Macroconfusion: The Dilemmas of Economic Policy

American macroeconomists are in disarray. Like a shell-shocked army, barraged by criticism because of poor forecasts, wearied from fruitless battles with chronic inflation, confused because of divided intellectual leadership, they are unsure which way to retreat. Out of the ashes of defeat rises a new phalanx of competing theories, a ragtag collection of discarded ideas from the past as well as unproved fancies for the future.

In this period of reconstruction, the time has come to assert the superiority of the earlier, too quickly discarded views. I believe that the intellectual consensus of the late 1960s was basically sound. The synthesis of Keynesian and neoclassical economics—the "neo-Keynesian synthesis" for short—although oversimplified, is the best way to understand the puzzles of the economy as well as the dilemmas of policy. The neo-Keynesian synthesis is in critical condition today, not because it is flawed, but because it has too often been on the losing side of the battles against inflation and unemployment. The new phalanx of theorists—monetarists, supply siders, rational expectations, "deficitists," goldbugs, and constitutionalists—have contributed little to resolving the dilemmas of economic policy. They only provide diversion from the real task of economic policymaking.

I sometimes wonder what Arthur Okun's view of the rise of the new army of macroeconomic theories would be. He remained a reconstructed Keynesian to the end. In his last paper, he criticized the rational expectations view as failing to explain many of the key features of the business cycle. His last book dismisses supply-side economists with one sentence in a footnote on page 353: "Their position simply cannot be taken seriously." I suspect Okun would have taken these theories increasingly seriously—as political happenings. But his rigorous demand that theory be consistent with reality would, I am sure, have left him untouched by their fanciful prescriptions.
The Central Problems for Economic Policy

The central problems for macroeconomic policy in the 1980s, while changed in nuance, are those of the 1970s—slow productivity growth, chronic inflation, high unemployment, and high vulnerability to volatile oil and foreign exchange markets remain the most important and durable issues. Contrary to much public discussion, we do not have a soaring budget deficit, public debt, or a runaway public sector. The task of macroeconomic theory is to understand the linkages between policy instruments and major economic problems so that policymakers can steer the economy in sensible directions.

The principal goals of macroeconomic policy are rapid growth in income, output, and consumption; high employment; price stability or low inflation; and external balance. As is shown in table 1, economic performance over the last decade has been depressing. In short, the goals have not been attained in the United States, or elsewhere.

As can be seen in the table, the last few years have witnessed a deterioration in all the major indexes of macroeconomic activity. Real growth of output, income, and consumption declined from one-third to one-half. Unemployment rates rose 2 percentage points. The inflation rate tripled, and the terms of trade deteriorated considerably after more than a decade of stability.

Having briefly suggested that the body economic is in critical condition—which few today would contest—I observe today little consensus about the diagnosis. There is deep division over the precise cause of the country’s economic maladies. Was economic policy responsible for driving the economy off the road? I believe that the deterioration in economic performance did not result mainly from economic policy errors of the past.

It is useful to clarify what I mean by acquitting economic policy of responsibility for the current economic mess. It is not to deny, for example, that by ruthless anti-inflation policy chronic inflation could have been kept at a much lower level. But given the economic costs of erasing chronic inflation, it would not have been sensible economic policy to do so. In technical language, it is unlikely that an ex ante optimal macroeconomic policy would have improved markedly a reasonable objective function when taking into account the actual constraints under which the economy was operating in the 1970s.
Table 1. **Measures of Economic Performance in the United States, 1960–80**

<table>
<thead>
<tr>
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<tbody>
<tr>
<td>Growth rate*</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Real GNP</td>
<td>4.2</td>
<td>2.4</td>
</tr>
<tr>
<td>Real consumption</td>
<td>4.2</td>
<td>2.8</td>
</tr>
<tr>
<td>Real national income*</td>
<td>4.5</td>
<td>2.1</td>
</tr>
<tr>
<td>Unemployment rate*</td>
<td>4.9</td>
<td>6.8</td>
</tr>
<tr>
<td>Inflation rate (CPI)*</td>
<td>3.2</td>
<td>9.2</td>
</tr>
<tr>
<td>Change in the terms of trade*</td>
<td>−0.1</td>
<td>−4.1</td>
</tr>
</tbody>
</table>


- a. Growth rates are geometric averages, percent per year.
- b. National income deflated by the consumption deflator.
- c. Annual averages.
- d. Ratio of implicit price of exports to implicit price of imports, 1960 = 100.

Two items can be used to illustrate the relative innocence of economic policy in the current economic mess, one concerning inflation, the other productivity growth.

An oft-repeated complaint about economic policy is that it has left the United States with a heritage of high inflation. In a recent study Otto Eckstein decomposes inflation into core, demand, and shock components.\(^1\) He estimates the contribution each of these components made to the acceleration of inflation from 1960 to 1979. When all the demand shocks in this period are added up, the total contribution to inflation is minus 0.7 percentage point. In view of this result, it is hard to see how anyone could conclude that excessively expansionary policies were responsible for the acceleration of inflation over the last two decades.

A second myth concerning economic policy is that the slow productivity growth in the United States and abroad has been due to successive bouts of self-inflicted wounds. The most prominently mentioned problem is discouragement of capital formation. It is claimed that stop-go policies, high inflation, high taxes, loose money, tight money, and burdensome regulation have significantly weakened the incentive for investment.

Evidence on the role of disincentives can be obtained by examining international trends in investment behavior. The Organization for Economic Cooperation and Development has collected data on capital stocks and other determinants of productivity in major countries for the years 1960, 1973, and 1978. The results are shown in table 2.

The first column indicates the estimated share of pretax profits in

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Table 2. Contribution of Slowdown in Capital–Labor Ratio to Labor Productivity, 1960–73 and 1973–78

<table>
<thead>
<tr>
<th>Country</th>
<th>Pretax share of profits in GDP (1)</th>
<th>Change in annual growth of capital–labor ratio (2)</th>
<th>Contribution slowdown to slowdown (3)</th>
<th>Actual slowdown (4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Canada</td>
<td>0.34</td>
<td>-0.5</td>
<td>+0.2</td>
<td>-2.1</td>
</tr>
<tr>
<td>France</td>
<td>0.37</td>
<td>+1.0</td>
<td>+0.4</td>
<td>-1.7</td>
</tr>
<tr>
<td>Germany</td>
<td>0.33</td>
<td>-0.4</td>
<td>-0.1</td>
<td>-1.0</td>
</tr>
<tr>
<td>Italy</td>
<td>0.25</td>
<td>-2.4</td>
<td>-0.6</td>
<td>-4.3</td>
</tr>
<tr>
<td>Japan</td>
<td>0.31</td>
<td>-3.4</td>
<td>-1.1</td>
<td>-5.6</td>
</tr>
<tr>
<td>United Kingdom</td>
<td>0.29</td>
<td>-0.5</td>
<td>-0.1</td>
<td>-2.4</td>
</tr>
<tr>
<td>United States</td>
<td>0.29</td>
<td>-0.6</td>
<td>-0.2</td>
<td>-1.9</td>
</tr>
</tbody>
</table>


a. Taken to be the elasticity of output with respect to capital.
c. Column 1 times column 2.
d. Output per employer, nonfarm business sectors.

gross domestic product (GDP)—conventionally taken as a good estimate of the elasticity of output with respect to capital services. The second column shows the acceleration or deceleration of the capital–labor ratio from the 1960–73 period to the 1973–78 period for each of seven industrial countries.

Multiplying column 1 by column 2 gives, in column 3, the growth-accounting estimate of the slowdown in labor productivity that should have come about because of the slowdown in the growth of the capital stock. This estimate is either of the wrong sign or very small in five of the countries, and above the noise levels only in Italy and Japan. But the major conclusion is clear: by the conventional analysis, in no country could the slowdown in investment and capital formation plausibly be a major part of the productivity slowdown. Indeed, in no country is the estimated contribution of capital more than one-fifth the size of the productivity slowdown.

While crude, these calculations give the same qualitative answers as the more careful estimates for the United States. In a review of recent studies of productivity behavior, I concluded that perhaps one-fifth of the slowdown in productivity in the United States could be attributed to economic mismanagement. It is a puzzle, best left to the political scientists, how so small a factor can have become the major popular explanation for the slowdown.
So I conclude that some of the claims about the failure of economic policy are groundless. This is not saying much. You don't get a medal for good driving by making it around the block without a crash, but at least you stay out of jail.

Of course, even agreement that policy played a relatively unimportant part in the dismal performance of the 1970s provides little guidance about the appropriate role for policy in the 1980s. Appropriate policy will depend more on which of the shards of the fragmented consensus one examines. The next two sections review some of today's theories, and the final section attempts to provide some prescriptions for economic policy.

**The Fragmented Consensus**

Since the central paradigm of macroeconomics today is the neo-Keynesian synthesis, I will first outline its elements, with particular attention focused on elements that are central to economic policy, and then compare it with the major competing paradigms—monetarism, rational expectations, and supply side views.

It is obviously impossible to summarize the complex body of neo-Keynesian thinking in a few pages. In what follows I will concentrate on the aspects most clearly related to economic policy. These are the distinction between, as well as the determination of, actual and potential output; the role of monetary and fiscal policies in the determination of output; and the division of the growth of nominal output between prices and quantities.

The first element in the neo-Keynesian synthesis is the distinction between actual and potential output. Actual output is whatever is produced in a given period. Potential output is what the economy could produce if resource utilization were at a high or benchmark level—today taken as a 5 percent unemployment rate for labor. It is not an oversimplification to think of actual output as "demand" and potential output as "supply"; and further to regard the forces determining supply and demand as quite distinct, acting with quite different time lags. One of the central elements of the neo-Keynesian synthesis—clearly laid out in the 1962 Economic Report of the President, but since then often forgotten by policymakers—is that both the demand and supply sides of the economy require attention from economic policy. But the kinds of
policies that affect the two are very different, and there is only a weak
link between actual and potential output, particularly in the short run.

The need to keep an eye on improving the performance of both actual
and potential output has proven a rigorous requirement. Central eco-

demic policy treatises of the 1970s—the McCracken report, Okun's
Prices and Quantities; most issues of the Economic Report of the
President—largely ignore the problem of increasing potential growth. If
there is any justification for "supply-side" criticism, it lies in the
tendency of Keynesian thinking in the 1970s to forget the lessons of
growth theory of the 1960s.

It should be noted in passing that the intellectual foundation of the
distinction between actual and potential output has never been well
articulated from a theoretical point of view. Its roots lie in the "fix-
price" view of the world, that is, one in which prices and nominal wages
are viewed as largely exogenous in the short run. The distinction would
not make much sense in a "flex-price" world, where all markets are
auction markets like corn or silver. In the flex-price world the short-run
outcomes closely approximate a competitive outcome, and there is little
reason to think output would be far from the level of output that would
be produced by a competitive economy with auction markets ("ideal
output" for short). In the fix-price world, in my view, output is often
far from ideal and probably has a secular bias below the ideal output. In
this case, if potential output is in the neighborhood of ideal output, the
gap between potential output is a measure of the deviation of actual from
ideal output. (Because of asymmetries in fix-price markets, ideal output
may even be considerably above potential output.) The infirm founda-
tion of fix-price behavior has been pounced on by critics from the rational
expectations school and will be returned to below.

The second feature of the neo-Keynesian synthesis relates to the
determination of potential output. In current thinking potential output is
determined in a way that is best described by neoclassical growth theory.
That is, output is determined by a production function with labor, capital,
energy, and other material inputs. This production function is often
described as exhibiting constant or modestly increasing returns to scale
and having a variable rate of technological change.

to the Organization for Economic Cooperation and Development (Paris: OECD, 1977); Arthur M. Okun, Prices and Quantities: A Macroeconomic Analysis (Brookings Institu-
Assuming that the rate of technological change is exogenously given, potential output growth is determined by the growth of factor inputs. Policy affects potential output growth chiefly by raising or lowering the rate of formation of human or reproducible tangible capital.

This feature of the neo-Keynesian synthesis is not subject to much debate by the critics. With the minor provisos discussed in the final section of this paper, the view of the growth of potential just outlined is shared by all the major schools of thought reviewed here. Indeed, sometimes other paradigms accept the neoclassical growth model as applying to the short run as well as to the long run.

One of the major findings of empirical economic growth theory, however, is the great difficulty of increasing the rate of growth of potential output by policy. (This is a corollary of the earlier proposition that policy has little to do with the productivity slowdown.) Edward Denison estimated that a large increase in private net investment (raising it one-quarter above what it would otherwise be) would raise the growth of potential output only 0.1 percentage point a year. Given this very modest response of potential output to policy, it may be understandable that policymakers, particularly those with short time horizons, have generally ignored the goal of increased potential output and focused instead on stabilization policy.

The short-run determination of actual output is the major difference between the new paradigms today. The differences arise from views about the determination of nominal GNP and views about how nominal GNP is split between prices and quantities.

The view of the neo-Keynesian synthesis of the determination of the level of actual output has changed little since the 1930s—though it has been refined and given considerable empirical flesh. In this view output is basically determined by aggregate spending, as in the Hicksian IS-LM curve. Of course, reality and the embodiment of this vision or reality in large-scale econometric models are much more complicated than the simple IS-LM framework, but the increased realism of the 1,000-plus equation econometric models mainly adds to the distinction between the impacts of different taxes or financial policies and better determination of the time lags. With the exception of the greater power currently given to money, there appear to be no major differences between the behavior

of the large models today and that of the earliest econometric Keynesian models.

The best way of summarizing the beliefs of the neo-Keynesian synthesis is by examining simulations of the major models—the DRI, Wharton, Chase, MIT-Penn-SSRC models. From model simulations and comparisons, the major features of the neo-Keynesian synthesis models emerge, as follows: fiscal policy appears to have substantial impact on actual output, at least in the short run, and the multipliers do not differ much among major models. Monetary policy also has substantial effects on output, but the money multipliers differ enormously among econometric models. Thus both money and fiscal policy matter, but the uncertainty is much greater for the former than the latter.

The final important feature of the neo-Keynesian synthesis concerns the split of impulses to demand between output and prices. In other words, what is the view of aggregate price determination? No issue has produced more intellectual turmoil among macroeconomists than inflation theory; and the evolution of thinking from the 1930s to today is considerable.

It seems a reasonable approximation to say that some early Keynesian thinking held prices and wages to be approximately constant up to the point where the economy hit full employment. Today the view is quite different. Inflation is taken to be the sum of inertial, cyclical, and volatile or random forces. The inertial element is the inherited "underlying" rate of inflation, particularly from wages, which changes slowly in response to experience and expectations. Cyclical elements include a very modest response of wage inflation to unemployment as well as some response of markups and material prices to the cycle. Volatile forces include such elements as oil and food prices, as well as the effects of interest rates.

In the view of the neo-Keynesian synthesis, inertial or chronic inflation poses one of the most difficult problems for economic policy. This is because chronic inflation is extremely costly to erase, while the benefits of lower inflation are subtle. According to Okun’s calculations of the short-run trade-off between unemployment and inflation, but with the use of an up-to-date Okun’s Law coefficient, it would cost two-thirds of one year’s GNP to lower chronic inflation by 10 percentage points. The high cost of reducing inflation, or the stubbornness of the inertial element in inflation, arises because inflation is so firmly embedded in our institutions in formal and informal contracts.
Put differently, a shock that lowers spending has its major short-run impact on output. Evidence indicates that around 90 percent of the first-year response to a spending shock shows up in output, while 10 percent is in prices. As the time period lengthens, this split changes, moving more toward price response and away from output response. Although econometric evidence is obviously unavailable, it seems likely that after several decades all the response is in prices. These numbers are, it must be emphasized, not known with certainty, nor are they independent of space, time, or expectations. But the evidence for the United States is that the short-run division of nominal demand shocks between quantities and prices is closer to 90–10 than the 10–90 or 0–100 envisaged by the other paradigms.

**Alternative Paradigms**

It will be useful to describe briefly the major alternative schools of thought that have affected thinking about economic policy in the United States. It should be emphasized that the summary below, as for the neo-Keynesian synthesis, cannot fairly represent the full richness of these theories. Moreover, I have emphasized only those aspects of the theories that relate to economic policy.

**Monetarism**

Monetarism is a venerable doctrine going back for centuries. It is, in my view, best interpreted as a special case of the neo-Keynesian synthesis. Monetarists accept the distinction between actual and potential output, as well as the view of the determination of potential output of the neo-Keynesian synthesis. The major difference lies in the view of output determination and the inflation process.

In the strict monetarist view, money velocity is interest-inelastic, so nominal GNP is determined by the money stock (although the definition of "the" money stock is quite volatile). In the standard Hicksian framework, such a proposition can be interpreted as a vertical LM curve. Fiscal policy affects the composition but not the level of nominal GNP. The money multiplier is large and stable, while the fiscal multipliers are zero. Today, most monetarists have backed off from the extreme view of the insensitivity of velocity to monetary and fiscal policy of earlier
periods. The fallback position is sometimes "constitutional monetarism" and sometimes the "new classical macroeconomics."

The second major aspect, perhaps less generally agreed upon by monetarists, is their view of the inflationary process. Along with all the other non-Keynesian paradigms adhering to the Walrasian conception of markets, monetarists believe that prices adjust relatively rapidly to demand or supply shocks. Thus any shock to aggregate demand ends up mainly in price shocks rather than in output shocks.

While they are optimistic compared to most neo-Keynesians, monetarists still diverge widely on the costs of disinflation. In testimony before a British select committee on monetary policy, Milton Friedman stated that he thought there would be virtually no loss of output from a program of monetary restraint, and David Laidler provided an estimate of the response of inflation to slack ten times greater than that cited above. On the other hand, work by Philip Cagan and Jerome Stein provides estimates that are from two to four times more optimistic.

The two basic propositions of monetarism—interest-inelastic demand for money and quick price adjustment—have received scant empirical support in most careful structural statistical studies. Once an exogenous velocity is abandoned, however, it becomes virtually impossible to distinguish the implications of monetarism from those of the neo-Keynesian synthesis.

New Classical Macroeconomics

A second major school of thought today is the rational expectations or new classical macroeconomics (NCM) view. This view has been developed by Robert E. Lucas, Jr., Thomas J. Sargent, and Neil Wallace over the last ten years.

The NCM school is based on two central premises. The first, and less controversial, is that economic agents form expectations on the basis of all available information. This premise has been a provocative tool for challenging established techniques for modeling expectations. It has led,

for example, to much better understanding of why financial markets appear to behave perversely—why "good news" looks like "bad news." It has also led to understanding of why "unstable" structural equations are to be expected in, say, price and wage behavior.

The second, more controversial premise is that all markets clear in the very short run, that is to say, prices are perfectly flexible. This premise is more an assumption than an empirical finding; moreover, it is at variance with considerable empirical work on actual price and wage behavior.

These two assumptions provide a rich set of propositions concerning behavior and policy. An early result—outdoing monetarist thinking—was to suggest that the Phillips curve is vertical in the short run as well as in the long run. A more general result was the "policy ineffectiveness theorem," which states that anticipated policies affect only prices, not real output.

One way of interpreting the NCM view is that it accepts the long-run but not the short-run half of the neo-Keynesian synthesis. That is, it views the economy as in neoclassical equilibrium, though subject to random shocks. According to this interpretation, the NCM view would share the prescriptions concerning acceleration of the growth of potential output, but not those concerning short-run stabilization policy. Thus actual output never deviates from potential except when there are random shocks. The division of output between prices and quantities is at the extreme end of the spectrum, with 100 percent of anticipated changes in spending on nominal GNP going into prices. In this view, disinflation is an easy and costless process that simply involves an announced and credible reduction in aggregate demand.

The professional verdict on the NCM is still out. Given the dubious nature of the fundamental flexible price assumption, many of the policy prescriptions of the NCM have been widely and correctly viewed as elegant but irrelevant. Thus while the NCM school has been extremely influential inside the economics profession, it has been adopted reluctantly by practitioners. Perhaps the idea that policy cannot affect the real economy is as foreign to policymakers as random walk theories of stock prices are to stockbrokers.

Supply-Side Economics

Conceived on a cocktail napkin, carried by an ambitious ex-quarterback congressman, and midwifed by a skillful president, supply-side
economics burst upon the economic scene physically full-grown but intellectually dwarfed. In contrast to the other major paradigms, particularly the new classical macroeconomics school, supply-side economics is fundamentally a political inspiration without serious scientific support. In this respect it resembles the limits-to-growth movement of a decade ago.

The major tenet of supply-side economics is that economic activity responds quickly to relative prices, particularly to changes in tax rates, but that income effects are unimportant. Supply siders predicted that the reductions in the personal tax rates in the Laffer-Kemp-Roth proposal enacted in the 1981 Revenue Act would lead to greatly expanded supplies of labor and capital, and thus to rapid economic growth. Therefore, ignoring the pessimism engendered by the work of Denison discussed above, supply siders appear to believe that the growth in potential output can be readily increased.

Aside from this central tenet of the supply-side school, it is difficult to glean a comprehensive (or even comprehensible) view of economic policy. The major problem, apparent even in the central proposition, is the failure to distinguish between actual and potential output. Thus the supply siders like to point to the Kennedy tax cuts of 1964 and 1965 as evidence of the validity of their views. Yet the Kennedy cuts were designed to increase actual output—and they clearly did so—and were only incidentally aimed at potential output.

Does this indicate that supply siders are simply closet Keynesians, assuming a new mantle of respectability for revving up the economy? Not likely. A more plausible interpretation is that the supply siders have failed to grasp the analytical distinction between aggregate supply and demand.

Since they do not distinguish between actual and potential output, it is easy to understand why the supply siders have difficulty articulating a consistent view of the inflationary process. If markets clear instantaneously, as the new classical macroeconomists believe, inflation can be quickly erased. If, on the other hand, inflation persists because wage and price behavior is sticky, then a notion of excess demand or supply is necessary to provide a mechanism by which inertial inflation accelerates or decelerates. Without either the market-clearing or the sticky-behavior model, inflation seems completely ad hoc. I have not seen any of the major supply-side enthusiasts outline a theory of inflation. This lack of theory has recently been compensated for by a new bold proposal to lick inflation—a return to the gold standard.
Policy Dilemmas

Over the coming years five issues must be faced by policymakers. Almost all of them have been part of the internal dialogue of macroeconomics for decades. But the economic turmoil of the 1970s has made the dilemmas more painful and the trade-offs more intractable.

Economic Constitutionalism

A pervasive issue concerns the movement that imposes on economic decisionmakers stricter economic discipline, such as fixed monetary rules or constitutional amendments. I call this trend "economic constitutionalism." Examples of such a trend are legion. Perhaps the first was the congressional resolution that required the Federal Reserve to announce monetary targets. More recent are proposals for constitutional amendments on the budget balance, expenditure limitations, and money growth, as well as a number of more informal operating rules for the fiscal and monetary authorities.

From an analytical point of view, there are two reasons for economic constitutionalism. The first is the need for credibility. Assume that we accept the view that a credible disinflation policy would lead to little output loss. What we need to find is a cheap way of being credible. As Thomas C. Schelling has shown, the best way to establish the credibility of a decision is for the decisionmaker to put himself in a position where changing the policy would be extremely costly to him. Thus by announcing, legislating, constitutionalizing policies—why not shoot inconsistent politicians?—credibility can be enhanced.

If credibility is the key to better policies, however, why do we need to impose stricter discipline to be credible? I would generally expect optimal credible policies to be the same as optimal incredible policies (although countere xamples do exist). In this case the worrisome element in economic policies would be—and this is the second point—that the optimal credible policy differs from likely actual policy.

According to this second interpretation, economic constitutionalism is necessary because political leaders are perceived as untrustworthy. This perception has many roots. One of the most general is the well-documented decline in respect for authority, particularly of political figures. This has spilled over into the economic debate in the form of
distrust of discretionary political management of the economy. Another would be the failed promises of the "new economics" of the 1960s, the impression that hard on the heels of the belated but short-lived Keynesian revolution in U.S. macroeconomic policy came the economic disasters of the 1970s. Perhaps a third source would be growing political conservatism, part of which is the result of earlier "liberal" programs' lack of success.

Many of the most radical proposals for management of the economy—particularly the monetarism and "deficism" discussed below—can be interpreted as reflecting a profound mistrust in the institutions of American democracy. The movement for economic constitutionalism is sometimes rationalized as a retreat from "fine-tuning" the economy, but this is inaccurate. It is rather a desire to abjure all discretionary management of the economy.

Recent economic theory has begun to incorporate explicitly some formal theory of the interaction between political and economic forces, as in the theory of the political business cycle. This line of thought suggests the possibility that elected policymakers will manipulate economic policy in ways that exacerbate business cycles.

The revulsion against democratic policymaking among economists has generally led them to suggest adding legal constraints to the policy process. Two of the most popular doctrines suggesting constraint are monetarism and deficism, discussed further below. These are attempts to substitute suboptimal but nonmanipulable rules for manipulable but potentially optimal policies as a way of inserting backbone into spineless politicians.

Not to be outdone, the supply siders have suggested a different economic rule that will discipline policymakers' return to the gold standard. The rationale for this is that by returning to a "high-quality money" inflation will automatically (and painlessly?) cease. Without the discipline of the convertibility of the dollar into gold, it is argued, policymakers will be subject to the temptation to use inflation as a way of resolving political conflicts.

Not all procedural reforms are without intrinsic merit. There are other and more constructive uses of procedures to remove the defects of current institutions, particularly piecemeal decisionmaking. The Congressional Budget and Impoundment Control Act of 1974 is a way of ensuring that Congress acts on the budget as a whole rather than bidding up the total budget in small increments. A similar proposal has been suggested for regulation—the so-called regulatory budget.
What is the economist's judgment on constitutionalism? These approaches have both an economic and political component. From a purely economic point of view, however, it seems clear that the use of general rules, like a fixed money growth or a balanced budget rule, is at best a second-best solution to stabilizing the economy or promoting the appropriate balance between public and private sector. One academic defender has labeled strict monetary constraints as "the half-blind leading the blind." A more apt analogy is that economic constitutionalism represents the lame leading the sometimes wicked.

If we accept the view that political management of the economy will be subject to impure motivation and incomplete knowledge, real dilemmas arise in the optimal design of economic policy institutions. Surely there are better institutional arrangements, however, than imposing rigid rules with little economic justification. Economic constitutionalism also reflects a conservative view of the role of government, a view that much of government nondefense spending is wasteful. With roadblocks to slow government spending or prevent deficits, social programs are likely to wither on the vine. The acid test of whether these programs are simply procedural or reflect an underlying conservative stance lies in their treatment of national defense: most proposals have an "escape clause" that exempts defense from their stringencies—a sure sign of the philosophical origin of the idea.

Deficism

In 1863 a man from Las Vegas, New Mexico, was found guilty of having murdered a witch who had supposedly given him tuberculosis. In 1925 John Scopes was convicted of having illegally taught the theory of evolution. By 1980 thirty-one of the states had passed resolutions calling for a constitutional convention to impose a balanced budget on the federal government. What these events have in common is that they exhibit the triumph of scientifically unsupported theories over the accumulated evidence.

Many economists and policymakers are calling for a balanced federal budget to cure high interest rates, high inflation, and swollen government. The movement, which I call "deficism," is fundamentally flawed as an economic doctrine, for the federal deficit has major shortcomings both as an accounting measure and as a device for controlling the economy. The only serious intellectual support for deficism can be found in its use as an indirect constraint on the political process, as explained above in
the section on constitutionalism. But it must be emphasized that it is highly defective as a constitutionalist constraint.

The first defect of deficitism is its reliance on an imperfect instrument for controlling economic activity, this problem being similar to the reliance of monetarists on an endlessly evolving concept of the money supply. An examination of the January 1981 estimates for the 1981 federal budget illustrates the problems. The "official" budget deficit was estimated to be a frightening $55 billion. However, this figure excluded two sets of programs: off-budget entities like the Tennessee Valley Authority and interest subsidies on various loan programs. After correcting for these omitted programs, the deficit became $105 billion.

However, the official deficit does not correct for two standard accounting concepts, investment and capital gains. The projection for 1981 was that $143 billion of outlays would be investment-type activities, like filling the strategic petroleum reserve. Also, capital gains on the debt (or, in accountants' jargon, correction for the real value of monetary assets) reduced outlays by $78 billion. With these four corrections the federal government was estimated to run a surplus of $116 billion for fiscal 1981.

Of course, even this "corrected" federal surplus is an inadequate measure. But the point is that conventional measurement tools are highly imperfect, underestimating federal deficits just as they overestimate corporate profits. How can we seriously consider using as a control variable a tool whose conceptual and measurement uncertainty is on the order of 10 percent of GNP?

Deficitism also suffers from an inadequate grounding in economic theory, for deficits and surpluses per se play no direct role in attaining any of our major economic goals. The federal budget deficit itself does not enter into any of the major behavior equations of the economy—into the determination of inflation, aggregate demand, potential output, or interest rates. Rather, the level and composition of spending and taxes, as well as other off-budget programs, are the major channels by which the budget affects economic activity. And even for these variables the route by which taxes and expenditures affect the economy is almost wholly through their influence on aggregate demand or potential output. Only when one enters into much more complicated general-equilibrium models of financial markets can a separate influence of federal debt and its growth be found, and even here the sign of the effect is ambiguous.

In sum, the current emphasis on bringing the federal deficit under
control can be considered misguided at best, disingenuous at worst, but in the end irrational.

Monetarism

Above I discussed briefly the monetarist economic philosophy. I will be even briefer in an analysis of the monetarist policy solutions, mainly because there is nothing new to add. The pros and cons of monetarism have been discussed ad nauseam. Two of the most illuminating debates were that between Milton Friedman and Walter Heller in the early 1970s, and that between James Tobin and David Laidler in the *Economic Journal* in 1981.6

The views of monetarists place a distinctive stamp on their policy proposals. Clearly monetarists look mainly to the central bank for policy execution. Moreover, since they estimate the output cost of reducing chronic inflation to be modest, they are less reluctant to recommend slow money and output growth as effective and inexpensive cures for chronic inflation. Finally, monetarists believe that the money demand function is stable; this reasoning leads to the conclusion that a stable path of money growth of 3 to 4 percent annually will lead quickly to stable, noninflationary growth paths.

For an outsider, the most striking feature of the debate is that it continues. With the several glaring weaknesses of the monetarist doctrine—the evidence of interest-sensitivity of the demand for money, the instability of the very definition of money, the highly unreliable nature of the relationship between interest rates and output—it is difficult to understand how the belief in the monetarist solution can survive and thrive.

The Productivity Slowdown

Over a period of a decade or more the growth of potential output will be the principal determinant of real economic performance, and the growth of total factor productivity, along with labor force participation patterns, the key to future growth in potential output. With some

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provisos, there is no real controversy among the major macroeconomic schools about the appropriate policy to spur productivity. The main way that an economy can increase the growth in potential is by increasing the fraction of output devoted to human and tangible capital formation. Virtually all economists have called for one of an assortment of pro-saving or pro-investment policies, from tax relief for investment to policies designed to spur personal savings to monetary policies that lower real interest rates.

The major dissenters from this consensus are the supply siders and the neo-Malthusians. The supply siders have extremely unrealistic notions about how easy it is to increase savings and investment. For example, supply siders suggest that personal tax rate cuts will have a significant impact on productivity growth. A careful review of the evidence indicates that there may well be a positive impact on productivity if the growth of actual output is accelerated; but this is clearly a "demand-side" effect, not a supply-side effect. It is highly unlikely that personal tax cuts will have a positive impact on the growth of cyclically corrected output per person-hour employed; indeed, given the labor force responses of different demographic groups, the impact on potential productivity is likely to be negative rather than positive.

On the other side the neo-Malthusians paint a pessimistic picture in which economic growth will be constrained by limitations of resources or energy. One might have predicted that this view would land on fertile soil during the turbulent 1970s, but it appears to have received little serious attention either by economists or by policymakers.

Chronic Inflation

The final policy dilemma is the enduring issue of chronic inflation. Arthur Okun devoted the last years of his professional life to the diagnosis and cure of chronic inflation. He had something to say about every economic issue, but he said everything worth saying about inflation theory and policy.

One of the ironies of economic history is that, while Keynesians have often been thought insouciant about inflation, most of the recent inquiry into the mechanics of the inflation process and the dilemmas of policy have been undertaken in the framework of the neo-Keynesian synthesis. This apparent paradox is understandable, given the views of the other major paradigms about price adjustment. Only when wage and price
adjustment is slow relative to the pace of other economic forces, as in the neo-Keynesian synthesis, does it become interesting to study inflation. It is also in just this circumstance of slow price adjustment that policy faces the problem of the Phillips curve trade-off in which inflation control involves significant economic costs.

To understand the dilemmas of anti-inflation policy it is necessary to restate an earlier point: the orthodox way to slow chronic inflation is by inducing slack in product and labor markets, and such a cure is extremely costly. To repeat the calculation given above, to reduce an underlying annual inflation rate of 10 percent to 0 would require forgoing approximately two-thirds of a year’s GNP, although it might be spread over an extended period of time. This embedded chronic inflation is akin to an external national debt of $2.0 trillion—a debt that we must either live with in the form of high chronic inflation (with the “interest payments” being inefficiencies, misallocations, and shoe leather) or pay off in lower economic activity. The fact that occasionally fortune allows actual inflation to drop quickly and painlessly, as it has over the last year, confuses markets about the causes and costs of disinflation. An occasional run of good luck, in economic policy as in roulette, should not blind us to the fact that substituting chance for a deliberate anti-inflation policy leads to central bankers’ ruin.

Since reducing chronic inflation is so costly, inflation has become the major constraint on economic activity in the United States over the last decade. The main reason policymakers have been unwilling to set higher targets for output and employment is simply their fear that higher targets would risk increasing inflation. It is difficult to guess how much higher output might have been without an inflation constraint; unemployment rates in the 2 to 3 percent range, and hence output 8 to 10 percent higher, would surely have been much closer to the ideal output than the outcome shown in table 1 was.

Once inflation is accepted as the major constraint on high levels of utilization of labor and capital, economists will have to think about economic problems in a novel and paradoxical way. This was the topic of the last two chapters of Okun’s Prices and Quantities. In such a world, the social cost of public- or private-sector activities is measured not only by their resource costs but also by their inflationary impacts. Thus, when considering alternative ways of cutting taxes, the differential impacts of taxes on the price level should enter into the cost-benefit calculation along with more Keynesian considerations of aggregate
demand or neoclassical strictures of resource allocation. Or in weighing alternative energy policies, it would be necessary to calculate the "energy price externality," the impact of different energy price trajectories on prices and thus on overall economic activity.

Given the view that inflation is the major constraint on economic activity in the short run, it is clear that much more thought should be given to devising "efficient" anti-inflation policies. An efficient policy is one that imposes lower economic costs than the orthodox anti-inflation policy of inducing economic slack.

Two classes of more efficient policies have been identified: cost-reducing policies and incomes policies. Cost-reducing policies consist of government actions, such as cutting indirect taxes or promoting productivity, that lower the normal costs of doing business. These provide one-shot reductions in the price level, but some fraction of them probably end up in a lower underlying inflation rate. There is little controversy about such measures, but the stock of cost-reducing actions is small and probably largely depleted.

Incomes policies are direct interventions in markets to moderate the pace of price and wage increases. A more modern version of incomes policies, relying on a marketlike mechanism, is "tax-based incomes policy," or TIP. TIPs continue to be the most promising of the "efficient" anti-inflation strategies.

Yet another efficient anti-inflation policy would be a monetary reform, such as that suggested by Jeffrey Shafer. This approach would revise all contracts and financial instruments (except currency) by reducing both future prices and nominal interest rates by a given amount, say, 6 percent a year compounded from today. A labor contract that contained annual increases of $2 an hour now and $1 an hour two years hence would thereby have these increases reduced to $2/(1.06) and $1/(1.06)^2, respectively. Such a scheme is complicated and might involve thorny issues of the extraterritorial impact of U.S. law on dollar-denominated foreign contracts. It has the advantage, unlike other price-wage policies, that such a monetary reform would in principle reduce inflation painlessly, that is, without changing relative prices.

Thinking about the efficient allocation of resources in a macroeconomic framework where inflation is the major constraint on economic activity has proved to be an arduous task. Like physics after relativity theory, the world seems upside down when the short-run marginal cost of an action is its effect on dollar prices rather than its opportunity cost.
in diverting resources from other uses. The construction of a theory of value in which the impact of inflation is a central part of the cost of an economic event, along with detailed analysis of the kinds of policies that would efficiently reduce chronic inflation or prevent its accelerating inflation, were the tasks that Arthur Okun had undertaken before his untimely death. Much further work remains to be done to synthesize Okun's theoretical and policy insights on the topic of chronic inflation into the body of modern macroeconomic thinking.

**Comment by Stanley Fischer**

The Nordhaus paper is refreshing for its boldness. Nordhaus presents a spirited defense of a currently unfashionable view: that the neo-Keynesian synthesis of the late 1960s constitutes an adequate foundation for understanding the behavior of the economy and for policymaking. Like any good strategist, he makes an active offense part of the defense. His sallies against monetarism, goldbugs, rational expectations, and especially supply siders provide entertainment along with instruction.

Many of Nordhaus's arguments are appealing. His evaluations of the current interest in the gold standard, of capital fetishism, and of supply-side economics are compelling. So is his view that the productivity slowdown is the most important macroeconomic phenomenon of the 1970s. I agree too with his belief that the neo-Keynesian synthesis of the 1960s remains a useful component of our understanding of the economy. But my comments will center on points of disagreement. Specifically I will argue, first, that Nordhaus substantially underestimates the intellectual coherence and cogency of the attack on the neo-Keynesian synthesis of the late 1960s that was mounted by Robert Lucas and others in the 1970s, that he therefore underestimates the extent to which the younger generation of macroeconomists have moved away from the view of the economy that he takes for granted, and that he underestimates the depth of the defense of his views that he has to mount; and second, that for policy purposes there is indeed a useful neo-Keynesian–Friedman–Phelps–Lucas synthesis, but that there remains an awkward gap between sensible policy advice and sound theory.  

Macroeconomics of the 1960s and 1970s

The neo-Keynesian synthesis of the late 1960s consisted of the IS-LM apparatus writ large (very large) with a wage-price sector appended. The Phillips curve was not yet vertical; perhaps there was an expected inflation term on the right-hand side of the reduced-form price-price Phillips curve, but its coefficient was certainly less than one. Progress was expected to come from further refinement, meaning enlargement, of the large econometric models. The models were confidently being used for policy simulations.

Three subsequent developments have affected professional views of the working of the economy and policy.

1. The rational expectations equilibrium with misperceptions (REEM) view of the economy has been worked out.

2. The Lucas econometric policy evaluation critique is accepted by many as invalidating any claims to usefulness of policy simulations in existing econometric models, or at least those of the late 1960s.

3. The notion of dynamic inconsistency of policy has been developed to justify the need for policy rules rather than discretion.

Nordhaus well describes the REEM approach to economics. He dismisses it as elegant but irrelevant on the grounds of its dubious assumption of price flexibility. He argues further that the approach implies both that output should not deviate persistently from the potential level and that changes in the inflation rate can easily be achieved through the implementation of a credible slower money growth policy. He also notes that it has had virtually no effect on policy: “Perhaps the idea that policy cannot affect the real economy is as foreign to policymakers as random walk theories of stock prices are to stockbrokers.”

Like Nordhaus, I believe that the price-flexibility assumption of REEM is a crucial weakness. But the approach is dismissed too quickly. First, REEM treats unemployment as search related, attaches no great significance to the unemployment rate, and does not regard itself as destroyed by the serial correlation of unemployment. The well-known early criticism that REEM could not explain the serial correlation of output is clearly false: once the economy is moved away from a particular

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output path, various sources of persistence, like capital accumulation or inventory accumulation or slow labor adjustment, can keep it away from that path for some time.

Indeed, I am not sure that REEM accepts the distinction between potential and actual output in the same sense as Nordhaus. The full employment level of output that appears in the Lucas supply function is usually subscripted with a $t$. That level of output is probably best interpreted as the level the economy would attain in a given period if there were no shocks in that period and all past shocks were taken into account. If a variety of mechanisms keep output serially correlated and if unemployment is generated by search behavior, then changing short-run wage prospects associated with output fluctuations can certainly be interpreted as producing serially correlated unemployment rates in REEM models.

As an aside, the 1970s were not easy on the notion of potential output, important though it may be. The decade saw a rising time series of estimates of the full employment rate of unemployment, along with changing Okun’s Law coefficients. In 1970 it was quite clear that the natural, or full employment, rate of unemployment was around 4.5 percent; now estimates range from 5 percent to above 6 percent. In 1970 the Okun coefficient was 3, today it is 2. Confidence in estimates of potential output must be lower than it was ten years ago.

Nordhaus’s second criticism of REEM concerns its implication that disinflation is easy as long as policy changes are credible. Credibility is the REEM dummy variable that explains the failure of disinflation policies to work rapidly. But it is not for that reason an empty argument. There are reasons other than credibility that a sharp reduction in money growth would not produce immediate disinflation and would produce unemployment. Nonetheless, the adjustment process would indeed be quicker if the policy change were credible. Given credibility as an economic variable, REEM can survive Mrs. Thatcher and probably even President Reagan’s failure to produce quick disinflation.

Finally, there are arguments that price stickiness is not a fatal flaw in REEM. It has been argued—for instance, by Robert Barro and Robert Hall—that prices and wages can be irrelevant to the allocation of resources, which magically arrange themselves into a Pareto optimum.9

The reason for this is that otherwise some people could be made better off without anyone being made worse off, a situation that proponents of the argument find it hard to believe could exist. The important point made in this argument is that, without having a theory that explains price inflexibility, it is difficult to know what policy or other implications follow from that fact. It is certainly possible to build models with price inflexibility in which anticipated monetary policy is irrelevant to output determination, as Bennett McCallum has shown. 10

The objections to REEM that Nordhaus raises have thus been discussed in the literature and countered with arguments of varying persuasiveness. Nordhaus and I both find some of the arguments unpersuasive, but that certainly does not dispose of the REEM approach. At this stage, many of the best and brightest young macroeconomists are working within that approach, trying to build a new paradigm to replace the one that existed at the end of the 1960s.

Indeed, the situation bears all the marks of the counterrevolutionary stage set out in Harry Johnson's remarkable 1971 Ely lecture. 11 The approach has spawned new theoretical and empirical techniques that can be applied to old problems but that need further technical development before they can reasonably be expected to deliver on their promise. Too much remains to be done to give up the REEM approach soon, especially in favor of a comparatively recent orthodoxy.

It is in this context that I believe Nordhaus substantially underestimates the impact of the second leg of the REEM triad—the Lucas


11. Harry G. Johnson, "The Keynesian Revolution and the Monetarist Counter-Revolution," *American Economic Review*, vol. 61 (May 1971, Papers and Proceedings, 1970), pp. 1-14. Johnson was of course discussing the monetarist counterrevolution, which he correctly predicted would fail. But his arguments can be adapted with very little change to describe the more successful REEM counterrevolution. One even sees—for instance, in Robert E. Lucas, Jr., "Understanding Business Cycles," in Karl Brunner and Allan H. Meltzer, eds., *Stabilization of the Domestic and International Economy*, Carnegie-Rochester Conference Series on Public Policy, vol. 5 (North-Holland, 1977), pp. 7-29—the claim to continuity with the prerevolutionary situation that Johnson described as necessary for the legitimacy of the counterrevolution. The reason the REEM counterrevolution is more successful than monetarism is that it is theoretically far more innovative.

Johnson argued that monetarism would fail because the problem on which it focuses, and which it was better equipped than Keynesianism to handle—namely, inflation—was inherently less important than unemployment. The prediction was right for the wrong reasons. Unemployment is now politically less important than inflation, but monetarism has lost ground because of its difficulty in solving the inflation problem.
econometric policy evaluation critique. The role of the critique in undermining the public's confidence in econometric models is no doubt minimal. But within the profession it is now possible, and increasingly common, for researchers to dismiss large econometric models as providing evidence on anything. Thus when Nordhaus cites Otto Eckstein's calculations as showing how little policy had to do with poor economic performance in the 1970s, he persuades no one who did not already believe that, nor does he even move their priors or posteriors one iota.

It is indeed remarkable that the Lucas policy evaluation critique has triumphed without any detailed empirical support beyond Lucas's accusation that macroeconometric models in the 1960s all predicted too little inflation for the 1970s. The general point made by the critique is correct and was known before it was so eloquently and forcefully propounded by Lucas. That the point has been important empirically, however, is something that should have been demonstrated rather than asserted.

The underprediction of inflation in the 1970s is a result of the omission of the expected inflation rate, or momentum, from the Phillips curves of the econometric models rather than of their failure to allow structure to change with policy. Subsequent breakdowns of important equations, such as the demand for money, perhaps come closer to justifying the policy evaluation critique dismissal of old-fashioned econometric models. In any event, it is now too late for most large econometric models to stage a comeback. It will be some years before we know whether any of the coming generation of models will fare better. It is by no means certain that they will, for there are many sources of misspecification in any econometric model and no evidence that current research is directed at overcoming a particularly serious one.

*What Difference Does It Make?*

So what if younger members of the profession are attracted in large number to the REEM approach? After all, as Nordhaus notes, the approach has had no effect on policy. However, the links between theory and policy work with long and variable lags. The reason that madmen in authority hear the voices of academic scribblers of a few years back is that both the madmen and the scribblers take time to rise through the ranks. Today's REEMers are tomorrow's Nordhauses.

Nordhaus remarks that the notion that policy can do nothing is as
strange to policymakers as random walk theories of stock prices are to
stockbrokers. But the REEM approach does not assert that all antici-
pated policy actions have no real effects. Certainly, anticipated changes
in fiscal policy will have real effects. And in an economy that has not
fully adapted to inflation, anticipated changes in monetary policy that
affect the expected inflation rate will also have real effects. Nor does the
truth of random walk theories of stock prices require that stockbrokers
accept them.

The third leg of the REEM triad—dynamic inconsistency—also
receives too little attention from Nordhaus. The notion, developed by
Kydland and Prescott,\textsuperscript{12} provides the most serious backing for the use
of rules rather than discretion that has appeared to date. Their most
important result is that a sequence of policy decisions, each of which is
made optimally, looking forward over an infinite horizon, may lead to
outcomes inferior to those obtained by sticking to an optimal policy rule.

The point is best made by example. Consider a model in which taxes
may be raised by taxing capital or labor, or both. In the current period,
in which the existing capital stock is already in place, it is optimal to tax
only capital. An optimal plan will have implied tax rates for all future
periods, which will in general tax both capital and labor. To produce
optimal private sector behavior, these future rates should be announced.
But of course, when the future arrives, it is optimal to tax only capital.
So long as the public believes that this is the last time capital will be
taxed, all is well. Once it believes that capital will be taxed heavily each
period, trouble sets in, for capital accumulation is discouraged. On the
average, the economy will do better if the authorities adhere to the rule
calculated at the beginning rather than trying each period to do what is
best.

This analysis should help clarify some of the real difficulties of
democratic decisionmaking. For instance, consider the inflation-unem-
ployment trade-off. From today's viewpoint, it is never better to have
more unemployment to get rid of inflation. Rather, given the dynamics
of inflation and unemployment, inflation is best left to be reduced slowly
in the future. But when the future arrives, it is no longer sensible to incur
more unemployment to fight inflation.

The argument may help explain how the economy reached a core

\textsuperscript{12} Finn E. Kydland and Edward C. Prescott, "Rules Rather Than Discretion: The
473-91.
inflation rate of around 8 to 9 percent rather than zero. The sense in
which policy was not responsible for the inflation of the 1970s—a view
that I have also argued—is a special one. Given the core inflation rate of
the early 1970s, monetary and fiscal policy was not responsible for most
of the variations in the inflation rate observed through the decade.
Monetary policy was too expansionary by far in 1971–73, and less
obviously so in 1978–79. But energy price shocks and the productivity
decline deserve most of the blame.

Nonetheless, there remains the question of why we are now talking
of getting the inflation rate down from 8 to 9 percent rather than down
from 4 to 5 percent. And the answer is that we have chosen, when there
has been a choice, not to try to get the inflation rate down at the expense
of employment. Perhaps this was on each occasion a rational decision.
And perhaps inflation does not have much cost. But that is not the
perception of most of the public. The feeling that 8 to 9 percent inflation
is intolerable and that the normal processes of democratic decisionmak-
ing have resulted, and will continue to result, in inflation of that level or
higher is a driving force behind constitutionalism. So is the feeling that
big government is the result of present processes of economic decision-
making. There are serious arguments behind these views, and they are
not disposed of by saying that the preferences for rules, constitutional
amendments, and so forth, reflect distrust of democratic ways of making
policy. Democracies make decisions in a variety of ways, including
through the judiciary and executive branches. It is reasonable to discuss
the question of whether some set of decisions is better made more or
less frequently, more or less regularly, how, and by whom.

*Is There a Post-Keynesian Synthesis of the 1980s?*

The preceding discussion is not intended to convince anyone that the
REEM approach to theory and policy is correct, but rather to argue that
the approach has more depth than Nordhaus allows, to agree with him
that the approach has become influential within the profession, and to
suggest that it may influence policy much more in the future than it has
so far.

13. The public opinion polls do not provide unambiguous support for the view that
inflation is more costly than unemployment. When asked, most people say that it is not
desirable to incur more unemployment in order to reduce inflation. See Stanley Fischer
and John Huizinga. "Inflation, Unemployment, and Public Opinion Polls." *Journal of
Money, Credit and Banking*, vol. 14 (February 1982), pp. 1–19.
On the issue of how useful the pure REEM approach is for understanding the behavior of the economy and for policy, I come out close to where Nordhaus does. The implication of the flexible-price REEM model that inflation should respond quickly to changes in monetary policy is palpably as far from accurate as the inflation predictions of macroeconomic models in the late 1960s were. Of course, credibility is an excuse, but invoking the credibility of policy as an argument explaining wage-price behavior brings us back close to an augmented neo-Keynesian synthesis.

There is a useful neo-Keynesian-Friedman-Phelps-Lucas synthesis that has (at least) the following elements.

1. In the long run the Phillips curve is vertical. Lower monetary growth, applied long enough, will eventually reduce the inflation rate and restore the unemployment rate to its natural level.

2. In the short run the Phillips curve is not vertical. The short-run nonverticality arises from several factors, among them nominal contracting, concern about relative wages, and expectational errors. The proportions in which these factors matter depend heavily on the state of the economy and the types of policy being followed. In normal times prices will be quite sticky, and policy can rely on the type of responses that Keynesian sticky price-wage models suggest. But radical policy changes could reduce stickiness rapidly. Price stickiness is not a structural characteristic of the economy. It can be relied on only so long as policy actions not too different from those of the past are undertaken.

3. Uncertainty about the structure of the economy and its robustness in the face of changes in policy is considerable, certainly more than the neo-Keynesian synthesis of the late 1960s realized.

4. Optimal policymaking is best viewed as a cooperative game in which the government maximizes a welfare function that is positively associated with the utilities of private agents. Actual policy may deviate from the optimum, and it is important both to study actual policymaking and to discuss institutional arrangements to improve its performance.

What is wrong with the synthesis, and why would it not command instant assent from the entire macroeconomics profession? The major difficulty is that there is no convincing theoretical basis for the policy conclusions I draw from the existence of price stickiness. Until there is—if there ever is, for price stickiness is not an immutable fact of nature—views on policy and on appropriate macro modeling will continue to differ. This is the source of the embarrassing gap between sound
theory and what Nordhaus and I believe to be sound policy, referred to at the beginning of my comments.

There is one other important issue on which there is likely to be substantial disagreement, and that is the importance of inflation. The neo-Keynesian synthesis of the late 1960s was that inflation was not a serious problem. There is agreement on this point from parts of the Chicago school, which argues that if inflation was a serious problem, far more private and public arrangements to deal with it would have been developed. This is not the place to discuss the merits of the arguments. But it is clear that disagreement within the profession on the relative importance of inflation and unemployment is a source of differing views on desirable policy, which are played up so extensively by those outside the profession.

**On the State of Macroeconomics**

When the outside world tells us that no two economists ever agree on anything, it is talking about macroeconomists. When colleagues announce that it is impossible to teach macroeconomics with a straight face, they are sending a similar message about the state of the field.

Should we macroeconomists bow our heads in shame, throw in the towel, and retire to the purity of the ivory tower, or should we cheerfully continue inflicting untold harm on the profession and the economy? The answer, obviously, is none of the above.

Macroeconomists are characteristically heard disagreeing in public because no one is interested in hearing them agree. There is, in fact, substantial agreement within the profession on many of the important issues. For instance, no reputable economists supported the supply-side claims underlying the recent Reagan budget. As many have observed, differences on policy matters where they exist stem in large part from differing value judgments.

A return of the academics to the ivory tower is unlikely to help the economy. Economic policy will continue to be made, whether or not academics participate in the process. The real issue is whether policy will be made better with or without their participation. It is difficult to see how a retreat by academics would help, particularly since they more than others know what it is they don’t know.

What about our colleagues? It is a good idea to start with a little questioning. Typically it turns out that the complainers are ill-informed
and can easily be answered. To restore balance it is helpful to ask about unresolved issues in the critic's field—for if there were no unresolved issues there would be no field worth working in. But sometimes the complaint is about the difficulty of modeling price stickiness and price dynamics. Here we can all agree that we need a more satisfactory theory to explain the facts—and that the issue is central to macroeconomics and our understanding of the behavior of the economy.

Comment by Larry A. Sjaastad

After providing a description of the problems facing the U.S. economy, which include slow economic growth, high inflation, high unemployment, and sluggish productivity, Nordhaus concludes that chronic inflation is the principal problem in that inflation constitutes the overriding constraint on economic policy. In between, Nordhaus covers a lot of territory and has something to say about almost everything. He finds the neo-Keynesian synthesis alive and well (presumably somewhere east of Chicago). Monetarism becomes a special case of the neo-Keynesian synthesis, rational expectations are dismissed as "elegant but irrelevant" to policymaking, and supply-side economics is characterized as essentially a political phenomenon. While not an advocate of supply-side economics, I think that it is more correctly (and fairly) described as a collection of unsubstantiated empirical assertions and, as such, just could turn out to be right, although in my view, this is quite unlikely. The disturbing aspect of the supply-side discussion is that it is a debate about the facts, with neither side doing the hard work required to resolve the issue.

There is much in the Nordhaus paper with which I disagree and little with which I can agree. The central assertion that the neo-Keynesian synthesis has merely suffered a setback is debatable at best, but it remains a proposition that can (and will) be tested only over time. His cavalier treatment of rational expectations is unfortunate, particularly because he attributes to that approach a foundation that is quite incorrect. Nordhaus states that the second premise of rational expectations is that "all markets clear in the very short run." Neither Lucas nor Sargent nor Wallace would subscribe to that; indeed, no one denies that inventories fluctuate, unemployment exists, and so forth. The novelty of the rational expectations approach lies in the suggestion that these disequilibria are
probably far less amenable to resolution by policy actions than is thought to be the case under the neo-Keynesian approach. Far from being irrelevant, the implications of the rational expectations approach to economic policy are fundamental.

Returning to the serious problems facing macroeconomists here and now, I suggest, at some risk of sounding complacent, that some of the problems may be less serious than Nordhaus suggests. For slow growth of output (and productivity), it is evident that the 1970s were far worse than the 1960s, but not very far out of line with the long-term experience of the United States. I submit that it is not that the 1970s were so bad, but rather that the previous decade was a particularly good one. Viewed that way, the interesting issue is why the 1960s were so far above the norm of historical experience.

The problem of inflation is, of course, a very real one, particularly under the Nordhaus interpretation. But again I disagree with his emphasis, as his concern with the short-run cost of stopping inflation does not extend to the long-run cost of living with inflation. It seems to me that one of the main reasons that inflation is a "problem" is that the rate of inflation is positively correlated with its variability, so that anticipating the rate of price rise becomes increasingly difficult as that rate goes up. The efficiency of markets—particularly long-term markets—suffers as a consequence. Nordhaus ignores the widely held view that these long-run costs can overshadow even exaggerated estimates of the short-run stabilization costs. In this context, I emphasize that stopping inflation obviously can be very expensive, but there is also evidence that it need not always be so. After all, there have been cases in which the inflation rate has fallen sharply without aggravating unemployment—the usual measure of the cost of stabilization. Thomas Sargent's recent work on post-World War II stabilizations is instructive here.

There are also some very recent experiences to draw on in Latin America, particularly those of Chile during the 1978-81 period and Argentina since 1979. Both countries had experienced extremely high (300 to 1,000 percent per year) rates of inflation that were rooted in enormous fiscal deficits. and both countries drastically reduced those inflations by what is best described as "scorched-earth" policies of demand reduction (1975-76 in Chile and 1977-78 in Argentina). The results were similar—large reductions in real output, particularly in the Chilean case, which appear to support the Nordhaus-Okun thesis that stopping inflation is terribly costly. Subsequently, however, both coun-
tries were able to all but eliminate the residual inflation without loss of output by a more subtle policy, one aimed at influencing expectations. The exchange-rate-based stabilization policy began in Chile at the end of 1977, with good results. The inflation rate fell from 65–70 percent a year to about 10 percent during calendar 1981; meanwhile the measured unemployment rate fell from 16 percent to between 7 and 8 percent. Argentina adopted a similar policy at the beginning of 1979 and induced a decline in the rate of inflation from 30 percent to 9 percent per quarter, with the unemployment rate both low and steady. Both examples fly in the face of the Nordhaus assertion.

The similarity between Argentina and Chile ends there, however. The Argentine policy terminated in disaster, with the rate of inflation during 1981 returning to its 1978 level, accompanied by a sharp drop in output and a rise in unemployment. The apparent explanation for the difference between the two experiments is relevant to the problem of inflation control in the United States. It is my contention that Chile succeeded, and Argentina ultimately failed, because a successful stabilization policy must have both coherence and continuity. One of the important differences is that Chile began the experiment with a balanced fiscal budget, whereas Argentina had a large deficit. I submit that economic agents in countries experiencing chronic inflation ultimately come to the view that, in the final analysis, fiscal deficits are always monetized and that a stabilization policy that does not incorporate fiscal reform is doomed to failure. Both Chile and Argentina were able to stabilize in the short run with virtually no unemployment cost, but the persistent fiscal deficit in the latter country eroded confidence in that stabilization so much that it was not only abandoned but totally reversed. The U.S. fiscal disaster that may well be waiting at the end of the supply-side tunnel has, I think, convinced a lot of people that the U.S. inflation is not about to disappear. Here I am in full agreement with Nordhaus and others that a scorched-earth monetary policy would entail mainly costs and few benefits unless the deficit was dealt with. While we can quibble about the definition of the deficit, my point is that the prospect for monetization of the debt is always enhanced whenever the Treasury is required to go to the market on a systematic basis.

The relevance of these episodes is not, of course, that we should adopt Argentine or Chilean exchange-rate policy (which we cannot do in any case), but rather that there is something important to be learned from them. Both were initially successful, measured by indicators acceptable even at Brookings, but one failed largely because of insuffi-
cient attention to the fiscal deficit. It goes without saying, of course, that if a fiscal deficit destroyed the Argentine attempt to adopt the dollar standard, our own fiscal deficit might well do the same if we were to restore the gold standard. Indeed, it seems to me most unusual to be contemplating a return to gold when thirty-year Treasury bonds carry a yield of 15 percent. If there is any lesson from history, it is the following: without systematic fiscal deficits, the gold standard is unnecessary, and with those deficits, it is impossible.

In conclusion, I point out that Nordhaus is vulnerable to the Friedman syndrome. Some years ago it was popular to say that if you began with Milton's assumptions, you always got Milton's results. Similarly, if you begin with the premise that it is incredibly expensive to stop inflation, you are driven to find ways to live with it or to devise schemes, such as incomes policies, that at least appear to be doing something about it. (I submit in passing, however, that the track record of incomes policies is not an enviable one.) But if we look outside the recent and narrow experience of the United States, we find a rather rich set of alternatives. Unfortunately, few nations demonstrate the ability to learn from the experiences of other countries; indeed, in some cases, we even fail to learn from our own. My own view is that we stand to gain at least as much by the historical approach as by devising new but untested techniques for dealing with old problems.

Comment by Robert M. Solow

Nordhaus is right, and it needed saying. I agree with him that the "neoclassical synthesis" is a good way of understanding the level of economic activity and its evolution. Perhaps eventually we will be able to share the same confidence about the price level and its evolution, but clearly there is still some way to go.

Why, then, is macroeconomics in disarray? "Disarray" is an understatement. Thoughtful people in other university departments look on with wonder. Professional disagreements exist in their fields too—at the frontier there is always disagreement—but as outsiders they are shocked at the way alternative schools of thought in macroeconomics describe each other as wrong from the ground up. They wonder what kind of subject economics is. (Some of them are not above a little Schadenfreude either.) The fathers-in-law of economics graduate students must be even
worse because, unlike good scholars in other subjects, they regard themselves as entitled to pontificate about macroeconomics. The person in the street would probably regard Nordhaus as just another special pleader, another candidate for alderman.

Two questions arise. How did we get into this fix? And how can we get out of it? Nordhaus has a lot to say about the first question. I will only supplement his comments. He has almost nothing to say about the second question. By implication he seems to be saying: if the neoclassical synthesis is essentially right, our duty is to keep the faith, to improve our understanding of the economy bit by bit, and to wait for the pendulum to swing our way again. I do not disagree, but I think part of the problem is not in our stars but in ourselves. So we could mend our ways a bit.

Why are we in a spot like this? Nordhaus has talked about some of the causes and hinted at others. Here are my marginal notes.

People have been stoning the messenger bearing bad news for millennia. When the economy is in trouble, so are economists. In fact, the reaction goes a little deeper. In the popular mind, economics is expected always to have a policy answer, whatever the current problem. The public is not prepared to take "You can't get there from here" or "There is no acceptable and costless way to end the inflation" as an answer. We may complain that they don't go after the biology department because death persists as a problem or even after doctors for not curing cancer or migraine. That will not get us off the hook.

Part of the crop of the 1973-80 inflation was planted in the financing of the Vietnam War. There is an important sense in which "we" are not to blame for that. Gardner Ackley and Arthur Okun warned Lyndon Johnson, and he disregarded them for reasons of his own. Outside commentators from within the profession expressed their misgivings; you could undoubtedly find Newsweek columns by Paul Samuelson urging that if we desired to bomb Vietnam into the Stone Age it would be better to do it with a smaller full employment deficit. But no one resigned from the Council of Economic Advisers and, as I remember, there were only half-hearted attempts to lobby public opinion and even less to lobby Congress.

As for that part of the inflation that stemmed from OPEC in 1974 and again in 1979, it may be that there was no practical way to avoid a very bad outcome in the second half of the 1970s. I think there is a passage in Nordhaus to suggest that he shares my suspicion. Unfortunately for us, the articulate public is unwilling to accept the notion that American society may for occasional intervals be at the mercy of forces it cannot
overcome. We might only ask ourselves whether the science of economics might not be better off if we played a smaller role in practical policy. Would society be better off? That depends, I suppose, on how much harm one thinks Ronald Reagan can inflict on the economy, and whether any of it could have been fended off by a more detached, less involved economics profession.

There is another way in which we may be to blame for the poor opinion the world has of us. I think we suffer from econometric illusion. We overestimate the accuracy and reliability of our models. Too few econometric routines can respond to a question with "Don't ask." If you are honest you will agree that, although you are fundamentally right and monetarists and new-classical theorists are wrong, they can always find sample periods, data sets, econometric specifications, and time series methods according to which they are at least as good as you (we) are. This is again a piece of unavoidable bad luck—the collinearity of the world, the shortness of stationary time series, the inapplicability of the experimental method. But the result is Babel, or even babble.

The low power of econometric tests combines with political involvement to give the profession a very bad image. Every reporter knows that two well-known economists can be found who will say exactly opposite things about any current event. There was an example just the other day in the New York Times. The question was, "Is monetary policy too tight?" There, on the first page of the business section, were side-by-side photographs of Allan Meltzer and James Tobin saying not quite opposite things, but opposite enough. There are probably few top economists who have not contributed to the world's bemusement in this way. I know which statement I agreed with, but what is the ordinary reader of the Times to think? Of course, it would be infinitely better if the accompanying story had gone something like this. Meltzer and Tobin disagree about this because Meltzer thinks that most substantial deviations from Walrasian equilibrium are caused by misperceptions of the course of monetary policy, and so on and so forth, whereas Tobin believes that wages and prices are sticky for institutional, historical, and other reasons, so that persistent deviations from Walrasian equilibrium can arise from any change in the environment, and so forth. But that would be asking for scholarship, not politics. What is suitable for the American Economic Review is not suitable for the New York Times.

It may be that the solution here is to keep our mouths shut more often. I am learning to discipline myself to say to reporters, "No, I'd rather not comment on that because to make sense I would have to explain a
five-equation model, for which you do not have the space or your readers the time." It has, of course, occurred to me that if responsible people adopt this policy, only the unscrupulous will be quoted in the Times.

There is a deeper and in some ways more disturbing connection that Nordhaus makes. What about the flood of simple-minded monetarists or, worse yet, the Laffers, the Wanniskis, the Claremonts, the goldbugs, the constitutionalists, the others? Why just now? I have mentioned two reasons—bad times and our own contribution to the undermining of the scientific character of economics. Nordhaus adds that this spate of snake oil is a by-product of the widespread loss of confidence in authority. I see what he means, but I am not sure that is the best way to put it. Many of the people who support constitutional amendments to balance the budget or require steady growth of some—any—monetary aggregate seem instead to be seeking authority. They are—with exceptions, of course—the same people who want constitutional amendments to prohibit abortion or to prohibit busing. They are motivated in all sorts of ways, often by a sense that they have been betrayed or conspired against. The kernel of truth in Nordhaus's view is that sometimes they feel that political or judicial authority has betrayed them. But not always: sometimes it is others, strangers. It is too simple to describe this movement as antidemocratic, though clearly there is a sense in which that is so. But what could be more democratic than a constitutional amendment duly passed? The bad thing, from our narrow point of view, would be for technical issues of economic analysis and policy to get caught up in people's acting out of their frustrations with their children, other races, sexual behavior, and so on.

So far I have been talking mainly about the state of macroeconomics as seen from the outside. How about the view from the inside? Nordhaus says very little on this subject. Fischer argues in his comment that the intellectual force of the attack on the neoclassical, neo-Keynesian synthesis is greater than Nordhaus admits, although it is not clear from his own exposition why that should be so.

I think we have learned something from the new classical macroeconomics of the past six or seven years, though I also think it becomes clearer with each passing day that equilibrium theory is simply incapable of providing a satisfactory explanation of what is happening out in the world. Baily's paper in this book demonstrates that failure for the 1930s, with something to spare. But it is proving to be true even for humdrum, everyday economic fluctuations. The notion that observed data are points of intersection of the relevant perceived Walrasian supply and
demand curves for labor and goods is not self-evident and does not appear to be consistent with the results of exploratory data analysis.

The fruitful idea to emerge from the new classical macroeconomics, I think, is the notion that economic policy is really a game in which private agents have strategies too. As in any game, participants will react to perceived changes in others’ policies, including those of the government. It offends neither common sense nor casual observation to suppose that this sort of behavior can have important consequences for the operating characteristics of the system and the effects of public policy. The new classical school has also been the source of nice technical improvements in the treatment of equilibrium dynamics with random disturbances in the environment, and there may be occasions when that is the appropriate setting for conducting analysis.

But this is hardly enough to explain why, in Fischer’s words, so “many of the best and brightest young macroeconomists are working within that approach.” Fischer’s comments seem intended to explain why that has happened. In the end, however, he leaves the mystery unresolved even on his own grounds. He is aware that the new classical school has no single empirical success to its credit, nothing that could count as a statistical verification, no “cross-equation restrictions” satisfied. No one has discovered a Phelpsian island or even a message in a bottle. No one has bothered to check if misperception is rife, in the right sequence, in the right direction, with the right people. I am not sure that anyone wants to.

The neoclassical synthesis is better off, but only slightly better off, on this account. I doubt that the track record of the large econometric models, good or bad, is even relevant. Fischer is probably right on this, not primarily because of the “economic policy critique” but because the record is so snarled by add factors, judgment calls, telephone calls, and fudging that it is incapable of providing a test of anything but the Bell System. Ray Fair is an exemplary exception to this judgment. Also, it is only fair to say that the neoclassical, neo-Keynesian mainstream has produced acres of small and partial models, which have been checked and estimated against data. Some of this is well done. Some not, but at least there is a record to be evaluated. That is more than one can say for equilibrium theory.

Perhaps I should add that the importance, as distinct from the elegance, of the “econometric policy critique” is an empirical matter. Even where it applies, it could make little quantitative difference or a lot. Is anyone trying to find out?
Whatever it is that attracts so many bright young macroeconomists to the new classical model, it cannot be its demonstrated superiority as a model of the world. Is it merely a better toy—new, shiny, unfamiliar, full of tabs A to be inserted into slots B, requiring and generating new skills that not everybody has? I spent too much of my youth being told by my elders that I was captivated by mere technique to be prepared to accept that explanation out of hand. Here is another puzzle in the sociology of science, a subject in which I have already demonstrated my amateur status. I think Fischer has not drawn the conclusions implicit in his own arguments and judgments.

Let me mention one last possibility that plays a key role in Fischer's argument. Perhaps the vogue for the new classical equilibrium theory is based not on its own merits, which appear to be limited, but on a yawning gap in the mainstream synthesis, the fact that "there is no convincing theoretical basis for the policy conclusions [drawn] from the existence of price stickiness." And again, "Price stickiness is not a structural characteristic of the economy. It can be relied on only so long as policy actions not too different from those of the past are undertaken." I do not want to argue about the substance of price and wage stickiness here; it is a worthwhile subject for a research program. But I do want to point out the methodological peculiarity of this argument. Why is it not symmetrical? Is price and wage flexibility a structural characteristic of the economy? It is surely a structural characteristic of the Walrasian model, but that points to the question, not the answer. Nothing Fischer says excludes the possibility that the changes from past policy needed to restore wage and price flexibility might include the abolition of trade unions and whispered conversations at work, the prosecution of all firms with a market share bigger than 3 percent, and the prohibition of all long-term buying-and-selling relationships, formal or informal. I do not argue that those minor differences in policy would actually be required to restore price flexibility. I do not even argue that the restoration of price flexibility ought to be regarded as the economist's equivalent of the Second Coming. It seems to me that equilibrium theorists are saying: the world must be like the Walrasian model because that is the model we have. Those who are more in touch with reality seem to say: the world is not like the Walrasian model, but perhaps the resemblance could be made closer. That is real progress. I would like to argue for even a little more imagination: the world may have its reasons for being non-Walrasian.