

is the average of the wage rise won by the other n groups in their negotiations plus or minus a term (αq_t) which is supposed to capture the effect of labor market tightness. In their negotiations, workers are backward looking. Since we are working within the Keynesian tradition, we do not think this is an important defect in the model.¹⁴ Moreover, any attempt to model the wage determination process fully within an optimizing framework would be hopelessly complex.

To close the model, the price level is taken to be a simple average of the prevailing wage levels:

$$(9) \quad p_t = \sum_{i=1}^{n+1} W_i$$

$$(10) \quad (1+n)(p_{t+1} - p_t) = \frac{1}{n} ((n+1)[(p_t - p_{t-1}) + \dots + (p_{t-n+1} - p_{t-n})]) + \alpha q_t$$

$$(11) \quad p_{t+1} - p_t = \frac{1}{n} (p_t - p_{t-n}) + \frac{\alpha}{n+1} q_t,$$

which, with (5) and (6), results in workable solutions for p_t and q_t

$$(12) \quad q_t = \frac{(\beta_2 - n\beta_1)p_t - \beta_2 p_{t-n} + n\beta_1 m_t + n\epsilon_t}{n - \beta_2 \alpha \left(\frac{n}{n-1} \right)}$$

$$(13) \quad p_t = \left(1 + \frac{1}{n} + \frac{\frac{\alpha}{n} (\beta_2 - n\beta_1)}{n+1 - \beta_2 \alpha} \right) p_{t-1} - \frac{1}{n - \beta_2 \alpha \left(\frac{n}{n+1} \right)}$$

$$p_{t-1-n} + \frac{\alpha \beta_1}{n+1 - \beta_2 \alpha} m_{t-1} + \frac{\alpha}{n+1 - \beta_2 \alpha} \epsilon_{t-1}.$$

In this framework, increases in the contract period— $n+1$ —can be interpreted as increases in price persistence. Increases in the labor market conditions coefficients—increases in α —can be interpreted as increases in short-run aggregate price flexibility. Because the model is designed to highlight the effects of inflation on output, it has no role for discretionary fiscal policy and no source of shocks other than ϵ_t , the shock to aggregate demand. We take monetary policy to be completely nonaccommodative: m_t is equal to its trend value (zero) always.

14. In subsequent work, we hope to examine the issues here within a model like that of Taylor 1979, where contracts are partly forward and partly backward looking. It seems unlikely that this will alter qualitative conclusions.

This rules out the possibility that the driving force behind economic instability is inappropriate government policy (a bad monetary reaction function) rather than the internal dynamics of the model itself. We wish to use this model to show *only* that the conventional wisdom holding that an increase in nominal rigidities (either in the form of a smaller response of wages to labor market conditions or in the form of a longer contract period—more “persistence”) is harmful to macroeconomic stability rests on shaky theoretical foundations.

We assume a white noise, unit variance generating process for the demand shock ϵ_t and simulate the model for various parameter values. Recall that a high value of β_1 implies either that the direct (“liquidity”) effect of a decline in real balances is large or that the effect of a decline in real balances on the interest rate is large—that is, that the elasticity of money demand with respect to the interest rate is small. A high value for β_2 implies that the expected inflation effects on aggregate demand are large, owing either to real interest effects or to redistributions between debtors and creditors. The parameter estimates are chosen to be reasonable. For example, if $\beta_1 = 1.0$ and $\beta_2 = 1.6$, the standard IS-LM Keynesian multiplier is 1.5.¹⁵ Experimenting with parameter values outside the range displayed frequently resulted in instability but did not alter the qualitative conclusions.

Three conclusions emerge from table 12.11, where the variance of output is calculated for various parameter values. First, in many cases the economy is unstable under the assumption that monetary policy is nonaccommodative with respect to output shocks. This result parallels that of Tobin (1975). Second, in the cases where stability is attained, the variance of output *decreases* with increases in the contract length.¹⁶ When the length of the period over which wages remain fixed increases, the volatility of output declines. This result implies that increasing wage flexibility by reducing the length of the contract period might well worsen macroeconomic performance. This inference is strengthened by noting that increasing the length of the contract period increases the likelihood that the economy will be stable at all. Third, increases in the sensitivity of current wages to output have an ambiguous effect on the volatility of output.

These results are entirely attributable to the fact that the real interest rate—and so $E p_{t+1}$ —enters into the determination of aggregate demand.

The model considered here obviously is highly stylized. No role is allowed for lagged responses of output or money demand to changes

15. Assuming that the constant interest rate multiplier is 3.0.

16. Except for cases in which a high adjustment parameter combined with a long contract length leads to negative feedback so strong that it is destabilizing.

Table 12.11 Output Variance Generated by a Unit Variance White Noise Demand Shock

$\alpha =$	$\beta_1 = 1.0$			$\beta_2 = 1.6$			$\beta_1 = 1.0$			$\beta_2 = 2.4$						
	2	3	4	5	6	2	3	4	5	6	2	3	4	5	6	
.25	*	*	*	*	2.1	1.2	1.2	$\alpha =$.25	*	*	*	*	*	14.4	1.4
.5	*	*	*	*	1.5	1.4		.5	*	*	*	*	*	*	6.2	1.6
1.0	*	*	*	*	3.9	2.3	1.9	1.0	*	*	*	*	*	*	4.2	2.9

$\alpha =$	$\beta_1 = 2.0$			$\beta_2 = 1.0$			$\beta_1 = 2.0$			$\beta_2 = 0.5$					
	2	3	4	5	6	2	3	4	5	6	2	3	4	5	6
.25	*	*	*	1.3	1.1	1.1	$\alpha =$.25	*	*	*	*	1.2	1.1	1.1
.5	*	*	2.9	1.4	1.3	1.2	.5	*	*	1.6	1.3	1.2	1.2	1.1	1.1
1.0	*	*	*	13.1	*	*	1.0	*	*	*	*	*	2.4	*	*

*Model unstable for these parameter values.

in real interest rates. Deflation has no direct effect on aggregate demand, operating only through its impact on real interest rates. Thus the distributional effects emphasized by Tobin (1975) are suppressed entirely. Perhaps most important, we assume no response of monetary or fiscal policy variables to demand shocks. This exercise hardly proves that price flexibility increased the volatility of economic activity before World War II. But it does strongly suggest that deviations of the real interest rates from its general equilibrium value caused by the process of equilibration in product and labor markets may contribute as much to economic instability as deviations in product prices or wages from their static equilibrium values, if not more.

It might be objected that our analysis here misses the point, since we assume an aggregate supply mechanism implying that a change in the monetary rule could have a long-run effect on output. Such an objection is made by McCallum (1983) to analyses similar to the one presented here. This objection is misplaced. At one level the criticism is irrelevant, since we do not use our model to consider alternative monetary rules. At a more fundamental level, it ignores the need for economic theory to provide a theory of how prices move to clear markets. As Fisher (1983) and others have eloquently argued, it is insufficient to assert that economies will always reach their Walrasian equilibriums without describing how they get there. Some sort of price adjustment equation like (11) is an indispensable part of any fully articulated economic model.

A macroeconomic view that stresses the dangerous potential for destabilizing deflation present under a regime of flexible prices can avoid some of the problems that economists have traditionally encountered while trying to analyze the origins of the Great Depression in the United States. Economists like Temin (1974), who attempt to account for the Great Depression by a decline in exogenous spending induced by falling "animal spirits," have a difficult time explaining why those who make investment decisions suddenly become more pessimistic. Without making reference to the destabilizing effects of deflation, it is also difficult to account for rising real interest rates in the face of an autonomous decline in spending.

Economists like Friedman and Schwartz (1963), who attempt to account for the Great Depression in terms of an inappropriately contractionary monetary policy, have a difficult time explaining the behavior of the real money supply. As figure 12.3 shows, the real money supply actually increased slightly between 1929 and 1933 while output was falling by close to 50%. Since aggregate demand should be closely linked to the real money supply, it is hard to see how a monetary impulse could have caused the depression without ever reducing real money balances. Moreover, without making reference to the effects of defla-

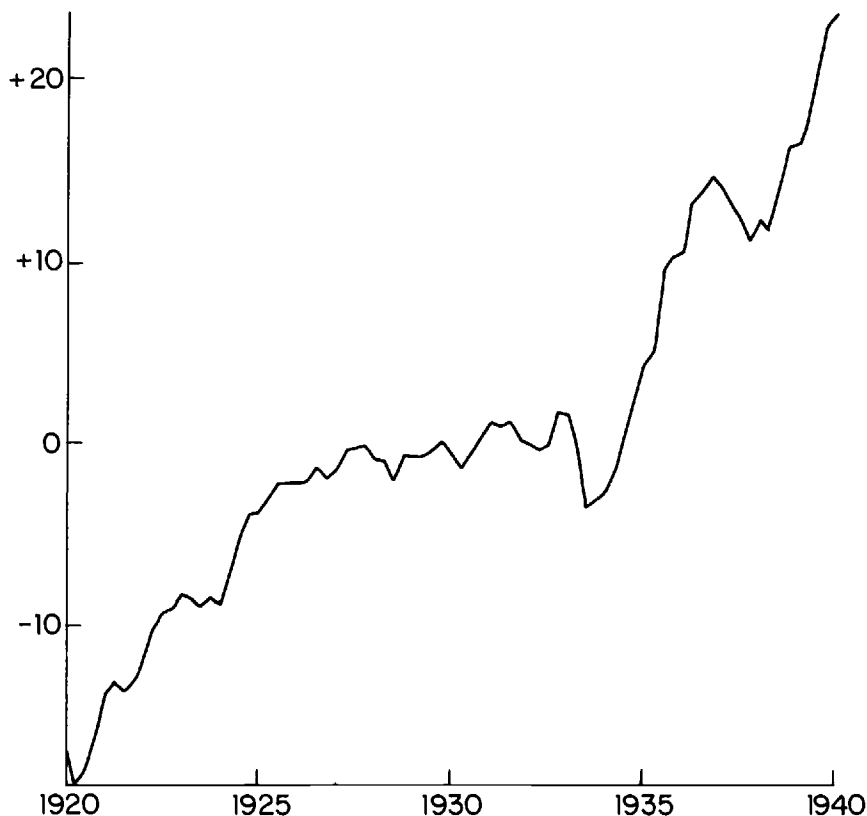


Fig. 12.3 Percentage deviation of the real money supply (M1) from its average 1926–29 value.

tion, it is hard to explain why nominal interest rates fell in the face of a monetary shock.

More generally, evidence for the view that increased price flexibility is destabilizing comes from an examination of the changing behavior of real interest rates plotted in figure 12.4. The standard deviation of ex post real rates on an annual basis was 3.10% in the period 1893–1915, compared with 0.57% in 1949–70 and 1.37% in 1971–82.¹⁷ Before 1979, the highest real interest rate observed on a quarterly basis was 6% in 1974, and in only five quarters in the pre-1979 post-World War II period were real rates greater than 4% observed. On the other hand,

17. The behavior of real rates since 1979 is, in the context of the rest of the post-World War II period, anomalous. A glance at recent real rates seems to suggest that American economic policymakers are attempting to restore the pattern of real rates characteristic of the 1890s.

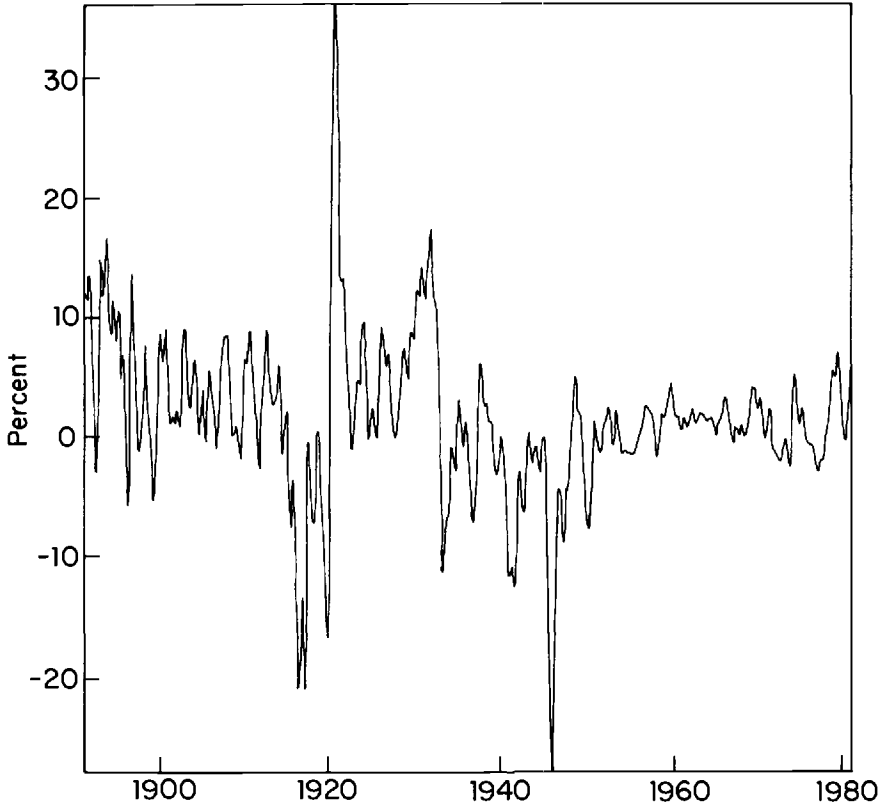


Fig. 12.4 Ex post short-term real interest rates.

real rates greater than 6% occurred in every single reference cycle recession (except 1903–04) during the pre–World War I period. It seems clear that these variations in real rates should have contributed greatly to economic instability.

12.4 Is Aggregate Price Flexibility Destabilizing?

In section 12.3 we argued that in the standard aggregate demand/aggregate supply framework there are no strong theoretical reasons for believing that a small increase in aggregate price flexibility—defined either as an increase in the responsiveness of wages to labor market conditions or as a decrease in “persistence”—would reduce the variance of output. We also expressed our suspicion that in the United States the relation between price and output flexibility goes the other way from that typically assumed. We suggested that some of the relative

macroeconomic good fortune of the United States since World War II can be traced to the possibility that a flatter short-run aggregate supply curve dampens fluctuations in the real interest rate and so dampens fluctuations in output.

We put forth this potential explanation because the other mechanisms we have identified cannot account for all of the decline in the variability of output from the prewar to the postwar period. The rising share of government expenditures can account for a small fraction of the decline in variance, and the smoothing of purchases of consumer nondurables and services as a result of automatic stabilizers and commercial credit can account for a significant portion. But there remains a substantial decline in the relative variance of “long-term” expenditures—construction, business investment, and consumer durable purchases—that is documented in Robert Gordon and John Veitch’s paper in this volume. The standard explanation is that this decline in the variability of “long-term” expenditures is due to the expectation of successful stabilization policy. But since we cannot find the traces in other economic variables that we expected to find if this were indeed correct, we believe that the decline in the variance of “long-term” expenditures needs further explanation. And since “long-term” expenditures are in theory very much dependent on the real interest rate, we advance the hypothesis that the primary channel through which price flexibility affects macroeconomic performance is the instability induced by aggregate price flexibility in the real interest rate.

12.4.1 Reduced-Form Evidence: Theory

In this section we present some empirical evidence to back up the hypothesis that price rigidity has contributed to macroeconomic stability. We had hoped to estimate a simple structural model and thus to see if the data supported our hypothesis by testing whether the parameters of the structural model fell in a region where aggregate price flexibility was destabilizing on the margin. But we are unable to do so. Attempts at estimation repeatedly failed to converge or converged to unstable parameter values. We appear to have been unable to nest our hypothesis in a structural model that is both tractable, in the sense of being simple enough for us to gain some analytic understanding of its properties, and believable, in the sense of not being rejected out of hand by the data.

Since the restrictions we found necessary if we were to formulate a model that we could understand and interpret also destroyed the fit of the model with the data, we shifted to nonstructural estimation. The current practice among economists seeking to draw conclusions that are not highly sensitive to minor changes in the underlying model is to use vector autoregressions and to plot the resulting impulse response

functions. In such an analysis, a positive response of output to an inflation shock might be taken as evidence in favor of our approach.

We have run analyses along these lines, but we find problems in interpreting the impulse response functions as evidence for any position, since we have no good idea of what an “inflation shock” is or what actual economic processes it represents. Therefore we also present (quasi-) reduced forms for output and argue that the pattern of coefficients that emerges is hard to justify with any underlying theoretical model other than our hypothesis.

According to the mainstream Keynesian macroeconomic approaches, the primary determinants of output are three: lagged output, (lagged) real money balances—operating through wealth and liquidity effects—and the nominal interest rate. Lower real money balances choke off aggregate demand in general, and higher nominal interest rates reduce the demand for investment goods in particular. Whether one believes that real balances are only a passive indicator of nominal interest rates, credit conditions, and animal spirits or that interest rates are only an index of the underlying determinant, real balances, it remains true that output should be, in any kind of reduced form, a positive function of (present and) lagged real balances and a negative function of (present and) lagged interest rates.

Implicit in the mainstream view is a “Keynesian” picture of price adjustment. Changes in real balances or nominal interest rates cause disturbances in aggregate demand. Because in the aggregate quantity adjusts more quickly than price, the changes in the movement of the price level associated with changes in real balances and in interest rates show up—in the time period relevant to the study of business cycles—only after the movement in output. In the mainstream view, the price level responds to its own lagged values and to the level of nominal demand. The mainstream view cannot account for a significant positive link running from prices to output without abandoning the “Keynesian” interpretation of the relative speeds of price and quantity adjustment that is its foundation. There is one set of events that, according to the mainstream view, should generate a correlation between present price movements and future output. This is the case of the “supply shock,” in which present jumps in prices are associated with declines in future output. But this produces a correlation with the opposite sign from that expected according to a theory centered on the real interest rate.

The explanation for output fluctuations usually given by classical economists follow these lines: some agents (workers, not firms) misperceive relative prices. They believe that the real wage is higher (or lower) than it really is and so work more (or less) than is optimal. If there are intermediate goods in the production process, it is possible

to claim that output depends both on the degree of relative price misperception and on lagged production of intermediate goods—on lagged output. This line of thought produces a Lucas aggregate supply function:

$$(14) \quad q_t = \alpha_1(p_t - E_{t-1}p_t) + \alpha_2q_{t-1}.$$

Note that the new classical approach predicts that, in a reduced form of output on lagged output, present and lagged prices, and other variables, the only variables that can enter with positive coefficients are lagged output. Lagged prices are useless as predictors of $p_t - E_{t-1}p_t$ and should, in the new classical framework, not enter into the reduced form at all.¹⁸ Therefore we conclude that a significant positive effect of lagged prices on present output fits easily into neither the mainstream nor the new classical view of the macroeconomy. And we believe that the existence of such a positive effect is evidence in favor of an older view of business cycles, a view that places special stress on the role of the real interest rate.

With these theoretical observations in mind, we estimated vector autoregressions for a variety of periods and specifications on quarterly data taken from Gordon (1982a) and annual data taken from Friedman and Schwartz (1983). The results provide some evidence in favor of our hypothesis. A price innovation has, looking at the impulse response functions, a positive effect on future output. And in the reduced form for output, lagged price enters with a generally positive coefficient.

We find this significant. According to the view that stresses the importance of nominal rigidities in causing business cycles, price innovations have to (when nominal balances are held constant) have a negative effect on future output. Deflation should raise the real money stock and thus increase output. But the equations indicate, in support of our more dynamic view, that deflation may itself lower output.

12.4.2 Reduced-Form Evidence: Empirical Results

The first set of vector autoregressions estimates the following three-equation system:

$$\begin{bmatrix} \dot{p} \\ q \\ i \end{bmatrix} = \begin{bmatrix} 0 & \alpha_{012} & \alpha_{013} \\ 0 & 0 & \alpha_{023} \\ 0 & 0 & 0 \end{bmatrix} \begin{bmatrix} \dot{p} \\ q \\ i \end{bmatrix} + A(L) \begin{bmatrix} \dot{p} \\ q \\ i \end{bmatrix} + \begin{bmatrix} \epsilon_p \\ \epsilon_q \\ \epsilon_i \end{bmatrix},$$

where $A(L)$ is a three-by-three matrix polynomial of order five in the lag operator. The variables in this autoregression are:

q the output ratio, real GNP/natural real GNP

18. According to the new classical view of things, shocks have persistent effects even though lagged prices are not in the equation for q_t . Past prices affect past output, and past output enters the equation that determines present output.

\dot{p} the quarter-to-quarter inflation rate

i the commercial paper rate.

(All data are taken from Gordon 1982a.)

Note that the arrangement of the variables in the VAR is such as to minimize the potential impact of any innovation in p . Only that part of

$$\dot{p}_t - E_{t-1}\dot{p}_t$$

that is uncorrelated with $q_t - E_{t-1}q_t$ and $i_t - E_{t-1}i_t$ will be counted as an inflation innovation. Thus the risk that our interpretation of the results is in error, that the VAR is reading correlations between \dot{p} and q that are really driven by causal links from q to \dot{p} and from lagged q to q as evidence in favor of our hypothesis, is minimized.

The VAR was initially estimated for time periods 1893:1 to 1915:4, 1923:1 to 1940:4, and 1949:1 to 1982:4. This particular three-variable system was chosen because no quarterly data on the money stock are available before 1907. Thus there are not enough data to estimate a VAR including the money stock for any pre-World War I period. We are reluctant to base any arguments on a comparison of the post-World War II period with the interwar period alone. The Great Depression represents an extraordinary cumulation of shocks and so is probably not well studied using the VAR method.

An objection to estimating this particular system might be made along the following lines: the choice of variables—output, inflation, and interest rates—implies that the effects attributed to the inflation variable are *only* the effects of movements in accommodated inflation. Unaccommodated movements in inflation will, because the interest rate is an index of the real money stock, also appear as movements in interest rates. And so some of the depressing effect of price rises on output will appear as an effect of interest rate movements on output.

Two facts militate against this argument. First, it implies that the contemporaneous correlation between inflation and interest rates should be positive, that α_{013} should be greater than zero. Instead, α_{013} is less than zero (though not significantly so).

Second, the equations were also estimated for the four-equation system consisting of inflation, the commercial paper rate, the output ratio, and the detrended nominal money stock. The variables were so ordered as to give the maximum potential scope to the monetary innovation, the second place to the output innovation, the third place to the interest rate innovation, and the least potential scope to the inflation innovation.

Quasi-reduced-form equations for output are shown in table 12.12. Impulse responses of output to an inflation innovation are plotted in figures 12.5 and 12.6. We note two things from these empirical results. First, this method is not suited to the interwar period. The interwar

Table 12.12 **Quasi-Reduced-Form Equations for Output**

Period	R ²	SEE	Interest Rate					Inflation Rate					Lagged Output					Nominal Money				
			Cont.	-1	-2	S(5)	Cont.	-1	-2	S(5)	Cont.	-1	-2	S(5)	Cont.	-1	-2	S(5)	Cont.	-1	-2	S(5)
1893-1915	.63	.038	-.06 (.65)	-1.5 (.76)	.87 (.80)	-2.6 (.99)	—	.44 (.34)	-.27 (.27)	.32 (.70)	—	.89 (.12)	.12 (.16)	.75 (.15)	—	—	—	—	—	—	—	—
1923-40	.95	.039	.05 (.46)	2.22 (2.38)	-2.35 (2.53)	-.07 (1.37)	—	.53 (.46)	-.47 (.49)	.92 (.59)	—	.93 (.14)	.03 (.18)	.96 (.06)	—	—	—	—	—	—	—	
1949-82	.94	.008	.19 (.10)	-.23 (.15)	-.30 (.16)	-.27 (.11)	—	.31 (.14)	-.02 (.14)	.21 (.19)	—	1.10 (.09)	-.01 (.14)	.93 (.03)	—	—	—	—	—	—	—	
1923-40	.96	.034	—	3.43 (1.52)	-3.24 (2.50)	1.32 (.74)	—	.22 (.43)	-.64 (.45)	-.48 (.66)	—	.83 (.15)	-.06 (.19)	.83 (.10)	.90 (.27)	1.33 (.18)	-.16 (.33)	.97 (.03)	—	—	—	
1949-82	.95	.008	—	-.09 (.10)	-.34 (.14)	-.09 (.04)	—	.33 (.14)	.00 (.15)	.15 (.18)	—	1.23 (.09)	-.15 (.13)	.96 (.03)	-.01 (.02)	.02 (.05)	.04 (.03)	.90 (.03)	—	—	—	

Source: Data from Gordon 1982a.

Note: Cont. = contemporaneous coefficient of variable; S(5) = sum of coefficients on five lags.

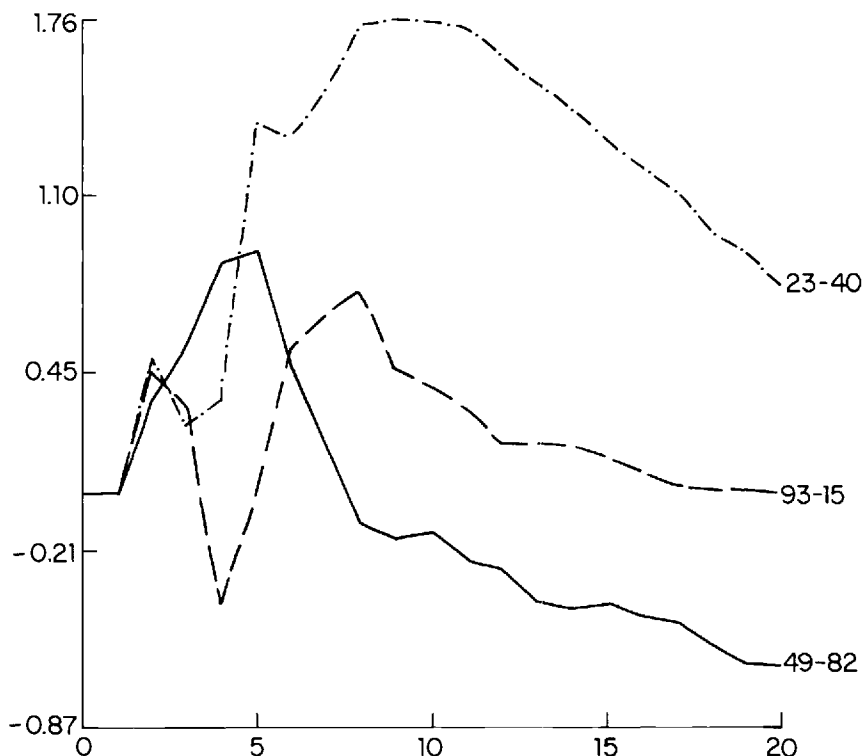


Fig. 12.5 Output response to inflation innovations, three-variable system, 1949-82, 1923-40, 1893-1915.

period is so strongly dominated by the Great Depression that all correlations are warped: the decline of the nominal interest rate during the onset of the depression is the only variable the model can latch on to in accounting for the depression, hence the excessively large difference in the coefficients on the first and second lag of the interest rate. If one turns back to figure 12.2, this should come as no surprise. The Great Depression was a unique event, and attempts to analyze the entire interwar period are, in essence, attempts to generalize from a sample of one.

Second, both the coefficients on lagged inflation in the output equation and the impulse responses of output to an inflation shock are positive and, in general, significant at at least the .10% level. This correlation is not easy to explain within either the new classical framework or the mainstream framework. The hypothesis urged here, with its emphasis on real interest rate effects, does provide a natural explanation.

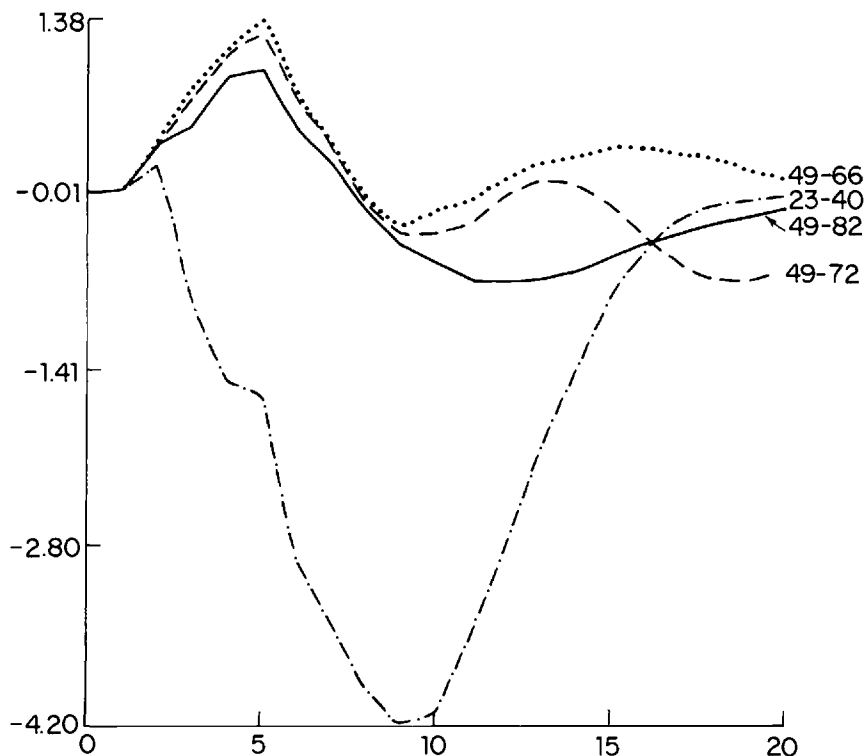


Fig. 12.6 Output response to inflation innovations, four-variable system, 1949-82, 1949-72, 1923-40.

This belief is bolstered by additional equations run (but not reported). For various combinations of interest rates, inflation rates, output ratios, real and nominal money stocks, the only equation that failed to generate a positive correlation between inflation innovations and future output and positive terms on lagged prices in the output equation was a VAR that included no interest rate variable—only the output ratio, inflation, and the nominal money stock. Furthermore, the effect of lagged inflation innovations on output is significantly greater for the four-variable system for those two post-World War II periods that do not include the supply shock ridden 1970s. This tends to support our hypothesis. The 1970s were dominated by supply shocks, by disturbances that first raised \dot{p} and then reduced q . These shifts in the short-run aggregate supply schedule should mask the effects we are looking for in the post-World War II period. That these supply shocks do reduce the positive effect for the period 1949:1 to 1982:4, and that this masking is only partial, encourages us to think that we are correctly interpreting our

VARs and that the effect of price innovations is, in the United States today, strongly procyclical.

To sum up: an unstructured analysis of the correlations between macroeconomic variables carried out by means of VARs produces a finding—inflation innovations have a positive effect on future output—that is hard to interpret from either an equilibrium business cycle or a nominal rigidities perspective. We can think of no other convincing reasons for this association besides the one we advocate: changes in the aggregate price level produce changes in the real cost of capital that have effects on the level of expenditures on items having a high interest elasticity of present value. Thus deflation at the beginning of a recession would deepen the recession by causing a further cutback in investment. This correlation suggests that reducing nominal price rigidity would not diminish the seriousness of business cycles.

12.5 Conclusions

We began by suggesting that the large change in the variance of output between the prewar and postwar periods was a fact that should be explicable within a satisfactory business cycle theory. We then argued that a number of factors frequently alleged to have led to greater stability, including structural changes in the economy, discretionary stabilization policy, and the avoidance of financial panics, probably did relatively little to enhance stability. We conclude that the two principal factors promoting economic stability have been greater public and private efforts to smooth consumption and the increasing rigidity of prices. We attribute the latter development to the increasing institutionalization of the economy.

Comment Robert Eisner

I am glad to find Keynes rediscovered, if only in part.

DeLong and Summers see the amplitude of cyclical fluctuations as less in the postwar period and attribute this principally to “greater public and private efforts to smooth consumption and the increasing rigidity of prices.” The public efforts are related to a greater government component in aggregate demand and automatic, but not discretionary, countercyclical fiscal instruments. The easing of liquidity constraints, which DeLong and Summers relate to greater amounts and

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ease of household borrowing, has further encouraged a divorce of consumption from fluctuations in current income and hence reduced the multiplier of exogenous shocks.

DeLong and Summers reject the argument attributed to unnamed “Keynesians” that rigid wages and prices contribute to fluctuations in employment and output. They suggest rather, going back to Keynes (via Tobin), that less rigid prices magnify fluctuations. A fall in prices, for example, generates an expectation of falling prices. This contributes to higher *real* rates of interest and thus aggravates the real decline that initiated the price movement.

DeLong and Summers might well have recalled the rigorous development of the argument by Oscar Lange (1952) that flexible prices could not be relied upon to eliminate excess supply of goods or labor (unemployment). This is not to claim, as DeLong and Summers do (sect. 12.3), that rigidities can make employment “more stable than it would be if a Walrasian equilibrium were attained in every period.” Walrasian equilibrium presumably means zero excess supply in all markets and hence no unemployment at all. But as Lange pointed out, if price expectations were relatively elastic, flexible prices might not correct a situation of excess supply in commodity markets, or excess demand for money; Walrasian equilibrium would not be attained. Simply enough, lower prices would then generate an expectation of still lower future prices, raising the current-to-future price ratio and hence reducing current demand for commodities and raising the real demand for money.¹

But this is not to say that, under these conditions of relatively elastic price expectations, less flexible prices are necessarily better than more flexible prices. Keynes argued, indeed in the lines quoted by DeLong and Summers, that “it would be much better that wages should be rigidly fixed and deemed incapable of material changes, than that depression should be accompanied by a gradual downward tendency of money-wages.” Complete rigidity would be better, but a more gradual fall—greater rigidity—may well generate more in the way of expectations of further declines than the quicker and more rapid decline that might be associated with less rigidity.

DeLong and Summers’s discussion of this issue seems at times to be caught in the misunderstandings among Keynes, neoclassicists, and new macroeconomists. To the neoclassicists and the new macroeconomists, it is presumably the real wage that matters. Excess supply of labor—or unemployment—would be eliminated if workers would allow their wages to fall. This would increase employers’ demand for labor

1. A “positive monetary effect,” to offset this and generate a net increase in the demand for commodities, would require action by the monetary authority to ensure that the supply of real cash balances increased *more* than the demand for them.

and decrease workers' supply of labor and hence restore equilibrium in the labor market.

To Keynes, however, this was nonsense. He insisted that workers had no means of lowering the *real* wage. If they agreed to lower nominal wages, since prices under conditions of perfect competition equal marginal costs and marginal costs depend overwhelmingly, if not exclusively, on variable labor costs, the reduction in wages could be expected to bring about an equal reduction in prices. Hence, for Keynes, flexible wages entailed equally flexible prices and a real balance effect that would lower nominal and (if the elasticity of expectations were unity) real interest rates. The neoclassical Pigou/Haberler argument could readily be appended to this so that increasing real money balances would raise aggregate demand via a direct wealth effect as well as by lowering the rate of interest.

But if this is the argument that DeLong and Summers think to test, they could not expect much empirical evidence, as Pigou and Patinkin long ago acknowledged. It is hardly plausible that a fall of 1% or 2% in prices, let alone a mere slowing in the rate of increase of prices, would have enough of a real balance or wealth effect to make much difference in consumption or in the amount of recession fall in aggregate demand. As Patinkin (1951) pointed out, even the major fall in prices in the Great Depression of the thirties could not have made much difference.

But the suggestions DeLong and Summers make about the role of lesser price flexibility, developed more rigorously, point further to a serious contradiction of another of their major arguments: that higher government expenditures and automatic stabilizers have probably contributed to lesser cyclical amplitude but that discretionary countercyclical policy has not. I should make clear that I am not disposed to argue very enthusiastically that discretionary policy has made much difference. My own view is that discretionary fiscal policy had been so rarely and fitfully—if ever—applied that nothing of a stabilizing nature is likely to show up in the data on its account. Monetary policy has been, understandably, generally so much more accommodative than countercyclical that, given the intrinsic limitations of the powers of the monetary authority, I do not look for much in the way of results of discretionary policy there.

That said, though, I find seriously suspect the authors' argument that the lesser magnitude of cyclical fluctuations may be attributed to automatic stabilizers but that allegedly greater persistence of fluctuations is evidence that discretionary countercyclical policies have not been effective.

To begin with, I have serious trouble with a number of DeLong and Summers's measures. It is not clear to me that the magnitude of cyclical fluctuations can be well grasped by the standard deviation of either

quarter-to-quarter changes in the log of real GNP or the difference between the log of real GNP and the log of “natural” or “trend” real GNP. I would measure the amplitude of cyclical fluctuations in terms of movements from trough to peak and peak to trough or, adjusting for trend, as the movement in the differences from trend as the economy progresses from trough to peak and peak to trough. The DeLong and Summers measures will tell us more about how abruptly movements are made or how long the economy is markedly above or below its trend or “natural” positions than about the total amplitude of fluctuations. The corollary of this is that something that slows a decline, or curbs a boom and hence stretches it out, will be viewed by DeLong and Summers as reducing “cyclical variability” but increasing persistence.

Yet there is no reason to assume that what discretionary countercyclical actions may have been implemented functioned to hasten turning points rather than merely to reduce rates of change. And classical views of the business cycle suggest that a slower decline might well delay a turning point by slowing the “cleansing” via working off of excess stocks of inventory and fixed capital. Similarly, the very purpose of slowing a boom (not a Keynesian recommendation) would be to prolong it.

The authors’ argument about greater price rigidity suggests that declines would have been slowed (and upturns as well) by reduction of the destabilizing expectations factor that would otherwise tend to raise real interest rates in a recession and lower them in a boom. But then this factor also would tend to increase the persistence of fluctuations. We appear to be left with no grounds at all for the argument that greater (or at least no lesser) persistence of recessions and booms in the post-war period must imply a failure of discretionary countercyclical policy (whatever it may have been).

But that leads to another problem with DeLong and Summers’s measures. They all are apparently addressed to the cycle as a whole, with no distinction between recessions and booms. In the prewar days, it was customary to think of Western economies as victims of chronic unemployment, recessions or depressions driving unemployment well below its chronic levels, and “sick recoveries which die in their infancy,” in the words of Alvin Hansen (1944, 370) and which rarely if ever brought us even briefly to periods of full capacity, full employment boom. As J. R. Hicks (1950, chap. 6) put it neatly, the economy could well struggle along a floor, with recovery never reaching the full employment ceiling, as I illustrate in figure C12.1. If it did, it would be aborted abruptly by a shortage of accelerated-induced investment when growth became limited by the slope of the ceiling at full capacity. But this view of the cycle, which I find appealing, suggests a rather different

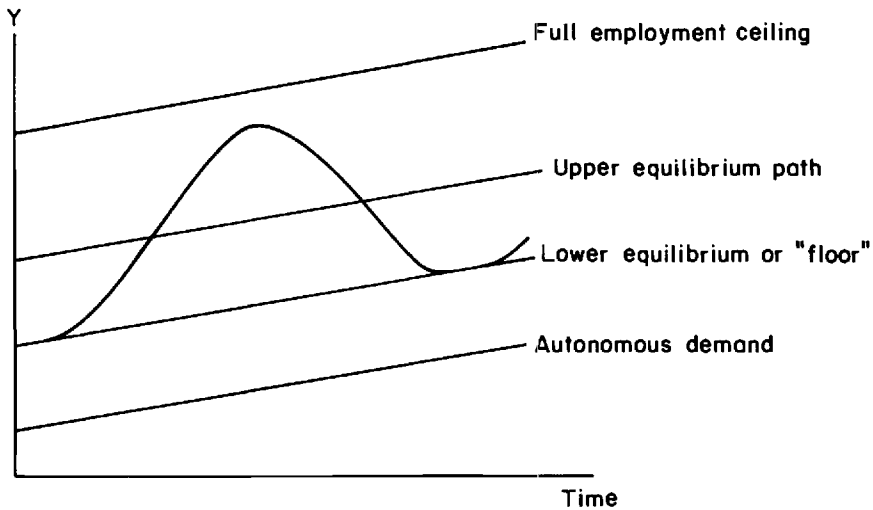


Fig. C12.1 Hicksian trade cycle: low autonomous (or government) demand.

interpretation of the data on which DeLong and Summers have focused. In the Hicksian treatment, the increase in the proportion of gross national product purchased by government as well as the increased cushion of exogenous consumption provided by government guarantees of income, current and (via social insurance) future, results in a higher floor, a higher equilibrium path, and, most important, much longer periods during which the economy can remain at or close to its ceiling, as shown in figure C12.2. The higher floor would mean that the amplitude of fluctuations is reduced, since the economy can fluctuate only between its floor and its ceiling. But the longer periods at the ceiling—witness the relatively full employment for five years from 1965 through 1969, for example—would turn up as greater “persistence” in the DeLong and Summers measures. Such persistence would not necessarily imply that recessions have been longer than in the prewar period.²

The explanation of alleged greater stability DeLong and Summers offer turns to lessened “liquidity constraints” on consumers in the postwar period. These are presumed to supplement the countercyclical tax and transfer payments that tend to divorce personal income from

2. Keynes, it may be recalled, was highly critical—very correctly, I would insist—of policies to stabilize the economy by lopping off the booms. He wanted, rather, to fill in the troughs. The Kennedy/Johnson tax cut may be viewed as a discretionary policy designed to do just that, and the Vietnam War, whatever its intent, served economically to prolong a boom.

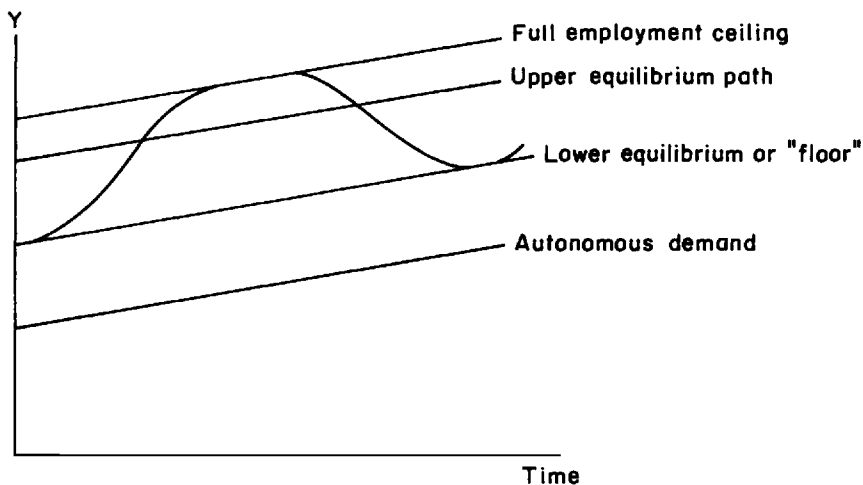


Fig. C12.2 Hicksian trade cycle: high autonomous (or government) demand.

gross national product. As DeLong and Summers point out, if there were no constraints that tied current consumption to current income, consumers optimizing in terms of a life cycle or permanent income consumption function would not even need countercyclical taxes and transfer payments to maintain consumption relatively constant despite loss of jobs and income.

Here DeLong and Summers seem to get some of their numbers wrong and ignore others that are relevant. For one thing, the evidence of greater postwar stability is somewhat marred by the actual facts of the 1973–75 and 1981–82 recessions. First, the maximum decline in real GNP, which did occur during the 1973–75 recession, was not 1.8%, as DeLong and Summers report in section 12.1, but 4.88% (from \$1,266.06 billion in the fourth quarter of 1973 to \$1,204.258 billion in the first quarter of 1975). And the most recent decline was not the 1.8% that they report in section 12.2.1 but 2.96% (from \$1,522.105 billion in the third quarter of 1981 to \$1,477.061 billion in the third quarter of 1982).³

What is more, real disposable income did not rise by the 1.1% and 1.0% that they report (sect. 12.2.1) but fell, by 3.24% in the earlier period (from \$874.1 billion in 1973:4 to \$845.8 billion in 1975:1) and by 0.50% in the most recent recession (from \$1,058.091 billion in 1981:3 to \$1,052.847 billion in 1982:1).⁴

3. See table 1.2 of the national income and product accounts, as revised in July 1984.

4. *Ibid.*, table 2.1.

But none of us should take these disposable income figures that seriously without careful adjustment. It should be well known that disposable personal income includes nominal interest payments, which have of course recently become enormous, now surpassing \$400 billion, rather than real interest payments. Another way of putting this is that in periods of inflation, holders of debt suffer losses in real value that should be netted against their interest receipts in calculating their income. With such inflation adjustments, real disposable income may be seen to have nose-dived considerably more than GNP in the sharp supply side, inflation shock recession of 1973–75 as well as during the onslaught of the 1981–82 recession. Although higher marginal rates of taxes and transfer payments probably did contribute to the sharply lower regression coefficient of changes in disposable income on changes in GNP in the postwar period noted in DeLong and Summers's table 12.5, I would not infer much from that without viewing movements of an appropriately adjusted disposable income in particular cycles.

I also find their associated argument about the changes in liquidity constraints suspect. In particular, in a number of places they equate the easing of liquidity constraints with greater amounts and hence, to them, greater ease of consumer borrowing. In fact, of course, consumer debt has increased greatly and is now vastly more than in the prewar period. But what is relevant in its impact on the smoothing of consumption is not the amount of consumer borrowing per se, but net *movements* in consumer debt during cycles.

My cursory examination of the data confirms my a priori notion that net changes in consumer debt varied procyclically. For example, net increases in total consumer credit (*Economic Report*, 1984, table B–69, p. 301) moved down from \$25.6 billion in 1973 to \$9.7 billion in 1974 and \$9.1 billion in 1975, and then up again to \$25.2 billion in 1976 and \$39.7 billion in 1977. \$48.2 billion in 1978, and \$44.6 billion in 1979. But then they declined to \$4.2 billion in 1980 and rose to \$24.0 billion in 1981, only to fall again to \$18.2 billion in 1982. These changes were in nominal dollars. There must certainly have been substantial declines in the real value of consumer credit outstanding during recession years.

Perhaps of even more moment were sharp declines in rates of increase in mortgage debt outstanding during recession years. For aside from the relation of this to residential investment, it seems apparent that substantial amounts of mortgage debt were used to finance consumption expenditures. We may note, for example, that mortgage debt outstanding on one- to four-family houses increased by \$96 billion in 1980, by \$78 billion in 1981, and by \$49 billion in 1982 (*Economic Report*, table B–71, p. 303). Since these figures are derived from mortgage debt outstanding in nominal dollars, the results of conversion to

changes in real debt suggest that these forms of liquidity contributed to magnifying rather than reducing downward pressures on consumption.

I find various of DeLong and Summers's analyses and tests imaginative and ingenious but less than fully convincing. On the whole question of lesser competitiveness in the postwar economy, it is difficult to get any clear measure, particularly as it may relate to price flexibility, and DeLong and Summers offer little more than intuitive judgments. One of their points, that union membership increased among nonfarm workers from 9% in 1930 to 29% in 1950, is hardly persuasive in view of the substantial subsequent decline in union membership since.

Relying on future stock market movements as measures of anticipated persistence of declines or rises in economic activity strikes me as particularly frail. I doubt that DeLong and Summers would risk much money investing on the basis of such a presumed relation. Indeed, one may wonder that they did not consider real interest rate movements, on which they had focused in their theoretical discussion. I should think that in at least some instances economic declines have been associated with increases in real interest rates and booms have correlated with declines, and these might well have contributed to greater "persistence" in economic movements as well as movements of stock prices.

And I must confess I am left somewhat cold by the variety of VARs DeLong and Summers (and many of our colleagues) display. I am not sure they know quite what to do with their results; they accept some of the equations reported (acknowledged to be only some of the many equations run) while they reject others with statements such as, "this methodology is not suited to the interwar period . . . so strongly dominated by the Great Depression that all correlations are warped" (sect. 12.4.2). "The Great Depression," we are told, "was a unique event." But how many other unique events might have accounted for the various coefficients and their movements, with relevant differences in coefficients rarely very statistically significant?

My own view of the story on cyclical fluctuations is fairly simple. There *has* been a change that relates essentially to a much larger role of government, perhaps facilitated by rationalizations stemming from the Keynesian revolution. The huge surge in aggregate demand stemming from government expenditures in World War II propelled the economies of the United States and much of the industrial world close to full utilization of capacity. Shortages of capital as a consequence of the war stimulated an investment boom. The increase in public debt, at least in the United States, contributed both a perception of wealth

and, directly or indirectly, liquidity. These brought on higher levels of consumption demand and, possibly also, a greater independence of consumption from current income. Consumption demand was increased further by the great growth in social insurance that raised individual perceptions of permanent income and reduced some of the risk and uncertainty with which it was anticipated.

Increased government expenditures and consumption did not, however, reduce investment demand. Even narrowly defined business non-residential investment in plant and equipment was far above its depression levels, while more broadly defined investment, including government and household expenditures for tangible capital as well as investment in intangible capital, boomed.

As a consequence of the secular upward movement in aggregate demand—the inverse of the secular stagnation of the prewar period—employment and output, aided by the particular stimuli of the Korean and Vietnam wars and generally high military spending (certainly high compared with prewar periods), were generally much closer to full employment and full capacity. With “autonomous” demand from government and government support programs generally higher and with substantial anticipations of continued long-term growth and prosperity, recessions were generally shallower; in some cases it was hard to find more than temporary retardations in the rate of growth, or recessions that did not show up in annual data. And booms were flatter and longer as persistent, high, government-generated demand, at least where it became excess, tended to generate inflation rather than quickly unstable peaks of output. Employment did not really become “overfull” or rise above its “natural” rate. In a relatively free economy and society, workers are not “tricked” into working more than they want, anybody’s parables to the contrary notwithstanding.

Hence, the postwar period does evidence generally lesser amplitude of fluctuations and, perhaps, greater persistence stemming from longer periods of relative prosperity. Some jarring exceptions must be noted, though: the severe 1973–75 dip stemming from the interaction of supply shocks and the resultant inadvertent (and apparently not greatly understood) contribution of inflation to effectively tight fiscal and monetary policy, and the also severe 1981–82 recession, again the result of the impact of inflation in creating largely unrecognized fiscal as well as monetary tightness.⁵

When you shake down the facts DeLong and Summers present and discount some of their imaginative but uncertain inferences, you may find their story not that inconsistent with mine.

5. See Eisner and Pieper 1984.

Comment Herschel I. Grossman

In the forty years since World War II, fluctuations in aggregate economic activity on average have been strikingly smaller than during the preceding fifty years. DeLong and Summers calculate various measures of this change, critically evaluate a variety of previously suggested explanations, and propose the unconventional hypothesis that nominal wage stickiness mitigates aggregate real fluctuations.

The only standard explanation to which DeLong and Summers give credence is the effect of fiscal arrangements—especially increases in both income tax rates and income maintenance expenditures—in reducing the aggregate marginal propensity to consume out of income and the demand multiplier. DeLong and Summers usefully stress that this effect results from the importance of liquidity-constrained consumers in determining the aggregate marginal propensity to consume and that the reduction in the multiplier probably has involved both a reduction in the aggregate response of disposable income to income and a reduction in the proportion of liquidity-constrained consumers.

DeLong and Summers reject, with good reason, explanations for the decreased magnitude of fluctuations based on changes in the composition of economic activity and on the absence of financial panics. They also reject the explanation that “discretionary stabilization policies” have been more efficacious, but here their argument seems largely semantic. They implicitly define discretionary stabilization policies as attempts to reduce the persistence of disturbances to aggregate activity, and they observe, interestingly, that persistence actually has not decreased in the post-World War II period. This observation, however, has no apparent relevance for determining whether aspects of policy other than the fiscal arrangements mentioned above have contributed to the reduced magnitude of aggregate fluctuations. The analysis they present leaves this general question largely unanswered.

In this regard, one of the weaker arguments in the paper is the attempt to dismiss the importance of decreased volatility of monetary aggregates in the post-World War II period. The distinctions DeLong and Summers stress between exogenous and endogenous changes in monetary aggregates and between monetary policy as a cause and an effect of fluctuations in real aggregates are both false and irrelevant. Monetary policy, like all purposeful human action, depends on prior events, but at the same time both the form of this dependence and the policy actions themselves can influence other events—past, present, and future. The relevant question is whether in the post-World War II period the re-

lation of monetary policy to the factors that influence it has changed in such a way as to produce both a reduction in fluctuations in monetary aggregates and a consequent reduction in the magnitude of fluctuations in real aggregates.

Much existing theoretical and empirical analysis suggests a positive answer to this question. DeLong and Summers do not pose this question directly or address the standard arguments. Their suggestion that the demonstrated unimportance of financial panics implies the unimportance of monetary policy hardly deals with the critical issue of the importance of the process generating monetary policy and does not even follow from their own analysis, which suggests that the unimportance of financial panics resulted from arrangements that stabilized monetary aggregates.

The most unusual idea DeLong and Summers present is that the magnitude of aggregate real fluctuations is inversely related to the degree of wage flexibility. To derive this hypothesis, they begin by pointing out that in the textbook IS-LM model aggregate output depends positively on both real money balances and inflationary expectations. Thus, assuming that the price level is positively related to the nominal wage level, a smaller decline in the nominal wage level in response to a negative demand disturbance would mean a smaller real money balance, which by itself would cause a larger decline in aggregate output. The net change in aggregate output, however, would also depend on the effect of a smaller decline in the nominal wage level on inflationary expectations.

DeLong and Summers append to this model a wage setting mechanism according to which a decrease in the fraction of workers whose nominal wages are adjusted each period—a presumed consequence of the apparent lengthening since World War II of the average duration of wage agreements—causes both the current nominal wage level and rationally expected future inflation to decrease less in response to a negative demand disturbance. DeLong and Summers argue that, for certain parameter values, the effect of the smaller decrease in expected inflation would more than offset the effect of the smaller increase in real money balances and yield a smaller decline in aggregate output. Although their illustrative parameter values are plausible for small changes, they probably would not remain constant as money balances became large. The combination of Pigou and Keynes effects associated with real balances should dominate in the limit.

DeLong and Summers point out that an essential characteristic of their wage mechanism is that it is backward looking. One implication of this mechanism is that workers are universally concerned not with their wages relative to the prices or other wages that will materialize over the term of their wage agreements, but with their wages over this

term relative to the history of other wages at the time of their wage negotiation. DeLong and Summers do not attempt to rationalize this aspect of their model.

Their setup, moreover, involves the even stronger assumption, which is critical but which they do not recognize explicitly, that current nominal wage changes depend as much on wages that were set in the distant past as on wages set in the recent past. This assumption implies that the importance of recent nominal wage changes for current nominal wage changes is negatively related to the average length of wage agreements. This effect creates the positive relation between the fraction of workers who negotiate lower nominal wages now and the expected future change in the nominal wage level.

DeLong and Summers judiciously regard the implications of this analysis as merely suggestive. Perhaps not surprisingly, they report no success in fitting the data to a structural representation of their model. As an alternative, they try to interpret the results of a vector autoregression involving aggregate output, the price level, and a nominal interest rate as supporting their hypothesis. The main empirical findings to which they appeal are positive effects of price level innovations and lags of the price level on current aggregate output. Contrary to their interpretation, however, we can take these results, together with the positive relation between current output and past output, merely to be indicative of the importance and persistent effects of demand disturbances.

The position DeLong and Summers take regarding the data suggests the following analogous argument: The amount of damage from a storm is positively correlated with the amount of swaying of buildings during a storm; therefore we could reduce storm damage by making buildings more rigid.

Although DeLong and Summers focus on the decreased post-World War II magnitude of fluctuations in aggregate economic activity, other notable characteristics of macroeconomic fluctuations also have changed since World War II. As DeLong and Summers also point out, fluctuations seem to have become more persistent. Another difference that possibly reflects a fundamental change in the economic structure involves the cyclical pattern of aggregate activity and inflation. For example, before World War II, inflation and unemployment almost always moved in opposite directions. Moreover, for a given inflation rate, unemployment tended to be higher when unemployment was decreasing than when it was increasing. In contrast, since World War II, this tendency has been dramatically reversed. In addition, inflation and unemployment have moved in the same direction for extended periods. The question naturally arises of the relations among all these changes. The characteristics of convincing explanations for what we

observe may become clearer if we try to keep the entire factual picture in mind.

Discussion Summary

Martin N. Baily commented on the relation between his own work and the DeLong/Summers paper. In 1978 he had argued that stabilization policy not only had a direct effect on the economy, but also induced a reinforcing response from the private sector, making consumption and investment less volatile in response to cyclical output movements. Contrary to a statement by DeLong and Summers, he had stressed the importance not only of discretionary policy, but also of the automatic stabilizers and the commitment by the Federal Reserve to avoid wild swings in the money supply. In the same paper he had also pointed out the destabilizing effect of price flexibility through its impact on real interest rates—a major feature of the current DeLong and Summers paper. Baily also argued that the existence of high serial correlation in output data is not evidence against the hypothesis of reinforcing response from the private sector. For example, the long sustained recovery after 1961 must have resulted in very high serial correlation in output data, but this was just the kind of period that strengthened the belief of the private sector in the stability of the economy and reduced the volatility of private expenditure decisions.

Robert Barro took exception to the paper's assertion that the multiplier had fallen. He claimed that the military spending multiplier had always been less than one and had not changed in the postwar era. Robert Hall agreed with Barro that the point estimate of the multiplier may have remained constant, but he noted that the standard error of the multiplier estimate was very large.

Moses Abramovitz observed that the most striking contrast between the prewar and postwar periods was that there had been no one really large depression. He hypothesized that the lingering memories of the Great Depression were an important factor in changing the character of the business cycle in the postwar period. Phillip Cagan recalled the 1949 NBER conference findings of Robert A. Gordon, that severe and moderate cycles differed in character. He maintained that since we have had no severe cycles since the war—owing to the lack of monetary panics—the character of the remaining postwar cycles may not differ from comparable prewar cycles.

John Taylor contrasted the role of prices in the model with his own paper, in which he avers that inflation control by the government is responsible for the increased persistence of output fluctuations. He felt that the omission of policy endogeneity from the analysis lost the main reason for the deterioration of economic performance.

Robert Gordon noted that the observation of the role of disposable income in business fluctuations was not new; it had reached the level of textbooks some years ago and had been discussed by Hickman and Coen in 1976 (See the reference in this volume's Introduction). He also pointed out that whereas the paper had enumerated three channels of influence of price changes on demand, the Keynes, Pigou, and expectations effects, there was one more—the redistribution effect (discussed in Fisher's 1933 article on debt and deflation).

Lawrence Summers doubted that the effects of the Great Depression would have lasted the entire postwar period. He also felt that comparing mild prewar cycles with all postwar cycles was not very meaningful, since one could always select *some* prewar cycles that were similar to postwar cycles.

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