressed in 1928 and 1929 while the boom in equities was in full swing. Temin's assertion that yields on high-grade corporate and on government bonds thereafter were constant is hard to assess. Yields on high-grade corporate bonds fell from December 1929 through October 1930, from 4.67 to 4.42. At the end of 1930, during the months of panic, they rose to 4.52. They then resumed a decline to 4.36 in July 1931. Yields to maturity on government bonds fell from 3.43 in January 1930 to 3.19 in November 1930, then rose to 3.30 in February 1931, falling thereafter until June, when the yield was 3.13. These small changes are consistent with monetary stringency before the first banking panic—given the steady decline in commodity prices, so that real rates rose appreciably; and the upward movements and subsequent declines at the end of 1930 are consistent with an intensification of monetary stringency during the panic.

In any event, the relative constancy of high-grade yields does not contradict the argument that the sale of low-grade bonds was induced by a scramble for liquidity. Temin counters that banks were net sellers of bonds in 1931 because "they perceived the risk more quickly or because they were more risk averse than individuals. The fact that they

sold while individuals bought is not evidence of a liquidity scramble" (p. 106 n.). This ignores the effect that dumping securities, for whatever motives, by some banks produced on the values of the investment portfolios of other banks. As for money and income, there is no reason to expect a one-way relation. The reflex influence of bond sales in setting off other bond sales is the essence of a liquidity crisis that Temin fails to recognize.

4.A.4 Price Deflation and the Behavior of Real Balances

Temin argues that the distinction between nominal interest rates and real interest rates can be neglected. To begin with, he doubts that anyone apart from professional economists makes such a distinction. Further, even if the distinction were made, it would not salvage the monetary explanation. If high real interest rates dominated all other explanations of 1929–33, he asks, why do we not observe a similar effect in 1920–21 with a greater deflation and the same institutional constraint that nominal interest rates cannot be negative? There were indeed high real rates in 1920–21, but their effect was not prolonged by banking panic effects on the money stock.

The final major indication for Temin that monetary causes cannot account for the severity of the economic decline is that real balances did not decline. Because prices fell so rapidly, the stock of real money balances did not fall from 1929 to 1931; hence, in his view there could not have been any deflationary effect from the decrease in the nominal stock of money. He asks:

> Why . . . 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)

For Temin, there is no contradiction between his assertion that the demand (i.e., demand function) for nominal balances declined while real balances (i.e., quantity of real balances held) were constant or increased. Real money balances are a statistical construct that he examines merely because quantity theorists consider it important. If he thought it represented the basic monetary total demanded, he would have had to explain why a decline in the demand for money did not produce a rise in prices, for a fixed nominal stock, to produce a decline in real money balances.

If one regards real money balances as the basic monetary total demanded, there is no evidence that the demand function declined. Gandolfi and Lothian (1976) have shown that the function that predicts actual real money balances for 1900–1929 predicts actual real money

balances during the Great Contraction with no loss in predictive power. The demand for real money balances is conventionally defined as related positively to real income and negatively to the rate of interest. Hence the movement of real balances over the cycle depends on the relative movements of the determinants. There is no evidence of a leftward shift in the demand curve during the Great Contraction. The rise in real money balances to 1931 and similarly the decline from 1931 to 1933 were due to changes in the determinants of the demand. There were movements along the demand function, not a shift in the function, as Temin would have it.

Gandolfi and Lothian have also challenged Temin's assumption that a fall in the nominal quantity of money accompanied by a corresponding fall in prices should leave real output unchanged, since real balances remain constant. In this case, Temin fails to note a distinction between anticipated and unanticipated price change. Suppose output depends on the price of output relative to expected price of inputs. An unanticipated fall in all prices, given imperfect information on input prices, will be perceived by producers as a relative fall in output prices. Temin ignores a growing literature on the supply effect of unanticipated price changes on real output change. Hence his assertion that the behavior of real balances is inconsistent with a monetary explanation of the contraction is untenable.

4.A.5 The Role of Bank Failures

As indicated earlier, Temin's explanation of the role of bank failures is that they served as the channel through which the supply of money adjusted to the falling demand. He alleges that the banking panic of October 1930 was induced by the decline in agricultural income and in the prices of relatively risky long-term securities presumably held by banks and, in particular, that the failure of the Bank of United States in the course of that panic did not precipitate a liquidity crisis. In *A Monetary History* we devoted a section to the question of the origin of bank failures during the contraction. Did the failures arise primarily because of imprudent financial practices of the 1920s? Or were they the product of developments of the early 1930s?

> Whatever may have been true of the initial bank failures in the first banking crisis, any ex ante deterioration in the quality of loans and investments in the later twenties or simply the acquisition of low-quality loans and investments in that period, even if no different in quality than in earlier periods, was a minor factor in the subsequent bank failures. As we have seen, the banking system as a whole was in a position to meet the demands of depositors for currency only by a multiple contraction of deposits, hence of assets. Under such

> circumstances, any runs on banks for whatever reason became to some extent self-justifying, whatever the quality of assets held by banks. Banks had to dump their assets on the market which inevitably forced a decline in the market value of those assets and hence of the remaining assets they held. The impairment in the market value of assets held by banks, particularly in their bond portfolios, was the most important source of impairment of capital leading to bank suspensions, rather than the default of specific loans or of specific bond issues. (P. 355)

So even if we were to concede that all the banks that failed in the first banking panic beginning October 1930 were bad banks that deserved to fail, this series of failures would have provoked difficulties for other good banks, the market value of whose assets would have been affected by the dumping of assets by the failing banks. Such failures could well have promoted panic among all depositors. In a panic the public is mired in doubts that institutions are as sound as they are said to be.

I believe that the concession to Temin about the first banking crisis is not supported by the evidence, however. Good banks went down in that panic. His allegation that the Bank of United States failed because of fraudulent practices of its officers will not be sustained by an impartial examination of the record of the bank. The charge of fraud tells you something about the temper of the times, not the facts of the case.

Moreover, the panic of October 1930 does not stand alone in the U.S. monetary history if we look back this time, not forward to the succeeding banking crises from 1931 to 1933. Cagan (1965) noted in his study, to which Temin does not refer, that panics in U.S. monetary history appeared in the early stages of cyclical contraction and therefore themselves could not have been the major cause of the contractions. He concluded that panics made ordinary business contractions severe when they led to a substantial decline in the rate of monetary growth and not otherwise. "Substantial decline in this rate, by itself, and with no panic, could and has produced severe business contraction" (p. 267).

4.A.6 Monetary Policy

I turn finally to the issue of monetary policy during the Great Contraction. In *A Monetary History* we argued that alternative policies were available that the Federal Reserve System could have pursued and that would have made the contraction less severe. Temin refuses to be drawn into a discussion of alternative policies. "The question posed" in his book "is not whether some alternative policy would have worked, but rather what happened to make such a corrective policy desirable" (p. 7). Nevertheless, he has himself referred to alternative policies, himself conducted a counterfactual "thought experiment," as

he labeled our section on alternative policies. It is counterfactual for Temin to state that, had there been no bank failures, the quantity of money would have been reduced to the same extent by a rise in the reserve-deposit ratio rather than the rise in the currency-deposit ratio that actually occurred. And this counterfactual assertion is refuted by Cagan's study. Temin assumes that the reserve-deposit ratio would have risen as a result of the decline in interest rates in the absence of bank failures. Cagan finds little interest elasticity in this ratio and concludes that the larger part of the change in the ratio was related to panics. A lagged reaction to a panic on the part of banks was to raise the ratio of their reserves to their deposit liabilities.

But to turn to the main question: We do have some evidence for 1930–31 on what alternative policies would have accomplished. We know that when the Federal Reserve System finally undertook open-market purchases of $1 billion between April and August 1932, the money stock grew at a 1.75 percent annual rate of rise from September 1932 until January 1933 compared with the preceding 14 percent annual rate of decline. We know that industrial production rose 14 percent in the second half of 1932 after sharp earlier declines and that commodity prices rose in the second and third quarters of 1932 after declining in the two preceding years. Temin counters that we merely assume that the change in the quantity of money changes the level of income and do not disprove the possibility of reverse causation. Can he really mean that the Federal Reserve undertook the open-market purchases in 1932 as a passive response to an increased demand for money that was a result of rising output and prices that lagged the change in monetary policy? There is evidence also on what alternative policies would have accomplished if we turn to the system's open-market purchases in 1924 and 1927. The omission of discussion of these policy measures in Temin's book reflects his assumption that money is passive. Supply simply adjusts to the demand. This is a real-bills vision with a vengeance.

In *A Monetary History* we found a contrast between the policy actions of the Federal Reserve in 1924 and 1927 on the one hand and 1930–33 on the other. Elmus Wicker (1966) denies such a contrast, arguing that international considerations accounted for the open-market purchases in the '20s and that international considerations were unimportant in 1930–33. In his view, the Federal Reserve never accepted domestic economic stability as a goal of monetary policy. Brunner and Meltzer (1968) also deny the contrast, arguing that in all three contractions, if market rates, particularly short-term rates, fell, policy was regarded as expansive, and if market rates rose, policy was regarded as contractionary. In the earlier contractions, gold inflows and a decline in the demand for currency and bank loans produced a decline in interest rates accompanied by an increase in high-powered money. As

a result, money supply rose and the economy recovered. In 1929–30, gold inflows and declines in the demand for currency and bank loans also produced a decline in interest rates, but high-powered money and the money supply fell. Hence the economy continued to deteriorate. But, as Brunner and Meltzer document, nearly all of the members of the Open Market Committee regarded monetary policy as easy.

We regard Wicker's view as untenable. If the Federal Reserve did not accept domestic economic stability as a goal of monetary policy, why did the system allocate resources to improving the data on economic activity, why did the staff prepare detailed studies on the state of the domestic economy in preparation for open-market committee meetings, why did the system claim credit for domestic prosperity when it occurred? There is without doubt some merit to the Brunner-Meltzer analysis, yet it cannot be accepted as a complete description of the situation. After all, the governor and the chief economists on the staff of the New York Federal Reserve Bank all recognized that the decline in interest rates was not equivalent to monetary ease; they urged, and with some support from others in and outside the system, extensive open-market purchases at various times in 1930, 1931, and 1932 and were not dissuaded from doing so by the decline in interest rates. And these were the people who, so long as Benjamin Strong was alive, effectively dominated Federal Reserve policy. Hence, we continue to believe that had Strong lived or had he been succeeded by someone of similar views and equal personal force, the same monetary growth policies followed in 1924 and 1927 would have been followed in 1930, hence the decline in high-powered money either would not have occurred or would have been promptly reversed, and the economy would have been spared its prolonged ordeal.

Notes

1. In their study of production during the business-cycle contraction of 1929–33, W. C. Mitchell and A. F. Burns (1936) noted:

> The long decline was interrupted by three partial and abortive revivals. Of these, the first, in the early months of 1930, was brief and restricted mainly to automobiles, steel, and heavy construction. The second, in the first half of 1931, had wider scope, lasted longer, and went further. It was especially pronounced in the textile, rubber tire, shoe, and leather industries. The revival in the summer and autumn of 1932 was fairly general, as is indicated by the preceding discussion of the "double bottom" in the terminal trough of this cycle. In some industries one of these abortive revivals lasted long enough and went far enough to produce an "extra" specific cycle during the depression. (P. 18)

2. Haberler (1976, pp. 22–23) notes that the Majority Report of the Gold Delegation of the Financial Committee of the League of Nations in 1932 also attributed the depression to maladjustments caused by the war, but Gustav Cassel in a Memorandum of Dissent disputed the importance of maladjustments and stressed instead monetary phenomena—the undervaluation of the French franc, the overvaluation of the pound, the cessation of U.S. capital exports, and the U.S. depression. Maladjustments were also the explanation of the Great Depression advanced in later studies issued by the Royal Institute of International Affairs (Arndt 1944) and the United Nations Economic Commission for Europe (Svenillson 1954).

3. Abramovitz, in private correspondence with me, has called to my attention qualifications to this statement in his paper. He notes that since the "small-country" model in that paper was designed to apply to long swings, it was inappropriate for use within a single business-cycle contraction and, in any event, could not apply in full force to the United States. On the basis of a subsequent paper (1977), in which he analyzed models of a "large country" and a "small country," Abramovitz believes efforts by the Federal Reserve to sustain the growth of the U.S. money supply in 1930–31, unaccompanied by similar actions by leading European countries, would not have been adequate to prevent the massive decline in income that in fact occurred.

4. See the appendix for a demonstration that Temin's interpretation of interest rate movements as showing no monetary stringency in 1930 and 1931 is contradicted by evidence on the supply of and demand for the relevant money-market instruments during that period.

5. Christopher Sims (1972) introduced a sophisticated alternative test of Granger causality between a pair of variables by running two regressions, with each as dependent variable and both leading and lagged values of the other as independent variables.

6. George Garvy (1959, pp. 71–73) has shown that bank debits to deposit accounts at these centers is a good proxy for nominal income. Peter Lindert (1981) objects to this conclusion since Garvy (p. 87) also reports a lack of perfect conformity of cyclical movements in debits with interwar NBER reference dates (debits lag the turns in January 1920 and July 1921 and skip turns in October 1926 and November 1927). Nonetheless, Moore (1961, vol. 1, chap. 5) includes debits in his list of coincident indicators for that period. Gordon and Wilcox (1981), using quarterly GNP estimates, obtained results similar to those in table 4.2.

7. Eight years because of the need to include lagged values.

8. One other approach to determine unidirectional relationship that some investigators have reported involves cross-correlations of the innovations in X and Y processes derived from Box-Jenkins procedures. Christopher Sims (1977*a*) has criticized that approach as biased "for any null hypothesis except the null hypothesis of no relation between the series." The defect in testing whether "x causes v," he points out, in a formulation

$$(1) y = a(L)y + b(L)c(L)x + v,$$

"*with* a, b, and c, as polynomials in positive powers of the lag operator, L, and v uncorrelated with past values of y or x," is as follows:

> The null hypothesis "x does not cause y" is represented by $b(L) = 0$. Whether or not a, b, and c are linear in the problem's parameters, maximum likelihood will be, for stationary x, y, asymptotically equivalent to choosing

> a, b, and c to minimize the sum of squares of v in the sample period. With any fixed c, an asymptotically valid test of the null hypothesis can be obtained by estimating a and b *jointly* by maximum likelihood or nonlinear least squares, then applying standard test statistics. Though this is not a difficult procedure, [the criticized author] instead chooses c as a filter which makes $c(L)x$ serially uncorrelated, and chooses a as a filter which makes $a(L)y$ serially uncorrelated, then *holding a and c fixed,* estimates b. But this amounts to testing the significance of b by first estimating the regression (1) with b set to zero, then testing for the contribution of b to the regression by examining correlations between the *residuals* of this first-stage equation and the omitted variables of the form $c(L)x$. Anyone versed in the theory of least-squares regression will recognize this as involving a bias in favor of the null hypothesis, except in the special case when the omitted variables are uncorrelated with the included variables. (P. 24)

9. Contrary to Temin (1981), the monetary events listed in the tabulation are, in the main, not "changes in the quantity of money" or "changes in [market] interest rates." They are events, like a change in the Federal Reserve discount rate or an episode of bank runs or Britain's departure from gold or the 1932 open-market purchase program, that are newsworthy and attract attention. They have immediate announcement effects. Moreover, a quick adjustment of prices does not preclude a long distributed lag adjustment. A partial adjustment that shows up quickly is not equivalent to the full adjustment of prices.

10. In a journal article that postdates Temin's review, Barber (1978) traces the origins of the Great Depression to demographic factors that he links to a decline in the residential construction market in the United States and to "a markedly unfavourable influence on the capital spending plans of business firms throughout the developed world" (p. 453).

Annual growth in standardized nonfarm households declined from 3 percent per year to under 2 percent per year from 1924 to 1932. This is supposed to have triggered the decline in U.S. residential construction. Yet the annual growth in standardized nonfarm households from the early 1950s to 1970 was lower than growth of households in any year from 1924 to 1932. Barber attempts to rationalize this inconsistency by citing the availability of mortgage finance since World War II. In that case, the demand for housing is not dependent on demographic factors exclusively.

Similarly, a rapid decline in the rate of population growth after World War I in developed countries, which was accompanied by a lower rate of labor-force growth in the United States and Germany, need not have had the consequence he assumes on capital spending. What evidence is there that firms throughout the world were aware of this demographic trend?

Essentially, Barber fails to establish a connection between his empirical evidence on the decline in population and disequilibrium in the steady-state growth model he presents and a model that would explain recessions. Disequilibrium in a steady-state sense does not explain why the peak in capital spending occurred in 1929 rather than 1928 or 1930.

11. Temin tries to determine (1976, p. 64) from the components of real GNP whether 1930 was a more depressed year than 1921 or 1938. Table 4.4, based on Commerce annual estimates of GNP in current and 1958 prices, the GNP implicit price deflator, and the unemployment rate, is an alternative to Temin's table which shows percentage changes in Kendrick's annual GNP estimates

in 1929 prices, the consumption and investment expenditures components of GNP, and merchandise exports deflated by wholesale prices. For the Commerce estimates, the components of GNP are available only since 1929. The first part of table 4.4, following Temin, relates the changes in the year following the peak in 1920, 1929, and 1937, to the magnitudes of the peak year. The bottom half of the table relates the changes in the year following those peaks to the year preceding the peaks on the ground that the 1929 magnitudes were not typical of the interwar years. One may ask whether 1920 or 1937 was any more typical. In any event, such comparisons between consecutive or nearly consecutive annual figures are subject to substantial error because of possible differences in patterns within the base year and the comparison year. For example, a cyclical peak in December preceded by a rapid rise during the year might be accompanied by a zero year-to-year change, despite a severe recession.

For whatever such comparisons may be worth, the real income decline was somewhat greater in 1930 than in 1921, the rise in unemployment was smaller, and the price decline was much smaller. In all of these respects, 1938 was much the mildest of the three contraction years. Over a two-year span, the results show the 1930 change to be even milder relative to 1921. Of course, 1930 was a contraction year from beginning to end, whereas in 1921 a trough was reached in July, and in 1938, in June. In addition, Temin's use of gross merchandise exports as if that were an independent component of GNP is misleading. The variable normally examined in the national income accounts is net export of goods and services. The change in the variable from 1929 to 1930 is one-third the magnitude of the change Temin reports for gross merchandise exports.

What sets 1930 apart from both 1921 and 1938 is that a banking panic that changed the monetary character of the contraction occurred in the last quarter of the year. In 1921 there were many bank suspensions—triple the number in 1920, for a total of 505 banks with deposits of $172 million. In 1930, there were 1,350 bank suspensions, with deposits of $837 million. In 1938, post-FDIC, suspensions are negligible, 54 banks with $10 million in deposits. Despite the increase in bank suspensions in 1921, there were no runs on banks. That is what distinguishes 1930 from 1921–there was panic in 1930 but not in 1921. Bank suspensions in 1921 were perceived by the public as special problems of agricultural and rural areas but not as affecting confidence in banks generally.

12. On the role of real wages, see section 4.5 of this paper.

13. The fall in prices made the Smoot-Hawley tariff level even higher than it otherwise would have been since specific duties are automatically raised with a declining price level (Haberler 1976, p. 34, n. 65). Meltzer (1976, pp. 459–60) assigns a large role to the Smoot-Hawley tariff and subsequent tariff retaliation by many countries in exacerbating the 1929–33 contraction. The effect of the tariffs was to impede the price-specie flow mechanism and the adjustment of the U.S. and the world economy. Absent the tariff, U.S. prices would have fallen relative to those abroad and led to an increase in foreign demand and net exports.

The protectionist policy that influential British economists in 1930 advised the British government to adopt played a role there parallel to that of the Smoot-Hawley tariff in the United States. In his memoir about the "golden age" of the great British economists, Colin Clark (1977) discusses a "might-have-been":

> It is now unmistakably clear that what Britain, being still a power strong enough to give a lead to the world, should have done in 1930–31, irrespective

> of whether or not other countries so acted, would have been to have preserved Free Trade, accompanied by an expansionist demand policy, and allowing the exchange rate to move freely in response to market pressures. It is now universally agreed that the exchange rate had been overvalued on the return to the Gold Standard in 1925, and a reduction would, in any case have been required. (Though he had protested strongly against the overvaluation in 1925, Keynes himself was not recommending devaluation in 1930–31—the only prominent men to recommend the policy were R. G. Hawtrey, the Treasury's economic adviser, and Ernest Bevin.) Once the exchange rate had been freed, a strongly expansionist policy would have been possible. The preservation of free trade would have allowed the benefits of this expansion to flow to other countries and also, a matter of equal importance, would have set the right example, and spread economic expansion more widely over the world. (P. 90)

Clark's "might-have-been" applied a fortiori to the case of the United States.

14. In *A Monetary History,* we noted that since China was on a silver standard, it was hardly affected internally, 1929–31, by the worldwide economic contraction. Choudri and Kochin (1977) provide similar evidence for Spain for those years. Spain then had flexible exchange rates and a reasonably stable monetary policy.

15. Allan Meltzer (1976) traces the start of the contraction to "economic policies in the United States and other countries operating under the rules of the interwar gold standard" (p. 457). In his view, a relative decline in prices in the United States, as in 1928–29, under the price-specie flow mechanism can induce a recession abroad. He attempts to account for subsequent U.S. price change by relating anticipated price change at the start of the year to the average rate of monetary growth in the preceding three years relative to the rate of monetary expansion in the most recent year, with acceleration from the maintained average having much the larger effect under the gold standard. He regards his predicted rates of U.S. price change for 1930–31 and 1933 as not substantially different from actual price change. For 1932, when the predicted rate was only half the actual rate of price decline, he concludes the decline cannot be explained by the price-specie flow mechanism and the expected response to monetary contraction.

16. The percentage of eligible paper offered for rediscount rejected by the Reserve Banks of New York, Dallas, Philadelphia, and St. Louis (of those reporting such figures) was higher in 1930 than in 1929, possible evidence that acceptability standards were higher despite the decline in discount rates. Of course, member banks had the option of borrowing against their 15-day promissory notes secured by government obligations. See Beckhart, Smith, and Brown (1932).

17. Minor increases in yields on short- and long-term governments and on municipals are reported for December 1930 and March-April 1931.

Brunner and Meltzer (1968) interpret the persistent decline in short-term interest rates despite currency drains and bank failures as the result of adventitious factors offsetting the effects on short-term market rates that would otherwise have been observed. They cite an inflow of gold—mostly from South America and Japan—in the last quarter of 1930 (p. 343).

18. On the change in the character of the commercial paper market since the 1920s, see Selden (1963). The commercial paper market during the 1920s is discussed in Beckhart (1932).