Taylor & Francis
Taylor & Francis Group

Evaluating Reaganomics

Author(s): PAUL A. SAMUELSON

Source: *Challenge*, 1987, Vol. 30, No. 6, Thirtieth Anniversary Issue (1987), pp. 58-65
Published by: Taylor & Francis, Ltd
Stable URL: https://www.jstor.org/stable/40720524

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at https://about.jstor.org/terms

Taylor & Francis, Ltd. is collaborating with JSTOR to digitize, preserve and extend access to *Challenge*

JSTOR

This content downloaded from
206.189.64.126 on Tue, 08 Apr 2025 00:53:18 UTC
All use subject to https://about.jstor.org/terms

PAUL A. SAMUELSON

Evaluating Reaganomics

Reaganomics was always less than the sum of its parts. By and large, mainstream economics, including the more sensible elements of supply-side thought, has won vindication from events of the 1980s.

It is possible that Ronald Reagan's landslide victory in 1980 will be reckoned by historians to be the second most important American electoral triumph in the twentieth century—second only to Franklin Roosevelt's 1932 victory over Herbert Hoover that culminated in the New Deal and the welfare state. Whether or not the conservative attempt succeeds in moving the United States back toward the pre-1929 pattern of market capitalism, with limited government regulatory and transfer operations, the Reagan program did begin with a challenge to conventional mainstream economics.

The story is not over. But what does the evidence on Reaganomics suggest so far? Are economists like me obsolete, fossils who perhaps do not even know that they are fossils? Was the Keynesian revolution all a big mistake—at best a temporary concession to transient populist distress, at worst a half-century detour from the main road of correct economic science?

It is hard to be nonpartisan about contemporary policy issues; value judgments so easily contaminate one's positivistic descriptions, interpretations, and forecasts. Nonetheless, I shall do my best. You are forewarned because you know me as a post-Keynesian, a one-time adviser to Adlai Stevenson and John F. Kennedy, an unrepentant idolator of Franklin Delano Roosevelt.

I caution at the beginning that my present task is not to evaluate the merits of eclectic post-Keynesianism against the merits of such contesting paradigms as monetarism, rational expectationism, Marxism, Kenneth Galbraithism, and much else. That is a hard task but one worth doing. It is a job, however, for the seminar room and the computer console. Treatises and monographs, rather than the spoken word in a public

PAUL A. SAMUELSON is Institute Professor of Economics at Massachusetts Institute of Technology. This article first appeared in *Challenge*, November-December 1984, and had been presented as the Horowitz Lecture in Tel Aviv, March 8, 1984.

This content downloaded from
206.189.64.126 on Tue, 08 Apr 2025 00:53:18 UTC
All use subject to https://about.jstor.org/terms

lecture, are requisites for so mighty a task. Reaganomics is the object my microscope is focused on here.

A melange of views

Reaganomics is not a coherent program. Certainly you could not put Ronald Reagan on a psychoanalyst's couch and, by interrogating him on what he believes, identify just what the content of Reaganomics really is.

Nor is there one economic adviser of the President whose views approximate closely to the core of Reaganomics. His last chief economic adviser, Professor Martin Feldstein of Harvard, does not agree with much of Reaganomics. But Feldstein's academic predecessor, whose name you will have forgotten, also found himself uncomfortable with much that went under the name of Reaganomics.

We are confronted with a doctrine that is less than the sum of its partly-cancelling parts. All one can do is enumerate some of the factions that have sought to formulate the Administration's policies. This is a task that a gossip columnist can do as well as or better than I. But only someone like me can relate these competing blocs to the doctrinal schools of thought that characterize modern economic science.

• *Business interests*. Reagan, an ideologue at the right wing of the Republican party, has from the beginning received support from business interests in California and Texas. They play hard political ball. Their ideology is minimal; self-interest sums it up. Laissez-faire and Chicago-style market economics are what they champion for the most part. But they have not had to read Hayek, Friedman, or Knight—or had to ponder over Harberger's consumer-surplus triangles of deadweight loss—to arrive at their positions. And they will as naturally seek a quota or tariff as buy cheap and sell dear, and will do so without the slightest feeling of ideological inconsistency.

I do not imply that there is something special about a conservative government's being responsive to the desires and interests of people of property. That's the nature of the beast of government. When a Democratic administration is in office, the special desires of trade unions and of various lower-income groups receive more favorable attention. On the whole, the record of the Reagan Administration in resisting protectionist pressures has been no worse than that of Democrats and may well have been somewhat better.

When a Reagan appointee—be it James Watt or Ann Burford—gets in terrible disrepute with the general public for some flagrant kowtowing to private interests, don't think for a moment that the President has been unlucky in his appointments. These officials, like much of the cabinet, subcabinet, and independent agency bureaucracy, were selected for their positions precisely because they could be counted on to favor a drastic reversal of previous practices. Only when carrying out these duties results in actions that are more than the political market will bear does the press become aware of what is going on all the time.

• *The Chicago-school wing*. American academic economics is becoming increasingly conservative. Anyone who believes that the economics profession is shot through with leftists has not recently attended a meeting of the Western Economic Association or done content analysis of our leading learned journals. Pareto optimality preoccupies much of microeconomics. What the schoolmen called distributive justice is not where the action is today.

That someone like Milton Friedman, confronted with the alternatives to Ronald Reagan, should think well of Reagan comes as no surprise. This admiration, as is so often the case in life, is reciprocated. Both adhere to the philosophical tenet that a strictly limited government role in the GNP is a good thing—a good thing in itself and quite beside the improvement in economic efficiency that it will entail. Personal freedom, and in significant degree that means freedom from government, is an ultimate good, perhaps even the ultimate good.

Professor Friedman, of course, represents many things. Aside from having within him elements of Knightian doctrines, he is an exponent of that special paradigm of macroeconomics called monetarism. Although Ronald Reagan goes whole hours not thinking about the Granger-causality of M_1 and M_2, his Treasury does contain a Beryl Sprinkel and his Council of Economic Advisers does have a William Poole. On the whole, the Reagan team has given its vague blessings to the Federal Reserve when it has seemed to be approximating to some version of the monetarist rule about steady-growing money supply—except when Reagan supply-siders and election-worriers have disliked the recessionary stagnation entailed by the corset of monetarism.

The wing of elderly conservatives who are obsessed with restoring the gold standard has been repulsed by

This content downloaded from
206.189.64.126 on Tue, 08 Apr 2025 00:53:18 UTC
All use subject to https://about.jstor.org/terms

the Reagan economists, who by and large favor floating exchange rates. I am about to describe the Reagan supply-siders, who do incline toward Robert Mundell's regime of stable exchange rates; but, as the conservative counterrevolution devours its young, these supply-siders were the first to go.

• *Radical-right supply-siders*. What received the most publicity as most novel in Reaganomics was what was called supply-side economics. The prominent names here include Arthur Laffer, Congressman Jack Kemp, the old David Stockman, Paul Craig Roberts, Norman Ture, and still others.

These supply-siders did not have very distinguished academic credentials. Laffer himself first rose to public prominence when, for Nixon's secretary of the Treasury, George Shultz, he prepared (with David Ransom) a famous $1,084-billion prediction for GNP that seemed enormously high to Nixon's chief economic adviser, the mainstream conservative scholar Paul McCracken. This forecast, which was perhaps the first to employ some of the techniques of the school we now call rational expectations, was proved in the event to be as excessive as the consensus forecasters of the time had judged it to be. (Later revisions made its numbers less ridiculous, but its implied *gains* were not borne out by the revised numbers, and that is what counts in judging a forecast.) This first venture in forecasting by rational expectations foreshadowed the irrational bloopers that this school's crystal ball has so characteristically generated on those relatively few occasions when its practitioners have chanced their arm in the hazardous game of predictions.

(Otto Eckstein's Data Resources, Inc. has for many months presented alternative economic forecasts based on Robert Barro's model involving economic surprises in the growth of the money supply. The squared errors of predictions have been Gargantuan in size and irrational in composition. Related methods of forecasting by Claremont Associates of John Rutledge and others were sought by the new Reagan Administration, but the results occasioned no loss of sleep or failure of nerve on the part of the consensus forecasters and their old-fangled Keynesian macromodels. Less unlucky has been the H.C. Wainright group in Boston, which attempts to derive benefit from rational expectations. I shall come later to the qualitative batting averages of the leading rational expectationists.)

Supply-side economics is not a new story; such Keynesian stalwarts as Robert Solow (estimator of econometric production functions) and Arthur Okun (of Okun's Law fame) long worked this furrow. And a long list of scholars—including Simon Kuznets, Colin Clark, John Kendrick, Moses Abramovitz, Edward Denison, and Angus Maddison—have investigated what contribution capital formation (human and tangible) and technical innovation and development can make to a society's productivity. This standard supply-side economics deals with long and slow processes that shape a society's progress.

Kemp-Laffer-Stockman supply-side economics is a different thing. Its authors prepared for the new President an initial document, declaring that he had inherited a crisis situation (a "Dunkirk") due to governmental overregulation and disastrous overtaxation. A dramatic program of deregulation and tax reduction would be the kiss to bring back to vigorous life the sleeping beauty of American capitalism.

The famous Laffer Curve, revealed not on stone tablets but on the back of a Chinese restaurant's menu, argued the impeccable logical syllogism: if the tax rates are zero, total tax receipts are zero; if tax rates are excessively high, bordering on taking all, tax receipts are again zero. Somewhere in between, at the zenith of the arch formed by two branches of the taxation curve, revenues are at their maximum.

To this banality was added the gratuitous premise that America in 1980 was in fact already deep into the falling branch of the curve. So, less is more; cutting tax rates for three years running would raise, rather than lower, tax collections. It would reduce inherited Keynesian deficits rather than add to them.

Whatever the merits and the rationalizations, Reaganomics started out by forcing through Congress massive tax cuts: three-year Kemp-Roth income tax cuts; fast depreciation; and other corporate tax-incentive benefits.

Can the supply-side logic have prevailed? Keynesian liberals like me testified before congressional committees that there was no cogent evidence in economic history, nor plausible presumption in sensible analytics, for the contentions of the radical-right supply-siders. Conventional conservative economists like Alan Greenspan, Herbert Stein, and Arthur Burns for once made common cause with the Solows, Modiglianis, and Tobins in warning against the rashness of massive tax cuts before one knew what the economic conditions would be like in 1981–84. Even Professor Feldstein, enamored as he was of the favorable *substitution* effects on incentives that would follow from tax

This content downloaded from
206.189.64.126 on Tue, 08 Apr 2025 00:53:18 UTC
All use subject to https://about.jstor.org/terms

cuts on corporations and the high-bracket taxpayers, testified in favor of delaying final decisions on the later-year cuts.

All to no avail. Why? Because the Laffer logic was convincing? I doubt that its economic logic explains the story.

Wall Street, in early 1981, panted for the tax cuts simply for their favorable *income* effects.

White House tacticians knew that the President's landslide victory was not yet a mandate. Strike while the iron's hot, was their watchword.

Wall Street misgivings and a Reagan recession

Economists like Friedman were long on record as believing tax cuts always to be in season. They discerned a law of politics: whatever the rascals in Washington are given in tax revenues, they will spend. Cut down on what they are given, then, as a lever to force down what they spend. You will note that Friedman could believe the Laffer argument to be 180 degrees wrong and still join with the supply-siders in favoring the three-year Kemp-Roth tax cuts.

The Achilles heel of this ploy to cut tax rates to force down spending lay in the fact that a sovereign nation can finance expenditures out of deficits. To borrow back the phrase that Paul Volcker took from me, Reaganomics involved a game of Russian roulette. If the revenue slashes induced commensurate slashes in nondefense welfare spending, a conservative could argue that the game had a favorable outcome. But if, as actually happened, revenues were decimated in defiance of Laffer's Law and that shortfall did not force from a Democratic Congress the desired Reagan-Friedman expenditure cuts, then the harmful bullet came up.

Once Wall Street got its heart's desire, it quickly worked out the consequences. The tax cuts meant huge structural deficits ahead. They meant high nominal and real interest rates. They meant the crowding out of some would-be domestic investors by the need to finance Treasury bonds. They meant interest rates high enough here to attract foreign savings, a process likely to bid up an American dollar already overvalued on current account. With the dollar dear relative to the mark and yen, jobs in our export and manufacturing industries would become even harder to get than in typical periods of recession and crusading against inflation.

So, immediately in mid-1981, Wall Street did what came naturally. It dumped its bonds, bidding up interest rates in the process and knocking down the housing industry (which had in any case hardly recovered from the 1980 Carter recession).

Reaganomics thus gave America the Reagan Recession of 1981–82. The new President had inherited an economy recovering from the short 1980 recession. Within only a year, an exceptionally short lifespan for a postwar expansion, Reaganomics was able to abort the recovery. To be sure, Volcker's Federal Reserve aided and abetted the operation. But it did so with the blessing of the Reagan team, the only protesters being the supply-siders who out-Keynesed the Keyserlings in their obsession with expansion.

Rationalizations of early failure

By autumn of 1981 it became evident that the claims of Lafferism were not going to materialize, and that a large structural deficit would be the harvest of the Reagan tax cuts. The three major paradigms of macroeconomics reacted in different ways to provide interpretations of what the effects of the deficit would be.

The simple Keynesian view is that a larger deficit will mean, other things equal, lower unemployment and higher real product. Since the economy was recognized to be in recession, post-Keynesians like me testified before congressional committees in early 1982 along the following lines:

1. We told you so, that Reaganomics wouldn't work out as promised. Events are right on target.

2. Since the economy has already suffered the effects of the Reagan deficits on interest rates, let us at least get the program's benefits in employment. So enact the 1982 tax cuts, but reserve judgment about whether the 1983 cuts should go into effect.

Monetarists (Beryl Sprinkel being a typical spokesman) defended the Reagan deficits along the following lines. All that matters for aggregate demand is the size and growth rate of the money supply. So the deficit and the debt as such do not matter, provided that the Federal Reserve does the right thing. The right thing, of course, involves adhering to a strict money-growth *rule*, with no bending in the direction of financing the deficit out of contrived increments in the money supply.

This quoted argument adheres closely to the views on money and aggregate demand of Milton Friedman, as expressed for example in the *International Encyclo-*

This content downloaded from
206.189.64.126 on Tue, 08 Apr 2025 00:53:18 UTC
All use subject to https://about.jstor.org/terms

pedia of the Social Sciences. But what is good monetarism is bad neoclassical economics.

Secretary of the Treasury Donald Regan—surprisingly, for a recruit from Merrill Lynch—hewed to the line put out by the Treasury staff that there would be no "crowding-out effect" from enlarged deficits. If fiscal policy has no interest-rate effects and the money supply is controlled by the Fed, then there would indeed be no reason for deficits to impinge on aggregate demand.

But what is there in monetarism to make a monetarist deny that systematic budget deficits affect the market-clearing interest rates at which investment balances out with reduced total saving propensities? Nothing. Even Milton Friedman expects large deficits to affect the mixture of aggregate investment and aggregate consumption at the natural rate of unemployment.

A monetarist who admits that there is a crowding-out effect and an enhanced interest rate ought to recognize, on neoclassical grounds, that the velocity of circulation of money will speed up when the opportunity cost of one's cash balance is high in terms of sacrificed yield on alternative earning assets. So such a monetarist ought to recognize, after all, that a large deficit does tend to raise nominal GNP.

But how could such a sensible monetarist be differentiated from an eclectic post-Keynesian? The late Harry Johnson, who had a stochastic position on monetarism—some months he castigated it, some months he defended it—argued at the 1970 World Econometric Congress in Cambridge, England that monetarism had at least generated subtle, sophisticated analyses and measurements of the demand for money. James Tobin, rising to the opportunity, replied with the citation of a dozen studies of the demand for money, all but one of which identified a systematic positive correlation between velocity and the interest rate. The one exceptional scholar, who claimed not to be able to find any such appreciable positive correlation, was Milton Friedman.

A third strand of defensive argument appealed to the rational expectations paradigm of Robert Barro, Thomas Sargent, and Robert Lucas. Using an argument associated with Barro, which had earlier been deduced in Robert Hall's doctoral dissertation at MIT, some defenders of Reaganomics denied any interest-rate or crowding-out effects of structural deficits. Rational people, they contended, who could foresee the higher future taxes that would be put on them and their heirs to service and pay off the accumulating deficits, would reduce their private thriftiness by exactly as much as public thriftlessness was augmented by the deficit. David Ricardo had used similar reasoning to argue that people who live forever will realize that they are not a penny richer because they own the public debt. Although Ricardo was notoriously foolish in confounding short-run practicalities with long-run theoretical possibilities, he was much more cautious than modern supporters of Barro and warned against accepting his argument as being fully applicable.

Since the many taxpayers I knew were not cutting down on their consumption by as much as their share of the deficit in order to leave their heirs as well off as if there were to be no future public debt, I could not believe that respected economists took this argument seriously. I burst out laughing when one of the ablest young macroeconomists put it to me.

"You don't believe that," I asserted.

"Indeed I do, and so do all the good economists under forty," I was told.

We used to expect graduate students to lose all good sense for a spell, and were not alarmed by this since we had the comfortable reassurance that later it would come back. For once, however, I felt old; the doubt kept asserting itself that maybe in this generation the loss of practical knowledge might be permanent and irreversible.

In any case, this bizarre notion of infinite time horizon has fallen into disrepute and oblivion. Instead of the Reagan deficits being accompanied by rises in the personal saving rate, they have coexisted in 1982–84 with historically low personal saving rates. Controlled experiments are never possible in economics, but the bulk of experience seems strongly at variance with the far-fetched Barro-Hall-Ricardo hypothesis.

Disillusionment with rational expectations

The Washington supply-siders, I have already mentioned, appealed to rational expectations in their tirades against Keynesianism. Scholars more respectable than the supply-siders argued that if Washington followed creditable policies to bring down the inflation rate, the cost of doing so would be surprisingly low.

Thus, in 1980 when I attended a Minneapolis conference in honor of Walter Heller's sixty-fifth birthday, a disciple of Sargent pressed on me at the Federal Reserve Bank of Minneapolis a review by Sargent of

This content downloaded from
206.189.64.126 on Tue, 08 Apr 2025 00:53:18 UTC
All use subject to https://about.jstor.org/terms

how easy it had been in 1923 to bring the German, Austrian, and Polish hyperinflations to an end. The moral of this retold ancient story, it was suggested to me, is that in the 1980s it would be easier to descend from double-digit inflation rates to reasonably stable price levels.

My reply at the time was that the student of mine explosions cannot expect to learn much from analyses of atomic bomb detonations. The retold Sargent story, I said, made me perhaps a shade more sanguine on the possibility of doing something about an inflation like Israel's. But the historic fact that short-term changes in nominal GNP, in P $\times$ Q, have tended generally to be divided about two-thirds in Q and one-third in P, suggested that extrapolating Sargent's tale would lead to serious error.

Now, four years later, we can examine the testimony of experience. It tells, I think, against the views of Sargent, Lucas, and William Fellner. It tells in favor of the predictions by Robert J. Gordon and George Perry.

The achieved reduction in U.S. inflation rates did not come easy or cheap: the costs in lost output and jobs was just about what an Okun, Gordon, or Perry would have predicted in 1980. (I suspect that Fellner and Lucas had a salutary effect earlier, in getting Gordon and Perry to lower their cost estimates a bit below previous, more pessimistic guesses.)

Rational expectationists, such as Lucas, were too canny to risk the credibility of their theory on the rosy predictions of the supply-siders. When economic history seemed to move in the direction of their Keynesian opponents, they and Fellner could always say that the Administration's policies had not been "credible."

This is precisely the position I had predicted *ex ante*. No conclusive, or even useful, test of the credibility hypothesis would ever be possible. Even though the recession contrived by the policies of the Reagan years had exceeded in amplitude and duration anything forecast by the best consensus forecasters, there is no meaningfully formulatable definition of credibility that can be put to empirical testing. It is incredible to any rational observer of the democratic process that any central bank could announce in advance a decelerating money-supply path—and be believed that it would stick to that policy through thick or thin! Hence, much of the rational expectationist palaver was irrelevant speculation concerning how many angels can stand on the point of an ill-defined pin. When the novelty of the credibility nomenclature wore off, boredom swiftly took over.

Elsewhere in my Jerusalem lecture, I have recounted how and why events in the early and late 1930s made me recognize the need for a Keynes-like theory of effective demand, which is precisely the kind of theory not dreamed of in the philosophy of neutral-money neoclassical paradigms. Such paradigms are consistent with Say's Law and classical macro-orthodoxies. The Lucas-Sargent school of rational expectationists has recently revived precisely this view. In light of America's 1979–1984 experience, have Keynesian I-S and L-M paradigms in fact retained any usefulness in the present day and age?

The events and interpretations that I have been giving for the Reaganomics era suggest to me that we need a nonclassical theory of effective demand in order to understand the contemporaneous developments in nominal and real GNPs and the fluctuations in the velocity of circulation of money.

How could it be otherwise, when the Gordon finding remains true up to the present minute—that intermediate fluctuations in normal P $\times$ Q are still divided up in the respective proportions of about one-third and two-thirds? Lucas's market-clearing formalisms can no more account for these modern phenomena than they can account for the Great Depression itself. Since Sargent's tales of 1923 currency stabilizations turned out to have so little relationship to the cost-benefit history of our most recent crusade against inflation, it appears that there is still need for a theoretical apparatus that can try to come to grips with systematic fluctuations in aggregate output.

I don't want to be misunderstood. I am not putting in a plug for 1936-style Keynesianism, or for 1959 Keynesianism of the type represented by the bulk of the 1959 Radcliffe Committee in the United Kingdom. Nor am I intending to cast doubt on the many important findings about the considerable efficiency of speculative markets. Yes, arbitrage does work well to keep option prices near to Black-Scholes and similar theoretical formulas; yes, the spectra are quite white for price differences of stocks, staple goods, and their futures contracts. Yes, workers and entrepreneurs do catch on more quickly to what is happening than they used to do in the 1930s and 1940s.

Therefore, Robert Lucas and Thomas Sargent have done well to stress the difference between surprising policy moves and anticipated policy moves. But it is a far cry from these admitted facts to a belief in neutral

This content downloaded from
206.189.64.126 on Tue, 08 Apr 2025 00:53:18 UTC
All use subject to https://about.jstor.org/terms

money in the short run, or to an expectation that it is a useful model which assumes that major markets are always cleared and that there is little deadweight loss involved in the business cycle.

The school of rational expectations has been accorded spurious honor because of the genuine honor earned by efficient-market theory. Saying this is not to say that rational expectationism is without honor; it is to say that we must fairly identify what are its earned honors.

Here is an important example. I can fairly safely assume that a seasonal wheat price will develop in accordance with rationally expected price. For if it does not, some well-informed people can sell short or buy long and make a mint of money wiping out the divergencies from Muthean wheat price. Does that justify my assuming that the Fortune 500 entrepreneurs act like a best-informed econometrician who can work out where the GNP and price level can be expected to be? No. There need be no single well-informed analyst in the whole crowd of American executives. Admittedly, in forecasting, the consensus of the crowd can be pretty good even when no one forecaster can be counted on as reliable. (Remember the quite respectable history of odds at the race tracks!) But what I am now asserting is that the whole consensus crowd can be expected to be wrong, and to stay wrong for long periods of time, on what is going to happen to the macro data of the economy. Even if God told me in advance that the crowd was wrong, there is no way I could make money in the short run by betting against the crowd and setting it aright. The case of spot wheat prices or of General Motors calls is another matter. It is not even necessarily the case that, over several cycles or several Ponzi-processes and tulip manias, the consensus can be counted on to learn not to be too volatile and not to be too sluggish. In the long run the consensus group is dead, in the sense of being made up of new generations with imperfect memories.

Fine-tuning by the Fed

All this is not academic. In mid-1982 the first Reagan recession refused to come to an end. Unemployment exceeded 10 percent. The stock market sagged. The President's popularity plummeted. The demand regressions for M_1 were displaying large squared deviations. The Federal Reserve was being blamed for its corset of monetarism by many in the public and by a few in the Reagan Administration (e.g., by Treasury high brass, Reagan the practical man, and by all the supply-side zealots). Three bills to curb the independence of the Fed as a central bank were introduced in Congress, two by Republicans (including one by Jack Kemp).

No wonder the Federal Reserve was tempted to throw off the corset of monetarism it had been imposing on itself since 1979. Both political expediency and its perception that bank deregulation had altered the significance of M_1-to-GNP relationships conspired to motivate a fundamental change toward credit easing.

What could be expected to be the rational reaction? Monetarists would counsel: stay the course. I myself believed then that, despite rational expectationism, the Fed had a window of opportunity to turn the economy around. I recognized that there was a risk of such actions igniting fears of inflation, because of the possibility that Wall Street would react perversely to signs of central bank expansionism. If that happened, the Fed's departure from monetarism would in fact raise nominal and real interest rates.

In historical fact, it was all a tremendously successful maneuver. Wall Streeters rejoiced at the Fed's move. They bid up bond prices. Immediately, common stocks took their cue from bonds. The Dow-Jones index soared in August 1982. Again, in October 1982, real and nominal interest rates fell. For the year following August 1982, stock prices generally rose by 60 percent.

This massive reduction in nominal and real interest rates brought the residential construction business back to life. Durable goods like autos began to sell well again. In the standard textbook pattern, the Fed's easier-money program brought the 1981 Reagan recession to its end in November 1982.

It was a consumer-led recovery. As usual, plant and equipment investment were lagging time series. As usual, the deceleration of inventory decumulation helped to end the recession.

Looked at another way, the colossal budget deficit finally had its Keynesian stimulating effects once the high real interest rates occasioned by fears of future deficits were offset by easier Federal Reserve credit policy.

The surprisingly strong recovery

Except for orthodox monetarists, everyone was happy: speculators, investors, job seekers, profit-seeking entrepreneurs, politicians in the White House. The ex-

This content downloaded from
206.189.64.126 on Tue, 08 Apr 2025 00:53:18 UTC
All use subject to https://about.jstor.org/terms

pert consensus forecasters, weighing the evidence from early 1983, predicted only a modest first-year recovery. Martin Feldstein, chairman of the Council of Economic Advisers, began with a pessimistic forecast that was below what the evidence suggested to the consensus crowd. Events soon forced him to rejoin the crowd.

By the middle third of 1983, the real economy was growing at the exuberant annual rate of 8 to 9 percent. In the autumn the Fed, as if determined to infuriate hard-line monetarists, turned down the rate of M_1 growth below its boisterous spring pace. (There is no particular warrant, in modern economic theory or in historical experience, for the view promulgated by monetarists that a high standard deviation of M growth is as such depressing to an economy. So their explanation for why the Fed's post-1979 attempt at monetarism had produced the 1981 recession is, to say the least, problematic. Also problematic is a naive identification of the cause of high M variance as being stupidity or duplicity by the Federal Reserve authorities. When the demand functions for the various definitions of money—M_1, M_2, M_3, M_0, . . . ,M_{99}—are shiftable, it is not surprising that policy should result in high standard deviations in their growth rates. As Arthur Burns observed, studies of experience do not bear out the view that the money market is unable to adjust to temporary aberrations in M growth rates.)

Just when sophisticated monetarist models were incurring large errors of estimation, Milton Friedman's speeches and *Newsweek* columns were warning against an inflationary recession by mid-1984. Karl Brunner, more cautiously, expressed similar misgivings. I find it hard to fabricate a plausible macromodel in which separate channels of causation operate in parallel, according to which the lag between dP/dt and dM/dt is longer than an independent lag between dQ/dt and dM/dt. So on *a priori* grounds the probability of both inflation and recession in early 1984 seemed low to me. Under sensible filtering, the actual pattern of dM/dt did not seem to me very frightening. But—and this is my major point—when one took into proper account the vast bulk of the relevant non-M data, a nonmonetarist would bet with strong odds against a recession before the November election.

Summing up the Keynesian tide

Readers will by now have recognized that my title was cunningly chosen. I have used "Evaluating Reaganomics" as a device for testing different macroparadigms against the evidence of recent experience. Here is the verdict:

A Hamlet-like student, poised in neutral equilibrium between eclectic post-Keynesianism, monetarism, and rational expectationism, would have to be pushed in the direction of post-Keynesianism by the brute factual experiences of America in the 1980s.

That is my message. That is my finding. Economics is not an exact science, so I cannot prove the correctness of this result in the way that one proves the Pythagorean Theorem or confirms the constancy of the speed of light.

Let me also point out that there are U.S. patterns of behavior in the 1980s that still require new explanatory theories and that mandate some eclectic blending of the competing schools of thought. We don't really understand why the short-term real rate of interest stays so high these days, or for that matter why long-term real interest rates stay higher than they seemed to be in most of the last many decades. (Why, by the way, don't such high real rates choke off homebuilding? One minor reason seems to be people's myopia: if mortgage lenders offer buyers floating-rate loans, and structure them to involve low initial monthly cash payments, apparently buyers will purchase new and old homes.) Why the dollar does not float downward is still a puzzle. Its strength on capital account could use a plausible theory, and one that permits prediction of the future.

When I say that post-Keynesianism has received some vindications from experience, I am also saying that some of the standard hypotheses of neoclassical microeconomics have been borne out by experience. Thus, good-sense supply-side economics would expect that tax reductions which reduce the wedge between pre-tax and post-tax returns might well strengthen investment demand, *ceteris paribus*. The record, I think, bears this out. Adjusting for cyclical factors and for the height of real interest rates, we do seem to observe stronger demands for equipment than past regressions would call for. The Reagan tax cuts, I would suppose, do explain part of this story.

Should we economists be pleased that for once, at least, our mainstream theories have seemed to be about right? Yes, perhaps. But I remind myself that science is most exciting when new findings are being made. There is still plenty of excitement—too much excitement, some citizens would say—in the world of political economy.

This content downloaded from
206.189.64.126 on Tue, 08 Apr 2025 00:53:18 UTC
All use subject to https://about.jstor.org/terms