

Comment John B. Taylor

Bradford De Long's paper is a wonderful read. It starts with a convincing demonstration of the historical significance of the 1970s inflation (the great inflation), documenting its long duration, its multinational dimension, and its probable lasting effect on the future course of economic policy and history. As the 1970s fade into the past—already today's college freshmen have no direct memory of this period—it is valuable merely to record these events and the lessons to be drawn from them. Monetary theory—more so than any other branch of economics—needs this type of history to supplement our understanding of how policy affects the economy. The paper brings this history alive with juicy quotes from both the economists and the politicians who made economic policy during this period.

De Long not only documents the history of the great inflation, he examines its causes. He concludes, and I agree, that the "price shocks" of the 1970s were not the cause of the inflation; in fact, the inflation was already under way before 1972 when the oil price shocks began. To this I would add that the oil price

John B. Taylor is professor of economics at Stanford University.

shocks of the late 1970s had very small inflationary effects in Japan after a much less accommodative monetary policy was put in place.

De Long also apparently rejects modern time-inconsistency arguments as an explanation of the great inflation. The rejection is implicit because he completely omits any discussion of the subject. Surprisingly, he does not even mention the well-known time-inconsistency work of Barro and Gordon (1983) or Kydland and Prescott (1977), which may be the most frequently cited reason why monetary policy led to excessively high inflation. Is De Long correct in dismissing this argument out of hand?

In fact, the time-inconsistency model does have the potential to explain the great inflation, as argued by Parkin (1993). In the basic Kydland-Prescott model of the inflation/unemployment trade-off, the "suboptimal" consistent policy (or what Barro and Gordon call the discretion policy) is assumed to be the long-run equilibrium inflation rate and unemployment rate. There is an important theorem about this suboptimal equilibrium: the higher the natural rate of unemployment is, the higher the equilibrium inflation rate is.

Parkin uses this theorem to explain the 1970s inflation in the United States by noting that the natural rate of unemployment rose in the 1970s, as the young postwar baby-boom generation entered the workforce, and declined in the 1980s as the baby-boom generation aged. Hence, the time-inconsistency model implies that the equilibrium inflation rate should have risen in the 1970s and fallen in the 1980s, just as the actual inflation rate rose and fell. I have questioned the Parkin explanation (Taylor 1993b) on the grounds that the time-inconsistency model is not persuasive as a positive economic theory in the case of the inflation-unemployment trade-off, because people would see the suboptimality of the equilibrium and attempt to fix it with laws or other social arrangements. But even if one finds the time-inconsistency model persuasive in this case, the Parkin explanation fails another important test; in particular, it does not explain why inflation also rose and then fell in Europe where the natural rate of unemployment kept rising throughout the 1980s. Hence, as my brief summary indicates, De Long is probably right to reject time inconsistency as an explanation of the great inflation.

De Long argues that the main reason for the great inflation—the "truest" cause—was the memory of the Great Depression itself and the deep fear people had of a return to high unemployment. In other words, he argues, policymakers and the public were willing to let inflation rise because, having recently experienced the high unemployment of the 1930s, they worried that maintaining price stability would lead to greater unemployment.

I have doubts about De Long's explanation. If the experience of the Great Depression caused Americans and their political leaders to sacrifice the goal of price stability in the late 1960s and 1970s, then why did monetary policy leave the price level so nearly stable during the 1950s and early 1960s—a period much closer to the Great Depression and nearly as long? We should have seen the inflation rate rise much earlier. The timing is off in De Long's

story. True, as De Long argues, the great inflation may just have been an accident waiting to happen, but I think there are more explicit factors that must have played a role.

In my view the development by economists and the adoption by policymakers of new macroeconomic ideas in the 1960s (the New Economics) deserves much of the credit, or blame, for the great inflation. The ideas were intellectually exciting, carefully explained, and widely disseminated; and the timing was just about perfect to explain the events.

First was the idea that there was a long-run Phillips curve, which appeared in the *Economic Report of the President* (for example, 1969, 95) and many textbooks, and which was widely discussed by the media. This idea indicated that the cost of an overheated economy would simply be higher inflation, rather than accelerating inflation.

Second was the view that the "full-employment unemployment rate" (what we would now call the natural rate) was 4%, and perhaps even lower. Although there was little evidence for this low figure at the time, it was put forth by many economists, including the Council of Economic Advisers (CEA), and it became widely accepted and difficult to change. As late as 1976 when a different CEA revised the estimate to 4.9%, they were widely criticized by politicians and the public for doing so (*Economic Report of the President* 1977). I recall that when Alan Greenspan and Burt Malkiel testified before the Joint Economic Committee about their CEA's upward revision, they were lambasted by Senator Hubert Humphrey. That their estimate did not quite hit 5% may be indicative of their concern about confronting too directly the persistent and strongly held views about the 4% estimate held outside of economists' circles.

This low estimate of the natural rate and the notion of a long-run Phillips curve trade-off led politicians to a certain fearlessness about using monetary policy to overstimulate the economy. For example, President Johnson was driven by his desire to put "easy money" people on the Federal Reserve Board. According to Joseph Califano in the "Guns and Butter" chapter of his *Triumph and Tragedy of Lyndon Johnson* (1991, 109), Federal Reserve Board chairman Martin "was threatening to resign if Johnson put another liberal on the Board." Califano then goes on to explain how, nevertheless, Johnson managed to find yet another Federal Reserve Board candidate, who the president was convinced had good "easy money" credentials, and then make this appointment to the board despite Martin's strong misgivings.

A counter to this argument about the influence of the long-run Phillips curve is that as early as 1968 Milton Friedman and Edmund Phelps were explaining that there was no such thing as a Phillips curve; excessive monetary expansion which temporarily brought unemployment below the natural rate would lead to *accelerating* inflation. However, at least in its early years, the Friedman-Phelps accelerationist model appears to have had little practical influence in leading to greater price stability. What the accelerationist model did, in my view, was transform analysis based on the old-fashioned Phillips-curve model,

which had already led to higher inflation, into an analysis showing that the costs of disinflation were so great that we should either not reduce inflation or we should do so incredibly gradually. For example, as late as 1978, in a *Brookings Papers on Economic Activity* issue entitled "Innovative Policies to Slow Inflation," George Perry (1978) showed that it would require 10% of GDP to reduce inflation by 1%. Pessimistic estimates such as these undoubtedly affected policymakers' thinking.

In the 1974 White House *Economists Conference on Inflation* with President Ford, virtually all the distinguished economists bemoaned the extraordinarily high costs of inflation reduction. Because of these costs Paul Samuelson and Walter Heller emphasized that perhaps inflation was not much of a problem. As Walter Heller stated at the conference, "in bringing inflation to its knees, we will put the economy flat on its back" (128). And Samuelson argued eloquently that we do not need a Winston Churchill-like "blood, sweat, and tears" program to reduce inflation (71). Among the economists at the conference only Milton Friedman argued unequivocally for inflation reduction: the "strength [of the U.S. economy] is currently being eroded by the disease of inflation. If that disease is not checked it will take a heavy toll including, in my opinion, the very likely destruction of our personal, political and economic freedoms. . . . I heartily applaud, also, the expressed determination of the Federal Reserve to slow monetary growth . . . despite the cries of anguish about this table and elsewhere about tight money, the slowing has so far lasted two or three months so we cannot yet be sure the Fed has really departed from the ever more inflationary path it has been following for the past decade" (122-23).

But Milton Friedman was the exception. The more common view among economists throughout the 1970s was that it was hardly worth the high costs to reduce inflation, and this view was based on the expectations-augmented Phillips curve, not simply the original Phillips curve.

In my view, the introduction of rational expectations as a model of the expectations term in the Phillips curve was ultimately influential in changing views both about the costs of reducing inflation and the costs of inflation itself. Thomas Sargent and Neil Wallace's striking estimate (1975) that the costs of disinflation were essentially zero for a credible policy certainly got people to think about alternative views. My own estimate made in the late 1970s (which incorporated both sticky prices and rational expectations) found that the disinflation costs were 60% smaller than George Perry had reported (see Taylor 1993a).

But whatever its source, the realization that the costs of disinflation might be smaller than the most dire warnings coupled with the clear dislike by the general public of inflation ultimately led to the end of the great inflation orchestrated by Paul Volcker at the Fed. Jimmy Carter and his advisers get credit for appointing Volcker to the Fed, and Ronald Reagan and his advisers get credit for helping to maintain the Fed's disinflation resolve through the early 1980s.

Ronald Reagan's explicit support for the Fed's price-stability goals in 1982 even when unemployment was high and the midterm elections approached (see Martin Feldstein's retrospective [1994]), contrasts sharply with Lyndon Johnson's attitude toward inflation in the late 1960s as reported by Joseph Califano. Hence, the fifteen-year cycle of macroeconomic opinion corresponds closely with changes of opinion of the top national economic policymakers as well as with the timing of the rise and fall of the inflation rate, that is, with both the great inflation and the great disinflation.

In my view, these changing economic theories and opinions about inflation are the ultimate cause of the changes in actual inflation. At the least this view provides a more complete explanation of the timing of the event than the "accident waiting to happen" view put forth in De Long's excellent history of the times.

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