

Editor's Introduction

The Federal Reserve Bank of St. Louis, held its first Economic Policy Conference in 1976, in conjunction with the Center for the Study of American Business at Washington University. Although this annual event has grown over time it has maintained a focus on many of the principles that Ted Balbach fostered during his tenure as director of Research. An open exchange of ideas was high on that list and an annual gathering of prominent academics to discuss important concerns of central banking was but another way in Ted's mind to move intellectual debates forward. With his retirement in October 1992, it was fitting, indeed essential, that the beginning of a new chapter in his life be marked with a tribute to the legacy he has left for all of us. Accordingly, the papers in these proceedings reflect economic and policy issues that were never far from Ted's attention.

The conference's first session dealt with two issues that, more than any others, dominated Ted's career at St. Louis: the effects of money on economic activity and the commitment of the central bank to a goal of price stability. Robert H. Rasche, in "Monetary Aggregates, Monetary Policy and Economic Activity," investigates the large and unanticipated shifts in money velocity during the 1980s that led to large errors in predictions of inflation and growing sentiment that the demand-for-money function is unstable. Rasche's paper approaches this task from three perspectives: controversies of the 1960s and 1970s that have been resolved, empirical failures of reduced-form spending equations in the 1980s and the short-run effects of changes in the growth rate of the nominal money supply on economic activity.

In reviewing the historical controversies, Rasche concludes that current mainstream macroeconomic thought embodies the important elements of the original Andersen-Jordan equation: changes in nominal government spending do not produce a permanent change in nominal income (or velocity) unless accompanied by a change in the growth

rate of the nominal money stock. He also concludes that shocks to the level of velocity are permanent. The first point was contradictory to the dominant Keynesian paradigm of 25 years ago and the latter anticipated the now commonplace care that is given to proper differencing of the data and the problem of spurious regression relationships.

Just as the world of monetary policy began to take St. Louis-type arguments seriously, the 1980s produced a sharp break in trend velocity that discredited the Andersen-Jordan equation in the minds of many. In the second section of his paper, Rasche explores whether this break more likely reflects a rejection of the underlying economic relationships or a specification error arising from a Lucas-type structural change. Rasche concludes in favor of the latter, arguing that a sharp break in inflationary expectations explains the break in trend velocity. Rasche also discusses how, if this explanation is correct, simple money growth rate rules for policy will be dominated by rules with feedback of the sort described by Meltzer and McCallum.

In the last section of his paper, Rasche investigates a current controversy: whether changes in nominal money growth affect real output. After evaluating several vector autoregression (VAR) models, Rasche concludes that there is evidence in support of both permanent real output shocks (of the real business cycle variety) and permanent money growth shocks on real output. Thus though the role of money in explaining fluctuations in real activity is not rejected, Rasche finds room for contributions from other sources as well.

In his commentary, Julio J. Rotemberg focuses on Rasche's claims of finding a stable money demand function. After estimating velocity regressions in the spirit of Rasche's analysis, Rotemberg finds that the apparently stable long-run specification coincides with an "incredibly unstable" money-demand function at shorter frequencies. He also finds that the residuals of such relationships are highly correlated.

As reasons for these unsatisfying results, Rotemberg renews an earlier call from our 1989 conference to use Divisia monetary aggregates in place of the conventional simple-sum measures. Although he only approximates a crude Divisia measure of M1, the large break in observed M1 velocity in the early 1980s is reduced substantially when based on data from a weighted monetary aggregate. Thus other explanations beyond those offered by Rasche may explain the velocity puzzle or there may be no puzzle to explain. Rotemberg also extends Rasche's VAR analysis with additional support for recent studies that have shown asymmetric effects of monetary shocks on output—negative shocks reduce output but positive shocks do not increase output. This asymmetry and other asymmetries affecting interest rates are left as important issues to be investigated in further work.

With this backdrop on how velocity has behaved over time and how monetary policy apparently affects economic activity, W. Lee Hoskins offered a philosophical overview of how a central bank should conduct itself. In his "Views of Monetary Policy," Hoskins drew on his previous experience as president of the Federal Reserve Bank of Cleveland and voting member of the Federal Open Market Committee to criticize central banks for their tepid commitment to the goal of price stability, if not their demonstrable bias toward inflationary policies. These flaws in the charters of most central banks can be overcome, in Hoskins' view, only by stating a specific mandate to achieve price stability, giving the central bank the necessary independence to achieve that goal and holding it accountable for any failure to do so.

With much of the academic literature focusing on technical issues (for example, interest rate vs. money stock targets) or public choice arguments to explain central bank failures to achieve price stability, Hoskins advances "a simple and less elegant explanation...that central bankers are suffering from a Keynesian hangover." The point is that, as products of a generation that learned an economic model in which central bankers should attempt to manage fluctuations in aggregate output, as well as inflation, modern central bankers are merely employing the training they acquired 20 or more years ago. Thus when the economy is weak, their vintage of training indicates a need for monetary stimulus—even if it ultimately will cause higher inflation. This view, which does not incorporate more re-

cent evidence on the dubious effects of monetary stimulus on output, also fits nicely with the views of elected officials more concerned with near-term issues, such as employment, than with the long-term issue of inflation. In this environment, an inflationary bias by central banks is not difficult to understand and the broad reforms Hoskins suggests are tied to the argument's main themes.

In his commentary, Georg Rich largely agrees with Hoskins' statement of principles but wonders whether the gap from theory to practice can be bridged, and if so, how. Rich first argues that a mandate to achieve price stability, such as embodied in legislation proposed by Rep. Stephen Neal (D-N.C.), is not sufficient to achieve zero inflation. Instead, Rich argues that an operational rule, perhaps of the form proposed by Meltzer or McCallum, is needed to keep an accumulation of short run operational decisions by the central bank from wandering too far from a long-run policy that will maintain price stability. At the same time, Rich recognizes that the need to react to shifts in money demand raises some doubts about the desirability of the slow, mechanical paths of adjustment proscribed by these rules.

Rich raises other practical issues as well. Should the central bank, for example, ignore any effects of changes in the real exchange rate or respond to appreciations and depreciations in the real exchange rate with equal zeal? Moreover, should—or can—the central bank disregard any and all real costs associated with a monetary policy consistent with the pursuit of price stability? Overall, Rich's comments suggest areas where the practical elements of Hoskins' broad proposal need to be specified.

The first day's afternoon session took a detour from a central bank's monetary focus to address related, but often overlooked, themes. The first is the functioning of competitive markets; price stability, after all, is not pursued from religious conviction but rather from the notion that the market mechanism will allocate resources more efficiently if economic agents can be reasonably certain about the future purchasing power of money. The second detour addresses the issue of good econometric practice; because Ted believed that economic understanding would advance only after theories were confronted by the data and refutable null hypotheses were tested, he viewed good econometric practice as essential to the work of a central bank.

On the topic of the market mechanism, Harold Demsetz presented his thoughts in "Financial Regulation and the Competitiveness of the Large U.S. Corporation." In particular, he addresses the effects of regulation of capital markets on shareholder control of corporate management. When diffuse ownership impedes stockholders from controlling self-interested corporate management and capital market regulations inhibit greater concentration of ownership, corporate efficiency can be impaired.

The story is not quite so simple, however. First, stockholders enter agreements with management voluntarily and in full knowledge of potential conflicts of interest. Second, even though corporate ownership is diffuse, it is not so diffuse that owners have no incentive or power to monitor management. Third, though greater concentration of ownership might enhance control of management, this is achieved at a cost of increased firm-specific risk.

Citing findings of other studies, Demsetz reports that the top five stockholders of U.S. corporations own about one-fourth of voting stock, whereas this share is substantially higher abroad: 50 percent or more in South Africa, 33 percent in Japan and similarly higher figures for Germany and Sweden. Demsetz argues that the higher concentration of ownership abroad can be attributed to restrictions in the United States that prevent or limit banks, insurance companies and others from taking equity positions in U.S. corporations. With most U.S. corporate equity then coming from individual investors, what effect might this have on corporate efficiency?

Demsetz clarifies the issues surrounding the separation problem of ownership and control by noting the difference between closed-end and open-end mutual funds. In the former, investor funds are converted to assets *owned by the fund*; thus a dissatisfied investor can sell his shares but, because he cannot force the fund to be the purchaser, there is no threat that poor performance will threaten management's control of the fund's assets.

In an open-end fund, however, investors who withdraw funds also diminish the fund's asset base. Moreover, it is important to note that this is different from the sale of stock in an individual company where, although the share price might decline, the assets controlled by management are unaffected. Thus by adding this new twist to the conditions necessary for the separa-

tion problem to be important, Demsetz suggests an ownership structure that can be quite diffuse while still exercising effective control over management.

In his commentary, Charles I. Plosser notes that this paper, like many others by Demsetz, raises an issue that (to his knowledge) has been overlooked by others. And, though he encourages efforts to assemble empirical evidence on the association between corporate ownership and regulation on the one hand and corporate performance and control on the other, Plosser has doubts that the issues are likely to be economically important.

Plosser's first doubt arises from his belief in the market mechanism and the ingenuity of individuals. Based on evidence from studies of the efficacy of other regulations and the typical response of individuals to the opportunity of large rewards for evading regulations, Plosser's instinct is that the costs of regulations on corporate ownership are small.

He devotes the remainder of his commentary to the notion of comparative advantage in investments. Some funds, for example, specialize in risk sharing and as a consequence limit their stake in any one firm. Not only is there no reason to expect that the managers of this type of fund have an advantage in corporate control, but there are also suggestions that some of these funds are largely *uninterested* in corporate control. Conversely, other funds specialize in corporate control by taking large ownership positions in single firms and by so doing do not diversify risk for their investors. Specialized funds of these types, in Plosser's view, are but one market response to distortions created by regulation of corporate ownership. At the same time, new regulation, such as the provisions in the Financial Institution Reform Recovery and Enforcement Act that limit bank holdings of high-risk securities, may create new distortions that are important for the efficiency of the market for corporate control.

Carl Christ's paper, "Assessing Applied Econometric Results," offers both philosophical comments on the desirable properties of econometric models and practical suggestions for evaluating real models against the standards of an ideal model.

The standards for accepting or rejecting a model and the quality of forecasts are discussed

in some detail. Christ also offers brief comments on the more popular methods that have been applied to macroeconomic time series in recent years.

Most of Christ's points are illustrated by a re-examination of what he calls "an old, plain-vanilla equation that still works, roughly": Latané's (1954) inverse velocity equation. Noting that the specification of $M1/GNP = a + b$ (inverse of long-term bond rate) has some of the properties of a money-demand function—negative interest elasticity and income elasticity restricted, by construction, to equal one—Christ wonders whether the original equation is stable when so many money demand equations have exhibited substantial instability over time. In a variety of experiments, no demonstrable instability is found.

Christ notes, however, that this specification has a number of undesirable characteristics including strong positive serial correlation. Embarking on a number of approaches to this problem, Christ employs strategies from the simple addition of an autoregressive term to the use of an error correction representation with partial adjustment parameters. He finds equations that fit much better but are terribly unstable over time. In doing so, he highlights the need for considerable judgment in addressing the important question of interest while resisting the temptation to find models that have better in-sample descriptive statistics.

Two discussants offered comments on Christ's paper: David Dickey on the suggested approach to econometric modeling and David Laidler, speaking for the "stochastically challenged" among us, on the economics of Christ's chosen example. Dickey agrees with Christ's general thrust and adds a few new examples of subtle relationships that are often lost in mechanical transformations of data. That the error term is multiplicative in a log specification of the Quantity Equation, and hence implies heterogeneity of variance in the untransformed data, probably has not been considered by most economists who have estimated reduced-form relationships of this sort. Nor is it always recognized that exact relationships hold on some scales but not on others. Although these might be considered simple examples by some, Dickey's point reinforces Christ's entire theme of taking care with the economic specification and the raw data used to estimate it.

Dickey also comments on Christ's evaluation of a forecasting model's performance: Should a good model see a quadrupling of the root mean squared error across a forecast horizon of eight quarters? At first glance, one might think that this is a reasonable standard. Dickey shows, however, that the probability of such a quadrupling is high even if the true model is known. This sobering result suggests a continuing reliance on judgment to supplement the information in econometric forecasts. Finally, on a related point, Dickey notes that the simple bivariate velocity regression in Christ's paper can be dressed-up in the adornments of cointegration. But at heart, the main ideas are similar to those in the original Latané study.

David Laidler applauds Christ for his reiteration of a point made at least 25 years earlier calling for a test of models against data that were not available when the model was formulated. Indeed, Laidler sees a full research agenda for applied econometricians who might investigate how a number of the classic equations of the literature fare when confronted by more recent data. If other relationships were found to be as stable over time as the Latané equation, we might come closer to some consensus on the enduring long-run relationships that govern the behavior of aggregate data.

This view stands in counterpoint to Laidler's reading of the money-demand literature and the philosophy behind its voluminous work. Much of this work has argued that the demand-for-money function is unstable and has done so with evidence on some instability in its short-run dynamics. But Laidler argues that no one has yet modeled these complex short-run interactions and, as such, we never had any reason to believe that we should be able to find a stable short run money-demand function. Thus it should not be surprising that more sophisticated attempts at modeling autocorrelation and other problems have produced models that are less robust than simple specifications of the long-run relationships for which we have a theory.

The conference's last session addressed topics in international economics of interest to Ted: flexible exchange rates, and the gold standard as a monetary policy rule. On the first topic, Allan Meltzer notes that the theoretical case for flexible exchange rates can go either way: they may be a relatively low cost way of reducing the variances of other variables or they may be

a source of excess burden. Surprisingly, however, little empirical evidence has been produced that permits comparisons of the welfare implications of alternative exchange rate regimes. Meltzer's paper is directed to this end.

After reviewing the case for flexible exchange rates as put forth by Friedman in 1953, Meltzer offers empirical evidence on several of the key issues in the flexible vs. fixed exchange rate debate. The first is the possible excess volatility and welfare burden of flexible rates. Looking at data since 1973 for both the Bretton Woods and flexible exchange rate periods and for flexible rate countries and the members of the Exchange Rate Mechanism (ERM) Meltzer finds that the variability of relative prices does not vary systematically across exchange rate regimes. He also finds no evidence to support the proposition that output is more variable under a fixed exchange rate system.

Moving on to policy issues, Meltzer reminds us that Friedman's 1953 work attributed a large role to rearmament in exchange rate determination (because it affects relative prices and the balance of payments) and distinguished between permanent and transitory changes in exchange rates. Incorporating the first idea into an equation for the real exchange rate, Meltzer reports that "contemporaneous changes in money and in defense spending are the principal factors keeping the predicted changes in step with actual changes." He also presents evidence against the common finding that the exchange rate is nonstationary. Overall, Meltzer finds much in his empirical evidence to support the main propositions in Friedman's 1953 essay.

Pedro Schwartz agrees with the thrust of Meltzer's analysis and applies it to current debates over monetary union in Europe. As the evidence indicates that real and nominal exchange rates move closely together and that exchange rate variability does not spill over into the goods market, an exchange rate objective does not seem to be an important or proper objective for a central bank. Indeed, with no ability to influence real exchange rates and unburdened of worries about spillover effects, Schwartz interprets Meltzer's evidence as more support for directing a central bank's attention to the attainment of price stability.

With regard to European Monetary Union, Schwartz sees the potential gains associated with a single currency and the lower transac-

tions costs of trade. He also sees, however, the drawbacks of another political institution, the European Central Bank, subject to varying demands to pursue goals apart from price stability. Rather than move to a stronger government institution, Schwartz believes competition between issuers of money may lead to better results for consumers of monetary services.

Michael Bordo picks up many of the themes raised by Meltzer and Schwartz: the welfare consequences of alternative exchange rate regimes, the insulating properties of flexible rates, rules vs. discretion in the conduct of monetary policy, international policy coordination and the case for international monetary reform. In a wide-ranging treatment of each issue, Bordo both reviews the existing literature and offers new empirical evidence to investigate why some exchange rate regimes have been more successful than others.

On the question of performance, Bordo finds the Bretton Woods convertible regime of 1959/1970 to dominate all others examined; only the recent floating rate period comes close to achieving its level of performance. He also notes that the classical gold standard performed well as a nominal anchor but poorly in terms of the stability of real variables. Moreover, he argues that the gold standard was more durable than Bretton Woods because it worked as a contingent rule and, as such, allowed the flexibility for governments to adjust to shocks.

Bordo goes further than Schwartz in concluding from his evidence that monetary arrangements that surrender monetary policy autonomy will not work over time. Because countries will not surrender this autonomy to another authority whose commitment to price stability they cannot trust, the key advantage of a flexible rate regime—the ability to pursue an independent monetary policy—is still valued highly. The stresses within the European Monetary System in September 1992 only reinforce Bordo's conclusion.

Manfred J.M. Neumann, who found much to agree with in Bordo's paper, first tries to enhance our understanding of Bordo's VAR evidence by supplementing it with a basic theoretical model. Although this exercise is frustrating in the sense that it identifies many unknowns confronting economists and policymakers, it is highly instructive as to where future research might be most profitably directed.

Neumann then spends the remainder of his commentary on the reasons international monetary arrangements tend to break down. His conclusion is that standards based on commitments to rules fail because the commitments are ultimately not credible. Discussing the relative merits of two alternatives—precommitment by one central bank to price stability with all remaining countries precommitted to fixed exchange rates vs. precommitments by each nation's central bank to price stability—Neumann prefers the latter. His reason is that it will provide the ability to absorb idiosyncratic shocks (of many varieties) while still providing a credible nominal anchor for the price level.

In sum, the papers in this proceedings issue reflect Ted Balbach's world view: markets work,

money matters, and empirical evidence is important. Add to these guideposts the principle that a policy institution supported by taxpayer dollars should direct attention to relevant, real-world issues and you have the framework that guided the St. Louis Fed's research effort during his tenure. Although the Bank's many clients and all of those who worked for or with Ted during his 20 years of service can continue to enjoy his legacy, his presence at the Bank will be dearly missed.

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