

# Overview

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Here we go again! This conference continues the chronic debate among economists and central bankers on fundamental issues of monetary policy: on the goals, capabilities, strategy, and tactics of demand management, on what we have or should have learned from the 1980s, the 1970s, and indeed the whole postwar period. The issues are familiar: rules versus discretion in policymaking; reactive versus fixed settings of instruments and targets; the importance, feasibility, and requisites of credibility of announced policies; the choice of instruments and targets; the unemployment-price tradeoff menu over short and long runs; the values to be placed on the choices offered.

Several papers by guest economists give the central bank hosts quite a beating. The Fed is accused of 'time inconsistency' specifically over-accommodating inflationary shocks and pressures in the short run, sacrificing its long-run goals and credibility to political expediency. These economists perceive the central bank's **tasks** and choices to be much simpler than the Fed itself has viewed them. Martin, Burns, Volcker, *et al.* will with some justice detect Monday-morning quarterbacking in these criticisms.

*Logical program* but *uncompleted synthesis*. The choice and order of the topics speak well for the logical thinking of the economists who organized the program. On the first morning we heard about the causes of inflation, then about its costs to society, and finally about the costs in unemployment of avoiding inflation. Our second session concerned how to conduct monetary policy so as to achieve price stability, at least in the long run, with minimum unemployment cost. Bob Hall used one of economists' favorite expository graphs, displaying a frontier of feasible choices of the two "bads," unemployment and inflation. From Fischer's paper we might perhaps distill a social indifference map to show us how to find the optimal choice within Hall's tradeoff menu.

Perhaps to the disappointment but hardly to the surprise of the organizers, so satisfying a synthesis did not quite jell. The separate pieces, *a fortiori* the discussion, meshed imperfectly. For example, most participants dissented from the optimistic monetarist views of Mishkin and McCallum on the causes of inflation and the unemployment costs of disinflation. But the critics did not agree with each other. Fair, Hall, and Gordon would all draw the feasible frontier differently, and they would not even use the same axes. Fischer's paper gave only qualified support to Hall's view, apparently shared by Mishkin and McCallum, that zero inflation is a desirable, as well as feasible, long-run goal. Pragmatic discussants like Nordhaus, Gordon, and Blinder would gladly settle for fairly stable single-digit inflation.

*Forward commitments in monetary policy: the issues.* Much of the debate at this symposium concerned the possibility and desirability of advance commitments in monetary policymaking. As a guide to this debate, I would distinguish several of its dimensions:

- How permanent should numerically specific commitments be? Forever, e.g., 3 percent per year growth in something for all time? Or periodically reconsidered and changed, like the Fed's targets for the aggregates?
- In what time series should commitments be expressed? Macroeconomic goal variables like unemployment, real GNP, prices, and inflation? Intermediate monetary and financial indicators like the monetary aggregates, credit, or interest rates? Instruments directly under central bank control, its balance sheet, its discount rate, or the federal funds rate? Magnitudes almost directly controllable, total or unborrowed reserves, or the monetary base?
- What role, if any, should actual observations and forecasts play in determining the actions to which the policymakers are committed? Should policy be blind to new information, on the grounds that determined disregard of current events and outlooks contributes to credibility? Or should policy respond to such information in pre-announced ways? Or should policymakers retain discretion to cope with unforeseen, perhaps unforeseeable, circumstances?
- Should the objectives, strategies, and tactics of the central bank be explicitly and promptly announced? Or does judicious use of confusion help monetary policy achieve its social goals?
- What should be the constitutional status and political responsibility of the monetary authorities? Independent, or answerable to the executive or the legislature?

**Rules versus discretion.** Three papers—by McCallum, Mishkin, and Hall—advocate rules, imposed or self-imposed, well publicized and understandable, numerically definite, and permanently binding. McCallum and Mishkin seem to prefer non-responsive rules, blind to observed outcomes and forecasts; anyway they see no advantage in reactive policies. Their reasons are mainly *a priori* theoretical rather than empirical. Hall, however, sees great superiority in a reactive rule. His "elastic price standard" is a very interesting suggestion, ingeniously documented by 30 years of macrodata—altogether a refreshing contribution to this well-worn subject.

**Targets and instruments.** This overviewer was gratified to find in the papers by Ben Friedman and Hall such emphatic recognition that operating instruments must somewhere in policy strategy and tactics be related to goal variables of ultimate value. Friedman shows, not for the first time, the virtual uselessness and irrelevance of intermediate monetary aggregates as targets. The aggregates have no objective importance and carry little information not otherwise available; yet they, just like variables of macroeconomic importance, can be controlled only indirectly, by reactive manipulations of instruments. In glossing over this fact, McCallum and Mishkin illustrate Friedman's complaint that economists facilely and fallaciously assume that the M's of their simple models are directly controllable or that actual central bank instruments have all the properties of those model M's. Our profession seems to be reaching consensus and clarity on these points. So perhaps the grip of mechanical monetary-aggregate monetarism on policymakers, politicians, journalists, and markets, which has already been loosened, will at last be broken.

The substitution of nominal GNP—or even better, Bob Gordon's candidate, final sales—for monetary aggregates would be an improvement, because it would allow the Fed to offset velocity shocks without risking credibility. (If cosmetics would smooth the transition, the new targets could be called "velocity-adjusted aggregates.") But Hall's results show that a permanent rule fixing numerically the target path of nominal income could be a harsh recipe for handling OPEC and other price shocks. It mandates a 1 percent loss of annual output for every 1 percent excess of price index over target. A more accommodative response, followed by tightening gradually to remove the price bulge, seems indicated by Hall's simulations. Of course, nominal income could be used, like the aggregates now, as a periodically changeable numerical target. For example, each annual application of Hall's elastic-standard policy could be expressed and announced as a nominal income, or final sales, target for a year ahead.

**Reactive rules as discretion.** A permanent numerical rule for any nominal quantity, instrument or target, will in this overviewer's view some day become intolerably disastrous, with probability approaching one, because of big or cumulative shocks. This is true of the monetary base, M1, credit, nominal GNP, what have you. Even a permanent reactive rule, like Hall's formula, can get into trouble. It is as hard to specify in advance **policymaker's** responses to all contingencies as it is to write those Arrow-Debreu contracts so beloved of economic theorists. Some hawks condemn Volcker for "blinking" when the going got tough in the summer of **1982**. I agree with Bill Nordhaus that the Fed's announced policy of October **1979** did not—could not—say what the Fed would do in case of Third World debt crises, big negative velocity shocks, and domestic financial troubles. I agree with Alan Blinder that economists' conceptions of commitments to complex feedback rules are allegorical or stylized descriptions of 'discretion.' My personal view is that the Fed has to have discretion to deal with contingencies, like those of **1982**, within its general commitment to macroeconomic goals shared with Congress and the Administration.

**Credibility.** There is something in the idea, but in my opinion less than McCallum and Mishkin think. There is something in it when the message gets **through—not** just to the financial community, a skeptical audience obsessed with credibility, but to business managers, workers, and unions who actually decide or negotiate prices and wages. As Dr. **Schlesinger's** informative address reminds us, the German authorities aim at the critical audience. When the Bundesbank tells management and union leaders the implications of its monetary policy for the year, it is carrying out simultaneously a 'credible-threat' policy and an 'incomes policy.'

In the decentralized wage- and price-setting institutions of the **U.S.** and U.K., threats by Volcker and Thatcher seem to have brought little or no amelioration of the time and cost of disinflation. Threats to everybody in general but to nobody in particular are evidently not very effective. McCallum, like other partisans of 'credible-threat' strategy, says that strategy wasn't really tried. Well, we never have perfect experiments in macroeconomics. Blinder's quotation from a previous symposium somehow struck me as right on the mark! Policymakers in a representative democracy can never tie their own and their successors' hands as securely as the advocates of permanent rules would like. Economists who would engrave their concepts and numbers in the Constitution 'have a lot more confidence in the stability of economic structure and in their understanding of it than history justifies.

**Monday morning quarterbacking.** A theme common to McCallum, Mishkin, and Hall is that the United States could easily have enjoyed a much better price or inflation record, along with an unemployment record as good or even better. For the first two of these authors, this is deemed an obvious truth. Greatly to his credit, Hall seeks to demonstrate it empirically.

Most likely we could have done better, but there are several reasons to believe that even Hall overstates the case:

- 1) Note that Hall's own simulations make unemployment no lower, generally higher, under his rules than actual unemployment every year before 1979, except 1975 under the 'dove' policy—as nearly as I can tell from his Figure 5. The improved outcomes come mostly since 1979. Hall's simulations say that we recently suffered much too high unemployment for the disinflation achieved.
- 2) Hall assumes that policymakers like William McChesney Martin could have known in the 1950s and 1960s what Hall knows now from a structure estimated on data through 1983. Hall knows, for example, that the "natural rate of unemployment" has been 6 percent all along, but no observations available to Martin or Burns or Heller told them that. **Shouldn't** Hall have calculated his simulations from "rolling" regressions and forecasts, using no data not available to policymakers each year? Moreover, uncertainty and fluctuation of the level of the "natural rate" are surely major problems in demand management, omitted from Hall's model.
- 3) Hall plots in Figure 7 actual results far above his variance frontier. The price variance is greatly exaggerated by taking it around its mean rather than its trend. Given that Hall's preference for **price** stability over inflation stability rests on his concern for fairness to long-run nominal savers, the measure he should use is the variance of the *ex post* real long interest rate.
- 4) The sharp price deflations in Hall's simulations may be harder to achieve and more devastating to aggregate demand than the model, estimated without such observations, contemplates. We cannot be sure the short-run Phillips curve does not become very flat at zero growth of nominal wages.
- 5) Actual inflation, especially bulges that accompany OPEC-like shocks to real wages and profits, may leave in their wake more upward wage and price pressures than Hall's Phillips curve allows. He optimistically assumes that public confidence in his policy would wipe the terms for **ex**-pectational and institutional inertia out of his wage and price equations.

6) I strongly suspect that errors of monetary control and forecast are bigger than those of Hall's model, and I just cannot follow his argument that we need not worry about the precision of the relation of the Fed's instruments to aggregate demand and prices because the "black boxes" of the forecasting profession will handle the problem.

*Tradeoff menu.* The orthodox view that there is no tradeoff to policy in the long run is, I gather, accepted by all authors but Fair. McCallum and Mishkin think that the long run is pretty short. Fair challenges orthodoxy; he says he has found a long-run tradeoff. It seems, however, to be between price level and unemployment, rather than between inflation and unemployment.

Fair's tradeoff seems to be the upward slope of the conventional aggregate supply curve, used in Mishkin's diagrams. Evidently the Fair Model (No. 1 in his paper)—although it has price inertia from the inclusion of lagged wages and prices in his equations—has no built-in *inflation* inertia. That is, the contractual, institutional, and expectational lags in wage and price formation would not prevent the rate of price increase from subsiding even at low maintained rates of unemployment. Evidently the model has no steady state with an inflation rate other than that consistent with the time trend in the money wage equation, a price inflation rate that will vary inversely with the productivity trend. In the Fair model, above-trend inflation occurs while the price level is adjusting to shocks or policies; when adjustment is complete, it stops.

Fair may have shown the econometric superiority of his model over the two opponents he sets up. I do not see what this demonstration implies about the existence or duration of a Phillips tradeoff. While I concur with Fair's preference for a structural approach to wages and prices, I find it hard to believe that the mechanisms of inflation inertia and expectations have not changed over the sample period, and hard to accept a "natural" inflation rate determined by an unexplained trend in nominal wages.

Mishkin proclaims the truth—in all macro theories—of Milton Friedman's dictum that inflation is always and everywhere a monetary phenomenon. Well, who could doubt it? Inflation is by definition a general rise in commodity prices in terms of the *monetary* unit. A rise in  $MV/Q$  is tautologically a rise in  $P$ .

The famous dictum may be a useful antidote to the naivete or willful blindness of many politicians and some economists. In small open economies with underdeveloped securities markets, government deficits are automatically monetized. They depreciate the exchange rate and generate domestic inflation, often hyperinflation. The malady is jointly fiscal and

monetary. This is not a description of the inflation problem in the United States. Here, unguarded repetition of the dictum too often conveys the message that inflation is easy to prevent and to cure, if only politicians and central bankers would be resolute and farsighted.

That message is terribly misleading. All serious macroeconomists agree that monetary policies and quantities have important effects on aggregate demand. They do not all agree, as this symposium illustrates, that monetary policies and events affect solely prices and have no effects on output and employment. That inflation is a monetary phenomenon does not exclude wage- and price-setting institutions as additional "causes" of inflation, in that they impose severe real costs as side effects of monetary anti-inflationary medicine. As Nordhaus pointed out, the shape of the "AS" curve, shifted as it frequently is by supply shocks, depends on the degree of monetary accommodation. That degree has been the big policy issue of recent years, and the critical issue of this conference. Reminding us of Friedman's aphorism contributes nothing to its resolution.

*The social value of price stability.* Fischer provides an updated catalogue of the costs of inflation. Its relation to the other papers is to guide the assignment of social values to price stability and high employment, to help us draw indifference curves tangent to policy frontiers. Fischer points out how the costs of inflation depend on society's institutions—tax laws, interest ceilings, indexations—and their adaptability. Of course, changing some of these institutions would also, by making prices more or less volatile, for example, alter a Hall or Taylor variance frontier.

When Fischer and other authors list or estimate 'costs of inflation,' I wish they would more consistently tie them to actual feasible policy choices. When inflation is a joint product of other disasters, it should not be charged with the unavoidable costs of those disasters. It should be charged only with the extra costs, if any, attributable to handling them in an inflationary way. The Weimar republic had to pay reparations, and we had to pay tribute to OPEC. These were not 'costs of inflation.' Confusion on this point, along with failure to understand that inflation raises the incomes you receive as well as the prices you pay, may be sources of popular anti-inflation sentiment. Fischer's costs are not in aggregate enough to explain their strength.

Certainly the 'money triangle' is not the source of popular passion. As Shiller remarked, Fischer did not point out here, though he has done so elsewhere, that depriving the Treasury of seignorage would necessitate additional explicit taxes, with their own distortionary costs. This would be true whether the loss of seignorage resulted from price stability or

