

Discussion

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Introduction

Reflecting the reward structure in academia and sincere disagreements over the conduct of monetary policy, criticisms of Fed actions are in ample supply. More generally, there is little doubt that academic economists and monetary policymakers are frequently disappointed with one another. Part of the problem, according to Henry Wallich, is that "academic economists do not have to live with their mistakes and some of them, therefore, are prone to understate the degree of uncertainty attached to their analyses" (1982, pp. 242-243). Specific impediments to a mutually beneficial exchange of views, which frequently surface at and frustrate participants in conferences such as this one, include the emphasis accorded shorter run technical issues related to the "plumbing" linking instruments and targets, the alleged robustness of the latest regression results, and critical evaluations of the Fed's performance by "outsiders." Ed Kane's insightful discussion of the latter, particularly his observation that depending on one's perspective and preferences, "the [1979] change in FOMC policy framework can be portrayed as spectacularly successful, relatively unimportant, or absolutely disastrous in its effects," vividly illustrates the gulf to be bridged.

In organizing my thoughts, I found it helpful to think about two issues: (1) Why would the Federal Reserve Bank of Kansas City and the System as a whole, already in possession of a highly competent staff with many ideas and reflections of its own, add to the growing list of post-1979 conferences on monetary control; (2) What does Kane's assessment of policymaking hold for the seemingly more mundane

technical, empirical, and analytical issues continually facing the staff and policymakers. The resulting collage follows.

Kane's Psychoanalysis of Fed Policy

Kane's paper brings together a number of important themes which he has developed over the past decade, including the scapegoat thesis and the regulatory dialectic, and assesses their implications for a variety of micro and macro, static and dynamic issues surrounding the formulation and implementation of monetary policy. The resulting careful synthesis provides a more solid and, therefore, more reliable frame of reference for real-world discussions of monetary control issues. Simply put, Kane believes, as do I, that the development of useful theoretical and empirical analyses of the policy process is not often enhanced by studies that abstract from salient features of the political and economic environment within which policy is made or by the natural tendency of policymakers to cover their trails and tails. When combined at the formal analytical level with the ever present and pernicious *ceteris paribus* assumption, which often seems to be taken seriously in the professional literature, the resulting **partial-equilibrium**, static, macro analysis of various plumbing issues, such as the optimal structure of reserve requirements and reforming the discount facility, abstracting as it does from the dynamic microeconomic adaptations Kane emphasizes, is subject to important limitations.

At the more general and practical **policymaking** level, Kane's analysis frames and examines the basic issue clearly; whether or not technical adjustments in policy procedures can alter economic outcomes depends to a considerable degree on whether procedures have ever, or can ever, sever the relationship between the so-called ultimate and proximate causes of economic fluctuations. Kane's sobering reflections on this issue suggest that logically prior to designing any alteration in existing procedures or regulations is a recognition that monetary policymakers have and do play a political role in the broadest sense of that term. Reforms that ignore this role may alter the appearance but not the reality of **policymaking**.

Put more dramatically, are the frequent misses of established targets and the intransigence displayed by policymakers regarding **often-suggested** procedural and regulatory reforms, the result of incompetence, corruption, or bad luck? I think not. In general, actual or

perceived constraints flowing from the political-social environment combine with uncertainties surrounding the economic outlook and central features of the transmission mechanism. The resulting tension between appearance and reality in a complex policymaking process, developed by Kane, may help to reconcile policymakers' calls for caution and eclecticism in decision making with policy critics' charges of myopia and amnesia.

At a deeper level, Kane's analysis has Kuhnian overtones: why did the Fed change procedures in 1979 when the "technology" had been on the shelf for over a decade and economic performance had been deteriorating for some time? Is the regime change or threshold definable and predictable *ex ante*? Less philosophically, are the adaptive forces Kane discusses of the "bang-bang" or evolutionary (gradual) type? On what does the presumably variable pattern and speed of adjustment depend? How precisely do the shock-absorbing properties of different procedures and regulations, discussed extensively by Tinsley (1981), change the distribution of costs and benefits across the Fed's clientele, emphasized by Kane? Are there any predictable aspects of the changing distribution and the resulting adaptive behavior? As Solow once said in another context, an adult could spend a lifetime trying to answer such questions (1979, p. 208). Nonetheless, as Bill Dewald has noted, knowing what you do not know and need to know is the beginning of knowledge (1982, p. 248).

To avoid misunderstanding and to introduce the remainder of my remarks, acceptance of Kane's basic thesis does not in my judgement render this conference, and others like it, nugatory. First, research should not be unduly constrained by what appears politically feasible today; tomorrow may require or tolerate changes which appear remote today. Second, as Willett and Laney (1982) have argued, positive analysis which indicates that political forces have shaped policy (and often produced procyclical and, on balance, inflationary outcomes) does not imply that the *only* way to produce a less destabilizing policy is to deal directly with the underlying political and social forces. My own perception is that in the short run the Fed operates in a zone of feasible actions with boundaries that are not unduly narrow or wholly exogenous. The resulting constrained optimization problem admits discussion of a host of issues regarding monetary policy in the 1980s to which I will now turn.

Current Research and Policy

It is doubtful that many students and practitioners of monetary policy view the last 2½ years with pride. While the infamous "incomplete success" at Desert One would seem an overly harsh analogy to apply to the volatility of interest rates, money, and economic activity experienced, the sterility and obfuscation of official reviews do tempt one. Leaving the policy record aside for the moment, System personnel have provided numerous useful explanations and evaluations of the intricacies and various technical aspects of the new procedures. However, the aura of precision and coherence which often results from such attempts to make complex matters understandable and tractable for both insiders and outsiders, belies the "judgement", "flexibility", and yes, even ad hocery, which I suspect permeates aspects of the Bluebook, the staff's two-volume and subsequent studies of the current operating procedure, and the actual execution of policy.

To be more specific, the economic rationale for multiple monetary aggregate targets is not obvious. Moreover, do the target ranges for the various aggregates reflect standard control errors or the degree of maneuvering somehow thought desirable? What are the analytical foundations for the shifting emphasis accorded various aggregates? Under what specific circumstances can such vacillation be shown to lead to improved policy? The ambiguities appearing at the strategy level are aggravated by questions raised by the various "adjustments" made in the nonborrowed reserve path and the borrowing assumption. Are the so-called technical adjustments to the path in the face of multiplier errors mechanical and consistent? If they vary in timing or size, what explains the variable adjustment? Similar questions apply to the more fundamental adjustments to the reserve path generated by the deviation of actual monetary growth from target. Of even more interest to monetary economists, are the relationships in the Bluebook among the relevant impact elasticities, implicit lag structures, and the "reentry paths" for the aggregates once they are off target well defined?

Unfortunately, various aspects of the staff's highly competent two-volume study of the new procedures (and various followups), suggest the analytical and empirical foundations for the existing strategy, and therefore the evidence against proposed alternatives, is not very robust. In particular, the poor performance of the borrowing, interest rate, and exchange rate equations which the staff uncovers, along with the questions recently raised about the Board's monthly model by Ander-

son and Rasche (1982), and the much-examined and still controversial perturbations to money demand, make one wonder whether the standard economists' tenet — "It's an empirical question" — is useful or useless in this context.

The degrees of freedom used up in identifying and estimating financial models, emphasized by Cooley and LeRoy (1981), and the finding that favorable simulation properties for money demand equations seem to be obtained only in the presence of unrealistically slow adjustment speeds (Offenbacher and Porter, 1982), reminds me of something Jim Pierce said some years ago. Reflecting on the ready availability of computer terminals, sophisticated software, and data banks, he speculated that every important macro variable had at some time and place and in some context been regressed against every other variable, thus producing a range of results whose implications for policy and future research were far from obvious. Along the same lines, I wonder about the staying power of the latest apparent winners in the Triple Crown of monetary aggregate correlation derbies — i.e., velocity equations, pseudo-reduced form equations for GNP, and demand or supply equations — namely, Ben Friedman's Credit, from the Radcliffe farm, and Bill Barnett's Divisia, from the Theil farm. Presumably, the tendency to regress one endogenous variable on another and Lucas's critique of policy-related econometric work (1976), coupled with Kane's less restrictive and, therefore, more general theorizing about the dialectical process governing the structural relationships linking the controllers and controllees, will produce healthy doses of both humility and skepticism regarding these and related matters.

Looking Ahead

The ongoing phase-in of the Monetary Control Act will be a force dominating discussions of monetary policy in coming years. Many Fed staffers agree with the position advanced by Bob Rasche; as reserve requirements become more uniform, universal, and contemporaneous, predictions of the relevant reserve aggregate multipliers will improve and the short-run precision of monetary aggregate control will increase significantly. Implementing some widely discussed reforms of discount policy are also believed to be conducive to tighter short-run control. In my judgement, the absence of an adequate model of the dynamic micro behavior of depository institutions, along with the usual

aggregation over the epidemic-like process which characterizes these intermediaries' collective adjustments to shocks, suggest the analytical and empirical macro models which point towards large payoffs to various regulatory and procedural reforms need to be supplemented by models which take account of micro dynamic factors. I take this to be one of Kane's central points. To illustrate, if we don't know anything specific about the volume and composition of reserves depository institutions desire to hold, how can we know whether a given reserve requirement ratio is effective or not, and, if effective, what adaptations are likely?

Although arguable, it does not seem to me that the above perspective immediately and inescapably leads one to the position recently espoused by Federal Reserve Bank Presidents Mims (1982) and Solomon (1981), and by Don Hester (1981). They argue that ongoing financial innovation and technological advances, along with increasing international integration, are in the process of rendering some or all of the monetary aggregates obsolete as policy targets. While some aspects of the underlying arguments are well taken, especially the call to "open up" our traditionally closed-economy models, it must be acknowledged that the growth rate of velocity on average in recent years, as Bob Weintraub and other monetarists have forcefully pointed out, has not yet deviated significantly from longer-term trends. Moreover, as detailed in some recent work by the Board staff, an aggregate encompassing the volume of the means of payment still performs about as well or better than other aggregates in the correlation derbies mentioned above (Offenbacher and Porter, 1982). To be sure, this empirical work does uncover some troubling problems; many coefficient estimates do not seem reasonable and numerous equations do not appear structurally stable over time. Looking ahead, I am inclined to believe that developments such as deposit sweeping and Super NOWs will plague such empirical work even more in the future.

More generally, my own work suggests that the forces leading to and resulting from various changes in regulations and procedures, as emphasized by Lucas and Kane, play an important role in empirical work in this area. To illustrate, our models usually include data points covering most of the last 25 years, a period when the Fed's policy rule imparted considerable flatness to the short-run LM curve. Assuming the current strategy produces a more positively-sloped function, it seems unlikely that our models will be insensitive to such a switch in

regimes. This is, of course, consistent with the analysis in Carl Walsh's paper. Along the same lines, there is evidence that the once important credit availability effects, which were associated with the movement of *nominal* interest rates above Reg Q and usury ceilings, have been reduced significantly by innovation and the advent of deregulation. Preliminary research suggests that the changing relative role of nominal and real after-tax interest rates, which may help to explain part of Kane's query about past and current levels of the real rate, has dramatic effects on the short-run dynamic impact of monetary policy on the economy.

An additional potential problem for both monetary targeting and our empirical work has been previewed by the changing character of M2. Over the last several years the proportion of the nontransactions component of M2 bearing market-related yields has risen from essentially zero to about two-thirds. One result appears to have been a more stable growth pattern for this aggregate in the face of considerable fluctuations in interest rates and economic activity, and the deterioration of its performance in some of the types of equations mentioned above. If transactions balances in the 1980s increasingly bear market-related yields, as seems likely, similar changes in empirical relationships may be observed. Moreover, the resulting steepening of the LM curve will presumably amplify the real effects of financial shocks.

Some Concluding Thoughts

Recognizing that the abiding short-run focus of policymakers has rarely meshed well with the abstractions traditionally embedded in economists' models, Kane has encouraged us to examine various monetary control issues from a deeper, broader, more forward-looking perspective. As with many such conceptual exercises, the conundrums which emerge are many and clearcut answers are few. As a result, questions associated with defining and measuring money, estimating supply and demand functions, and designing improved regulations and procedures will continue to plague us. Like with the video games Kane mentions, frustration is part of what addicts us to the study of money and macroeconomics.

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