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Essays in Honor of
Anatol B. Balbach

Proceedings of the
Seventeenth Annual Economic Policy Conference of the
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In This Issue . . .

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President's Message

It is with great pleasure that we dedicate these proceedings in honor of Anatol B. (Ted) Balbach and his years of distinguished service at the Federal Reserve Bank of St. Louis. Ted first joined the Bank as a visiting scholar in August 1970. After returning to his position as Dean of the School of Business at California State University at Northridge one year later, he rejoined the Bank in August 1972 as a Senior Economist; in 1975 he became Senior Vice President and Director of Research, the position he held until his retirement on Oct. 31, 1992.

The period of his tenure offered a wide spectrum of research topics for monetary economists, and Ted led the research staff to put the Bank's imprint on most. In the early days, the stress of 4 percent inflation precipitated an ill-advised experiment with peacetime wage and price controls and the closing of the gold window; a move to freely floating exchange rates soon followed. Those days also saw a Republican president declare "We are all Keynesians now!" at the same time serious discontent with that dominant paradigm was being articulated.

Building on the foundation already established by Homer Jones and notable work such as the Andersen-Jordan equation, Ted led the staff to write rigorous, but still readable, articles on controversial topics: the links between money growth and both nominal spending and the inflation rate; the link between rapid money growth and higher, rather than lower, nominal interest rates; the merits of flexible exchange rates; and the adverse consequences of unconstrained discretionary actions in monetary policy. Although this research agenda ran in parallel to one thread of the academic literature, it contributed in important ways to making the controversial issues of the 1970s the common ground for discussion in the 1990s.

The proceedings of this conference represent the main areas of research Ted emphasized during his tenure at St. Louis. These interests, in turn, reflect his ongoing concern that a research department in a Federal Reserve Bank has to be accountable for its expenditure of taxpayer dollars. As such, the research topics must be relevant to the concerns of a central bank, and the lesson of the work must be understandable to a broad audience. Whatever recognition has accrued to the Bank over time can be attributed in large measure to an adherence to these simple principles.

Many staff members achieved great success working for Ted and moved on to important responsibilities elsewhere. This occurred because Ted insisted on high productivity from the Research staff, while seeing to it that they received proper credit for their work. Clearly, he played a key role in the recognition that the Bank received during his tenure and in the professional success of many who worked for him. Nonetheless, Ted, like Homer Jones before him, was content to be anonymous to most people.

These proceedings are a small effort to shed that anonymity and acknowledge that many people owe a great debt to Ted Balbach. Armen Alchian's warm opening comments give much of the flavor of sentiments expressed by speakers and guests alike. Indeed, the authors of the papers in this volume produced papers of uniformly high quality as a sign of respect for Ted's high standards: They knew he would have been offended by a routine "festschrift" that stressed style and forgot the substance. Students of a central bank's responsibilities should find something of interest in each of these contributions.

Thomas C. Melzer
President and Chief Executive Officer
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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 recent 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 separation 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 transactions 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.

Michael T. Belongia
St. Louis, Missouri
April 23, 1993

Ted Balbach: An Appreciation

Anatol Berkman Balbach. Welcome to what is naively called the leisure class. And kiss your leisure good-bye: You had that by working for the Fed. Retirement? No, you've shed that and now you're on your own time as you rush from here to there, no longer working under the 5:00 o'clock mentality. You and Rae have shed one taskmaster for another. But there is some good news. You are now old—a senior citizen or "senile" as I like to say. You'll get senior discounts at restaurants and hotels and theaters, but not from doctors or hospitals. You'll get haircuts that are overpriced.

By the way, I'll offer you some advice. Because the capital value of us old people is very small—we've got a short life expectancy—we lose little by breaking our contracts and commitments. You may therefore wonder why I am here even though I promised to come. Well, I'd really planned to call at the last minute and say, "I'm sorry I have to go to the hospital to have an angiogram and I can't make it." I've done that twice already at other places and it's a good excuse. So why'd I come? Rae—she's the one. She's smarter than you or me. She knew what I was going to do, so she phoned me about a month ago and said, "I got a golf date for you on Friday afternoon at Bellerive." That's why I'm here.

Enough about me and about the wisdom of maturity. You, Ted, were born in Konigsburg, Germany, on October 31, 1927, Halloween's day, a mother's trick-or-treat. We are glad to know now that it was a treat. As a youth I understand you were drafted into the labor camp and you shoveled stuff from here to there learning about comparative advantage. But then something remarkable happened I'm told. You, in another labor camp, met your mother quite by surprise. I think that's right—a story we have to hear sometime. You came with her as displaced persons to Los Angeles because of the prevalence in that area of some earlier relatives, immigrant relatives. Los Angeles still is a haven for refugees and in many ways is better for it. Driving from the west side of Los Angeles, where UCLA is, to the east side is like a quick trip through Israel, Japan, Korea, China, Vietnam, Mexico, Iran, Armenia and Africa. You came to UCLA, Ted, after attending high school in Los Angeles, where you thought you learned to speak English. Well, your pronunciation was a lot better than the graduate students we still have there.

But the problem is: Why did you choose UCLA? You didn't know anything about it except it was a low-tuition school and near your home. I presume you hadn't yet learned about the principle about the travels of good and bad grapes. You weren't shipped very far. To do you justice, though—and the principle, too—you really were shipped a long way from Konigsburg to Los Angeles. So that's pretty powerful evidence that you really were a good grape.

I can't say you came to UCLA because I was there. I only taught statistics during your undergraduate years, 1947-51. It might have been Karl Brunner, about whom I'll say more later. You graduated in 1951 and, confronted with earning a real living, you entered graduate school. You set a record that will never be broken, earning your Ph.D. in record time: only 11 years. You must have learned an awful lot, though the truth is you were taken in the U.S. Army in 1955 until 1957. For that underpaid service to your new nation and assuming confidently your army discharge, I thank you now on behalf of the people of the United States of America.

After release from the army, you became an instructor at the University of California at the Santa Barbara campus and then a teaching assistant at UCLA. You now had to earn a living—you were married. In '52, I believe, you started as my research assistant, or agent, in managing a study of common stock prices to test the anticipation of inflation, done with Reuben Kessel.

You foisted on to me a graduate student, Rachel Benveniste, you told me would help in the data collection. She did—she took over and managed the whole project. I now know that you and she had something going. I hope you realize your appointment was close to planned nepotism. But it worked well, and I soon received an invitation to attend the marriage of Rae and Ted. I wished you both well and that wish has been granted. What you would have done without me I don't know.

But for that matter, what of the many other students I have had who have married in my class, or at least married later? But only a word of gratitude. Upon reflection after 50 years experience as a successful though unintended marriage broker, my conviction is that college's main function in one in 10 cases is as a marriage market.

As I recall your years at UCLA, I'm astounded at the superb quality of graduate and undergraduate students we had at UCLA in the late 1950s and '60s. Ted and Rae, you were prime examples. I used to contend seriously that the economics department at UCLA during those years had a ratio of student quality to faculty quality that was the highest in the nation. I like ambiguities, but it was true. You, Ted and Rae, were two of those who helped raise the stature of UCLA's department to where Chicago became known as the "UCLA of the East."

Speaking of Chicago, particularly "Chicago boys," Rae was, I believe, the first of what really distinguished UCLA from Chicago. We had the "UCLA girls" compared to the Chicago boys. Not many of you know we had, without question, the most spectacular female graduate students: Rae, Anita Dance, Linda Kliger, Judith Mann, Susan Woodward, Vicki Carnahan to name only a few. You haven't heard of most of them: after graduation they said to hell with it and they married rich people. Rae married a little too early.

The question this provokes in my mind is: "From whence came all these good students?" The likes of Walter Oi, Tibor Fabian, Martin Bailey, Allan Meltzer, Steven Cheung, Mike Mussa, Lee Hoskins, Art Devaney, Jerry Jordan, Cliff Stone, and Robert Summers, who married Ken Arrow's sister and founded a dynasty. I apologize to others whom I could name. Of course I haven't forgotten Bill Sharpe of whose Nobel award-winning dissertation I was a director. But we all know, of course, that it was Harry Markowitz, whose help I asked for, who really was a director. Incidentally, upon hearing about Bill Sharpe's receiving the award, I went and got his dissertation and reread it. It is an astonishing expository paper; it is beautifully written. I now tell all the students there, "Go read Bill Sharpe's dissertation. It is so well written and it contains a lot of good economics."

So, when I heard about this honor to you, I went and got your dissertation. Would I be one of the signers as I was on Bill Sharpe's? No. My God, Ted, if I had signed it, you would be getting a Nobel Prize now. But it wasn't a bad dissertation; after all, it was signed by J. Fred Barron who suffered shortly thereafter a tragic, debilitating stroke. And by Bob Baldwin of international trade fame and William R. Allen, author of the best textbook in economics. And Robert Rutland and Robert Williams.

And last, the director, Karl Brunner, to whom you and I owe so much. No one enjoyed his students more than Karl. He was very demanding. He treated them as his children, training, disciplining, scolding, abusing and teaching. I have a hard time speaking about him, such a dear friend. As a bright side of one who got him to UCLA, after Lloyd Metzler. He's the one who said to me: "He's the one you should get—the young man from Switzerland, on a Rockefeller Scholarship." So I met with him in Chicago at the Palmer House hotel. And I was convinced. So I went back to UCLA and, after some hard work, convinced the older, senile citizens of the department to bring him there and they did and struck a load for all of us.

During his early years at UCLA, Karl interrupted his career to study statistics, philosophy of logic and the scientific method. As a result of that time taken out, his promotion in rank was scandalously delayed and denied because they said he hadn't published enough. He suffered, but we gained by what he had learned. The gains to you are evident in the rigor of your dissertation about the meaningfulness and the evidence pertinent to Clark Warburton's interpretation of the effects of money supply changes. You showed and told Warburton that he was arguing essentially only that money supply changes predicted changes of the price level better than would have a purely random prediction.

And it did, but it did not exceed a naive model.

Your dissertation reported evidence consistent with the implication that the elasticity of the demand for money was positive with respect to wealth and negative with respect to the rate of interest. You blasted a conception of a distinction between idle and total cash balances and ended by saying the data you collected were consistent with the proposition of money relevance.

But that was way back in the '50s. Most significant, in my opinion, was your emphasis on the evidence of the usefulness of the higher level hypothesis, the basic economic model from which it is derived, the existence and effects of money supply changes. Money is implied, yet, as you said in a footnote on page 34, we allow for information costs. We now know they will also imply the real income effects that we attribute to money. You illustrated in that section that cheap information about recognition of commodities reduces those costs as compared to barter and is the key characteristic explaining the effects of money—what makes it money. Had Earl Thompson been there, he'd have explained why virtually costless recognizable receipts for prepayment of taxes serve as our money.

As I reread your dissertation, signed September 20, 1962, almost 30 years to the day I was reading it, I was reminded vividly of the hours we all spent with Karl Brunner trying to clarify and find answers to questions like "What is money?" "Why is it money?" "Why do changes in its supply affect real income and employment, even if only transiently, whereas changes in the supply of shoes, automobiles and wheat do not?" "What presumptions or higher-level hypotheses had to be altered for that effect to be implied?" You had seen beyond the simple monetary patterns. By the way, I noticed you labeled your series M1 and M2. Had that been done before or is it original to you?

Fortunately your early understanding of a prescription for research remained with you and is reflected in what you have been doing here ever since at the St. Louis Fed. Your career exemplifies and extends the American Promise: immigrant to honorable success and freedom in a capitalist economy. Ted and all of you here associated with Ted at the St. Louis Fed, from Homer Jones to Ted and most likely those who follow, you have our gratitude. Thank you and best wishes.

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Monetary Aggregates, Monetary Policy and Economic Activity

ALMOST A QUARTER century has passed since the publication of the (in)famous Andersen-Jordan (AJ) equation.[1] For a good portion of that time, Ted Balbach has been associated with the research department of the Federal Reserve Bank of St. Louis, and for a significant fraction of the period directed the research efforts of that department.[2] Throughout that period the Bank consistently advocated a monetarist approach to monetary analysis and monetary policy. It is appropriate at this point to look back and examine what lasting influence this perspective has contributed, both to analysis and to policymaking.

This study has three parts. The first is a reexamination of what monetarism and the St. Louis empirical representation thereof contributed. In particular, what controversies of the late 1960s and 1970s now can be considered settled? The second examines the empirical failures of the AJ equation in the 1980s and argues that these failures represent specification problems of the "Lucas variety" and not a rejection of the underlying theoretical framework. The implication of such a "Lucas effect" for prominent monetarist policy prescriptions is then analyzed. The third part examines the monetarist proposition that has remained most controversial in recent years, namely the short-run impact of changes in nominal money growth on real economic activity. In particular, the analysis attempts to address the question raised by Cagan—why do vector autoregressions (VARs) produce inferences about the impact of money on economic activity that contrasts dramatically with the conclusions of historical analyses?[3]

ST. LOUIS ON THE ROLE OF MONEY

Two aspects of the AJ equation seemed particularly controversial in the late 1960s. First, the analysis focused on the relationship between nominal measures of fiscal and monetary policy and *nominal income*. Second, the analysis focused on growth rates or first differences. Reduced to simplest terms, the analysis stated that the growth in velocity of narrow money, defined as the ratio of nominal GNP to a weighted moving average of M1, fluctuated around a positive deterministic trend and that some fraction of these fluctuations were correlated with fluctuations in the growth

[1]See Andersen and Jordan (1968).

[2]A precusor of the AJ equation can be found in Brunner and Balbach (1959). Michael Belongia is responsible for bringing this well known article to my attention.

[3]See Cagan (1989).

of nominal government spending.[4] This contrasts sharply with macroeconometric models that were developed contemporaneously. The implicit reduced forms of the latter models specified relationships between the *level* of nominal money balances and the *level* of real output. The models also endogenized the price level or inflation rate, but the typical reduced forms implied little if any price level response over the time periods in the AJ specification.

The lightning rod in the AJ equation was the conclusion that a maintained change in nominal government spending, unaccompanied by changes in the nominal money stock did not produce a permanent change in nominal income (or velocity) and that changes in high employment nominal tax receipts produced no statistically significant changes in nominal income (or velocity). These implications, which dramatically refuted the fixed-price Keynesian model, did not go unchallenged. Numerous counter regressions were published which reported that the implied fiscal policy implications of the AJ equation were artifacts of measurement error and/or sample specific.[5] The point that seems to get lost in the background of these challenges is the robustness of the long-run response of nominal income growth to monetary growth shocks: the conclusion that monetary shocks, in the absence of fiscal shocks, have only transitory impacts on velocity growth held its ground in the face of repeated "regression attacks".[6]

In retrospect it appears that in two significant respects the macroeconomics profession has largely surrendered and accepted the perspective of the AJ equation. First, velocity has been rehabilitated as a useful theoretical device across a broad range of macroeconomic thought. Monetarists have steadfastly maintained the usefulness of this concept. Two of Greg Mankiw's (1991) "dubious Keynesian propositions" speak directly to the points raised in the AJ equation: Point No. 2—"[T]he lessons of classical economics are not helpful in understanding how the world works"; and Point No. 4—[F]iscal policy is a powerful tool for economic stabilization and monetary policy is not very important." Mankiw further asserts "for purposes of analyzing economic policy, a student would be better equipped with the quantity theory of money (together with the expectations augmented Phillips curve) than with the Keynesian cross." Some new Keynesians may repudiate Mankiw, since this statement could be paraphrased that a student would be better equipped with the AJ equation (together with the St. Louis model) than with the Keynesian cross.[7] Nevertheless, a statement such as this (original or paraphrase) was heresy 25 years ago, and it can only be said of the St. Louis view of monetary analysis and monetary policy "you've come a long way baby."[8]

Most of the attention that real–business cycle theorists give to money has focused on the relationship between money and real output in the short run. Proponents of this approach generally dismiss any causal effect from money to real output, arguing that correlations between changes in money and changes in real output reflect feedbacks from real output onto an endogenous money stock. This is not a denial of all significant parts of the St. Louis position. Plosser (1991), for example, argues that "money, without question, plays the dominant role in determining the rate of inflation." Presumably money then also has important impacts on the path of nominal income, though real shocks are also important from this perspective. Real–business cycle specifications have recently expanded to include inflation and nominal variables. At least some of these expanded specifications incorporate a traditional demand-for-real-balances function, with point estimates of long-run income elasticities that are fairly close to unity.[9] Thus these models do not reject the usefulness of velocity

[4]This interpretation of the AJ equation was not widely recognized at the time of publication, I suspect in part because the original specification was published in first differences rather that log differences and also because the specification was never was presented as a hypothesis about velocity. The original presentation was intended as a sequel to the Friedman-Meiselman debate. See Jordan (1986).

[5]See, for example, deLeeuw and Kalchbrenner (1969), Corrigan (1970) and Davis (1969).

[6]For example, Benjamin Friedman (1977) argued that the original Andersen-Jordan conclusion with respect to fiscal policy was sample specific. However, the permanent effect of money growth on velocity is robust to his changes in sample periods.

[7]See Andersen and Carlson (1970) for a discussion of the St. Louis model.

[8]That the St. Louis view is still contested in discussions of public policy is evidenced by the report of March 31, 1992, that 100 economists, including six Nobel Memorial Prize laureates, sent in an open letter to President Bush, Chairman Greenspan and members of Congress calling for additional government spending, lower interest rates and tax credits for business investment to stimulate economic growth ("Top Economists Urge Officials to Boost Federal Spending to Stimulate Growth", Wall Street Journal, March 31, 1992, p. A2).

[9]See King, Plosser, Stock and Watson (1991).

as a long-run concept relating money to nominal income.

The second aspect of the evolution of macroeconomic thought toward the AJ equation involves the modeling of shocks to velocity. The AJ equation was consistently estimated in differenced form, and thus the implicit assumption of the specification is that shocks to the level of velocity are permanent. At the time this analysis was constructed, the discussion of the role of permanent and transitory shocks that is so prominent in recent analyses was unforeseen. Nevertheless, there is vindication for the St. Louis modeling approach in the now conventional wisdom that many macroeconomic time series (including velocity) appear to be "difference stationary" and that there are serious problems of "spurious regressions" in estimations involving levels of such data series.[10]

The conclusion from this discussion is that from current theoretical and econometric perspectives there are important ways in which the original St. Louis analyses "got things right." Nevertheless, the AJ equation has disappeared from contemporary discussions of monetary policy.[11] Why then the demise of the AJ equation?

THE DEMISE OF THE AJ EQUATION: ANALYSIS AND SOME IMPLICATIONS FOR MONETARIST POLICY PRESCRIPTIONS

The demise of the AJ equation is well illustrated in figure 1. Two different measures of velocity are plotted there. The first is the conventional ratio of nominal GNP to M1. The second is the ratio of nominal GNP to a geometric moving average of M1, where the weights in the moving average approximate the weights in the lag polynomial of the log of differences in money in the AJ equation.[12] It is clear that the velocity measure implicit in the AJ equation replicates the behavior of the traditional M1 velocity quite closely, both before and after 1980. Both measures have a strong positive deterministic trend that ends in the early 1980s. This trend was captured in the AJ equation by a significant positive intercept on the order of 2.5 percent to 3.0 percent per year. With the break in the trend in velocity in the 1980s, it is clear that the AJ equation falls apart.

In Rasche (1987) I showed that essentially all narrowly defined monetary aggregate velocities in the United States exhibit similar breaks in their deterministic treads in the early 1980s but that once these breaks are considered, the time series properties of the various velocities are not substantially different in the 1980s compared with the earlier period (see figure 2).[13] Thus to understand the demise of the AJ equation, it is crucial to understand the origins of the trend in velocity.

A considerable number and variety of explanations have been advanced for the change in velocity behavior observed in the 1980s, but most of these are not consistent with the patterns observed in the data.[14] Monetarism in general, and the AJ equation in particular, is based on the proposition that a stable long-run demand function for money exists; that is, the demand for real balances depends on relatively few variables, including real income, or wealth, and various rates of return on nonmoney assets.

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[10]See Nelson and Plosser (1982) and Granger and Newbold (1974).

[11]Relatively few attempts to reestimate the St. Louis equation have occurred in recent years. Batten and Thornton (1983) extend the sample period through third quarter 1982. Belongia and Chalfant (1989) estimate regressions using M1A, M1 and divisia variants of both those aggregates (including variables for relative energy prices and strike dummies, but excluding fiscal policy variables) over a first quarter 1976–third quarter 1987 sample. With the exception of Divisia M1A they find money growth elasticities that are significantly less than 1.0. They conclude the following: "Results indicate that the M1 and broad aggregates are all associated with significant structural change in the money-income relationship around 1981.

Gavin and Dewald (1989) estimated St. Louis equations (again omitting fiscal policy variables) over second quarter 1961–third quarter 1980 and first quarter 1971–fourth quarter 1982 samples. They conclude from out-of-sample forecasting experiments that "M1 has done so poorly in the 1980s that it does worse on average than the monetary base over the entire postwar period, even though it performed better than the base for the 30 years before 1980."

[12]The weights are taken from Appendix Table 2 in Carlson (1982) as .40, .40 and .20 on $\ln M_t$, $\ln M_{t-1}$ and $\ln M_{t-2}$ respectively.

[13]These conclusions are not altered by updated data. Over the sample period first quarter 1948 – fourth quarter 1981 the mean change in velocity (St. Louis velocity) is 3.45 (3.46) percent per year, and the standard deviation is 4.74 (4.74) percent per year. The mean for the first quarter 1982 – third quarter 1990 is –.72 (-.72) percent per year and the standard deviation is 6.04 (5.75) per cent per year. The mean change in the second sample is not significantly different from zero [p = .49 (.46)]. In Rasche (1990) I concluded that the velocities of the broadly defined monetary aggregates M2 and M3 showed little if any changes in trends at this time.

[14]See Rasche (1987).

Figure 1

Velocity Measures First Quarter 1948 through Third Quarter 1990

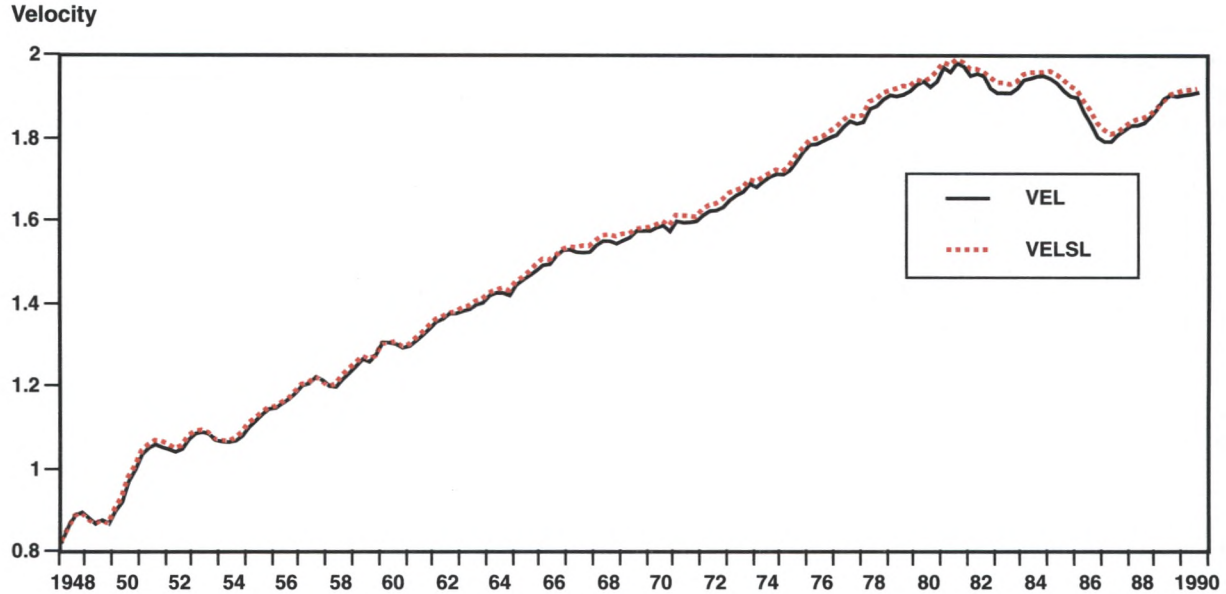
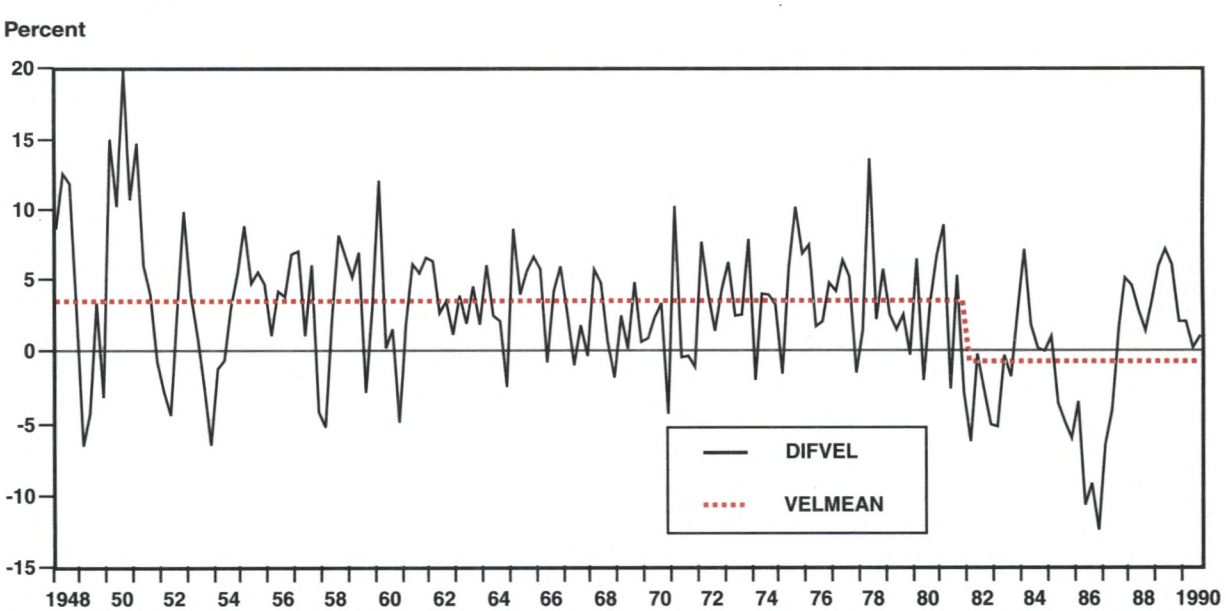


Figure 2

Growth Rate of St. Louis Equation Velocity Measures First Quarter 1948 through Third Quarter 1990



The theory relates the *level* of real balances demanded to the *level* of specific variables. However, the AJ equation, proposed as a reduced form of a model containing such a money-demand specification, is estimated in difference form. Such statistical methodology is correct in that it properly adjusts for the apparent nonstationarities of the observed data series. Unfortunately, differencing data series maintains only short-run relationships among the various series and overlooks any long-run relationships that may exist simultaneously.

In the last decade, particularly in the past five years, innovations in econometric technique allow for the simultaneous treatment of nonstationary data and estimation of long-run relationships among the levels of variables.[15] These techniques, namely cointegration analyses, maintain the spirit of the reduced form approach in differences of the data, but permit the analysis to incorporate the specification of long-run relationships among the levels of the variables, if such relationships exist. If identifying restrictions are satisfied, such a relationship can be interpreted as the long-run money demand function that is fundamental to the AJ analysis.[16]

Some studies have documented the existence of such a cointegrating relationship among real balances, real income and nominal interest rates.[17] The implied long-run income elasticity of money demand in such estimated equations is not significantly different from unity; hence there is a long-run stationary relationship between the level of velocity and the level of nominal interest rates.

What then of the changes in the mean growth rate of velocity in the 1980s relative to the mean growth rate in previous decades? If a stable long-run money demand equation that relates the level of velocity to the level of nominal interest rates exists and if the deterministic trend (drift) in nominal interest rates changes, then the drift in velocity must change correspondingly to accommodate the stable money demand specification. Hence a reduced form in differences of velocity such as the AJ equation, given a stable money demand function, implies an unchanged constant *only* as long as there are no significant changes in interest rate trends. Since during the 1980s there is a complete break from the upward trend of nominal rates of the previous two decades, the break in velocity drift is completely consistent with stability of the money demand function.

Although the velocity break of the 1980s does not invalidate the theoretical propositions on which the AJ equation is based, it suggests that some rethinking of traditional monetarist policy prescriptions is in order. What forces are likely to generate breaks in interest rate trends? A plausible candidate, and the one of most concern for monetary policy prescriptions, is inflation expectations. Assume that there is an established initial regime in which expected inflation has a positive trend. Assume that the monetary authorities take successful actions to stabilize the inflation rate and that this regime change is reflected in the expectation of future inflation at some constant rate.[18] The likely outcome of such a policy shift is that the drift in nominal interest rates will disappear as will the drift in velocity.[19]

This suggests that the time series properties of velocity and the constants in reduced form equations specified in differences are dependent on specific monetary policy regimes through expected inflation trends specific to the policy regimes. If true, this stands as one of the few clear-cut examples of a "Lucas effect" beyond the original Phillips curve example.[20]

One of the consequences of such a "Lucas effect" is that straightforward application of no-feedback monetary growth rules for narrowly defined monetary aggregates can lead to outcomes different from those predicted or desired.[21] A monetary authority that desires to stabilize an inflation that has been drifting upward might be inclined to set a monetary growth objective equal to a projected growth rate for natural output plus a desired stabilized inflation rate, minus the historically observed drift in the

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[15]See Granger (1981); Engle and Granger (1987); Johansen (1988 and 1991); and Phillips (1991).

[16]See Hoffman and Rasche (1991b).

[17]See Hoffman and Rasche (1991a, 1991c and 1992).

[18]Survey data and inflation forecasts for the United States are consistent with such an interpretation of the outcome of the 1981–82 recession.

[19]The break in velocity drift as a result of a break in expected inflation is the hypothesis advanced by Milton Friedman, though to the best of my knowledge he did not elaborate the mechanism described here.

[20]See Lucas (1976).

[21]In Milton Friedman's defense it must be noted that he originally proposed a no feedback rule in terms of a more broadly defined aggregate, old M2. An aggregate such as new M2, in a regime without interest rate ceilings, is unlikely to suffer from the problem discussed here. For some evidence on the stationarity of new M2 velocity over the post-Accord period, see Hallman, Porter and Small (1991).

velocity of a narrowly defined monetary aggregate. If the authority maintains this money growth rate after expected inflation has stabilized, under the above "Lucas effect" the drift in velocity will have disappeared and the actual steady rate of inflation will prove to be lower than the planned inflation rate. During the transition period to the steady inflation regime, the drift in velocity will be slowing and hence the growth of nominal income will drop below the planned inflation rate plus the projected growth rate of natural output.[22] If the aim of the monetary authorities is to reduce, as well as to stabilize, inflation and if actual and expected inflation adjust to the change in monetary policy slowly so that $p_t > p^*$ while the drift in velocity is in transition, then real output growth will fall below q^* for some time during the transition period.[23]

Meltzer [1987] and McCallum [1988] propose alternatives to a fixed money (base) growth rule that allow feedback from velocity to the planned growth in money (base). The rules are designed to account for permanent shocks to velocity, but not to respond to transitory velocity shocks. The rules set the growth rate of the monetary base equal to a desired growth of nominal income ($p^* + q^*$ in the above notation) less a moving average of the drift in base velocity.[24] The rules establish base growth consistent with the planned stable inflation once stabilization is achieved, and the rules also adjust base growth to compensate for the declining velocity drift during the transition period to the stabilized inflation rate. Thus on the surface it appears that these feedback rules immunize monetary policy from the adverse consequences of the "Lucas effect" on velocity drift.

However, this conclusion depends critically on the credibility of the monetary authority. As long as private agents believe that the monetary authority is following the feedback rule consistently, inflation expectations should adjust either in anticipation of or with the observation over time of falling inflation. The feedback mechanism will adjust base growth as desired. Both the Meltzer and McCallum rules are deterministic. In practice, stochastic fluctuations around such deterministic rules will be observed which may make direct verification of the rule difficult. If the monetary authority lacks credibility, feedback rules such as these could prove unstable. Suppose the rule is implemented by the monetary authority and inflation and inflation expectations begin to stabilize. This lowers the drift in velocity, and the feedback rule calls for base growth to be adjusted upward (see figure 3). The McCallum rule, which ultimately restores nominal income to the specified path of nominal potential income, requires that base growth and nominal income growth overshoot equilibrium base growth during the transition period (see figures 3 and 4). If private agents do not understand the rule well, or if the increase in base and nominal income growth is interpreted by such agents as an abandonment of the rule, then inflation expectations could start adjusting upward. This would change the drift of velocity, and the rule would then call for reductions in base growth. It is not difficult to conceive of a situation where the monetary authority lacks credibility, in which the Meltzer-McCallum rules suffer from instrument instability (Holbrook [1972]) if the observed behavior of the monetary base affects inflation expectations, and through this the drift in base velocity.[25]

The conclusions from these observations on the reduced form behavior of velocity is that constant growth rules applied to narrowly defined monetary aggregates are unlikely to be successful in stabilizing a nonzero inflation trend. The success of feedback rules that depend on observed velocity behavior can depend critically on the credibility of the monetary authority. In the

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[22]Set $m_t = p^* + q^* - v$, where m_t is the maintained growth rate of the nominal money stock, p^* is the planned steady inflation rate, q^* is the projected growth rate of natural output and v is the historically observed drift of velocity. Then during a transition period $(p_t + q_t) = (m_t + v_t) = (p^* + q^*) + (v_t - v)$. When the drift in velocity starts to react to the change in expected inflation, $(v_t - v) < 0$ so $(p_t + q_t) < (p^* + q^*)$.

[23]$(q_t - q^*) = (p^* - p_t) + (v_t - v) < 0$.

[24]McCallum's rule provides an additional adjustment to base growth as nominal output is observed to deviate from nominal natural output.

[25]See Holbrook (1972). Consider, for example, a feedback rule of the form: $b_t = \theta(L)LV_t + LX_t + \varepsilon_t$, where b_t is the growth rate of base velocity, X_t is other factors to which the feedback rule responds, and ε_t are random fluctuations generated by fluctuations of sources of monetary base outside the control of the monetary authorities and that cannot be perfectly forecasted. Let inflation expectations respond to observed base growth $p^e_{t+1} = \delta(L)b_t$. Finally, let velocity growth respond to trends in inflation expectations: $v_t = \omega(L)p^e_{t+1}$. Substituting the latter two equations into the first equation gives $[1 - \theta(L)\delta(L)\omega(L)L]\, b_t = X_t + \varepsilon_t$. Invertibility of the polynomial $[1 - \theta(L)\delta(L)\omega(L)L]$, and hence the absence of instrument instability depends upon the expectation formation mechanism, $\delta(L)$.

Figure 3
McCallum Rule: Nominal Income Growth

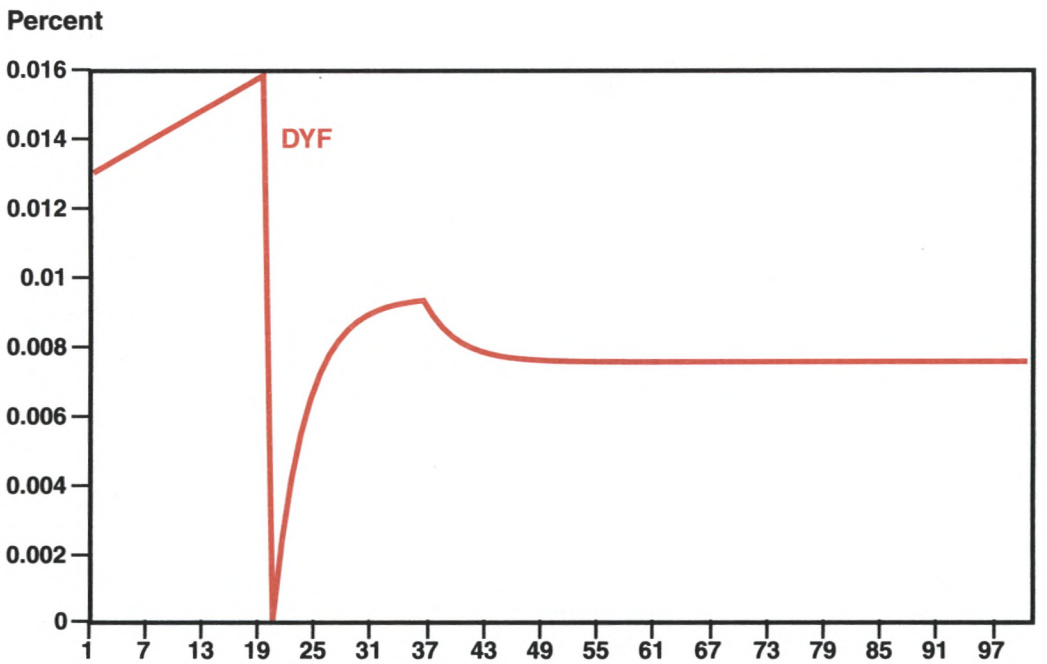
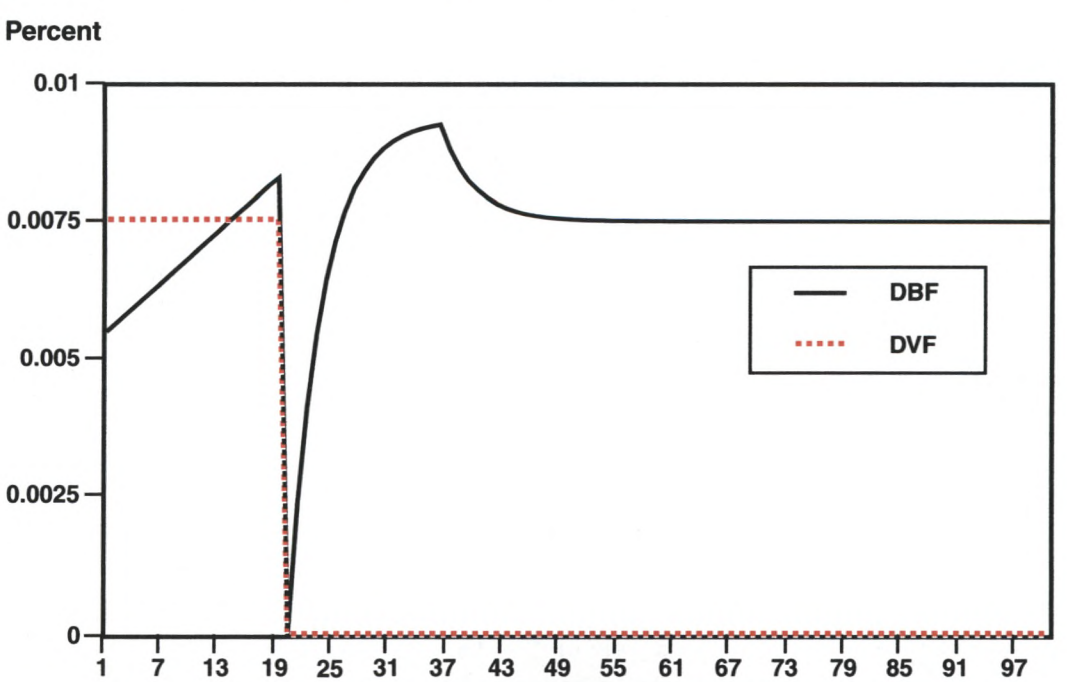


Figure 4
McCallum Rule: Base and Base Velocity Growth



absence of credibility, the adjustments to the growth of the aggregate required by the feedback rule can provoke adjustments to inflation expectations that introduce instrument instability into the feedback rule.

CAN THE TRANSITORY RESPONSE OF REAL OUTPUT TO MAINTAINED CHANGES IN MONEY GROWTH BE INFERRED FROM REDUCED-FORM MODELS?

The Role of Identifying Restrictions

The focus of much of the recent discussion of the role of money and monetary policy is not on the response of nominal income, but rather on the response of real output. Cagan (1989) summarizes a large body of recent empirical research and reaches the conclusion that "lately ... monetary research has turned again ... and new studies claim that money has little or no effect on output and other real variables." VARs figure prominently in recent research and are the source of much of the evidence from which the negative conclusions about the impact of nominal money changes on real output are drawn. Cagan faults the VAR approach as follows: "The VAR seems ... to be hopelessly unreliable and low in power to detect monetary effects of the kind that we are looking for and believe, from other kinds of evidence, to exist." I will argue here that Cagan's skepticism about the conclusions of VAR analysis is justified, but for reasons beyond those he enumerated.

The most important aspect of VAR analysis is the one most frequently slighted in drawing conclusions about policy shocks from such analyses. VARs are reduced forms of some unspecified economic model; as such they have common roots with the AJ equation. Reduced forms, in themselves, provide *no* information about the impact of nominal money shocks, or any other policy shocks of interest to economists. To provide such information, VARs must be supplemented with *sufficient identifying restrictions,* derived from *some economic* model, to uniquely extract information about the impact of monetary shocks on real output within the economic structure defined by the identifying restrictions.

Sims (1986) clearly explains the critical role of identifying restrictions in VAR analysis. Sims defines the economic model as follows:

$$(1)\quad \sum_{s=0}^{m} A(s)Y(t-s) = \sum_{s=0}^{m} B(s)e(t-s);\ \mathrm{Var}(e(t)) = \Omega$$

and the corresponding VAR (reduced-form) model for Y as follows:

$$(2)\quad Y(t) = \sum_{s=1}^{m} C(s)Y(t-s) + u(t);\ \mathrm{Var}(u(t)) = \Sigma$$

Sims notes the following:

> The most straightforward example of identifying restrictions on A(0), B(0) and Ω is the Wold causal chain. According to this idea, Ω should be diagonal, B(0) = I and A(0) should be triangular and normalized to have ones down the main diagonal when the variables are ordered according to causal priority. Using the fact that with B(0) = I, Σ = A(0) Ω A(0)′, the triangularity of A(0) implies that, once we have put the variables in proper order, we can recover A(0) and Ω from Σ as Σ's unique LDL decomposition. That is, it is known that there is a unique way to express a positive definite matrix Σ in the form LDL′, where L is lower triangular with ones down its diagonal and D is diagonal. Applying the standard LDL algorithm to Σ gives us A(0) as L and Ω as D. *This triangular orthogonalization has become a standard practice as part of the interpretation of econometric models* (emphasis added) (p. 10).

Though this set of identifying restrictions has become so common in VAR analysis that only rarely is it acknowledged explicitly, it is neither unique nor uncontroversial. Criticisms of and arguments against both the appropriateness and necessity of the causal-chain (triangular) specification are longstanding.[26] A simple example of the nonuniqueness of this approach is given by the three separate sets of identifying restrictions that Sims applies to his six-variable VAR. All of these identification schemes maintain the assumption that Ω is diagonal, but they impose different exclusion restrictions on A(0), including restrictions that do not impose a triangular structure on A(0).

Recently, attention has turned to identification by restrictions on the steady-state coefficient

[26]See Basmann (1963) and Leamer (1985).

Table 1

Estimates of Four Variable VAR: II/1955–IV/1981

Dep. Variable:	$\ln(Q_t)$	t	$400\Delta\ln(P_t)$	t	$\ln(M_t)$	t	RTB_t	t
$\ln(Q_{t-1})$	0.8790	7.74	−14.3778	−0.68	0.0274	0.38	−3.7567	−0.40
$\ln(Q_{t-2})$	0.0171	0.11	−140.4030	−0.52	0.0091	0.10	4.2816	0.35
$\ln(Q_{t-3})$	−0.1580	−1.08	30.2668	1.11	−0.0930	−1.00	−10.7887	−0.89
$\ln(Q_{t-4})$	0.2778	2.51	−3.9988	−0.19	0.0780	1.12	11.3726	1.25
$400\Delta\ln(P_{t-1})$	−0.0005	−0.86	0.1616	1.43	−0.0002	−0.54	0.0770	1.55
$400\Delta\ln(P_{t-2})$	−0.0007	−1.13	0.0708	0.64	−0.0003	−0.67	0.0487	1.00
$400\Delta\ln(P_{t-3})$	−0.0006	−0.98	−0.0095	−0.08	−0.0006	−1.49	0.0339	0.67
$400\Delta\ln(P_{t-4})$	0.0007	1.17	−0.0264	−0.23	−0.0002	−0.53	−0.0451	−0.89
$\ln(M_{t-1})$	0.3883	1.99	−2.1363	−0.06	1.1862	9.62	22.0609	1.38
$\ln(M_{t-2})$	0.1956	0.69	33.6234	0.64	−0.1555	−0.87	5.9233	0.25
$\ln(M_{t-3})$	−0.4626	−1.63	−10.9309	−0.21	−0.0131	−0.73	0.8224	0.04
$\ln(M_{t-4})$	−0.1095	−0.53	−16.6633	−0.43	−0.0100	−0.08	−29.0417	−1.70
RTB_{t-1}	−0.0015	−1.08	0.6513	2.51	−0.0044	−5.03	1.0494	9.15
RTB_{t-2}	−0.0019	−0.88	−0.8327	−2.03	0.0039	2.83	−0.7993	−4.42
RTB_{t-3}	0.0026	1.12	0.5965	1.39	−0.0010	−0.70	0.7571	4.01
RTB_{t-4}	−0.0029	−1.57	−0.1565	−0.46	0.0007	0.63	−0.3761	−2.49
CONSTANT	−0.1530	−1.36	0.4184	0.02	−0.1879	−2.64	−6.5233	−0.71
D67	−0.0104	−2.21	1.2788	1.47	0.0016	0.55	−0.4775	−1.24
D79	0.0041	0.54	−1.5515	−1.11	0.0018	0.37	1.6174	2.61
$\bar{R}^2$	0.9900		0.7000		0.9900		0.9400	
SEE	0.0089		1.6500		0.0056		0.7260	

matrix, $A = \sum_{s=0}^{m} A(s)$, rather than by restrictions on A(0).[27] This latter approach seems more promising because there appears to be considerable agreement over a broad range of macroeconomic theories on identifying restrictions that apply to a steady-state macroeconomic model.[28] In contrast, economic theory provides little if any information about identifying restrictions on the dynamic structure of macroeconomic specifications. In particular, during the past 10 years researchers have broadly debated the identification of a short-run money-demand function, to the extent that alleged short-run money demand functions are at best problematic and at worst fall into a class of "incredible" identifying restrictions.[29]

If identification of a short-run money demand function is "incredible," then any "shocks" extracted from VARs under these restrictions will at best represent linear combinations of money-demand and money-supply shocks. Under these conditions it is impossible to separate the impact effects of money on output from the reaction of money to output through whatever reaction function characterizes the behavior of the monetary authorities.

The Importance of Specification and Identifying Assumptions

The questions discussed previously are particularly important in the discussion of the effect of nominal money shocks on real output. To illustrate this, consider a four-variable VAR, that includes real output, inflation, nominal money (M1) and a short-term nominal interest rate (Treasury bill rate).[30] The general conclusion that has emerged from the study of such VARs is that "most of the dynamic interactions among the key variables can best be explained as arising from an economic structure in which monetary phenomena do not affect real variables. Thus ... monetary instability has not played an

[27]See Bernanke (1986); Blanchard and Quah (1989); and King, Plosser, Stock and Watson (1991).

[28]See Hoffman and Rasche [1991c] for an illustration of how the restrictions on the KPSW (1991) common trends model are consistent with the identifying restrictions for the steady-state of a standard textbook macroeconomic model.

[29]See Laidler (1982 and 1985); Cooley and LeRoy (1981); Carr and Darby (1981); Judd and Scadding (1981); and Gordon (1984). See also Sims (1980).

[30]These VARs are in the form of Sims (1980) and Litterman and Weiss (1985).

Table 2
Estimates of Four Variable VAR: II/1955–III/1990

Dep. Variable:	$\ln(Q_t)$	t	$400\Delta\ln(P_t)$	t	$\ln(M_t)$	t	RTB_t	t
$\ln(Q_{t-1})$	1.1398	11.85	−8.654	−0.54	0.0216	0.33	11.6235	1.45
$\ln(Q_{t-2})$	−0.0428	−0.30	−4.8998	−0.21	0.0509	0.52	7.5080	0.64
$\ln(Q_{t-3})$	−0.2074	−1.51	15.1429	0.66	−0.1026	−1.09	−18.6523	−1.64
$\ln(Q_{t-4})$	0.1281	1.30	−3.7327	−0.23	0.0369	0.54	1.7967	0.22
$400\Delta\ln(P_{t-1})$	−0.0002	−0.30	0.1557	1.64	−0.0003	−0.75	0.1181	2.50
$400\Delta\ln(P_{t-2})$	−0.0004	−0.71	0.0775	0.82	−0.0005	−1.22	0.1022	2.18
$400\Delta\ln(P_{t-3})$	−0.0003	−0.42	−0.0040	−0.04	−0.0005	−1.27	0.0287	0.58
$400\Delta\ln(P_{t-4})$	0.0007	1.15	0.0101	0.10	−0.0002	−0.43	−0.0228	−0.48
$\ln(M_{t-1})$	0.0455	0.34	13.1115	0.58	1.4116	15.13	7.3153	0.65
$\ln(M_{t-2})$	0.2742	1.19	−17.6696	−0.46	−0.2389	−1.51	−16.9362	−0.89
$\ln(M_{t-3})$	−0.4082	−1.78	15.3508	0.40	−0.1368	−0.87	17.7711	0.93
$\ln(M_{t-4})$	0.0837	0.64	−7.5236	−0.30	−0.0316	−0.35	−9.6530	−0.89
RTB_{t-1}	−0.0004	−0.31	0.4783	2.51	−0.0056	−7.19	0.9899	10.45
RTB_{t-2}	−0.0031	−1.83	−0.3581	−1.27	0.0057	4.88	−0.6275	−4.47
RTB_{t-3}	0.0034	1.84	0.1302	0.42	−0.0005	−0.37	0.5228	3.42
RTB_{t-4}	−0.0018	−1.24	0.0592	0.25	0.0015	1.47	−0.1952	−1.63
CONSTANT	−0.0951	−1.03	0.5120	0.03	−0.0695	−1.10	−9.3111	−1.22
D67	−0.0031	−0.74	1.4683	2.06	0.0019	0.65	−0.1573	−0.44
D79	0.0027	0.39	−1.4610	−1.25	−0.0024	−0.50	1.8046	3.10
D82	−0.0010	−0.14	−3.8349	−3.18	−0.0111	−2.24	−0.1683	−0.28
$\bar{R}^2$	0.9900		0.6900		0.9900		0.9300	
SEE	0.0090		1.4900		0.0062		0.7440	

important role in generating fluctuations."[31]

Estimates of this four-variable VAR are shown in table 1 and table 2, for sample periods that begin in second quarter 1955 and end in fourth quarter 1981 and third quarter 1990, respectively. The starting point for both samples is chosen to avoid the pre-Accord data. The first sample ends before the apparent break in the trend of M1 velocity discussed previously. The second sample includes the 1980s. The VAR is supplemented with three dummy variables chosen to define roughly four inflation regimes with different trends.[32]

The implications of these VARs for the response of real output to "money shocks" identified by the Wold causal chain structure with variables ordered as real output, inflation, money and interest rates are quite sensitive to the choice of the sample period (figure 5). Closer examination reveals that this is associated with dramatically different long-run responses of the nominal money stock to the "money shock" (figure 6). Both samples show the real output response to the "money shock" rises to a peak and then trails off. However, the nominal interest rate exhibits a transitory positive response to the "money shock" in both samples which is difficult to reconcile with the identification of the "money shock" as a monetary policy action (figure 7).[33]

Two other variables of interest are implicit in the VAR menu: real money balances and velocity. The impulse response function for velocity to a "money shock" is shown in figure 8. The implicit velocity response is almost uniformly negative in both sample periods, and in the third quarter 1990 sample has the peculiar characteristic of having a response below -1.0 even after 40 periods.

The realization that the four-variable VAR

[31]See Litterman and Weiss (1985).

[32]The dummy variables are as follows: D67 = 1.0 for 67:4 and subsequent observations; D79 = 1.0 for 79:3 and subsequent observations; and D82 = 1.0 for first quarter 1982 and subsequent observations.

[33]It is also difficult to reconcile the "interest rate shock" identified by the Wold causal chain specification with a monetary policy action because although the immediate impact of such a shock on interest rates is positive, the permanent effects of this shock on nominal rates, inflation, money and real output are all negative in both sample periods.

Figure 5
Real Output irf to Nominal Money Shock

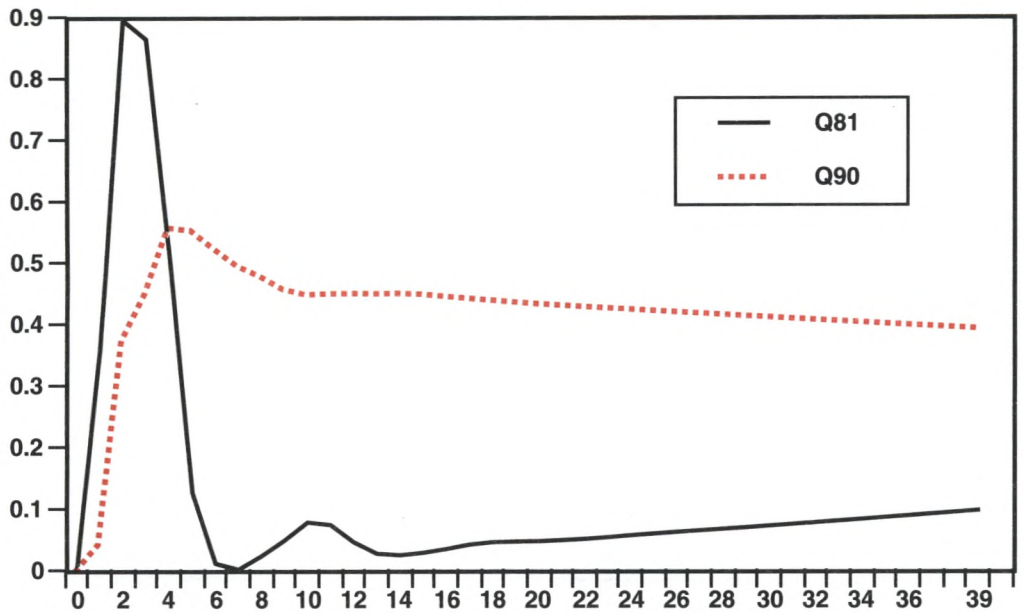


Figure 6
Nominal Money irf to Nominal Money Shock

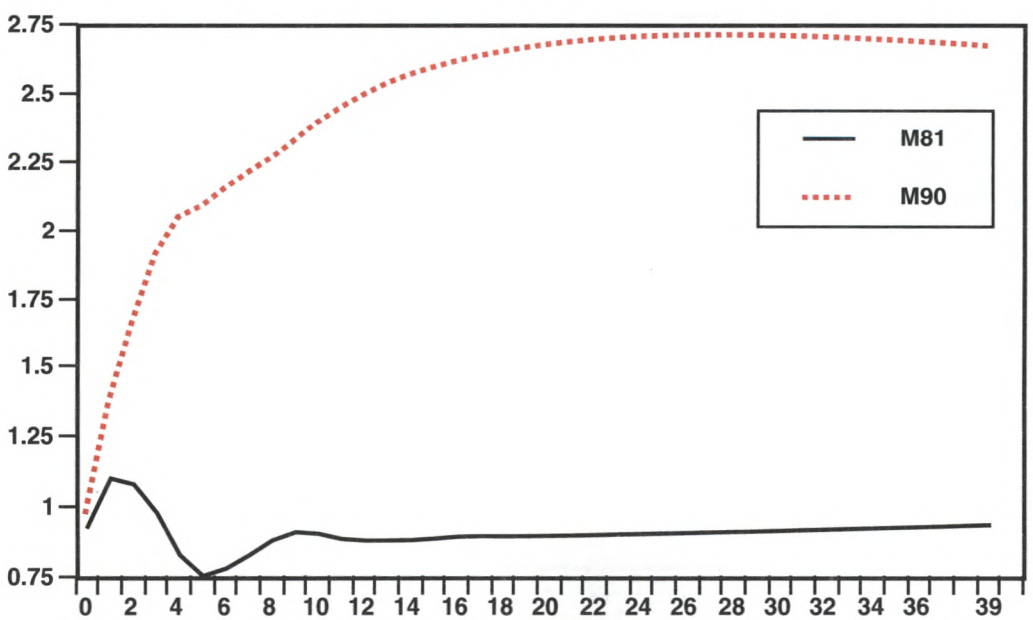


Figure 7
Inflation irf to Nominal Money Shock

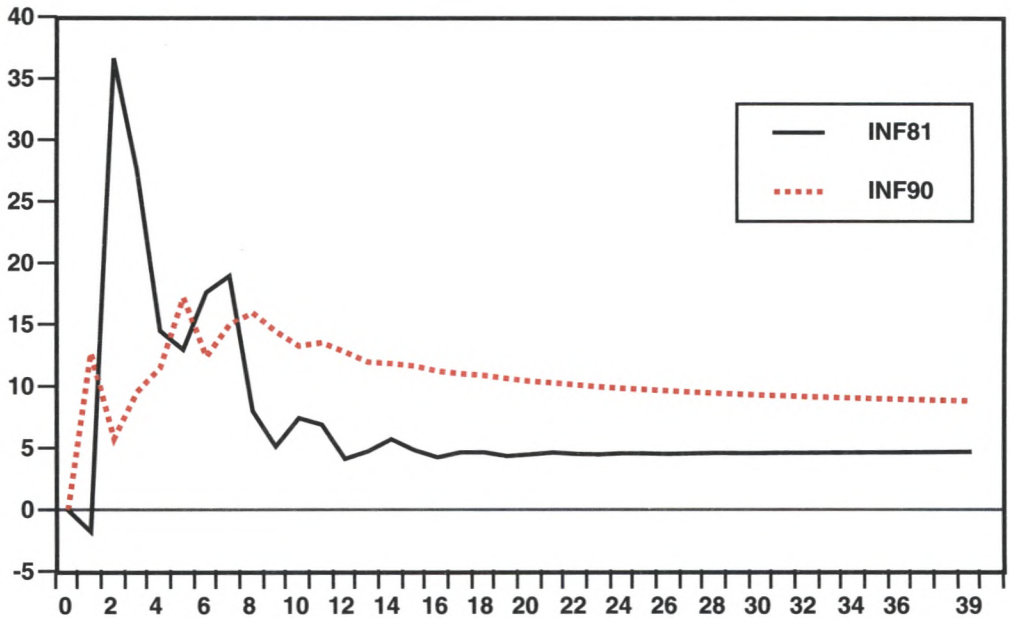


Figure 8
Velocity irf to Money Growth Shock

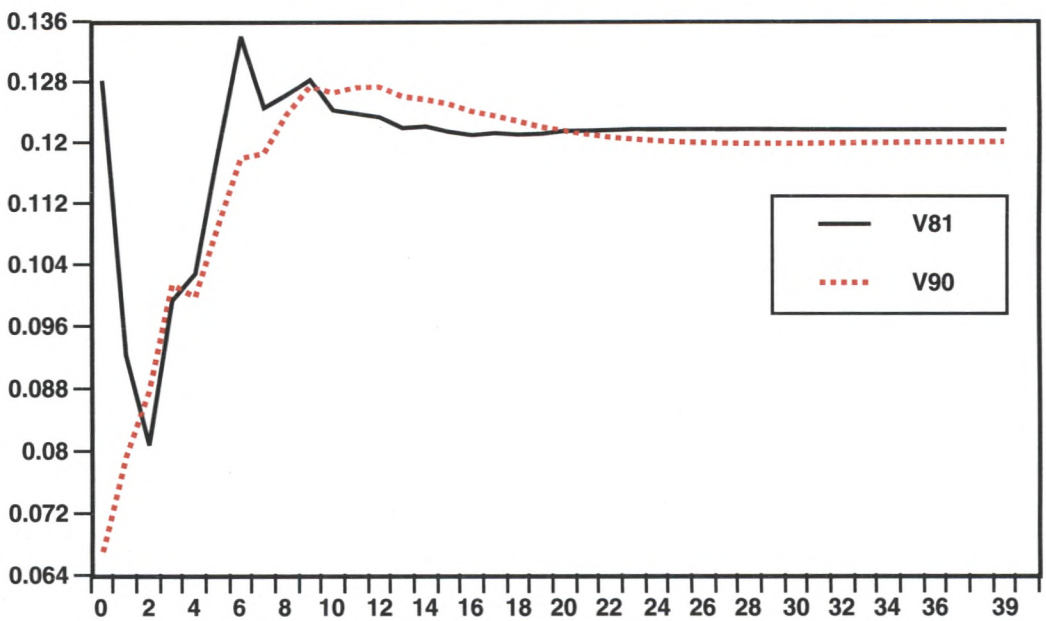


Table 3

Johansen Maximum Likelihood Estimation of Four Variable VECM ln(M/P), 400*ΔlnP, lnQ, Rtb (Real Balances and Real Income Coefficients Constrained)

Sample	Trace Test			Max λ Test			Normalized Cointegrating Vectors β_c'			
	r=0	r≤1	r≤2	r=0	r≤1	r≤2				
II/1955—IV/1974	39.8	16.9	.29	22.9	16.6	.29	1.0	0.0	−1.0	.1224
							0.0	1.0	0.0	−.9198
II/1955—IV/1975	52.1	21.7	2.2	30.4	19.5	2.2	1.0	0.0	−1.0	.1200
							0.0	1.0	0.0	−.7311
II/1955—IV/1976	52.7	20.3	2.4	32.4	18.0	2.4	1.0	0.0	−1.0	.1185
							0.0	1.0	0.0	−.7296
II/1955—III/1979	54.7	20.5	.31	34.1	20.2	.31	1.0	0.0	−1.0	.1109
							0.0	1.0	0.0	−.7315
II/1955—IV/1981	67.1	17.2	.77	49.9	16.4	.77	1.0	0.0	−1.0	.0965
							0.0	1.0	0.0	−.7498
II/1955—III/1990	64.8	22.6	4.6	41.3	18.9	4.6	1.0	0.0	−1.0	.0987
							0.0	1.0	0.0	−.8106
Critical values from Osterwald-Lenum (1990)										
10 percent	28.7	15.7	6.5	18.9	12.9	6.5				
5 percent	31.5	18.0	8.2	21.1	14.9	8.2				
1 percent	37.2	23.5	11.7	25.8	19.2	11.7				

defines additional interesting economic measures as linear combinations of the menu entries raises the following question: Are the results invariant to the explicit choice of menu entries? Clearly if the degree of differencing of the variables in the VAR were the same, the OLS estimates would produce the same results regardless of the particular linear combinations explicitly chosen. However, the degree of differencing varies among the variables in the typical VAR study as log levels of real output and nominal money appear along with log differences of the price level (inflation). An alternative menu is to enter real balances along with either inflation or nominal money growth. The advantage of these choices is that the three variables that are traditionally included in money-demand specifications—real balances, real output and nominal interest rates—now explicitly appear in the VAR.[34]

In table 3, some results are reported from the estimation of a VAR with real output, inflation, real money balances and the Treasury bill rate. These results indicate the tests for stationary linear combinations (cointegrating vectors) among the four variables using the Johansen maximum likelihood estimator under the restriction that the log of real balances and the log of real output enter any such cointegrating vectors with equal and opposite signs.[35] Both of the likelihood ratio tests—the trace test and the maximum eigenvalue test—typically reject the hypothesis of one or fewer cointegrating vectors at the 5 percent level, and in some samples at the 1 percent level. In every case the tests fail to reject the hypothesis that two or fewer cointegrating vectors exist. Thus we conclude that among these four variables there are two permanent and two transitory shocks.

To obtain a unique (to a scalar multiple) economic interpretation of the two cointegrating

[34]Such a VAR is an expanded version of the VAR used by Hoffman and Rasche [1992] to investigate long-run money demand.

[35]See Johansen (1988 and 1991). This restriction was imposed because it was never rejected in the three variable menus investigated by Hoffman and Rasche (1992) and because in that study the unrestricted long-run income and interest elasticities were found to be quite imprecise and sensitive to the choice of the sample period.

Table 4

Johansen Maximum Likelihood Estimation of Four Variable VECM ln(M/P), 400*ΔlnM, lnQ, Rtb (Real Balances and Real Income Coefficients Constrained)

Sample	Trace Test			Max λ Test			Normalized Cointegrating Vectors			
	r=0	r≤1	r≤2	r=0	r≤1	r≤2			β'_c	
II/1955—IV/1981	65.2	20.3	1.5	44.9	18.8	1.5	1.0	0.0	−1.0	.1068(.02)
							0.0	1.0	0.0	−.8067(.22)
Wald Test of Overidentifying Restriction $\beta'_{c(2,4)} = -1.0\ \chi^2_{(1)} = 1.54\ p = .21$										
II/1955—III/1990	75.8	26.1	5.3	49.7	20.8	5.3	1.0	0.0	−1.0	.1088(.03)
							0.0	1.0	0.0	−.8867(.22)
Wald Test of Overidentifying Restriction $\beta'_{c(2,4)} = -1.0\ \chi^2_{(1)} = .27\ p = .60$										
Estimates of Restricted Cointegration Vectors										
II/1955—IV/1981							1.0	0.0	−1.0	0.1217
							0.0	1.0	0.0	−1.0000
II/1955—IV/1990							1.0	0.0	−1.0	0.1202
							0.0	1.0	0.0	−1.0000

vectors present among these four variables, identifying restrictions must be imposed on the estimated matrix of cointegration vectors.[36] In this case the exclusion of one variable from each cointegrating vector is sufficient to achieve identification. The exclusion restrictions introduced here eliminate the inflation rate from one cointegrating vector and real balances from the other. The resulting identified cointegrating vectors, normalized for real balances and inflation respectively, are reported as $\hat{\beta}_c$ in table 3. The remaining unconstrained coefficients in these matrices are quite stable across sample periods. The estimated interest rate coefficient in the cointegrating vector with real balances is close to the estimate that Hoffman and Rasche obtained for the long-run interest semielasticity of money demand in the United States.[37] The estimated interest rate coefficient in the cointegrating vector with the inflation rate ranges from -0.9 to -0.7 and is not significantly different from -1.0 consistent with a long-run Fisher effect, which implies a stationary real interest rate.[38]

The difficulty in interpreting results from this specification of the VAR is that nominal money or its growth rate does not appear explicitly among the variables in the VAR. An alternative specification is to replace the inflation rate with the growth rate of nominal money and allow the inflation rate to be determined implicitly by the identity relating nominal money growth and real balances to inflation. Some results from the estimation of this VAR are presented in table 4 using the same sample periods as in table 1 and table 2. These results are basically the same as those in table 3. The Johansen likelihood ratio tests again reject the hypotheses that one or fewer cointegrating vectors exist. When the identifying exclusion restrictions and normalization are applied to the two estimated cointegrating vectors ($\hat{\beta}'_c$), the interest semielasticity in the velocity vector is approximately 0.11 and the interest coefficient in the vector error with the money growth rate is between -0.8 and -0.9. The latter estimates are not significantly different from -1.0 on the basis of a Wald test.[39]

[36]See Hoffman and Rasche (1991c).

[37]See Hoffman and Rasche (1992).

[38]Significance is examined using Wald tests developed by Johansen (1991). Stock and Watson (1991) also report evidence for a stationary real interest rate.

[39]See Johansen (1991).

Table 5

Estimates of Four Variable Restricted VECM: II/1955–III/1990

Dep. Variable:	$\Delta\ln(M/P)_t$	t	$400\Delta^2\ln(M_t)$	t	$\Delta\ln(Q_t)$	t	ΔRTB_t	t
$\Delta\ln(M/P)_{t-1}$	0.3418	2.07	44.87	0.78	0.1106	0.52	−41.32	−2.46
$\Delta\ln(M/P)_{t-2}$	0.3287	1.98	81.18	1.40	0.1793	0.84	−28.47	−1.68
$\Delta\ln(M/P)_{t-3}$	0.2738	1.62	81.06	1.37	0.1302	0.60	−6.98	−0.41
$400\Delta^2\ln M_{t-1}$	0.0000	0.008	−0.75	−4.56	−0.0004	−0.60	0.1166	2.44
$400\Delta^2\ln M_{t-2}$	−0.0004	−0.63	−0.79	−3.85	−0.0001	−0.17	0.2007	3.35
$400\Delta^2\ln M_{t-3}$	−0.0011	−1.61	−0.99	−4.32	−0.0007	−0.81	0.2795	4.18
$\Delta\ln Q_{t-1}$	0.1312	0.18	−3.73	−0.14	0.0926	0.98	8.22	1.09
$\Delta\ln Q_{t-2}$	0.0903	1.22	19.67	0.76	0.0598	0.63	22.31	2.97
$\Delta\ln Q_{t-3}$	−0.0473	−0.64	−16.49	−0.64	−0.1452	−1.54	0.2366	0.32
ΔRTB_{t-1}	−0.0066	−7.55	−2.13	−6.93	−0.0001	−0.11	0.0837	0.94
ΔRTB_{t-2}	−0.0005	−0.49	−0.15	−0.43	−0.0037	−2.81	−0.6973	−6.73
ΔRTB_{t-3}	−0.0010	−0.87	−0.20	−0.50	−0.0003	−0.19	0.1120	0.94
CONSTANT	−0.0118	−1.97	−8.99	−4.32	−0.0154	−2.10	−0.5980	−0.99
D67	0.0004	0.26	1.06	1.91	−0.0014	−0.67	−0.1429	−0.88
D79	0.0020	0.60	0.0116	0.01	0.0022	0.51	1.1099	3.26
D82	−0.0039	−1.04	−3.62	−2.77	−0.0055	−1.14	−0.3777	−1.00
$CIV1_{t-4}$	−0.0121	−1.96	−9.03	−4.18	−0.0258	−3.27	−0.5504	−0.88
$CIV2_{t-4}$	−0.0012	−1.81	−1.05	−4.62	−0.0008	−0.93	0.1792	2.71
$\bar{R}^2$	0.6100		0.54		0.24		0.4100	
SEE	0.0068		2.38		0.0087		0.6914	

NOTE: CIV1 and CIV2 are the two stationary linear combinations of the four dependent variables.

The vector error correction model (VECM) in table 4 can be reestimated with the overidentifying restriction $\hat{\beta}'_{c(2,4)} = -1.0$ imposed. The constrained estimates of $\hat{\beta}'_c$ are obtained using the two-step estimator in Rothenberg and the asymptotic covariance matrix for $\hat{\beta}'_c$ derived by Johansen.[40] The restricted estimates of $\hat{\beta}'_c$ are given at the bottom of table 4. These estimates are used to construct two linear combinations of the levels of the four different variables to obtain estimates of the remaining parameters of the restricted VECM. The estimated coefficients of the restricted VECM are shown in table 5 for the II/1955–III/1990 sample.[41]

The interesting question that these results raise is: Can the two permanent shocks among these four variables be associated with individual variables? Or in the terminology of King, Plosser, Stock and Watson (KPSW) (1991): Can we derive a structural model from the reduced-form model with steady-state characteristics suggested by economic theory? The interesting hypotheses to test are as follows:

- One permanent shock corresponds to a real-output (productivity) shock as suggested by real–business cycle theories; and
- The second permanent shock corresponds to a money growth–inflation–nominal interest rate shock consistent with a broad spectrum of macroeconomic theories.

The common-trends modeling approach of KPSW identifies the permanent components of each time series by restricting them to be random walks. A common-trends model exists if

[40]Rothenberg (1973) proves that his two-step estimator is a restricted maximum likelihood estimator when the unrestricted estimator is asymptoticly normal and coverges at rate $T^{-1/2}$. Johansen (1991) shows that his estimator of β'_c is asymptotic normal, but converges at rate T^{-1}. The maximum likelihood properties of the restricted estimator have not been established for this case.

[41]The dummy variables are not important for the estimation of the cointegrating vector (CIV) involving real balances. The separation of the shift in the constant of the VECM into components representing shifts in the deterministic trend and shifts in the mean of this cointegrating vector indicates that the mean of the CIV is little changed in the 80s compared with the previous 25 years. (See Yoshida and Rasche [1990].) The dummy variables are important for the estimation of the second (real interest rate) cointegrating vector. They suggest a large increase in the mean real interest rate during fourth quarter 1979–fourth quarter 1981 followed by a substantial, though not fully offsetting reduction in the mean real rate after 1981. This is consistent with the work of Clarida and Friedman (1984), Huizinga and Mishkin (1986) and Roley (1986) all of whom found shifts in the relationship of nominal rates and inflation in 1979 and 1982.

the permanent components of each time series are equal to linear combinations of the orthogonal permanent shocks that are suggested by economic theory. In the case under consideration here, the existence of the hypothesized common-trends model requires that the permanent components of real output and money growth are equal to the two permanent shocks and hence are orthogonal. These correlations are 0.047 and -0.065 for the samples ending in fourth quarter 1981 and third quarter 1990, respectively. The extent that the permanent components of real output and money growth violate the necessary conditions for the existence of a common-trends model can be judged by the size of the off-diagonal element of the Π matrix as defined by KPSW.[42] In the sample ending fourth quarter 1981 the estimated restricted VECM implies that $\Pi_{21} = 0.107$ and in the sample ending third quarter 1990 the estimated restricted VECM implies that $\Pi_{21} = -0.007$ under the identifying restrictions that the permanent components are random walks. Because the absolute values of these estimates are both close to zero, we conclude that the data are consistent with a common-trends representation with independent, permanent real-output and permanent money-growth shocks.

KPSW (1991) show how impulse response functions are constructed for permanent shocks in such a common-trends model. Graphs of these impulse response functions are shown in figures 5–18. The long-run properties of these impulse response functions are completely determined by the cointegrating vectors and the near orthogonality of the permanent components of real output and money growth. The long-run responses of velocity, inflation, money growth and nominal interest rates (figures 14, 16, 17 and 18) to a permanent shock to real output are all identically equal to zero. This follows from the orthogonalization of the common trends when real output is ordered before money growth. The long-run responses of real output to a permanent money growth shock are not identically zero (figure 10), reflecting the small correlations between the permanent components of real output and money growth. The long-run responses of inflation and nominal interest rates to a permanent money-growth shock (figures 11 and 13) are identically equal to 1.0 as determined by the values of the estimated coefficients in the cointegrating vectors. In the long run, the level of velocity is increased slightly by the permanent increase in money growth in response to the permanently higher value of nominal rates (figure 8). The long-run responses are consistent with the steady-state properties of most macroeconomic models, but this is not "news" once the elements of the cointegrating vectors have been estimated.

Additional interesting information can be found in these figures. Estimates for both samples suggest that the transitory responses to either permanent shock die out after two to three years. These implied lags in the adjustment to the steady state seem quite short relative to much of the conventional wisdom, though the length of the transitory reaction of velocity to a permanent money-growth shock is surprisingly similar to that in the AJ equation.

The reactions to a real-output shock are not exactly those implied by a pure real–business cycle model because output effects from this type of shock build only gradually (figure 15), during which period there are highly serially correlated negative impacts on the inflation rate (figure 16). The real output response here is quite similar to the output response to a "balanced-growth" shock obtained by KPSW in their six-variable restricted VAR model (figure 6).[43] There is a transitory money-growth response (figure 17) associated with the output shock, but because the money measure here, M1, includes inside money, this response is consistent with the picture drawn by some real–business cycle theorists.[44]

At first glance, it appears that the variance decomposition of real output in this model is consistent with the conclusion that "monetary instability has not played an important role in generating fluctuations."[45] The variance decomposition of real output from the fourth quarter 1981 sample indicates that the permanent "money-growth" shock accounts for about 23 percent of the variance of real output at all forecast horizons. In contrast, the permanent "real-output" shock accounts for only 7 percent of the variance of real output at a one-period hori-

[42]See King, Plosser, Stock and Watson (1991).

[43]See King, Plosser, Stock and Watson (1991).

[44]See Plosser (1991).

[45]See Litterman and Weiss (1985).

Figure 9
T-bill Rate irf to Nominal Money Shock

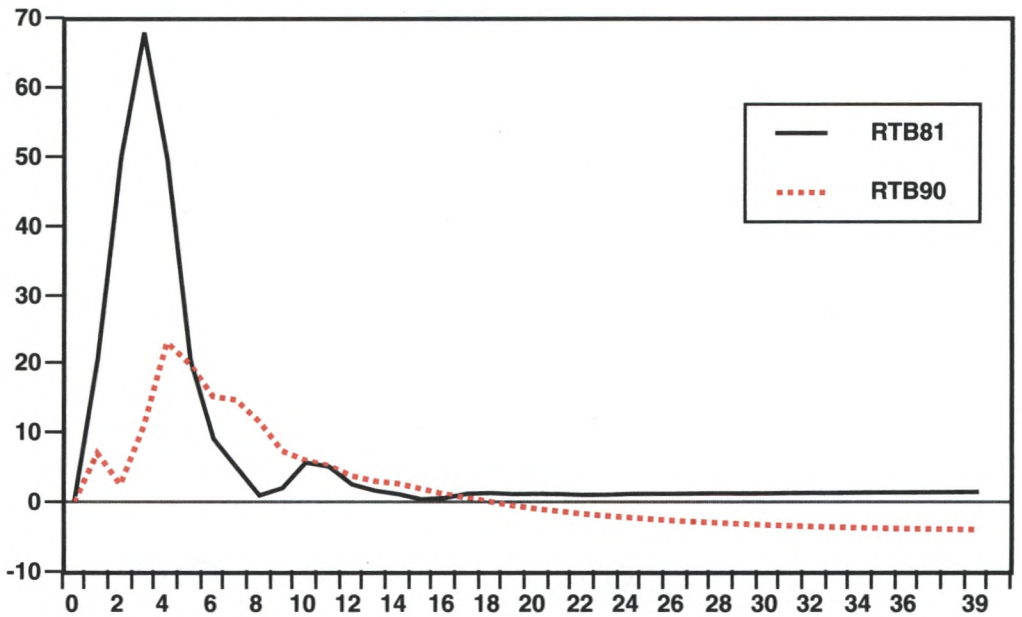


Figure 10
Real Output irf to Money Growth Shock

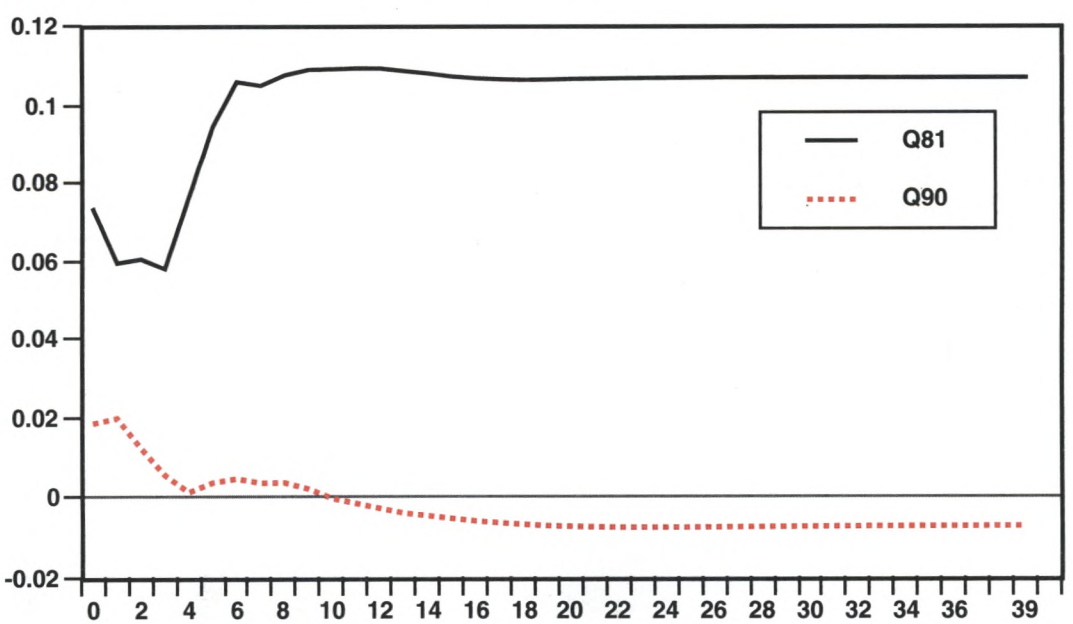


Figure 11
Inflation irf to Money Growth Shock

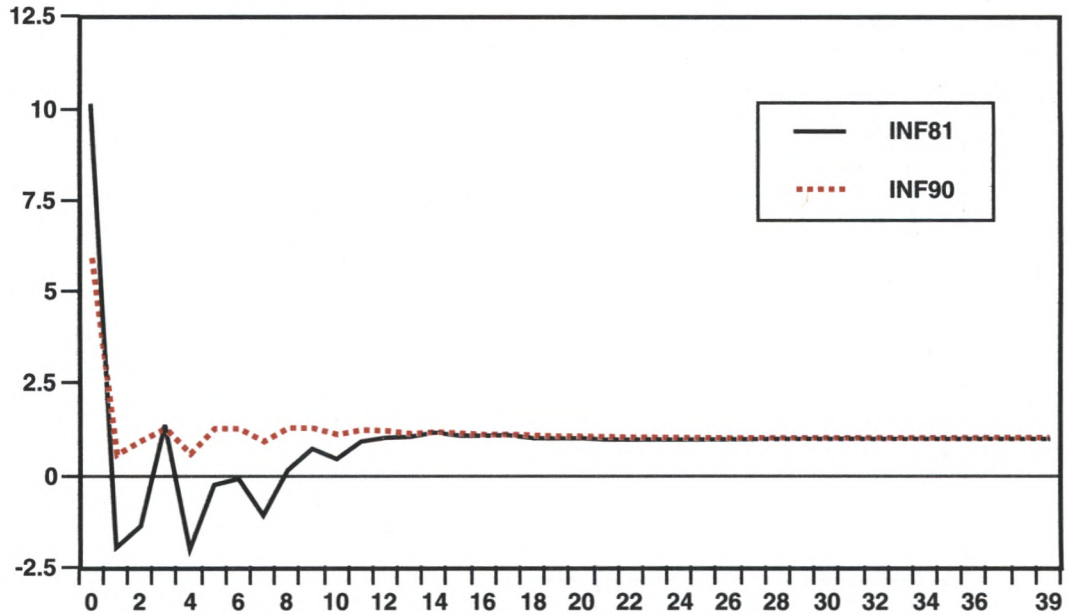


Figure 12
Money Growth irf to Money Growth Shock

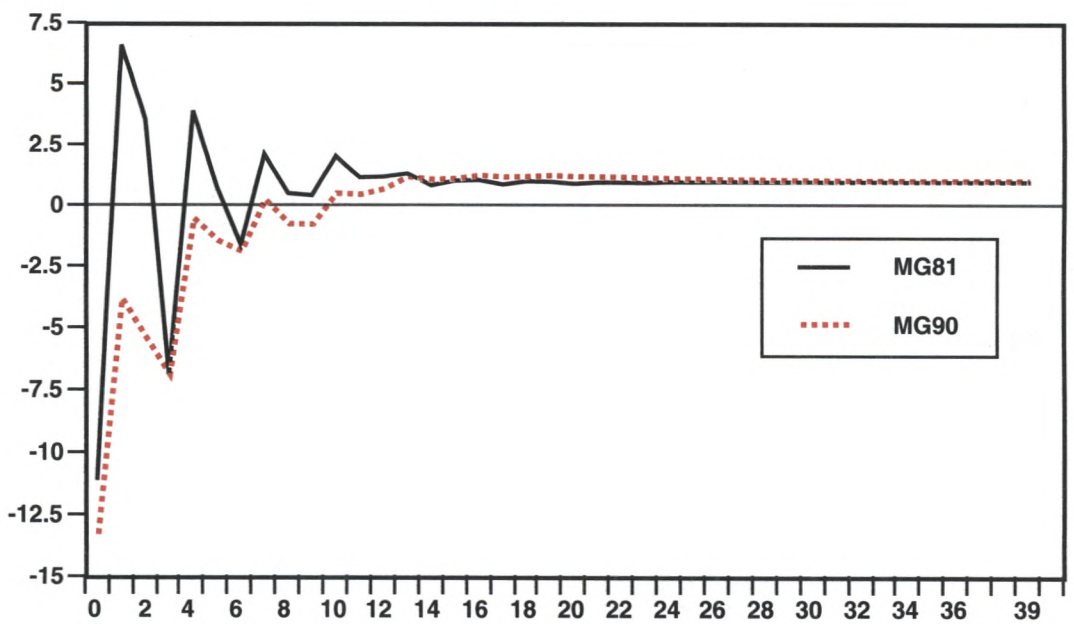


Figure 13
T-bill Rate irf to Money Growth Shock

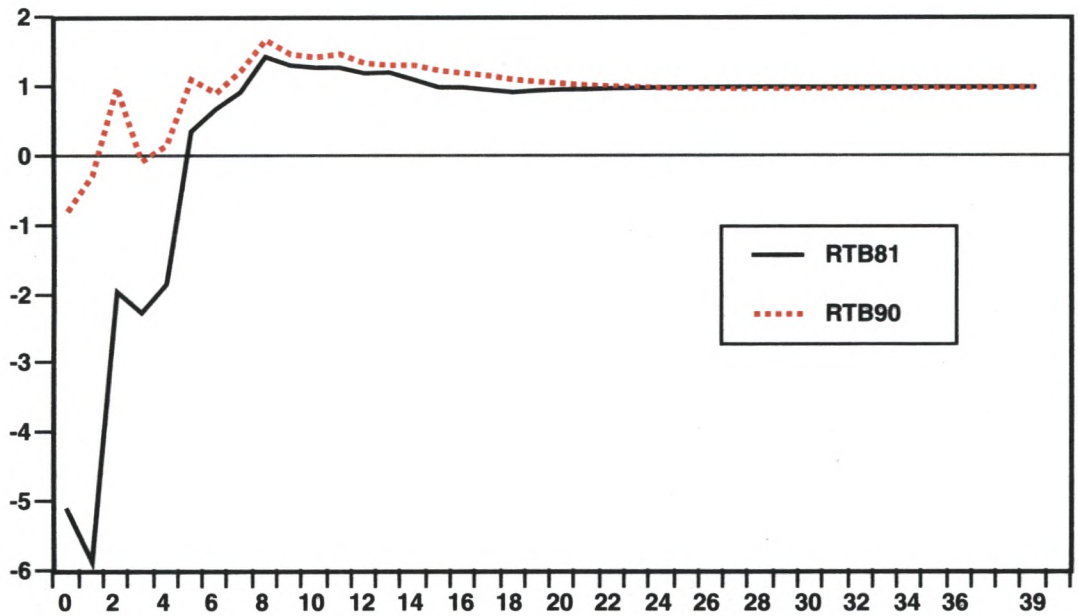


Figure 14
Velocity irf to Real Output Shock

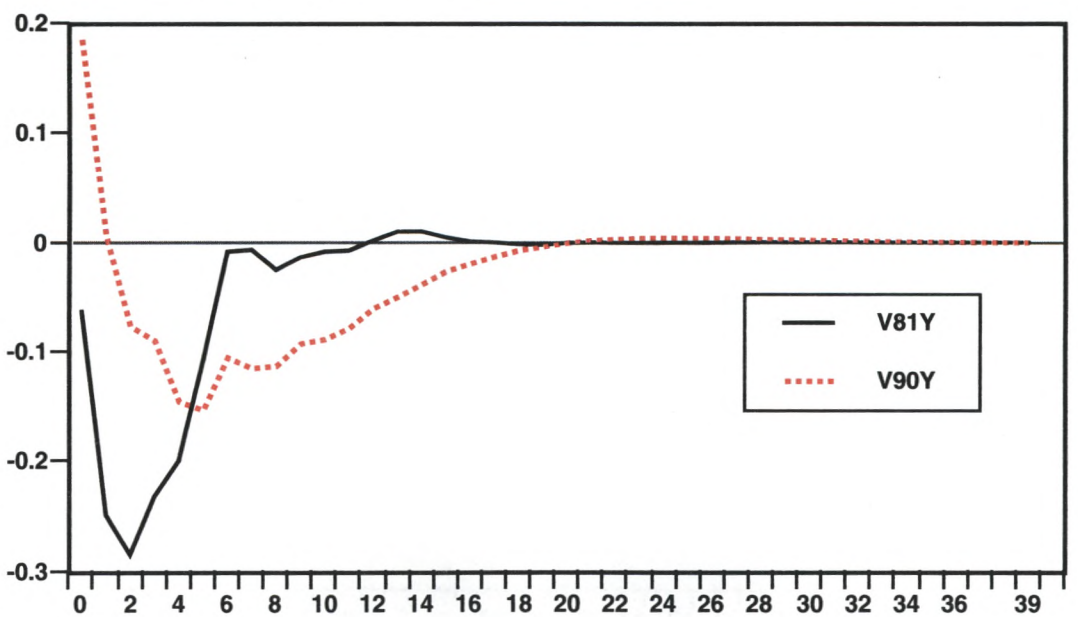


Figure 15
Real Output irf to Real Output Shock

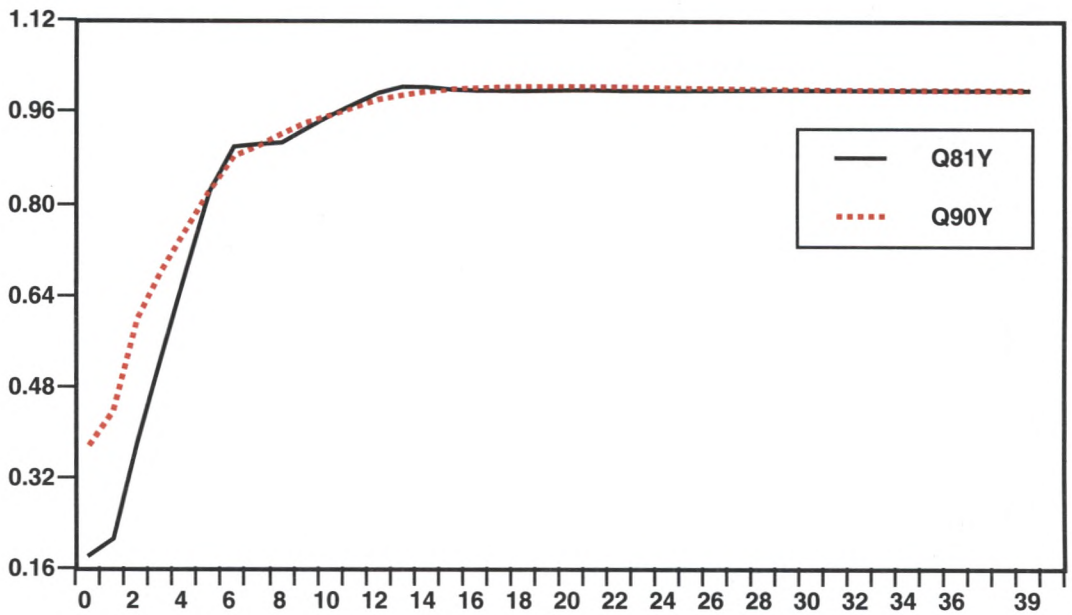


Figure 16
Inflation irf to Real Output Shock

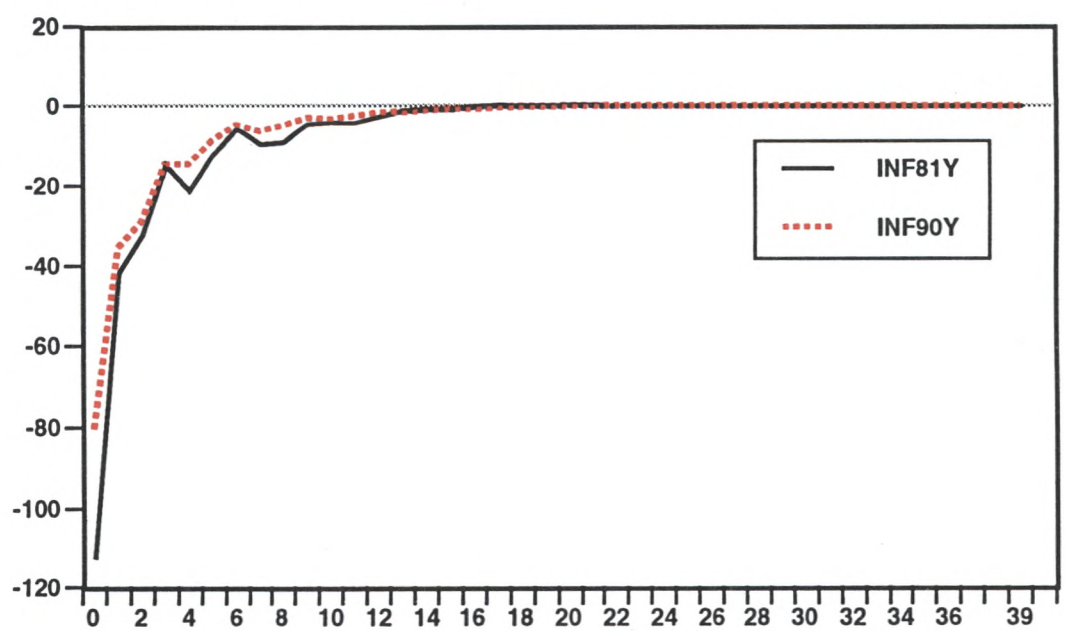


Figure 17
Money Growth irf to Real Output Shock

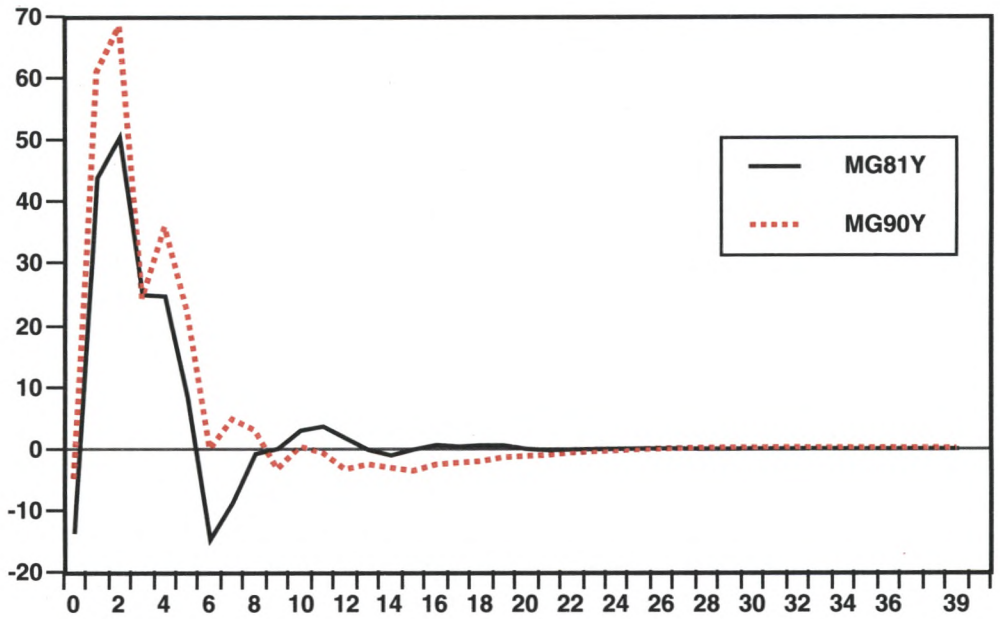
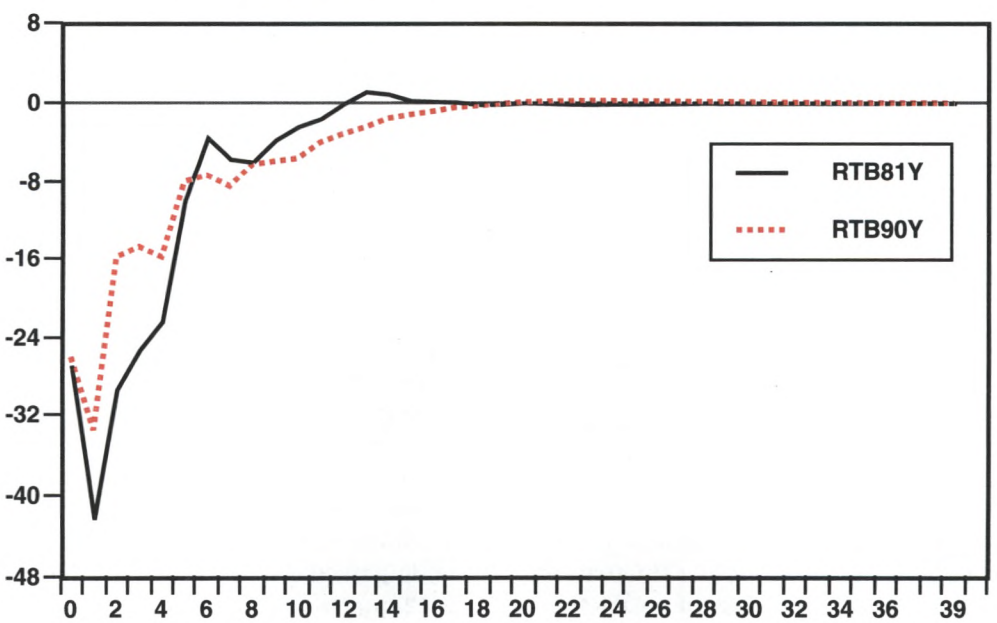


Figure 18
T-bill Rate irf to Real Output Shock



zon but increases to 66 percent of the variance at a 12-period horizon. When the sample is extended through third quarter 1990, the permanent "money-growth" shock accounts for only 7 percent of the forecast variance at a one-quarter horizon, and this declines steadily to one percent of the forecast variance at a 12-quarter horizon. In this sample the permanent "real output" shock accounts for 31 percent of the forecast variance of real output at a one-quarter horizon, and this increases steadily to 87 percent of the variance at a 12-quarter horizon. From this information and the impulse response functions in figure 10, it is tempting to conclude that monetary shocks have little short-run effect on real output.

The variance decomposition of each cointegrating vector can also be computed. At a one-quarter horizon 6 (32) percent of the variance of real balances around equilibrium real balances is attributable to the permanent "money-growth" shock, 27 (24) percent is attributable to the permanent "real-output" shock, and 67 (44) percent is attributable to the two transitory shocks. At a 12-quarter horizon the corresponding decomposition is 4 (21) percent and 71 (48) percent. At a one-quarter horizon the corresponding decomposition of the variance of the real interest rate around the equilibrium real interest rate is 41(3) percent, 1 (1) percent and 58 (96) percent. At a 12-quarter horizon the decomposition is 24 (10) percent, 20 (27) percent and 56 (63) percent. These decompositions are based on the third quarter 1990 (fourth quarter 1981) sample estimates.

It is also possible to allocate the deviation of actual real balances from equilibrium real balances (or the actual real rate from the equilibrium real rate) at any point in the sample period to the history of the permanent and real shocks. Following KPSW (1991) write $X_t = \mu t + A\tau_t + \Gamma^*(L)\eta_t$ where η_t is a vector of "structural" disturbances.[46] Let β'_c be the matrix of cointegrating vectors. Then $\beta'_c X_t$ measures the deviations of actual real balances from equilibrium real balances and the actual real rate from the equilibrium real rate. But $\beta'_c X_t = \beta'_c \mu t + \beta'_c A\tau_t + \beta'\Gamma^*(L)\eta_t = \beta'_c \Gamma^*(L)\eta_t$ because β'_c is orthogonal to μ and A.

The fallacy of concluding that monetary instability is not important for economic fluctuations from this system under this class of restrictions involves the interpretation of the "money-growth" shock (figure 12). Ultimately this shock becomes a maintained change in the growth of nominal money. However this is not the case initially. For the first two to three years, the money growth response to the permanent "money-growth" shock contains a large transitory component and the net effect is frequently of the opposite sign to the permanent effect. This response pattern certainly does not conform to the traditional monetarist policy experiment. In the latter case, the policy intervention involves a shift from one maintained growth rate of money (or the monetary base) to a different maintained monetary growth rate. Under these conditions the traditional monetarist hypothesis is that the initial impact of the policy intervention will largely affect real output, but that over time this effect will disappear as the inflation rate approaches its new steady-state rate.[47]

The only identifying characteristic of a monetary shock in this analysis is the steady-state restriction that the impulse response of money growth to such a shock is one. However, this restriction does not define a unique monetary shock, but rather a whole class of such shocks. This is clear from the impulse responses of money growth to the two "transitory shocks" that are plotted in figures 19 and 20. By construction, in both samples the steady-state response of money growth (and all other variables defined by the VAR) is zero. Thus it is possible to define the class of monetary shocks equal to the permanent "monetary shock" plus any weighted sum of the two transitory shocks and satisfy the identifying restriction for a monetary shock. Within this class of monetary shocks it is impossible to determine the short-run impact of monetary policy on real output. For example, consider defining the response of real output as the sum of the responses to the permanent "money-growth" shock and the two transitory shocks. Such a composite shock has the identical steady-state response as the permanent "money-growth" shock and so satisfies the identifying restrictions for a permanent monetary intervention imposed by our model. Yet on a one-quarter forecasting horizon such a composite shock accounts for 69 (93) percent of the variance in real output for the sample period ending third quarter 1990 (fourth quarter 1981). On a 12-quarter horizon the fraction of the forecast

[46]See Appendix B.

[47]See Friedman (1974) and Andersen and Carlson (1970).

Figure 19
Money Growth irf to Transitory Output Shock

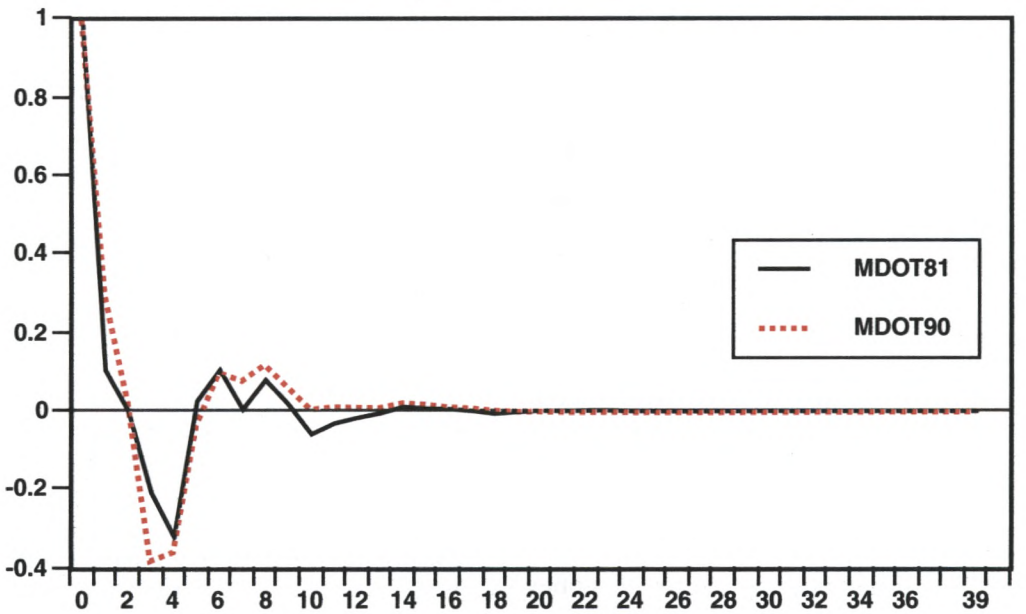
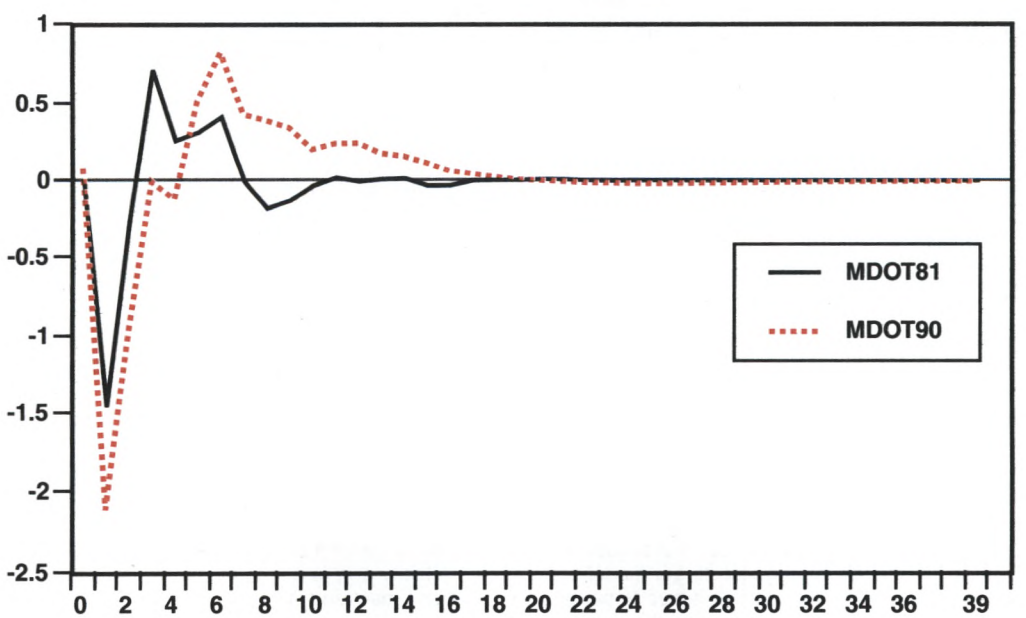


Figure 20
Money Growth irf to Second Transitory Shock



variance in real output attributable to such a composite shock decreases to 13 (34) percent for the sample period ending in third quarter 1990 (fourth quarter 1981).

The fraction of the variance of deviations of real balances from equilibrium real balances attributable to this composite shock is 73 (76) percent at a one-quarter horizon and 75 (66) percent at a 12-quarter horizon for the sample period ending third quarter 1990 (fourth quarter 1981). The fraction of the variance of deviations of the real interest rate attributable to this composite shock is 99 (99) percent at a one-quarter horizon and 80 (73) percent at a 12-quarter horizon for the sample periods ending third quarter 1990 (fourth quarter 1981).

In contrast, the monetary intervention of traditional monetarist analysis is not contained in the general class of monetary shocks defined as the permanent "monetary shock" plus a weighted sum of the transitory shocks. Consider a regression of the following form:

$$(IMPMP_i - 1.0) = \beta_1 IMPMT1_i + \beta_2 IMPMT2_i + \varepsilon_i$$

where $IMPMP_i$ is the impulse response of money growth to the permanent "money shock" and $IMPMT1_i$ and $IMPMT2_i$ are the impulse responses of money growth to the transitory shocks. The traditional monetarist policy experiment is defined in the class of identified monetary shocks if there are β_is that produce an estimated impulse response pattern that replicates the deviations of the impulse response function to the permanent "money shock" from unity. This result does not hold for either sample period. For the sample ending fourth quarter 1981,

$$(IMPMP_i - 1.0) = \underset{(-10.56)}{-11.39(IMPMT1_i)} - \underset{(-9.28)}{6.19(IMPMT2_i)} + \varepsilon_i$$

$$\bar{R}^2 = 0.81 \quad SEE = 1.15$$

while for the sample period ending third quarter 1990,

$$(IMPMP_i - 1.0) = \underset{(-4.31)}{-9.06(IMPMT1_i)} - \underset{(-1.0)}{0.94(IMPMT2_i)} + \varepsilon_i$$

$$\bar{R}^2 = 0.23 \quad SEE = 2.47$$

The weighted-sum impulse response functions for money growth are shown in figures 21 and 22 for the two sample periods. Large transitory deviations from unity remain in both cases.

The lack of identification of the short-run real output response in the absence of a specification of the monetary rule, or monetary policy reaction function, that prevails during the sample period can be shown easily using a simple macroeconomic model that satisfies all of the steady-state identifying restrictions imposed on the VECM. Consider the following:

$$(1)\ \ln P_t = {}_{t-1}\ln P_t^e + \gamma[\ln Q_t - \ln Q_t^n] + \varepsilon_{1t}$$
$$(2)\ \ln MR_t = \ln Q_t - \beta i_t + \varepsilon_{2t}$$
$$(3)\ i_t = r_t + {}_t\ln P_{t+1}^e - \ln P_t$$
$$(4)\ \ln Q_t = k + \ln A_t - \alpha r_t + \varepsilon_{3t}$$

where equation (1) is an expectations-augmented Phillips curve (Lucas supply function) that relates deviations of real output (Q_t) from natural output (Q_t^n) to inflation expectation errors ($\ln P_t - {}_{t-1}\ln P_t^e$). Equation (2) is a money-demand function that relates real money balances (MR_t; $\ln MR_t = \ln M_t - \ln P_t$) to real output and nominal interest rates (i_t) with a unitary income elasticity of money demand. Equation (3) defines nominal interest rates as the sum of the real rate (r_t) and the expected future rate of inflation (${}_t\ln P_{t+1}^e - \ln P_t$). Equation (4) defines the demand for real output in terms of the real interest rate and autonomous planned expenditures (A_t). This model is closed by two additional specifications. First, we assume that expectations are generated by adaptive expectations of inflation:[48]

$$(5)\ {}_{t-1}p_t^e = {}_{t-2}p_{t-1}^e + \lambda(p_{t-1} - {}_{t-2}p_{t-1}^e)$$

where $p_t = \ln P_t - \ln P_{t-1}$ and ${}_{t-i}p_{t-i+1}^e = {}_{t-i}\ln P_{t-i+1}^e - \ln P_{t-i}$

Second, a stochastic monetary rule (policy reaction function) is specified as follows:

$$(6)\ \Delta\ln M_t = \mu + \phi_1 \Delta i_t + \phi_2(\Delta\ln M_{t-1} - \mu) + \varepsilon_{4t}$$

This rule allows for contemporaneous interest rate smoothing ($\phi_1 > 0$) and for offsetting of past deviations from the steady-state money growth

[48]Adaptive expectations in the inflation rate are chosen as an algebraicly convenient way of generating a model that potentially has transitory real output responses to permanent nominal money growth shocks and has the steady-state characteristics of the estimated VECM. This is only for illustration of the identification problem. In particular, the type of inflation expectation shift discussed in the section beginning on p. 3 as the root cause of the shift in velocity drift is not consistent with an adaptive expectations mechanism.

Figure 21
Weighted Sum of Permanent and Transitory irf

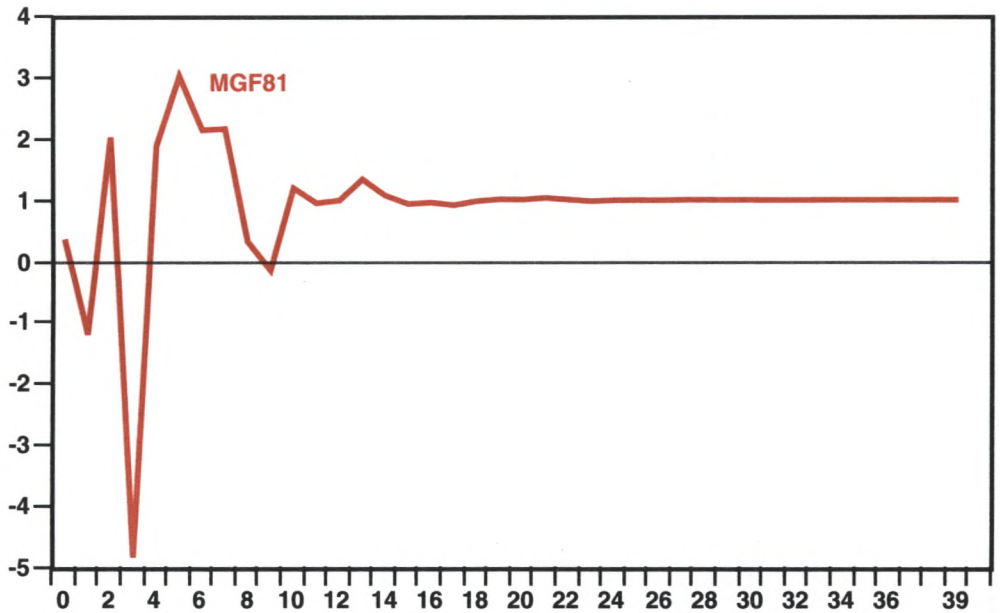


Figure 22
Weighted Sum of Permanent and Transitory irf

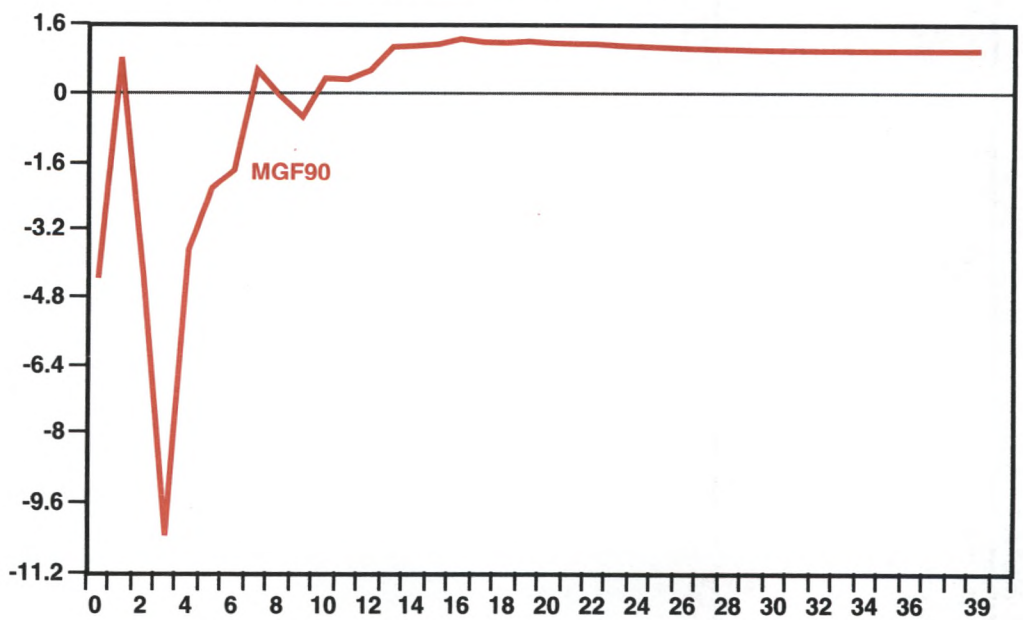


Table 6

Adjoint Matrix (A*)

$-[1+\Phi_2 B]\alpha[1-(1-\lambda)B-\beta\lambda(1-B)] + \alpha\Phi_1\lambda[1-B]$	$-[1+\Phi_2 B]\alpha[1-B]^2$	$[\beta(1+\Phi_2 B) + \Phi_1][1-B]^2$	$\alpha[1-B]$
$-[1+\Phi_2 B][(\alpha+\beta)(1-(1-\lambda)B)] + \alpha\Phi_1\lambda[1-B]$	$-[1+\Phi_2 B]\alpha\gamma[1-(1-\lambda)B] + \Phi_1[1-\alpha\gamma\lambda-B][1-B]$	$-[1+\Phi_2 B]\beta\lambda[1-(1-\lambda)B] - \Phi_1[1-B]^2$	$[(\alpha+\beta)(1-B) - \alpha\beta\gamma\lambda]$
$[1+\Phi_2 B][(1+\alpha\lambda)(1-B) + \lambda B]$	$[1+\Phi_2 B][1-\alpha\gamma\lambda-B][1-B]$	$[1+\Phi_2 B][\gamma(1-(1-\lambda)B) + (1-B)]^2$	$-[1-\alpha\lambda-B]$
$\Phi_1[(1+\alpha\lambda)(1-B) + \lambda B][1-B]$	$\Phi_1[1-\alpha\gamma\lambda-B][1-B]^2$	$\Phi_1[\gamma-\gamma(1-\lambda)B + (1-B)^2][1-B]$	$[\alpha\gamma(1-(1-\lambda)B) + (\alpha+\beta)(1-B)^2 - \alpha\beta\gamma\lambda(1-B)]$

$$\det = [1+\Phi_2 B][\alpha\gamma(1-(1-\lambda)B) + (\alpha+\beta)(1-B)^2 - \alpha\beta\gamma\lambda(1-B)] + \Phi_1[1-\alpha\gamma\lambda-B][1-B]$$

path ($\phi_2>0$). Thus with appropriate parameter values this specification can accommodate a range of central bank behavior from nominal–interest rate smoothing to a stochastic no-feedback money growth regime. This model can be reduced to a four-variable VAR in $\ln Q_t$, $\ln MR_t$, i_t and $\Delta \ln M_t$, driven by the exogenous variables $\ln Q_t^n$, $\ln A_t$ and the shocks ε_{it}. With some tedious algebra, the moving average representation of the model can be expressed as follows:

$$(7)\quad \begin{bmatrix} \ln Q_t \\ \ln MR_t \\ i_t \\ \Delta \ln M_t \end{bmatrix} = \begin{bmatrix} \frac{1.0}{\det} \end{bmatrix} * A^* \begin{bmatrix} -\gamma \ln Q_t^n + \varepsilon_{1t} \\ \varepsilon_{2t} \\ k + \ln A_t + \varepsilon_{3t} \\ \mu(1+\phi_2) + \varepsilon_{4t} \end{bmatrix},$$

where the polynomial matrix A^* and the polynomial det are given in table 6. In the deterministic steady state, the impulse response functions are independent of the parameters of the monetary rule (ϕ_1, ϕ_2) and real output responds only to changes in $\ln Qn_t$ (1.0). Similarly in the deterministic steady-state $\Delta \ln M_t$ responds only to μ (1.0). However, the transitory responses of real output to money-growth shocks are not zero. In particular, the greater is the interest-rate smoothing (ϕ_1), the *smaller* are the transitory responses of real output to monetary shocks. Thus estimation of VARs in this type of model will produce different impulse response functions based on different behaviors of the monetary authorities, and it is not possible to infer from those impulse response functions the short-run impact of a change in money growth under a no-feedback rule, without prior knowledge of the form and parameter values of the sample period monetary rule(s).

A recent analysis by Strongin (1991) is an attempt at defining a monetary policy disturbance. His identifying restriction is that monetary policy shocks have exactly offsetting impacts on nonborrowed reserves and borrowed reserves and hence have no effect on total reserves. In contrast he assumes that "reserve-demand" shocks in principle affect all three aggregates. Much of Strongin's discussion of historical Federal Reserve operating procedures focuses on the likely distribution of reserve-demand shocks (his ϕ parameter) between nonborrowed and borrowed reserves. The size of this parameter is not relevant to his identification problem, though it is important for estimation if the parameter value differs across subsamples.[49] The identifying restriction allows him to construct a measure of monetary policy shocks but does not address the structure of the monetary rule or policy reaction function. Strongin implicitly assumes that there is no contemporaneous feedback from interest rates onto his monetary-policy shock because both total and nonborrowed reserves precede the fed funds rate in his Wold causal chain. Thus his identifying restriction does not address all of the problems raised here.

Unfortunately, inference about monetary regimes (policy reaction functions) using regression techniques has proved illusory. Khoury (1990) reviews 42 attempts at the estimation of reaction functions for the Fed over various sample periods. He concludes that "the results were in disarray" and "the specification search showed that very few variables were robust in a reaction function ... consistent with the lack of robustness in the literature." The additional attempts at developing reaction functions that are included in Mayer do not overturn this conclusion.[50] Thus it appears appropriate to conclude that at present we lack adequate information to make inferences from time series analyses on the vexing question of the short-run impact of nominal shocks on real output.

CONCLUSIONS

Significant elements of the St. Louis research agenda are now widely accepted, at least in U.S. academic circles and to some extent within the Federal Reserve System. Nevertheless, issues of short-run impacts of monetary policy remain unresolved. Among these are the following two critical topics: 1) changes in the drift of velocity and the extent to which such changes are generated by changes in inflation expectations and 2) the short-run impacts of nominal money shocks on real output.

The first of these questions is critical to the design of monetary rules and/or operating procedures that will retain credibility during the

[49]The exclusion of monetary policy disturbances from total reserve shocks is analogous to identifying the supply function by the exclusion of income in a classical demand/supply model.

[50]See Mayer (1990).

transition to an alternative inflation regime. The second question has long been debated and appears to be re-emerging as a focus of time series analysis. The analysis presented here suggests that the information necessary to pursue this agenda successfully is not yet available. One critical precondition to such analysis is a reasonable specification of the monetary regime(s) during the sample period. In this respect, Cagan's (1989) appeal for more "historical" research warrants careful consideration.

A potential application of such a historical analysis is a test of Strongin's (1991) identifying restriction for monetary policy shocks. His restriction provides an estimated time series for such shocks. We can infer from published *Records of Policy Actions* of the FOMC the timing and to some extent the magnitude of policy interventions to change the fed funds rate and/or borrowed reserves targets.[51] If the identifying restriction is valid, time series estimates of the monetary policy shocks should correlate well with the data extracted from these historical records.

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[51] See Brunner and Meltzer (1964) and Rasche (1987).

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Appendix A Technical Description of the Assumptions in the Simulation of the McCallum Rule

The initial regime (periods 1-19) in figures 3 and 4 before the implementation of the McCallum rule are base velocity growth at $t_v = .0075$ per period. The monetary base is assumed to grow at a rate that increases at $t_r = .0001563$ per period. Thus nominal income growth is increasing at a rate of $t_r = .0001563$ per period. The rate of increase in nominal income growth is assumed to reflect the trend in inflation, which in turn is assumed to reflect the trend in nominal interest rates. The trend in nominal interest rates and the trend in velocity must satisfy the restriction $t_v - e_r t_r = 0$ where e_r is the interest semielasticity of velocity if base velocity and interest rates are cointegrated. e_r is assumed to be 48, using the estimated semielasticity of M1

velocity from Hoffman and Rasche [1992]. Nominal income and nominal potential income are assumed equal throughout this period.

The regime switch to the McCallum rule is announced and implemented in period 20. The desired growth rate of nominal income in the new regime is .0075 per period. It is assumed that the announcement of the new policy results in an immediate elimination of base velocity growth.

It should be noted that the path of nominal income growth once the McCallum regime is implemented is independent of the assumptions about growth in the prior regime. Nominal income growth in the McCallum regime is totally determined by the assumed growth of velocity starting 16 periods prior to the implementation of the rule, and the reaction of velocity growth to the institution of the new regime. The particular initial conditions for base growth used here are chosen strictly for consistency with the assumed initial growth rate of base velocity.

Appendix B Confidence Intervals for Impulse Response Functions

Estimates of the precision of the impulse response functions from the sample ending in 90:3 were constructed from a Monte Carlo integration. The estimated coefficients and covariance matrix of residuals from a VAR augmented by two error correction variables were shocked using the algorithm described in Doan [1990], example 10.1. The elements of the cointegrating vectors were held constant at their estimated values, since Johansen [1991], Theorem 5.5, proves that the estimated asymptotic covariance matrix of $\hat{\Pi} = \hat{\alpha}\,\hat{\beta}'$ depends only on the estimated asymptotic covariances of $\hat{\alpha}$ and the estimated $\hat{\beta}$ and not on the estimated asymptotic covariance of $\hat{\beta}$. 1000 replications on the parameter values were constructed and the parameters of the KPSW common trends model were recomputed for each replication. The mean value of KPSW's critical Π_{21} parameter across all replications is -.0057 with a standard deviation of .0371. These parameters were used to derive impulse response functions. The means of various impulse responses across the 1000 replications are plotted in Figures A1–A10, together with confidence bands of $\pm$ 1.96* (standard deviations of the impulse responses across replications). The graphs suggest that the short-run responses of real output and velocity with respect to both permanent shocks are measured with considerable precision, in particular that the real output response to a permanent "money growth" shock is initially significantly positive and that the real output response of a permanent " real output" shock is significantly less than 1.0 for about 10 quarters. In contrast, the measurement of the short-run responses of inflation, money growth, and interest rates to both permanent shocks is highly imprecise.

Appendix C Sources of Data

All data series were extracted from Citibase. The primary sources are as follows:

Treasury Bill Rate: Three month secondary market rate. Federal Reserve Bulletin. Table 1.35, line 15.

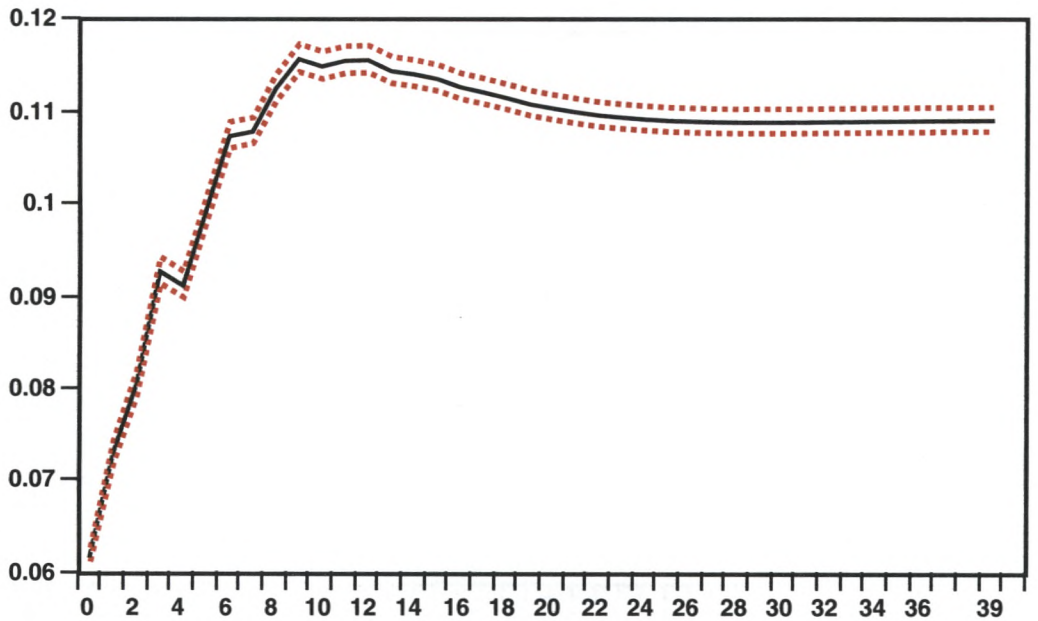
M1: Seasonally adjusted monthly data.
January 1947–December 1958 constructed following Rasche (1987), Appendix A.
January 1959–December 1989 Board of Governors of the Federal Reserve System, Money Stock Revisions, March 1990, Table 1.
January 1990–March 1990 Federal Reserve Bulletin, July 1990, Table 1.21, line 1.
April 1990–June 1990 Federal Reserve Bulletin, October 1990, Table 1.21, line 1.
July 1990 Federal Reserve Bulletin, January 1991, Table 1.21, line 1.
August 1990 Federal Reserve Bulletin, February 1991, Table 1.21, line 1.

GNP: Seasonally adjusted quarterly data.
January 1947–April 1987 Survey of Current Business, July 1990
January 1988–March 1990 Economic Report of the President, February 1991, Table B-1.

Real GNP: Seasonally adjusted quarterly data.
January 1947–April 1987 Survey of Current Business, July 1990
January 1988–March 1990 Economic Report of the President, February 1991, Table B-2.

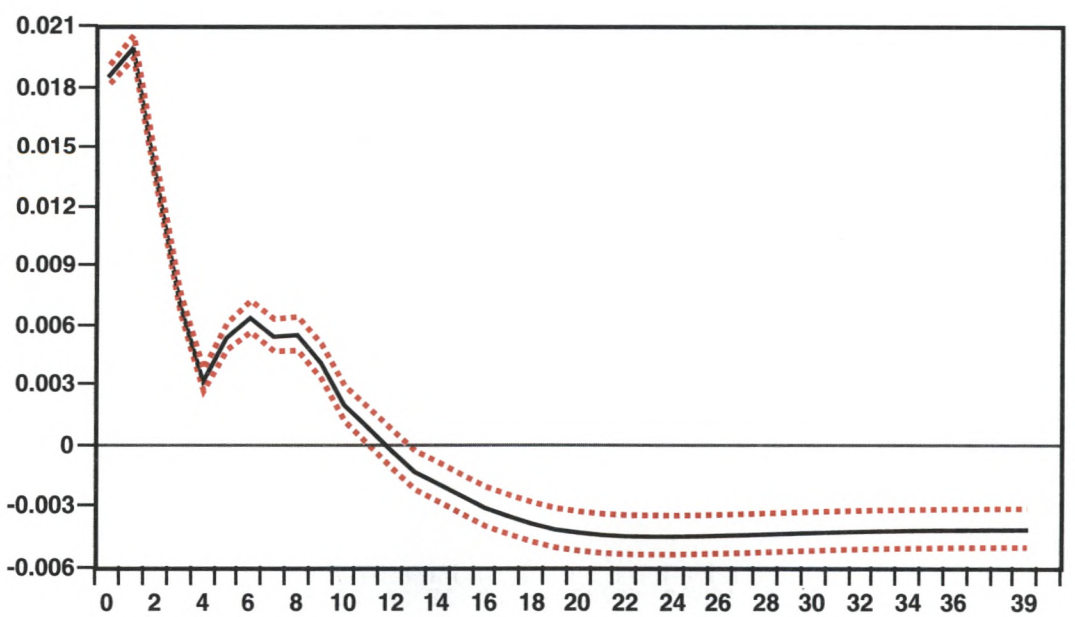
Appendix Figure 1

Velocity irf to Money Growth Shock



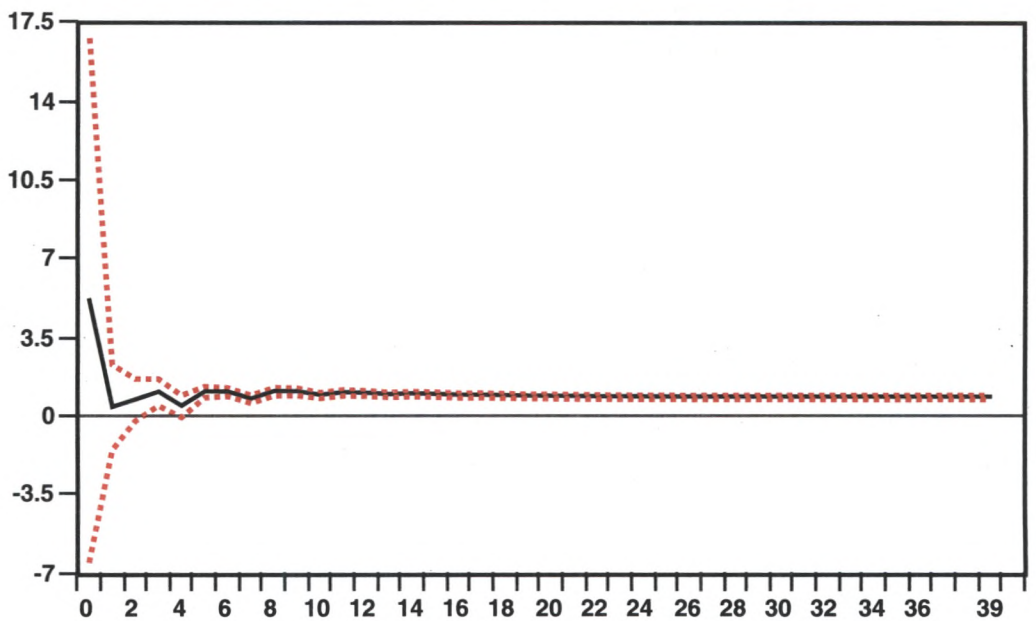
Appendix Figure 2

Real Output irf to Money Growth Shock



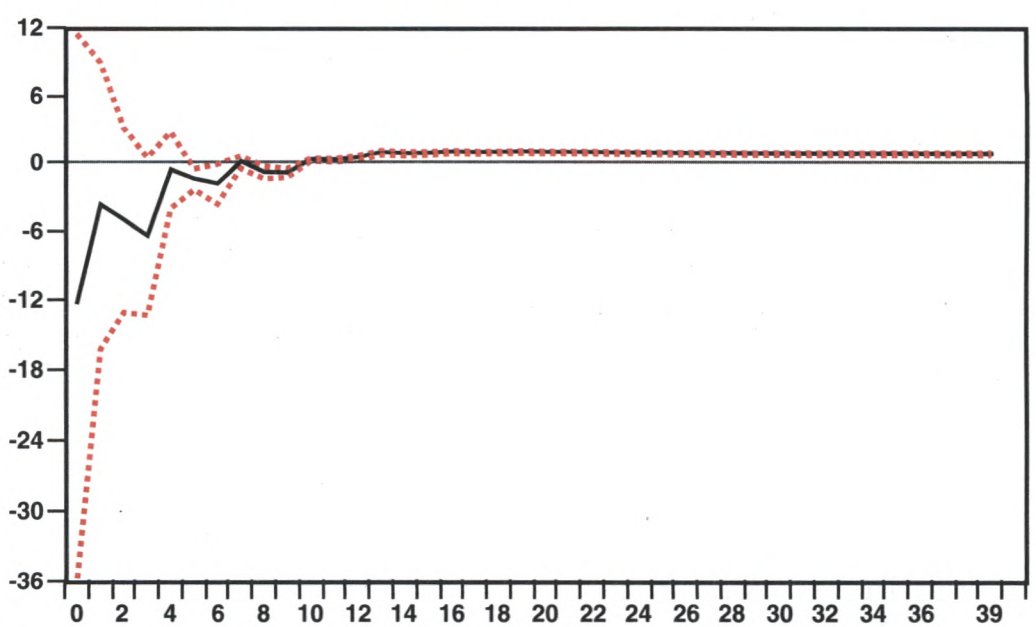
Appendix Figure 3

Inflation irf to Money Growth Shock



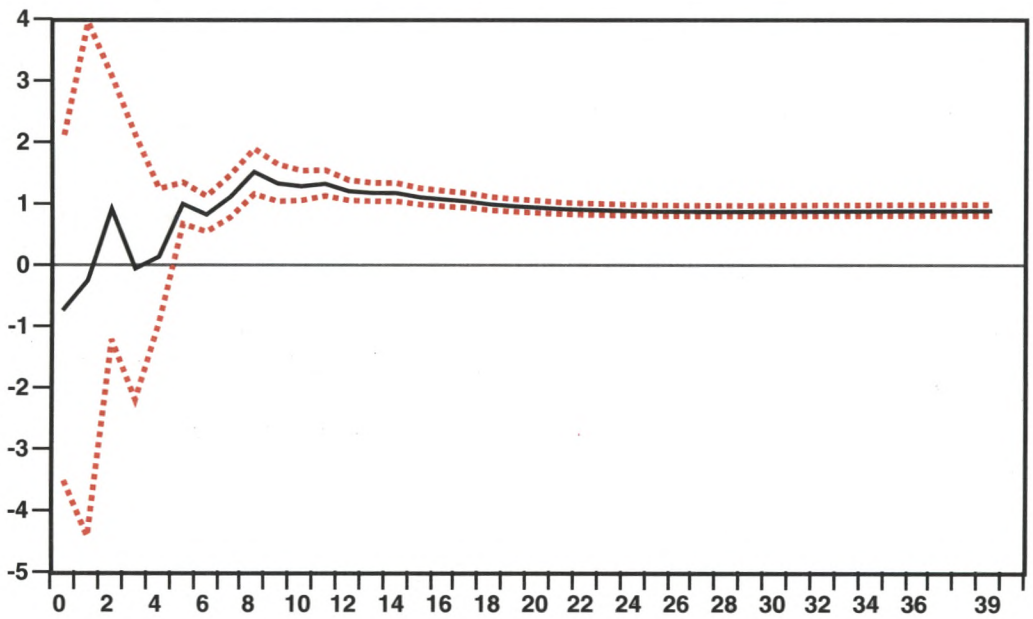
Appendix Figure 4

Money Growth irf to Money Growth Shock



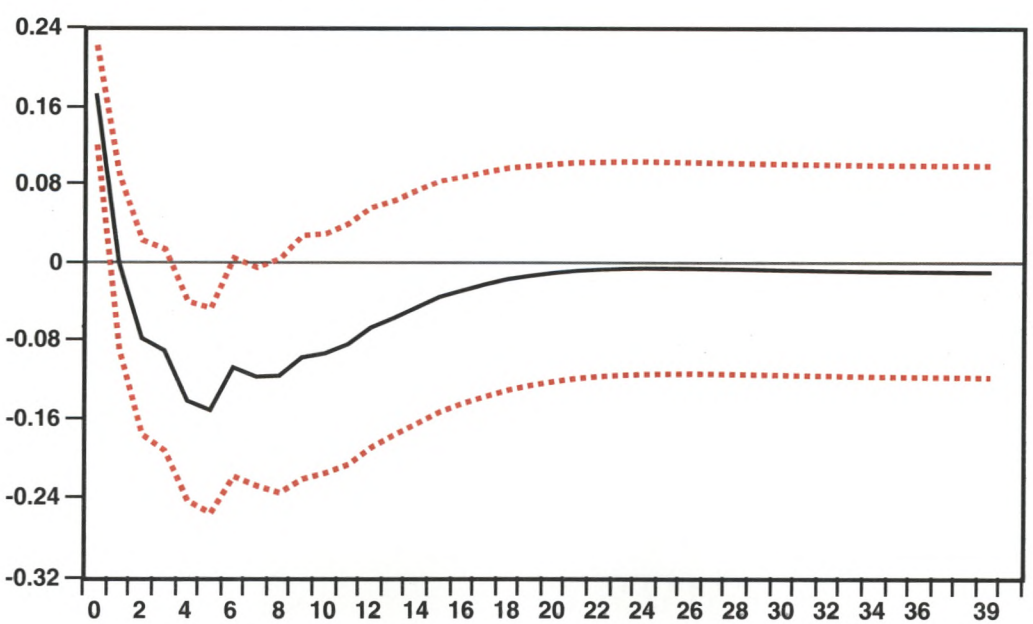
Appendix Figure 5

T-bill Rate irf to Money Growth Shock



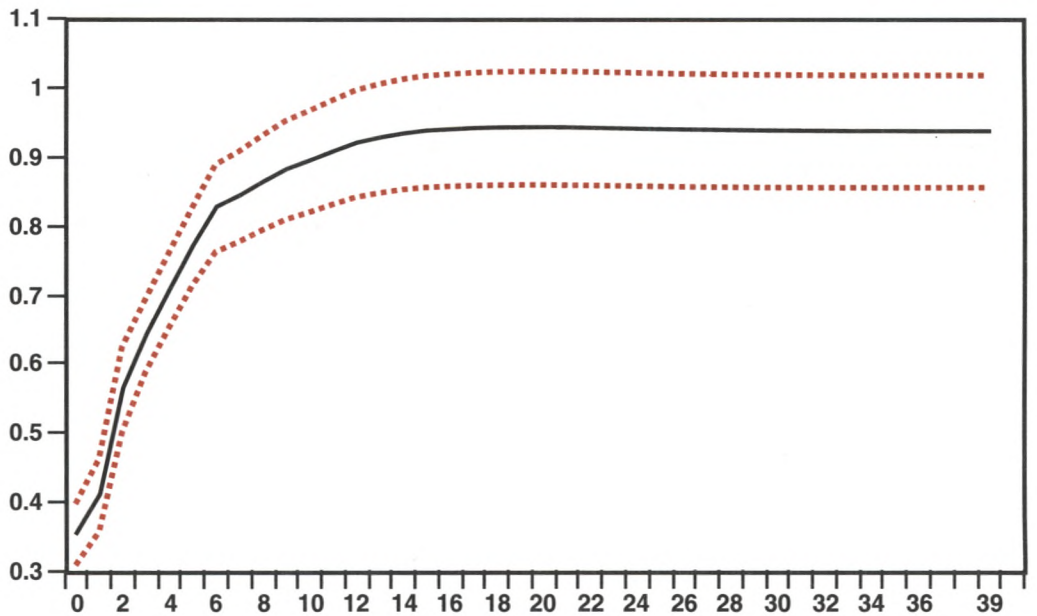
Appendix Figure 6

Velocity irf to Real Output Shock



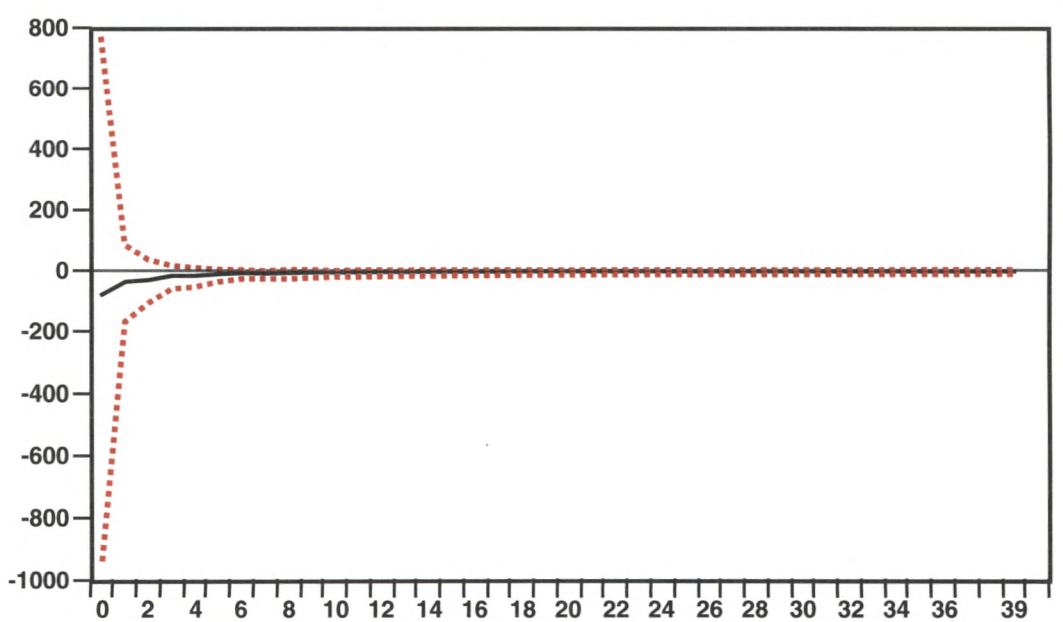
Appendix Figure 7

Real Output irf to Real Output Shock



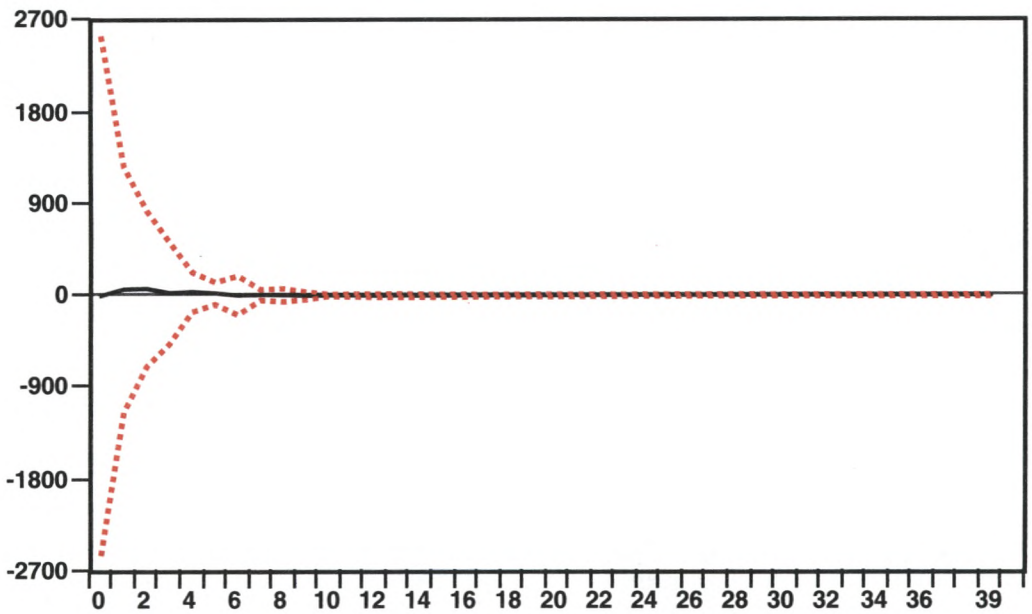
Appendix Figure 8

Inflation irf to Real Output Shock



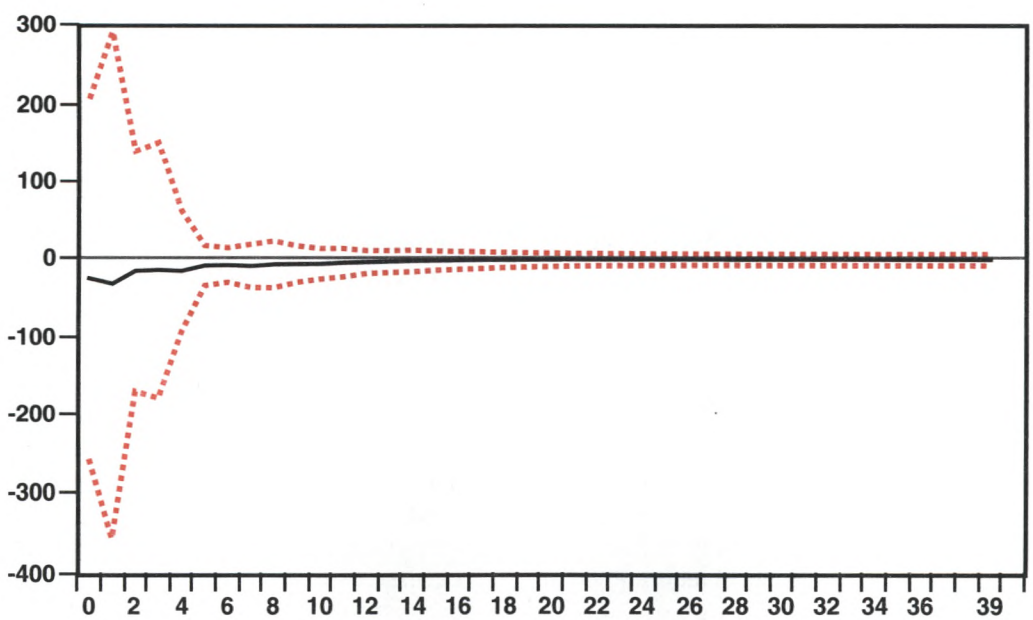
Appendix Figure 9

Money Growth irf to Real Output Shock



Appendix Figure 10

T-Bill Rate irf to Real Output Shock



Julio J. Rotemberg

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Commentary

ROBERT RASCHE'S PAPER is divided into three parts. In the first part Rasche argues that many monetarist propositions have become widely accepted by macroeconomic theorists. With this I agree completely. The work of the so-called New Keynesians should really be called New Monetarist. Many of the papers in this tradition focus exclusively on the effect of monetary shocks. Moreover, the key building block for these papers is a quantity equation where velocity is treated as constant.

This is consistent with a basic monetarist tenet that money demand is pretty stable and changes in money represent mainly autonomous changes in money supply. The second part of Rasche's paper is concerned with bolstering the view that money demand, at least in some sense, has been stable over time. This is the part of the paper with which I disagree most.

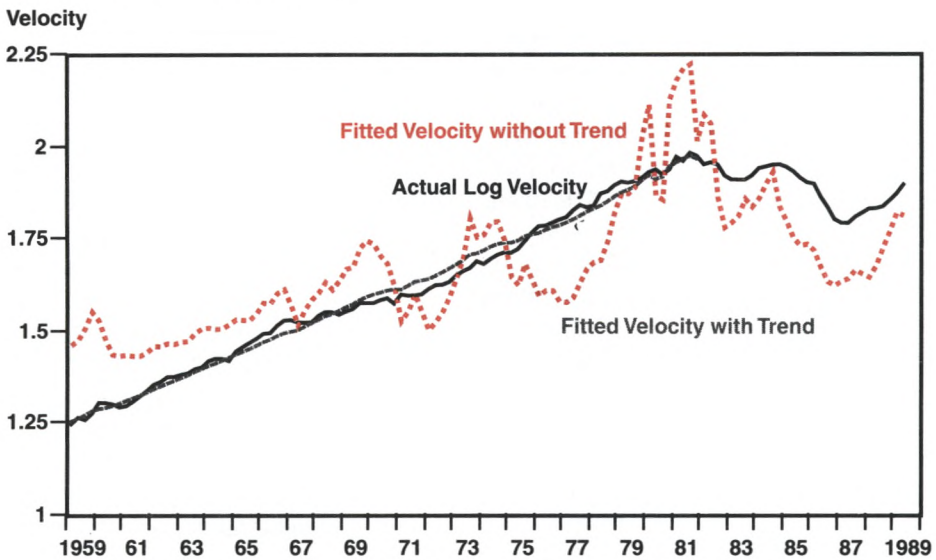
The third part of the paper concerns vector autoregressions and the ambiguous role they give to monetary disturbances. Rasche argues that it is hard to identify the economic meaning of the residuals in these vector autoregressions. It is particularly hard to determine which residual or which combination of these residuals represents shocks to the money supply. Therefore it is difficult to use these particular statistical techniques for evaluating monetary policy. I am in basic agreement with this part of the paper, so when I discuss it, I will mainly elaborate on themes that Rasche develops.

Let me start with the issues raised by the second part of the paper. The Andersen-Jordan equation relates the change in nominal GNP to the change in money (measured by the monetary base) and changes in several measures of fiscal stance. Regressions of output on money are a basic staple of empirical macroeconomics today, and Andersen and Jordan deserve credit for pioneering regressions of this form. As Rasche emphasizes, such regressions really make sense only if you can think of monetary growth as representing an exogenous impulse. This requires among other things that money demand be stable. The instability of money demand has been researched at length. One apparent instability on which Rasche focuses is the change in velocity's trend around the early 1980s. He attributes this change to a change in interest rate trends. The implicit suggestion is that money demand is stable after all.

Rasche's view, which echoes Lucas (1988), is that there exists a stable, long-run money demand equation that can be estimated using levels of velocity and interest rates.[1] This cointegrating regression explains the trend in velocity with the trend of interest rates in the pre-1980

[1]Lucas' data actually run from 1900, so he displays even more stability than reported here.

Figure 1
Actual and Fitted Velocity



period. I have rerun a similar equation using CITIBASE data from 1959:1 to 1989:2, and the results are as follows:

$$(1) \qquad \log\left(\frac{GNP_t}{M1_t}\right) = \underset{(0.029)}{1.286} + \underset{(0.004)}{0.062 i_t}$$

$$R^2 = 0.649 \qquad D.W. = 0.18,$$

where i is the interest rate on Treasury bills. My estimated interest elasticity is a bit smaller than Rasche's, but the cointegration result is the same. To understand what is going on, it is helpful to look at figure 1, which shows the fitted values of this regression together with the actual values for the logarithm of velocity. We see that the fitted values match the trend in velocity rather well through the early 1980s. The ability of interest rates to account for the long-run movements in velocity is remarkable. I have to confess that the ability to track the trends in velocity is so amazing that I wanted to check whether the interest rate's regression coefficient was unchanged from the pre-1979 to the post-1979 period. I thus ran the following regression:

$$(2) \log\left(\frac{GNP_t}{M1_t}\right) = \underset{(0.038)}{1.346} + \underset{(0.008)}{0.047 i_t} + \underset{(0.005)}{0.011 d_t * i_t}$$

$$R^2 = 0.665 \qquad D.W. = 0.16,$$

where d_t represents a dummy variable that equals zero before 1979:1 and one afterwards. The coefficient on the interest rate is thus significantly higher in the second part of the sample. Therefore all stability problems should not be considered solved. Nonetheless, it is impressive that a single interest rate coefficient can track the trends in velocity.

The question at this point is whether Rasche has found *the* money demand equation. An alternative view is that the ability of interest rates to explain long-run trends in money is a coincidence and that economists should really search for a money demand equation that explains deviations around trends. In this view trends in money are caused by secular changes in regulation and technology, which have nothing to do with interest rates. Thus trends in interest rates and velocity are unreliable sources of information about the semi-elasticity of money demand with respect to interest rates.

On *a priori* grounds, one should prefer Rasche's interpretation because it doesn't rely on anything outside the model, such as regulatory changes and technical progress. And yet I must admit that I resist Rasche's view that he has found the true underlying money-demand function. I resist because I am bothered by the huge residuals in

this equation. The fitted values are often 20 percent or more away from the actual level of velocity. Moreover, these huge residuals aren't just random; they are strongly correlated with high-frequency movements in interest rates. Thus around 1975 when interest rates were relatively low, velocity was also predicted to be low. There must have been a huge reduction in money demand around this period to explain the actual behavior of velocity. Put differently, the medium- and high-frequency movements in interest rates, including the spike of 1979, must be attributed to large temporary changes in money demand. I find this hard to believe, however, and tend to trust the conventional wisdom that attributes many of these changes in interest rates to monetary policy. But to believe this conventional wisdom, you have to believe that the short-run interest elasticity of money demand is different from that estimated by the cointegrating regression. In other words, you have to believe that the cointegrating regression does not deliver *the* stable money demand curve that can be used for short-run policy analysis.

To see what difference this makes, I have rerun the regression explaining the log of velocity with interest rates through the end of 1981 but adding a trend. In other words, in this regression the trend is due to technical progress in credit cards and other advances that allow individuals to conserve on money balances. In figure 1 the fitted value with the trend is closer to the actual value than is the fitted value without the trend. Figure 1 supports Rasche's view that money demand is stable after all because it explains long-run swings but this stability is purchased at a heavy price. It must be that money demand is incredibly unstable at short and medium frequencies.

This comment just puts the shoe on the other foot because it raises the question of why money demand rose greatly (and velocity declined) in the early 1980s. Although I am far from having a complete explanation of this phenomenon, I want to return briefly to the theme I presented when I was here three years ago. I said then that simply adding monetary assets that pay different interest rates makes no sense and that procedures such as the Divisia method advocated by Barnett (1980) should be used instead. This seems particularly germane to the question of why velocity fell in the 1980s. The reason is that currency and non-interest-paying demand deposits did not rise unprecedentedly in this period. Figure 2 shows the velocity of the aggregate that includes only these non-interest-bearing assets and it continues to trend upward. What did increase dramatically in this period is the holding of other checkable deposits that pay interest. But adding these to the rest is simply misleading. Other checkable deposits are much more attractive as savings instruments than the other components of M1, so they should be regarded as less monetary.

Exactly how much less monetary than other checkable deposits is perhaps a matter of debate. For current purposes, let me propose that they are about one-third as monetary as the other ingredients in M1, so a proper aggregate can be constructed by adding one-third of other checkable deposits to the other two monetary components.[2] The logic behind this is as follows. In the late 1980s the Treasury bill rate used by Rasche averaged about 7 percent, and interest rates on other checkable deposits were between 4 percent and 5 percent. Thus the gap between these two interest rates is about one-third of the gap between the interest rate on currency and that on Treasury bills. Figure 2 displays the result of treating other checkable deposits as being one-third as monetary as the other assets. Little change in trend can now be detected.

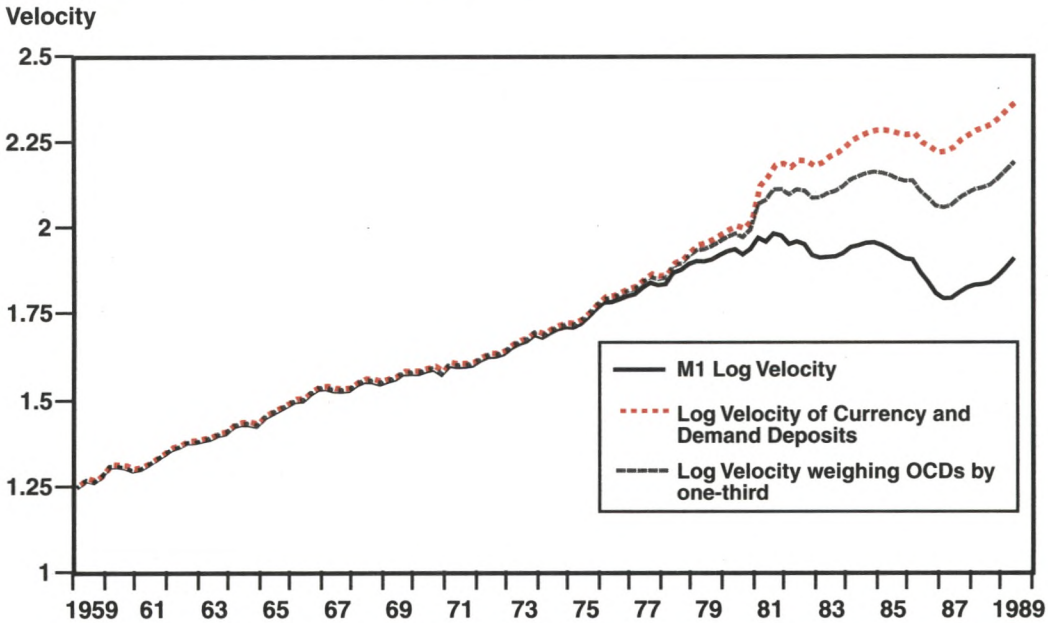
The exercise I just finished is hopelessly crude, and I am not really trying to push the idea that the velocity trend is a natural constant. Rather I am trying to say that the remarkable fit of the Rasche regression explaining velocity should be taken with a grain of salt. There are other plausible reasons for velocity to have fallen in the 1980s.

In conclusion, even if Rasche's regression gives us the true money-demand relation, the volatility of velocity is substantial. This makes a rule where money grows at a constant rate unattractive relative to a rule where monetary changes track changes in velocity. This raises the following two related questions. How do you ascertain that the Fed has done a good job of accommodating changes in velocity, and how do you measure actual monetary impulses over and above those needed to satisfy changes in money demand?

I agree with Rasche's basic thrust that a purely statistical approach cannot disentangle the endogenous and exogenous parts of changes in

[2]See Rotemberg, Driscoll and Poterba (1992) for a more detailed analysis.

Figure 2
The Velocity of Different Aggregates



money. It is hard to know exactly what is being captured by innovations in money within typical VARs. I am particularly bothered by the inconsistency between regressions of various variables on money and money innovations on the one hand and regressions that use variables that reflect changes in Federal Reserve intentions on the other.

In the case of regressions that use money and money innovations on the right-hand side, one generally finds that money raises output and interest rates. Over the years, several authors have constructed variables on the basis of the FOMC minutes that are supposed to reflect Federal Reserve intentions. Boschen and Mills (1992) show that all these proxies have similar correlations with subsequent levels of GNP and interest rates. In particular, after the proxies indicate that the Fed wishes to tighten, output falls while interest rates rise. These opposite reactions of output and interest rates are quite consistent with textbook models and it is inconsistent with the simultaneous increase in output and interest rates that tends to follow increases in money. So how can one reconcile regressions on proxies of Fed intentions on the one hand and regressions on money growth on the other?

One feature of the U.S. postwar period is that many of the well-known episodes of changes in Fed policy involve deliberate tightening to slow inflation.[3] Thus the historical evidence appears consistent with the asymmetries found by Cover (1992) and De Long and Summers (1988). These authors find that output is much more strongly correlated with negative monetary innovations than with positive ones. The latter have a small positive effect on output that is statistically insignificant.

De Long and Summers construct their innovations by running the following regression:

$$(3)\quad \Delta\log M1_t = \underset{(0.002)}{0.005} + \underset{(0.091)}{0.457}\ \Delta\log M1_{t-1} - \underset{(0.083)}{0.145}\ \Delta\log GNP_{t-1} + \underset{(2e\text{-}5)}{6e\text{-}5}\,Trend$$

$$R^2 = 0.321 \qquad D.W. = 1.93$$

The residuals of this regression can be used to construct DM^n, which equals the smaller of the residual and zero. Thus this variable captures

[3]See Romer and Romer (1989).

negative innovations. As a further analysis of asymmetries, I considered a regression of the Treasury bill rate on changes in money and on DM^n over the period 1960:3-1989:2. The results are as follows:

$$(4)\ i_t = \underset{(0.198)}{-0.04} + \underset{(0.088)}{1.31}i_{t-1} - \underset{(0.151)}{0.68}i_{t-2} + \underset{(0.166)}{0.72}i_{t-3} - \underset{(0.106)}{0.5}i_{t-4}$$

$$+ \underset{(15)}{51.4}DM_{t-1} + \underset{(18)}{4.9}DM_{t-2} + \underset{(17)}{3.4}DM_{t-3} - \underset{(14)}{12.8}DM_{t-4}$$

$$- \underset{(25)}{75.0}DM^n_{t-1} - \underset{(27)}{10.0}DM^n_{t-2} + \underset{(25)}{8.2}DM^n_{t-3} - \underset{(22)}{52.7}DM^n_{t-4}$$

$$R^2 = 0.944 \quad D.W. = 2.13,$$

where DM represents the change in the logarithm of M1.

We see here that, as in typical VARs, lagged changes in money tend to increase interest rates. But on the other hand, the effect of lagged negative monetary innovations is negative. This means that negative monetary innovations actually raise interest rates; they have the same correlation with interest rates as the proxies that indicate that the Fed wishes to tighten.[4] Combining these results with those of Cover and those of De Long and Summers, I conclude that negative monetary innovations affect the economy as money supply shocks should, whereas positive shocks do not.

How should you interpret the positive innovations in money in light of their correlation with GNP and the Treasury bill rate? One cannot say that they represent simply monetary accommodation to increases in the stochastic component of the demand for money. Given that these innovations lead to rises in interest rates, the accommodation can be only partial. But partial accommodation of money-demand disturbances should lead to declines rather than small increases in output. I am thus inclined to believe that these positive innovations in money represent in part accommodation by the Fed of other shocks whose effect is to increase future output. Thus the Fed is accommodating increases in money demand that are due to increases in output rather than mere money-demand disturbances. If this is true, it suggests that the Fed is sometimes farsighted.

These results can be used to discuss an alternative explanation of the asymmetries found by Cover and by De Long and Summers. This alternative view holds that all monetary innovations represent exogenous increases in the money supply but that the effects of changes in the money supply are intrinsically asymmetric. The traditional analogy is that monetary policy operates like a string and that strings are useful only for pulling the economy down, not for pushing it up. Several theories of such asymmetries have been proposed. One cause of this structural asymmetry could be that reductions in reserves force banks to cut loans, whereas banks can react to an increase in reserves by raising their holding of securities. If investment depends on the supply of loans and not on the interest rate and the supply of loans is affected only by monetary contractions, then only money-supply reductions have a powerful effect on the economy. Another possible cause of this asymmetry is discussed by Caballero and Engel (1992) who show that asymmetry is a natural consequence of the steady-state distribution of prices in an economy with costs of price adjustment.

Both of the preceding hypotheses may well be able to explain the asymmetry found by Cover and by De Long and Summers. They cannot, however, explain the asymmetric effect on interest rates as easily. The asymmetric response of interest rates casts doubt on the hypotheses by showing that the effects are asymmetric even in securities markets, not just in markets subject to frictions (for example, the market in which banks intermediate loans to businesses and the markets in which firms set prices that are relatively rigid).

As Robert Rasche emphasizes, there is still much to be done to understand the precise role of monetary innovations in statistical models. To understand this role, we have to connect money innovations with other historical and institutional data. Only then can we ascertain the empirical importance of money-supply disturbances in the economy.

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[4] I also checked whether I could detect asymmetric effects on inflation (to see whether the asymmetric effects on interest rates represent asymmetric effects on real returns as well). Inflation does not appear to respond asymmetrically.

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W. Lee Hoskins

W. Lee Hoskins is president and chief executive officer of The Huntington National Bank in Columbus, Ohio. *This paper is given in honor of Ted Balbach and his service to the Federal Reserve Bank of St. Louis. His resolute pursuit of sound economics as the bedrock of monetary policymaking and his indomitable spirit, even when the policy process ran amok, has served us all well. I thank John Davis, Sandra Pianalto and members of the Research Department of the Federal Reserve Bank of Cleveland for helping to shape and advance my views on monetary policy during my four years with them.*

Views on Monetary Policy

THE IDEAL MONETARY POLICY requires a credible and predictable commitment to maintain the long-term purchasing power of a currency. The performance of central banks, which have traditionally been entrusted with monetary policymaking, is far from this ideal simply because a clear mandate for price-level stability—zero inflation—is absent. In practice, central banks serve as instruments that governments use to pursue multiple objectives that they believe serve their interests. Therefore central banks pursue monetary policies that at best have only a fragile commitment to price stability. Governments are currently pursuing policy coordination or monetary union strategies that are little more than attempts to implement a regime of monetary protectionism in the global economy. The future of monetary policy rests on the continuing struggle between politicians seeking policies that serve their short-term agendas and global financial markets that limit the actions of an individual central bank.

In my remarks I discuss why central banks have been established, their bias toward inflation and the importance of independence and accountability to their effectiveness. I also argue that zero inflation should be the dominant objective of a central bank and that current efforts to coordinate monetary policies are likely to conflict with that objective.

WHY CENTRAL BANKS?

What is the justification for a central bank? Can some configuration of private institutions in a so-called free-banking environment perform the functions of a government-sponsored monetary authority? Are central banks necessary?

In his 1959 Millar Lectures at Fordham University, Milton Friedman provided a classic statement of the economic rationale for central banks.[1] Friedman's argument appealed fundamentally to the costs inherent in a pure commodity-standard system, for example, a gold-standard system. These costs arise both from pure resource costs and perhaps more significantly from substantial short-run price variability resulting from inertia in the adjustment of commodity-money supply to changes in demand. The inefficiencies these costs represent are a significant disadvantage of commodity-money exchange systems.

As a consequence there is a natural tendency, borne out by history, for pure commodity standards to be superseded by fiat money. But particular aspects of fiat money systems—such as fraudulent

[1]These lectures were subsequently published as *A Program for Monetary Stability*.

banking practices, natural monopoly characteristics and tendencies for localized banking failures to spread to the financial system as a whole—resulted in the active participation of government. We have come to know this active participation as central banking.

Rationales for establishing central banks have not gone unchallenged, not even by Friedman.[2] Disruptions in payments can be costly, but so are the instabilities and inefficiencies caused by the lack of an effective anchor for the price level in fiat money systems. Moreover, theoretical discoveries in finance and monetary economics, closer attention to the lessons of historical banking arrangements and advances in information and financial technologies have contributed to a healthy skepticism about the superiority of central banks and government regulation to alternative market arrangements. For example, some of the financial-backstop functions performed by central banks and banking regulators may have weakened private market incentives to control and protect against risk.[3]

Still, those who argue for alternative monetary structures must at least recognize that their case rests on untested propositions. Yes, it would be wrong to accept unthinkingly our current central banking system as the best alternative for performing the monetary functions of advanced economies, but it would also be wrong to claim that the current central banking system does not reflect society's choice of an institutional arrangement to perform those functions.

It is not sufficient to argue that market-oriented alternatives to our current central banking systems functioned better in other times and places, for example, in 18th-century Scotland.[4] This begs the question of why such a system did not prove to be sustainable. Nor is it sufficient to argue that this system would have prevailed if not for government intervention and interference. This line of debate fails to consider whether a political equilibrium that would support a market-oriented system in an advanced economy exists anywhere.

It is premature to claim that some hypothetical monetary system can or should dominate institutional arrangements that have already evolved from extended political and economic experience. I believe that the prudent first course is to consider the advantages of improving the performance of central banks. The benefits of a properly managed fiat currency are considerable, and the issue is or should be how to provide the central bank with a proper charter to ensure policy action that generates price-level stability in the long term. If such efforts fail, market alternatives should be sought.

Because I am most familiar with the Federal Reserve, let me use it as an example. Before the creation of the Federal Reserve in 1913, the country prospered without a central bank. Broadly speaking, the impetus for creating the Federal Reserve was a series of banking panics that led to contractions in money and credit that in turn caused serious disruptions in economic activity. The nation sought to improve its banking system by establishing a means for providing an elastic money in the context of a monetary standard based on full convertibility to gold. The gold link was severely weakened by the Gold Reserve Act of 1934.

The Federal Reserve was the result of a compromise between those who would have kept the banking system entirely private and those who wanted government to assume a prominent role in a rapidly growing economy. Other nations have grappled with the same problems and created similar institutions. Today many republics of the former Soviet Union and several eastern European nations are facing these same issues. We now have a world monetary system in which governments, through central banks, monopolize the supply and management of inconvertible fiat monies.

The displacement of the commodity standard that prevailed at the time the Federal Reserve was founded has exposed problems not otherwise envisioned in 1913. For example, the price level has no anchor except for that provided by the resolve of Federal Reserve policymakers. The quadrupling of prices since 1950 dramatically demonstrates the failure of Federal Reserve policymakers to provide such an anchor for the monetary exchange system. Fed policymakers' commitment to price stability is neither as explicit nor as strong as necessary for the successful management of a fiat currency. The gradual demise of our convertible monetary standard has brought us to a point that requires a basic

[2]See Friedman and Schwartz (1986)

[3]See Goodhart (1988).

[4]For a discussion of the free banking era in Great Britain, see White (1984).

change to the framework within which the Federal Reserve functions if the benefits of a fiat currency are to be achieved without large offsetting costs.

The evolution of the global monetary system reflects a common, though unstated, acknowledgment that the benefits of a fiat monetary standard are substantial. Wise administration of that standard requires a central bank in some capacity. In this context, the essential issue is this: How can nations achieve the benefits of a fiat money standard and simultaneously constrain the exercise of that power to the service of the public good? Put another way: How can a nation prevent its central bank from debasing the monetary standard it is charged to protect?

INFLATIONARY BIAS OF CENTRAL BANKS

The answer to these questions seems to elude us. Witness the universal debasement of currencies by central banks since the loss of a commodity standard as a price-level anchor. To find the answer, we must review central bank charters and the incentives provided to those who control monetary printing presses. Public-choice economists have focused on this issue and developed a rich literature; however, I feel they fail to provide a satisfactory explanation of the secular bias toward inflation among central banks (with different charters and varying degrees of independence from political influence). Moreover, this approach fails to explain why in earlier periods governments did not consistently exploit the opportunities to inflate by realigning their currencies against gold or dropping their convertibility.

Another explanation for persistent inflation that has some appeal is policy mistakes, or inappropriate targets or operating procedures of central banks. This explanation also leaves some unanswered questions. Why aren't policy mistakes symmetrical? That is, why don't they cause deflations as well as inflations, leaving the average price level unchanged over time? Perhaps policy mistakes are biased toward inflation because of the operating procedures employed, such as interest rate targeting. Yet the Bundesbank, which uses monetary aggregate targets, produces a rising price level. The Bank of Japan uses interest rate targets and has generated a similar increase in its price level during the past two decades. If a central bank is dedicated to price-level stability over time, the choice of targets or operating procedures probably only influences the variability of inflation rates around a zero mean. In short, a central bank that truly wants to achieve price-level stability can do it with any number of operating techniques, as long as they control money growth over time.

Perhaps a simple, and less elegant, explanation for persistent inflation is that central bankers are suffering from a Keynesian hangover. Central bankers, politicians and the public are merely reflecting the prevailing economic dogma that government has the responsibility and ability to manage aggregate output and employment, as well as inflation. I have argued and continue to believe that a major source of price-level instability comes from multiple objectives assigned to central banks—economic growth, employment, price stability and exchange rates. It is true that politicians pressure central banks to achieve different objectives at different times. Such political pressure can produce inappropriate policy actions; however, the responsibility for assigning multiple objectives to central banks rests as much with the economics professions as it does with politicians. For the last 50 years, many economists have supported various theories of business-cycle management, which required that central banks shift from one objective to another. Today businessmen, politicians and most economists continue to believe that if the economy is weak, the central bank should respond regardless of the cause of the weakness. And so it does.

Some of the current discussions about monetary policy and the Federal Reserve suggest that the lessons of the 1970s may be fading from our memories. Calls for lower interest rates or more rapid money growth are not at all unusual. More often than not, those suggestions seem impelled by desires for growth or desires to offset the problems of particular sectors of the economy. They seem based on the notion that there is a trade-off between inflation and output or between inflation and employment that can be exploited by the central bank. Some of us learned from the experience of the 1970s that such a trade-off does not occur over time. Instead, higher inflation only added to uncertainty, distorted resource allocation and reduced economic performance below the maximum sustainable level with price stability.

Members of a central bank policy committee such as the Federal Open Market Committee (FOMC) reflect what is believed by the mainstream.

In January 1990 the National Association of Business Economists surveyed its members and asked the following question: "Is reducing the inflation rate to zero over the next five years the appropriate objective of monetary policy?"[5] More than 80 percent of the respondents answered no. Their responses indicate that they believe the FOMC should trade off inflation for some other objective, presumably economic growth. At about the same time, the House Subcommittee on Domestic Monetary Policy surveyed 500 members of the American Economics Association who list monetary economics as either their first or second specialty. The unpublished survey shows that only a slight majority of those who responded favored zero inflation over the next five years.

I believe that much of the inflationary bias of central banks over the past 50 years reflects the prevailing view that output and employment fluctuations can be smoothed with monetary policy. Currently, before each FOMC meeting, members of the Committee are presented with the policy views of several prominent economists. Either explicitly or implicitly, these views invariably present the policy choice in terms of a Phillips curve trade-off. Staff projections at the FOMC meeting also imply such a trade-off, as do the statements by some FOMC members. Moreover, policy actions, such as a reduction in the federal funds rate, often follow the release of employment or output statistics, further reinforcing the notion that the Federal Reserve can manage real variables. To the extent that this explanation of central bank behavior is valid, inflationary bias will not be eliminated until there is agreement within the profession on price-level stability as the dominant objective for central banks.

INDEPENDENCE AND ACCOUNTABILITY

The problems that emanate from multiple, and often incompatible, objectives are well known. To contribute to maximum economic growth over time, central banks must achieve price-level stability. Achieving this goal requires that central banks be free from political expediencies—that is, that they have independence within government. Substantial evidence indicates a link between central-bank independence and the ability to achieve price stability. Recent studies show that countries that grant their central banks the greatest degree of independence have had the lowest rates of inflation.[6] Even taking into account other sociopolitical factors that might cause inflationary pressures, the degree of central-bank independence appears to have an important effect on a country's inflation rate.

However, with independence must come accountability. Even the clearest objectives will prove elusive without accountability; independence without direct accountability is a dangerous brew for those who drink it. Great harm has come from well-intentioned, independent central bankers with little or no accountability—witness the United States in the 1930s. Many mechanisms exist today to bring accountability to central banking; for example, the employment contract of the governor of the central bank of New Zealand contains a price-stability requirement.

The objectives, degree of independence, and accountability of the central bank are substantially determined by its legal structure. For example, a clear legislative directive to achieve price-stability goals above all others and the freedom to pursue price-stability initiatives would all but eliminate potential conflict with other objectives. The vexing question of what extent, if any, a central bank should compromise the price-stability objective to pursue auxiliary goals, such as smoothing real output fluctuations or stabilizing exchange rates, should be resolved and dictated in the legislative charter. True independence and strict accountability can be attained only legislatively.

Compared with the central banks of other countries, the Federal Reserve System has a better structure to execute monetary policy effectively; however, the Fed is not as well positioned as other central banks. The Federal Reserve is charged with multiple objectives that are often incompatible but that at least include price stability. It is functionally independent within government, but it faces intermittent challenges to its autonomy. Its independence comes from both its charter and its practice. Independence is essentially a delineation between the responsibilities of Congress and the executive branch on one side and the monetary

[5]See NABE Policy Survey (1990).

[6]See Alessina (1988) and Banaian (1983).

authority on the other to limit the motive and means to debase the value of the nation's money.

The source of tension between monetary and fiscal authorities is the central bank's ability to create money. Because the creation of fiat money imposes an implicit tax on money balances, the monetary authority is one source of government revenues. For the most part, the long-run viability of the government's fiscal operations requires that its real current debt burden plus the present value of its expenditures equal the present value of revenues. Thus if the path of debt plus expenditures diverges from the path of explicit tax revenues, fiscal viability requires that the discrepancy be satisfied by seigniorage from monetary growth. This scenario is typically referred to as fiscal dominance over the monetary authority.

The original Federal Reserve charter left many doors open for the executive branch to influence monetary policy. These were partially closed when the Banking Act of 1935 removed the Secretary of the Treasury and the Comptroller of Currency from the Board of Governors of the Federal Reserve System. In addition, the law established the FOMC, with the seven governors and five of the Federal Reserve Bank presidents as voting members, ensuring that power within the Federal Reserve would be shared between political appointees and regional bank presidents. Thus the fire wall that made the Federal Reserve, and not the executive branch, responsible for monetary policy objectives was reinforced. It was strengthened further by the Treasury-Federal Reserve Accord of 1951, which served as a clear statement that the Fed would not be coerced into solving the federal government's debt-management problems. The institutional structure was designed to ensure enough Federal Reserve independence within the government to carry out this mandate without interference.

This independence in principle has held up in practice. The dramatic increases in federal deficits in the early- and mid-1980s prompted fiscal dominance believers to predict that it would be impossible to achieve and maintain inflation rates below the disastrous levels of the decade's start. So far, this prediction has not come to pass. In 1983 the federal budget deficit was 3.8 percent of GNP, a level far above the post-World War II average and nearly equal to the postwar peak realized in 1975. In the same year, inflation measured by the consumer price index fell to 3.2 percent—a 16-year low. As the decade proceeded, the deficit relative to GNP rose, fell, and rose again to its present level above 5 percent. The inflation rate was impervious to these patterns.

Astute observers might question the relevance of the early- and mid-1980s to the fiscal dominance proposition, because deficits as they are conventionally measured do not necessarily reflect the government's long-run fiscal operations. To name just a few of the problems, the value of long-run government net liabilities is inherently ambiguous, the path of future revenues is uncertain and the appropriate method of discounting future tax and expenditure flows is problematic. Although sympathetic to this view, I am still left with the strong suspicion that if any period in recent history was ripe for the emergence of fiscal dominance, it was the last 10 years.

Indeed, as the decade progressed and the predictions of the fiscal-dominance theory failed to materialize, more sophisticated variants of the relationship between fiscal and monetary policy began to find their way into economic research. The fiscal authority's reign over the subservient monetary authority was replaced by a more subtle and complicated institutional structure, a world in which fiscal and monetary authorities played a game of chicken, the outcome of which left both parties less than fully satisfied.[7] Although deficits may be detrimental to economic performance, the ability of the Federal Reserve to resist monetizing debt has protected the economy from even worse consequences. The Federal Reserve's decision to resist monetizing the federal debt resulted in lower inflation and contributed to fiscal reforms that started with the Gramm-Rudman-Hollings legislation.

In my view the Federal Reserve has sufficient independence to achieve price stability. The core-problem, however, is that the Federal Reserve is not accountable for that objective. Without accountability, the policy process will be neither credible nor predictable. The more credible the commitment to the policy goal, the fewer wrong decisions will be made by the markets. The more predictable the policy reaction to unforeseen economic events, the more limited will be the market reaction to those events. Credibility and predictability can substantially lower the costs

[7]See Sargent (1985).

of achieving and maintaining a stable price level. Yet with the disintegration of the monetary aggregates as intermediate policy guides, discretionary monetary policy actions may seem especially hard to predict because policy objectives and accountability for them are unclear. The existing policy process, with its focus on short-term economic or financial developments does not provide credibility.

How can we change the process to reinforce the credibility of a consistent goal? I think the most secure way would be to give the FOMC a legislative mandate to meet a consistent, attainable and unchanging economic goal. Passage of House Joint Resolution 409, introduced by Representative Stephen Neal, would provide that crucial reinforcement. The Neal resolution simply directs the Federal Reserve to make price stability the primary goal of monetary policy and to achieve that goal within five years. History gives us little basis for expecting price stability or even a stable rate of inflation because the FOMC has had no mandate to produce that result. Giving the FOMC that mandate and knowing that the FOMC intended to stabilize the inflation rate at zero, would provide one gigantic piece of policy information to any rational decision-maker in any dollar-denominated market. The Federal Reserve would remain independent, and it would retain complete discretion about how to carry out policy. The only change would be that Congress would provide more direction about the basic policy objective, and the Federal Reserve would be accountable for achieving it. True accountability would also require an incentive or enforcement mechanism for achieving the objective.

The FOMC can deliver lower inflation without a legislative mandate. Of that you should have no doubt! Inflation is a monetary phenomenon, and the FOMC is the sole custodian of the quantity of money in the United States. If a zero-inflation mandate were in effect, short-term deviations from zero inflation might occur, but one way or another the FOMC could provide a stable price environment. As many scholars have urged, the FOMC might impose accountability on itself by tying policy actions to some intermediate target variable by an agreed-on formula that would ensure price stability. These days, the most popular candidates for an intermediate policy target seem to be nominal GDP and M2, either of which is thought capable of producing reasonable price stability. Another approach would be for the Committee to specify achieving the ultimate policy goal as the rule, while using discretion in choosing actions to achieve the goal.

Of course having today's FOMC impose accountability on itself (by adopting an explicit rule tying an instrument to a goal) is not a foolproof way to achieve an official policy goal. Credibility would have to be earned through predictable actions consistent with the goal. To adopt an explicit rule, at least a majority of today's FOMC members must not only agree on an overriding macroeconomic goal, but also renounce some discretion to pursue other goals. Moreover, tomorrow's FOMC could decide to change the goal and hence the rule. In the current policy regime, today's policy choice can in no way bind tomorrow's. Unless directed by society through specific mandate, tomorrow's FOMC always has the discretion to change the goal. And with shifting goals there is no accountability. I believe that the lack of accountability for a dominant policy goal of price stability is the major cause of the inflationary bias in the U.S. economy since World War II.

Although the specifics of the Federal Reserve charter differ from those of other central banks, the problems of conflicting objectives and the lack of secure independence and explicit accountability are common to all central banks in varying degrees. Experience around the world and through time repeatedly demonstrates that central banks require independence from day-to-day political life to perform their price-stability role. If we could create legal and cultural conditions that truly fix a central bank with accountability for anchoring the price level, the structure of the central bank itself would become less important. Those circumstances would be a joy to behold, but I am afraid they will be some time in coming.

WHY A ZERO-INFLATION OBJECTIVE?

I strongly believe for three reasons that the dominant objective of monetary policymakers should be price stability. First, in the long run, a central bank can control the price level of goods and services denominated in its own currency, but it cannot control the growth of output (potential or actual). Second, a credible commitment to a price-stability objective enables a central bank to promote economic efficiency

and growth (potential and actual). Third, price-level stability, popularly called zero inflation, is superior to inflation-rate stability.

Among economists, support for the first reason is nearly universal. There is also widespread agreement on the second point. A central bank that pursues price stability promotes economic efficiency and growth. I would venture further to say that experience shows that central banks that have sought to enhance economic growth directly have failed miserably at providing stable price levels and ironically have undercut economic growth in the process. The last reason—that no inflation is preferable to stable, non-zero inflation—is most contentious, particularly when people attempt to compare the transitional costs of achieving price stability with the costs of stabilizing the inflation rate at the status quo.

The argument that the cost of pursuing a zero-inflation target would outweigh the benefit of reaching that target has two dimensions. The first is that the benefit of achieving zero inflation would be small. The second deals with the costs of moving from a 4 percent trend rate of inflation to zero inflation. This is the transition-cost argument, which essentially says that even if zero is the place to be, getting there is not worth the ride. I believe that the benefits of zero inflation are great and that the transition costs can be reduced if the Federal Reserve commits to an explicit plan for achieving zero inflation.

The interaction between inflation and our current tax system, especially as it applies to income generated by capital, represents one of the more significant channels through which non-zero inflation can exact economic costs.[8] This channel of distortion is often not taken seriously because people think that its effects are minimal or that it would be easy to index the tax system. Correcting the tax code is a good idea of course, but until that happens, what possible excuse is there for not letting the monetary authorities do what is necessary to improve social welfare?

It is clear that the horrendous U.S. inflationary experiences of the 1970s and early 1980s created the impetus for the limited inflation indexation of the current tax system; however, the job is far from complete. Capital gains, corporate depreciation and interest expenses, and personal interest income remain untouched by efforts to index the tax system for inflation. Even the bracket indexation implemented by recent tax reform does not fully protect taxpayers from bracket creep, or nonlegislated increases in marginal tax rates created by inflation. Complete indexation of the tax code, however desirable it may be, will be extremely difficult to achieve. Will another inflationary experience like that of the 1970s be required to induce further progress on tax indexation? I fail to understand why some feel that these inflation-tax interactions are a significant drag on the economy, yet argue that only Congress should be concerned with the problem. The problem exists because of the interactions between inflation and a tax system based in current dollars. Therefore it seems that the responsibility for minimizing these costs lies as much with the monetary authorities as with Congress. Doesn't it make more sense for monetary authorities to try to correct the inflation part of the problem rather than simply hoping that Congress will implement changes that it may be unable or unwilling to pursue? We speak about the costs of achieving zero inflation, but what about the costs of fully indexing the tax system? Surely they would be significant.

Another area of concern is the role of uncertainty as a source of inflation costs. How important are the distortions that arise from price—level uncertainty? There is a class of models—the market-clearing, imperfect-information paradigm associated with Robert Lucas and others—in which inflation uncertainty harms the economy by distorting the period-to-period relative price signals that facilitate the efficient allocation of scarce resources.[9] Despite the pervasive intellectual influence exerted by the Lucas framework to this day, the empirical evidence accumulated since the development of the paradigm in the early 1970s has not been entirely supportive. This point is not lost on critics, who think that the lack of evidence on short-term distortions should persuade us that inflation uncertainty is simply not that important to social welfare. Surely the relative-price/aggregate-price confusion stressed by the Lucas-type models is a special type of uncertainty. The failure to find significant effects from uncertainty that is resolved within a few quarters tells us next to nothing about the type of long-run uncertainty with which the zero-inflation position has always been fundamentally concerned.

[8]See Altig and Carlstrom (1990).

[9]See Lucas (1972).

Indeed, it seems likely that the uncertainty occurring over *extended* time horizons is precisely what is most affected by the average inflation rate.[10] This is one reason why I favor a price-level target. An inflation-rate target enables the price level to drift without bound, and with no enforcement mechanism to ensure that inflation mistakes will be corrected, the long-run variance of the price level is infinite. When people have reason to believe that this standard will erode over time, they invest numerous resources to protect themselves. Those who have nominal debt outstanding will drag their feet in paying it back, whereas creditors will invest in ways to accelerate the collection of funds. The private gains to self-protection are clear, as are the social costs.

Recent experience is the best testimony to the real resource cost of inflation. During the 1970s, people could see that inflation accelerated each year. They guessed, reasonably at the time, that financial assets had limited value in protecting their wealth from inflation. Consequently, farmland, commercial and residential property, and precious metals became much more expensive as people sought to shelter their wealth. Not only was time spent seeking these investments, which was socially wasteful, but also the resource misallocation itself resulted in a great waste of land, labor and capital that society is still paying for today.

It is difficult to comprehend how efficient planning within the public and private sectors could not be inhibited by this type of long-run uncertainty. Furthermore, the intuition that long-run inflation uncertainty is costly has empirical support. In cross-country comparisons, economic growth is negatively related to the variability of inflation.[11] One finds that the case for reducing price level uncertainty is far more compelling than a cursory analysis might indicate.

In evaluating the costs of attaining zero inflation, economists almost always use models in which markets do not clear or do not clear without cost. Gone is the market-clearing, flexible-price, rational-expectations model. In its place is a model with price contracts that make the transition to zero inflation extremely costly. The source of the friction is usually not entirely explicit, but the implication is that we must assume some frictions. These frictions, coupled with the inability of markets to clear, make ending inflation appear as costly as it does.

Isn't it sensible to assume that the implicit sources of frictions that make lowering the inflation rate costly would also contribute to making inflation costly in and of itself? For instance, a variety of explicit and implicit nominal contracts already exist, and a transition to zero inflation could alter the real values of payments from those that were originally intended. But surely the entire institutional apparatus that generates these contracts must involve resource costs that are positively related to the average rate of inflation.

One should not compare the costs of achieving zero inflation in non-market-clearing models, where such costs are high, to the benefits of being at zero inflation in frictionless, continuously clearing models, where the benefits are low. If we use a model with frictions to measure the cost of getting to zero inflation, then we should also use such a model to examine the benefits of being there. This is one reason I am skeptical of so many cost/benefit estimates of reducing inflation.

I am also skeptical about transition-cost estimates that do not account for the possibility that a price-stability objective will be regarded as credible by the public. Economic theory and reasonable model simulations persuade me to believe that with credible precommitment, a central bank can greatly minimize private-sector planning errors during the transition period. I think that much of the disagreement among economists on the size of transition costs centers on the ability of a central bank to commit itself credibly to achieving its objective. Until I see some hard evidence to dissuade me, I plan to continue my advocacy of price stability as the overriding objective of central banks.

It still puzzles me that volumes of research have been published on central bank operating procedures and management of monetary aggregates, yet relatively little research has been published on the value of a credible precommitment to a price-stability objective. My intuition tells me that the latter is far more important than the former in terms of economic welfare. Of course, credibility depends on policy information avail-

[10]See Ball and Cecchetti (1990).

[11]See Grier and Tullock (1989) and Lebow, Roberts and Stockton (1990).

able to market participants so that they can monitor progress toward the objective.

One major benefit of imposing an explicit intention on monetary policy is that policy actions in the money market would become far less momentous than they are now. Currently, detecting a change in the federal funds rate target from the pattern of open market operations is crucial because it provides markets with one of the few clues as to what monetary policy the Federal Reserve is pursuing. Canvassing the positions of individual FOMC members is a way of predicting future policy. If policy intent were explicit and credible, however, finding the clues in open market operations would have less significance.

I see the greatest payoff in more information about policy intentions. An explicit FOMC commitment to price stability would allow markets to shift resources from watching the Federal Reserve to watching the economy for productive investment opportunities . Focusing on the intent of policy contrasts markedly with conventional concerns for more certainty about the current degree of reserve restraint. There are many ways to reduce uncertainty about the immediate funds-rate implications of policy, just as there are many time schedules by which the FOMC directive might be released. More certainty about the immediate policy implications of the federal funds rate might make Fed-watching a bit easier, but it would not do much to help identify policy intentions beyond short horizons. Releasing Fed directives early might provide a slightly brighter glimmer of policy intentions, but only for a slightly longer policy horizon. We do not need better information about the latest directive; we need better information about the process through which all future directives will be crafted—that is, policy intentions. Nothing would provide more insight than a clearly stated goal

MONETARY POLICY AND MONETARY PROTECTIONISM

Let me turn now to the effects of international policy coordination on the pursuit of zero inflation.[12] Exchange-rate regimes and attempts at monetary union are currently undermining the price-stability objective. Many actions taken by central banks are not aimed at price stability, but rather are attempts to establish monetary protectionism. By monetary protectionism, I refer to attempts to alter *real* exchange rates through manipulation of monetary policies and with the hope of ultimately promoting a balance-of-payments objective. In the case of a deficit country, monetary protectionists call for an expansion of money growth (or lower nominal interest rates). A monetary expansion, other things being equal, will produce a *nominal* depreciation. If individuals are unable to adjust prices immediately, or if they are slow in perceiving the inflationary aspects of this policy, a real depreciation will accompany the nominal depreciation. As most economists realize, however, the inflation rate will eventually respond to the monetary expansion, offsetting the nominal depreciation and returning the real exchange rate to its initial position. Nevertheless, the tenuous, short-lived relationship between money and the real exchange rate is seductive enough to convince politicians and other fine-tuners that monetary policy can serve mercantilist designs.

My focus on this issue stems from a firm belief that central banks can do no better than guarantee long-run price stability and that any efforts to limit this guarantee are not likely to raise world welfare. Central banks can juggle a real exchange rate and inflation target no better than they can slide back and forth along a stable Phillips curve. A central bank that attempts to maintain price stability and a nominal exchange rate target has more policy targets than policy instruments. At times, these two objectives might be compatible. For example, in the late 1970s, limiting rapid dollar depreciation through intervention could have been compatible with a contractionary monetary policy to eliminate inflation. As often as not, however, these two policy objectives will be incompatible, and the central bank must trade one objective for the other.

Under such conditions, markets will view neither price stability nor exchange-rate stability as a credible policy. The knowledge that central banks will deviate from a policy of price stability to pursue an exchange rate objective will raise uncertainty about real returns and will distort the allocation of resources across sectors and through time. The resources devoted to protecting wealth from possible inflation could

[12]This section summarizes ideas presented in Hoskings and Humpage (1990).

be applied to more productive uses under a policy of price stability. Moreover, attempts to maintain nominal exchange rates will not eliminate exchange rate uncertainty because countries will inevitably resort to periodic exchange-rate realignments. Hedging exchange risk will remain an important aspect of international commerce.

Although monetary protectionism seems most prevalent under the present system of floating exchange rates, it does not follow that floating exchange rates promote its use. Monetary protectionism can result any time a government accepts nonmarket criteria for exchange rates. In principle, a gold standard or a fixed exchange rate regime can limit the scope of monetary protectionism because, if all countries play by the rules of the game, they link money supplies closely to the flow of international reserves. In practice, however, such regimes do not destroy the political motives for monetary protectionism, and examples of monetary protectionism under fixed exchange rates abound. By allowing some discretion in the choice of exchange rate adjustments, fixed exchange rate regimes often produce a mechanism that weakens the allocative efficiency of exchange markets and promotes mercantilist objectives.

In contrast to the interventionist literature, which presupposes an all-wise government acting in the public's best interest, a rich, growing literature on political economy characterizes elected officials as seeking to enhance their own power, prestige and wealth by maximizing their ability to gain votes. Politicians and bureaucrats attempt to extend the scope of their influence by responding to the demands of the most politically active constituencies.[13] A political justification for exchange rate manipulation is that it defers criticism and postpones more fundamental actions. For instance, in 1985 dollar exchange rates were at their zenith, the U.S. current account was deteriorating rapidly and evidence suggested that the United States was becoming a debtor country for the first time since World War I. U.S. manufacturers, facing increasingly stiff competition worldwide, besieged Congress for trade legislation. Most important, analysts increasingly linked the deterioration in the external accounts with the fiscal policies of the Reagan Administration and Congress. The opportunity cost of government inaction, measured in terms of votes lost, seemed to rise sharply in the early 1980s.

The U.S. current account deficit reflected imbalances between savings and investment in the United States, West Germany and Japan. Politicians, however, cannot easily redress such structural relationships through fiscal policies because of strong vested interests in maintaining various tax and expenditure patterns. Unable to address these structural problems directly and quickly, policymakers might resort to exchange-market intervention. When coordinated through the Group of Seven, such intervention offers a highly visible signal that governments are responding to the desires of their constituencies.[14]

Exchange rate policies can also offer temporary benefits to specific constituencies. When goods prices are slow to adjust, a nominal currency depreciation is equivalent to a temporary, across-the-board tax on imports and a subsidy to exports. With the terms of trade temporarily altered, certain groups in the traded-goods sectors can realize benefits from monetary protectionism similar to those afforded by more traditional forms of protectionism. Ultimately, any benefits from monetary protectionism dissipate with a high inflation rate and with reduced credibility of monetary policy. The inflation costs of monetary protectionism, however, are dispersed across a wider spectrum of individuals and over a longer time horizon than the benefits. A constituency that receives net benefits from monetary protectionism (export- and import-competing firms) can exist. Such a constituency is likely to be more politically cohesive than any constituency for price stability. Consequently, a policy that seems myopic from an economic perspective can be politically attractive.

Another seemingly attractive aspect of monetary protectionism is that Congress and the administration can justify it in terms of broader macroeconomic considerations, such as exchange rate misalignment or current account imbalance, instead of industry—specific considerations, such as automobile and steel employment. Consequently, the rent-seeking aspects of monetary protectionism are less obvious than those of standard protectionist policies.

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[13]See Quibria (1989).

[14]The Group of Seven countries are Canada, France, Italy, Japan, the United Kingdom, the United States, and West Germany.

Countries interested in establishing exchange rate targets have a strong incentive to collude in their efforts with foreign governments.[15] In the case where countries attempt to alter nominal exchange rates, such collusion provides tacit foreign approval of these policies and limits the chances that a foreign government will take steps to neutralize the exchange policies of another government. Sometimes such collusion involves having cartel members delay policy negotiations, or exchange rate adjustments, when individual cartel members face critical elections. Bretton Woods and the European Monetary System (EMS) are examples of collusion that were fairly successful for a period. The competitive currency devaluations of the 1930s show what can happen when governments attempt to fix a price but their cartel breaks down. Coordinated efforts to fix exchange rates can allow individual countries to influence the policies of others and to defer some of the adjustment burdens of maintaining the peg. Such mechanisms are found in the EMS and figure in some proposals for target zones and for fixed exchange rates. Many support the proposal for a European Central Bank for just this reason. The alternative is to sacrifice monetary sovereignty to maintain a fixed exchange rate and to follow the monetary policy of a major trading partner.

Under floating exchange rates, a rapid depreciation in the nominal exchange rate in response to such inflationary policies signals the market's displeasure and constrains governments. Through collusion to fix the exchange rate, however, governments can temporarily blunt the exchange rate reaction to their policies and reduce the political costs of pursuing inflationary policies. Coordination to limit exchange rate fluctuations is politically attractive because it eliminates an important, immediate barometer of the market's opinion of government policies.

For their part, central banks often are willing participants, viewing exchange rate management as a legitimate aim of monetary policy. Exchange rate movements can impart useful information for policymaking, and as already noted, exchange rate targets can sometimes be consistent with a monetary policy of price stability. As often as not, however, exchange rate policies conflict with price stability. For example, U.S. purchases of foreign currencies in 1990 seemed inconsistent with a goal of price stability. When these objectives conflict, the Federal Reserve System is torn between its independence and its accountability to the broad national policy goals set by Congress and the Administration. The Federal Reserve does not wish to appear to the public as unresponsive to the objectives of Congress and the administration. Participation also enables a central bank to influence policy formulations that it is powerless to prevent. Such reasoning is a certain sign of a central bank unsure of its objective and insecure about its independence.

In countries with independent central banks, intervention policies might enable fiscal agents to extend their influence beyond the foreign exchange market to domestic monetary policy. Elected officials often seek more stimulative monetary policies than do central banks, hoping to lower nominal interest rates and to stimulate real growth and employment. In choosing a nominal exchange-rate target, intervening and encouraging the central bank not to sterilize the intervention, fiscal agents have a mechanism for such influence that would usually not be open. At times, however, such as when the central bank policy committee is not in unanimous agreement, such an influence, marginal though it may be, could prove decisive in charting future monetary policy actions.

INTEGRATED MARKETS AND POLICY CONSTRAINTS

I have attempted to instill a healthy skepticism for exchange market manipulation, arguing that it is a form of monetary protectionism that harms economic welfare. Monetary protectionism stems as a near-term palliative from the political interactions between policymakers and constituencies with vested interests in particular market outcomes. Any international monetary order willing to accept nonmarket criteria for exchange rates and failing to bind governments with a price-stability objective is ripe for monetary protectionism. To counter the political incentives toward monetary protectionism, nations should adopt monetary mandates, such as the Neal Resolution in the United States, that focus monetary policy on achieving and maintaining long-term price stability.[16] This would do more to eliminate exchange market uncertainty and foster the efficient worldwide use of real

[15]See Vaubel (1986).

[16]See Hoskins (1990).

resources than any program to manipulate nominal exchange rates.

My comments are not meant as a blanket condemnation of international policy cooperation. I strongly support cooperation that makes price stability the dominant objective and recognizes market-determined exchange rates. Only cooperation based on these conditions seems both feasible and credible because it recognizes that nations want monetary sovereignty and will pursue different economic policy objectives.

Contrary to what some might infer, this approach does not preclude European monetary unification in the future, but it suggests a different approach than currently seems to be favored. European governments are not likely to relinquish national monetary sovereignty on adoption of a single market. Consequently, greater exchange rate flexibility than the EMS currently provides seems necessary to ensure that exchange rates do not interfere with the efficient flow of goods, labor and capital following the removal of restrictions. The free flow of resources, if it occurs, will foster a convergence of policy preferences within Europe as governments compete for these resources by providing stable economic and political environments. Governments that fail to provide such an environment will lose resources as markets vote on policies. The resulting convergence of monetary and fiscal policies will lead to greater exchange rate stability. If in time, governmental competition for resources attains a convergence of macroeconomic policy, issues of national policy sovereignty will be muted. Only then will monetary union augment the efficiency gains of a single market. As seems obvious from recent developments in Europe, efforts to rush monetary union are efforts that put the cart before the horse and may well interfere with the progress toward a single market.

To fix exchange rates before a convergence of policy preferences within the European Economic Community seems to ensure that interest rates and prices will bear more of the adjustment burden. Moreover, judging from the experience of Bretton Woods, fixed exchange rates would seem to guarantee speculators periodic exchange rate adjustments and to encourage governments to impede the flow of goods and capital through the reintroduction of restraints. The dynamics of achieving monetary union are as important as the goal, and price stability is a more important goal than either.

Scores of new nations are busy constructing central banks to implement monetary policy. Using history as a guide, these new central banks will try to pursue objectives other than price stability, especially since they are being counseled by central bankers with weak records on price stability. Short-term political agendas will likely dominate their policy actions and push them away from the pursuit of price stability. Yet it seems that there are powerful market forces that will crimp the efforts of central banks to mismanage their currencies.

The integration of world markets, particularly financial markets, is limiting the degree to which policymakers are willing to drift away from price stability, at least for the major economies. Twenty years ago the Federal Reserve paid scant attention to the effect of foreign markets on the price of U.S. government securities and interest rates in the United States. Yet when I participated in FOMC deliberations, we almost always discussed the effect of a policy action on long-term Treasury rates, currency values or the shape of the yield curve. The FOMC now looks at how world financial markets assess the credibility of its policy actions with respect to inflation expectations. This process, in effect, limits the degree to which the FOMC is willing to risk inflationary policy actions.

In Europe, smaller countries often peg their currencies to the German mark, allowing the Bundesbank to determine their monetary policies. The German central bank is also limited by world markets in terms of the inflation path it chooses to pursue. I am not so bold as to argue that markets will cause central banks to wither away to agencies that simply pump out monetary growth rates that provide price stability. It does seem to me, however, that market forces are strengthening the hand of central banks in fighting political pressures for short-term "quick fixes" to economic problems. Perhaps even politicians will learn the limits of governments in solving economic problems.

If this view proves incorrect, central banks will face the prospect of market participants developing private money to a much greater degree than exists today. When government management of particular institutions results in failure, private-sector alternatives appear—witness the privatization trend in U.S. schools

and courts. Perhaps those who yearn to revisit the Scottish system of free banking may live to see a version of it replace central banking. If so, we are likely to pay a heavy price along the way.

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Commentary

LEE HOSKINS HAS WRITTEN a fine paper on monetary policy. I share most of his views on the role and duties of central banks. Hoskins discusses why the conduct of monetary policy has been entrusted to central banks. He also examines the conditions that must be satisfied for central banks to play an effective policy role.

Hoskins' principal thesis is that central banks are needed to manage a standard based on fiat money. But a fiat standard imposes few constraints on central banks. If central banks are permitted to issue fiat money, there is always the risk that they will abuse their powers. Consequently, under a fiat standard it is necessary to ensure that central banks act in the public interest.

Why do central banks frequently harm the public interest by debasing the currency? Hoskins discusses several possible reasons. He dismisses the answers offered by public-choice economists and also rejects the notion that unsatisfactory performance of central banks is due to the pursuit of inappropriate targets or operating procedures. Instead, he maintains that "central bankers are suffering from a Keynesian hangover." Frequently they do not direct monetary policy solely at price stability but attempt to pursue multiple objectives that often conflict. Many central bankers attempt to achieve at least two goals—to keep prices stable and to smooth cyclical fluctuations in output and employment. Too often, Hoskins maintains, central bankers also try to manipulate the exchange rate with a view to strengthening the competitive position of domestic industry. Of course they do not pursue multiple objectives because of a character defect. They merely reflect prevailing opinions held by politicians, bankers, economists and other members of the general public.

In Hoskins' view, the performance of central banks could be much improved if they were granted independence from governments and given a single objective—price stability. The central banks—though independent—would not be allowed to choose policy objectives but would be given a clear legislative mandate to achieve and maintain price stability. Moreover, they would be accountable to the public for their policy actions.

I am largely sympathetic to Hoskins' suggestions. An independent central bank with a clear mandate to pursue price stability is likely to perform better than an institution attempting to

respond to diverse and conflicting political pressures. I also agree with Hoskins that the social value of a credible price-stability objective is often underestimated, whereas the costs of eradicating inflation are overstated.

Thus I support Hoskins' call for committing central banks to a price-stability objective. In my view, however, the story does not end here. A clear price-stability mandate by itself is not enough to improve the performance of central banks. Even if we agree that the objective of monetary policy should be price stability, we still have to address a second question: *How* should central banks achieve and maintain a stable price level?

Hoskins plays down the problems of designing operational policy rules consistent with the price-stability mandate. Yet as practitioners of monetary policy know, the translation of such a mandate into specific policy rules is far from trivial. Switzerland offers a good case in point. I argue that the Swiss National Bank (SNB) possesses a clear mandate to achieve and maintain price stability even though Swiss law does not precisely define the objectives of monetary policy. This mandate, albeit informal, rests on a remarkable consensus among the Swiss public about the objectives of monetary policy.

The SNB's informal mandate explains why the inflation rate in Switzerland has tended to be low by international standards. Since the beginning of 1975—when Switzerland shifted to money stock targeting—inflation in Switzerland has averaged 3.5%. This average, however, still far exceeds the SNB's stated inflation target of 0 percent to 1 percent. Consequently, the SNB has failed to achieve price stability despite the informal mandate. The SNB's failure to meet its stated target results largely from two short episodes of accelerating inflation. From 1979 to 1981 and from 1989 to 1991, Swiss inflation temporarily rose to more than 7 percent and 6 percent, respectively.

NEED FOR OPERATIONAL RULES

The SNB's failure to achieve price stability did not reflect a Keynesian hangover. Rather, the SNB encountered various problems when it attempted to translate its price-stability mandate into suitable operational policy rules. The need for operational rules arises because monetary policy affects the inflation rate with a long and frequently variable lag. In Switzerland the time lag may be as much as three years. Therefore monetary policy decisions do not affect the inflation rate until long after they are implemented. Because of the lag, such decisions invariably entail a great deal of uncertainty. Central banks may err even if they try to adhere closely to their mandate. Once they recognize their mistakes, it is usually too late to take corrective action.

To lower the danger of policy blunders, central banks require reliable early warning signals or leading indicators of inflation. Operational rules centered on these leading indicators give central banks a good chance of accomplishing a goal of achieving and maintaining price stability.

Do central banks possess reliable leading indicators of inflation? This question cannot be answered straightforwardly. Monetarists tend to emphasize the close relationship between money growth and the inflation rate. They maintain that the money stock serves as a good leading indicator of price movements. Therefore central banks are likely to meet the price-stability objective if they adopt an operational rule providing for steady growth in the money supply.

Most central banks today share the monetarist view that inflation is due largely to excessive money growth. Nonetheless, they hesitate to opt for strategies of steady money growth. The SNB is no exception. In Switzerland the growth in both the monetary base and the money stock M1 tend to lead inflation. Therefore the SNB focuses attention on these two aggregates and sets an intermediate target for the Swiss monetary base. It strives to increase the monetary base at a rate of 1 percent per year. The SNB views this target as consistent with price stability in the medium and long runs.

Although the SNB follows a money-growth rule, it need not augment the monetary base by 1 percent year after year. Depending on the circumstances, it may temporarily undershoot or overshoot the 1 percent target. For this reason, the SNB frames its money-growth rule in terms of a medium-range target, to be met on the average of a five-year period. Temporary deviations from the 1 percent growth path may be required if serious unexpected shocks hit the Swiss economy. Two kinds of shocks may prompt the SNB to deviate: unexpected shifts in money demand and other unexpected shocks such as excessive movements in the exchange rate.

SHIFTS IN MONEY DEMAND

A strategy of steady money growth is effective only if money demand is stable. In contrast to many other countries, Switzerland has been blessed with reasonably stable money-demand patterns. But this does not imply that instabilities have not occurred. Serious instabilities arose in the late 1980s as a result of two financial innovations. A new electronic interbank payments system and a major overhaul of liquidity requirements, or minimum reserve requirements, imposed on banks caused a huge permanent drop in the demand for base money. Much of that decline occurred in the first half of 1988, but stability was not restored until about 1990 or 1991.

It is clear that central banks must adjust the money supply to permanent demand shifts or long-lasting temporary demand shifts if they are to keep the price level stable. It is not always advisable to react quickly to demand shifts, however. Money demand is subject to frequent transitory movements that do not call for a central-bank response. Moreover, demand shifts are hard to detect. They often become fully apparent only after considerable time has elapsed. For these reasons, Meltzer (1987) and McCallum (1989, Ch. 16) recommend a slow reaction pattern. They propose mechanical rules that would prompt central banks to adjust the money supply gradually to demand shifts. I support Meltzer and McCallum's call for a gradual response, but I doubt that central banks should be committed to a mechanical reaction pattern. The speed of the response is likely to depend on the nature of these shifts. For example, if central banks know in advance that a major shift will occur, they should adjust the money supply quickly.

Confronted with the demand shift of the late 1980s, the SNB opted for caution. SNB officials knew that a shift would occur but did not know how big the shift would be or how fast base-money demand would fall. As a result of the SNB's cautious response, short-term domestic interest rates fell sharply at the beginning of 1988 but rose again as the SNB gradually lowered the supply of base money. By summer 1988, short-term domestic interest rates returned to their pre-shift levels. Long-term rates, however, did not budge. Thus market participants correctly regarded the fall in short-term interest rates as transitory.

With hindsight, various students of Swiss monetary policy attribute the most recent surge in the Swiss inflation rate to the SNB's cautious reaction to the demand shift. The SNB, they assert, should have acted more aggressively. The SNB's cautious response no doubt was equivalent to a temporary easing of monetary policy. Nonetheless, it cannot be regarded as the main cause of the rise in inflation. I am not aware of any economic theory able to explain how six months of easy money, which the market correctly regarded as transitory, could have generated three years of high inflation. For this reason, I still maintain that central banks should react cautiously to shifts in money demand.

OTHER UNEXPECTED SHOCKS

Similar problems arise from other unexpected shocks that may impinge on the central banks' anti-inflationary monetary policies. In small countries like Switzerland, central banks are frequently compelled to take the real exchange rate into account when setting monetary policy. Real exchange rate movements often fail to reflect economic fundamentals. As I pointed out before, Swiss inflation picked up temporarily in the early 1980s and early 1990s. Although the SNB attempted to keep the monetary base on a growth path consistent with medium-run price stability, the Swiss franc weakened sharply in real terms during both periods of high inflation; that is, the depreciation was much larger than would have been expected on the basis of inflation differentials between Switzerland and other countries. Therefore the exchange-rate depreciation reinforced the inflationary pressures in Switzerland. The SNB reacted to this situation by tightening monetary policy. As a result, the monetary base fell below the medium-run growth path. The tightening of the monetary reins eventually caused the Swiss franc to appreciate again. In this way, the SNB counteracted the inflationary pressures emanating from the exchange rate.

Lee Hoskins takes a dim view of central-bank attempts to manipulate the exchange rate. However, he considers only central-bank efforts to stimulate domestic employment by means of an exchange-rate depreciation. Such policies, I agree, may be inconsistent with the mandate to achieve and maintain price stability. But we should not overlook the situations in which exchange-rate movements undermine central banks' anti-inflationary policy stances.

Nevertheless, Hoskins' objections to exchange-rate policy are often valid. Exchange-rate policy may or may not be consistent with price stability. Swiss experience offers examples of both types of exchange-rate policy. The SNB did more than try to counteract excessive real depreciations of the Swiss franc. In 1978 and 1987 it reacted to an excessive real appreciation by relaxing monetary policy.

Although the real appreciation supported the fight against inflation, the SNB tried to halt or even reverse the upward movement in the exchange rate. The SNB thought that its efforts to curb the appreciation of the Swiss franc were consistent with its mandate to stabilize the price level. In 1978 and 1987 inflation was low and declining. In principle it followed an operational strategy of gradually lowering the inflation rate. In its view a gradual approach would minimize the real costs of achieving and maintaining price stability. Considering its preference for gradualism, the SNB did not welcome the real appreciation of the Swiss franc because it affected the domestic economy in the same way an unnecessary tightening of monetary policy would. Therefore the SNB allowed money growth to rise temporarily above the level consistent with medium-run price stability.

Unfortunately, the SNB's strategy of adjusting money growth to the real appreciation of the Swiss franc turned out to conflict with the price-stability objective. In both periods inflation rose again in due course. The two short episodes of rising inflation are largely explained by the SNB's efforts to counteract an excessive real appreciation of the Swiss franc.

Thus Swiss experience lends at least partial support to Hoskins' objections to exchange-rate policy. However, strict compliance with a price-stability mandate need not imply that central banks should abstain totally from manipulating the exchange rate. Even if the SNB tried to rule out any risks of erring on the side of inflation, it could not afford to ignore real exchange rate movements entirely. Instead it had to react asymmetrically. With an excessive real appreciation of the Swiss franc, the SNB would keep the monetary base on the medium-run growth path. Faced with an excessive real depreciation, on the other hand, it would push the monetary base below that path. The resulting policy might be closer to shock therapy than to gradualism. The real costs of the shock therapy would constitute the price the SNB would have to pay for playing it safe.

In practice, I doubt that central banks are able to disregard entirely the real costs of eliminating inflation. The SNB has repeatedly emphasized that it cannot stabilize the price level without accepting a temporary increase in unemployment. But the Swiss public also expects the SNB to keep the real costs of its anti-inflationary monetary policy as low as possible. Therefore the SNB, in principle, must follow a gradualist approach. We could probably improve our performance if in the future we display greater reluctance to react to excessive real appreciations of the Swiss franc than we have in the past.

CONCLUSIONS

Let me conclude by emphasizing again that I agree with the thrust of Hoskins' reasoning. Monetary policy should be entrusted to independent central banks with a clear legislative mandate to achieve and maintain price stability. But in my view, independence and a clear mandate are not sufficient to guarantee a good monetary policy performance. It is also important that central banks adopt operational policy rules consistent with their mandate. Although central banks should be free to choose appropriate operational rules, they should be committed to spell out explicitly *how* they intend to fulfill their mandates. In particular, they should state how they intend to respond to shifts in money demand and other unexpected disturbances.

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Financial Regulation and the Competitiveness of the Large U.S. Corporation

A CENTRAL QUESTION OF THE DAY is whether U.S. business firms are capable of success in highly competitive world markets. The question is embedded in hotly debated calls for the United States to develop an explicit industrial policy, in frequently expressed concerns about our loss of market leadership in the computer chip, television and automobile industries, and in charges of excessive executive compensation. It is important to consider the efficiency of the large corporation when answering this question, and what I discuss here is a connection between corporate efficiency and the regulation of capital market institutions.

The legal setting of a large U.S. corporation is usually thought of in terms of regulations that bear directly on the activities of business firms. These include business tax policy, environmental protection legislation, worker safety and health regulation, and antitrust. Because legal settings for business vary from nation to nation, regulations undoubtedly affect relative efficiencies of business firms differently in different parts of the world. Business regulation of this type has been discussed explicitly on many occasions, so I set it aside here. Instead, I give attention to the neglected connection between corporate efficiency and the regulation of capital market institutions. My purpose is to show how the regulation of banks, insurance companies and mutual funds impinges on shareholder control of top management in U.S. corporations.

Because most persons at this conference do not work in corporate economics, it is useful to begin by considering the potential control problem created by the diffuse ownership structure on which the modern large corporation rests—separation of ownership and control. This well-known agency problem has been around for some time. Even Adam Smith voiced concern in *The Wealth of Nations,* precisely because he believed that those who manage the funds of others cannot be expected to do as good a job as if their own funds were at stake. Along with many contemporary economists, the works of Veblen (1921), Berle and Means (1932), and Galbraith (1967) build heavily on this corporate control problem. Their works assert that owners of shares each have an ownership stake in the corporation that is too small to motivate efforts to control management and that is too small to convey disciplinary weight even if such efforts were made.

Dissatisfied shareholders can do little better than sell their shares. If such sales are large, the price per share will fall and adversely affect the terms on which management can raise new capital from the capital markets. This price effect penalizes errant management, but it does so only indirectly and only to the extent that the corporation finds it necessary to raise new capital.

The alleged weakening of the link between ownership and control makes the proposition correlating private ownership and efficient resource allocation more problematic in the minds of some students of the corporation. Uncontrolled professional management is likely to see its interest served, at least partly, by high management compensation, large firm size, altruism toward friends and community, leisure and other forms of on-the-job consumption and by indulging in these to an extent that seems inimical to shareholder interests.

The thesis appears to be much like that which popularly explains the failings of socialism. If all citizens are in principle owners of state property then no person *qua citizen* can exercise control over this property. Ownership is simply too diffuse to be effective. Managing this property then becomes the task of state bureaucracies. State employees, however, have interests that do not coincide with those of the population at large, and the pursuit of these interests is not guided by market incentives. A separation between ownership and control arises and undermines the credibility of socialism. The separation thesis as applied to the large corporation substitutes professional management for state bureaucracies.[1]

Studies of corporate takeovers, mainly corporate takeovers undertaken in the United States during the 1980s, provide evidence of some instances of separation between ownership and control. These show that shareholders of target companies benefit considerably from a takeover of their firm. Successful takeovers increase share prices of target firms by an average of about 30 percent.[2] Increases in share price may derive in part from several aspects of takeovers. The dominant view is that most of target shareholder gains derive from the removal of inept management, whose presence is consistent with the separation thesis.[3] It should be noted, however, that only a small fraction of corporate assets has become the target of takeover attempts. This can be interpreted as statistical support for a proposition contrary to the separation thesis—that most modern corporations are *not* afflicted by significant separation between ownership and control.

The indictment of the modern corporation implicit in the separation thesis creates its own puzzle. Because the corporation, including its ownership structure, arises from contractual agreements voluntarily entered into, the separation thesis implies that serious, systematic and persistent errors are made by owners of the corporation in relying on ownership structures that are too diffuse. Owners fail to anticipate that they are abandoning control over their assets. This is inconsistent with the belief held by most economists that all parties to an agreement reached voluntarily *expect* to benefit from the agreement and that if the agreement is used repeatedly and extensively, this expectation is usually correct.

However, the empirical supposition of the separation thesis, the "fact" to which all adherents to the thesis have subscribed, is not at all fact. It is simply not the case that the ownership structure of the typical large corporation is so diffuse that it undermines the incentive and power of shareholders to influence management. That thousands of shareholders jointly own the typical large corporation is true, but recent studies show that not every owner of corporate stock owns an insignificant number of shares. A few of the thousands of shareholders usually own a relatively large fraction of the firm's equity.[4] In fact, the *typical* large corporation has a more concentrated ownership structure than serves the separation thesis well. For Fortune-500-sized U.S. corporations, the aggregate fraction of equity owned by the five largest shareholders is about one-fourth, and in Japan and several important European countries

[1]Socialized property and jointly owned corporate property are, however, far from equivalent. Socialized ownership is coerced into being, whereas corporate ownership is devised voluntarily. Given the facts of economic development and per capita wealth in East and West, we can surmise only that if there is separation between ownership and control in the large private corporation, it is less severe by several orders of magnitude than it is in the socialist state.

[2]See Jarrell, Brickley and Netter (1988).

[3]Evidence to date seems to indicate that target company gains do not derive from wealth transferred from bond holders [Jarrell, Brickley and Netter (1988)] or from most lower level employees. Although management personnel are released in disproportionately large numbers from target companies when a takeover occurs, the mass of laborers are not.

[4]Demsetz and Lehn (1985) and Shleifer and Vishny (1986).

this fraction is much larger. The typical case then is one in which a relatively small number of shareholders have well focused interests and nontrivial blocks of votes. Facing such concentrated share holdings, professional management cannot be as unguided by shareholder interests as the separation thesis supposes, although there surely are some cases in which ownership structure has become too diffuse to serve shareholder interests well. When this occurs, ownership should be restructured.[5]

Restructuring occurs in two ways. Corporate takeovers provide a dramatic mechanism for concentrating existing diffuse ownership structures. Less dramatically but more continuously, ownership is restructured through the normal issuing and purchasing of equity shares. At any given time the diffuseness with which shares of firms are held varies across corporations, but restructuring should adapt ownership structures to the different situations confronted by different firms. This implies that observed structures should bear a sensible relationship to these situations. More specifically, we may posit that variations in ownership structure reflect the benefits and costs to shareholders of controlling professional management tightly.

Concentrated ownership (and consequent tight control over management) comes at a cost. If this cost is high, the ownership structure that is truly profit maximizing must look much like that of the single-owner firm. This is the case in particular for large firms because size of firm correlates with one of the major costs of concentrated ownership—the bearing of firm-specific risk. Because controlling shareholders would tend to have a large fraction of their wealth invested in a single firm if this firm is large, they would be exposing themselves to firm-specific risk. The larger the firm, the larger is the wealth they must commit to own a controlling share of equity, and hence the greater is their exposure to firm-specific risk. The risk-adjusted, utility-maximizing ownership structure for large firms, contrary to what is suggested by the separation thesis, is not the single-owner firm.[6] It is a more diffuse ownership structure because the cost of bearing firm-specific risk should be reflected in the optimal ownership structure. Nonetheless, this structure should be one in which enough shares are owned by a few shareholders that they can exercise more than a modicum of control over professional management. The data reveal precisely this—greater diffuseness in ownership structure for larger firms accompanied by enough concentration of ownership to imbue large shareholding interests with influence over management. This pattern of ownership, which suggests that shareholders choose ownership structures that maximize the value of their firms, has been confirmed for Swedish, Japanese and South African firms.[7]

Of course, management cannot be disciplined so thoroughly by controlling shareholders and by the threat of corporate takeovers that ineptness is dethroned at once wherever and whenever it exists. In this respect, it is important to remember that *ownership is not structured exclusively for the purpose of dealing with management ineptness.* Other things matter to ownership structure and to risk-adjusted profit, such as the avoidance of firm-specific risk. If ownership is structured to maximize risk-adjusted utility, it must not be so tightly structured that all error in judging professional management is eliminated. Moreover, because there is a cost to altering the structure of ownership quickly, profits are also maximized by tolerating a lag between evidence of ineptness and altering ownership structure appropriately. Things will get out of whack on

[5]There are several ways by which professional management can be guided to serve shareholder interest in the modern corporation—concentrated ownership (achieved through the normal financing of corporations or through corporate takeovers), the consequences of the capital market's measurement of management performance, legal proceedings, and compensation systems. Time and space allow me to consider here only concentrated ownership. This is unfortunate especially in regard to executive compensation, for there are new empirical results to report about this. It is improbable that all these mechanisms transform the modern corporation into a precise analog of the firm pictured in neoclassical theory, but they do raise serious questions about the Berle and Means thesis.

[6]I speak somewhat superficially in reference to risk-adjusted utility maximization. Suppose a real corporation *is* owned by a single person, and suppose further that he guides his professional managers without error to pursue his chosen ends. Although risk-adjusted profit always looms important to this owner, it need not be his sole concern. He might derive *satisfaction* from owning a larger firm even if it is less profitable, or from using the firm's assets to cater to personal *utility* maximization. The reduction in profit he thereby bears must not be thought of as a loss sustained because an agency problem separates his interests from management's behavior. There is no agency problem here, there is simply the recognition that in cases such as this, profit maximization for the owner does not equate to utility maximization for the owner. This may also hold for degrees of ownership concentration less than the 100 percent just assumed.

[7]See Bergstrom and Rydqvist (1990), Prowse (1991), and Gerson (1992).

occasion, and when they do, dramatic restructuring of ownership is more likely to be called forth in the guise of a corporate takeover.

What seems to be true then is that professional management is not free of substantive guidance by shareholders, but that the degree of guidance, because it responds to problems of risk and other similar concerns, will not generally be designed solely for the purpose of controlling management malfeasance. From a shareholder's perspective, the optimal amount of management malfeasance is positive, not zero. Just what is optimal, however, is affected by the legal environment, and especially by laws bearing on the operation of capital market institutions. Ownership will tend to be more concentrated, and management malfeasance will consequently be less pervasive to the extent that these laws do not raise the cost of maintaining concentrated ownership structures.

Recent data reveal a puzzle regarding ownership concentration. After standardizing for variables that should influence the ownership structure of corporations, such as firm size and firm-specific risk, studies of corporate ownership reveal large differences across countries in the typical degree to which ownership is concentrated. Ownership is noticeably more diffuse in U.S. corporations than in Japanese, European and South African corporations. In the typical large corporation in the United States, the top five shareholders, as a group, own about one-fourth of the firm's outstanding voting stock. Most corporations traded on South Africa's Johannesburg Stock Exchange are controlled by small shareholder groups who own 50 percent or more of voting stock.[8] Ownership structures in Germany and Sweden are more like South Africa than the United States.[9] In Japan, the five largest shareholders own about 33 percent of voting shares.[10]

The differences between the United States and these other countries are so large that we must suspect that the cost of concentrating corporate ownership differs substantially from one country to another and for reasons not captured by the variables being used to index this cost. If a five-shareholder group owning one-fourth of the voting equity of the typical large corporation is a suitable ownership structure in the United States, why is it not in other countries? A plausible source of this difference is in variation across nations of regulations that impinge on ownership structure and which make it more costly to maintain control in the typical large U.S. corporation than in the typical large non-U.S. corporation. Important capital market institutions in the United States do bear special costs to hold large stakes in the equities of other companies, and our banks are barred from holding any stake.[11]

One potential source of equity capital is the investment company, but the Investment Company Act of 1940 restricts the ability of investment companies to take concentrated equity positions in the firms in which they invest if they advertise themselves as diversified investment companies. There is a tax advantage to registering as a diversified investment company, since this entitles the company to pass income through to its investors without paying taxes, but even investment companies that do not register as diversified are barred from exercising control over any firm engaged in interstate commerce. Hence funds channeled through investment companies are unlikely sources of controlling positions in the equity of corporations.

Insurance companies are another potentially important source of equity capital, and most states do allow insurance companies to invest a percentage of their assets in common stock. The percentage varies from state to state but is commonly about 20 percent. New York, a particularly important state for insurance, limits the amount that insurance companies can invest in one company to 2 percent of the insurance company's assets. Most other states have similar restrictions, but the percentage varies over a large range. States generally bar insurance companies from owning more than a stipulated percentage of the shares of other companies. A common upper bound is 10 percent. Finally, there frequently is a penalty borne by insurance companies that invest in common stock; most states require that capital be set aside to maintain a financial cushion against declines in the price of stock held for investment purposes. Although it is not impossible to use funds channeled through

[8]See Gerson (1992).

[9]See Sundqvist (1986).

[10]Prowse (1991).

[11]My summary discussion of the details of some of the relevant legislation rests heavily on work by Paul S. Clyde (1990).

insurance companies to take a concentrated equity position in a given corporation, it clearly is an investment tactic that is generally discouraged by state-imposed restrictions. Hence, a second capital market institution is handicapped in such an undertaking.

For more than 60 years the Glass-Steagall Act has barred banks from directly owning equity in U.S. corporations. There is no counterpart to this law in South Africa and in much of Western Europe, and only recently has Japan adopted a similar law. Although banks would seem to be low-cost conduits of equity capital, Glass-Steagall forces corporations to raise equity funds from other sources. In fact, banks play important equity roles in other nations, where they supply enough equity to own sizeable positions in corporate ownership structures. The possible connection between Glass-Steagall and ownership structure, however, is not generally suspected even though banks are potentially a major source of equity investment capital in the United States.[12] If the behavior of foreign banks in their own countries is a guide to what U.S. banks would do if allowed to invest in corporate equity, it seems likely that U.S. banks would be important sources not only of equity capital, but also of concentrated ownership positions. A third major source of concentrated ownership is thus barred by legislation.

Because of recent court decisions, employment retirement funds remain one important source of capital that is free to take equity positions, even concentrated equity positions. In fact, we find a few of these funds playing key roles in monitoring and disciplining corporate management by virtue of their large holdings of stock in particular corporations. Most notable in this regard is the California State Employees Retirement Fund, but others have also become activist. For reasons discussed later, however, I do not believe that these funds offer monitoring and disciplining services as good as those likely to come from capital market institutions presently barred or penalized from taking large equity positions in specific corporations.

The consequence of these legal barriers is that corporations housed in the United States rely on capital that is secured directly from individual investors to a much greater extent than corporations located overseas. Really large controlling positions in the equity of U.S. corporations are taken mainly by individual and family investors in the United States. Because of greater portfolio specialization, these individuals and families are exposed to more firm-specific risk than capital market institutions would be. Moreover, individual or family wealth is seldom large enough to allow concentrated holdings of the equity of large corporations. The heavy reliance in the United States on this source of equity capital results in corporate ownership structures much more diffuse than those that exist for comparable foreign corporations. The optimal degree of control exercised by shareholders over the managements of their U.S. corporations, as a result of such legislation, is less than elsewhere.

Arguments pro and con can be made in regard to the various legal hurdles that keep important institutional conduits of capital from accessing the equity markets easily. Whatever the truth in this regard, the effect of these legal hurdles on ownership structure and control has not yet been taken into account. The control problem created by these hurdles, taken by itself, offers a novel basis for opposing such legislation.

But can institutional investors—for example, investment companies, insurance companies and banks—be relied on to perform the ownership function well? Since their capital comes from diffuse sources, it would seem that their own operations should be subject to the separation problem believed to plague large corporations. If so, institutional investors holding controlling positions in the equity structure of large corporations cannot be expected to perform the duties of owner as well as investors whose own wealth is at stake. I discuss this issue in the remainder of this paper, showing that the control problem can be ameliorated by such institutions but not as completely as if individuals owned concentrated ownership positions in corporations directly.

There are institutional investors that seem capable of circumventing the problem created by their own diffuse ownership structure, and there are others that seem not so capable of doing this. The distinction between the two lies in the ease with which individual investors can reclaim their funds from the institution. The open-ended mutual stock fund is organized so that investors can insist that the fund buy back their shares at the net asset value they represent in

[12]Exceptions include Prowse (1990) and Gerson (1992).

the fund's portfolio. The closed-end stock fund has no such obligation; an investor who wants to convert his shares in such a fund to cash may sell them at whatever market-determined price they fetch, but he cannot demand their redemption by the fund. This is an important difference when it comes to the issue of separation between ownership and control. To see its importance, let us reconsider the separation problem in the context of the corporation.

Two conditions must exist for the separation problem to be severe in a corporation. One is the generally recognized condition that ownership structure be diffuse. The other is the condition that assets made available to a corporation by shareholders must belong to it and not to shareholders. This second condition has not been recognized explicitly in economic literature, but it is important. It refers to the fact that, although the shareholder may sell his shares if he is dissatisfied, the shareholder cannot insist that the corporation be the buyer of his shares. Thus the corporation, not the shareholder, has title to the productive assets it has purchased with funds secured from its initial issue of stock.[13] If shareholders could reclaim these assets, the severity of the separation problem would be lessened even for diffuse ownership structures. It would be lessened even more if share ownership were concentrated, because shareholders with much at stake will be more attentive to what management has been doing with the firm's assets.

It is not practical to allow shareholders to reclaim their share of the firm's assets in the general case of the business firm. The typical corporation makes commitments to supply goods and services that, if they are dependably honored, require the corporation to have continuing control of its assets. A steel company cannot reliably stand by a commitment to fill an order for steel if its shareholders can force it to sell its assets to purchase back their stock. The typical corporation therefore must be organized in a way that bars investors from reclaiming their fraction of the firm's assets, and once the typical corporation sells a new issue of shares, the funds it acquires belong to *it*, not to those who purchased the shares.

Continued control by the firm over its assets is not a prerequisite to doing business if credible commitments of this sort are not necessary. Consider the open-ended mutual stock fund. This firm gathers capital from investors and uses its skill to place these funds in the shares of other corporations. These shares can be sold by the mutual stock fund on a moment's notice should it decide to do so, and in doing so it will not be jeopardizing any business commitments. Consequently, investors who place their capital at the disposal of open-ended mutual funds can withdraw their *pro rata* share of the value of the fund's assets should they become displeased with the fund's performance. *De facto,* the open-ended mutual fund is obligated to repurchase *pro rata* investment positions. These investors are not shareholders in the conventional sense. They are purchasers of investment services, but they also are providers of the capital that is in turn invested in shareholdings of other companies. In the absence of the Glass-Steagall Act, the same arrangement could work for banks who reinvest depositor funds in the shares of other corporations (but probably could not work well for that part of bank investments that constitutes time-commitment loans to business firms). Should those depositors who have made no commitment to keep their funds with a bank decide to withdraw deposits, the bank could sell its shareholdings in other corporations to cover the withdrawals.

It is this characteristic, the ability of investor-depositors to reclaim capital from a firm, that distinguishes these institutions from others for our purposes. The closed-end fund does not have this characteristic. It is organized like the typical corporation. It issues shares and converts the funds from their sale to assets that belong to it. Dissatisfied shareholders may sell their shares, but they cannot force the closed-end fund to be the purchaser. This allows the fund's management to make its investment plans without fear of being forced to alter them should investor desires for cash or beliefs about the investment environment change, but it also eliminates the threat to management that it will lose control of fund assets if the fund performs poorly.

It is this threat in the case of the open-end mutual investment fund that reduces the

[13]Subsequent sale of shares by shareholders has a depressing effect on the price of the corporation's stock if enough shareholders offer to sell, and this has some disciplining effect on management, but even so, the corporation remains in control of the assets it has acquired.

severity of the separation problem. Should investors become dissatisfied in large numbers, mass withdrawals would diminish the assets available to a fund's management, forcing it to sell the shares they own in other companies. This reduction in the wealth available to the managements of these institutions can take place even if no single investor or small group of investors has provided a lion's share of the capital being invested. This disciplines the managements of these institutions in a way not available to stockholders when they are disappointed with the managements of typical corporations. The large scale sale of shares in the typical corporation depresses share price but does not reclaim assets from management control.

What this means is that managements of institutional investors of the open-ended mutual fund variety can be disciplined directly by providers of capital even when there is no concentrated provision of this capital. The diffuse owner problem is ameliorated, but only to a limited extent. It is more effectively defused if capital is provided in concentrated fashion to the institutional investor, for concentration of rewards and penalties makes the large shareholder more attentive and astute. Now suppose that this type of institutional investor has taken controlling positions in the equity of the firms whose shares it has purchased. The ability of even diffuse contributors of its capital to withdraw their assets surely makes the institutional investor represent its investors' interests better than if the threat of withdrawal did not exist—as long as the ability of the institution to make long-term commitments is not important to its productivity. Because of this effect, capital secured from even diffuse sources can be combined without suffering fully from a separation between ownership and control.[14]

One final point may be raised about another source of diffuse ownership in the United States. The New York Stock Exchange (NYSE) requires that firms it lists raise their equity capital on a one-share, one-vote basis. The NYSE did not always use this standard. It was adopted during the 1920s under considerable pressure from government and intellectuals who feared that the growing use of differing vote entitlements was disenfranchising many equity capital providers. Nonvoting equity shares are used much more extensively in other countries. This makes for a lower cost of establishing controlling equity positions in a company because only voting shares must be reckoned with when considering the direct control of management. Discussion of this issue, however, cannot be undertaken here.

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[14]In fact, a doctoral dissertation recently completed at UCLA [Clyde (1990)] gives evidence that institutional investors behave much as do individual and family shareholders who own controlling positions in the ownership structure of a corporation.

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Commentary

HAROLD DEMSETZ RARELY FAILS to deliver a creative and thought-provoking paper, and this one is no exception. I have learned a great deal from Harold's writings over the years and usually find myself persuaded by his arguments. In this paper Demsetz explores the implications of certain restrictions on the behavior of financial institutions for the efficiency of the market for corporate control. This is a potentially important consideration and one that, to my knowledge, has not been systematically investigated.

The basic thesis of the paper can be summarized in three steps. The first step is to recognize that the diffusion of ownership of large corporations creates a control problem for owners (that is, stockholders). This well-known agency problem has long been the focus of intense study by economists. It is important to recognize, however, that the degree of diffusion of ownership reflects both costs and benefits to shareholders. The benefits arise from the reduction of firm-specific risk borne by owners through the diversification of their holdings. The costs arise from the potential loss of control over management.

The second step in the analysis is to argue that financial regulations limiting the extent of ownership in a corporation by certain types of institutional investors, including insurance companies, investment companies and commercial banks, potentially raise the costs of controlling management. The third step in the argument is to suggest that this reduced ability of owners to monitor and control managers reduces the efficiency of large corporations and thus tends to make them less competitive than corporations in countries where institutional investors are not subject to such restrictions.

I have no difficulty with the logic or thrust of this line of reasoning. It is rare that regulations are neutral and thus fail to distort resource allocations. The questions I am interested in focusing on have to do with whether the market has created alternative means of monitoring management. If so, then the question is, which means is most cost efficient?

Demsetz recognizes that concentrated ownership is not the only means of exercising control over management. Boards of directors provide important control mechanisms and have begun to reassert their authority. The recent cases of General Motors, American Express and IBM are good examples. Management compensation is another means to align management and shareholder interests. In general, the market for corporate control is an important monitoring device. Though it requires individuals or firms to obtain a concentrated ownership in a company, it does not necessarily depend on large financial institutions acting as the investors doing the monitoring. Large pension funds that are not regulated like banks or investment companies have become increasingly active in monitoring management. CALPERS is one of the most well-known funds and has been deeply involved in pressuring for management changes in several companies.

Demsetz stresses that there must be a cost to regulation that prohibits investment companies,

insurance companies and banks from taking positions that encourage them to monitor management more closely. This is undoubtedly true. But it is an empirical question as to the importance of these restrictions. I would like to suggest that there may be reasons to believe the effects are small—if for no other reason than the marketplace is innovative in getting around such restrictions, particularly when there are large rewards for doing so.

Institutional investors can provide two sorts of services—risk sharing or diversification and management monitoring. There is no particular reason why expertise in one activity implies expertise in the other. In fact, it is easy to imagine that some institutional investors would specialize in one activity or the other. For example, Dean LeBaron of Batterymarch Funds and Rex Sinquefield of DFA view themselves primarily as portfolio managers. Neither of them seems to have the slightest interest in monitoring management. Why? Even though they must file 13d's indicating when they own a significant share of a particular company, probably only a small percentage of their portfolios is made up of companies of which they own a significant share. These fund managers specialize in risk sharing, not control or monitoring. Why should they have a comparative advantage in monitoring management? Just because they are skilled at managing risk does not mean they are skilled at management control. LeBaron has even pushed the idea of selling voting rights that would allow the separation and specialization in monitoring and risk sharing.

Why should one expect that managers of regulated insurance companies or investment companies have a comparative advantage in monitoring management? If they do not, the regulation is likely to have little substantive effect.[1]

Researchers sometimes feel that banks are different in this regard. Some view banks as having access to an informational advantage over other parties and thus being in a particularly good position to exercise control over management. Indeed, in Japan and to a lesser extent in Germany, this has been standard practice. If banks held both debt and equity then they would clearly have a strong interest in managerial monitoring. It is not clear, however, that they would always represent shareholder interests. It is worth recalling that the Glass-Steagall Act was not motivated by a desire to limit managerial control by banks but from a desire to stabilize the payments system involving the other side of the bank's balance sheet. In fact, certain types of banks are not subject to these limitations because they are not depository institutions, and they sometimes do take concentrated ownership positions.

The marketplace has clearly responded to the demand for corporate control through a variety of mechanisms and institutional arrangements that go far beyond the regulated financial intermediaries. In the case of monitoring management, new funds and partnerships have been created that specialize in seeking concentrated ownership for the purpose of control. One of the earliest of these was WESRAY, which was a partnership between William E. Simon and Raymond Chambers. They engineered successful leveraged buyouts (LBOs) for Gibson Greeting Cards, Avis and Wilson Sporting Goods. Kohlberg, Kravis and Roberts (KKR) is another successful partnership that specializes in obtaining concentrated ownership for the purpose of controlling management. In fact, by 1990, almost every major investment bank had created its own LBO fund (for example, J. P. Morgan and The First Boston Corp.). These funds and their managers specialize in ownership and control, not in providing risk sharing for investors. Thus it would appear that financial institutions and the market have responded to the demand for corporate control in innovative ways that circumvent some of the distortions caused by financial regulations on banks and other institutions regarding ownership.

There are other areas where regulation of financial institutions may be affecting the market for corporate control. Many of the LBO funds frequently obtain bridge financing from commercial banks. Unfortunately, under the Financial Institutions Reform, Recovery and Enforcement Act, banks are now much more limited in their ability to deliver such financing because of direct restrictions on purchases of high-risk securities and generally higher capital requirements. Thus, there remain potentially

[1]Of course, given the history of regulation, skilled monitors are likely to have migrated out of managing funds in these regulated firms.

important avenues for financial regulations on ownership to affect the efficiency of the corporate control market.

The final element I would like to comment on briefly is what, if anything, all this says about international competition. It is certainly true that U.S. corporations operate in a global market and that to the extent we reduce the efficiency or raise the costs of corporate control mechanisms serving to make U.S. companies better managed, we put ourselves at a competitive disadvantage. The two observations that Demsetz makes are that many other countries do not have the same restrictions on ownership by financial institutions and that in some foreign countries, structures are less diffuse than in the United States. Though both observations are potentially relevant, it would be more interesting if someone could gather evidence that linked the cross-country patterns of ownership and regulation to patterns of corporate performance and corporate control.

In summary, I think this is an interesting paper that helps focus attention on a set of issues that deserves more study. Regulations often have subtle and unintended effects and in some cases those may turn out to be of first-order importance. The issues discussed in this paper may fall into this category. Nevertheless, we must never underestimate the creative genius of market participants in circumventing regulations when large profit opportunities exist.

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Assessing Applied Econometric Results

IT IS A GREAT HONOR to be asked to participate in this conference to celebrate the work of Ted Balbach, who has long upheld the standard of relevant, independent, intelligible economic studies at the Federal Reserve Bank of St. Louis.

My invitation to this conference asked for a philosophical paper about good econometric practice. I have organized my views as follows. Part I of the paper defines the concept of an ideal econometric model and argues that to tell whether a model is ideal, we must test it against new data—data that were not available when the model was formulated. Such testing suggests that econometric models are not ideal, but are approximations to a changing reality. Part I closes with a list of desirable properties that we can realistically seek in econometric models. Part II is a loosely connected set of comments and criticisms about several econometric techniques. Part III discusses methods of evaluating econometric models by means of their forecasts and summarizes some results of such evaluations, as proposed in part I. Part IV resurrects an old, plain-vanilla equation relating monetary velocity to an interest rate and tests it with more recent data. The rather remarkable result is that it still does about as well today as it did nearly 40 years ago. Part V is a brief conclusion.

HOW TO RECOGNIZE AN IDEAL MODEL IF YOU MEET ONE

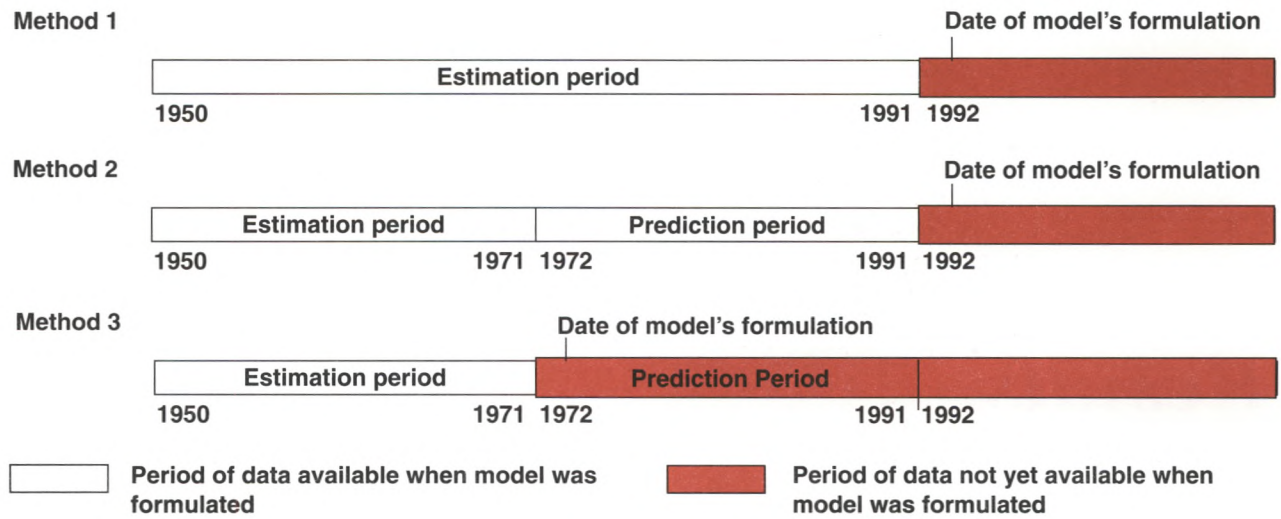
The Goal of Research and the Concept of an Ideal Model

The goal of economic research is to improve knowledge and understanding of the economy, either for their own sake, or for practical use. We want to know how to control what is controllable, how to adapt to what is uncontrollable, and how to tell which is which. The goal of economic research is analogous to the prayer of Alcoholics Anonymous (I do not suggest that economics is exactly like alcoholism)—"God grant me the serenity to accept the things I cannot change; the courage to change the things I can; and the wisdom to know the difference."

The goal of applied econometrics is *quantitative* knowledge expressed in the form of mathematical equations.

I invite you to think of an ideal econometric model, by which I mean a set of equations, complete or incomplete, with numerically estimated parameters, that describes some interesting set of past data, closely but not perfectly, and that

Figure 1
Three Methods of Formulating and Estimating a Model and Checking Its Correspondence with 1950-1991 Data



will continue to describe all future data of that type.

The Need for Testing Against New Data

How can we tell whether we have found an ideal econometric model? We can certainly tell how well a model describes a given set of *past* data. (We will discuss what is meant by a good description later). Suppose we have a model in 1992, with estimated parameters, that closely describes past data for 1950–91. To tell whether it is the ideal model we seek, we must try it with future data. Suppose that after three years we try the model with data for 1992–94, and it describes them closely also. Still, in 1995 all we will be sure of is that it describes data closely for a past period, this time from 1950 through 1994. In principle we can never be sure we have found an ideal model because there will always be more future data to come, so we will never be able to say that a model is ideal. The longer the string of future data that a model describes closely, however, the more confidence we have in it.

Is this only a matter of the *amount* of data that the model describes, or is there something else involved? I argue that something else is involved.

Suppose again that in 1992 we have a model that closely describes an interesting data set for the past period 1950–91. Consider the following three methods, shown in figure 1, by which this model might have been obtained and by which its ability to describe data for 1950 through 1991 might have been assessed:

1. It was formulated in *1992*, and fitted to data for the *entire* period 1950–91.
2. It was formulated in *1992*, fitted to data for the *sub-period* 1950–71, and used to predict data from 1972 through 1991.
3. It was formulated in *1972*, fitted to data for the *sub-period* 1950–71, and used to predict data from 1972 through 1991.

Methods 1 and 2 differ in that method 1 fits the model to *all* the available data, whereas method 2 fits it to the first part only and uses the result to predict the second part, from 1972 onward. 1972 is not a randomly chosen date. It was the year before the first oil crisis. Method 3 differs in that the model builder did not yet know about the oil crisis when formulating the model.

Now consider the following question: Given the goodness of fit of this model to data for the whole period 1950–91, does your confidence in the model depend on which of these three methods was

used to obtain it? I argue that it should. In particular, I argue that an equation obtained by a method similar to method 3, which involves testing against data that were not available to the model builder when the model was formulated, deserves more confidence than the same equation obtained by either of the other two methods.

The argument has to do with the goal of an econometric model—to describe not only past data, but also future data. It is easy to formulate a model that can describe a given set of past data perfectly but cannot describe future observations at all. Of course, such a research strategy should be avoided.

Here is a simple example. Imagine a pair of variables whose relationship we want to describe. Suppose we have two observations on the pair of variables. Then a line, whose equation is linear, will fit the data perfectly. Now suppose we obtain a third observation. It will almost certainly not lie on the line determined by the first two observations. But a parabola, whose equation is quadratic (of degree 2), will fit the three observations perfectly. Now suppose a fourth observation becomes available. It will almost certainly not lie on the parabola. But a sort of S-curve, whose equation is cubic (of degree 3), will fit the four observations perfectly. And so on. In general, a polynomial equation of degree n will fit a set of n+1 observations on two variables perfectly, but a polynomial of higher degree will be required if the number of observations is increased. Methods of this type can describe any set of past data perfectly but almost certainly cannot describe any future data.

If a model is to describe future data, it needs to capture the enduring systematic features of the phenomena that are being modeled and it should avoid conforming to accidental features that will not endure. The trouble with the exact-fitting polynomial approach just discussed is that it does not try to distinguish between the enduring systematic and the temporary accidental features of reality. In the process of fitting past data perfectly, this approach neglects to fit enduring systematic features even approximately.

This relates to the choice among methods 1, 2 and 3 for finding a model that describes a body of data. When formulating a model, researchers typically pay attention to the behavior of available data, which perforce are past data. One tries different equation forms and different variables to see which formulation best describes the data. This process has been called data mining. As a method of formulating tentative hypotheses, data mining is fine. But it involves the risk of being too clever, of fitting the available data too well and hence of choosing a hypothesis that conforms too much to the temporary accidental and too little to the enduring systematic features of the observed data. In this respect it is similar to the exact-fitting polynomial approach described earlier, though not as bad.

The best protection against having done too good a job of making a model describe past data is to test the model against new data that were not available when the model was formulated. This is what method 3 does, and that is why a model obtained by method 3 merits more confidence, other things equal.

Trygve Haavelmo once said to me, not entirely in jest, that what we economists should do is formulate our models, then go fishing for 50 years and let new data accumulate, and finally come back and confront our models with the new data.

Wesley Mitchell put the matter very well when he wrote the following:[1]

> The proposition may be ventured that a competent statistician, with sufficient clerical assistance and time at his command, can take almost any pair of time series for a given period and work them into forms which will yield coefficients of correlation exceeding ±.9. It has long been known that a mathematician can fit a curve to any time series which will pass through every point of the data. Performances of the latter sort have no significance, however, unless the mathematically computed curve continues to agree with the data when projected beyond the period for which it is fitted. So work of the sort which Mr. Karsten and Professor Fisher have shown how to do must be judged, not by the coefficients of correlation obtained within the periods for which they have manipulated the data, but by the coefficients which they get in earlier or later periods to which their formulas may be applied.

Milton Friedman, in his review of Jan Tinbergen's pioneering model of the U.S. economy, referred to Mitchell's comment and expressed a similar idea somewhat differently:[2]

> Tinbergen's results cannot be judged by ordinary tests of statistical significance. The reason is that

[1]See Mitchell (1927).

[2]See Friedman (1940) and Tinbergen (1939).

the variables with which he winds up, the particular series measuring these variables, the leads and lags, and various other aspects of the equations besides the particular values of the parameters (which alone can be tested by the usual statistical technique) have been selected after an extensive process of trial and error *because* they yield high coefficients of correlation. Tinbergen is seldom satisfied with a correlation coefficient less than .98. But these attractive correlation coefficients create no presumption that the relationships they describe will hold in the future. The multiple regression equations which yield them are simply tautological reformulations of *selected* economic data. Taken at face value, Tinbergen's work "explains" the errors in his data no less than their real movements.

That last statement can be strengthened. Tinbergen's method, which has been the method of most model builders ever since, explains whatever temporary accidental components there may be in the data (regardless of whether they are measurement errors), as well as the enduring components.

Most macroeconometric models formulated before the 1973 oil crisis had no variables representing the prices and quantities of oil and energy. Most of these models were surprised by the oil crisis and its aftermath, and most of them made substantial forecast errors thereafter. Many models formulated after 1973 pay special attention to oil and energy. Of course many of those models provide better explanations of the post-oil-crisis data than do models that ignore oil and energy. But my point is different. A model that was formulated after the oil crisis was specifically designed to conform to data during and after the crisis, and if there are temporary accidental variations, the model will conform to them just as much as to the systematic variations. Hence the task of explaining data between the onset of the 1972 oil crisis and 1992 is easier for a model that was formulated in 1992 than for a model that was formulated before the crisis. Therefore if both models do equally well at describing data from 1950 to 1991, the one formulated before the crisis has passed a stricter test and merits more confidence.

What about the relative merits of methods 1 and 2? Sometimes method 2 is recommended; that is, it is recommended that researchers estimate a model using only the earlier part of the available data and use the later part as a test of the model's forecasting ability. When thinking about this proposal, consider a model that has been formulated with access to all of the data. It does not make much difference whether part of the data is excluded from the estimation process and used as a test of that model, as in method 2, or whether it is included, as in method 1. Either way, we draw the same conclusions. If the model with a set of constant coefficients describes both parts of the data well, method 1 will yield a good fit for the whole period and method 2 will yield a good fit for the estimation period and small errors for the forecast period. If the model with a set of constant coefficients does not describe both parts of the data well, in method 1 the residuals, if examined carefully, will reveal the flaws, and in method 2 the residuals, the forecast errors or both will reveal the flaws. And with both methods 1 and 2 we have a risk that the model was formulated to conform too much to the temporary accidental features of the available data.

One noteworthy difference between methods 1 and 2 is that if the model's specification is correct, method 1 will yield more accurate estimates of the parameters because it uses a larger sample and thus has a smaller sampling error.

Econometric Models Are Approximations

When I began work in econometrics, I believed a premise that underlies much econometric work—namely, that a true model that governs the behavior of the economy actually exists, with both systematic and random components and with true parameter values. And I believed that ultimately it would be possible to discover that true model and estimate its parameter values. My hope was first to find several models that could tentatively be accepted as ideal and eventually to find more general models that would include particular ideal models as special cases. (One way to top your colleagues is to show that their models are special cases of yours. Nowadays this is called "encompassing.")

Experience suggests that we cannot expect to find ideal models of the sort just described. When an estimated econometric model that describes past data is extrapolated into the future for more than a year or two, it typically does not hold up well. To try to understand how this might happen, let us temporarily adopt the premise that there is a true model. Of course, we do not know the form or parameters of this true model. They may or may not be changing, but if they are changing according to some rule, then in principle it is

possible to incorporate that rule into a more general unchanging true model.

Suppose that an economist has specified a model, which may or may not be the same as the true model. If the form and parameters of the economist's model are changing according to some rule (not necessarily the same as the rule governing the true model), again in principle it is possible to incorporate that rule into a more general unchanging model.

Now consider the following possible ways in which the economist's model might describe past data quite well but fail to describe future data:

1. The form and parameter values of the economist's model may be correct for both the past period and the future period, but as the forecast horizon is lengthened, the forecasts get worse because the variance of the forecast is an increasing function of the length of the horizon. This will be discussed later.
2. The form of the economist's model may be correct for both the past period and the future period, but some or all of the true parameters may change during the future period.
3. The form of the economist's model may be correct for the past period but not for the future period because of a change in the form of the true model that is not matched in the economist's model.
4. The form of the economist's model may be incorrect for both periods but more nearly correct for the past period.

The last possibility is the most likely of the four in view of the fact that the economy has millions of different goods and services produced and consumed by millions of individuals, each with distinct character traits, desires, knowledge and beliefs.

These considerations lead to the conjecture that the aforementioned premise underlying econometrics is wrong—that there is no unchanging true model with true parameter values that governs the behavior of the economy now and in the future. Instead, every estimated econometric model is at best an approximation of a changing economy—an approximation that becomes worse as it is applied to events that occur further into the future from the period in which the model was formulated. In this case we should not be surprised at our failure to find an ideal general model as defined earlier. Instead, we should be content with models that have at best only a temporary and approximate validity that deteriorates with time. We should sometimes also be content with models that describe only a restricted range of events—for example, events in a particular country, industry or population group.

Desiderata for an Econometric Model

If no ideal model exists, what characteristics can we realistically strive for in econometric models regarded as scientific hypotheses? The following set of desiderata are within reach:

1. The estimated model should provide a good description of some interesting set of past data. This means it should have small residuals relative to the variation of its variables—that is, high correlation coefficients. The standard errors of its parameter estimates should be small relative to those estimates, that is, its t-ratios should be large. If it is estimated for separate subsets of the available data, all those estimates should agree with each other. Finally, its residuals should appear random. (If the residuals appear to behave systematically, it is desirable to try to find variables to explain them.)
2. The model should be testable against data that were not used to estimate it and against data that were not available when it was specified.
3. The estimated model should be able to describe events occurring after it was formulated and estimated, at least for a few quarters or years.
4. The model should make sense in the light of our knowledge of the economy. This means in part that it should not generate negative values for variables that must be non-negative (such as interest rates) and that it should be consistent with theoretical propositions about the economy that we think are correct.
5. Other things equal, a simple model is preferable to a complex one.
6. Other things equal, a model that explains a wide variety of data is preferable to one that explains only a narrow range of data.
7. Other things equal, a model that incorporates other useful models as special cases is preferable to one that does not. (This is almost the same point as the previous one.)

In offering these desiderata, I assume that the purpose of a model is to state a hypothesis that describes an interesting set of available data and that may possibly describe new data as well. Of course, if the purpose is to test a theory that we are not sure about, the model should be constructed in such a way that estimates of its parameters will tell us something about the validity of that theory. The failure of such a model to satisfy these desiderata may tell us that the theory it embodies is false. This too is useful knowledge.

COMMENTS AND CRITICISMS ABOUT ECONOMETRIC TECHNIQUES

Theory vs. Empiricism

Two general approaches to formulating a model exist. One is to consult economic theory. The other is to look for regularities in the data. Either can be used as a starting point, but a combination of both is best. A model derived from elegant economic theory may be appealing, but unless at least some of its components or implications are consistent with real data, it is not a reliable hypothesis. A model obtained by pure data mining may be consistent with the body of data that was mined to get it, but it is not a reliable hypothesis if it is not consistent with at least some other data (recall what was said about this earlier), and it will not be understood if no theory to explain it exists.

The VAR Approach

Vector autoregression (VAR) is one way of looking for regularities in data. In VAR, a set of observable variables is chosen, a maximum lag length is chosen, and the current value of each variable is regressed on the lagged values of that variable and all other variables. No exogenous variables exist; all observable variables are treated as endogenous. Except for that, a VAR model is similar to the unrestricted reduced form of a conventional econometric model. Each equation contains only one current endogenous variable, each equation is just identified, and no use is made of any possible theoretical information about possible simultaneous structural equations that might contain more than one current endogenous variable. In fact, no use is made of any theoretical information at all, except in the choice of the list of variables to be included and the length of the lags. In macroeconomics it is not practical to use many variables and lags in a VAR because the number of coefficients to be estimated in each equation is the product of the number of variables times the number of lags and because one cannot estimate an equation that has more coefficients than there are observations in the sample.

The ARIMA Approach

The Box-Jenkins type of time-series analysis is another way to seek regularities in data. Here each observable variable is expressed in terms of purely random disturbances. This can be done with one variable at a time or in a multivariate fashion. In the univariate case an expression involving current and lagged values of an observable variable is equated to an expression involving current and lagged values of an unobservable white-noise disturbance; that is, a serially independent random disturbance that has a mean of zero and constant variance. Such a formulation is called an autoregressive integrated moving average (ARIMA) process. The *autoregressive* part expresses the current value of the variable as a function of its lagged values. The *integrated* part refers to the possibility that the first (or higher-order) differences of the variable, rather than its levels, may be governed by the equation. Then the variable's levels can be obtained from its differences by undoing the differencing operation—that is, by integrating first differences once, integrating second differences twice, and so on. (If no integration is involved, the process is called ARMA instead of ARIMA.) The *moving average* part expresses the equation's disturbance as a moving average of current and lagged values of a white-noise disturbance. To express a variable in ARIMA form, it is necessary to choose three integers to characterize the process. One gives the order of the autoregression (that is, the number of lags to be included for the observable variable); one gives the order of the moving average (that is, the number of lags included for the white-noise disturbance); and one gives the order of integration (that is, the number of times the highest-order differences of the observable variable must be integrated to obtain its levels). The choice of the three integers (some of which may be zero) is made by examining the time series of data for the observable variable to see what choice best conforms to the data. After that choice has been made, the coefficients in the autoregression and moving average are estimated. The multivariate form of ARIMA modeling is a generalization of the

univariate form. And, of course, VAR modeling is a special case of multivariate ARIMA modeling.

VAR and ARIMA models can be useful if they lead to the discovery of regularities in the data. If enduring regularities in the data are discovered, we have something interesting to try to understand and explain. In my view, however, one disadvantage of both approaches is that they make almost no use of any knowledge of the subject matter being dealt with. To use univariate ARIMA on an economic variable, one need know nothing about economics. I think of univariate ARIMA as mindless data mining. To use multivariate ARIMA, one need only make a list of variables to be included and choose the required three integers. To use VAR, one need only make a list of the variables to be included and choose a maximum lag length. Knowledge of the subject the equations deal with can enter into the choice of variables to be included.

It may seem that the ARIMA approach and the conventional econometric model approach are antithetical and inconsistent with each other. Zellner and Palm (1974), however, have pointed out that if a conventional model's exogenous variables are generated by an ARIMA process, the model's endogenous variables are generated the same way.

General-to-Specific Modeling

General-to-specific modeling starts with an estimated equation that contains many variables and many lagged values of each. Its approach is to pare this general form down to a more specific form by omitting lags and variables that do not contribute to the explanatory power of the equation. Much can be said for this technique, but of course it will not lead to a correct result if the general form one starts with does not contain the variables and the lags that belong in an equation that is approximately correct.

The Error Correction Mechanism

The error correction mechanism (ECM) provides a way of expressing the rate at which a variable moves toward its desired or equilibrium value when it is away from that value. Economic theory is at its best when deriving desired or equilibrium values of variables, either static positions or dynamic paths. ECM has so far not been good at deriving the path followed by an economy that is out of equilibrium. Error correction models are appealing because they permit the nature of the equilibrium to be specified with the aid of theory but permit the adjustment path to be determined largely by data.

Testing Residuals for Randomness

I have already discussed testing residuals for randomness. If an equation's residuals appear to follow any regular or systematic pattern, this is a signal that there may be some regular or systematic factor that has not been captured by the form and variables chosen for the equation. In such a case it is desirable to try to modify the equation's specification, either by including additional variables, by changing the form of the equation, or both, until the residuals lose their regular or systematic character and appear to be random.

Stationarity

It is often said that the residual of a properly specified equation should be stationary, that is, that its mean, variance and autocovariances should be constant through time. However, for an equation whose variables are growing over time, such as an aggregate consumption or money-demand equation, it would be unreasonable to expect the variance of the residual to be constant. That would mean that the correlation coefficients for the equation in successive decades (or other time intervals) would approach one. It would be more reasonable to expect the standard deviation of the residual to grow roughly in proportion to the dependent variable, to one of the independent variables, or to some combination of them.

The Lucas Critique

Robert Lucas (1976) warned that when an estimated econometric model is used to predict the effects of changes in government policy variables, the estimated coefficients may turn out wrong and hence the predictions may also turn out wrong. Under what conditions can this be expected to occur? Lucas says that this occurs when policymakers follow one policy rule during the estimation period and begin to follow a different policy rule during the prediction period. The reason for this, he argues, is that in many cases the parameters that were estimated are not constants that represent invariant economic relationships, but instead are variables that change in response to changes in policy rules. This is because they depend both on constant parameters and on varying expectations that private agents formulate by observing policymakers and trying to discover what policy rule is being followed. Jacob Marschak (1953) foreshadowed this idea when he cautioned that

predictions made from an estimated econometric model will not be valid if the structure of the model (that is, its mathematical form and its parameter values) changes between the estimation period and the prediction period. Therefore, to make successful predictions after a structural change, one must discover the nature of the structural change and allow for it.

I take this warning seriously. It need not concern us when policy variations whose effects we want to predict are similar to variations that occurred during the estimation period. But when a change in the policy rule occurs, private agents will eventually discover that their previous expectation formation process is no longer valid and will adopt a new one as quickly as they can. As they do so, some of the estimated parameters will change and make the previously obtained estimates unreliable.

Goodhart's Law

Lucas' warning is related to Goodhart's Law, which states that as soon as policymakers begin to act as if some previously observed relationship is reliable, it will no longer be reliable and will change.[3] A striking example is the short-run, downward-sloping Phillips curve.

Are Policy Variables Exogenous?

Most econometric models treat at least some policy variables as exogenous. But public policy responds to events. Policy variables are not exogenous. The field of public choice studies the actions of policymakers, treating them as maximizers of their own utility subject to the constraints they face. Econometric model builders have so far not made much use of public choice economics.

BY THEIR FORECASTS YE SHALL KNOW THEM (MODELS, THAT IS)

Methods of Evaluating Models' Forecasts

A conventional econometric model contains disturbances and endogenous and exogenous variables. Typically some of the endogenous variables appear with a lag. Consider an annual model with data for all variables up to and including 1992.

Suppose that at the end of 1992 we wish to forecast the endogenous variables for 1993, one year ahead. This is an *ex ante* forecast. For this we need estimates of the model's parameters, which can be computed from our available data. In addition, we need 1993 values for the lagged endogenous variables. These we already have because we have values for the years 1992 and earlier. Further, we need predicted 1993 values for the disturbances. We usually use zeros here because disturbances are assumed to be serially independent with zero means. (Some modelers, however, would use values related to the residuals for 1992 and possibly earlier years if the disturbances were thought to be serially correlated.) Finally, we need predicted 1993 values for the exogenous variables. These predictions must be obtained from some source outside the model.

Our predictions of the endogenous variables for 1993 will be conditional on our estimated model and on our predictions of the disturbances and exogenous variables. If we make errors in forecasting the endogenous variables, it may be because our estimated model is wrong, because our predictions of the disturbances or exogenous variables are wrong, or because of some combination of these.

It is possible—and desirable—to test the forecasting ability of an estimated model independently of the model user's ability to forecast exogenous variables. This is done with an *ex post* forecast. An *ex post* forecast for one period ahead, say for 1993, is made as follows: Wait until actual 1993 data for the exogenous variables are available, use them instead of predicted values of the exogenous variables to compute forecasts of the 1993 endogenous variables, and examine the errors of those forecasts.

When comparing forecasts from different models, bear in mind that the models may differ in their lists of exogenous variables and that this may affect the comparison. For example, a model that has hard-to-forecast exogenous variables is not going to be helpful for practical *ex ante* forecasting, even if it makes excellent *ex post* forecasts.

Errors of *ex ante* and *ex post* forecasts tell us different things. *Ex ante* forecasting errors tell us about the quality of true forecasts but do not allow us to separate the effects of incorrect estimated models from the effects of bad predictions of exogenous variables and disturbances. *Ex post* forecasting errors tell us how good an estimated model has been as a scientific hypothesis, which is

[3]See Goodhart (1981).

Table 1

Root Mean Square Percentage Errors of *Ex Post* Forecasts with No Subjective Adjustments of the Forecasts, from about 1965 to 1973, Averaged over Eight Models

	Horizon		
Variable	**1 quarter**	**4 quarters**	**8 quarters**
Nominal GNP	0.7	2.0	4.5
Real GNP	0.7	1.9	2.5
GNP Deflator	0.4	0.6	1.9

distinct from anyone's ability to forecast exogenous variables and disturbances. If you are interested in the quality of practical forecasting, you should evaluate *ex ante* forecasts. If you are interested in the quality of a model as a scientific theory, you should evaluate *ex post* forecasts. *Ex post* forecasts are usually more accurate than *ex ante* forecasts because the predictions of the exogenous variables that go into *ex ante* forecasts are usually at least somewhat wrong.

What if we want to make forecasts *two* years ahead, for 1994, based on data up to and including 1992? We need 1993 values for the endogenous variables to use as lagged endogenous values for our 1994 forecast; however, we do not have actual 1993 data. Hence we must make a one-year-ahead forecast for 1993 as before. Then we can make our 1994 forecast using our 1993 forecasts as the lagged values of the endogenous variables for 1994. Thus the errors of our 1994 forecast will depend partly on the errors of our 1993 forecast and partly on the values we use for the 1994 exogenous variables and disturbances. If we want to make forecasts for n years ahead instead of two years ahead, the situation is similar except that n steps are required instead of two. We can still consider either *ex ante* or *ex post* forecasts. As before, *ex post* forecasts use actual values of the exogenous variables.

When making *ex ante* forecasts, the typical econometric forecaster does not automatically adopt the forecasts generated by a model. Instead the forecaster compares these forecasts with his subjective judgment about the future of the economy, and if there are substantial discrepancies, he makes subjective adjustments to his model's forecasts. This is usually done with subjective adjustments to the predicted disturbances. Thus the accuracy of *ex ante* forecasts typically depends not only on the adequacy of the estimated model, but also on the model builder's ability to forecast exogenous variables and to make subjective adjustments to the model's forecasts. Paul Samuelson once caricatured this situation at a meeting some years ago by likening the process that produces *ex ante* econometric forecasts to a black box inside which we find only Lawrence R. Klein!

Errors of Forecasts from Several Econometric Models

Most presentations of forecasting accuracy are based on *ex ante* rather than *ex post* forecasts, often with subjective adjustments, perhaps because of the interest in practical forecasting. I like to look at *ex post* forecast errors without adjustments because I am interested in econometric models as scientific hypotheses.

Fromm and Klein (1976) and Christ (1975) discuss root mean square errors (RMSEs) of *ex post* quarterly forecasts of real GNP, nominal GNP and the GNP deflator one quarter to eight quarters ahead by eight models with *no subjective adjustment* by the forecaster. The models were formulated by Brookings, the U.S. Bureau of Economic Analysis, Ray Fair, Leonall Andersen of the Federal Reserve Bank of St. Louis, T. C. Liu and others, the University of Michigan and the Wharton School (two versions). For GNP they show RMSEs rising from 0.7 percent to 2.5 or 4.5 percent of the actual value as the horizon increases from one quarter to eight quarters. For the GNP deflator they show RMSEs rising from 0.4 percent to 1.9 percent, as shown in table 1.

In a series of papers over the past several years, Stephen McNees (1986, 1988 and 1990) has reported on the accuracy of *subjectively*

adjusted ex ante quarterly forecasts of several macroeconometric models, for horizons of one to eight quarters ahead, and has compared them with two simple mechanical forecasting methods. One is the univariate ARIMA method of Charles Nelson (1984), which is called BMARK (for benchmark). The other is the Bayesian vector autoregression method of Robert Litterman (1986), which is called BVAR. The models discussed in McNees (1988) are those formulated by the U.S. Bureau of Economic Analysis, Chase Econometrics, Data Resources Inc., Georgia State University, Kent Institute, the University of Michigan, UCLA and Wharton.

McNees' results for *quarterly* forecasts may be summarized in the following five statements:

1. The models' forecast errors were usually smaller than those of BMARK.[4]

2. The models' forecast errors were usually slightly smaller than those of BVAR for nominal GNP and most other variables and slightly larger than those of BVAR for real GNP. Thus BVAR was usually better than BMARK for real GNP.[5]

3. Forecast errors for the *levels* of variables became worse as the forecast horizon lengthened from one quarter to eight quarters, roughly quadrupling for most variables and increasing tenfold for prices. However, forecast errors for the *growth rates* of many variables (but not for price variables) improved as the horizon lengthened. In other words, for many variables, the forecasts for growth rates averaged over several quarters were better than the forecasts for short-term fluctuations.[6]

4. Mean absolute errors (MAEs) of the models' forecasts of the level of nominal GNP were usually about 0.8 percent of the true level for forecasts one quarter ahead and increased gradually to about 2.2 percent for forecasts one year ahead and about 4 percent for forecasts two years ahead. Real GNP forecast errors were somewhat smaller. Errors for other variables were comparable. Price-level forecast errors were smaller for the one-quarter horizon but grew faster and were larger for the two-year horizon.[7]

5. When subjectively adjusted forecasts were compared with unadjusted forecasts, the adjustments were helpful in most cases, though sometimes they made the forecast worse. Usually the adjustments were larger than optimal.[8]

One-year-ahead *annual* forecasts of real GNP by the University of Michigan's Research Center in Quantitative Economics, by the Council of Economic Advisers and by private forecasters covered by the ASA/NBER survey all had MAEs of about 0.9 percent to 1.1 percent of the true level, and RMSEs of about 1.2 percent to 1.5 percent of the true level.[9] (The relative sizes of the MAEs and RMSEs are roughly consistent with the fact that for a normal distribution, the RMSE is about 1.25 times the MAE.)

Implications of Worsening Ex Post *Forecast Errors*

Because the root mean square error of an econometric model's *ex post* forecasts roughly quadruples when the horizon increases from one quarter to eight quarters as in table 1, can we conclude that the model is no longer correct for the forecast period? The answer is possibly, but not certainly.

For a static model we could conclude this because the error of each forecast would involve disturbances only for the period being forecast, not for periods in the earlier part of the horizon. Hence there is no reason to expect great changes in the size of the forecasting error for a static model as the horizon increases. Small increases will occur because of errors in the estimates of the model's parameters if the values of the model's independent variables move further away from their estimation-period means as the horizon lengthens. This is because any errors in the estimates of equations' slopes will generate larger effects as the distance over which the slopes are projected increases.

But most econometric forecasting models contain lagged endogenous variables. Therefore, as noted previously, to forecast n periods ahead, we must first forecast the lagged endogenous-variable values that are needed for the n-periods-

[4]See McNees (1988 and 1990).

[5]See McNees (1990).

[6]See McNees (1988).

[7]See McNees (1988).

[8]See McNees (1990).

[9]See McNees (1988).

ahead forecast. This involves a chain of n steps. The first step is a forecast one period ahead, whose error involves disturbances only from the first period in the n-period horizon. The second step is a forecast two periods ahead, whose error involves disturbances from the second period in the horizon and also disturbances from the first period because they affect the one-period-ahead forecast, which in turn affects the two-periods-ahead forecast. And so on, until the nth step, whose forecast error involves disturbances from all periods in the horizon from one through n. Thus, for a dynamic model, the variance of a forecast n periods ahead will depend on the variances and covariances of disturbances in all n periods of the horizon, and except in very special circumstances, it will increase as the horizon increases.

To decide whether the evidence in table 1 shows that the estimated models it describes are incorrect for the forecast horizon of eight quarters, we need to know whether the RMSEs of a *correct* model would quadruple as the forecast horizon increases from one quarter to eight quarters. If they would, then the quadrupling observed in the table is not evidence of incorrectness of the estimated models. If they would not, then evidence of incorrectness exists. We do not have enough information about the models underlying the table to settle this issue definitively, but some simple examples will illustrate the principle involved.

Suppose the model is linear and perfectly correct, and suppose it contains lags of one quarter or more (as most models do). Then the variance of the error of an n-periods-ahead forecast will be a linear combination of the variances and covariances of the disturbances in all periods of the horizon. In the simple case of a single-equation model, if the disturbances are serially independent and if the coefficients in the linear combination of disturbances are all equal to one, the variance of the linear combination of disturbances for a horizon of eight quarters will be eight times that of one quarter. So the RMSE of *ex post* forecast errors from a correct model will increase by a factor of the square root of eight (about 2.8) as the horizon goes from one quarter to eight quarters. If the coefficients in the linear combination are less than one, as in the case of a stable model with only one-period lags, the variance of the linear combination for eight quarters will be less than eight times that for one quarter. Hence the RMSE of *ex post* forecast errors from a correct model will increase by less than a factor of the square root of eight as the horizon goes from one quarter to eight quarters. In such a case, if the observed RMSEs approximately quadrupled, it would cast some doubt on the validity of the model.

Consider a single-equation model with a single lag, and no exogenous variables as follows:

$$y_t = \alpha + \beta y_{t-1} + \varepsilon_t$$

where ε is a serially independent disturbance with zero mean and constant variance σ^2. Suppose that the values of α and β are known and thus no forecast error is attributable to incorrect estimates of these coefficients. Then the variance of the error of a one-period-ahead forecast is σ^2, that of a two-periods-ahead forecast is $(1 + \beta^2)\,\sigma^2$, that of a three-periods-ahead forecast is $(1 + \beta^2 + \beta^4)\sigma^2$, and so on. The variance of an n-periods-ahead forecast is $\sum_0^{N-1} \beta^{2i}\,\sigma^2$, which is equal to $(1 - \beta^{2N})\,\sigma^2/(1 - \beta^2)$.

Table 2 shows how the standard deviation of such a forecast error increases as the horizon increases from one quarter to eight quarters for several values of the parameter β. Table 2 suggests that if the RMSE of a model's forecasts quadruples as the horizon increases from one quarter to eight quarters, either β (the rate of approach of the model to equilibrium) must be large or close to one, or the model is inadequate as a description of the forecast period.

Corresponding expressions can be derived for multi-equation models with many lags and serially correlated disturbances, but they are rather cumbersome.

AN OLD, PLAIN-VANILLA EQUATION THAT STILL WORKS, ROUGHLY

Nearly 40 years ago Henry Allen Latané published a short paper in which he reported that for 1919–52 the inverse of the GNP velocity of M1 is described by a simple least squares regression on the inverse of a long-term, high-grade bond rate RL as follows:[10]

(1) $M1/GNP = .100 + .795/RL$, $\bar{r}^2 = .75$
(t-ratio) (10)

[10]See Latané (1954).

Table 2

Standard Deviation of Error of N-Periods-Ahead Forecast from the Equation $y_t = \alpha + \beta y_{t-1} + \varepsilon_t$ Relative to the Standard Deviation of ε, as a Function of N and β, when α and β Are Known

		Horizon, N				
β	β^2	1	2	4	8	∞
0.7070	0.50	1.00	1.22	1.37	1.41	1.41
0.8944	0.80	1.00	1.34	1.72	2.04	2.24
0.9486	0.90	1.00	1.38	1.85	2.39	3.16
1.0000	1.00	1.00	1.41	2.00	2.83	∞

Here and in what follows, I have expressed interest rates in units of percent per year, so a 5 percent rate is entered as 5, not as 0.05, and its inverse 0.20, not 20. The Appendix gives the definitions and data sources for variables in this and subsequent equations. Latané showed the unadjusted correlation coefficient r, but showed neither the standard deviation nor the t-ratio of the slope. I calculated the adjusted $\bar{r}^2$ and the t-ratio. The latter is the square root of $r^2 (df)/(1-r^2)$, where df, the number of degrees of freedom, equals 32.

This specification has some of the properties of a theoretical money demand equation—namely, a positive income elasticity (restricted to be constant and equal to one by construction) and a negative interest elasticity (restricted to have an absolute value less than one and not constant). But its least-squares estimate would almost certainly be biased or inconsistent, even if the form of the equation were correct, because the bond rate is almost certainly not exogenous and hence not independent of the equation's disturbances.

Nevertheless, this specification has continued to work fairly well for other periods. Nearly 30 years ago M1/GNP was described for 1892–1959 by a similar regression on the inverse of Moody's Aaa bond rate with almost the same coefficients, as follows:[11]

(2) M1/GNP = .131 + .716/RAaa, $\bar{r}^2$ = .76
(t-ratio) (14)

For 1959–91 the same specification describes the ratio of M1 to GNP with almost the same coefficients, as follows:

(3) M1/GNP = .085 + .774/RAaa, $\bar{r}^2$ = .90
(t-ratio) (13) (17)

If GNP in equation (3) is replaced by the new output variable GDP for 1959–91, the result is almost identical, as follows:

(4) M1/GDP = .086 + .771/RAaa, $\bar{r}^2$ = .91
(t-ratio) (13) (18)

David Dickey's discussion is based on the 1959–91 data that underlie equation (3).

For 1892–1991 a similar result is again obtained, as follows:

(5) M1/GNP = .083 + .874/RAaa, $\bar{r}^2$ = .89
(t-ratio) (11) (28)

Table 3 shows the estimated equations (1)–(5) and several other estimated equations that will be described soon. Equations (1′) and (2′) are attempts to duplicate the results in equations (1) and (2) using the same data base that is used in equations (3), (5) and later equations. The Appendix gives data sources.

Figure 2 shows the graphs of M1/GNP and 1/RAaa over time. Figures 3 and 4 show the scatter diagrams for equations (3) and (5), respectively. (I should add that, of the four

[11]See Christ (1963).

Table 3

Regressions of M1/GNP or M1/GDP on 1/RAaa and Other Variables* (t-ratios are in parentheses)

		Estimated Coefficient (and t-ratio)					
Eq	Sample	Constant	1/RAaa	$(M1/GNP)_{-1}$	$(1/RAaa)_{-1}$	$\bar{R}^2$	DW
PLAIN-VANILLA EQUATIONS							
1	1919–1952	.100	.795(10)			.75	
1′	1919–1952	.136(7)	.713(10)			.75	.56
2	1892–1959	.131	.716(14)			.76	
2′	1892–1959	.132(9)	.712(14)			.73	.62
3	1959–1991	.085(13)	.774(17)			.90	.38
4	1959–1991	.086(13)	.771(18)			.91	.36
5	1892–1991	.083(11)	.874(28)			.89	.48
AR(1) EQUATIONS**							
15	1960–1991	.013	.267(2.8)	.896(26)	−.239	.98	1.82
17	1893–1991	.020	.711(7)	.831(12)	−.591	.95	1.60
ECM EQUATIONS**							
18	1960–1991	.016(2.2)	.275(2.8)	.857(11)	−.212(−2.2)	.98	1.78
19	1893–1991	.016(2.1)	.593(5)	.807(12)	−.428(−3.6)	.95	1.59

*The dependent variable is M1/GNP in all equations except equation (4), where it is M1/GDP. Definitions and data sources for the variables M1, GNP, GDP and RAaa are given in the Appendix. As explained in the Appendix, a uniform set of data for M1, GNP and RAaa was used for equations (1′), (2′), (3), (5) and later equations; slightly different data were used for equations (1), (2) and (4). David Dickey's discussion is based on equation (3) and the data underlying it. Equations (1′) and (2′) are attempts to reproduce equations (1) and (2), respectively, using the same data that were used for equations (3), (5) and later equations.

**AR(1) means "first-order autoregressive." ECM means "error correction mechanism."

equations that can be obtained by regressing either the velocity of M1 or its inverse on either RAaa or its inverse, the form that is presented here fits the best.)

It is rather remarkable that this plain-vanilla specification continues to describe the relation between M1's velocity and the long-term Aaa bond rate with such similar regression and correlation coefficients for the four periods, especially in view of the changes in interest-rate regulation and in the definition of M1 that have occurred over the last century. However, the differences among the four estimated versions are not negligible, as seen in a comparison of the computed values of M1/GNP that they yield. For 1959–91 these computed values are shown in figure 5 together with the actual values of M1/GNP. Note that those computed from equations (1) and (2) using 1919–52 and 1892–1959 data are *ex post* forecasts, whereas those from equations (3) and (5) using 1959–91 and 1892–1991 data are within-sample calculated values. Figure 6 shows the values of M1/GNP obtained when equation (3) based on 1959–91 data is used to backcast M1/GNP for 1892–1958, and it also shows the actual values and the calculated values from equation (5) using 1892–1991 data. The forecasting and backcasting errors are by no means negligible, but the general pattern of behavior of M1/GNP is reproduced.

The estimates of the plain-vanilla equation are rather stable across time, as indicated by figures 7 and 8 which show the behavior of the slope as the sample period is gradually lengthened by adding one year at a time. In figure 7 the sample period starts with 1959–63 and is extended a year at a time to 1959–91. In figure 8 the sample period starts with 1892–97 and is gradually extended to 1892–1991. In each figure the slope settles down quickly after jumping around at first and varies little as the sample is extended thereafter.

However, this simple specification does not by any means satisfy all of the desiderata listed previously. In particular, the 1959–91 Durbin-Watson statistic is a minuscule 0.38, and the 1892–1991 Durbin-Watson statistic of 0.48, is not much better, which suggests that the residuals have a strong positive serial correlation. This by itself would not create bias in the estimates if

Figure 2
M1/GNP and 1/RAaa, 1892-91

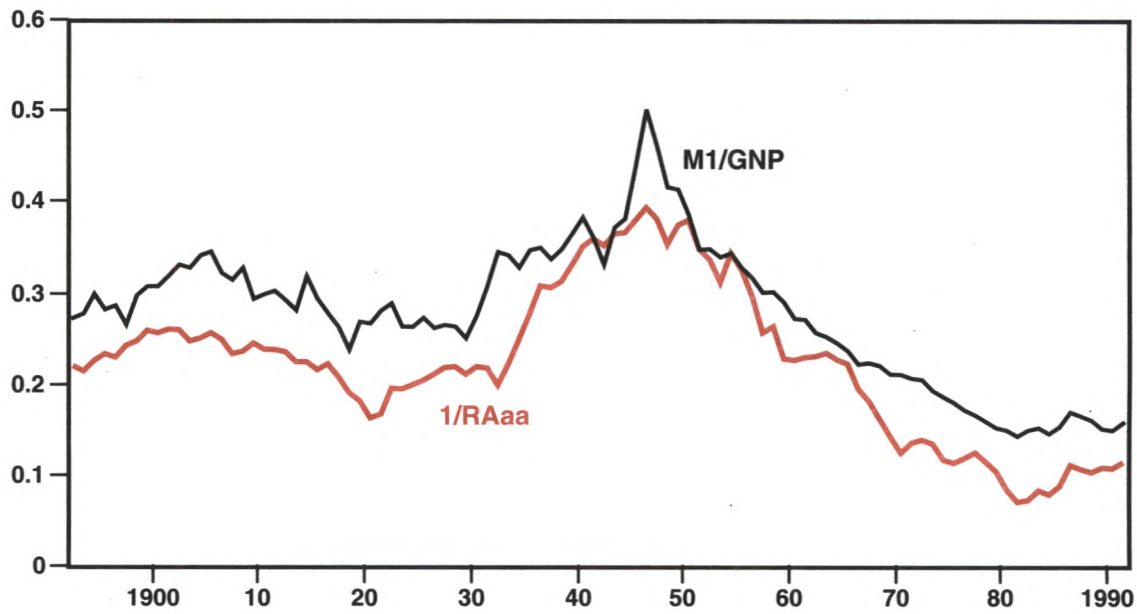


Figure 3
Regression of M1/GNP on 1/RAaa, 1959-91

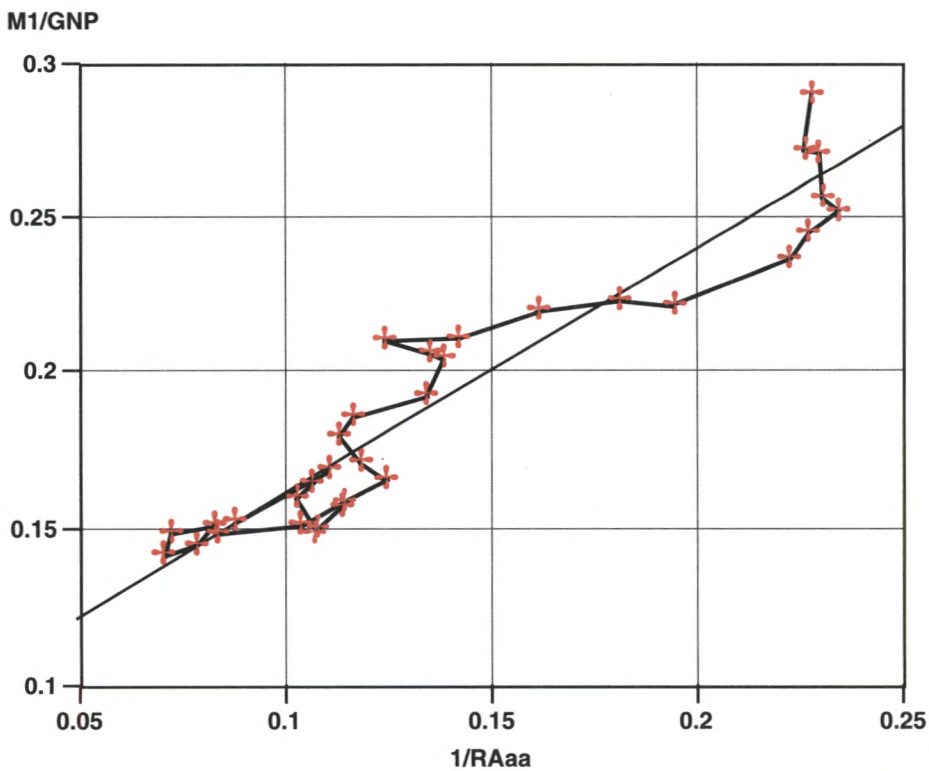


Figure 4
Regression of M1/GNP on 1/RAaa, 1892-91

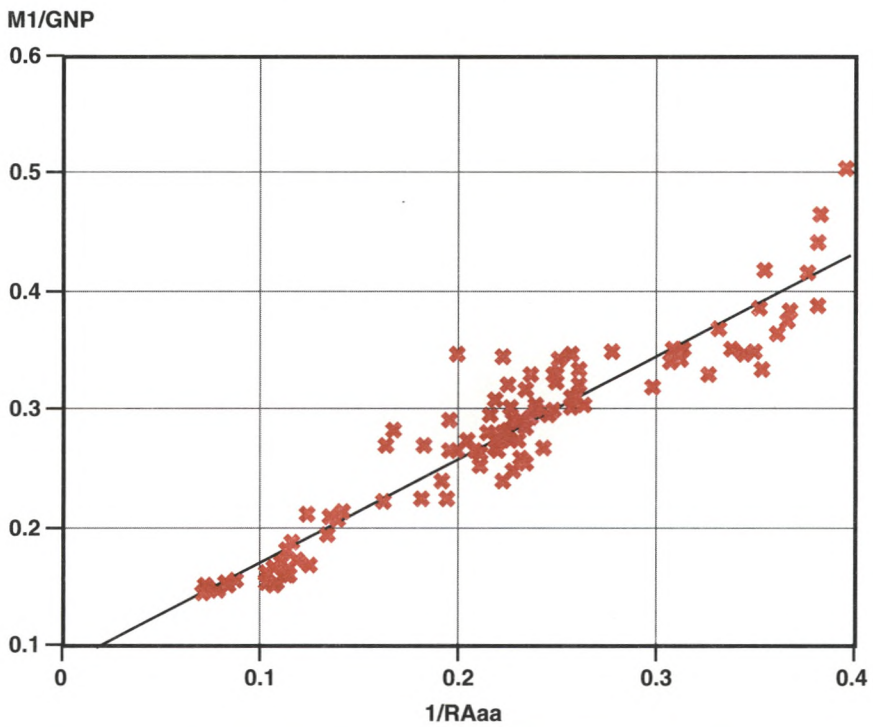


Figure 5
Actual, Computed and Forecast Values of M1/GNP from Regressions on 1/RAaa for Four Periods

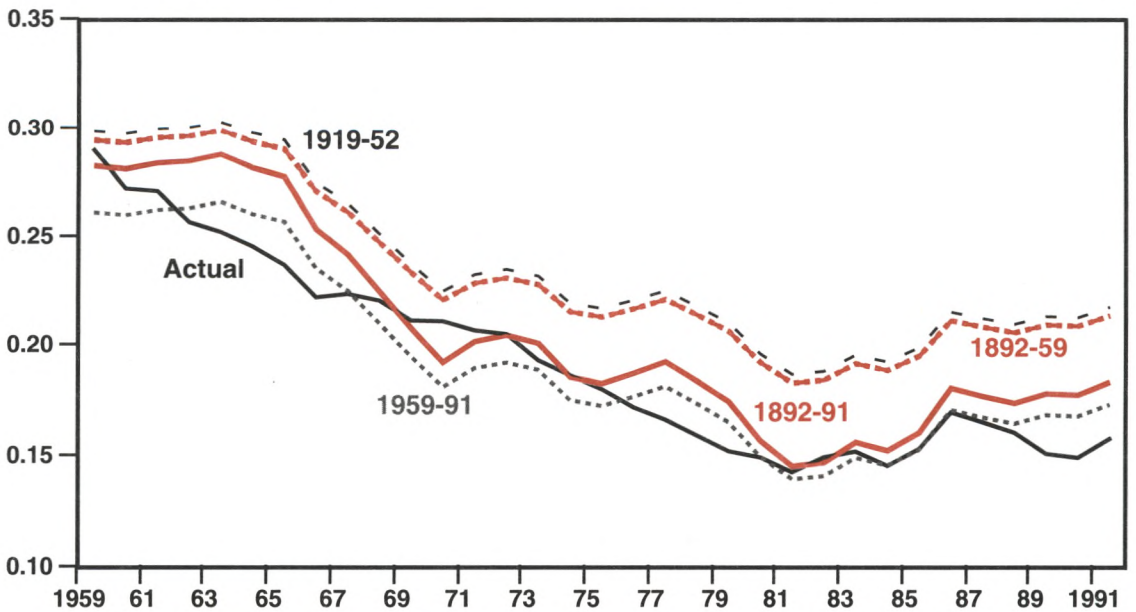


Figure 6

Actual, Computed and Backcast Values of M1/GNP from Regressions on 1/RAaa for Two Periods

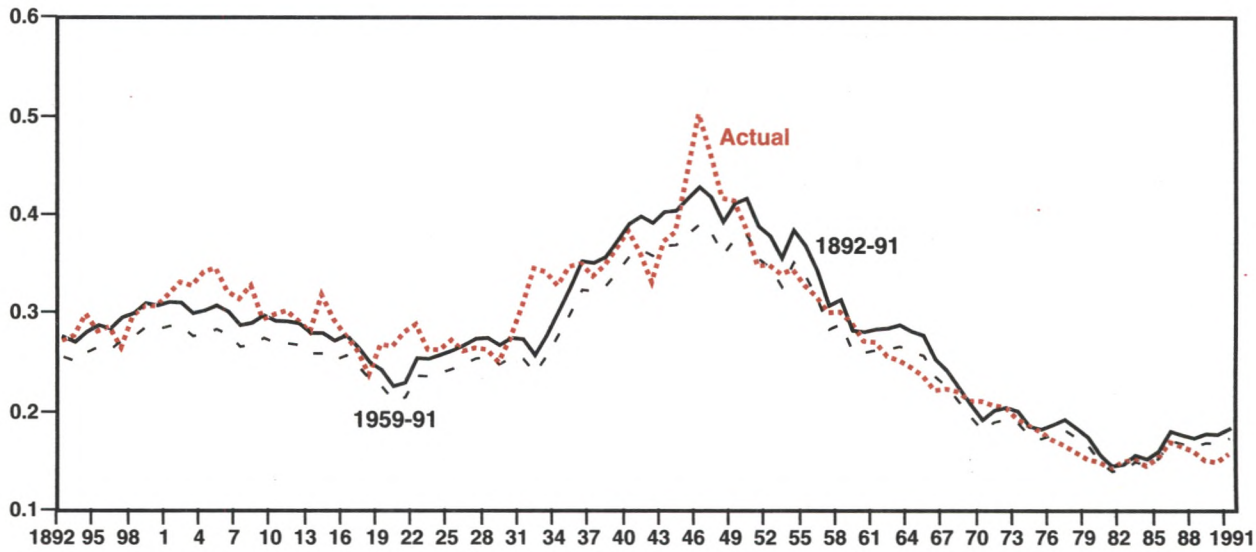


Figure 7

Estimates of Slope in Regression of M1/GNP on 1/RAaa for Samples Starting in 1959 and Ending in 1963...1991

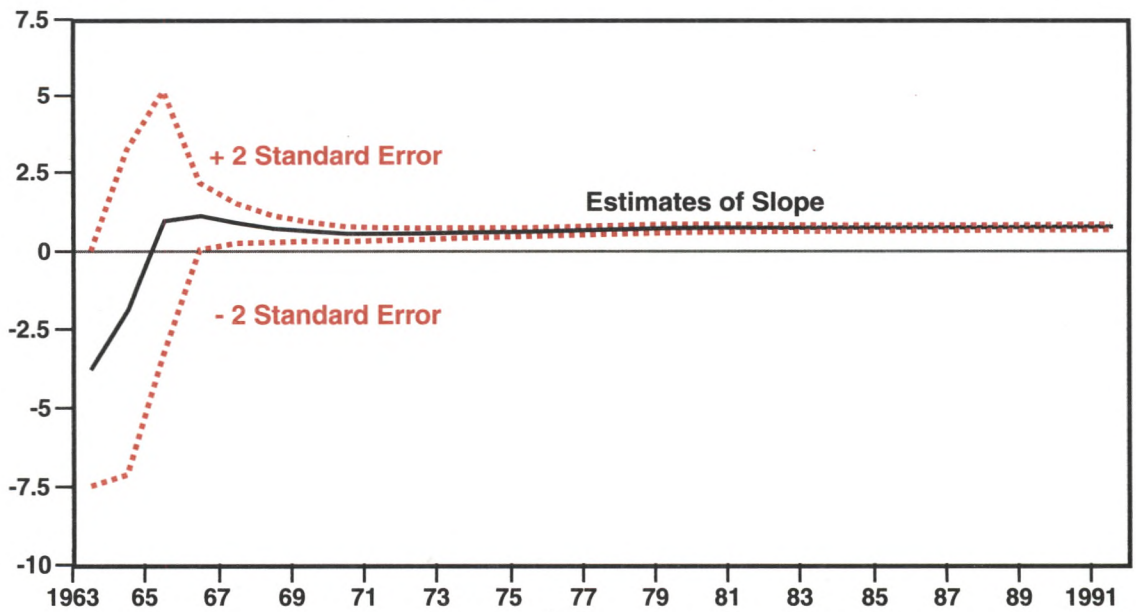
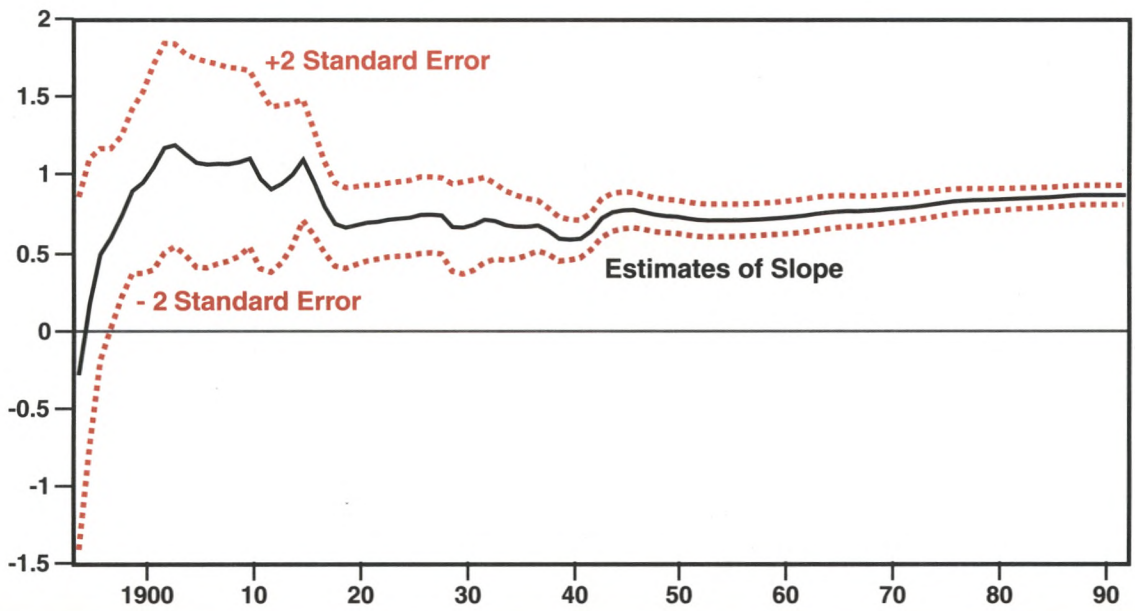


Figure 8

Estimates of Slope in Regression of M1/GNP on 1/RAaa for Samples Starting in 1892 and Ending in 1897...1991



the equation form were correct and if the disturbance were independent of the interest rate and had zero mean and constant variance. But it certainly suggests strongly that the equation has not captured all its relevant systematic factors. The graph of the residuals of the 1959–91 equation (3) against time is illuminating. It shows an almost perfect 12-year cycle of diminishing amplitude with peaks (positive residuals) in 1959 (or possibly earlier), 1970 and 1982 and troughs (negative residuals) in 1965, 1977 and 1990. It also suggests a negative time trend. The residuals of the 1892–1991 equation (5) show a roughly similar pattern. (See figures 9 and 10.)

The very low Durbin-Watson statistics suggest that the equation should be estimated either using the first differences of its variables, or better, using the levels of its variables with a first-order autoregressive [AR(1)] correction applied to its residuals. Estimation in levels with an AR(1) correction would be appropriate if the disturbance u in the original equation were equal to its own lagged value times a constant, ρ, plus a serially independent disturbance, ε, with constant variance, as follows:

$$(6)\quad u_t = \rho u_{t-1} + \varepsilon_t$$

In this case, if the original equation is

$$(7)\quad y_t = \alpha + \beta x_t + u_t = \alpha + \beta x_t + \rho u_{t-1} + \varepsilon_t,$$

the AR(1) correction subtracts ρ times the lagged version of equation (7) from equation (7) itself and produces the following equation:

$$(8)\quad y_t = \rho y_{t-1} + (1 - \rho)\,\alpha + \beta x_t - \beta\rho x_{t-1} + \varepsilon_t$$

This equation is nonlinear in the parameters because the coefficient of lagged x, $-\beta\rho$, is the negative of the product of the coefficients of x and lagged y. If that restriction is ignored and the coefficient of lagged x is denoted by γ, the equation becomes as follows:

$$(9)\quad y_t = \rho y_{t-1} + (1 - \rho)\,\alpha + \beta x_t + \gamma x_{t-1} + \varepsilon_t$$

This equation can be given the following error correction interpretation. Suppose that the equilibrium value y^* of a dependent variable y is linear in an independent variable x, as follows:

$$(10)\quad y_t^* = \alpha + \beta x_t$$

and that the change in y depends on both the change in the equilibrium value and an error

Figure 9

Residuals from Regression of M1/GNP on 1/RAaa, 1959-91

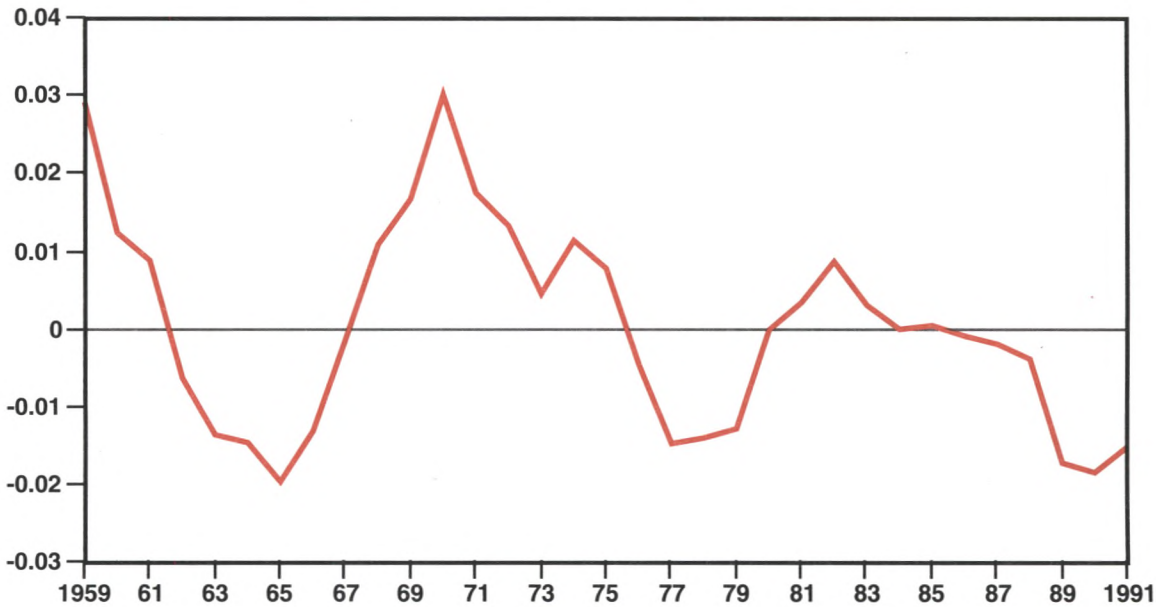
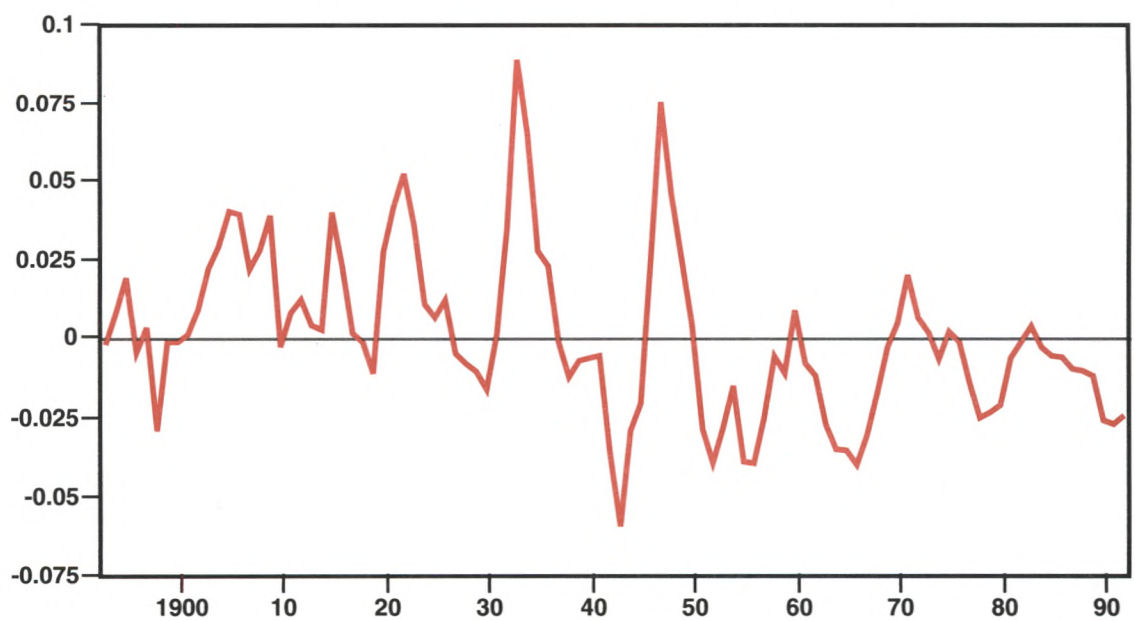


Figure 10

Residuals from Regression of M1/GNP on 1/RAaa, 1892-1991



correction term proportional to the gap between the lagged equilibrium and the lagged actual values, as follows:

$$(11)\ \Delta y_t = \theta \Delta y_t^* + (1 - \rho)(y_{t-1}^* - y_{t-1}) + \varepsilon_t$$

Substitution from equation (10) into equation (11) implies an equation with the same variables as the AR(1) equation (8) but with some different parameters, as follows:

$$(12)\ y_t = \rho y_{t-1} + (1 - \rho)\alpha + \theta\beta x_t + (1 - \rho - \theta)\beta x_{t-1} + \varepsilon_t$$

If the adjustment parameter θ in equation (12) were equal to one, then equation (12) would become the same equation as (8).

Estimates in first differences would be appropriate if the value of ρ in equation (6), (7) and (8) were one. In this case, equation (8) becomes a first-difference equation, as follows:

$$(13)\ \Delta y_t = \beta \Delta x_t + \varepsilon_t$$

The least-squares estimate of equation (8) in levels with the AR(1) correction for 1960–91 is as follows:

$$(14)\ \text{M1/GNP} - .896(\text{M1/GNP})_{-1} = (1 - .896).126 + .267(1/\text{RAaa} - .896/\text{RAaa}_{-1})$$
(t-ratio) (8) (2.8) (26)

with an adjusted R squared of .98 and DW equal to 1.82. This is equivalent to the following equation:

$$(15)\ \text{M1/GNP} = .896(\text{M1/GNP})_{-1} + .013 + .267/\text{RAaa} - .239/\text{RAaa}_{-1}$$

There is no evidence of a trend.

The least-squares estimate in levels with the AR(1) correction for 1893–1991 is as follows:

$$(16)\ \text{M1/GNP} - .831(\text{M1/GNP})_{-1} = (1 - .831).117 + .711(1/\text{RAaa} - .831/\text{RAaa}_{-1})$$
(t-ratio) (5) (7) (12)

with an adjusted R squared of .95 and DW equal to 1.60. This is equivalent to the following equation:

$$(17)\ \text{M1/GNP} = .831(\text{M1/GNP})_{-1} + .020 + .711/\text{RAaa} - .591/\text{RAaa}_{-1}$$

There is again no evidence of a trend.

Least-squares estimation of the ECM equation (12) for 1960–91 (without restricting θ to be one) yields the following equation:

$$(18)\ \text{M1/GNP} = .857(\text{M1/GNP})_{-1} + .016 + .275/\text{RAaa} - .212/\text{RAaa}_{-1}$$
(t-ratio) (11) (2.2) (2.8) (−2.2)

with an adjusted R squared of .98 and DW equal to 1.78. This is quite close to the AR(1) result in equation (15), which suggests that the adjustment coefficient θ in equation (12) is not very different from one. The hypothesis that in equation (18) the coefficient of lagged 1/RAaa is equal to the negative of the product of the coefficients of 1/RAaa and lagged M1/GNP, as required by equation (8) and as satisfied by equation (15), is strongly accepted by a Wald test (the p-value is .59).

Least-squares estimation of equation (12) for 1893–1991 (again without restricting θ to be one) yields the following equation:

$$(19)\ \text{M1/GNP} = .807(\text{M1/GNP})_{-1} + .016 + .593/\text{RAaa} - .428/\text{RAaa}_{-1}$$
(t-ratio) (12) (2.1) (5) (−3.6)

with an adjusted R squared of .95 and DW equal to 1.59. This is quite close to the AR(1) result in equation (17), which again suggests that the adjustment coefficient θ in equation (12) is not very different from one. The hypothesis that in equation (19) the coefficient of lagged 1/RAaa is equal to the negative of the product of the coefficients of 1/RAaa and lagged M1/GNP, as required by equation (8) and as satisfied by equation (17), is accepted by a Wald test (the p-value is .11).

Equations (15), (17), (18) and (19) are better than the plain-vanilla equations (3) and (5) in some respects, and worse in others. They have substantially higher adjusted R-squared values, much less serial correlation in their residuals, no evidence of a time trend, and significant coefficients. The ECM equations (18) and (19), however, are very unstable over time. In equation (18) the coefficient of 1/RAaa varies from about .6 for 1960–70, to .05 for 1960–78 and 1960–81, to .3 for 1960–86 and 1960–91. In equation (19) the coefficient of 1/RAaa varies almost as much but remains at about .7 or .6 for samples that include at least the years 1893–1950.

Table 4

Regressions of Δ(M1/GNP) on Δ(1/RAaa) without a Constant* (t-ratios are in parentheses)

Eq	Sample	Coef of Δ(1/RAaa)	$\bar{R}^2$	DW
24	1960–1991	.380(3.6)	.05	1.23
25	1893–1991	.494(4.1)	.15	1.76

*Definitions and data sources for the variables M1, GNP and RAaa are given in the appendix.

I conjecture that in the AR(1) equations (15) and (17) the coefficient of 1/RAaa is also unstable across time because the AR(1) and ECM equation estimates are quite similar.

By comparing equations (12) and (18), one can solve for the 1960–91 estimates of the four parameters ρ, α, β and θ, in that order, to obtain:

(20) $\hat{\rho} = .857$, $\hat{\alpha} = .112$, $\hat{\beta} = .441$ and $\hat{\theta} = .624$

This implies that the equilibrium relation in equation (10) embedded in the ECM is as follows:

(21) $(M1/GNP)^* = .112 + .441/RAaa$

Similarly, by comparing equations (12) and (19) one can solve for the 1893–1991 estimates of the four parameters as follows:

(22) $\hat{\rho} = .807$, $\hat{\alpha} = .083$, $\hat{\beta} = .855$ and $\hat{\theta} = .694$

This implies that the equilibrium relation in equation (10) embedded in the ECM is as follows:

(23) $(M1/GNP)^* = .083 + .855/RAaa$

The two equilibrium relations in equations (21) and (23) for the two periods 1960–91 and 1893–1991 are quite different, which is consistent with the instability of the ECM specification across time.

Now let us return to the first-difference equation (13). The least-squares estimate for 1960–91 is as follows:

(24) $\Delta(M1/GNP) = .380\Delta(1/RAaa)$, $\bar{r}^2 = .05$
(t-ratio) (3.6)

with DW = 1.23. For 1893–1991 it is as follows:

(25) $\Delta(M1/GNP) = .494\Delta(1/RAaa)$, $\bar{r}^2 = .15$
(t-ratio) (4.1)

with DW = 1.76. Table 4 shows the estimated equations (24) and (25). The estimates of this first-difference specification are not quite as stable across time as those of the specification in levels of the variables. This can be seen by comparing equations (24) and (25) and also from figures 11 and 12, which show the values of the estimates as the sample is increased one year at a time, starting respectively with 1960 and 1893. In each figure the estimates stabilize after an initial period of instability, but the values at which they settle differ by a factor of about .75.

If a constant term is included in equation (24), which implies a trend term in equation (3), the constant is small but significantly negative, the slope falls to about .3, and the adjusted R-squared and DW values improve slightly. The estimated slope, however, becomes wildly unstable across time. If a trend variable is included in equation (3), its coefficient is small but significantly negative, the interest-rate coefficient falls to .49 and remains highly significant, the adjusted R-squared and the DW values rise slightly, and again the estimated slope is wildly unstable across time.

If a constant term is included in equation (25), it is small and insignificantly negative, the rest of the equation is almost unchanged, and the slope becomes quite unstable through time, varying from .6 to zero and back to .6 again. If a trend is included in equation (5), its coefficient is small but significantly negative, the interest-rate coefficient is almost unchanged at .81, the adjusted R-squared value rises a bit, the DW value rises a bit, and the coefficient is again wildly unstable across time.

Figure 11
Estimates of Slope in Regression of Δ(M1/GNP) on Δ(1/RAaa) for Samples Starting in 1960 and Ending in 1962...1991

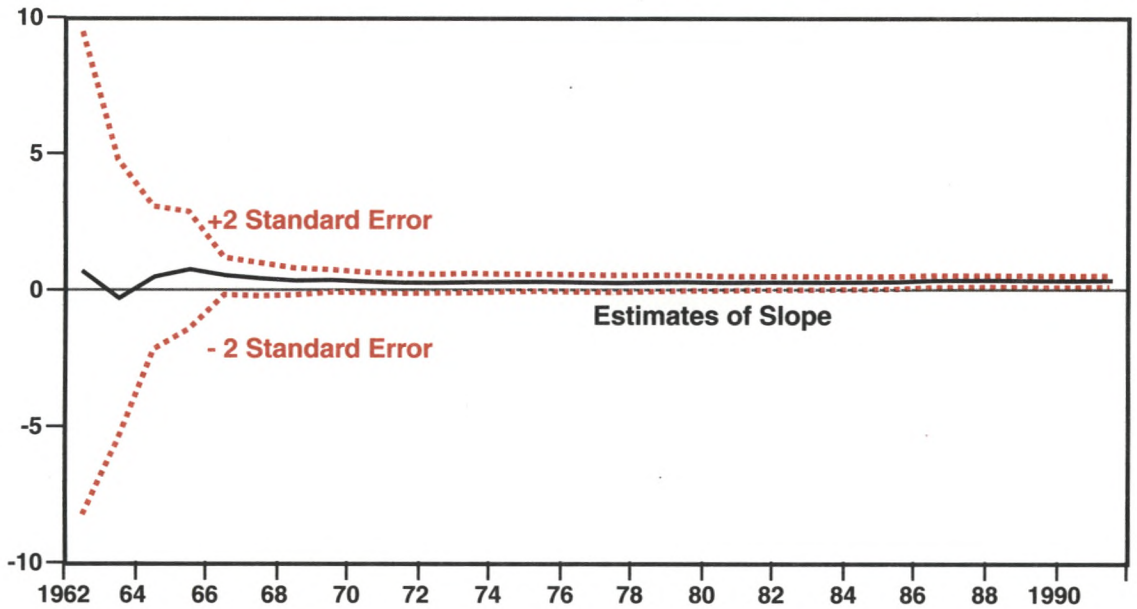
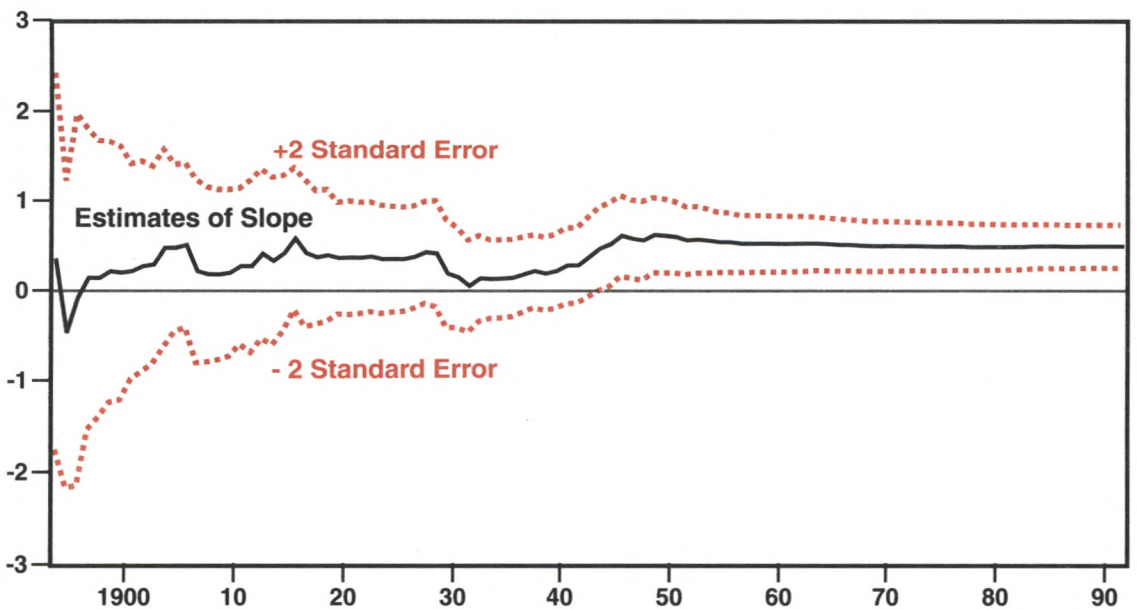


Figure 12
Estimates of Slope in Regression of Δ(M1/GNP) on Δ(1/RAaa) for Samples Starting in 1893 and Ending in 1896...1991



On the whole, the first-difference specification does not stand up well.

Where do matters stand? On the one hand, we have the plain-vanilla equation such as equation (3), which fits only moderately well and has severe serial correlation in its residuals but has an estimated slope that is rather stable across time. On the other hand, we have more complicated dynamic equations such as the ECM equation (18), which fit much better and have nice Durbin-Watson statistics but have estimated coefficients that vary greatly across time. Neither is quite satisfactory, but if the aim is to find an estimated equation that will describe the future as well as it does the past, I think I would now bet on the plain-vanilla specification, even though the relation of its estimated coefficients to structural parameters is unclear.

CONCLUSION

Econometrics has given us some results that appear to stand up well over time. The price and income elasticities of demand for farm products are less than one. The income elasticity of household demand for food is less than one. Houthakker (1957), in a paper commemorating the 100th anniversary of Engel's law, reports that for 17 countries and several different periods these income elasticities range between .43 and .73. Rapid inflation is associated with a high growth rate of the money stock. Some short-term macroeconometric forecasts, especially those of the Michigan model, are quite good.

But there have also been some nasty surprises about which econometrics gave us little or no warning in advance. The short-run downward-sloping Phillips curve met its demise in the 1970s. (Milton Friedman [1968] and Edmund Phelps [1968] predicted that it would.) The oil embargo of 1973 and its aftermath threw most models off. The slowdown of productivity growth beginning in the 1970s was unforeseen. The money demand equation, which appeared to fit well and be quite stable until the 1970s, has not fit so well since then.

How then should we approach econometrics, for science and for policy, in the future? As for science, we should formulate and estimate models as we usually do, relying both on economic theory and on ideas suggested by regularities observed in past data. But we should not fail to test those estimated models against new data that were not available to influence the process of formulating them. As for policy, we should be cautious about using research findings to predict the effects of any large policy change of a type that has not been tried before.

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Appendix On Data For Tables 3 and 4

A. Data for equations (1′), (2′), (3), (5), (14–19), and (24–25):

M1 = currency plus checkable deposits, billions of dollars

1892–1956, June 30 data: U.S. Bureau of the Census. *Historical Statistics of the U.S. from Colonial Times to 1957* (Government Printing Office, 1960), p. 646, series X-267.
1957–58, June 30 data: *Economic Report of the President,* 1959, p. 186.
1959–91, averages of daily data for December, seasonally adjusted: *Economic Report of the President,* 1992, p. 373.
Note: December data, seasonally adjusted, are close to June 30 data.

GNP = gross national product, billions of dollars per year

1892–1928: Kendrick (1961), pp. 296–7.
1929–59: *Economic Report of the President,* 1961, p. 127.
1960–88: *Economic Report of the President,* 1992, p. 320.
1989–91: *Survey of Current Business,* July 1992, p. 52.

RAaa = long-term high-grade bond rate, percent per year

1892–1918: Macaulay's unadjusted railroad bond rate, U.S. Bureau of the Census. *Historical Statistics of the United States from Colonial Times to 1957* (Government Printing Office, 1960), p. 656, series X-332.
1919–91: Moody's Aaa corporate bond rate:
1919–38: U.S. Bureau of the Census. *Historical Statistics of the United States from Colonial Times to 1957* (Government Printing Office, 1960), p. 656, series X-333.
1939–91: *Economic Report of the President,* 1992, p. 378.

Note: For pre-1959 data I used sources that were available in 1960, in an attempt to make equation 2′ reproduce the 1892–1959 equation 2, which originally appeared in Christ (1963). These same sources also yield equation 1′, which is an approximate reproduction of the 1919–52 equation 1, from Latané (1954).

B. Data for 1959–91 for equation (4):

M1 = currency plus checkable deposits, billions of dollars: same as above.
GDP = gross domestic product, billions of dollars per year: *Economic Report of the President,* 1992, pp. 298 or 320.
RAaa = Moody's Aaa corporate bond rate, percent per year: same as above.

C. Data for 1919–52 for equation (1), as described in Latané (1954), p. 457:[1]

M1: "demand deposits adjusted plus currency in circulation on the mid-year call date, (Federal Reserve Board Data)."

U.S. Bureau of the Census. *Historical Statistics of the United States from Colonial Times to 1957* (Government Printing Office, 1960). Series X-267

GNP: "Department of Commerce series from 1929 to date; 1919–28 Federal Reserve Board estimates on the same basis (National Industrial Conference Board, *Economic Almanac,* 1952, p. 201)."

[1]Though Latané's work was published in 1954, research analysts at the Federal Reserve Bank of St. Louis used more recent data to replicate his work.

RAaa: "interest rate on high-grade long-term corporate obligations. The U.S. Treasury series giving the yields on corporate high-grade bonds as reported in the *Federal Reserve Bulletin* is used from 1936 to date. Before 1936 we use annual averages of Macaulay's high-grade railroad bond yields given in column 5, Table 10, of his *Bond Yields, Interest Rates, Stock Prices,"* pp. A157–A161. Macaulay, Frederick R. *Bond Yields, Interest Rates, Stock Prices* (National Bureau of Economic Research, 1938).

D. Data for 1892–1959 for equation (2), as described in Christ (1963), pp. 217–18:[2]

M1: "currency outside banks" plus "demand deposits adjusted", "billions of dollars as of June 30."

U.S. Bureau of the Census. *Historical Statistics of the United States from Colonial Times to 1957* (Government Printing Office, 1960). Series X-267

U.S. Bureau of the Census. *Historical Statistics of the United States from Colonial Times to 1957; Continuation to 1962 and Revisions* (Government Printing Office, 1965). Series X-267

RAaa: "long-term interest rate (Moody's Aaa corporate bond rate, extrapolated before 1919 via Macaulay's railroad bond yield index)", "percent per year."

GNP: "gross national product, billions of dollars per year."

[2]Though Christ's work was published in 1963, research analysts at the Federal Reserve Bank of St. Louis used more recent data to replicate his work.

David A. Dickey

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Commentary

FIRST, LET ME EXPRESS my appreciation for the invitation to participate in this conference. I have made several visits to the Federal Reserve Bank of St. Louis and have enjoyed the hospitality of Ted and his associates. Carl Christ's paper was interesting and thoughtful, prompting us to look again at some philosophical issues in econometric modeling.

Trying to describe an ideal econometric model makes sense to me. When I am in the market for a car, camera or other piece of technological equipment, I often look at the top-of-the-line item to see what it can do and then decide which features I can give up to make my purchase affordable. Carl Christ has done the same sort of shopping for an econometric model, searching for the best of all possible models. We likely cannot afford it, in the sense that we cannot really afford to formulate a model now and go fishing for several years while test data accumulates. Nevertheless, looking at the top-of-the-line type of model will let us see an upper bound on what we can expect models to give us and will give us a target point to move toward even though we have no hope of actually hitting the target.

Researchers see some of the same statistical strengths and weaknesses of econometrics when they apply statistics to the biological and physical sciences. In both sciences you must decide which independent variables are of interest. Often these are control variables like fertilizer, water, insecticides or in our case, interest rates. In actual agricultural practice, insecticide and water are often applied in response to observations on the state of the growing plants. Similarly in economics, it is often hard to tell if a control variable, the Aaa bond rate, for example, is a response to observations on the economy or a driver of them. Agronomists perform greenhouse experiments in which they fertilize plants in amounts long and short of the perceived optimum to map out a response curve for yield as a function of fertilizer. In contrast, economists are reluctant to experiment by (knowingly) setting control variables at nonoptimal values.

It is well known in agriculture that greenhouse results often do not translate directly to the field, so agronomists, like econometricians, distinguish micro from macro environments. Biologists also typically know the lag relationships, if any, involved in their experiments. Yield in August may be related to fertilizer application in June, but when do we finish harvesting the effects of a bank closure or a tax increase? Biological organisms in the field and the economy respond to a great number of inputs and a big decision is which to put into the model and which to leave as part of the error term.

An aspect of model choice that Christ does not particularly stress is the choice of model form. This is sometimes chosen to fit the data at hand well and so can be part of a data mining operation. Many physical models, as well as econometric

models, are not linear. Einstein's famous $E = MC^2$ is an example. In economics, the well-known $MV = PY$ can be made linear by taking logarithms. Such transformations have implications for variance on the original scale—a point I think is not often appreciated. If $\log(M) = \log(P) + \log(Y) - \log(V) + e$ with e normal, then $MV = PY[\exp(e)]$ and therefore the error is multiplicative, causing heterogeneity of variance in the untransformed data. Further, nonlinear models like $MV = PY$ pose problems of aggregation. For example, suppose such a relationship holds in all segments of an economy. Will it then hold in the aggregate? Not necessarily. To illustrate, note that

$$(20)(2) = (4)(10)$$

and

$$(12)(6) = (18)(4).$$

However, if we average 20 and 12, average 2 and 6, average 4 and 18, and average 10 and 4, we find that $(16)(4) = 64$, but that $(11)(7) = 77$. So apart from any estimation errors, even exact relationships can hold on some scales but not on others.

Despite all these potential problems, people have an inherent tendency to observe their environment and draw inferences. There seems to be an optimism that with enough information we can solve any of our problems, regardless of whether they are economic problems, medical problems or other kinds of problems. Attempts at problem solving will certainly persist, and analysis and criticism of these attempts, such as Christ's, are worthwhile activities. In fact, I think one of his main points is that we are all statisticians, observing our world and modifying our models based on the data. This may be done with or without numerical calculation. Model selection is influenced by our previous observations in a way that is hard to quantify.

I found the Mitchell quote from 1927 somewhat offensive. The idea that with enough calculations, any two series can be found correlated at 90 percent surely cannot be true of informed and careful statisticians and econometricians. Nevertheless, I can agree with the nature, if not the extent, of the problem. I can imagine someone noticing how a black cat had crossed his path on several occasions before a misfortune, thus giving birth to a superstition.

Surely, however, the past must be somewhat like the future. Living in North Carolina, for example, I do not carry earthquake insurance, but I might if I lived in San Francisco. I suspect that early mankind anticipated being cold in winter even without a good understanding of meteorology. I do not think we should dismiss modeling as a whole based on Lucas-critique types of considerations. Christ gives an example of a simple model that seems to have held up over a fairly long period. This is good news and I would go further to suggest that we not give up on statistical modeling even if we can't get quite such good results every time. Along these lines, I agree with Christ that ARIMA and VAR are not as informative as a good econometric model, but they may do less damage to our understanding of the economy than a mediocre econometric model.

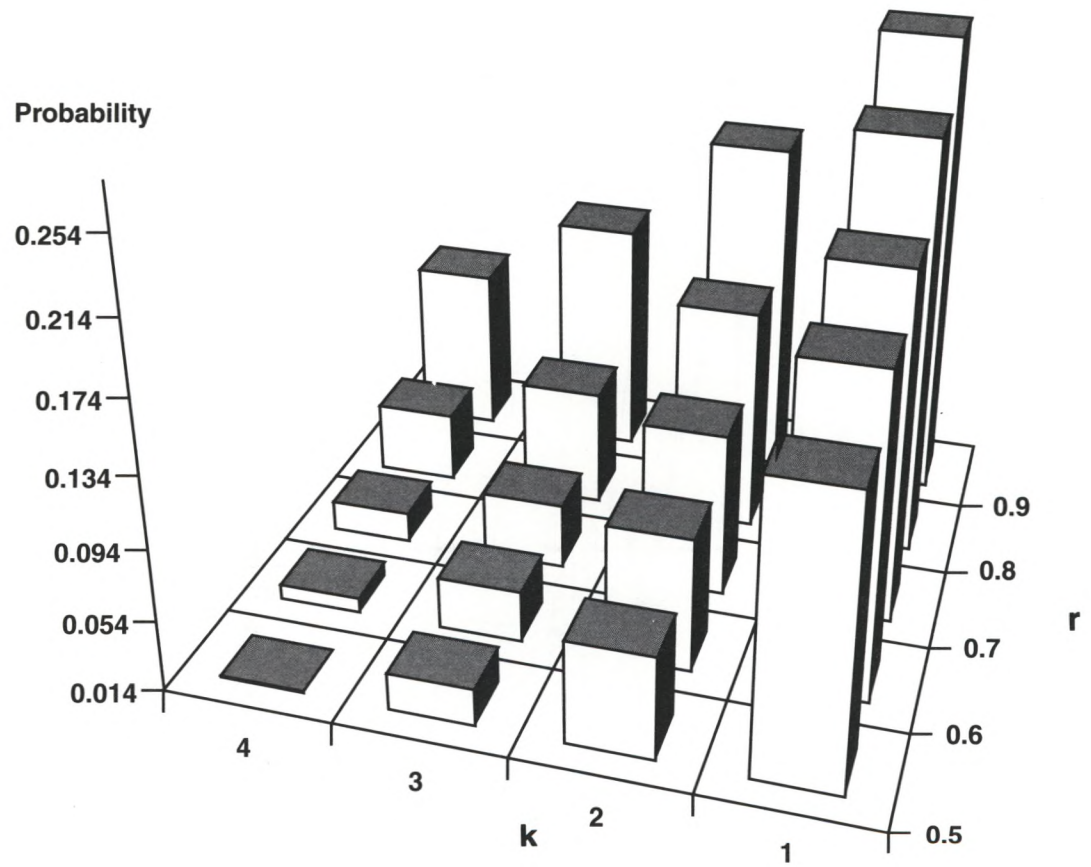
As a technical person, I feel obliged to address at least one or two technical points. I note that in the paper, some time was spent trying to decide whether a quadrupling RMSE would be reasonable in a good forecasting model. When we look at the theoretical forecast error variances, we can argue that this variance could not increase by more than a factor of eight in going from a one-step-ahead to an eight-step-ahead forecast. To compensate for the difference between estimated and theoretical MSEs, it is then concluded that if the estimated error mean square goes up by a factor of 16 (RMSE up by a factor of four) our model would be suspect. The probability of this quadrupling of sample RMSE would depend on the autocorrelation and the number of forecasts used to estimate RMSE; for example, if we just look at a single one-step-ahead residual and a single eight-step-ahead residual, the estimated RMSEs will simply be the ratio of the absolute errors and hence will vary a lot around the true values.

Suppose MSE is calculated by averaging the squares of k independent one-step-ahead errors $e(n+1)$ and the squares of the k corresponding eight-step-ahead errors

$$z(n+8) = e(n+8) + r\,e(n+7) + r^2\,e(n+6) + \ldots + r^7\,e(n+1)$$

from an AR(1) with autoregressive parameter r. I estimated the probability that the sum of k values $z(n+8)^2$ is more than 16 times the sum of the k corresponding values $e(n+1)^2$ by a Monte Carlo experiment with 10,000 replicates at each r and k. Figure 1 summarizes the results with r = 0.5, 0.6, 0.7, 0.8 and 0.9. The number of forecasts

Figure 1
Probability of Quadrupling
(versus number of squares in RMSE and first order autocorrelations)



from which RMSE is calculated is k = 1, 2, 3 or 4. It is seen that, because of the variation in RMSE around its theoretical expected value, the probability of the eight-step-ahead RMSE exceeding four times the one-step-ahead RMSE can be reasonably large (greater than 0.2 in the case that k = 1) even with a perfect model and relatively mild autocorrelation. As k gets large, and hence as RMSE converges to the theoretical value discussed in the paper, the probability declines. Figure 1 shows the empirical frequency of RMSE quadrupling.

As another minor technical point, I would like to say that a lot of new ideas are the same old vanilla ones with a few sprinkles thrown on. In his figure 3, Christ plots the inverse velocity against the inverse Aaa bond rate data with connecting lines indicating the time order of the data and with the regression line overlaid. We

Figure 2
Aaa Bond Data

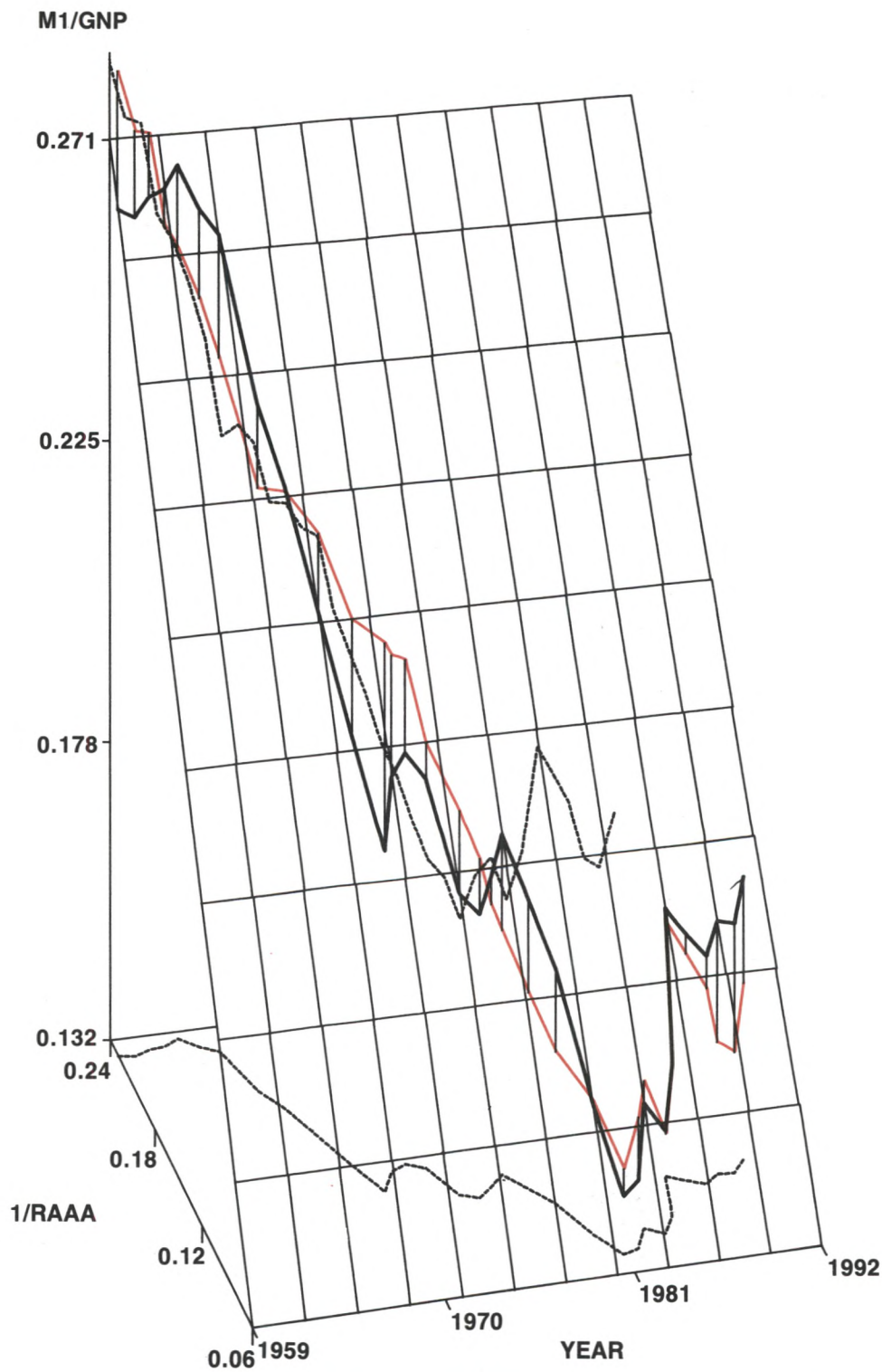
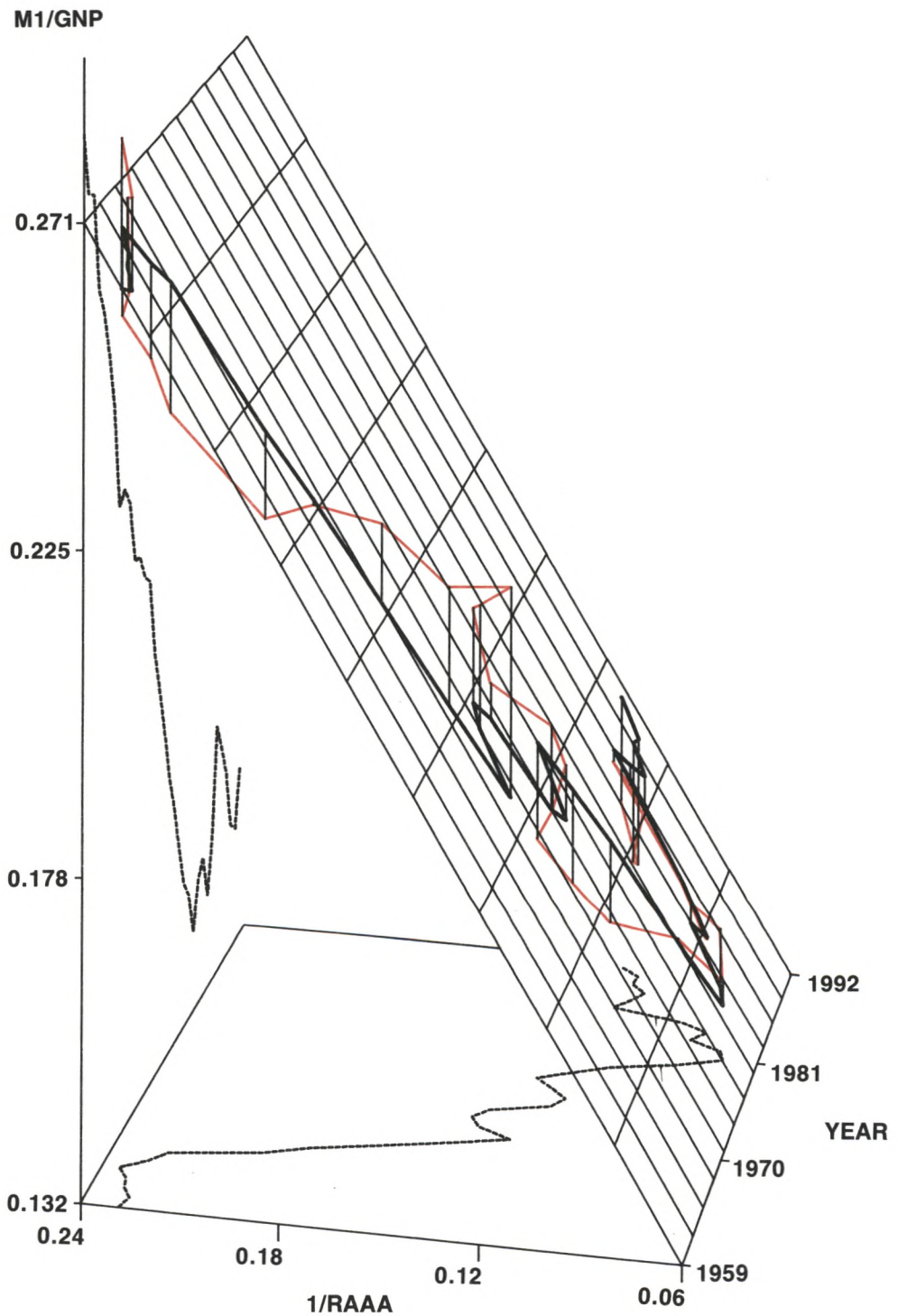


Figure 3
Aaa Bond Data



could think of this as the end view of a three-dimensional picture, which we rotate and tilt a bit in figures 2 and 3. The line is seen as the end of a three-dimensional plane. The data wander pretty far up and down and right and left but never get too far from the plane. Projections into the wall and floor of the plot show the two nonstationary looking series, also depicted in Christ's figure 2. The tightness of the data about the plane shows that a linear combination of the two series looks fairly stationary. This is the idea of cointegration. Regression is one way of finding cointegration in bivariate series. Other methods may give slightly different planes, but we can see that the main ideas of this currently popular econometric method are quite close to simpler time-tested ones.

In closing, I think we are at an exciting time for econometrics. Some of the computational burdens have been lifted, and we can concentrate more on proper model forms and formulation methods. Philosophical guidance such as that offered by Christ is important to keep in mind in our search.

David Laidler

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Commentary

CARL CHRIST'S PAPER is a worthy tribute to Ted Balbach. It is broad ranging, thoughtful and provocative; and it deals with serious issues too. Moreover, no small matter for this discussant, it is readily accessible to the stochastically challenged. The best compliment I can pay it is to add a few reflections of my own on the questions it raises.

It must now be at least 25 years since I first heard Carl Christ discuss the importance of testing models against data that had not been used to build them. Even then he distinguished between data generated before and after not just the model's estimation period, but also the actual time at which the model was constructed. This last distinction is not often made, but Carl convinced me that it is more important than we might think. I am glad he still stresses it. The simple fact is that what we know about economic history influences how we build our models in ways that we barely recognize. Suppose we decided today to build a model of the U.S. business cycle, to estimate it for the period 1948–70, and then to test it further against data for 1971–92. When we constructed our model, would we be able to ignore the two oil price shocks during the 1970s, and would we even be right to ignore them if we could? But if we did remember the activities of the Organization of Petroleum Exporting Countries, would it really be the case that the structure fitted to the data for 1948–70 would yield parameter estimates unaffected by any influence from data generated after 1970?

It is at least safer, and more convincing too, if we test our models against really new data—data of which we were unaware at the time those models were constructed. I must confess, though, that the first time I heard Carl Christ make this point, I was discomfited by his argument. In the 1960s I was estimating demand-for-money functions, and I did not much like the idea of waiting another decade or so before submitting my results to a journal. The right scientific approach was all well and good in its place it seemed to me, but there were more mundane matters to consider—promotion and tenure, for example. But here we are 25 years later, and the back issues of economics journals are full of empirical studies, which were influential in their time but are now half forgotten, whose results could be subjected to real tests. How would the Jorgenson investment equation or the Andersen-Jordan equation stand up?[1] There is a market niche here waiting to be filled by applied econometricians, not least those currently worrying about the above-mentioned publishing criteria for promotion and tenure.

In his paper, Christ has shown us how to do such work with his investigations of what he calls the plain-vanilla velocity equation, first proposed by Henry Latané in 1954. This rather odd equation has held up surprisingly well. The use of the inverse of the rate of interest as an argument surely (as Robert Rasche has suggested to me) reflects Latané's reluctance to use logarithms to deal with a nonlinear relationship in an age when such a transformation of data had to be carried out using tables and much tedious interpolation therefrom. In the light of Carl's results I am relieved to be able to report that, even before reading his paper, I had decid-

[1]See Jorgenson (1963) and Andersen and Jordan (1968).

ed to retain the paragraph dealing with Latané's study in the new edition of *Demand for Money*.[2]

From a certain viewpoint, the survival of the Latané equation for a full three and a half decades is remarkable. It is, after all, best interpreted as a rearrangement of a supply-and-demand-for-money system, and as Carl also tells us, the last two decades have not been kind to empirical demand-for-money functions. But at least one precedent in the literature occurred, namely Robert E. Lucas Jr.'s demonstration that Allan Meltzer's long-run demand-for-money function also seems alive and well when viewed in light of more recent data.[3]

Now we must not claim too much here, and Carl does not. The Latané equation displays many faults calculated to shock the econometric purist—for example, auto-correlated residuals. When these are attended to within sample, the out-of-sample performance of the more sophisticated formulation seems to deteriorate. Similarly, Lucas showed that though subsequent data still seemed to move around Meltzer's relationship, they did so with a great deal of complex serial correlation. But still, I think there is a lesson to be learned here, one which I began to develop in the second (1977) edition of *Demand for Money* and which work using co-integration techniques is now tending to support. The lesson is this: what we call the long-run demand-for-money function is indeed a stable structural relationship, give or take ongoing institutional change, which we often deal with by adapting our way of measuring money. What we call the short-run function, however, is not structural at all. It is rather an ill-understood, quasi-reduced form characterizing the mutual dynamic interaction of the money supply and the variables on which the demand for money depends in the long run.

This way of looking at things helps explain why co-integration studies produce evidence consistent with the existence of a stable long-run demand-for-money function and why simple regressions of the type estimated by Latané and Meltzer hold up rather well. As David Dickey has told us here, simple regression is one way of looking for co-integration. It also helps explain why the error correction mechanisms associated with co-integration relationships are complicated and unstable, why the dynamics of so-called short-run demand-for-money functions have tended to break down as sample periods are extended, and why more sophisticated estimation techniques, designed to cope with auto-correlated residuals, applied to relationships like the Latané equation produce results that are less robust over time than the plain-vanilla version. Have we not, after all, known all along that changes in the money supply affect the economy with long and variable time lags, which, among other things, involve feedbacks to the money supply itself? And if we have known that all along, should we be surprised if we get nowhere with studies of monetary dynamics that do not begin by specifying a model of the aforementioned interaction that will permit us to identify the structural parameters of the system we are investigating?

It is all much easier said than done, of course, but it will not be done if no one tries, and I hope therefore that Carl Christ's striking results for Latané's equation will prompt someone to carry his investigation further.

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[2]See Laidler (1993).

[3]See Lucas (1988) and Meltzer (1963).

Allan H. Meltzer

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Real Exchange Rates: Some Evidence from the Postwar Years

THE MOVE TO FLEXIBLE EXCHANGE RATES early in 1973 is the type of experiment that economic researchers experience rarely. A marked change in monetary regime from fixed to flexible rates was followed by years of floating rates. Initially, some governments may have thought of flexible rates as a temporary expedient to last only until new parities were firmly established. Within a few years, however, the governments of principal developed countries, including the United States, accepted flexible rates as a durable arrangement. Although there has been considerable intervention in the currency markets, attempts at policy coordination and talk about target zones (particularly in recent years), the dollar and several other currencies have continued to float. Most major trading countries have reduced or removed exchange controls and other restrictions on capital mobility.

A frequent, and probably the dominant, assessment of experience with flexible rates is that they have not worked as anticipated. Robert Aliber (1992, p. 44) writes that "Few of the advantages noted by proponents of floating exchange rates have been realized in the 1970s and the 1980s." Krugman and Miller (1992, p. 1) share this view and, in addition, criticize theories of exchange rate determination. They write that "interventionist economists believed that left to themselves exchange markets would introduce unnecessary and harmful volatility into the exchange rate." These writers summarize the current state of research as showing that monetary models "have had almost no empirical success. Indeed, money supplies, if they enter at all, typically enter with the wrong sign." (ibid, p. 9)

Singleton (1987, p. 9) reports the professional judgment that "by most measures, exchange rates have been relatively unstable since 1973." He recognizes, however, that the instability may reflect uncertainty that the public faces in adjusting to information about the future. And he notes that observed variability of exchange rates may have lower welfare costs than alternative regimes.

Mussa (1986) studied fluctuations in bilateral exchange rates for the principal market economies. He showed that the variability of bilateral real exchange rates from 1957 to 1984 was eight to 80 times higher in flexible-rate periods. There were no examples of lower variability under flexible rates among the 17 countries studied. The reason is clear from Mussa's data. Under flexible exchange rates the variability of nominal exchange rates increases much more than the variability of the ratio of relative price levels declines. In fact, the variance of bilateral rela-

tive price levels was not always lower in flexible-rate regimes.

Mussa did not draw any conclusion about the welfare properties of alternative regimes. The increased variance of bilateral real exchange rates may substitute for the variance of other variables, may be absorbed at relatively low cost by hedgers and speculators in financial markets, or in part may represent permanent shocks, such as the oil shocks of the 1970s and 1980s, that require adjustment of relative prices and real values. But the alternative is also plausible. Some of the higher variances under fluctuating rates may be the source of excess burden.

A main problem in reaching a judgment about the operation of fluctuating rates is that there is no benchmark for comparing alternative regimes. Economic models of exchange rates have performed poorly compared with statistical models such as the random walk. Many papers report that there is no significant relation, often no evidence of any reliable relation, between exchange rates and other economic variables. Meese and Rogoff's (1983) well-known paper found that a random walk performed as well out of sample as any estimated structural model. This suggests that many changes in exchange rates are random events, unrelated to policy or macroeconomic performance. Chinn (1991) summarizes recent tests for cointegration of real and nominal exchange rates with standard economic aggregates such as money and output at home and abroad or, for nominal exchange rates, relative rates of inflation. The tests reject cointegration, suggesting that there is no long run relationship between exchange rates and any of these aggregates.

Critics have commented especially on the relatively large change in dollar exchange rates in the 1980s. Even Haberler (1987), a long-time proponent of floating refers to "the widespread disenchantment with floating exchange rates." Critics have not been satisfied with computations showing that the variances of exchange rates, like the prices of other traded assets, exceed the variances of prices of current production. Nor have they accepted as sufficient explanation for observed variability that foreign exchange markets, like other markets for traded assets, respond to new information, which arrives continuously in a changing world.[1] Without evidence showing that the news is systematically linked to exchange rate changes and that the adjustments are toward a new equilibrium, the proposition is nearly empty.[2]

A longer summary of the large literature on flexible rates would belabor the obvious. Neither the critics nor the proponents of flexible exchange rates have produced much evidence on which to base comparative judgments about exchange rate regimes. Claims that variability is larger or too large are meaningless unless an alternative is specified and its properties compared. Yet it is common to find statements that flexible rates have not worked as expected. They "do not substantially shield a country from events abroad"; that "current account imbalances have been protracted", and that "wide movement and reversals have contributed to the widespread impression that floating rates tend to overshoot." "Although clean floating has not yet become a dirty word, the simple faith that the market is always right has been shaken."[3]

This paper reconsiders experience under flexible exchange rates. Section 1 summarizes the claims about flexible rates in Milton Friedman's classic 1953 paper to show that Friedman's claims are more modest than is often supposed. Section 2 presents some key facts about exchange rates and comparative variability of several variables under fixed and flexible rates. Section 3 estimates a model of the so-called real exchange rate under Bretton Woods and flexible rates and tests for the effect of economic aggregates on the exchange rate. The model incorporates some of the principal variables affecting exchange rates suggested by Friedman. Section 4 discusses some limitations of the results. A conclusion completes the paper.

FRIEDMAN'S CASE FOR FLEXIBLE EXCHANGE RATES

In "The Case for Flexible Exchange Rates," written shortly after the Bretton Woods System started, Friedman claims four benefits for flexible rates: (1) increased liberalization of trade, (2) avoidance of direct controls, (3) facilitation of

[1]See Frenkel (1983).

[2]The qualification is needed because some testable propositions result. Changes may be unbiased or larger in periods of large shocks such as wars and oil price changes.

[3]See Wallich (1984).

rearmament, and (4) harmonization of internal monetary and fiscal policies.[4] The point that concerned later critics most, variability or instability, is dismissed early with the claim that exchange rate instability reflects instability in the economy and is not a property of a flexible or floating rate system. This claim is not self-evident, and it has not been accepted by the principal critics of flexible rates. Friedman appears to have anticipated this outcome. He devotes more space to refuting or dismissing the charge of instability than to making the positive case for the four benefits claimed for flexible rates.

Friedman's essay does not claim that flexible exchange rates are optimal for all countries or even for a single country. When discussing the former sterling bloc, he considers a mixed system in which groups of countries may elect to maintain fixed exchange rates internally and flexible rates against all other groups or countries. Although there are structural differences between the sterling bloc and the proposed European Monetary Union, Friedman anticipates the principal issues: policy harmonization, avoidance of trade controls and exchange restrictions, absence of a political authority and, in the absence of controls, the need to choose between unemployment and exchange rate changes in the short term.

Recognizing that optimality of flexible rates cannot be established, Friedman limits his claim to the judgment that flexible exchange rates are more desirable socially than the four alternative means of offsetting changes in international position. The four alternatives are: (1) official changes in currency reserves; (2) changes in domestic price levels and incomes; (3) periodic realignment of parities; and (4) direct controls.

The key conditions are posited. First, with flexible exchange rates, there are "broad, active, and nearly perfect markets ... in foreign exchange" whenever they are permitted. Second, a fixed but adjustable exchange rate "insures a maximum of destabilizing speculation. Because the exchange rate is changed infrequently and only to meet substantial difficulties, a change tends to come well after the onset of difficulty, to be postponed as long as possible."[5] These conditions, it seems fair to say, have not been accepted by the critics of flexible rates. The critics typically argue that speculation is (or can be) destabilizing.

Friedman considers and rejects some common conjectures about destabilizing speculation. His main argument is that there is no empirical foundation for these claims. Appearances to the contrary are misleading and subject to misinterpretation. A main problem in any study is to separate the actions of speculators based on correct predictions of parity changes and actions that cause parity changes that would have been avoided. These problems arise under an adjustable peg, but Friedman claims they would be prevented under continuous adjustment of flexible rates. Friedman is cautious, however. He avoids a general claim that speculation is stabilizing. Instead, he argues that if destabilizing speculation is common, governments (or exchange stabilization funds) would profit by intervening. And he recognizes that governments may have more information or more timely information that gives them an advantage over private speculators. He is willing to let a government agency intervene to smooth temporary fluctuations if they can do so profitably (p. 188), but he is skeptical that they would be able to profit consistently. They are less likely to profit, he claims, than private speculators who risk their own wealth.

The reason for choosing flexible rates is that other means of adjustment are less satisfactory. Fixed exchange rates were maintained in the 19th century because the public and governments tolerated larger fluctuations in domestic prices and employment than would be acceptable in the late 20th century. Direct controls are least satisfactory because they introduce distortions and do not correct permanent differences in relative prices in foreign and domestic markets.

Timing of adjustments is a source of variability about which little is known with precision. Anticipating future discussion, Friedman considers overshooting and undershooting of exchange rates. Overshooting arises because initial adjustment is borne by prices that adjust most readily. The exchange rate is such a price. Later other prices adjust, and the overshooting reverses, although it may be replaced by undershooting of the final change, followed by a series of adjustments around the new equilibrium.

[4]The essay was written in 1950 but not published until 1953.

[5]See Friedman (1953), pp. 162-64.

Thus Friedman recognizes that there will be variability and fluctuations of exchange rates, not prompt, rapid adjustment from the old to the new equilibrium. The possibility that the fluctuations, though not destabilizing, produce excess burden and welfare loss is not addressed directly. Friedman's main response to this central issue is comparative. His conclusion can be summarized in two paragraphs.

First, comparison of exchange rate regimes must include the costs of adjustment under alternative policies. The comparison cannot be limited to the size of changes in exchange rates or the variability of exchange rates under different regimes. Changes in the relative prices of goods and services are not the same under different policies. With gradual adjustment of real wages and other relative prices, labor market adjustment, hence unemployment rates, will differ under different regimes. And direct controls introduce distortions and welfare losses.

Second, there is no presumption that social costs could not be increased by flexible exchange rates. "About all one can say ... is that there seems no reason to expect the timing or pace of adjustment under the assumed conditions [flexible exchange rates] to be systematically biased in one direction or the other from the optimum or to expect that other techniques of adaptation—through internal price changes, direct controls, and the use of monetary reserves with rigid exchange rates—would lead to a more nearly optimum pace and timing of adjustment."[6]

EXCHANGE RATE CHANGES AND VARIABILITY, 1973–90

Excessive variability is one of the main issues raised by the critics of flexible rates. Evidence of increased variability of real or nominal exchange rates after 1973 is easy to produce. To draw any conclusion about the effects on welfare, two issues must be resolved. First, as Friedman noted, increased variability of exchange rates may reduce variability of output, consumption, employment or other variables of interest to consumers. Reduced variability of these variables can produce a welfare gain despite the increased variability of exchange rates. Second, increased variability of exchange rates may result from real shocks, such as an oil shock, or from policy activism, or it may reflect increased knowledge of the operation of exchange markets.

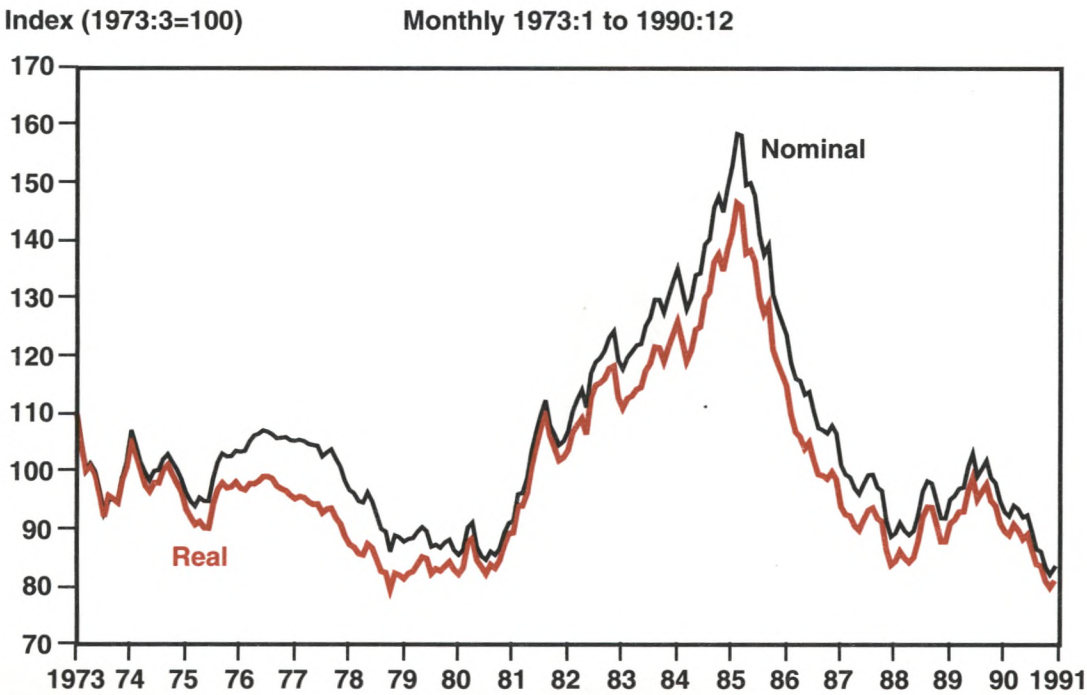
This section considers changes and variability of exchange rates and some other variables under Bretton Woods and flexible rates. Figure 1 shows the monthly trade-weighted nominal and real exchange rate for the United States, using Federal Reserve weights, for the period 1973–90. A rise in the index is an appreciation of the dollar. Two facts are immediately apparent. First, real and nominal exchange rates move together and by similar amounts.[7] This fact has been demonstrated repeatedly for bilateral rates. See Mussa (1986) and Edwards (1989) for studies of developed and developing countries. Second, trade-weighted exchange rates moved over a relatively wide range during the 18-year period. The movement is dominated by a persistent appreciation from 1980 to 1985 followed by a persistent depreciation lasting to early 1987. Both exchange rates then returned to approximately the same range they had left in 1979.

Other measures of trade-weighted exchange rates developed by the International Monetary Fund (IMF) using wholesale prices or unit labor costs in the various countries to compute real exchange rates show the same general pattern. Experiments with different weighing patterns

[6]See Friedman (1953). The conflicts in the system developed more slowly than Friedman predicted. He predicted that "direct controls over exports and imports would be reimposed on a large scale within two or three years at the most." This prediction was inaccurate. The United States introduced some controls on capital movements in the 1960s, but the trend in the 1950s and 1960s was toward reduction of trade barriers under General Agreement on Tariffs and Trade rules. The conflicts in the system were resolved partly by changes in parities abroad but mainly by inflation in the 1960s and early 1970s.

[7]The so-called real exchange rate measures the ratio of the price level in the United States to a weighted average of foreign price levels expressed in a common currency.

Figure 1
Trade-Weighted Nominal and Real Exchange Rates



do not appear to change the general features, although computed variances and ranges differ for the individual measures.[8]

The exchange rate data shown in figure 1 raise two issues that will concern us. First, why do real and nominal exchange rates move together? Second, is the higher variability of real exchange rates under fluctuating exchange rates caused by policy actions, or is there evidence of excess burden arising from increased variability unrelated to policy action?

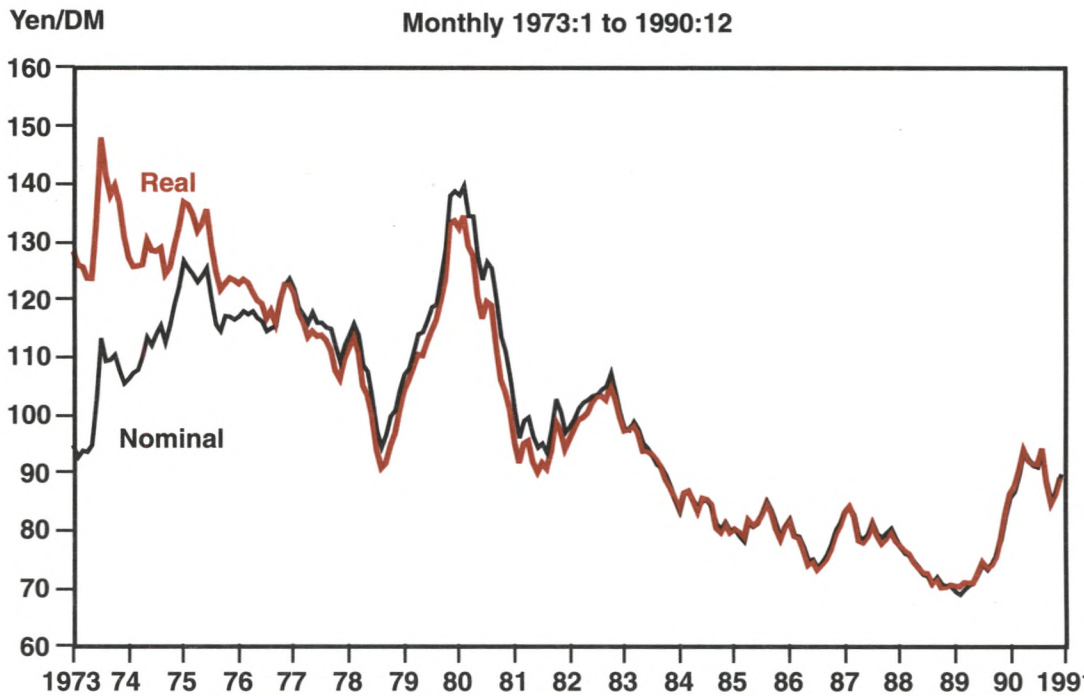
The similarity of real and nominal exchange rate changes in figure 1 is not peculiar to U.S. data. Figure 2 shows monthly values of the exchange rate of the Japanese yen for the German mark during the period 1973–90. The real exchange rate is obtained using the relative consumer price indexes for the two countries. In the first years, the real and nominal exchange rates differ; consumer prices rose more rapidly in Japan than in Germany. In real terms Japan paid more yen per mark than in nominal terms. After 1976, the two price levels had about the same rate of change, so the real and nominal exchange rates are often indistinguishable on figure 2.

Mussa's (1986) study of changes in bilateral exchange rates for a broad sample of developed countries during the years 1957–1984 found the same result. Under flexible exchange rates, changes in nominal and real exchange rates are highly correlated, but changes in nominal or real exchange rates are not closely correlated with changes in the ratio of price index numbers.

[8]Becketti and Hakkio (1989) computed the correlation between innovations in seven alternative measures of trade-weighted exchange rates. Most of the correlations are above 0.9 using quarterly data for 1976 to 1988. They show that similar results hold for percentage rates of change of exchange rates.

The Federal Reserve index uses weights reflecting country shares of world trade. I computed an alternative index based on U.S. trade weights and reweighted the index at the start of each decade-1960, 1970 and 1980-to adjust for changes in relative trade shares. The main conclusion sensitive to the change in weights is that the variance of the trade-weighted real exchange rate is lower for the alternative measure. I have used the Federal Reserve index throughout.

Figure 2
Real and Nominal Yen/DM Exchange Rates



Meltzer (1990) considered the variability of multilateral exchange rates using data from the IMF. Real exchange rates are based on both relative wholesale prices and relative unit labor costs, and variances are used to measure variability. Again, countries with flexible exchange rates had greater variability of nominal and real exchange rates than countries in the European Monetary System (EMS) that maintained an adjustable peg with other members of the EMS. Changes in real and nominal exchange rates were highly correlated under flexible rates. However, the variability of relative unit labor costs was typically lower in the countries with flexible exchange rates, whereas the variability of wholesale price ratios was higher.

Table 1 summarizes these data. Both nominal (N) and real (R) exchange rate changes are more variable under flexible exchange rates than under fixed but adjustable rates, whereas relative prices are not. The variability of R or N under flexible rates is significantly different at the 1 percent level from the variability experienced under EMS or the mixed regimes (denoted other) that had crawling pegs or some other type of partially fixed nominal exchange rate during this period. Changes in multilateral real exchange rates are 4 or 5 times more variable in flexible-rate countries than in the EMS. Generally, the variances for "other" countries lie between the variances for the EMS and flexible-rate countries. The exception is P_{ULC}—the variability of changes in relative prices based on unit labor costs, P_{ULC} has been lower on average under flexible rates, although the difference between regimes is not significant by the usual standards.

The much-discussed increase in the variability of real exchange rates in a flexible exchange rate regime may reflect only that flexible exchange rates change more frequently, whereas the relative price ratios are not much affected by the change in regime. Using the terms of trade as a measure of relative prices, table 2 shows that the variances of relative price ratios do not differ systematically across exchange rate regimes. Variability of the terms of trade rose in all countries but to different degrees unrelated to the exchange rate regime. The comparatively high variability of Japan's terms of trade suggests that there is no simple relation between the variability of this measure and the growth of trade.

Table 1

Variances of Changes in Relative Prices, Real and Nominal Exchange Rates I/1979–III/1989

	Average Quarterly Values x 100		
	EMS[1]	Flexible Rates[2]	Other[3]
P_{ULC}	.013	.007	.017
P_{WP}	.017	.037	.030
R_{ULC}	.037	.154	.058
R_{WP}	.033	.158	.059
N	.025	.143	.043

[1]Austria plus seven EMS countries (Belgium, Denmark, France, Germany, Ireland, Italy, Netherlands)

[2]Japan, Switzerland, United Kingdom and the United States

[3]Norway, Spain, Sweden, Finland and Canada

Source: IMF where $N \equiv R + P$

NOTE:

P is the first difference of the logarithm of the relative price of domestic to foreign goods or services.
R is the first difference in the logarithm of the real exchange rate
N is the first difference in the logarithm of the nominal exchange rate

Table 2

Variances under Fixed and Flexible Rates United States, Germany, Japan and the United Kingdom (quarterly values at annual rates)

	Real GNP or GDP				TOT			
Period	U.S.	Germany	Japan	U.K.	U.S.	Germany	Japan	U.K.
I/1960–III/1971	11.0	27.9	32.3	30.0	15.5	26.0	23.5	25.9
I/1973–III/1991	15.7	8.5	11.3	32.6	92.2	92.5	390.8	62.6
Relative value	1.4	0.3	0.3	1.1	5.9	3.6	16.6	2.4
I/1973–II/1975	24.2	10.6*	37.4	70.5	202.3	285.2	237.7	197.3
III/1975–II/1980	19.7	7.1*	5.1*	59.8	60.2	52.4	431.1	40.8
III/1980–III/1987	13.3	6.6*	5.8*	10.6*	61.7	78.2	388.8	31.6
IV/1987–III/1991	5.2*	3.5*	7.8*	11.0*	71.0	24.8*	170.8	27.6

TOT is terms of trade; variances are squared deviations from $x_t = (X_t - X_{t-1})/X_{t-1}$

NOTE: * denotes that the variance is lower than under Bretton Woods.

Table 2 also compares real output variances under fixed and flexible exchange rates in four countries. There is no relation between the relative variances and the monetary system. Real ouput variability declined in the same proportion in Germany and Japan with (mainly) fixed and flexible rates respectively and rose moderately in the United States and the United Kingdom.[9]

The last four lines of the table show variances for subperiods. The oil shocks of the 1970s increased the variances in table 2 in the early years of flexible rates. Variability of output fell in the United States in each successive period. In all countries the variance of real GDP was lower in 1987–91 than under the Bretton Woods regime.

[9]Meltzer (1986) reports similar results for the four countries using unanticipated variances. Unanticipated variances were computed using forecasts obtained from a multistate, univariate Kalman filter.

The countries shown in table 2 have different exchange rate systems. Japan and the United Kingdom had flexible exchange rates during the period, although the United Kingdom fixed to the exchange rate mechanism (ERM) of the EMS at the end of the period. Germany has been in the fixed-but-adjustable-rate ERM system since March 1979, and it experimented with other fixed-but-adjustable-rate systems with its neighbors beginning in the mid-1970s. The mark fluctuated, however, against the dollar, yen and many other currencies.

Though the variability of Germany's output growth is, on average, lowest of the countries in table 2, this cannot be attributed entirely to the reliance on fixed-but-adjustable rates. Variability of output growth in Germany was also lower than in Japan or the United Kingdom during the Bretton Woods period, and the relative decline in variability is the same for Germany and Japan. Further, during 1975–80 and 1980–87, periods of declining inflation, variability of Japan's output growth is comparable to (and even slightly below) Germany's.

The main conclusion drawn from table 2 is that there is no basis for a general proposition that output is more variable under fixed rates than under flexible rates. Relative prices (terms of trade) are more variable in all countries after 1973, but the increase is smallest in the United Kingdom.

POLICIES AND REAL EXCHANGE RATES

Friedman (1953) made two suggestions that have been overlooked. He gave prominence to policy—particularly rearmament—as one of the main factors affecting U.S. real exchange rates. Rearmament changes relative prices and the balance of payments (Friedman, pp. 159–60). Also, Friedman distinguished permanent and transitory changes in exchange rates. He noted the different response of speculators to changes that were expected to reverse and those that were expected to persist.[10]

Real government spending for defense rose and fell during the postwar years. Spending rose during the Vietnam War and declined during the 1970s both absolutely and relative to real output. Spending rose again in the 1980s, reached a peak in the mid-1980s and declined modestly to the end of the decade. Maintained changes in the level of real defense spending act like any fiscal change. Increases in real defense spending raise aggregate spending and interest rates. Higher interest rates attract a capital inflow, appreciating the exchange rate. In the absence of capital controls and restrictions, the capital inflow reverses the rise in the interest rate. Reductions in real defense spending have the opposite effects.[11] The sign of real defense spending per unit of output should be positive.

Real money balances also affect real exchange rates. Injections of money temporarily increase real balances, and if the price level does not adjust instantly, the increase in money depreciates the real exchange rate. Reductions in real balances brought about by reductions in money or by a rise in prices for a given quantity of money appreciate the exchange rate.

Let r, the real exchange rate, have a permanent and transitory component, so that

$$(1)\quad r_t = \bar{r}_t + u_t$$

where $\bar{r}_t$ is the permanent component and u_t is the transitory disturbance. In the absence of changes in defense spending, real U.S. money balances and foreign real balances, the expected value of the exchange rate is the permanent value. The current permanent value is a weighted average of last period's exchange rate and any persistent effect of defense spending (relative to GDP) and real money balances at home and abroad as shown in equation (2).

$$(2)\quad \bar{r}_t = \alpha r_{t-1} + (1-\alpha)\, f(d_t, m_t, m_t^*) + v_t,$$

Combining equations (1) and (2) gives equation (3), a testable equation for the real exchange rate.

$$(3)\quad r_t = \alpha r_{t-1} + (1-\alpha)\, f(d_t, m_t, m_t^*) + \varepsilon_t$$

where ε_t has the usual properties.

If the real exchange rate is mainly a random walk, $r_t = r_{t-1}$ plus a transitory white noise term.

[10]See Friedman (1953, p. 162). I began work on the relation of permanent and transitory fiscal and monetary changes to real exchange rates before I reread Friedman's essay. I was pleased to find that the results I had obtained provided evidence on some of his principal propositions.

[11]Defense spending is a large share of government spending on goods and services. It has the advantage of being independent of income and hence a good measure of the thrust of exogenous fiscal policy. It also permits a test of Friedman's proposition.

Table 3

Determinants of the Real Exchange Rate* (t statistics in parentheses)

Periods	(1) 1962–91 Annual	(2) 1972–91 Annual	(3) II/1971–IV/1991 Quarterly	(4) 1962–91 Annual	(5) 1972–91 Annual
RER_{t-1}	0.80	0.72	0.82	0.67	0.55
	(6.40)	(3.69)	(11.76)	(5.37)	(3.12)
m_t	−0.15	−0.16	−0.11	−0.13	−0.16
	(4.45)	(4.49)	(2.60)	(3.87)	(4.43)
m_t^*	0.28	0.26	0.07	0.21	0.16
	(2.55)	(1.72)	(0.52)	(1.77)	(1.06)
d_t	5.32	9.75	3.37	5.88	13.02
	(2.92)	(1.92)	(1.62)	(2.82)	(2.45)
constant	53.64	44.59	20.87	62.31	52.53
	(3.26)	(2.45)	(2.42)	(3.16)	(2.63)
$\bar{R}^2$/DW	0.89/1.89	0.78/2.03	0.92/1.95	0.88/1.92	0.77/2.00
ρ(AR1)	0.09(0.36)	−0.11(0.26)	0.26(1.75)	0.14(0.50)	−0.10(0.23)

*See appendix for definition of variables. ρ(AR1) is the coefficient of the AR1 serial correlation correction and its t-statistic.

But if monetary and fiscal actions have persistent effects, these effects will be found significant in estimates of equation (3). Equation (3) therefore permits a test of the influence of the defense spending share and real money balances against the alternative hypothesis that real exchange rates are approximately a random walk and independent of systematic monetary and fiscal effects. If the real exchange rate is mainly a random walk, α is close to one. If there are persistent and systematic effects of money and the defense spending share, current values of these variables will have a significant effect on the real exchange rate.

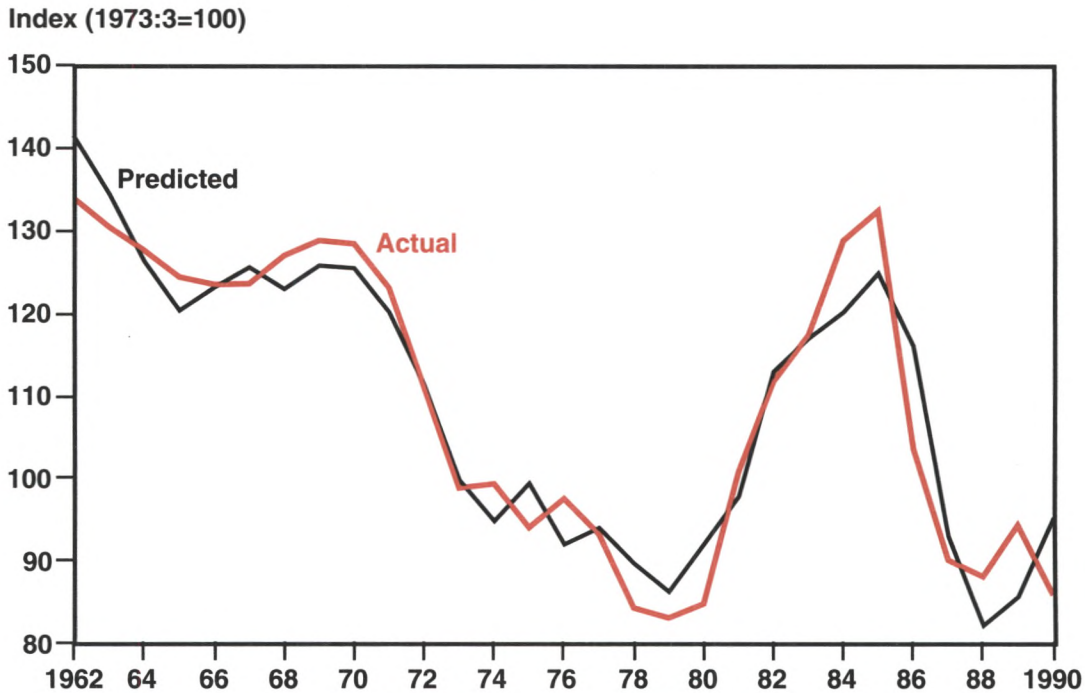
The first two columns of table 3 show estimates for 1962–91 and 1972–91 based on annual data; the former includes the fixed exchange rate period, whereas the latter does not. The two sets of estimates are similar. The standard errors of estimate for the two equations are 5.9 and 6.8, a difference of approximately 1 percent of the mean value of the real exchange rate. The implied standard error of estimate for the Bretton Woods period is 3.6, about half the value for the flexible rate period. These values suggest that transitory random variation increased under flexible rates, but the increase is much smaller than is commonly alleged. A main reason is that the estimates here remove the effects of permanent changes in m, m*, and d. These variables, particularly real money balances, have significant effects on the trade-weighted real exchange rate.

One problem with these estimates is that the coefficient of m_t^* is much larger than the coefficient of m_t using annual data. The difference may not be meaningful, however. The definitions of money differ (as described in the Appendix), and the difference in coefficients is not significant.

Figure 3 shows the actual and predicted values of table 3 using equation (1). Many of the claims about exchange rate instability are based on the relative changes in the 1980s. The chart suggests that much of the swing in the trade-weighted real exchange rate during the 1980s is driven by the variables in the model. The defense spending share rose by more than a percentage point in the early 1980s then fell after the middle of the decade. Real money balances moved in the opposite direction, falling through 1982, then rising, particularly in 1985 and 1986. The forecasts and actual values are extremely close for 1981–83. There is some evidence of overshooting by the actual rate in 1984-85, but the errors are not much larger than the standard error of estimate. The subsequent decline in the forecast value lags the actual decline, however, in 1986 and 1987. The largest error in the 29-year span is in 1986.

The third column in table 3 shifts the time interval from annual to quarterly data. The results are similar to the annual data except that m_t^* is no longer significant. Current real money balances remain significant at the usual level, and the defense spending share nearly so.

Figure 3
Trade Weighted Real Exchange Rate



The dependent variable in the regressions reported in the first and third columns of figure 3 is the average trade-weighted real exchange for the period. The fourth and fifth columns repeat the regressions for annual data using the monthly average value for December as the dependent variable. The results are similar.

The estimates in table 3 permit a test of the unit coefficient on RER_{t-1} implied by the random-walk hypothesis. All of the estimates are below unity, but two are not significantly different from unity; these are in the first and second columns of table 3. The estimates in the third and fifth columns differ from unity by more than two standard errors, so they reject this central implication of the random walk.

Recent work on the causes of fluctuations emphasizes the importance of real shocks to aggregate supply as a cause of fluctuations. The effects on the real exchange rate of the rise and fall of the relative price of oil in the 1970s and 1980s is an obvious candidate for investigation. The relative price of oil can be included in equation (2) as an additional variable affecting the permanent component of the real exchange rate. Annual data for 1972–90 and 1962–90 reject the effect; the coefficient of the relative oil price is small (−0.03) in each period and has a standard error larger than the estimated coefficient.

The use of real money balances combines the separate effects of money and prices. To separate the effect of policy actions from the effects of prices, I first differentiate $m_t = (M_t/p)$ then lag the denominator by one period to get

$$(4)\quad dm_t = \frac{dM_t}{p_{t-1}} - \frac{dp_t}{p_{t-1}}\left(\frac{M}{p_{t-1}}\right)$$

The first term is the real value (in past prices) of the current change in nominal balances. The second is the revenue from the inflation tax on last period's real money balances. To estimate responses to these variables, I take first differences of equation (3) using equation (4) to replace dm_t.

Table 4 shows estimates relating the annual change in the real exchange rate to changes in some policy variables and real shocks. I have omitted the change in m* to conserve a degree of freedom. Δm^* typically has a small negative coefficient and is not significant. Changes in money and changes in defense spending relative

Table 4

Response of ΔRER to Changes in Policy

Periods	(1) 1972–90 Annual	(2) 1972–89 Annual
ΔRER_{t-1}	0.60	0.72
	(3.05)	(6.66)
$\Delta M_t/p_{t-1}$	−0.39	−0.47
	(4.42)	(8.41)
Δd_t	23.91	13.86
	(2.62)	(3.60)
Inflation tax	−0.01	−0.001
	(0.07)	(0.00)
Δ real debt	−0.005	
	(0.93)	
Δ gov't net worth*		0.004
		(0.30)
Δ RGDP	0.07	0.03
	(2.39)	(1.81)
constant	13.95	19.32
	(1.23)	(2.47)
$\bar{R}^2$/DW	0.59/1.79	0.80/2.51
ρ(AR1)	−0.19(0.43)	−0.68(2.51)

t-statistics are in parentheses. Coefficients of ΔRER_{t-1} are significantly different from unity at the 2σ level.

*The data are from Bohn (1992). These data are available only through 1989. For other data, see appendix.

to GDP have considerable effect. For example a 0.1 percentage point change in the share of defense spending changes the real exchange rate between 1.4 percentage points and 2.4 percentage points based on the two equations. The 1982 increase in defense spending alone appreciated the dollar by 8.7 percentage points using the coefficient estimate for 1972–89.[12]

The inflation tax is not significant in the regression or in alternative estimates. This is unsatisfactory. Without a significant response to inflation, the equations imply that a change in nominal money has a permanent effect on the real exchange rate. If the equations are interpreted as short-term responses, they leave an important part of the dynamics unspecified.

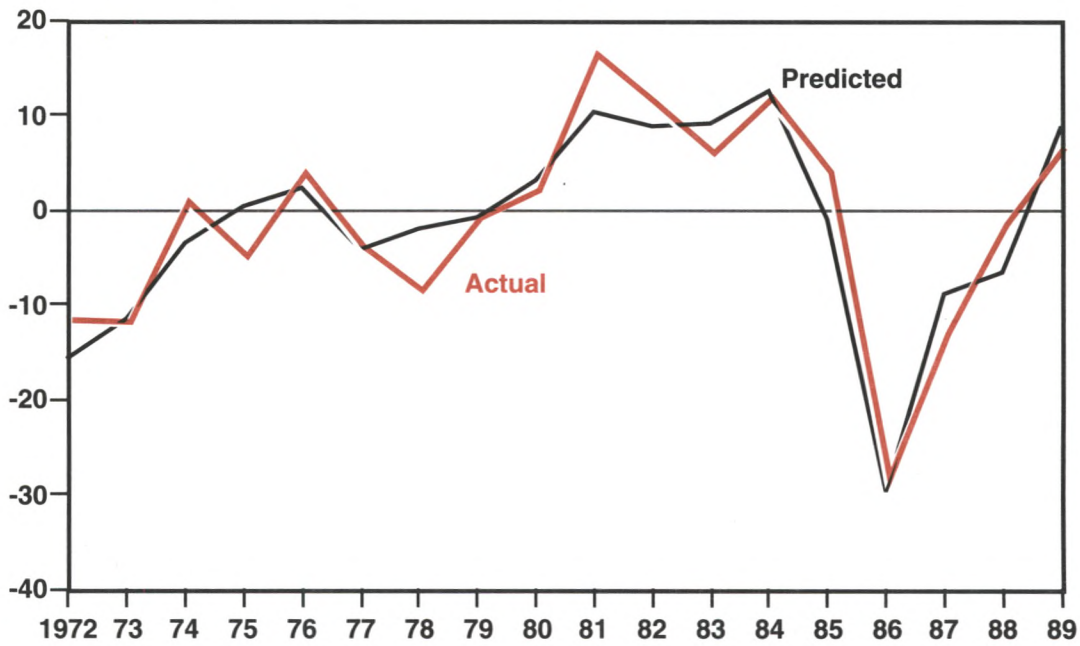
Much recent discussion of the appreciation of the real exchange rate in the early 1980s, following the Reagan tax cuts, linked the appreciation either to the budget deficit or to the increased after-tax return to real capital. The change in the real value of government debt measures the part of the current federal budget deficit financed by borrowing. I used the change in real GDP (RGDP) as a measure of the real return to real capital. This variable also captures the effects of changes in real output emphasized in the business cycle literature. Because real output is close to a random walk, changes in RGDP are a measure of unanticipated changes.

The change in RGDP has a significant effect on the change in the real exchange rate. The size of the coefficient is misleading because the changes are in billions of dollars. A more suggestive comparison is given by the change in the real exchange rate induced by changes in RGDP and the defense spending rates during four years of appreciation—1981–84. The total appreciation of the real exchange rate for this period is 44. The coefficients in the first column of table 4 assign slightly less than half of this change to the change in the defense spending ratio and slightly more than half to the change in RGDP. These calculations neglect other variables, particularly changes in money and lags of the real exchange rate. And the calculation overstates the importance of supply shocks or changes in tax rates because the changes in RGDP include the recovery from the 1981–82 recession that would have occurred in the absence of tax changes or supply shocks.

The response to deficit finance, measured by the change in real government debt, is small and insignificant. A problem with testing for effects of the budget deficit is the incomplete and imprecise way in which the deficit is measured. Eisner and Pieper (1984) called attention to this problem and showed that there are large differences between current accounting measures and measures of a more economically relevant magnitude. Bohn (1992) computed a measure of government net worth that includes principal government assets and liabilities other than Social Security and Medicare liabilities. The second column substitutes the change in real government net worth from Bohn for the change in the real value of the federal debt as a measure of the deficit. Government's net worth is negative, and if properly measured, the level of government net worth is the value of future tax payments. Changes in net worth have no significant effect. The responses to changes in RGDP and changes in the defense spending share both fall. Each explains a smaller fraction of the

[12] I neglect possible changes in the properties of the error term when taking first differences of equation (3).

Figure 4
Annual Changes in Real Exchange Rates



change in the real exchange rate during 1981 to 1984 (and other periods). The implied change in the real exchange rate resulting from changes in RGDP and the defense spending share are now approximately 25 percent and 29 percent respectively.

Figure 4 shows predicted and actual changes in the real exchange rate based on the estimates in the second column of table 4. Inspection suggests that the equation explains the annual changes more accurately for the 1980s than for the 1970s. This is particularly true in 1974 and 1975. There are only three years in which actual and predicted changes go in opposite directions—1975, 1978 and 1983. Actual and predicted changes move together during the appreciation and subsequent depreciation of the dollar in the 1980s. The equation suggests that contemporaneous changes in money and in defense spending are the principal factors keeping the predicted changes in step with actual changes.

LIMITATIONS

The empirical results are subject to some limitations. This section briefly discusses some problems arising from the absence of a structural model, neglect of simultaneity, and problems of stationarity.

First, the estimates are obtained from a simple model of permanent and transitory changes, not from a structural model. The equations are neither structural equations nor reduced forms of a structural model. An advantage of the model is that it nests the effects of money and defense spending within a popular statistical model, the random walk.

Second, several of the variables such as the price level, output, the real value of money and the defense spending share are simultaneously determined. Simultaneity has been neglected throughout. The changes reported in table 4 and the use of lagged prices removes some of these problems. That the principal results are unaffected suggests that simultaneity may not impart serious bias to the estimates in table 3.

Third, many studies of exchange rates have investigated the stationarity of exchange rates. Tests of non-stationarity at first seemed to support the hypothesis. More recent work using longer time series, however, casts doubt on this conclusion. Engel and Hamilton (1990) did not test for stationarity, but they found persistent

departures from a random walk. Earlier, Krasker (1980) coined the term *peso problem* for persistent deviations of exchange rates in a particular direction. Papers by Huizinga (1987), Hakkio and Joines (1990), Lothian (1991), and Diebold, Husted, and Rush (1991) are part of the growing literature rejecting non-stationarity based on evidence that real exchange rates return to a mean value.

A main reason for the differences in findings between earlier and later studies is the use of a longer span of years. Some early studies used daily or monthly data to obtain a larger number of observations. Recent studies suggest that an increased number of high-frequency observations is a poor substitute for the relative paucity of low-frequency data.[13]

The principal conclusion to draw from many of the studies is that the real exchange rate is subject to persistent and transitory changes. Some changes in the real exchange rate persist for long periods. Some of the changes are reversed quickly. Diebold, Husted, and Rush (1991) conclude that on average the half-life of a shock to the real exchange rate has been about three years. This finding is similar to the decay rates implied by the coefficients on annual values of the lagged real exchange rate in table 3.

Inspection of figure 1 suggests that the multilateral real exchange rate remained within a range of 95 $\pm$ 15 from 1973 to 1980 and returned to approximately the same range in 1987. To test for stationarity, I used quarterly data for first quarter 1973 to fourth quarter 1990 but omitted the sharp appreciation and depreciation from third quarter 1980 to first quarter 1987.[14] The coefficient of the lagged multilateral real exchange rate on the change in the real exchange rate is -0.14 with a t-statistic of 2.72. The Dickey-Fuller test statistic is 2.93 at the 5 percent level and 2.60 at the 10 percent level. On this basis, I reject non-stationarity.

CONCLUSION

Milton Friedman's (1953) essay on flexible exchange rates anticipated much of the discussion and many of the controversies of the next 40 years. Friedman did not claim that flexible exchange rates would be stable rates. Stability depends on the size and frequency of shocks. Friedman claimed that flexible exchange rates would (1) contribute to trade liberalization, (2) avoid reliance on direct controls, (3) facilitate rearmament and (4) allow countries to follow domestic policies to achieve price stability.

Several of these conjectures were correct. Direct controls on capital movements have been reduced since 1973 in all developed countries and in some developing countries. It seems likely that rearmament (defense spending) would have provoked greater conflict about payments imbalances in the 1980s under fixed exchange rates than under the system that prevailed. Flexible rates permitted countries to choose how much of the stimulus emanating from the United States they wished to absorb. Many countries, indeed most developed countries, both purchased dollar securities and appreciated their currency.

The average rate of inflation has been brought down under flexible rates, and some countries have achieved price stability at times. Trade restrictions, however, increased in the 1980s, particularly in the United States, and the movement toward trade liberalization slowed.

Friedman did not argue that exchange rates would be stable. He argued that the path followed by real exchange rates would depend on the real and monetary disturbances to which the economy is subject and on the persistence of shocks. Critics argued that destabilizing speculation and random movements dominate exchange rate changes and create an excess burden. This burden, some suggested, could be reduced by fixing exchange rates or establishing target zones.

The paper does not address the issue of excess burden. However, I compare variability of output and the terms of trade for four countries under the Bretton Woods System and the different regimes adopted after 1973. There is no evidence that real output is generally more variable under flexible exchange rates. Terms of trade are more variable after 1973, but the data do not suggest that the increased variability is mainly the result of the exchange rate regime.

Further, I compare levels and changes in real exchange rates to the values predicted by a model.

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[13]Hakkio and Rush (1991) reach the same conclusion based on more formal tests.

[14]The hypothesis implies and the data suggest that the appreciation and depreciation in this period is mainly the result of policy action.

The forecast errors do not give evidence of large, persistent errors. On the contrary, the models call the turning points in the level and changes in the exchange rate with considerable accuracy. The data suggest, however, that there is more unexplained variability of real exchange rates after 1973 than before when measured by the standard error of estimate for the regression equation.

The evidence also suggests that much of the movement in both levels and changes in annual values of the U.S. multilateral real exchange rate is explained by permanent or persistent changes in a few variables. The principal variables are real money balances and the share of defense spending in GDP. When the change in real balances is separated into variables measuring the current change in nominal money and the current change in the price level, the data suggest that the change in nominal money (measured at past prices) has a more important short-run effect. Quarterly data on levels of the variables support the principal findings.

Monetary and fiscal variables are nested within a random walk model of the real exchange rate. If the random-walk component dominated the exchange rate, the data would reject the relevance of the monetary and fiscal variables. The tests based on annual and quarterly data and on annual changes support the opposite conclusion: monetary and fiscal effects are persistent and reliable, and their effect is contemporaneous—within the current year or quarter. Of course, none of the findings here deny that the random walk may dominate levels or changes of the exchange rate at higher frequencies.

Two principal observations about fluctuating exchange rates during the past 20 years are: (1) the close relation between real and nominal exchange rates and (2) the sharp appreciation and subsequent depreciation of the real dollar exchange rate in the 1980s. I conjecture that the principal reason for the correspondence between movements in real and nominal exchange rates is that real exchange rates are driven by contemporaneous permanent changes in real variables, particularly real defense spending and real money balances, whereas nominal exchange rates are driven by the nominal values of the same variables. Much of the short-term effect of money on the real exchange rate appears to be the result of changes in nominal money, so it would not be surprising to find that changes in nominal money balances have a significant effect on the nominal exchange rate also.

DATA APPENDIX

Nominal exchange rate (FNER): Index of the trade-weighted foreign exchange value of the United States dollar compiled by the Federal Reserve. The index is a geometric average of 10 industrialized countries' dollar value of their currencies weighted by their shares of world trade between the years 1972 and 1976. The 10 countries are Germany, Japan, France, United Kingdom, Canada, Italy, Netherlands, Belgium, Sweden and Switzerland.

Trade-weighted price level (TWCPI): Geometric average of 10 industrialized countries' consumer price indexes weighted by their shares of total world trade.

Real exchange rate: FNER deflated by the ratio of the United States consumer price index (CPI) to the 10 countries' trade-weighted CPI.

Real money balances (m): United States M1 monetary aggregate deflated by the United States CPI.

Defense spending share (d): Ratio of the United States defense spending in current dollars to GDP in current dollars.

Foreign money balances (TWNM): Arithmetic average of indexes of M2 monetary aggregates of Canada, Germany, Great Britain, and Japan (M2 & CD), weighted by their shares of total world trade between the years 1972 and 1976.

Foreign real money balances: TWNM deflated by TWCPI.

Real government net worth: Real government deficit measured by the US government real net worth from Bohn (1992).

Relative price of oil: Oil price measured by composite refiners' acquisition cost deflated by GNP deflator.

Real federal debt: Gross federal debt net of Federal Reserve holdings deflated by the CPI.

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Commentary

ALLAN MELTZER HAS PRESENTED a paper that rescues Milton Friedman's 1953 defense of flexible exchange rates from its critics and presents an impressive amount of empirical evidence against today's received opinion favoring fixed exchanges. Nothing is more bracing than the refutation of conventional wisdom nor more satisfying than the vindication of the simple faith that the market is always right against the latest interventionist fads of central bankers, which is what I take to be the practical consequence of the paper. Anything that could make Europeans think again about imposing a contrived monetary union in the EC is most welcome.

My comments start with Meltzer's summary of Friedman's case for floating rates. Then I make a quick evaluation of the force of the data presented in refutation of those who have asserted that flexible exchange rates have not worked as anticipated. I make this evaluation without in any way claiming, however, to have redone Meltzer's calculations or amassed some different evidence. The reason for not focusing on the empirical part of Meltzer's paper, except as evidence of how the world seems to function, becomes apparent in the following section. After a simple-minded exposé of what Meltzer's results mean for the day-to-day business of a central banker and a portfolio manager, I try to contrast the empirical relations given in Meltzer's paper with the assumptions implicit in the project for a European Monetary Union. Finally, I reflect on the conditions that explain why monetary zones appear and on whether the benefits of a freely floating independent currency become larger than the costs.

Meltzer underlines how Friedman refrained from claiming too much for floating exchange rates and also how prescient he was about problems that would plague fixed exchanges at the time when the Bretton Woods accord was being implemented.

Friedman, as I have confirmed by rereading his 1953 paper, did not present flexible exchange rates as optimal under all circumstances. He started by defining the new arrangement he wanted to criticize:[1]

> the Western nations seem to be committed to a system of international payments based on exchange rates between their national currencies fixed by governments and maintained rigid except for occasional changes to new levels.

He then prudently said that "whatever may have been the merits of this [Bretton Woods] system for another day it is ill suited to current economic and political circumstances." If the fixed exchange system must have had some merit when it was being used within currency zones—some as large as the one in which he lives—for Friedman, the flexible rates system had the superior political consequences, namely: (a) fostering the liberalization of trade; (b) reducing the need for exchange controls; (c) easing the path for necessary extraordinary expenditures, such as the rearmament that turned out to be necessary with the Cold War; and (d) alleviating the constant brushes between central bankers and treasuries over domestic monetary and fiscal policies.

[1]See Friedman (1953).

These indirect advantages are important and correspond contrastingly with the political advantages claimed by those who defend pegging exchanges of countries or a group of countries to some standard or to some more reliable currency. In this case the political advantage lies in putting a check on the oversupply of money and forcing monetary authorities to minimize their inflation tax and maintain the value of money. An exchange fixed onto gold, some basket of goods or another reliable currency such as the deutsche mark is a sort of superindependence clause in the central bank bylaws because it turns the Bank in effect into a currency board.

In Meltzer's paper there is little discussion of possible data related to these elements of comparative advantages of flexible vs. fixed exchange rates from the political and social point of view. In his paper there is a useful explanation of what Friedman really meant, but not an empirical evaluation of the relative size of the functional relationship he posited. The elements of a comparative analysis of floating exchange rates vs. fixed are listed by Meltzer, but their quantitative importance is not evaluated.

> Recognizing that optimality of flexible rates cannot be established, Friedman limits his claim to the judgment that flexible exchange rates are more desirable socially than the...alternative means of offsetting changes in international position... (1) official changes in currency reserves; (2) changes in domestic price levels and incomes; (3) periodic realignment of parities; and (4) direct controls.

Of these, periodic realignments are the most important for a judgment on the functioning of the European Monetary System (EMS).

I found a remark in Meltzer's conclusion that "several of ...[the four] conjectures were correct." These conjectures are such things as the positive political effects on the liberalization of trade. Regarding Friedman's (b) avoidance of direct controls, Meltzer says the following:

> Direct controls on capital movements have been reduced since 1973 in all developed countries and in some developing countries.

With respect to Friedman's (c), Meltzer notes the following:

> It seems likely that rearmament (defense spending) would have provoked greater conflict about payments imbalances in the 1980s under fixed exchange rates than under the system that prevailed.

Finally, there is the following indirect treatment of the possible evidence on Friedman's (d), the harmonization of internal monetary and fiscal policies:

> Flexible rates permitted countries to choose how much of the stimulus emanating from the United States they wished to absorb. Many countries, indeed most developed countries, both purchased dollar securities and appreciated their currency.

that is, they both absorbed and sterilized the stimulus.

This is very little on a large part of the controversy about fixed vs. flexible exchange rates, but it will have to be a topic for a different paper because Meltzer prefers to concentrate on a previous problem that Friedman dealt with implicitly, though at length. Says Meltzer:

> The point that concerned later critics most, variability or instability, is dismissed early with the claim that exchange rate instability reflects instability in the economy and is not a property of a flexible or floating rate system . . Friedman appears to have anticipated this outcome. He devotes more space to refuting or dismissing the charge of instability than to making the positive case for the four benefits claimed for flexible rates.

This is also what Meltzer does, in the belief that the question of overshooting and of destabilizing speculation has to be resolved before the political cost-benefit analysis of flexible rates can be addressed.

The evidence Meltzer does present bears on five points that would clearly be important as preconditions for evaluating Friedman's main political theses: (1) whether the variability of exchange rates in the main OECD countries increased after the breakdown of Bretton Woods—especially whether the EMS currencies showed less variability; (2) whether and over what period can exchange rates be considered to move along a random walk; (3) connected with 2., whether money supply and government spending policies do affect real and money exchange rates significantly; (4) connected with 3., why real and monetary rates seem to move together; and (5) whether exchange rates are congenitally unstable, whether they show a tendency to return to mean values and if so, over what period.

The results presented by Meltzer are most valuable and should become standard with the profession if confirmed by rerunning them for

different periods and countries, and especially for the present episode of instability in the EMS.

VARIABILITY AND ITS POSSIBLE WELFARE EFFECTS

The figures presented for 1973 through to 1990 moved over a relatively wide range. This behavior of the exchanges could, as Friedman said, turn out to reduce the variability in some real phenomena, such as output or employment. The variability could also derive from more acute and frequent real shocks in this period.

This is as may be, but Meltzer concentrates on whether the variability is merely apparent and is robust under different measures used. If the said variability could be traced to the more frequent movements in a flexible regime but left relative prices unmoved, the visible variability could have only small real consequences. See table 1: *R*, the real exchange rate, and N, the nominal rate, varied much less in the EMS countries than in other OECD countries. (One would in any case want to see the variance after what happened on September 26.) The relative price changes between countries, however, were unaffected by the exchange regime.

THE RANDOM WALK MODEL

At the end of his paper, Meltzer has a section on the possible limitations of his empirical findings. One is possible simultaneity, which he corrects by lagging and for which he finds no evidence of relevance. Another is that the results are not derived from a structural model, but from a simple model of permanent and transitory changes which is, by the way, a traditional Friedmanite approach. Meltzer maintains that the whole empirical exercise "nests the effects of money and defense spending within a popular statistical model, the random walk," so significant departure from the null hypothesis would precisely be a most striking refutation of the random walk theory of foreign exchanges.

I can summarize the results with a quotation: "transitory random variation increased under flexible rates, but the increase is much smaller than is commonly alleged"; and a move from annual to quarterly data does not significantly change the results. Daily data would, however, probably show much more randomness.

REAL CAUSES

And the yearly and quarterly movements of exchange rates are not random because

> changes in money and changes in defense spending relative to GDP have considerable effect. For example, a 0.1 percentage point change in the share of defense spending changes the real exchange rate between 1.4 percentage points and 2.4 percentage points. ... The 1982 increase in defense spending alone appreciated the dollar by 8.7 percentage points.

Not only government expenditures, but also increases in real GDP and changes in real money balances seem to have significant effects on the real rate of exchange. Within the black box, we could surmise that increases in expenditures will contribute to raising interest rates and attracting foreign capital and that increases in GDP will also lead to higher rates through the same mechanism and through the increased demand for money. On the other hand, a fall in real money supply will also push up the real exchange rate. Deficits, on the other hand, seem to have no significant effect of the real (and money) rates.

REAL AND MONEY EXCHANGE RATES

David Ricardo in 1817, in the passages of his *Principles* where he discussed the distribution of precious metals in the world, under the gold standard, saw that advances in productivity in a country led first to a fall in costs and real prices, then to an accumulation of reserves and an increase in money prices, and finally for a time to an overvaluation of the money exchange rate until domestic prices fell to an equilibrium. The process would be the inverse for a country falling back in productivity. Hence the tendency of real and money exchanges to fluctuate constantly in separate directions turned out to be characteristic of a fixed exchange rate regime. (Of course, it is contrary to the rules of a fixed interest regime, especially of the gold standard for the bank to sterilize foreign funds.)

In contrast, it was therefore expected that, under a flexible exchange regime, because the inflow of money from increased productivity and exports does not go into reserves but into foreign exchanges, the index of money prices would be governed much more directly by the prices of tradeable goods.

Meltzer gives an additional reason for the joint movement of *real* and nominal rates. He has shown reductions in real balances to be a powerful cause of the increase of real exchange rates: and real balances also govern money exchange rates.

NON-STATIONARITY

One last element in the description of a flexible exchange rate regime is the rejection of non-stationarity by Meltzer. That there are observably persistent departures from a random walk led some authors to think that speculation could be permanently destabilizing. However, if one uses a longer span of years it becomes clear that exchanges are subject to both persistent and transitory changes. I take it that the persistent changes are responsible for the time illusion of non-stationarity. If the average real life of a shock is 3 years, the period for return to the mean rate of exchanges can be long: but return to the mean they do (I should add, if there are no capital or trade controls).

Though it may be subject to correction from further empirical research, the picture of the exchange world given by Meltzer's empirical research is striking, both for the central banker and the investor. I read these provisional conclusions with some trepidation, but hope to be corrected by the audience before I become a central bank governor or a large investor.

First, real and money exchanges move together. Price indexes will move up or down with the real exchange rate and will be governed by the real causes of real exchange appreciation or depreciation. In an open economy, therefore, a central banker can aspire to a steady or zero inflation rate only as an average over a long period—perhaps a three-year half-life.

Second, there is money to be made in foreign currency (at least until everybody starts reading Meltzer) because of long-term systematic and predictable forces in the foreign currency market. Government expenditures, GDP growth and reductions in real balances portend of revaluations to come (as long as people do not expect the Government to inflate the accumulated debt away). To put it in another way, a good long-term investment in a country blessed with a central bank that does not panic can discount exchange fluctuations if it has enough capital or is not subject to quarterly scrutiny at the stock market.

Third, the variability observed when exchanges float does not seem to spill over into the goods and services market because it does not affect relative prices. Hence the decision to float or to fix will have to be taken on sociopolitical grounds and cannot be settled on evidence of persistent overshooting.

Now given all this, the arguments with which the monetary part of the Maastricht Treaty is being defended begin to sound less convincing. The following pros and cons are usually presented.

The reduction in transaction costs from having to deal in a single currency is a benefit. Cecchini has calculated a once-and-for-all gain equivalent to 4 percent of European GDP. This may be exaggerated and is much lower than the recurrent gain from the single market.

Another benefit is the control of the central bankers of the constituent states by a European Central Bank (ECB) with the express duty to defend the value of the single currency. This has the following two drawbacks, however. The first is the suspicion that the states' central bankers do not want to reduce their sovereignty, but want rather to increase it with their seat on the ECB's executive committee because the markets themselves have made state central banks lose much of their freedom; the other drawback is that the new ECB will have quite a task being independent and refraining from playing with the exchanges, as can be guessed by the pressures recently put on the Bundesbank.

The solution to the difficulties posed by a monetary union among widely differing countries is problematic. We have already seen the points that Friedman foresaw would plague such a monetary union: policy disharmonies and the possible pressure for a central government (a sure cause of friction in Europe); a temptation to impose exchange or capital and trade controls; and indifference to implementing cushions to prevent unemployment in the less productive parts of the union.

The question is then, why do I sometimes advocate a currency board for small countries, which is a strict form of monetary union, and why are the monetary unions made up of rather large countries and sometimes of a large economy such as Germany and its close surrounding trade partners?

Let us imagine a world of competing monies that float against each other. Their market share will be decided as in any other oligopolistic industry, the producers obtaining a seignorage or markup over marginal cost, a markup limited by potential entry; and demand being for the well-known services that money provides. These services are: for transactions (of which a part is coinage for small change subject to the metal content being of less than face value); for pricing goods, services and savings; and for holding a real cash balance.

The picture that emerges from this is not only that of a world divided horizontally in zones, but also a world subject to a division in layers, where different currencies may be used for different purposes: for example, deutsche marks or dollars for trade, Swiss francs for pensions, and pesetas for local payments.

Apparently it would be ideal for consumers of money, especially those that need it to produce goods and services, if all dealings could be in only one currency. This in fact is not necessarily so, for all the reasons we have noted in this commentary. The union could, however, be approximated by the market, and the study of the non-stationarity hypothesis will confirm that over long periods currencies tend to stay around their historical rates. If there were monetary competition, I doubt that there would be more than three currencies circulating in the United States. The smaller nations around Germany that trade intensely with her and that have similar economic structures to her—for example, the Benelux nations, Austria and Switzerland—will find it in their interest to stick to the deutsche mark. Only competition will tell how big monetary zones must be.

Avoiding hyperinflation and enjoying the services of a currency that is reasonably stable for purposes of valuing goods, services and savings may lead some people to ask that the issuer of money be separated from the creator of the budget deficit as they have recently been in the Baltic States and the Ukraine. In other places, such as Hong Kong, a currency board that pegs the local money to the dollar may inspire confidence in a highly volatile situation.

In questions of currency we live very much in a second-best world.

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The Gold Standard, Bretton Woods and Other Monetary Regimes: A Historical Appraisal

INTRODUCTION

Two Questions

Which international monetary regime is best for economic performance? One based on fixed exchange rates, including the gold standard and its variants? Adjustable peg regimes such as the Bretton Woods system and the European Monetary System (EMS)? Or one based on floating exchange rates? This question has been debated since Nurkse's classic indictment of flexible rates and Friedman's classic defense.[1]

Why have some monetary regimes been more successful than others? Specifically, why did the classical gold standard last for almost a century (at least for Great Britain) and why did Bretton Woods endure for only 25 years (or less)? Why was the EMS successful for only a few years?

This paper attempts to answer these questions. To answer the first question, I examine empirical evidence on the performance of three monetary regimes: the classical gold standard, Bretton Woods, and the current float. As a backdrop, I examine the mixed regime interwar period. I answer the second question by linking regime success to the presence of credible commitment mechanisms, that is, to the incentive compatibility features of the regime. Successful fixed-rate regimes, in addition to being based on simple transparent rules, contained features that encouraged a center country to enforce the rules and other countries to comply.

The Issues

These questions touch on a number of important issues raised in economic literature. The first is the effect of the exchange rate regime on welfare. The key advantage of fixed exchange rates is that they reduce the transactions costs of exchange. The key disadvantage is that in a world of wage and price stickiness the benefits of reduced transactions costs may be outweighed by the costs of more volatile output and employment.

Helpman and Razin (1979), Helpman (1981) and others have raised the welfare issue. This theoretical literature concludes that it is difficult to provide an unambiguous ranking of exchange rate arrangements.[2]

Meltzer (1990) argues the need for empirical

[1]See Nurkse (1944) and Friedman (1953).

[2]See DeKock and Grilli (1989 and 1992).

measures of the excess burdens associated with flexible and fixed exchange rates—the costs of increased volatility on the one hand compared with the output costs of sticky prices on the other hand. His comparison of EMS and non-EMS countries in the postwar period, however, does not yield clear-cut results.

Earlier literature comparing the macroeconomic performance of the classical gold standard, Bretton Woods and the current float also yielded mixed results. Bordo (1981) and Cooper (1982) showed that the classical gold standard was associated with greater price level and real output volatility than post–World War II arrangements for the United States and United Kingdom. On the other hand, Klein (1975) and Schwartz (1986) presented evidence that the gold standard provided greater long-term price stability than did the post–World War II arrangements.[3]

Bordo (1993) compared the means and standard deviations of nine variables for the Group of Seven countries under the three regimes, as well as the interwar period.[4] According to these measures, the Bretton Woods convertible period from 1959 to 1970 was the most stable regime for the majority of countries and variables examined. Eichengreen (1992a) measured volatility applying two filters (the first difference of logarithms and a linear trend).[5] Comparing Bretton Woods and the float for a sample of 10 countries, he found no clear-cut connection between the volatility of real growth and the exchange rate regime. He also found no significant difference in the correlation of output volatility across countries between the two regimes.

A second issue is whether the exchange rate regime provides insulation from shocks and monetary policy independence. Under fixed rates, coordinated monetary policy may provide effective insulation from common supply shocks, but not from country-specific shocks. Under flexible rates, country-specific shocks can be offset by independent monetary policy.[6]

The evidence on this issue is limited. Bayoumi and Eichengreen (1992a) applied the Blanchard-Quah approach to show that both supply (permanent) and demand (temporary) shocks, for a sample of five countries, were considerably greater under the gold standard than under post–World War II regimes. However, they found little difference in the incidence of shocks between Bretton Woods and the floating exchange rate regime. Their results also showed that the dispersion of shocks across countries was higher under the gold standard than under the two more recent regimes and slightly higher under Bretton Woods than under the floating exchange rate regime. They attributed the ability of the gold standard to withstand greater shocks to evidence of a faster speed of adjustment of both prices and output, as measured by impulse response functions.[7]

A third issue is the case for rules vs. discretion. A fixed exchange rate may be viewed as a commitment mechanism or rule. It binds the hands of policymakers to prevent them from following inflationary discretionary policies.[8] The monetary authority, in a closed economy or under flexible rates, might be tempted to engineer an inflation surprise to raise revenue.[9] The outcome is higher inflation because the public, assuming rational expectations, will anticipate the policy. Were some credible mechanism, such as a monetary rule, in place the expansionary policy would not be implemented. Alternatively, a commitment to a fixed exchange rate through a pledge to maintain gold convertibility, for example, could achieve the same results, but because it is more transparent, it would possibly cost less.[10] Such binding commitments may, however, be undesirable in the presence of extreme emergencies such as major wars, supply shocks or financial crises.[11] Under such circumstances

[3]This result is disputed by Meltzer and Robinson (1989).

[4]The Group of Seven countries are Canada, France, Germany, Italy, Japan, the United Kingdom and the United States.

[5]Eichengreen followed the methodology of Baxter and Stockman (1989).

[6]Similarly a monetary union such as the proposed European Monetary Union could provide effective insulation from common supply shocks for its members. However, giving up monetary independence imposes additional burdens in the case of member-specific (regional) shocks, Feldstein (1992).

[7]Addressing the issue of the optimum currency area, Bayoumi and Eichengreen (1992b, 1992c and 1992d) also apply this methodology to examine the incidence of shocks within Western Europe and within regions of North America.

[8]See Kydland and Prescott (1977), Barro and Gordon (1983), and Persson and Tabellini (1990).

[9]Alternatively the monetary authority may create an inflation surprise to offset a labor market distortion that raises the unemployment rate above some desired level.

[10]See Giavazzi and Pagano (1988).

[11]See Rogoff (1985a) and Fischer (1990).

a contingent rule, or one with escape clauses that allow member countries to suspend parity (convertibility) temporarily, may be optimal.[12]

The rule constrains the government to adhere to the fixed exchange rate except in the case of a well-understood emergency, when it can suspend parity (convertibility under the gold standard) and issue fiat money. Once the emergency has passed, with allowance for a suitable delay, the authority is expected to return to the rule—that is, to the fixed rate at the original parity. If the public believes in the government's commitment to return to the rule, the government will be able to raise more revenue than it could with no credibility. The inflation rate during the emergency would be higher than under the rule (when presumably it would be zero) but less than in the case of pure discretion. The pattern of alternating fixed and floating exchange rate regimes over the past 200 years may be well explained by adherence to a rule with an escape clause.[13]

On the other hand, in a regime of floating exchange rates the inflationary bias of discretionary policy may be overcome by instituting credible monetary rules or other commitment mechanisms, such as an independent conservative central bank.[14] Such mechanisms may prevent the perceived disadvantage of sacrificing national sovereignty to the supernational dictates of a fixed exchange rate.

A fourth issue is that of international cooperation and policy coordination. Recent game theory literature has demonstrated that coordination of policies (by fixing exchange rates) can offset spillover effects from uncoordinated policy actions.[15] Cooperative fixed exchange rate arrangements, however, unless enforced by a supernational authority whose power exceeds national sovereignty, tend to break down as individual members devalue. Cooperation is more likely, without a supernational authority, in a world of repeated games because the benefits of reputation can offset the advantages to each country of cheating.[16] But even in this case, cooperation between nations may produce an inflationary bias when no credible commitment mechanism is present to prevent governments from following discretionary policies.[17] Thus for an international monetary arrangement to be effective both between countries and within them, a consistent credible commitment mechanism is required. Such a mechanism likely prevailed under the gold standard but was less evident under Bretton Woods.

A fifth and final issue is the case for international monetary reform. Several, prominent proposals have been made to reform the present managed floating exchange rate regime and move it back toward one of greater fixity. These proposals in part derive from a perception, based on the historical record, that fixed exchange rates are preferable to the current float. These proposals include McKinnon's case for a gold standard without gold, Mundell's proposal to target the real price of gold and the case for target exchange rate zones presented by Williamson and Bergsten.[18] Even more immediate is the move to convert the adjustable peg of the EMS to a unified currency area with irrevocably fixed exchange rates.

Overview

The paper accomplishes a number of tasks. The next section answers the first question, which international monetary regime is best for economic performance, by presenting a compilation of statistical evidence on different aspects of the performance of alternative monetary regimes. The measures cover the stability of several macroeconomic variables; the dispersion of macroeconomic variables across countries; the persistence of inflation; forecast errors in inflation and growth; the incidence of supply (permanent) and demand (temporary) shocks; the dispersion of shocks between countries; and the mean response of prices and output to supply and demand shocks. The third section stresses the importance of adhering to credible rules in a historical examination of three international monetary regimes: the classical gold standard, Bretton Woods and the EMS. The final section answers the question why some regimes endured longer than others. It concludes by discussing why even a regional exchange rate arrangement—the EMS—has considerable difficulty surviving.

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[12]See Bordo and Kydland (1992), Flood and Isard (1989a and 1989b), and DeKock and Grilli (1989 and 1992).

[13]See DeKock and Grilli (1989) and Giovannini (1992).

[14]See Rogoff (1985a).

[15]See Hamada (1979) and Canzoneri and Henderson (1988 and 1991).

[16]See Dominguez (1993).

[17]See Rogoff (1985b).

[18]See McKinnon (1988), Mundell (1992), Williamson (1985a) and Bergsten (1992).

The statistical evidence on performance of alternative monetary regimes in the next section makes it clear that the performance of regimes in the post–World War II era is superior to that of the regimes in the preceding half century. The key exception is the classical gold standard, which exhibits the lowest inflation persistence and a relatively high degree of financial market integration. The Bretton Woods convertible regime from 1959 to 1970 performed the best by far on virtually all criteria.

The greater durability of the gold standard compared with Bretton Woods cannot be explained by a lower incidence of shocks. The key explanation for its success lies with the credibility of the commitment to the gold standard rule of convertibility by England and the other core countries and its near universal acceptance. As a contingent rule, it was flexible enough to withstand the major shocks that buffeted it. The Bretton Woods adjustable peg was in some respects similar to the gold standard contingent rule, but it invited speculative attack hence weakening the escape clause. Unlike England, the leading country before World War I, the United States, the dominant country under Bretton Woods, maintained a credible commitment to a noninflationary policy for only a few years. The world, faced with imported inflation in the late 1960s, lost the incentive to follow its leadership, and the system collapsed in 1971.

The longevity of general floating exchange rate regimes since 1973 suggests that the lessons of Bretton Woods have been well learned. Countries are willing to subject their domestic policy autonomy neither to that of another country whose commitment they cannot be sure of in a stochastic world, nor to a supernational monetary authority they cannot control. Even the recent experience of the EMS—a regional exchange rate arrangement between countries supposedly pursuing common goals—revealed differing national priorities in the face of asymmetric shocks that placed intolerable strains on the system.

THE PERFORMANCE OF ALTERNATIVE MONETARY REGIMES

To make the case for one monetary regime over another, empirical and historical evidence on their performance is crucial. In this section I present some evidence on different aspects of the macroeconomic performance of alternative international monetary regimes over the past 110 years. The comparison for the seven largest (non-Communist) industrialized countries (the Group of Seven countries) is based on annual data for the classical gold standard (1881–1913), the interwar period (1919–39), Bretton Woods (1946–70), and the present floating exchange rates regime (1971–89). The Bretton Woods period (1946–70) is divided into two subperiods: the preconvertible phase (1946–58) and the convertible phase (1959–70).[19]

The comparison relates to the theoretical issues raised by the debate over fixed vs. flexible exchange rates. According to the traditional view, adherence to a (commodity-based) fixed exchange rate regime, such as the gold standard, ensured long-run price stability for the world as a whole because the fixed price of gold provided a nominal anchor to the world money supply. Individual nations, by pegging their currencies to gold, fixed their price levels to that of the world.[20] A fixed-rate system based on fiat money, however, may not provide a stable nominal anchor unless a credible commitment mechanism constrains the growth of the world's money supply.[21] The disadvantage of fixed rates is that individual nations are exposed to both monetary and real shocks transmitted from the rest of the world through the balance of payments and other channels of transmission.[22] The advantage of floating exchange rates is to provide insulation from foreign shocks. The disadvantage is the absence of the discipline of the fixed-exchange-rate rule because monetary authorities might adopt inflationary policies.

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[19] I also examined the period (1946–73) which includes the three years of transition from the Bretton Woods adjustable peg to the present floating regime. The results are similar to those of the 1946–70 period.

[20] The common world price level under the gold standard, however, exhibited secular periods of deflation and inflation reflecting shocks to the demand for and supply of gold. See Bordo (1981) and Rockoff (1984). A well-designed monetary rule, it is argued, could have prevented the long-run swings that characterized the price level under the gold standard. See Cagan (1984).

[21] See Giovannani (1992).

[22] See Bordo and Schwartz (1989a).

Theoretical developments in recent years have complicated the simple distinction between fixed and floating exchange rates. In the presence of capital mobility, currency substitution, policy reactions and policy interdependence, floating rates no longer necessarily provide insulation from either real or monetary shocks.[23] Moreover, according to recent real business cycle approaches, there may be no relationship between the international monetary regime and the transmission of real shocks.[24] Nevertheless, the comparison between regimes may shed light on these issues.

One important caveat is that the historical regimes presented here do not represent clear examples of fixed and floating exchange rate regimes. The interwar period comprises three regimes: a general floating rate system from 1919 to 1925, the gold exchange standard from 1926 to 1931 and a managed float to 1939.[25] The Bretton Woods regime cannot be characterized as a fixed exchange rate regime throughout its history: The preconvertibility period was close to the adjustable peg envisioned by its architects, and the convertible period was close to a *de facto* fixed dollar standard.[26] Finally, although the period since 1973 has been characterized as a floating exchange rate regime, at various times it has experienced varying degrees of management.

Stability and Convergence

Table 1 presents descriptive statistics on nine macroeconomic variables for each Group of Seven country, with the data for each variable converted to a continuous annual series from 1880 to 1989. The nine variables are the rate of inflation; real per capita growth; money growth; short-term nominal interest rates; long-term nominal interest rates; short-term real interest rates; long-term real interest rates; and the absolute rates of change of nominal and real exchange rates. The definition of the variable used, for example, M1 vs. M2, was dictated by the availability of data over the entire period. For each variable and each country I present two summary statistics: the mean and standard deviation. For the countries taken as a group, I show two summary statistics: the grand mean and a simple measure of convergence defined as the mean of the absolute differences between each country's summary statistic and the grand means of the group of countries.[27] I comment on the statistical results for each variable.

Inflation. Countries using the classical gold standard had the lowest rate of inflation and displayed mild deflation during the interwar period. The rate of inflation during the Bretton Woods period was on average and for every country except Japan lower than during the subsequent floating exchange rate period. The average rate of inflation in the two Bretton Woods subperiods was virtually the same. This comparison, however, conceals the importance of two periods of rapid inflation in the 1940s and 1950s and in the late 1960s. See figure 1.[28] Thus the evidence based on country and period averages of very low inflation in the gold standard period and of a lower inflation rate during Bretton Woods than the subsequent floating period is consistent with the traditional view on price behavior under fixed (commodity-based) and flexible exchange rates.

In addition, the inflation rates show the highest degree of convergence between countries during the classical gold standard and to a lesser extent during the Bretton Woods convertible subperiod compared with the floating rate period and the mixed interwar regime. This evidence also is consistent with the traditional view of the operation of the classical price specie flow mechanism and commodity arbitrage under fixed rates and insulation and greater monetary independence under floating rates.[29]

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[23]See Bordo and Schwartz (1989a).

[24]See Baxter and Stockman (1989).

[25]To be more exact, the United States stayed on the gold standard until 1933, and France stayed until 1936. For a detailed comparison of the performances of these three regimes in the interwar period, see Eichengreen (1991a).

[26]Within the sample of seven countries, Canada floated from 1950 to 1961.

[27]This is a very crude measure of convergence or divergence between the different countries' summary statistics. Because it is based on the average for the whole period, it suppresses unusual movements within particular subperiods. Bayoumi and Eichengreen (1992d) presented an alternative measure of convergence or dispersion—the GDP-weighted standard deviation of the individual country series around the G-7 aggregate. I calculated this alternative measure of convergence for the data in table 1. The results are very close to those reported here for virtually every variable.

[28]The data sources for figure 1 and all subsequent figures are listed in the Data Appendix to Bordo (1993).

[29]For similar evidence see Bordo (1981), Darby, Lothian et.al. (1983) and Darby and Lothian (1989).

Table 1

Descriptive Statistics of Selected Open Economy Macroeconomic Variables, the Group of Seven Countries 1881-1989 Annual Data: Mean, Standard Deviation

	Gold Standard (1881-1913)		Interwar (1919-1938)		Bretton Woods (Total) (1946-1970)		Bretton Woods (Preconvertible) (1946-1958)		Bretton Woods (Convertible) (1959-1970)		Floating Exchange (1974-1989)	
Inflation PGNP[1]	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
United States	0.3	3.1	−1.8	7.6	2.4	2.6	2.8	3.5	2.6	1.5	5.6	2.4
United Kingdom	0.3	3.1	−1.5	7.8	3.7	2.2	4.6	2.5	3.4	1.5	9.4	6.1
Germany	0.6	2.6	−2.1	4.7	2.7	4.0	2.1	6.2	3.2	1.8	3.3	1.2
France	0.0	4.9	2.2	9.1	5.6	4.1	5.6	5.1	5.5	3.6	8.8	3.2
Japan	4.6	5.5	−1.7	7.3	4.5	4.6	4.7	7.3	5.1	1.3	2.6	2.4
Canada	0.4	1.4	−1.9	6.1	2.7	3.0	3.9	3.9	2.9	1.5	7.3	2.6
Italy	0.6	3.2	−1.1	11.7	3.8	11.5	5.8	16.0	3.8	2.1	12.9	4.6
mean	1.0	3.4	−1.1	7.8	3.6	4.6	4.2	6.4	3.9	1.9	7.1	3.2
convergence	0.9	1.0	1.0	1.5	0.9	2.0	1.1	3.0	0.9	0.5	2.8	1.2
Real per capita growth[1]												
United States	1.8	5.1	0.0	8.1	2.0	2.8	1.8	3.4	2.9	1.9	2.1	2.7
United Kingdom	1.1	2.4	1.2	4.5	2.1	1.8	2.1	2.2	2.3	1.4	1.5	4.2
Germany	1.7	2.9	2.6	8.5	5.0	3.3	7.3	3.9	3.5	2.6	2.1	1.9
France	1.5	4.6	1.3	7.2	3.9	2.1	4.6	2.7	3.9	1.3	1.7	1.5
Japan	1.4	3.8	2.0	6.1	8.1	2.7	5.7	1.1	8.9	2.5	3.5	1.1
Canada	2.3	2.8	0.2	8.8	2.5	2.6	2.4	3.3	3.5	1.7	1.3	2.4
Italy	1.0	4.0	0.9	4.7	5.6	3.3	5.2	4.4	5.8	1.9	2.5	2.2
mean	1.5	3.7	1.2	6.8	4.2	2.7	4.2	3.0	4.4	1.9	2.1	2.3
convergence	0.3	1.0	0.7	1.5	1.7	0.4	1.7	0.9	1.6	0.4	0.5	0.7
Money Growth[1]												
United States	6.1	4.1	0.6	8.6	6.3	5.8	6.4	8.3	7.0	1.5	8.6	2.4
United Kingdom	2.1	1.7	0.8	4.7	3.2	3.2	1.7	2.9	5.5	2.9	13.5	5.5
Germany	5.7	4.7	1.3	10.1	12.8	5.9	17.6	5.6	10.9	4.7	5.7	4.5
France	2.1	4.7	6.4	8.5	11.5	7.5	14.7	7.2	8.6	6.5	8.8	3.4
Japan	7.2	14.5	0.5	9.7	16.2	16.2	20.1	21.8	14.9	7.0	5.8	6.3
Canada	7.4	5.3	1.1	4.7	6.0	4.0	5.1	3.2	8.3	3.9	10.6	3.9
Italy	3.2	3.1	3.6	6.2	13.3	7.8	15.9	10.5	12.4	2.0	13.4	4.9
mean	4.8	5.4	2.0	7.5	9.9	7.2	11.6	8.5	9.7	4.1	9.5	4.4
convergence	2.1	2.4	1.7	2.0	4.1	3.7	6.2	4.4	2.6	1.7	2.1	1.0

	Gold Standard (1881-1913)		Interwar (1919-1938)		Bretton Woods (Total) (1946-1970)		Bretton Woods (Preconvertible) (1946-1958)		Bretton Woods (Convertible) (1959-1970)		Floating Exchange (1974-1989)	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Short-term interest rate												
United States	4.8	0.9	3.5	2.0	3.4	1.9	2.0	0.9	4.8	1.6	8.9	2.6
United Kingdom	2.8	0.8	3.0	1.8	4.0	2.5	2.3	1.8	5.8	1.6	11.2	2.1
Germany	3.2	0.9	4.6	1.6	4.0	1.5	4.1	1.1	4.0	1.7	5.9	2.4
France	2.5	0.6	3.1	1.4	4.1	1.9	3.2	1.5	5.1	1.9	10.3	2.6
Japan	2.4	0.5	2.0	0.5	6.5	0.8	6.9	1.0	6.3	0.6	5.2	2.0
Canada	n.a.		0.9	0.4	2.9	2.0	1.4	1.1	4.6	1.3	10.3	2.8
Italy	n.a.		n.a.		n.a.		n.a.		n.a.		n.a.	
mean	3.1	0.7	2.9	1.3	4.2	1.8	3.3	1.2	5.1	1.4	8.6	2.4
convergence	0.7	0.2	0.9	0.6	0.8	0.4	1.5	0.3	0.6	0.4	2.1	0.3
Long term interest rate												
United States	3.8	0.3	4.2	0.6	3.9	1.3	3.0	0.4	5.0	1.1	10.4	2.1
United Kingdom	2.9	0.2	4.1	0.7	5.2	1.8	3.9	0.8	6.6	1.3	12.1	2.8
Germany	3.7	0.2	6.8	1.6	6.3	0.7	5.9	0.5	6.7	0.7	7.8	1.5
France	3.2	0.3	4.6	0.8	5.7	0.8	5.8	0.5	5.7	1.0	10.9	2.4
Japan	n.a.		n.a.		7.0	0.1	n.a.		7.0	0.1	7.1	1.8
Canada	3.5	0.4	4.7	0.8	4.5	1.5	3.3	0.5	5.8	1.1	11.0	2.0
Italy	4.2	0.5	5.9	0.6	6.0	0.7	6.3	0.4	5.7	0.7	13.7	3.3
mean	3.6	0.3	5.1	0.9	5.5	1.0	4.7	0.5	6.1	0.9	10.4	2.3
convergence	0.4	0.1	0.9	0.3	0.8	0.5	1.3	0.1	0.6	0.3	1.7	0.5
Real short term interest rate[2]												
United States	4.8	2.0	3.8	6.7	0.3	3.9	−1.6	4.7	2.4	0.4	2.5	2.8
United Kingdom	2.9	2.3	4.2	7.1	−0.1	3.4	−2.4	3.2	2.3	1.1	1.3	5.1
Germany	2.4	2.3	5.1	5.2	2.2	2.6	3.0	3.6	1.6	1.5	2.5	1.9
France	2.8	6.4	1.2	14.7	−0.9	5.2	−3.3	6.9	1.2	1.4	2.1	2.8
Japan	−1.5	5.5	1.4	8.8	1.9	2.5	3.4	3.5	1.1	1.6	1.4	3.5
Canada	n.a.		−0.8	1.3	−0.3	4.2	−2.6	4.8	2.2	0.8	3.1	3.2
Italy	n.a.		n.a.		n.a.		n.a.		n.a.		n.a.	
mean	2.3	3.7	2.5	7.3	0.5	3.6	−0.6	4.5	1.8	1.1	2.2	3.2
convergence	1.5	1.8	1.9	3.0	1.0	0.8	2.5	1.0	0.5	0.4	0.6	0.7

Table 1 (continued)

Descriptive Statistics of Selected Open Economy Macroeconomic Variables, the Group of Seven Countries 1881-1989 Annual Data: Mean, Standard Deviation

	Gold Standard (1881-1913)		Interwar (1919-1938)		Bretton Woods (Total) (1946-1970)		Bretton Woods (Preconvertible) (1946-1958)		Bretton Woods (Convertible) (1959-1970)		Floating Exchange (1974-1989)	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Real long term interest rate[2]												
United States	3.7	2.2	4.6	6.8	0.8	3.6	−0.7	4.4	2.5	0.7	3.9	3.8
United Kingdom	3.0	2.5	5.4	7.1	1.1	2.8	−0.8	2.6	3.2	1.0	2.2	3.7
Germany	2.9	2.4	6.9	6.0	4.3	2.8	4.3	4.4	4.3	0.9	4.4	0.9
France	3.5	6.5	1.0	15.1	0.4	4.4	−1.2	6.2	1.8	1.0	2.7	3.1
Japan	n.a.		n.a.		1.7	1.3	n.a.		1.7	1.3	2.0	4.2
Canada	2.9	4.1	5.4	6.5	1.3	3.8	−0.7	4.4	3.4	0.7	3.8	3.1
Italy	4.2	2.4	3.4	9.3	0.3	9.4	−1.5	12.9	2.3	2.2	1.4	4.8
mean	3.4	3.4	4.5	8.5	1.4	4.0	−0.1	5.8	2.9	1.1	2.9	3.5
convergence	0.4	1.3	1.5	2.5	0.9	1.6	1.5	2.5	0.8	0.4	1.0	0.7
Nominal exchange rate[5]												
United States[4]					0.7	0.8			0.7	0.8		
United Kingdom	0.2	0.2	6.8	7.9	2.4	6.2	3.3	7.9	1.5	4.0	10.0	4.7
Germany	0.2	0.1	3.9	9.5	1.8	3.8	2.4	5.3	1.3	2.1	9.3	8.2
France	0.0	0.0	18.4	15.6	2.5	7.7	4.4	11.3	1.1	3.3	10.7	7.8
Japan	2.9	4.5	6.8	9.0	15.9	37.2	30.5	47.9	0.2	0.2	8.8	9.5
Canada	0.0	0.0	2.7	3.4	1.6	1.9	1.8	1.9	1.4	2.0	3.7	2.4
Italy	1.4	1.5	13.6	20.1	7.4	20.6	14.1	27.4	0.2	0.2	10.9	9.0
mean	0.7	0.9	7.5	9.4	4.6	11.2	8.1	14.5	0.9	1.8	8.9	6.9
convergence	0.9	1.3	4.5	4.2	4.0	10.1	7.7	12.9	0.6	1.2	1.8	2.3

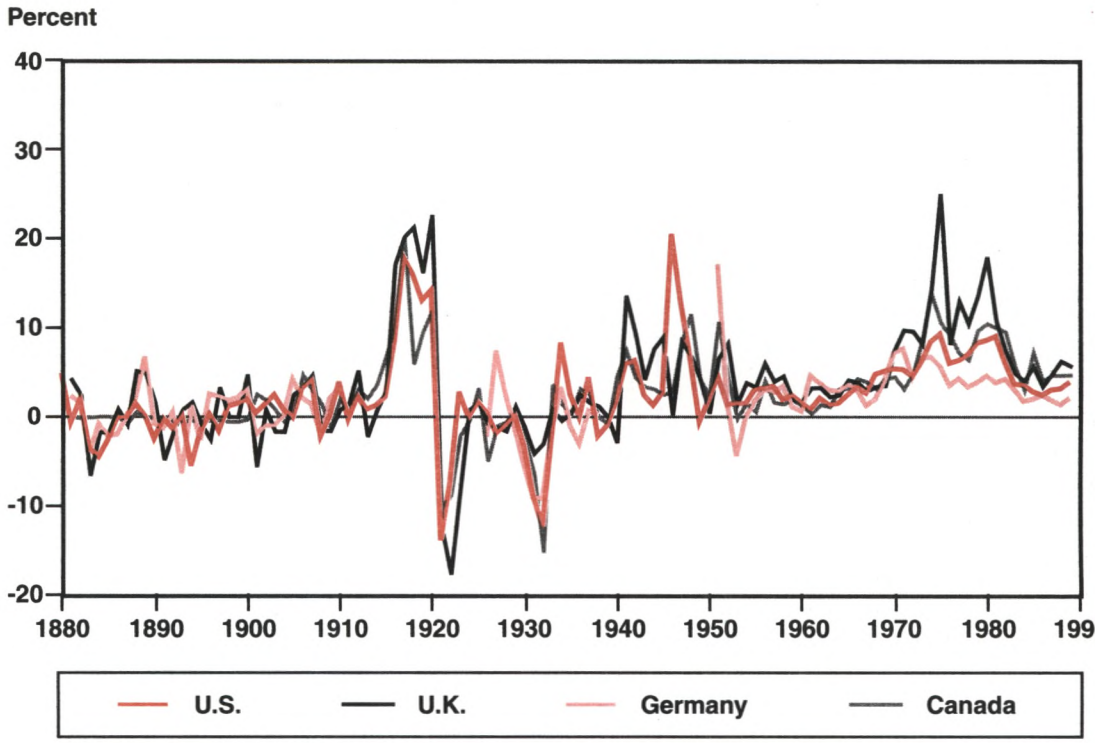
	Gold Standard (1881-1913)		Interwar (1919-1938)		Bretton Woods (Total) (1946-1970)		Bretton Woods (Preconvertible) (1946-1958)		Bretton Woods (Convertible) (1959-1970)		Floating Exchange (1974-1989)	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Real exchange rate[3,5]												
United States[4]					1.7	1.0			1.7	1.0		
United Kingdom	1.7	1.5	8.5	10.0	4.2	6.5	5.9	8.2	2.5	3.9	12.3	6.1
Germany	2.4	1.2	5.8	9.2	2.8	5.1	3.9	7.3	1.9	1.8	8.8	8.2
France	4.3	5.0	9.0	6.9	4.1	5.6	6.2	7.7	2.5	2.9	9.2	7.7
Japan	6.6	5.5	7.8	7.2	3.5	2.9	4.8	4.1	2.7	1.5	9.6	8.9
Canada	2.6	2.2	3.2	2.8	2.4	2.3	3.3	2.5	1.5	1.7	3.8	2.0
Italy	2.1	1.7	13.3	16.9	8.0	18.7	13.1	25.2	2.4	1.6	8.6	7.8
mean	3.3	2.9	7.9	8.8	3.8	6.0	6.2	9.2	2.2	2.1	8.7	6.8
convergence	1.5	1.6	2.3	3.2	1.3	3.8	2.3	5.4	0.5	0.8	1.4	1.8

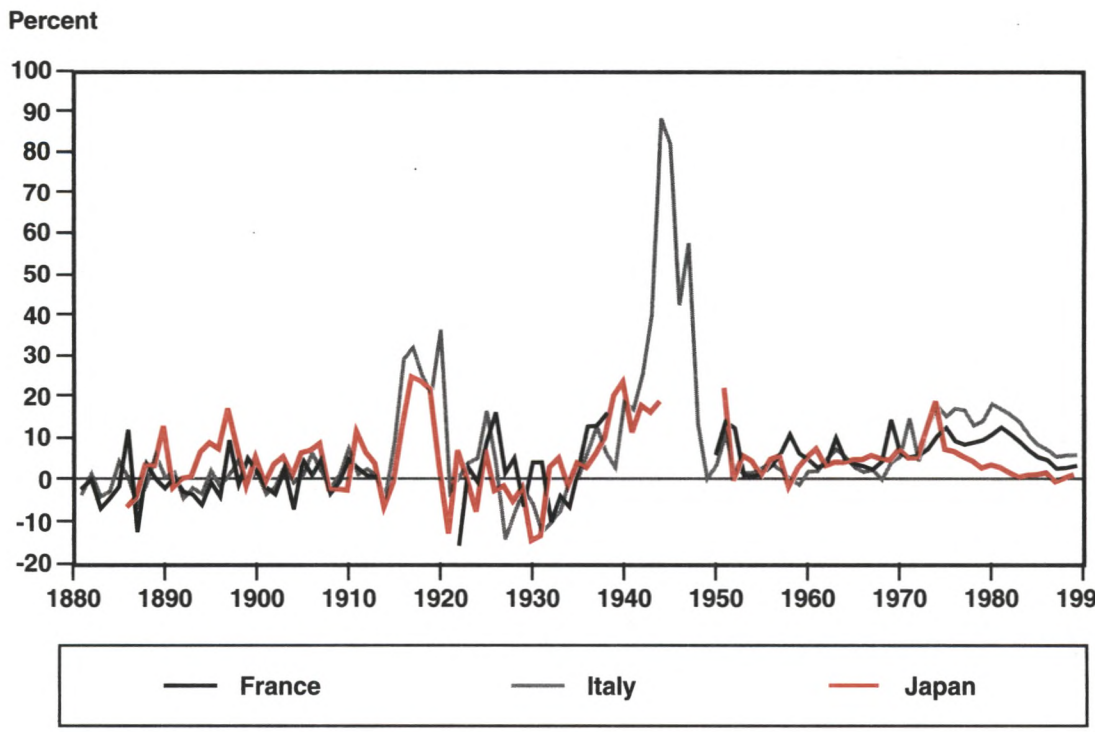
[1]Mean growth rate calculated as the time coefficient from a regression of the natural logarithm of the variable on a constant and a time trend.
[2]Calculated as the nominal interest rate minus the annual rate of change of the consumer price index (CPI).
[3]Absolute rates of change.
[4]Trade-weighted nominal and real exchange rate starting in 1960.
[5]Calculated as the nominal exchange rate divided by the ratio of foreign CPI to the U.S. CPI.

Data Sources: See Data Appendix to Bordo (1993).

Figure 1

Inflation Rates, 1880-1989, G7 Countries





The Bretton Woods convertible subperiod had the most stable inflation rate of any regime as judged by the standard deviation. By contrast, the preconvertible Bretton Woods period exhibited greater inflation variability than either the gold standard period or the recent floating exchange rate period. The evidence of a high degree of price stability in the convertible phase of Bretton Woods is also consistent with the traditional view that fixed rate (commodity-based) regimes provide a stable nominal anchor; however, the remarkable price stability during this period may also reflect the absence of major shocks.

Real per capita GNP. Generally, the Bretton Woods period, especially the convertible period, exhibited the most rapid output growth of any monetary regime, and not surprisingly the interwar period the lowest (see figure 2). Output variability was also lowest in the convertible subperiod of Bretton Woods, but because of higher variability in the preconvertible period, the Bretton Woods system as a whole was more variable than the floating exchange rate period. Both pre–World War II regimes exhibit higher variability than their post–World War II counterparts.[30] The divergence of output variability between countries was also lowest during the Bretton Woods regime, with the interwar regime showing the highest divergence.[31] The greater convergence of output variability under Bretton Woods may reflect conformity between countries' business fluctuations, created by the operation of the fixed exchange rate regime.[32]

Money growth (M2). Money grew considerably more rapidly across all countries after World War II than before the war (see figure 3). There is not much difference between Bretton Woods and the subsequent floating exchange rate regime. Within the Bretton Woods regime, money grew more rapidly in the preconvertibility period than in the convertibility period. Money growth rates showed the least divergence between countries during the fixed-exchange-rate gold standard and the convertible Bretton Woods regime, with the greatest divergence in the preconvertible Bretton Woods period and the interwar period.

Like inflation and real output variability, money growth variability was lowest in the convertible Bretton Woods period. This, however, was not the case for the preconvertible period, which was the most variable of any regime. It also exhibited the greatest divergence in variability between countries. To the extent that one of the properties of adherence to a fixed-exchange-rate regime is conformity of monetary growth rates between countries, these results are sympathetic to the view that the Bretton Woods system really began in 1959.

Short-term and long-term interest rates. The underlying data for short-term and long-term interest rates are seen in figures 4 and 5. As in other nominal series, the degree of convergence of mean short-term interest rates is highest in the convertible Bretton Woods period. Long-term rates are most closely related in the classical gold standard regime, with the convertible Bretton Woods period not far behind. McKinnon (1988) has similar findings. He views them as evidence of capital market integration under fixed exchange rates. The lack of convergence in the preconvertibility Bretton Woods period reflects the presence of pervasive capital controls. Convergence of nominal interest rates would not be expected under floating exchange rates. Convergence of standard deviations is also highest in the gold standard period followed by Bretton Woods. Long-term rates were most stable and least divergent under the classical gold standard, followed by the two Bretton Woods subperiods, with floating exchange rates the least stable. The evidence that nominal interest rates are more stable and convergent between countries under fixed exchange rate (commodity-based) regimes is consistent with the traditional view.

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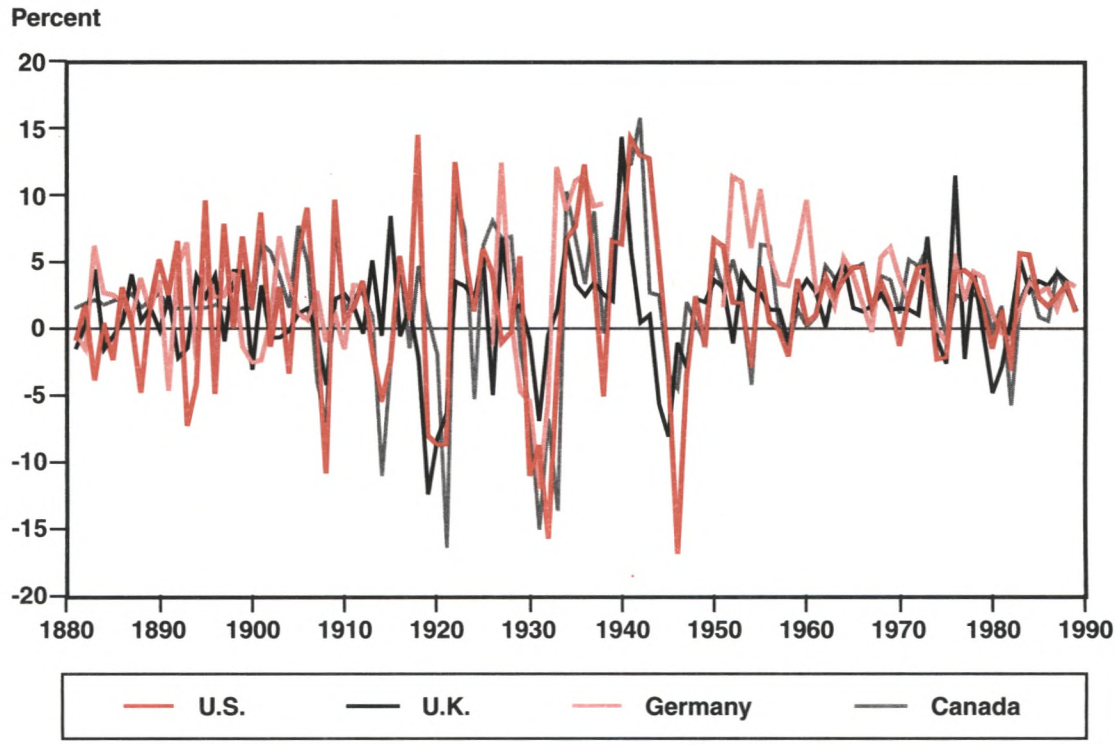
[30]Baxter and Stockman (1989) and Eichengreen (1992a) use residuals from a linear trend to the logarithm of real output as a detrending filter rather than the logarithmic first difference used here. According to their results, real output variability is not greater in the floating than in the fixed period.

[31]However, using their alternative measure of convergence—the GDP-weighted standard deviation of the individual country series around the G-7 aggregate—Bayoumi and Eichengreen (1992a) report that the lowest degree of dispersion of real GDP growth was in the floating rate period, followed by the Bretton Woods convertible period. Similar results hold for the real GNP per capita data in table 1. For Bayoumi and Eichengreen (1992a) the decline in the dispersion of real growth and the rise in the dispersion of inflation rates between the Bretton Woods convertible period and the float have the following explanations: the move to flexible rates allowed countries to stabilize their relative growth rates in the face of asymmetric supply shocks at the expense of their relative inflation rates. They also report that, when they apply the linear trend filter of Baxter and Stockman (1989), evidence of a rise in the cross country correlation between output movements after 1970 is considerably reduced.

[32]See Bordo and Schwartz (1989a) and Darby and Lothian (1989).

Figure 2

Per Capita Income Growth Rates, 1880-1989, G7 Countries



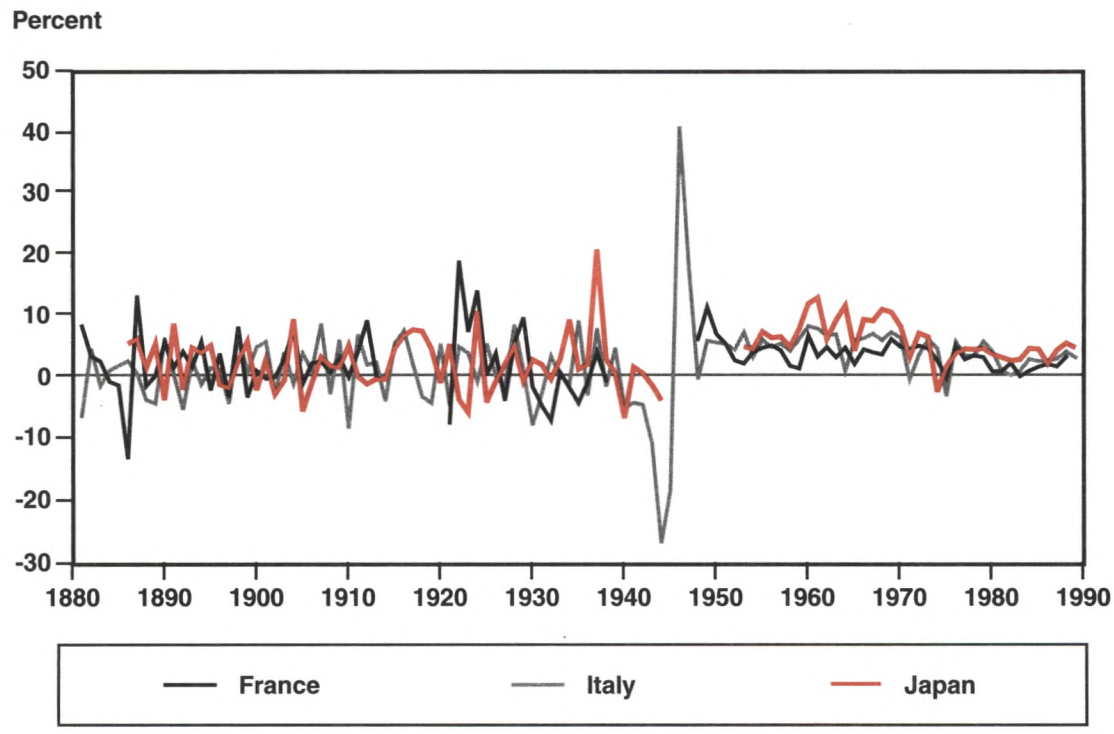
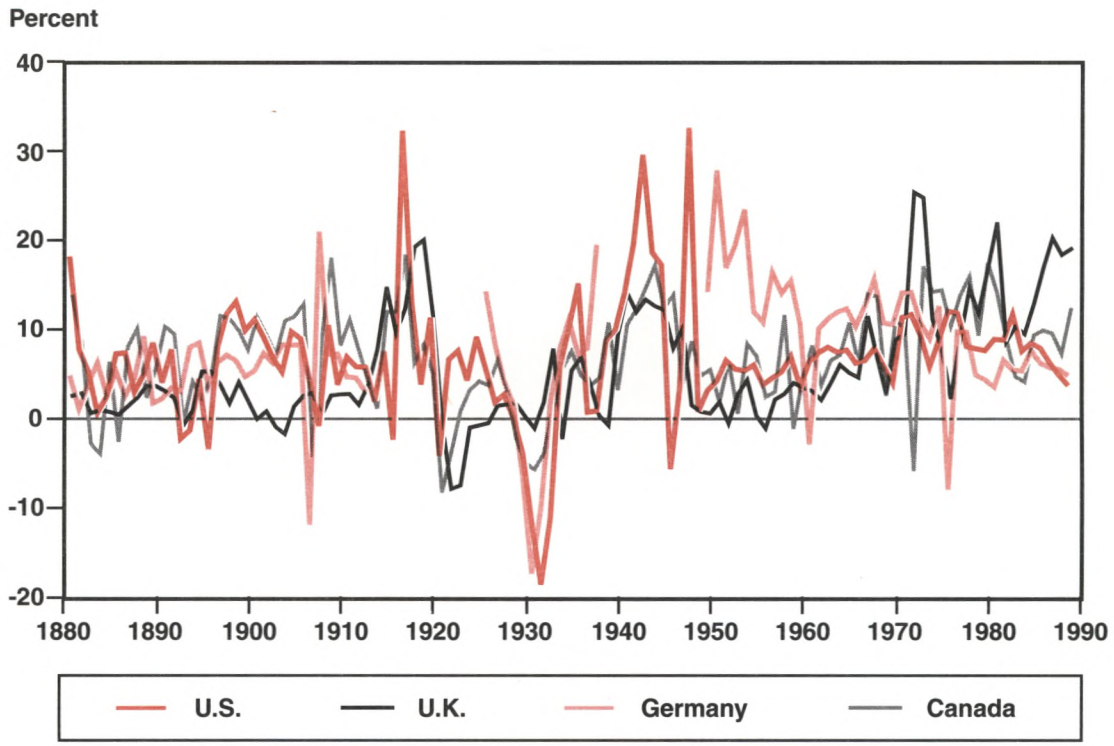


Figure 3

Money Growth Rates, 1880-1989, G7 Countries



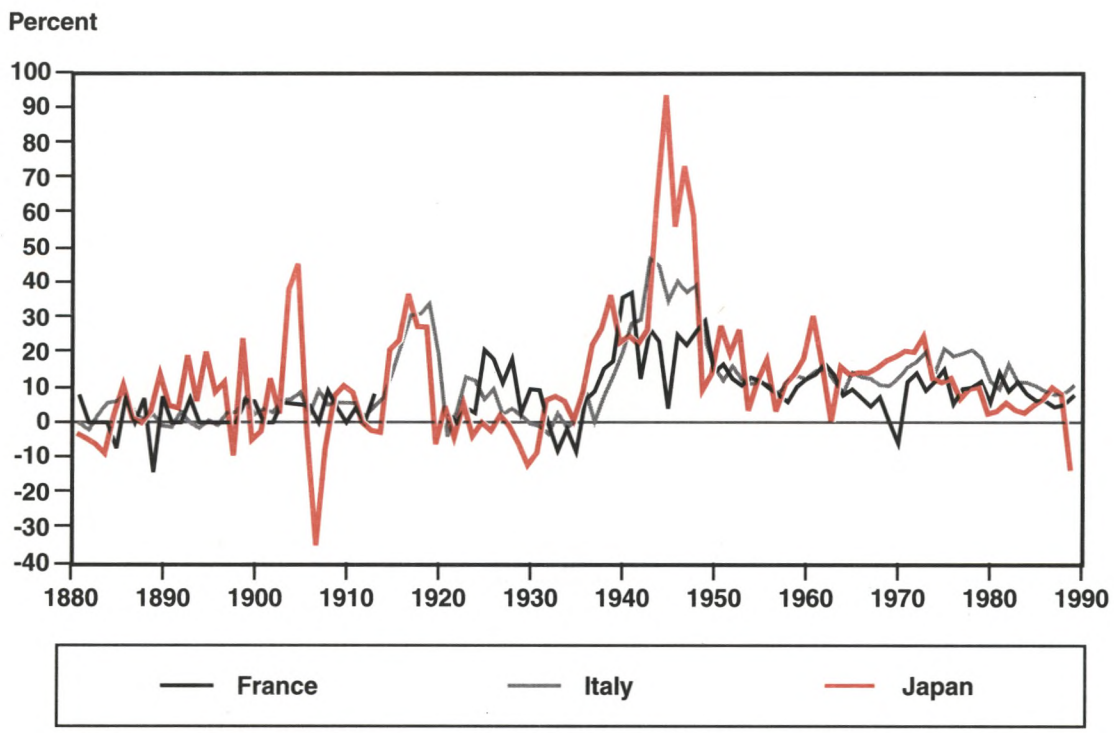


Figure 4

Short-Term Interest Rates, 1880-1989, G7 Countries

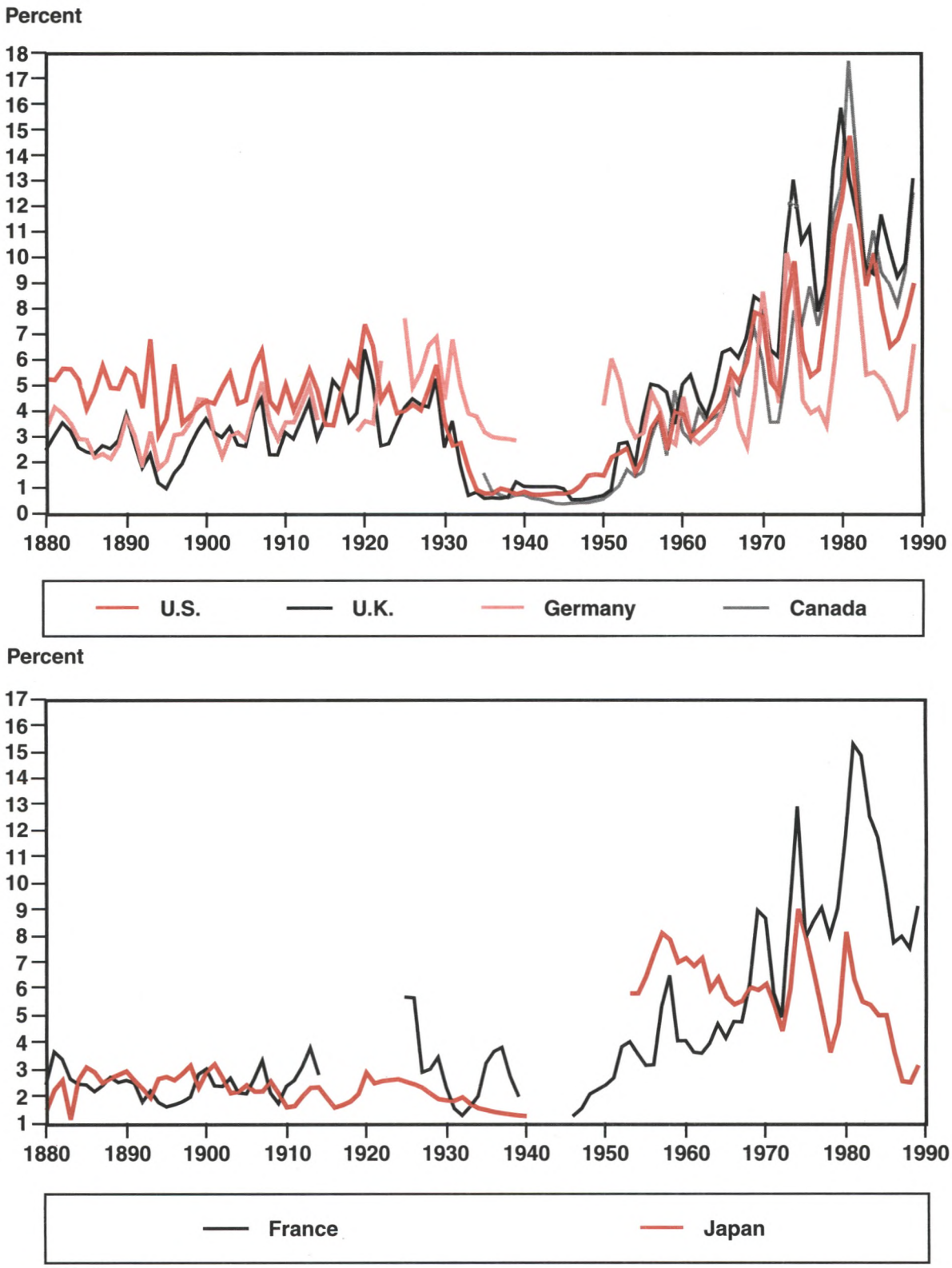
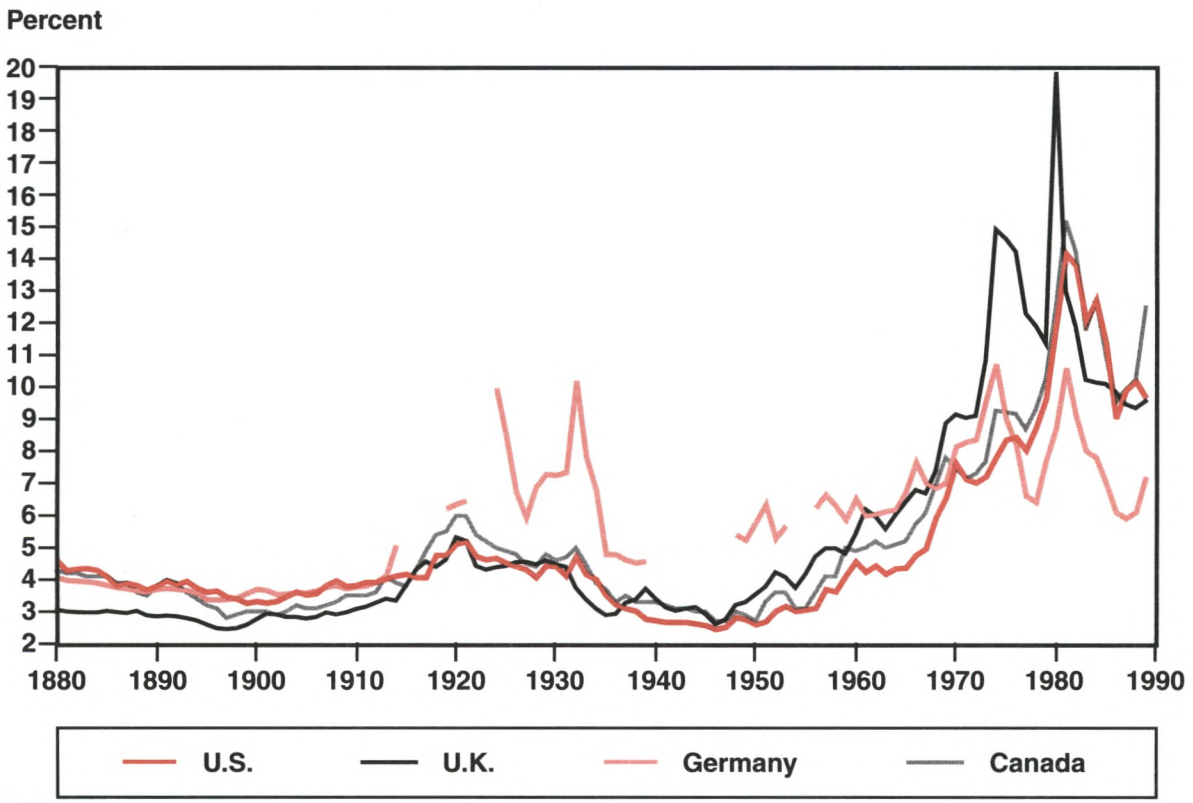
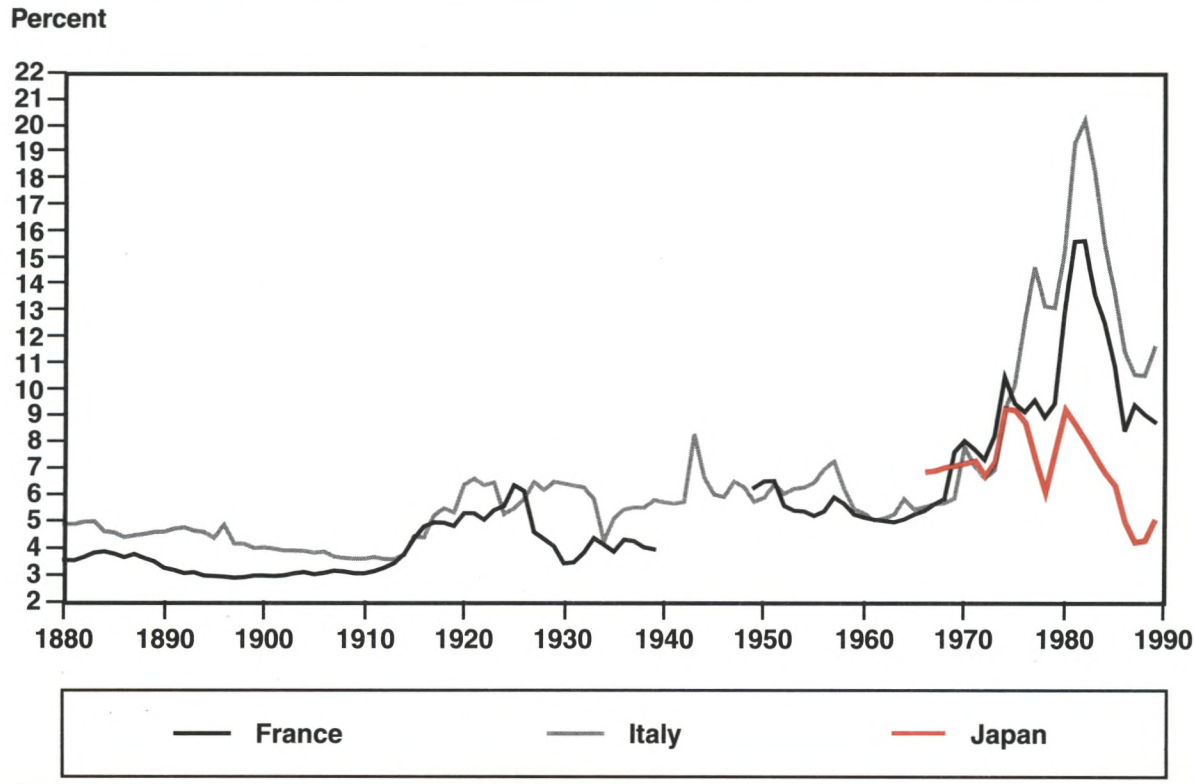


Figure 5

Long-Term Interest Rates, 1880-1989, G7 Countries





Real short-term and real long-term interest rates. For the underlying real short-term and real long-term interest rate data, see figures 6 and 7. The real interest rates are *ex post* rates calculated using the rate of change of a consumer price index.[33] Unlike the nominal series, the degree of convergence in means between real short-term interest rates is lowest in the floating exchange rate period, next lowest in the Bretton Woods convertible period and highest in the preconvertible period. For long-term real rates, as in the case of nominal rates, convergence is highest under the gold standard followed by the Bretton Woods convertible regime. It is lowest under preconvertible Bretton Woods. The real short-term interest rate is most stable across countries during the Bretton Woods convertible period. It also shows the least amount of divergence in standard deviations. The same holds for real long-term interest rates.

The behavior of real interest rates across regimes is consistent with McKinnon's explanation.[34] He argued that fixed exchange rates encourage capital market integration by eliminating devaluation risk. This reduces variability in short-term real interest rates. Similarly, real long-term interest rates are stabilized by pooling across markets, which reduces capital market risk.

Nominal and real exchange rates. The lowest mean rate of change of the nominal exchange rate and the least divergence between rates of change occurred during the Bretton Woods convertible and gold standard periods, with the former exhibiting the lowest degree of divergence. Exchange rates during the preconvertibility Bretton Woods regime changed almost as much as during the floating period. This mainly reflected the major devaluations of 1949 (see figure 8).[35] Nominal exchange rates were least variable in the gold standard and convertible Bretton Woods periods and the most variable and most divergent in the Bretton Woods preconvertible period.

As with the nominal exchange rate, the lowest mean rate of change in the real exchange rate across countries and the least divergence between countries was in the Bretton Woods convertible period, with the divergence in gold standard period next smallest (see figure 9). The highest rate of change was in the floating exchange rate period. Similarly data from the Bretton Woods convertible period had the lowest standard deviation across countries and the least divergence between standard deviations, with the gold standard again next in these rankings. The other regimes were characterized by much greater variability and divergence. These results shed light on the relationship between the nominal exchange rate regime and the behavior of real exchange rates. Mussa (1986) presented evidence for 16 industrial countries in the post–World War II period showing the similarity between nominal and real exchange rate variability under floating rates. His explanation for greater real exchange rate variability under floating rates than under fixed rates is nominal price rigidity.[36] The explanation may, however, be questioned. For example, a fixed nominal exchange rates may produce greater trade stability that will be reflected in the real exchange rate, as is evident for both the Bretton Woods and gold standard periods. Yet as Eichengreen (1991b) points out and as can be seen in table 4, these results could be explained by the fact that both periods were characterized by few shocks.[37]

Finally, based on monthly data between 1880 and 1986 for the United Kingdom and the United States, Grilli and Kaminsky show that, with the exception of the post–World War II period, no clear connection exists between the nominal exchange rate regime and the variability of real exchange rates.[38] My results for the Group of Seven countries show a clear correlation between nominal exchange rate rigidity and lower real exchange rate variability for the gold standard and Bretton Woods convertible regime. For the preconvertible Bretton Woods period–*de jure* a type of fixed exchange rate regime—the correlation is not evident. I do not distinguish between fixed and flexible periods in the interwar segment as do Grilli and Kaminsky,

[33]Define the real interest rate as $r_t = i_t - \Delta\log P_t$; where i_t is the nominal interest rate and $\Delta\log P_t = \log P_t - \log P_{t-1}$ is the percentage change in the consumer price index.

[34]See McKinnon (1988).

[35]See Bordo (1993) table 2.

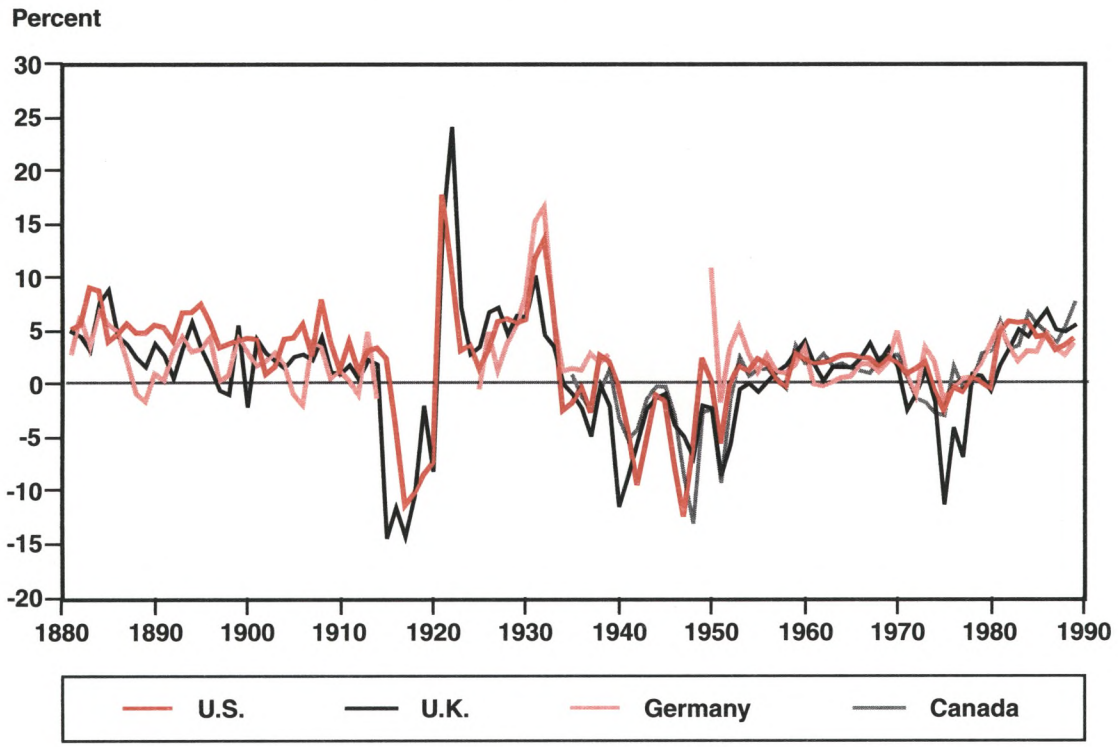
[36]Also see Dornbusch (1976).

[37]Stockman (1983 and 1988) argues that greater variability in real exchange rates under floating rates than under fixed rates reflects the response of real exchange rates to productivity shocks, with changes in the real exchange rate producing nominal exchange rate volatility. This volatility is offset under fixed rates by exchange market intervention.

[38]See Grilli and Kaminsky (1991).

Figure 6

Real Short-Term Interest Rates, 1880-1989, G7 Countries



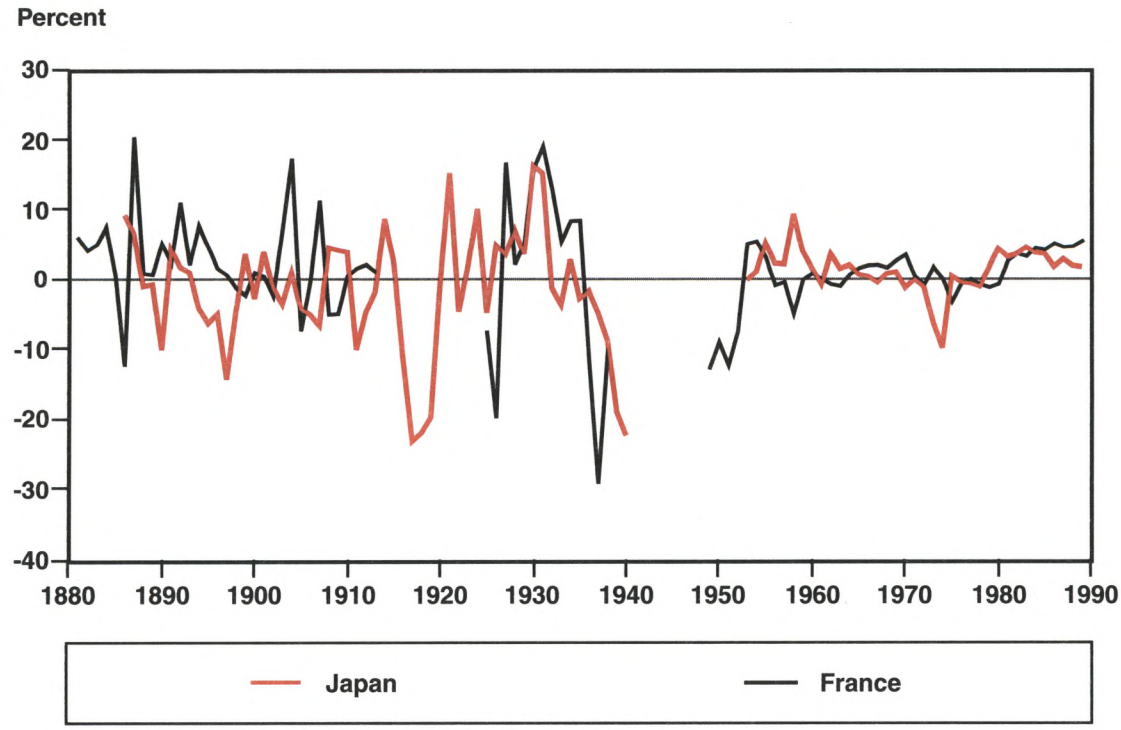
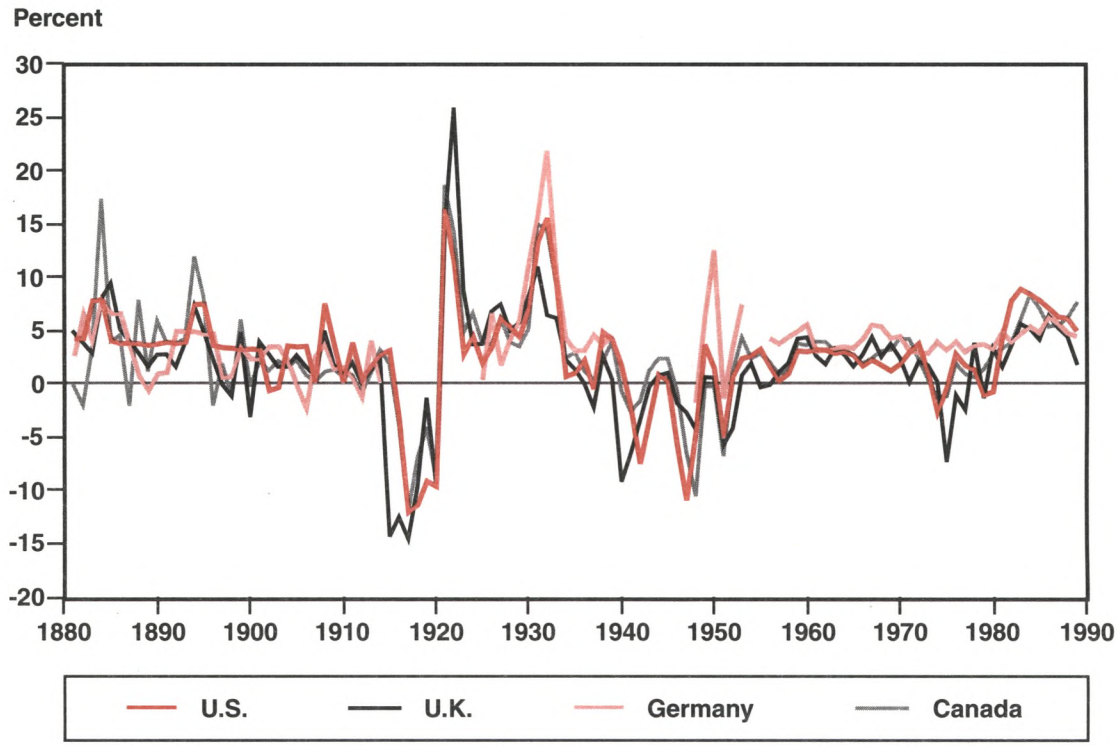


Figure 7

Real Long-Term Interest Rates, 1880-1989, G7 Countries



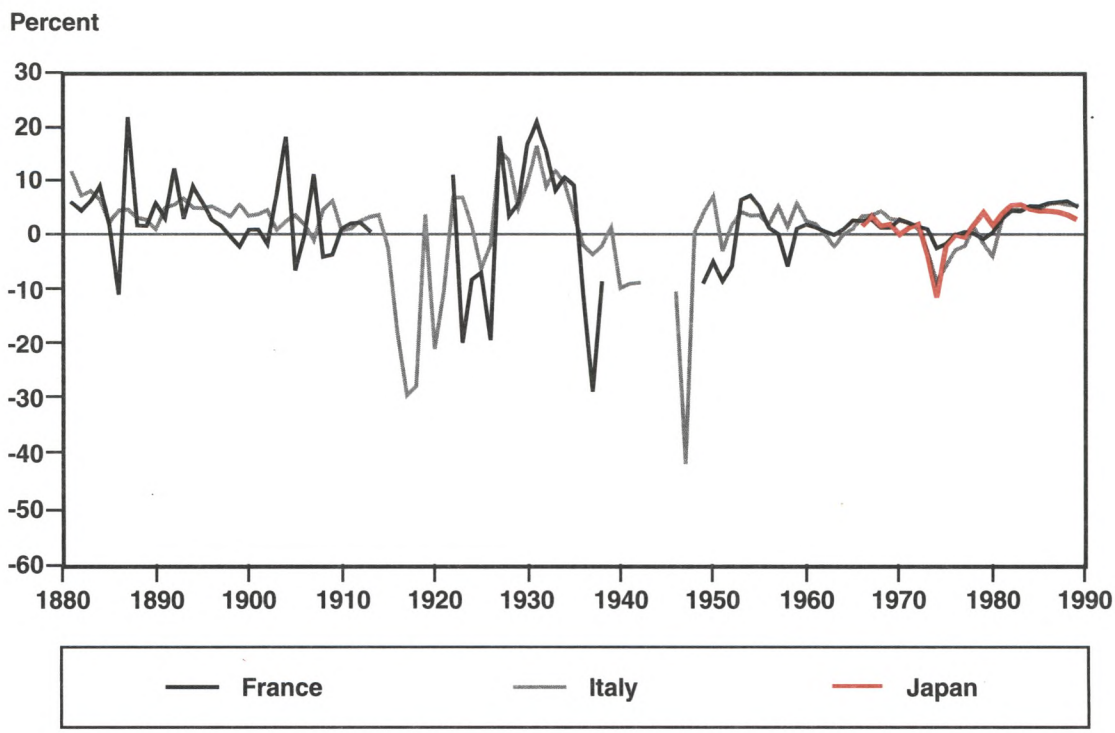
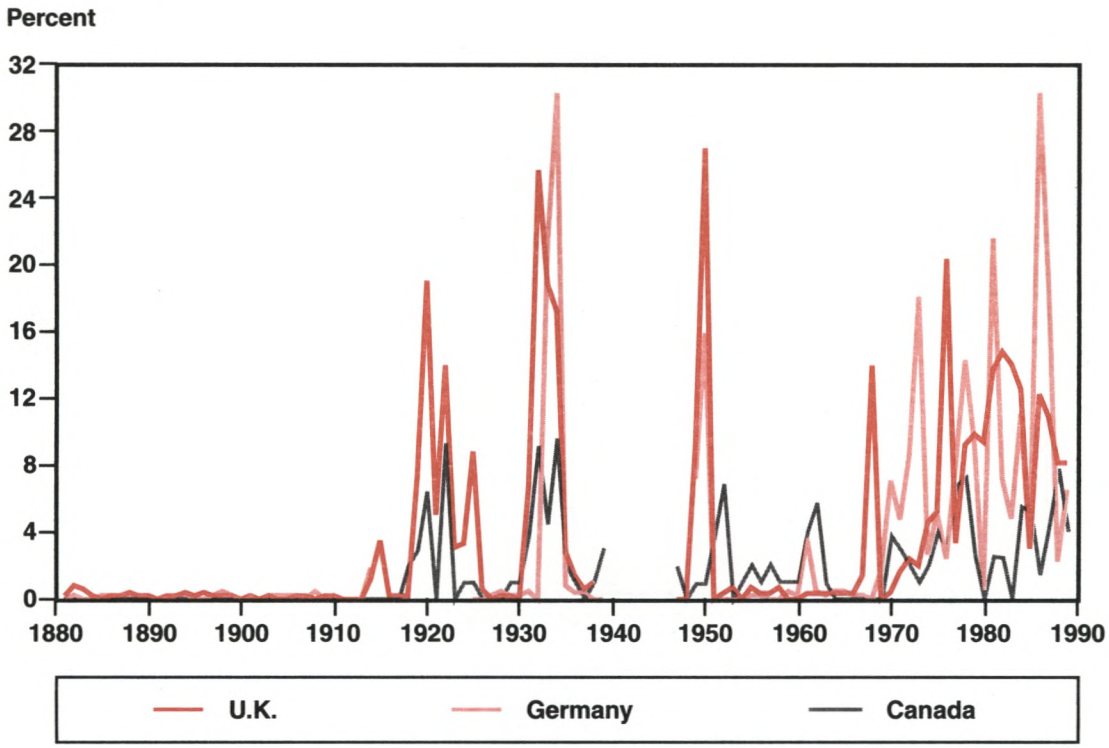


Figure 8

Absolute Change in Nominal Exchange Rates, 1880-1989, G7 Countries



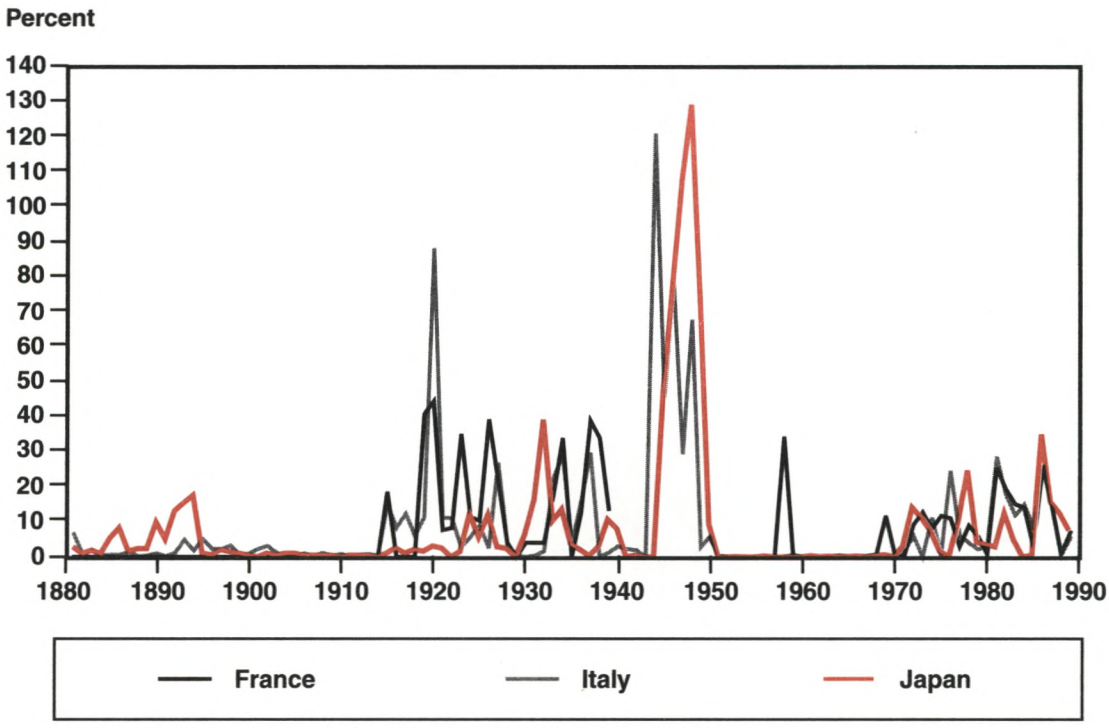
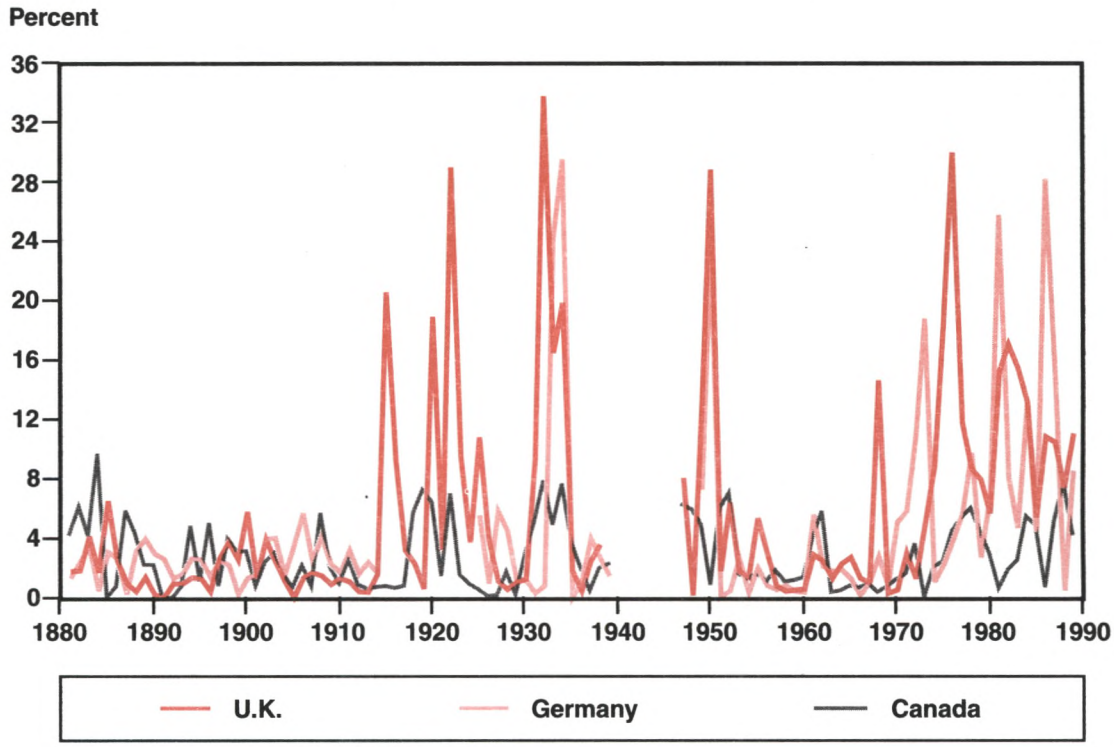
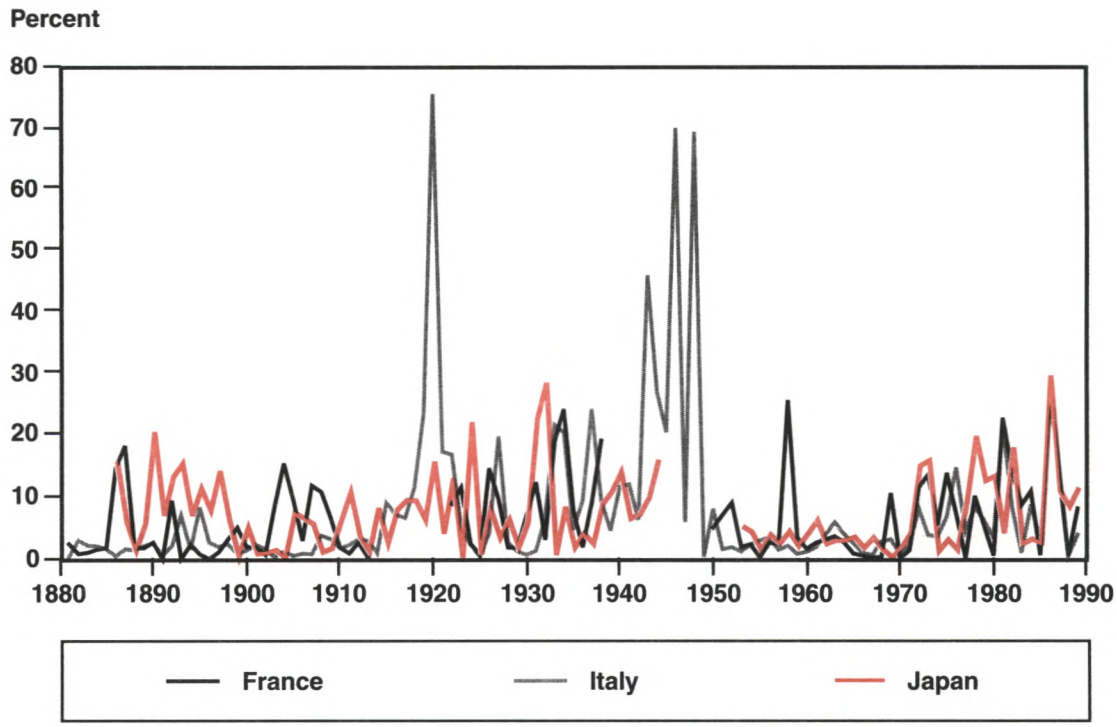


Figure 9

Absolute Change in Real Exchange Rates, 1880-1989, G7 Countries





hence that period cannot be used in the comparison.[39]

In summary, the Bretton Woods regime exhibited the best overall macroeconomic performance of any regime. This is especially so for the convertible period (1959–70).[40] As the summary statistics in table 1 show, both nominal and real variables were most stable in this period. The floating exchange rate regime, on most criteria, was not far behind the Bretton Woods convertible regime, whereas the classical gold standard exhibited the most stability and the closest convergence of financial variables.

The preconvertible Bretton Woods period (1946–58) was considerably less stable for the average of all countries for both nominal and real variables than other regimes. Also both nominal and real variables did not vary nearly as closely together. These differences likely reflect the presence of pervasive exchange and capital controls before 1958 and, related to these, more variable and more rapid monetary growth. These data, however, are limited. Although they show excellent performance for the convertible Bretton Woods regime, they do not tell us why it did well—whether it reflected a set of favorable circumstances, whether it reflected the absence of aggravating shocks, whether it reflected stable monetary policy by the key country of the system, the United States, or whether it masked underlying strains to the system.

Inflation Persistence

A second piece of evidence is persistent inflation. Evidence of persistence in the inflation rate suggests that market agents expect the monetary authorities to continually follow an inflationary policy; its absence would be consistent with the belief that the authorities are following a stable monetary rule, such as the gold standard's convertibility rule. Barsky (1987) presented evidence for the United Kingdom and United States based on both autocorrelations and time series models that inflation under the gold standard was very nearly a white-noise process, whereas in the post–World War II period, the inflation rate exhibited considerable persistence. Alogoskoufis and Smith (1991) also show, based on AR(1) regressions of the inflation rate, that inflation persistence in the two countries increased between the classical gold standard period and the interwar period and between the interwar period and the post–World War II period.[41]

Table 2 presents the inflation-rate coefficient from the type of AR(1) regressions on consumer price index inflation estimated by Alogoskoufis and Smith, for the Group of Seven countries over successive regimes since 1880, as well as the standard errors and the Dickey-Fuller tests for a unit root.[42]

The results, as in Alogoskoufis and Smith, show an increase in inflation persistence for most countries between the classical gold standard and the interwar period, and also between the interwar period and the post–World War II period as a whole. Within the post–World War II period, inflation persistence is generally lower, with the exceptions of France and Japan, in the preconvertible Bretton Woods than the convertible period. This suggests that though the immediate post–World War II period was characterized by rapid inflation, market agents may have expected a return to a stable price regime. The higher degree of persistence in the convertible regime suggests that this expectation lost credence. Finally, the evidence that persistence was generally highest during the floating exchange rate regime may imply that the public realized that there was no longer a stable nominal anchor.

Forecast Errors in Inflation and Growth

A third piece of evidence relates to the forecast errors of inflation and real output growth. According to Meltzer and Robinson (1989), "a welfare maximizing monetary rule would reduce variability to the minimum inherent in nature and

[39]Meltzer (1990) in a comparison of EMS and non-EMS contries in the floating rate period also finds a strong correlation between changes in nominal and real exchange rates.

[40]McKinnon (1992) treats the period 1950 to 1970 as the *de facto* dollar standard. He views this period rather than 1959 to 1971 as the appropriate one for making the type of regime comparisons undertaken here. I made the same calculations as those shown in table 1 for the period 1950 to 1971. Virtually every variable for each country exhibited greater instability than in the 1959 to 1970 period. This reinforces my choice of dates.

[41]Also see Alogoskoufis (1992), who attributes the increase in persistence to the accommodation by the monetary authorities of shocks. This evidence is also consistent with the results of Klein (1975).

[42]Eichengreen (1992b) also presents these statistics for four of the countries.

Table 2

Persistence of CPI Inflation: Group of Seven Countries 1880-1989
Annual Data: Coefficient of AR1 Regression; (Standard error); t – statistic for unit root test

	United States			United Kingdom			Germany			France		
	AR1 Coefficient	Standard Error	t – statistic	AR1 Coefficient	Standard Error	t – statistic	AR1 Coefficient	Standard Error	t – statistic	AR1 Coefficient	Standard Error	t – statistic
Gold Standard	0.27	(0.18)	4.05	0.30	(0.17)	4.03	0.51	(0.16)	3.06	–0.22	(0.18)	6.78
Interwar	0.45	(0.17)	3.18	0.35	(0.19)	3.37	0.51	(0.21)	2.33	0.42	(0.24)	2.42
Bretton Woods (Total)	0.49	(0.19)	2.68	0.33	(0.20)	3.35	–0.03	(0.21)	4.90	0.56	(0.16)	2.75
Bretton Woods (Preconvertible)	0.41	(0.27)	2.19	0.15	(0.29)	2.93	–0.07	(0.31)	3.00	0.60	(0.27)	1.48
Bretton Woods (Convertible)	1.07	(0.20)	–0.35	0.57	(0.34)	1.26	0.44	(0.31)	1.81	0.12	(0.14)	6.29
Floating Exchange	0.68	(0.18)	1.78	0.69	(0.19)	1.63	0.83	(0.14)	1.21	0.85	(0.16)	0.94
Post World War II	0.65	(0.12)	2.92	0.75	(0.10)	2.50	0.31	(0.15)	4.60	0.69	(0.11)	2.82

	Japan			Canada			Italy		
	AR1 Coefficient	Standard Error	t – statistic	AR1 Coefficient	Standard Error	t – statistic	AR1 Coefficient	Standard Error	t – statistic
Gold Standard	0.22	(0.18)	4.33[1]	0.08	(0.18)	5.11	0.28	(0.14)	5.14
Interwar	0.70	(0.25)	1.20	0.35	(0.20)	3.25	0.28	(0.18)	4.00
Bretton Woods (Total)	0.52	(0.18)	2.67	0.39	(0.19)	3.21	0.21	(0.12)	6.58
Bretton Woods (Preconvertible)	0.47	(0.27)	1.96	0.32	(0.28)	2.43	0.18	(0.18)	4.56
Bretton Woods (Convertible)	0.18	(0.31)	2.64	0.81	(0.20)	0.95	0.38	(0.29)	2.14
Floating Exchange	0.70	(0.19)	1.58	0.75	(0.17)	1.47	0.75	(0.17)	1.47
Post World War II	0.54	(0.13)	3.54	0.65	(0.11)	3.18	0.28	(0.10)	7.20

For data sources see table 1.

The 5 percent significance level for unit root test with 25 observations is 3.00.

[1]GNP deflator was used because of unavailability of CPI data.

institutional arrangements." They measure variability by the mean absolute error (MAE) of a one-period forecast based on the univariate multistate Kalman Filter (MSKF). Following their approach, table 3 presents the MAEs for inflation and real growth for the Group of Seven countries over successive regimes. The MSKF forecasts incorporate both transitory and permanent shocks to the rate-of-change series.[43]

The smallest forecast errors for inflation on average were for the Bretton Woods convertible period, followed by the gold standard and the floating rate periods. The largest errors were for the interwar period, followed by the preconvertible Bretton Woods period. The most notable exception to the pattern was the United Kingdom, where the floating rate period exhibited the largest variability.

For real growth, as for the inflation rate, the lowest MAE, on average, occurred in the convertible Bretton Woods period. Another exception to this pattern was Japan. The highest MAE was again in the interwar and the preconvertible Bretton Woods period. The floating exchange rate period, though more variable than the convertible Bretton Woods period, was slightly less variable than the gold standard regime.

These results are quite consistent with those of table 1. The Bretton Woods convertible period was the most stable both in an *ex post* and *ex ante* sense. The performance of the gold standard and the float, however, are not much worse, at least for real growth for the float and inflation for the gold standard.

Demand and Supply Disturbances

An important issue is the extent to which the performance of alternative monetary regimes, as revealed by the data in the preceeding tables, reflects the operation of the monetary regime in constraining policy actions or the presence or absence of shocks to the underlying environment. One way to shed light on this issue, following earlier work by Bayoumi and Eichengreen, is to identify underlying shocks to aggregate supply and demand.[44] According to them, aggregate supply shocks reflect shocks to the environment and are independent of the regime, but aggregate demand shocks likely reflect policy actions and are specific to the regime.

The approach used to calculate aggregate supply and demand shocks is an extension of the bivariate structural vector autoregression (VAR) methodology developed by Blanchard and Quah.[45] Following Bayoumi and Eichengreen (1992a), I estimated a two-variable VAR on the rate of change of the price level and output.[46] Restrictions on the VAR identify an aggregate demand disturbance, which is assumed to have only a temporary effect on output and a permanent effect on the price level, and an aggregate supply disturbance, which is assumed to have a permanent effect on both prices and output.[47] Overidentifying restrictions, namely, restrictions that demand shocks are positively correlated and supply shocks are negatively correlated with prices, can be tested by examining the impulse response functions to the shocks.

The methodology has important limitations that suggest that the results should be viewed with caution. The key limitation is that one can easily imagine frameworks in which demand shocks have permanent effects on output, whereas supply shocks have only temporary effects.[48]

I estimated supply (permanent) and demand (temporary) shocks, using annual data for each of the Group of Seven countries, over alternative regimes in the period 1880–1989. The VARs are based on three separate sets of data—1880–1913, 1919–39 and 1946–89—with the war years omitted because complete data on them were available for only four of the countries.[49] The VARs have two lags. I also did the estimation for

[43]Meltzer and Robinson (1989) present their results for levels, growth rates, and permanent growth rates of the series. I present only growth rates to make the results comparable to those in table 1.

[44]See Bayoumi and Eichengreen (1992a, 1992b, 1992c and 1992d).

[45]See Blanchard and Quah (1989).

[46]Both variables were rendered stationary by first differencing.

[47]Specifically, four restrictions are placed on the matrix of the shocks: two are simple normalizations, which define the variances of the shocks to aggregate demand and aggregate supply; the third assumes that demand and supply shocks are orthogonal; the fourth is that demand shocks have only temporary effects on output, that is, that the cumulative effect of demand shocks on the rate of change in output must be zero.

[48]See Keating and Nye (1991).

[49]For results using the complete data set for these four countries, see appendix table 1 and appendix figure 1.

Table 3

Forecast Errors in Inflation and Real Growth: Group of Seven Countries 1880-1989
Annual Data: Mean Absolute Errors Using the Multistate Kalman Filter

	Gold Standard (1880-1913)		Interwar (1919-1939)		Bretton Woods (Total) (1946-1970)		Bretton Woods (Preconvertible) (1946-1958)		Bretton Woods (Convertible) (1959-1970)		Floating Exchange (1973-1989)	
	Growth	Inflation	Growth	Inflation	Growth	Inflation	Growth	Inflation	Growth	Inflation	Growth	Inflation
United States	2.04	1.59	4.79	4.61	3.22	2.32	5.02	3.44	1.28	1.12	1.65	2.46
United Kingdom	1.42	2.10	4.11	4.41	1.41	1.47	1.82	1.76	0.97	1.15	2.83	4.66
Germany	1.69	1.69	8.07	3.97	2.77	2.59	3.24	4.99	2.49	1.18	1.49	1.30
France	2.25	2.47	5.54	7.07	1.51	3.13	2.28	4.62	0.87	2.14	1.47	2.74
Japan	1.69	3.95	2.71	6.83	1.59	2.63	0.98	5.48	1.60	0.96	1.34	2.95
Canada	1.58	0.80	7.16	3.99	2.19	1.92	3.06	2.96	1.24	0.79	1.72	2.29
Italy	2.08	2.01	2.08	7.58	4.21	6.35	7.04	10.53	1.15	1.81	1.14	3.84
Average	1.82	2.09	4.92	5.50	2.41	2.92	3.35	4.82	1.37	1.31	1.66	2.89

Note: For data sources, see table 1.

aggregated price and output data for the Group of Seven countries.[50]

The overidentifying restrictions that demand shocks be positively correlated and supply shocks be negatively correlated with the price level are satisfied for all countries for the two post–World War II regimes. But for the period before World War II, for a number of countries, including the United States, United Kingdom, and France, they are not. Supply shocks were positively correlated with prices. This can be seen in the impulse response functions displayed in figure 10. Figure 10 shows the impulse responses, to one standard deviation shocks in aggregate supply and aggregate demand, on output and prices for the Group of Seven countries aggregate by regime.[51]

Keating and Nye (1991) attempted to explain this result by possible hysteresis effects. Bayoumi and Eichengreen (1992a) argued that the perverse impulse response patterns for the classical gold standard and interwar periods reflected the interaction of a positive aggregate demand curve with a very steep aggregate supply curve. They explain the positively sloped aggregate demand curve as reflecting the effects of gold discoveries induced by the supply shock of agricultural settlements in the United States and Australia. These results may also reflect a limitation of the Blanchard-Quah methodology.

Table 4 presents the standard deviations of supply and demand shocks for the Group of Seven countries and the Group of Seven countries taken as a whole (Group of Seven aggregate) by regime. I also show, following Bayoumi and Eichengreen, the weighted average of the individual country shocks.[52] Figures 11 and 12 show the shocks for the Group of Seven aggregate and for each of the seven countries.

Table 4 shows for the Group of Seven aggregate that the convertible Bretton Woods regime was the most tranquil of all the regimes—neither supply nor demand shocks dominated. It was not, however, that much less turbulent than the succeeding float. The interwar period, unsurprisingly, shows the largest supply and demand shocks.[53] Sizeable supply and demand shocks that are two or three times greater than the post–World War II period also characterize the classical gold standard.[54]

For individual countries, the Bretton Woods convertible period was the most stable in four countries and the flexible exchange rate period was the most stable in three. The difference between the convertible Bretton Woods period and the floating exchange rate period, however, was not great in any country. The interwar period as expected was the most volatile. Both types of shocks were the largest in every country except the United Kingdom. Finally, in the majority of countries, with the principal exceptions being the United Kingdom and Germany, both supply and demand shocks were considerably greater in the gold standard period than in the post–World War II period.

The dispersion of demand shocks across countries, as measured by the GNP-weighted standard deviation of the individual country shocks around the Group of Seven aggregate, reveals very little difference between the gold standard and the post–World War II regimes, with the convertible Bretton Woods regime displaying the highest degree of convergence. Dispersion is much greater in the interwar period. The dispersion of supply shocks is considerably greater during the gold standard and the interwar periods than in any of the post–World War II regimes.

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[50]The Group of Seven aggregate income growth and inflation rate are a weighted average of the rates in the different countries. The weights for each year are the share of each country's nominal national income in the total income in the Group of Seven countries, where the national income data are converted to U.S. dollars using the actual exchange rates.

[51]The impulse response functions were calculated from VARs run for the separate regime periods. Because the number of observations was limited, the Bretton Woods regime could not be split into the two subperiods shown in preceding tables.

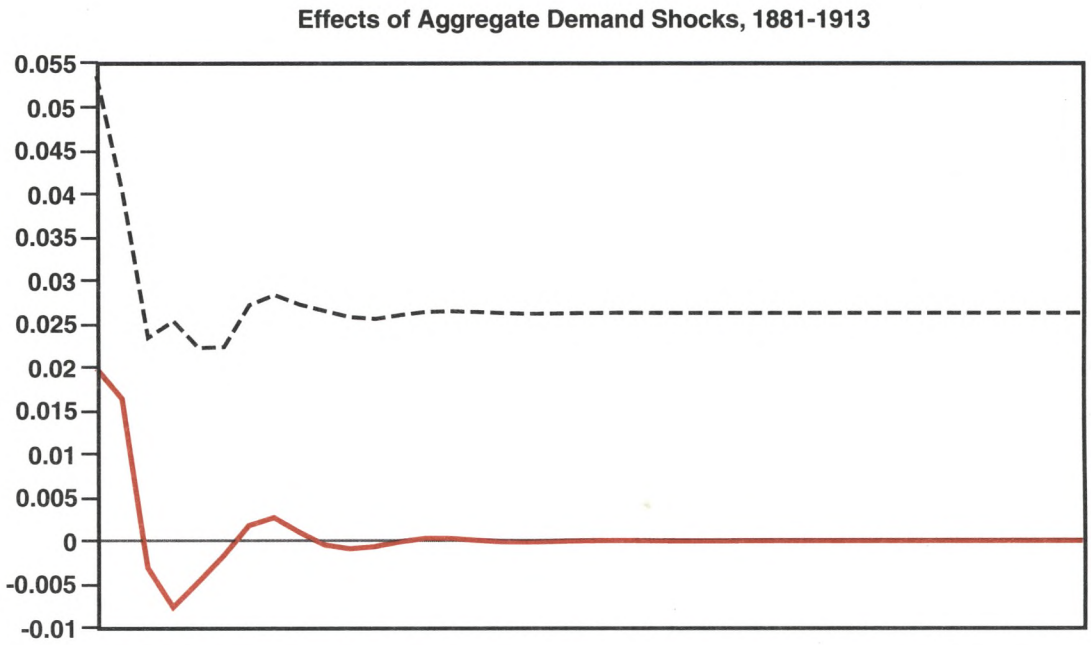
[52]See Bayoumi and Eichengreen (1992a).

[53]The results for the Group of Seven in the interwar period (figures 11 and 12) as well as those for four countries (appendix figure 1) are similar to those reported for the United States by Cecchetti and Karras (1992), who estimate a three-variable VAR with monthly data. The late 1920s and early 1930s reveal a major negative demand shock consistent with Friedman and Schwartz's (1963) attribution of the onset of the Great Depression to monetary forces. After 1931, negative supply shocks predominate, consistent with Bernanke's (1983) and Bernanke and James (1991) explanation for the severity of the Great Depression that stresses the collapse of the financial system.

[54]Though the shocks are smaller, the rankings by regime for the weighted average of individual country shocks are similar to the G-7 aggregate.

Figure 10

Impulse Response Functions of Demand and Supply Shocks on Prices (Dotted Lines) and Output (Solid Lines), G7 Aggregate by Regimes, Annual Data, 1881-1989



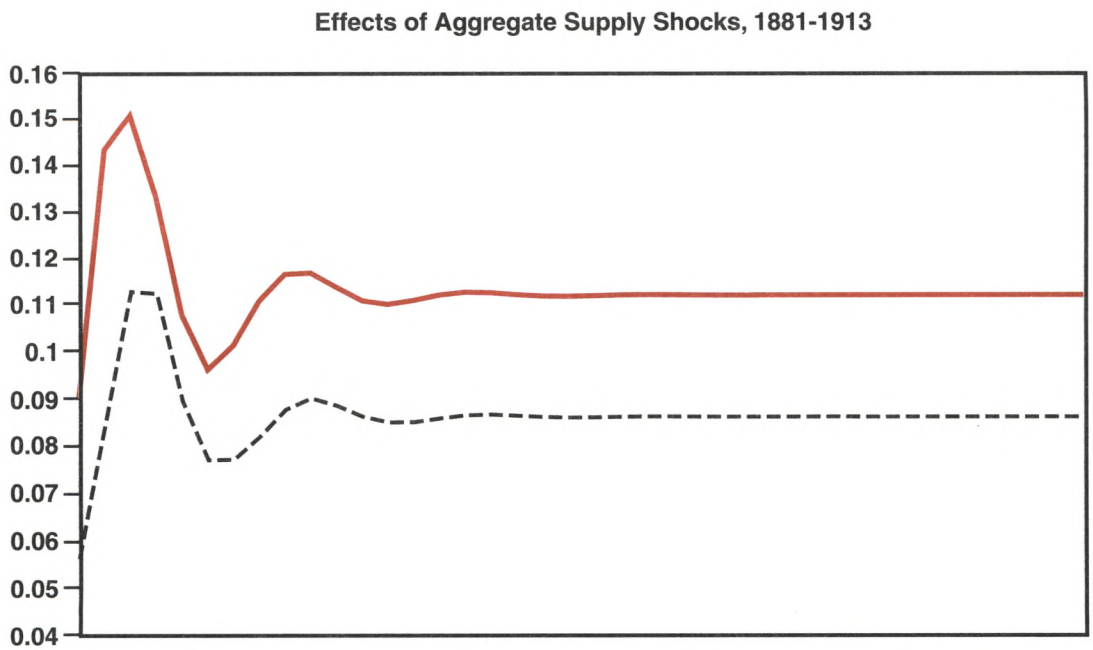
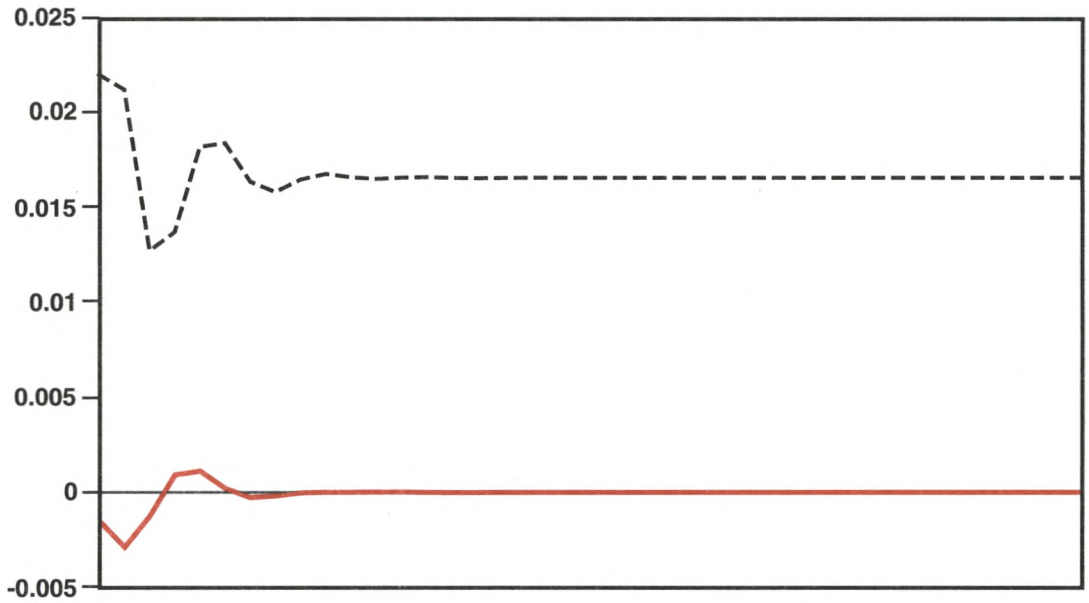


Figure 10 (continued)

Impulse Response Functions of Demand and Supply Shocks on Prices (Dotted Lines) and Output (Solid Lines), G7 Aggregate by Regimes, Annual Data, 1881-1989

Effects of Aggregate Demand Shocks, 1919-1939



Effects of Aggregate Supply Shocks, 1919-1939

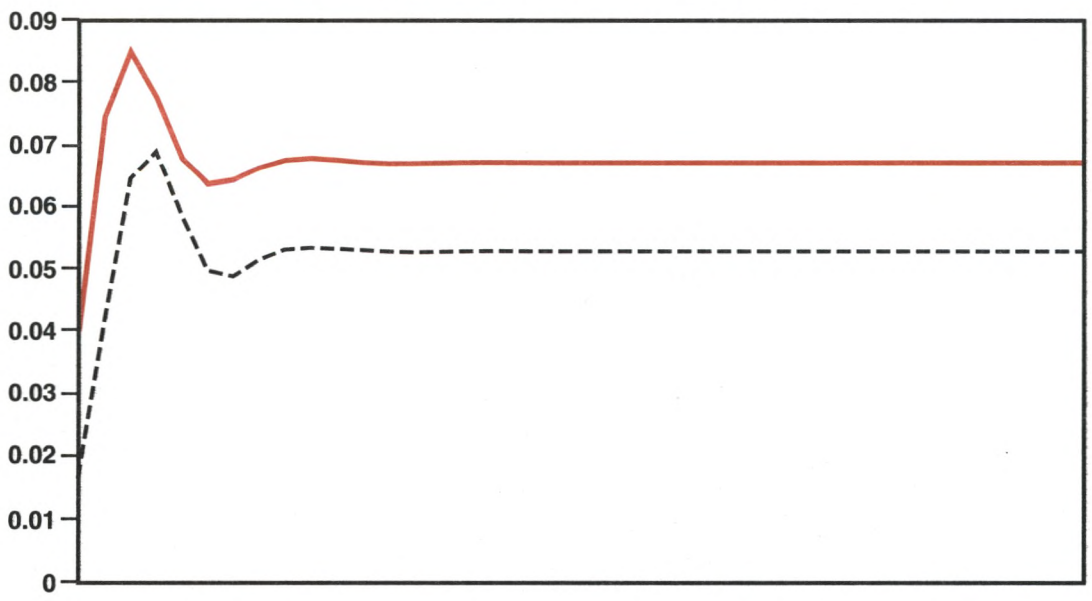
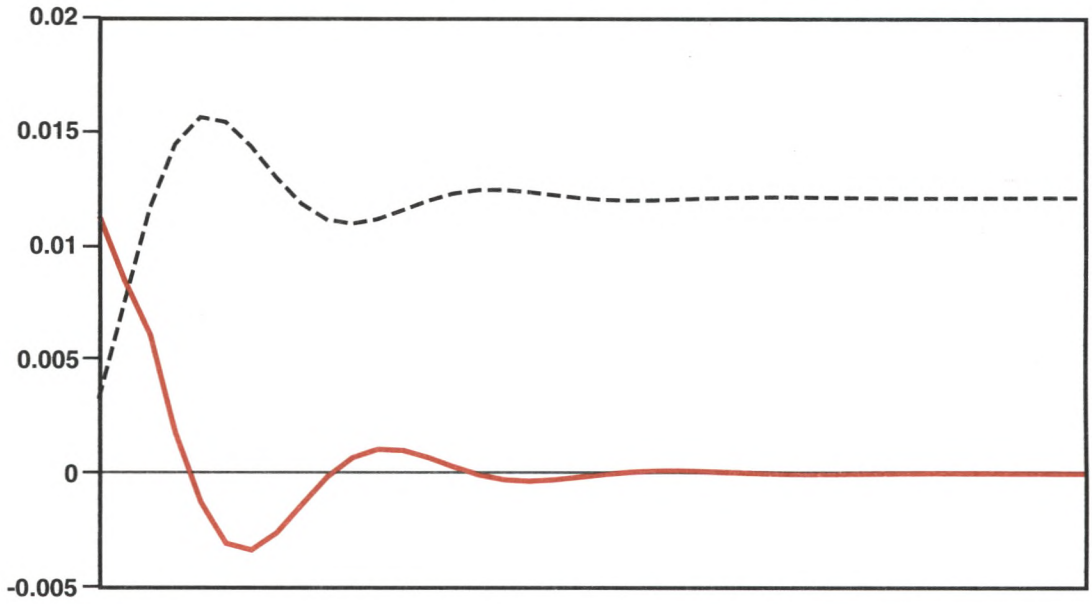


Figure 10 (continued)

Impulse Response Functions of Demand and Supply Shocks on Prices (Dotted Lines) and Output (Solid Lines), G7 Aggregate by Regimes, Annual Data, 1881-1989

Effects of Aggregate Demand Shocks, 1946-1970



Effects of Aggregate Supply Shocks, 1946-1970

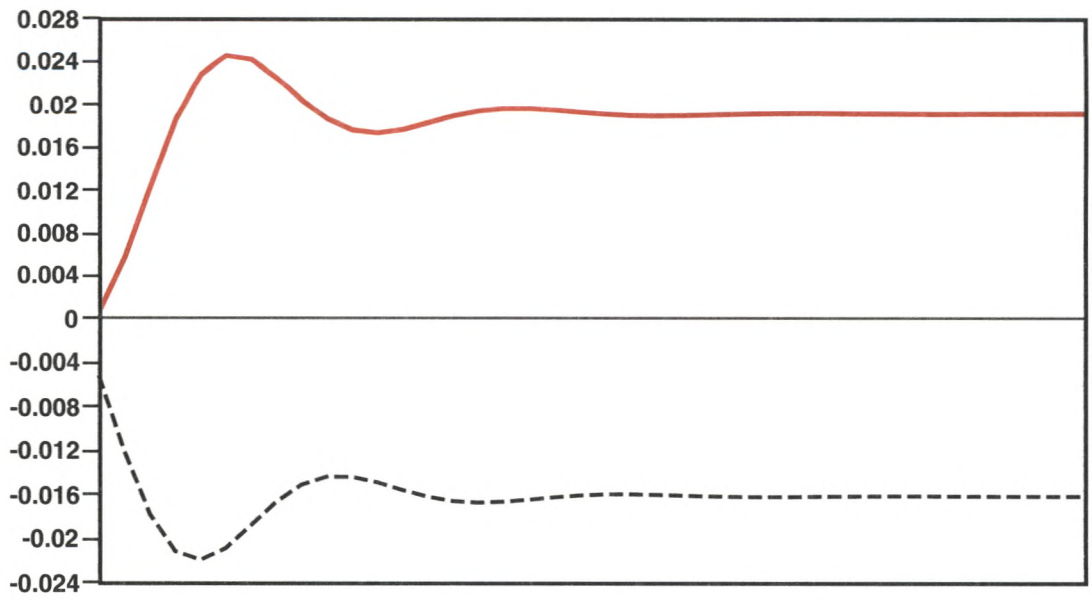
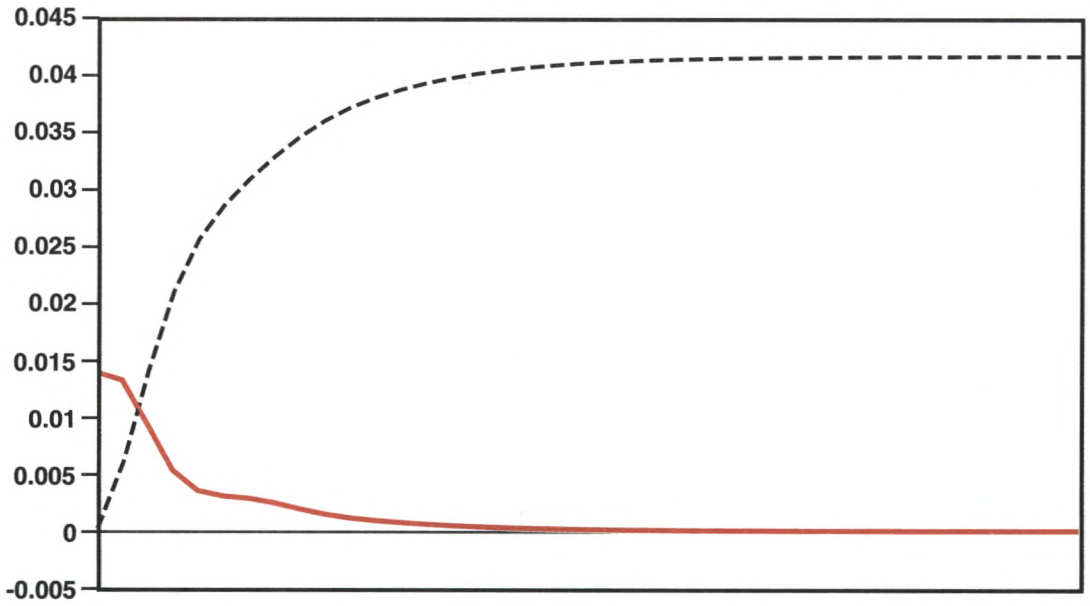


Figure 10 (continued)

Impulse Response Functions of Demand and Supply Shocks on Prices (Dotted Lines) and Output (Solid Lines), G7 Aggregate by Regimes, Annual Data, 1881-1989

Effects of Aggregate Demand Shocks, 1946-1989



Effects of Aggregate Supply Shocks, 1946-1989

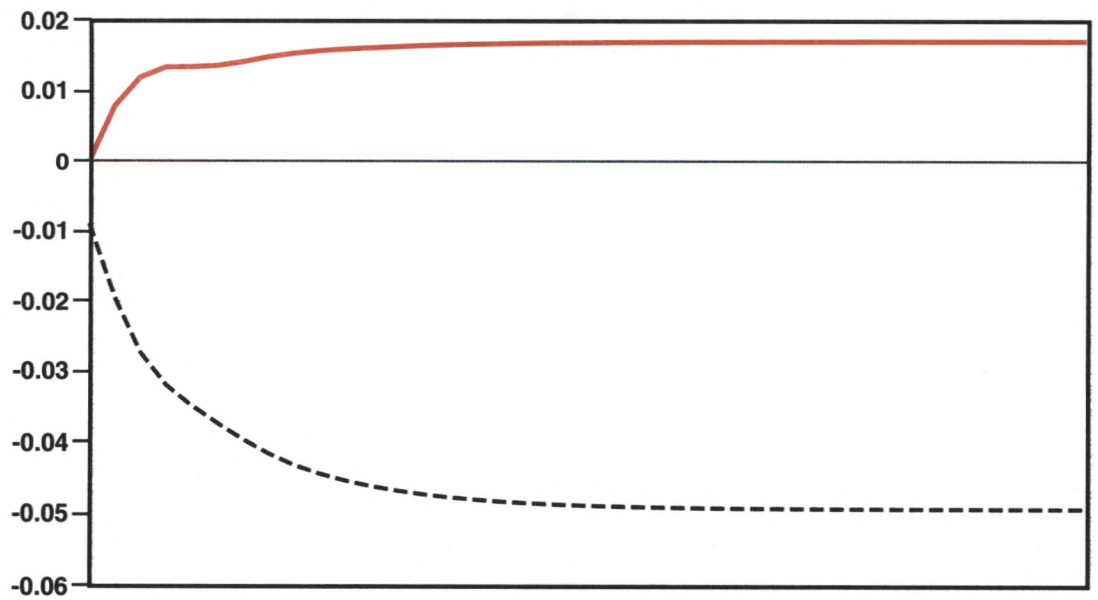
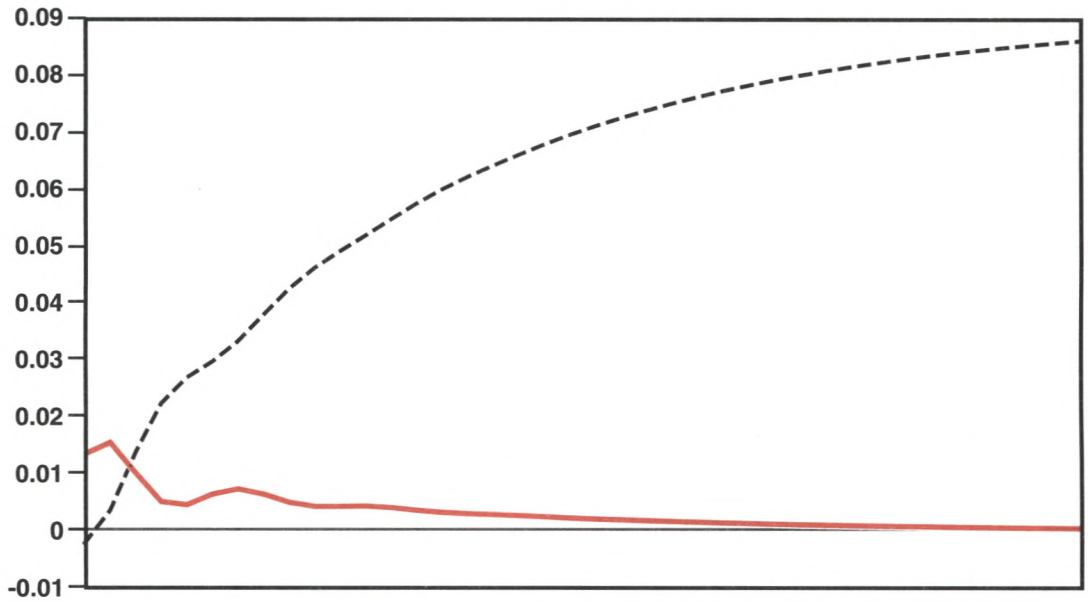


Figure 10 (continued)

Impulse Response Functions of Demand and Supply Shocks on Prices (Dotted Lines) and Output (Solid Lines), G7 Aggregate by Regimes, Annual Data, 1881-1989

Effects of Aggregate Demand Shocks, 1971-1989



Effects of Aggregate Supply Shocks, 1971-1989

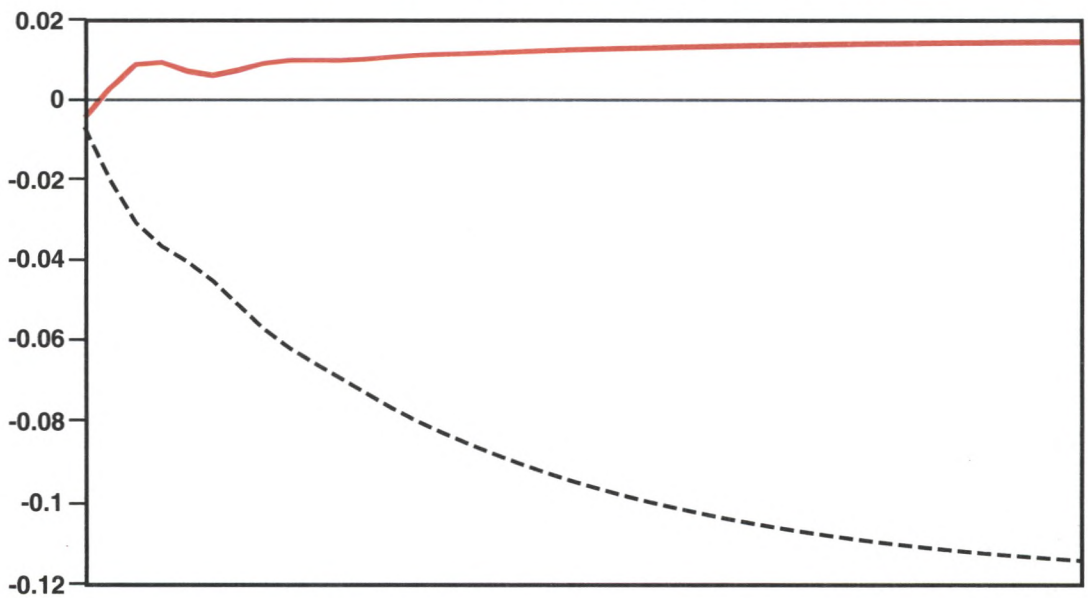


Table 4

Supply (Permanent) and Demand (Temporary) Shocks: 1880-1989
Annual Data: Standard Deviations of Shocks (percent); Dispersion of shocks across countries (percent)

	Gold Standard 1883-1913		Interwar 1921-1939		Bretton Woods (Total) 1948-1970		Bretton Woods (Preconvertible) 1948-1958		Bretton Woods (Convertible) 1959-1970		Floating Exchange 1973-1989		Post WW II 1948-1989	
	Demand	Supply	Demand	Supply	Demand	Supply	Demand	Supply	Demand	Supply	Demand	Supply	Demand	Supply
United States	2.03	3.81	4.45	6.73	2.33	1.54	3.11	1.98	1.37	1.07	1.72	1.94	2.07	1.68
United Kingdom	2.66	2.16	1.93	3.52	2.62	1.95	3.06	2.61	2.26	1.14	3.57	4.31	3.03	3.10
Germany	2.37	2.32	4.47	3.13	2.88	2.65	1.85	2.87	3.34	2.66	1.66	1.39	2.36	2.20
France	4.58	3.75	7.17	5.19	3.50	1.75	3.23	1.61	3.77	1.84	1.93	1.52	2.84	1.70
Japan	4.85	3.39	6.28	5.36	3.18	2.56	4.05	1.97	2.83	2.78	2.39	2.50	2.75	2.69
Canada	0.93	2.75	4.01	8.61	2.42	2.60	3.09	3.70	1.74	0.98	2.66	2.10	2.48	2.47
Italy	3.16	3.13	7.40	4.14	2.76	1.75	2.93	1.85	2.56	1.73	3.58	1.91	3.30	2.47
G7	1.56	2.21	3.09	4.12	1.24	0.86	1.99	1.00	0.75	0.80	1.50	0.96	1.40	0.91
G7*	1.49	2.08	2.99	4.61	1.11	0.81	1.64	1.01	0.75	0.71	1.40	1.60	1.28	1.32
Dispersion	3.94	5.03	8.25	8.58	3.93	2.38	3.44	1.89	4.09	2.54	3.53	2.74	3.69	2.63

G7: G7-aggregate data

G7*: Weighted average of individual country shocks; the weights are calculated as the share of each country's National Income in the Total Income in the G7 countries, where the GNP/GDP data are converted to dollars using the actual exchange rate.

Dispersion = Σ (weight$_i$(shock$_i$-Σ weight$_i$*shock$_i$)^2)$^{0.5}$
for i = United States, United Kingdom, Germany, France, Japan, Canada, Italy

Figure 11

Supply and Demand Shocks: G7 Aggregate, 1880-1989, Annual Data

Percent

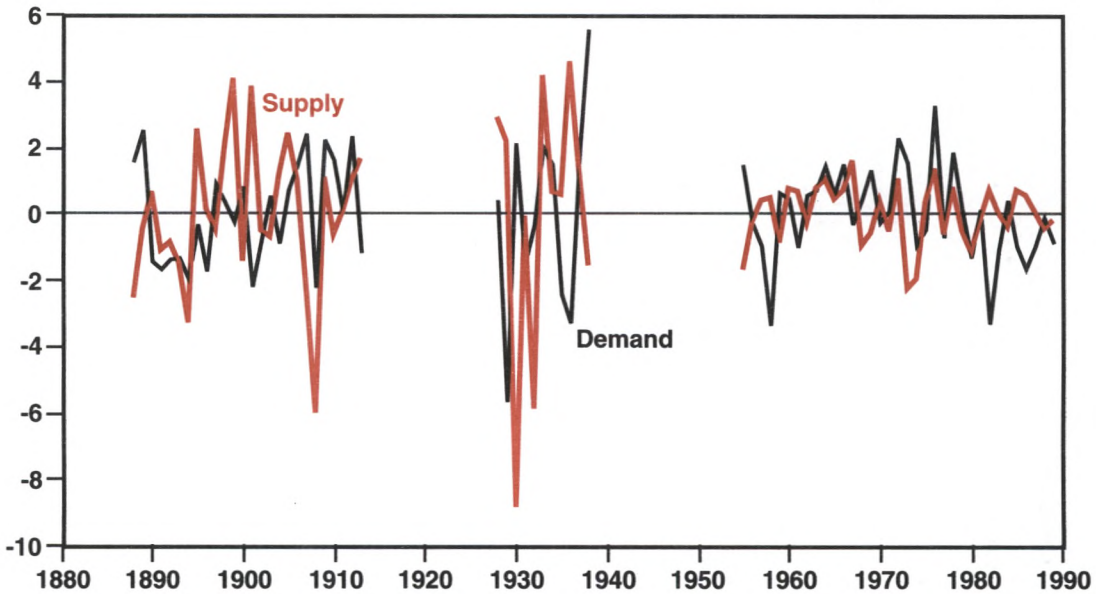


Figure 12

Supply and Demand Shocks: 1880-1989, United States

Percent

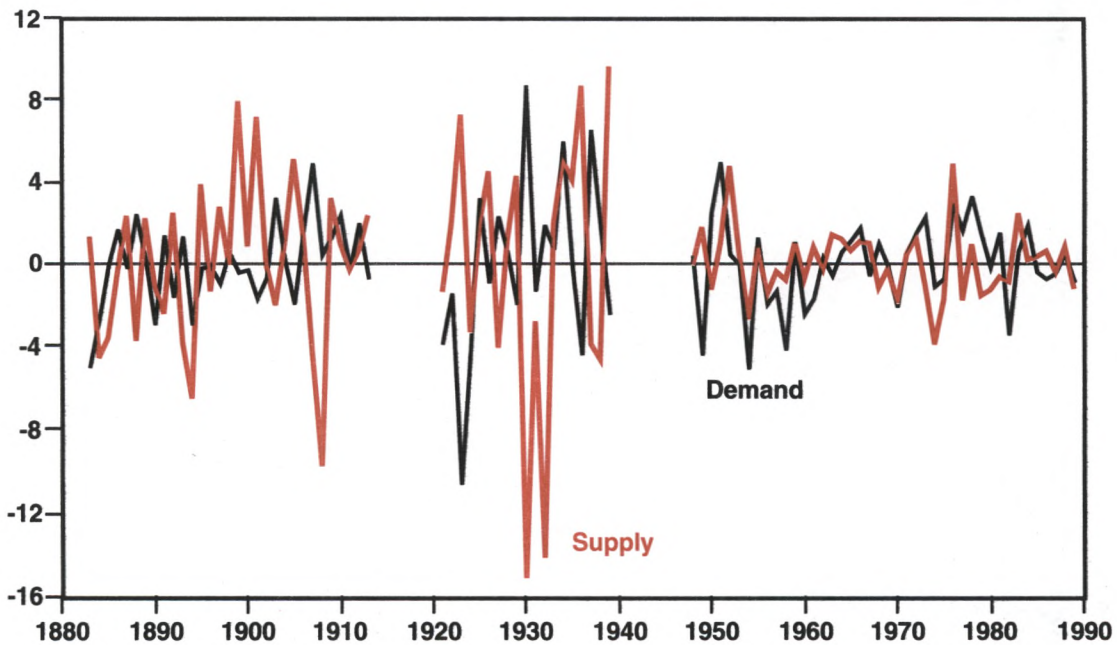
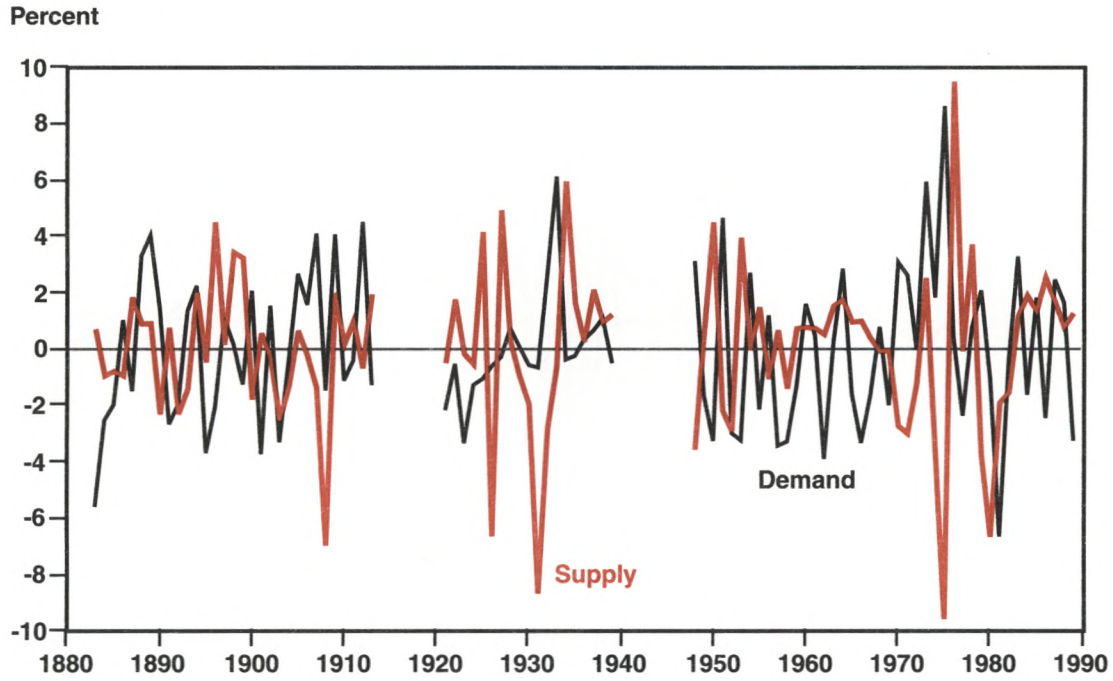


Figure 12 (continued)

Supply and Demand Shocks: 1880-1989, United Kingdom



Supply and Demand Shocks: 1880-1989, Germany

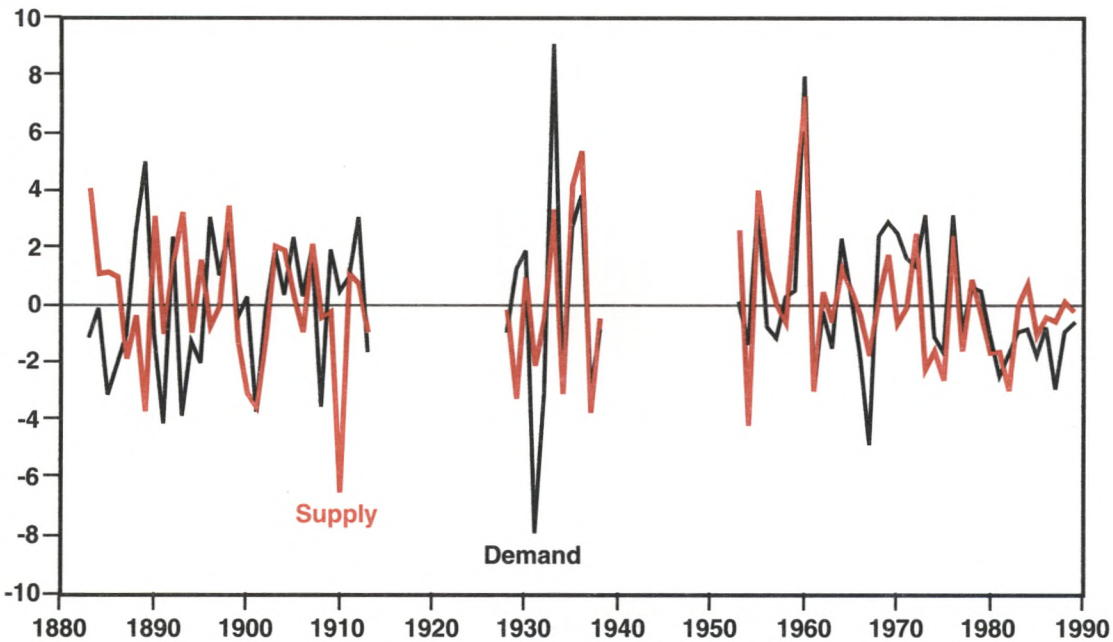
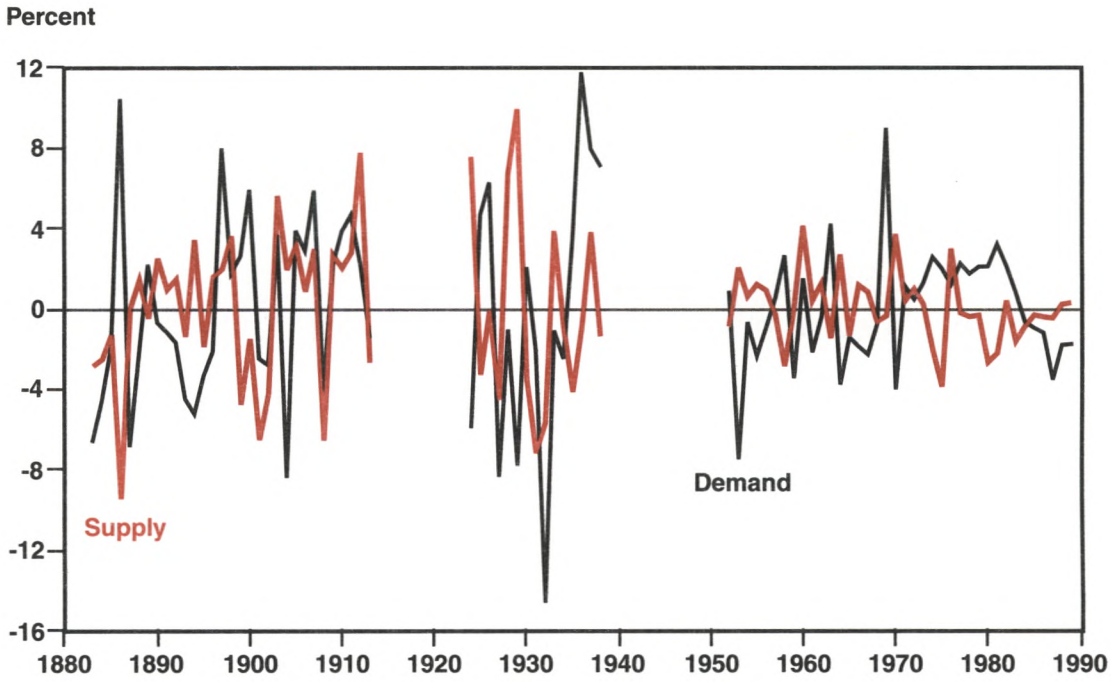


Figure 12 (continued)

Supply and Demand Shocks: 1880-1989, France



Supply and Demand Shocks: 1880-1989, Japan

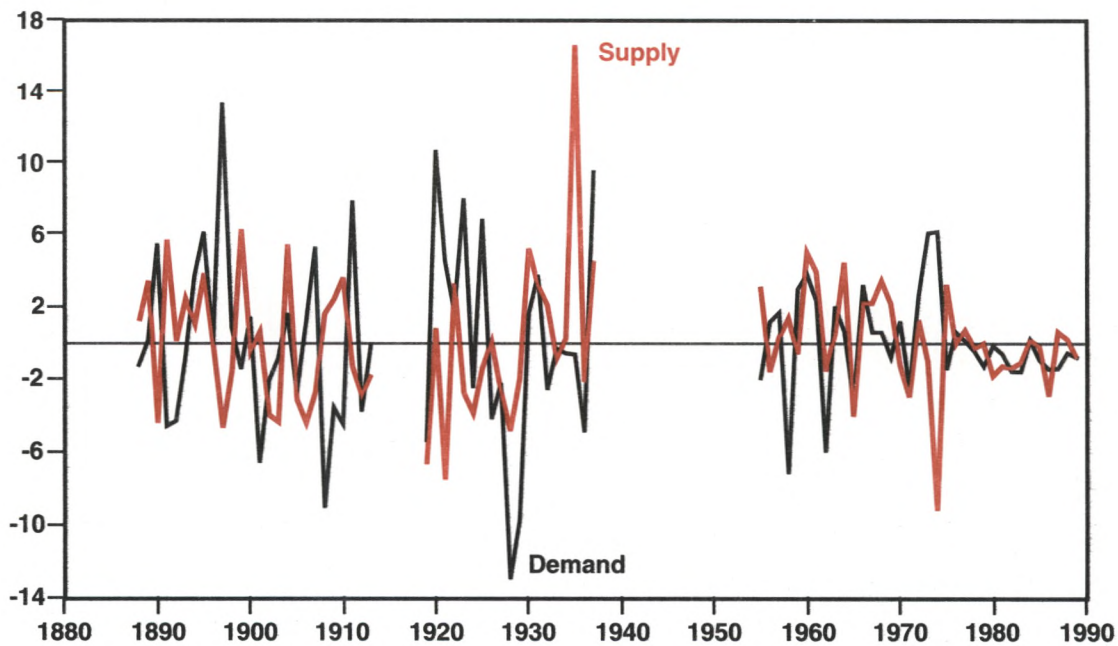
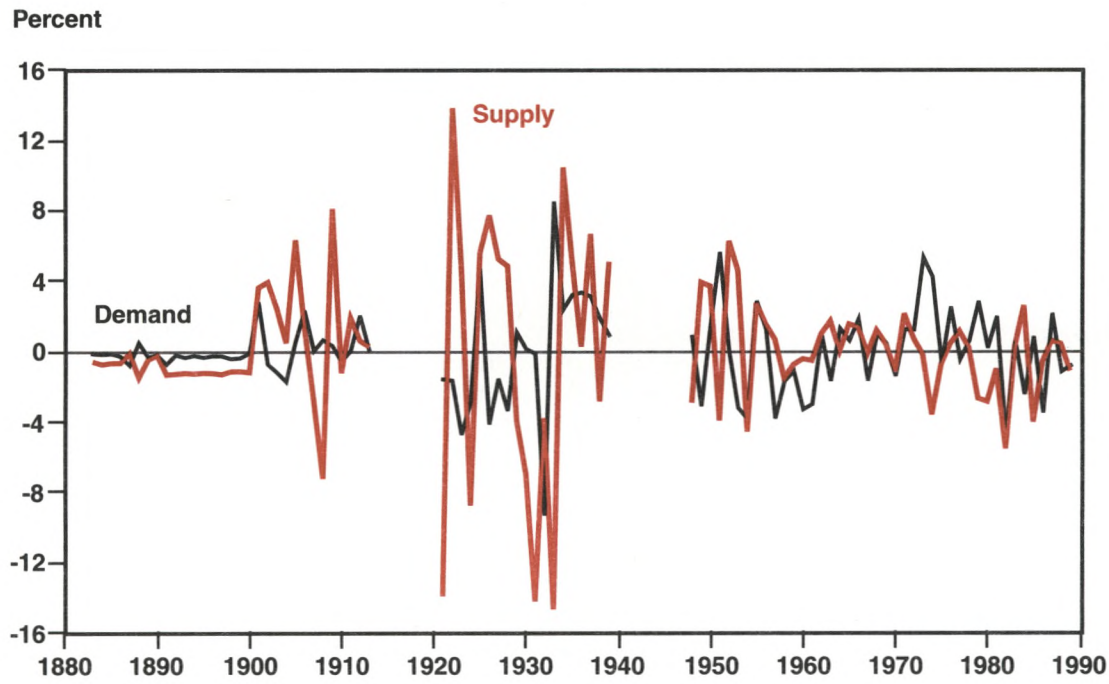
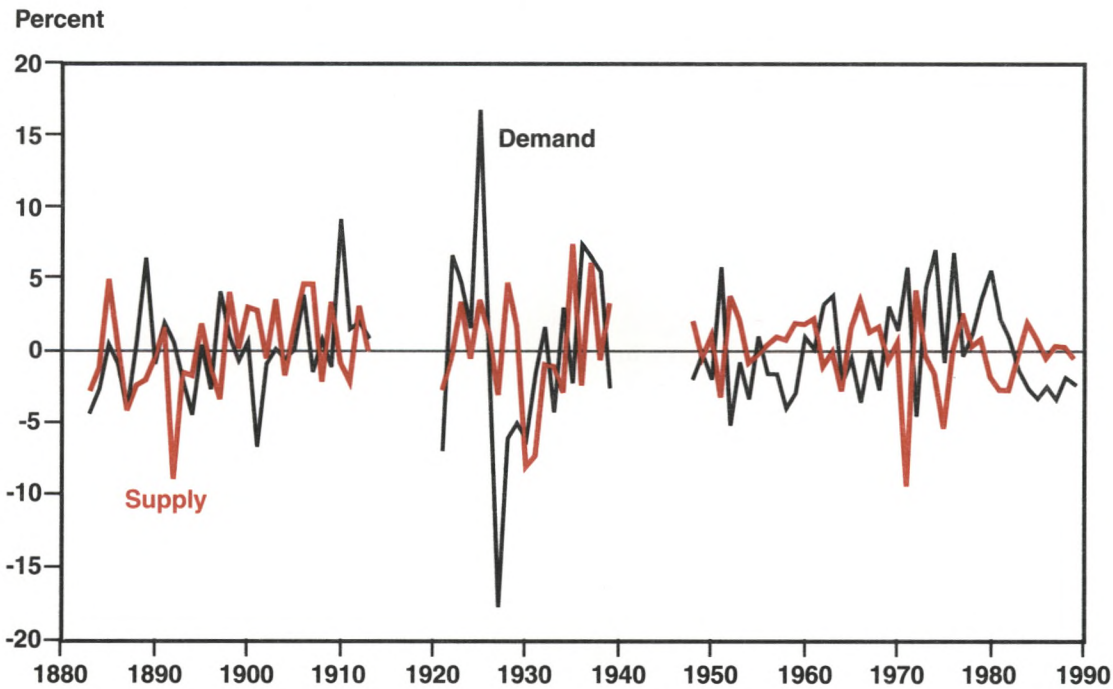


Figure 12 (Continued)

Supply and Demand Shocks: 1880-1989, Canada



Supply and Demand Shocks: 1880-1989, Italy



In sum, the evidence on supply and demand shocks is quite similar to the measures of volatility drawn from the forecast errors using the MSKF. The gold standard regime, as well as the interwar period, emerges as a relatively unstable period stressed by widely dispersed supply shocks. By contrast, the Bretton Woods convertible period is the most stable, with the floating exchange rate period not far behind.

These results raise the interesting question, why in the past century was the classical gold standard so durable in the face of substantial shocks, whereas Bretton Woods was so fragile in the face of the mildest shocks?

Responsiveness to Shocks

The final piece of evidence to be calculated in the comparison of regime performance is the response of the price level and output to the aggregate supply (permanent) and aggregate demand (temporary) shocks. Evidence of a more rapid adjustment of prices and output to shocks may help explain why one regime may have been more durable than another.

A measure of speed of response can be gleaned from the impulse response functions derived from the bivariate VARs. In addition, as a crude measure of response speed, which allowed easy comparison of all seven countries during the four regimes, I calculated the mean absolute lag of the response functions.[55] Table 5 presents these measures.

The response of output to both demand and supply shocks for the Group of Seven aggregate and for most of the individual countries was markedly more rapid under the gold standard regime than under the postwar regimes (an exception is the U.S. response to demand shocks) and within the postwar regimes was slightly more rapid under the Bretton Woods regime than the floating exchange rate regime. The response of prices to both demand and supply shocks was considerably more rapid during the gold standard (and the interwar) regime than the postwar regimes for the Group of Seven and most countries.[56] Within the postwar period, it was considerably more rapid under Bretton Woods than under the floating exchange rate period.

Perhaps the gold standard was able to endure the greater shocks that it faced because of both greater price flexibility and greater factor mobility before World War I. Alternatively, the gold standard was more durable than Bretton Woods because before World War I, suffrage was limited, central banks were often privately owned and, before Keynes, there was less understanding of the link between monetary policy and the level of economic activity. Hence there was less of an incentive for the monetary authorities to pursue full employment policies, which would threaten adherence to convertibility.

In addition, the Bretton Woods regime was both more stable and seemingly more flexible than the floating exchange rate regime and yet more fragile. This suggests that its collapse is attributable less to outside shocks to the environment or the structure of the Group of Seven economy and more to flaws in the design of the regime.

Summary

The performance of alternative international monetary regimes suggests that the Bretton Woods convertible regime (1959–70) was the most stable, followed by the floating exchange rate and the classical gold standard regimes. The stability of forecast errors to both inflation and growth paralleled that of the *ex post* data. Limited inflation persistence—evidence for credibility of the nominal anchor—was lowest during the classical gold standard. Though considerably higher than under the gold standard, persistence was less under Bretton Woods than under the float. Under Bretton Woods the nominal anchor of the U. S. commitment to peg the price of its currency to gold was apparently still effective. Finally, supply shocks were greater and less symmetric, and demand shocks were greater under the classical gold standard than under the post–World War II regimes. A more rapid response of both prices and output to these shocks also occurred under the gold standard.

The question still remains why some fixed exchange rate regimes endured longer than others

[55]The formula was $\Sigma\, i \mid \Delta\, c_i \mid / \Sigma \mid \Delta c_i \mid$ for $i = 1$ to 40, where i is the year and $\Delta\, c_i$ the impulse response, calculating absolute changes because of the presence of both positive and negative responses. This measure is only a rough approximation because it is not possible to calculate the standard errors.

[56]As mentioned above, according to the overidentifying restrictions of the Bayoumi-Eichengreen-Blanchard-Quah approach, supply shocks should have produced a negative response in prices, not the positive one shown here for the pre-World War II periods.

Table 5

The Mean Lag of Adjustment to Demand and Supply Shocks, G7 Countries 1880-1989[1]

		Gold Standard	Interwar	Post World War II	Bretton Woods	Floating Exchange
United States	a)	2.77	3.79	2.16	2.29	3.65
	b)	1.65	2.89	2.68	1.85	6.48
	c)	1.61	1.65	2.21	2.12	4.64
	d)	2.24	2.31	2.55	3.06	5.52
United Kingdom	a)	2.52	2.01	3.38	4.24	3.43
	b)	2.13	1.97	4.93	3.06	4.46
	c)	1.88	1.48	1.96	2.39	2.62
	d)	3.14	3.06	3.04	2.23	2.44
Germany	a)	2.74	3.08	2.70	3.48	3.53
	b)	2.23	2.86	2.67	3.13	6.06
	c)	1.87	3.09	1.86	1.80	2.22
	d)	2.03	3.50	1.92	2.85	4.14
Canada	a)	1.76	2.53	2.55	3.12	2.40
	b)	1.75	1.87	2.75	1.79	2.67
	c)	1.20	1.36	2.32	1.52	2.26
	d)	2.51	2.62	2.87	2.98	2.98
France	a)	1.79	3.84	2.19	3.50	4.97
	b)	1.70	2.60	1.83	3.06	5.53
	c)	1.85	2.05	2.54	2.19	3.79
	d)	2.85	4.80	1.92	3.50	6.02
Japan	a)	2.67	4.16	2.94	3.14	4.55
	b)	1.85	3.87	2.70	1.99	9.42
	c)	1.96	3.41	2.97	1.96	3.66
	d)	1.52	3.13	4.69	2.81	8.34
Italy	a)	2.13	2.50	3.51	1.86	2.99
	b)	1.71	1.60	4.20	1.43	5.57
	c)	2.43	1.57	6.20	1.63	1.94
	d)	1.66	2.19	6.68	1.66	3.02
Average	a)	2.34	3.13	2.77	3.09	3.63
	b)	1.86	2.52	3.11	2.33	5.74
	c)	1.83	2.09	2.87	1.94	3.02
	d)	2.27	3.09	3.38	2.73	4.63
G7 aggregate	a)	2.19	3.61	3.27	4.59	5.16
	b)	1.38	2.68	6.00	5.14	11.83
	c)	1.78	2.63	4.18	6.03	6.14
	d)	2.10	3.35	4.74	5.11	11.26

a) Effect of temporary shock on output
b) Effect of temporary shock on prices
c) Effect of permanent shock on output
d) Effect of permanent shock on prices

[1]the mean lag of adjustment is calculated as:

$\Sigma\, i|\Delta c_i| \,/\, \Sigma\, |\Delta c_i|$ for $i = 1$ to 40
where c_i is the value of the impulse response function in period i.

or why the world periodically shifted between fixed and flexible rates. The durability of the gold standard may be due to greater price flexibility and factor mobility before World War I that allowed the world economy to respond to shocks more rapidly. It also may be due to the absence of discretionary monetary policies dedicated to maintaining full employment. But the fragility of the most stable regime, Bretton Woods, in the face of mild shocks, suggests that an understanding of its demise requires a closer look at the history, institutions and rules of behavior of alternative monetary regimes.

THE GOLD STANDARD, BRETTON WOODS, AND THE EMS AS COMMITMENT MECHANISMS

Perhaps the answer to the foregoing questions concerning regime performance and durability may be linked to the commitment technology of the regime. In this section I argue that the gold standard rule of convertibility was a credible commitment mechanism that was crucial to its success and that the absence of such a mechanism underlies the failure of the Bretton Woods variant. The EMS, though not anchored to gold convertibility, may have been successful for several years because it embodied a commitment technology reminiscent of the gold standard. However, like Bretton Woods, it was subject in September 1992 to intolerable strains because the commitment mechanism proved to be not credible for many of the members.

Under the classical gold standard, the monetary authorities committed themselves to fix the prices of their currencies in terms of a fixed weight of gold and to buy and sell gold freely in unlimited amounts. The pledge to fix the price of a country's currency in terms of gold represents the basic rule of the gold standard. The fixed price of domestic currency in terms of gold provided a nominal anchor to the international monetary system. Under the Bretton Woods system only the United States fixed the price of its currency in terms of gold. All other convertible currencies were pegged to the dollar. Also, under Bretton Woods, free convertibility of gold into dollars was limited. Thus Bretton Woods was a weak variant of the gold standard. Although the Bretton Woods system in its convertible phase (1959–71) was the most stable monetary regime of the past century, it was short lived. It collapsed both because of fatal flaws in its design (the adjustable peg in the face of improved capital mobility and the confidence problem associated with the gold dollar standard) and the lack of commitment by the United States to the gold standard convertibilty rule.

The EMS, although not based on gold, incorporated many of the features of the Bretton Woods adjustable peg system. Its success in promoting the convergence of inflation rates among its members in the 1980s has been linked to the presence of an effective commitment mechanism—the adherence by the German central bank to price stability and the willingness of other members to tie their currencies to the German mark. However, like Bretton Woods, it suffered serious stress in September 1992 in the face of massive shocks because of the basic incompatibility of pegged exchange rates, capital mobility and policy autonomy. Both the center country and the members were unwilling to commit to a common policy.

An overview of the three regimes as embodying the operation of credible monetary rules follows.

The Gold Standard as a Commitment Mechanism

In the recent literature on the time inconsistency of optimal government policy, the absence of a credible commitment mechanism leads governments, in pursuing stabilization policies, to produce an inflationary outcome.[57] In a closed economy environment, once the monetary authority has announced a given rate of monetary growth, which the public expects it to validate, the authority then has an incentive to create a monetary surprise to either reduce unemployment or capture seigniorage revenue. The public, with rational expectations, will come to anticipate the authorities' perfidy, leading to an inflationary equilibrium. A credible precommitment mechanism, by preventing the government from cheating, can preserve long-run price stability. The gold standard rule of maintaining a fixed price of gold can be viewed as such a mechanism.

The gold standard rule can be viewed as a form of contingent rule or a rule with escape clauses.[58] The monetary authority maintains the standard—that is, keeps the price of the currency in terms of gold fixed—except in the event of a well-understood

[57]See Kydland and Prescott (1977) and Barro and Gordon (1983).

[58]See Grossman and Van Huyck (1988), DeKock and Grilli (1989), and Bordo and Kydland (1992).

emergency, such as a major war or a financial crisis. In wartime it may suspend gold convertibility and issue paper money to finance its expenditures, and it can sell debt issues in terms of the nominal value of its currency on the understanding that debt will eventually be paid off in gold. The rule is contingent in the sense that the public understands that the suspension will last only for the duration of the wartime emergency plus some period of adjustment. It assumes that afterward the government will follow the deflationary policies necessary to resume payments at the original parity. Following such a rule will also allow the government to smooth its revenue from different sources of finance, such as taxation, borrowing and seigniorage.[59]

According to Bordo and Kydland (1992), the gold standard contingent rule worked successfully for three core countries of the classical gold standard: the United Kingdom, the United States, and France. In all these countries the monetary authorities adhered faithfully to the fixed price of gold except during major wars. During the Napoleonic War and World War I for England, the Civil War for the United States, and the Franco–Prussian War for France, specie payments were suspended, and paper money and debt were issued. But in each case, after the wartime emergency had passed, policies leading to resumption were adopted.[60] Indeed, successful adherence to the rule may have enabled the belligerents to obtain access to debt finance more easily in subsequent wars. Other countries, such as Italy, which did not continuously maintain gold convertibility, nevertheless adopted policies consistent with long-run convertibility.[61]

The gold standard rule may also have been enforced by reputational considerations. Long-run adherence to the rule was based on the historical evolution itself of the gold standard. Gold was accepted as money because of its intrinsic value and desirable properties. Paper claims, developed to economize on the scarce resources tied up in a commodity money, became acceptable only because they were convertible into gold. An alternative commitment mechanism was to guarantee gold convertibility in the constitution. This was the case for example in Sweden before 1914, when laws pertaining to the gold standard could be changed only by two identical parliamentary decisions with an election in between.[62] Convertibility was also enshrined in the laws of a number of gold standard central banks.[63]

The gold standard originally evolved as a domestic commitment mechanism, but its enduring fame is as an international rule. The classical gold standard emerged as a true international standard by 1880 following the switch by the majority of countries from bimetallism, silver monometalism and paper to gold as the basis of their currencies.[64] As an international standard, the key rule was maintenance of gold convertibility at the established par. Maintenance of a fixed price of gold by its adherents in turn ensured fixed exchange rates. Recent evidence suggests that, indeed, exchange rates throughout the 1880–1914 period were characterized by a high degree of fixity in the principal countries. Although exchange rates frequently deviated from par, violations of the gold points and devaluations were rare.[65]

According to game theory literature, for an international monetary arrangement to be effective both between countries and within them, a time-consistent credible commitment mechanism is required. Adherence to the gold convertibility rule provided such a mechanism. In addition to the

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[59]See Lucas and Stokey (1983) and Mankiw (1987).

[60]A case study comparing British and French finances during the Napoleonic Wars shows that Britain was able to finance its wartime expenditures by a combination of taxes, debt and paper money issue—to smooth revenue; whereas France had to rely primarily on taxation. France had to rely on a less efficient mix of finance than Britain because she had used up her credibility by defaulting on outstanding debt at the end of the American Revolutionary War and by hyperinflating during the Revolution. Napoleon ultimately returned France to the bimetallic standard in 1803 as part of a policy to restore fiscal probity, but because of the previous loss of reputation France was unable to take advantage of the contingent aspect of the bimetallic standard rule. See Bordo and White (1991).

[61]The behavior of asset prices (exchange rates and interest rates) suggests that market agents viewed the commitment to gold as credible. See Roll (1972) and Calomiris (1988), who present evidence of expected appreciation of the greenback during the American Civil War based on a negative interest differential between bonds that were paid in greenbacks and those paid in gold.

Giovannini (1992) finds that the variation of both exchange rates and short-term interest rates varied within the limits set by the gold points in the 1899–1909 period consistent with market agents' expectations of a credible commitment by the four "core" countries to the gold standard rule in the sense of this paper.

[62]See Jonung (1984).

[63]See Giovannini (1993).

[64]See Eichengreen (1985).

[65]See Officer (1986) and Eichengreen (1985).

reputation of the domestic gold standard and constitutional provisions that ensured domestic commitment, adherence to the international gold-standard rule may have been enforced by other mechanisms. These include improved access to international capital markets, the operation of the rules of the game, and the hegemonic power of England.

Support for the international gold standard likely grew because it provided improved access to the international capital markets of the core countries. Countries were eager to adhere to the standard because they believed that gold convertibility would be a signal to creditors of sound government finance and the future ability to service debt.[66]

This was the case both for developing countries seeking access to long-term capital, such as Austria-Hungary and Latin America, and for countries seeking short-term loans, such as Japan, which financed the Russo-Japanese war of 1905–06 with foreign loans seven years after joining the gold standard.[67] Once on the gold standard, these countries feared the consequences of suspension.[68] That England, the most successful country of the nineteenth century, as well as other progressive countries were on the gold standard was probably a powerful argument for joining.[69]

The operation of the rules of the game, whereby the monetary authorities were supposed to alter the discount rate to speed up the adjustment to a change in external balance, may also have been an important part of the commitment mechanism to the international gold standard rule. To the extent the rules were followed and adjustment facilitated, the commitment to convertibility was strengthened and conditions conducive to abandonment were lessened.

Evidence on the operation of the rules of the game questions their validity. Bloomfield (1959), in a classic study, showed that, with the principal exception of England, the rules were frequently violated in the sense that discount rates were not always changed in the required direction (or by sufficient amounts) and in the sense that changes in domestic credit were often negatively correlated with changes in gold reserves. In addition, a number of countries used gold devices—practices to prevent gold outflows.

For the major countries, however (at least before 1914) such policies were not used extensively enough to threaten the convertibility to gold—evidence of commitment to the rule.[70] Moreover, as McKinnon (1992) argues, to the extent that monetary authorities followed Bagehot's rule and prevented a financial crisis while seemingly violating the rules of the game, the commitment to the gold standard in the long run may have been strengthened.

An additional enforcement mechanism for the international gold standard rule may have been the hegemonic power of England, the most important gold standard country.[71] A persistent theme in the literature on the international gold standard is that the classical gold standard of 1880 to 1914 was a British-managed standard.[72] Because London was the center for the world's principal gold, commodities and capital markets, because of the extensive outstanding sterling-denominated assets, and because many countries substituted sterling for gold as an international reserve currency, some argue that the Bank of England, by manipulating its bank rate, could attract whatever gold it needed and, furthermore, that other central banks would adjust their discount rates accordingly. Thus the Bank of England could exert a powerful influence on the money supplies and price levels of other gold standard countries.

The evidence suggests that the Bank did have some influence on other European central banks.[73] Eichengreen (1987) treats the Bank of England as one engaged in a leadership role in a Stackelberg strategic game with other central banks as followers. The other central banks accepted a passive role because they benefited from using sterling as a reserve asset. According to this interpretation, the gold standard rule may

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[66]A case study of Canada during the Great Depression provides evidence for the importance of the credible commitment mechanism of adherence to gold. Canada suspended the gold standard in 1929 but did not allow the Canadian dollar to depreciate nor the price level to rise for two years. Canada did not take advantage of the suspension to emerge from the depression because of concern for its credibility with foreign lenders. See Bordo and Redish (1990).

[67]See Yeager (1984), Fishlow (1989) and Hayashi (1989).

[68]See Eichengreen (1989a) and Fishlow (1987 and 1989).

[69]See Friedman (1990) and Gallarotti (1991).

[70]See Schwartz (1984).

[71]See Eichengreen (1989b).

[72]See Bordo (1984).

[73]See Lindert (1969).

have been enforced by the Bank of England.[74] Thus the monetary authorities of many countries may have been constrained from following independent discretionary policies that would have threatened adherence to the gold standard rule.

Indeed, according to Giovannini (1989), the gold standard was an asymmetric system. England was the center country. It used its monetary policy (bank rate) to maintain gold convertibility. Other countries accepted the dictates of fixed parities and allowed their money supplies to respond passively. His regressions support this view—the French and German central banks adapted their domestic policies to external conditions, whereas the British did not.

The benefits to England as leader of the gold standard—from seigniorage earned on foreign-held sterling balances to returns to financial institutions generated by its central position in the gold standard and to access to international capital markets in wartime—were substantial enough to make the costs of not following the rule extremely high.

The classical gold standard ended in the face of the massive shocks of World War I.[75] The gold exchange standard, which prevailed for only a few years from the mid-1920s to the Great Depression, was an attempt to restore the beneficial features of the classical gold standard while allowing a greater role for domestic stabilization policy. This in turn created a growing conflict between adherence to the rule and discretion. It also attempted to economize on gold reserves by restricting its use to central banks and by encouraging the use of foreign exchange as a substitute. As is well known, the gold exchange standard suffered from a number of fatal flaws.[76] These include the use of two reserve currencies (the pound and the dollar), the absence of leadership by a hegemonic power, the failure of cooperation between the key members (England, France and the United States), and the unwillingness of its two strongest members, the United States and France, to follow the rules of the game. Instead they exerted deflationary pressure on the rest of the world by persistent sterilization of balance-of-payment surpluses. The gold exchange standard collapsed, but according to Friedman and Schwartz, Temin, and Eichengreen, not before transmitting deflation and depression across the world.[77]

The Bretton Woods International Monetary System

The planning that led to Bretton Woods aimed to prevent the chaos of the interwar period.[78] The perceived ills to be prevented included (1) floating exchange rates that were condemned as subject to destabilizing speculation; (2) a gold exchange standard that was vulnerable to problems of adjustment, liquidity and confidence, which enforced the international transmission of deflation in the early 1930s; and (3) the resort to beggar-thy-neighbor devaluations, trade restrictions, exchange controls and bilateralism after 1933. To prevent these ills, the case for an adjustable peg system was made by Keynes, White, Nurkse and others.[79] The new system would combine the favorable features of the fixed exchange rate gold standard—stability of exchange rates—and of the flexible exchange rate standard—monetary and fiscal independence.

Both Keynes, leading the British negotiating team at Bretton Woods, and White, leading the American team at Bretton Woods, planned an adjustable peg system to be coordinated by an international monetary agency. The Keynes plan gave the International Currency Union substantially more reserves and power than the United Nations Stabilization Fund proposed by White, but both institutions would have had considerable control over the domestic financial policy of the members.

The British plan contained more domestic policy autonomy than did the U.S. plan, whereas the

[74]According to Eichengreen (1989a), the Bank of England's ability to ensure convertibility was aided by the cooperation of other central banks. In addition, as mentioned above, belief based on past performance that England attached highest priority to convertibility encouraged stabilizing private capital movements in times of threats to convertibility, such as in 1890 and 1907.

[75]The standard deviations of both supply and demand shocks during World War I for the countries for which we have continuous data were two to three times as great as during the classical gold standard. See appendix table 1 and figure 1.

[76]See Kindleberger (1973), Temin (1989), and Eichengreen (1992c).

[77]See Friedman and Schwartz (1963), Temin (1989) and Eichengreen (1992c).

[78]This section draws heavily on Bordo (1992).

[79]See Bordo (1993).

American plan put more emphasis on exchange rate stability. Neither architect was in favor of a rule-based system.[80] The British were most concerned with preventing the deflation of the 1930s, which they attributed to the constraint of the gold standard rule and to deflationary U.S. monetary policies. Thus they wanted an expansionary system.

The American plan was closer to the gold standard rule in that it stressed the fixity of exchange rates. It did not explicitly mention the importance of rules as a credible commitment mechanism, but there were to be strict regulations on the linkage between UNITAS (the proposed international reserve account) and gold. Members, in the event of a fundamental disequilibrium, could change their parities only with approval from a three-quarters majority of all members of the fund.[81]

The Articles of Agreement of the International Monetary Fund incorporated elements of both the Keynes and White plans, although in the end, U.S. concerns predominated.[82] The main points of the articles were: the creation of the par value system; multilateral payments; the use of the fund's resources; its powers; and its organization.

The Par Value System

Article IV defined the numeraire of the international monetary system as either gold or the U.S. dollar of the weight and fineness on July 1, 1944. All members were urged to declare a par value and maintain it within a 1 percent margin on either side of parity. Parity could be changed in the event of a fundamental payments disequilibrium at the decision of the member, after consultation with other fund members. The fund would not, however, reject the change if it was not more than 10 percent; if the change was more than 10 percent, the fund would decide within 72 hours. Unauthorized changes in the exchange rate could make members ineligible to use the fund's resources, and if a member continued to make unauthorized changes, it could be expelled from the fund. A uniform change in par value of all currencies (in terms of gold) required approval by a majority of all voting fund members and also had to be approved by every member with 10 percent or more of the total quota.

Multilateral Payments

Members were supposed to make their currencies convertible for current account transactions (Art. VIII), but capital controls were permitted (Art. VI.3). They were also to avoid discriminatory currency and multiple curency arrangements. Countries could avoid declaring their currencies convertible, however, by invoking Art. XIV, which allowed a three-year transition period after establishment of the fund. During the transition period, existing exchange controls could be maintained.[83]

The Fund's Resources

As under the White plan, members could obtain resources from the fund to help finance short- or medium-term payments disequilibria. The total fund, contributed by members quotas (25 percent in gold and 75 percent in currencies) was set at $8.8 billion. It could be raised every five years if the majority of members wanted to do so. The fund set a number of conditions on the use of its resources by deficit countries to prevent it from accumulating soft currencies and from depleting its holdings of harder currencies.[84] It also established requirements and conditions for repurchase (repayment of a loan), including giving the fund the right to decide the currency in which repurchase would be made.

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[80]In the sense of a commitment mechanism to prevent the time consistency problem. According to Meltzer (1988) and Moggridge (1986), Keynes had a strong preference for rules over discretion, interpreting rules in the traditional sense.

[81]See Giovannini (1993).

[82]At the same time as the Articles of Agreement for the International Monetary Fund were signed, the International Bank for Reconstruction and Development (the World Bank) was established. The Charter of the International Trade Organization (ITO) was drafted and signed in 1947 but never ratified. It was succeeded by the General Agreement for Tariffs and Trade (GATT) originally negotiated in Geneva in 1947 as an interim institution until the ITO came into force.

[83]Under Art. XIV, three years after March 1, 1947, the IMF would begin reporting on the countries with existing controls, two years later it would begin consulting with individual members and advising them on policies to restore payments equilibrium and convertibility. Countries that did not make satisfactory progress would be censured and ultimately asked to leave the fund. In fact, the fund always accepted the member's reason for remaining under Art. XIV.

[84]Members could draw on their quotas without condition. Beyond that, referred to later as the credit tranches, although not spelled out in the articles, increasingly more exacting conditions were required.

In the case of countries prone to running large surpluses, the scarce currency clause (Art. VII) would come into play. If the fund's holdings of a currency were insufficient to satisfy the demand for it by other members, it could declare it scarce and then urge members to ration its use by discriminatory exchange controls.

The Powers of the Fund

The fund had considerably less discretionary power over the domestic policies of its members than either of the architects wanted, but it still had power to influence the international monetary system strongly. These powers included its authority to approve or reject changes in parity; the use of multiple exchange rates and other discriminatory practices; the conditionality implicit in members' access to the credit tranches of their quotas, which was made explicit by 1952; its authority to declare currencies scarce; its authority to declare members ineligible to use its resources (used against France in 1948 following an unapproved devaluation); and its ultimate authority to expel members.[85] The fund also had considerable power as the premier international monetary organization in consulting and cooperating with national and other international monetary authorities.

Organization

The fund was to be governed by a board of governors appointed by the members. The board would make the major policy decisions, such as approving a change in parity. Operations of the fund were to be directed by executive directors appointed by the members and a managing director selected by the executive directors. Major changes such as a uniform change in the par value of all currencies or the second amendment to the articles, which created the special drawing right (SDR), would require a majority vote by the members. The number of votes in turn was tied to the size of each member's quota, which was determined by its economic size.

Though the articles could not be interpreted strictly as a return to the gold standard rule of the fixed price of gold with free convertibility, the fixed price of gold at $35 per ounce, which the U.S. was to maintain, represented the nominal anchor of the system. Members were required to maintain parity of their exchange rates in terms of dollars (or gold). Also, like the gold standard, it was a rule with an escape clause. Members at their initiative could alter their parities in the event of a fundamental disequilibrium.

The architects never spelled out exactly how the system was supposed to work. Subsequent writers, however, have suggested a number of salient features.[86] First, currencies were treated as equal in the articles. This meant that in theory each country was required to maintain its par value by intervening in the currency of every other country—a practice that would have worked at cross purposes. In fact, because the United States was the only country that pegged its currency in terms of gold (bought and sold gold), all other countries would fix their parities in terms of dollars and would intervene to monitor their exchange rates within 1 percent of parity with the dollar.

Second, countries would use their international reserves or draw resources from the fund to finance payments deficits. In the case of surpluses, countries would temporarily build up reserves or repurchase their currencies from the fund. In the event of medium-term disequilibria, they would use monetary and fiscal policy to alter aggregate demand. In the event of a fundamental disequilibrium, which was never defined but presumably reflected either some permanent structural shock or sustained inflation, a member was supposed to alter parity by an amount sufficient to restore external equilibrium.

Third, capital controls were permitted to prevent destabilizing speculation from forcing members to alter their parities prematurely or unintentionally.

THE HISTORY OF BRETTON WOODS: PRE-CONVERTIBILITY 1946–58

The international monetary system that began after World War II was far different from the system that the architects of Bretton Woods envisioned. The transition period from war to

[85]See Diz (1984) for a discussion of conditionality implicit in members' access to the credit tranches of their quotas.

[86]See, for example, Tew (1988), Scammell (1976) and Yeager (1976). For Williamson (1985b) it was a comprehensive set of rules for assigning macroeconomic policies—exchange rates to medium-run external balance, monetary and fiscal policy to short-run internal balance and international reserves—to provide a buffer to allow short-run departures from external balance.

peace was much longer and more painful than anticipated. Full convertibility of the major industrial countries was not achieved until the end of 1958, although the system had started functioning normally by 1955. Two interrelated problems dominated the first postwar decade: bilateralism and the dollar shortage.

Bilateralism

The legacy of World War II for virtually every country except the United States was one of pervasive exchange controls and controls on trade. No major currency except the dollar was convertible.[87] Under Art. XIV of the Bretton Woods agreement, countries could continue to use exchange controls for an indefinite transition period after the establishment of the International Monetary Fund (IMF) on March 1, 1947. In conjunction with exchange controls, every country negotiated a series of bilateral payments agreements with each of its trading partners. The rationale for the continued use of controls and bilateralism was a shortage of international reserves. After the war, the economies of Europe and Asia were devastated. To produce the exports needed to generate foreign exchange, industries required new and improved capital. There was an acute shortage of key imports, both foodstuffs to maintain living standards and raw materials and capital equipment. Controls allocated the scarce reserves.

The Dollar Shortage

By the end of World War II, the United States held two-thirds of the world's monetary gold stock (see figure 13). The gold avalanche in the United States in the 1930s was the consequence of both the dollar devaluation in 1934, when the Roosevelt administration raised the price of gold from $20.67 per ounce to $35.00, and capital flight from Europe. During the war, gold inflows continued to finance wartime expenditures by the allies. At the end of World War II, gold and dollar reserves in Europe and Japan were depleted. Europe ran a massive current account deficit, reflecting the demand for essential imports and the reduced capacity of the export industries. The Organization for European Economic Cooperation (OEEC) deficit, aggravated by the bad winter of 1946–47, reached a high of $9 billion in 1947. The OEEC deficit equaled the amount of the U.S. current account surplus, which was large because the United States, as the only major industrial country operating at full capacity, supplied the needed imports. The dollar shortage was likely aggravated by overvalued official parities set by the major European industrial countries at the end of 1946.[88]

By the mid–1950s both problems had been solved. The currencies of western Europe were virtually convertible by 1955 and their current accounts were generally in surplus. The key developments in this progress were the Marshall Plan and the European Payments Union.

The Marshall Plan

The Marshall Plan funneled approximately $13 billion in aid (grants and loans) to western Europe between 1948 and 1952.[89] The plan required the recipients to cooperate in the liberalization of trade and payments. Consequently, the OEEC was established in April 1948. It presided over the allocation of aid to members based on the size of their current account deficits. U.S. aid was to pay for essential imports and to provide international reserves. Each recipient government provided matching funds in local currency to be used for investment in the productive capacity of industry, agriculture and infrastructure. Each country also had a delegation of U.S. administrators that advised the host government on the spending of its counterpart funds.

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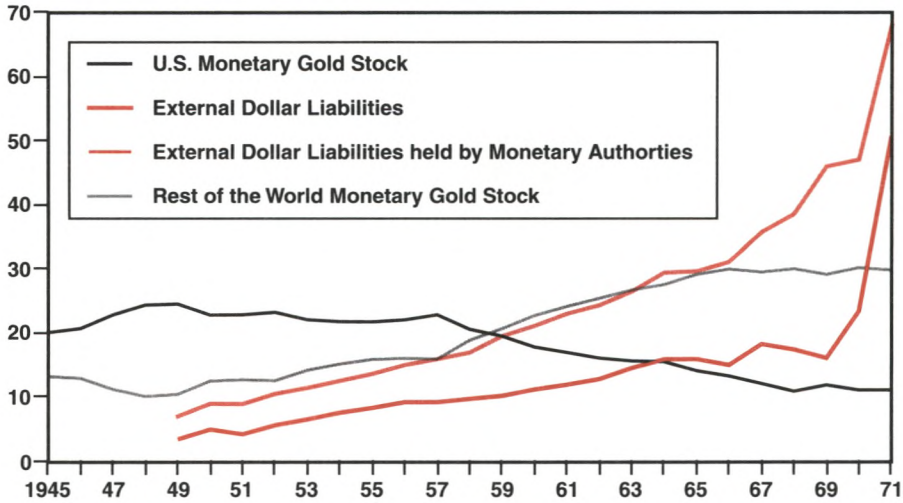
[87]Under the classical gold standard, convertibility meant the ability of a private individual to freely convert a unit of any national currency into gold at the official fixed price. A suspension of convertibility meant that the exchange rate between gold and national currency became flexible but the individual could still freely transact in either asset. See Triffin (1960). By the eve of World War II, convertibility referred to the ability of a private individual to freely make and receive payments in international transactions in terms of the currency of another country. Under Bretton Woods, convertibility meant the freedom for individuals to engage in current account transactions without being subject to exchange controls. Tew (1988) defines this as market convertibility and distinguishes it from official convertibility whereby the monetary authorities of each country freely buy and sell foreign exchange (primarily dollars) to keep the parity fixed (within the 1 percent margin) and the United States freely buys and sells gold to maintain the fixed price of $35 an ounce (within the 1 percent margin). He refers to both market and official convertibility as Bretton Woods convertibility. See also McKinnon (1979) and Black (1987).

[88]See Triffin (1957).

[89]See Milward (1984) and Hoffman and Maier (1984).

Figure 13

Monetary Gold and Dollar Holdings: the United States and the Rest of the World, 1945-1971 Billions of U.S. dollars



The plan encouraged the liberalization of intra-European trade and payments by granting aid to countries that extended bilateral credits to other members. Finally, the European Payments Union (EPU) was established in 1950, under the auspices of the OEEC, to simplify bilateral clearing and pave the way to multilateralism.

By 1952, in part thanks to the Marshall Plan, the OEEC countries had achieved a 39 percent increase in industrial production, a doubling of exports, an increase in imports by one-third and a current account surplus.[90]

The European Payments Union and the Return to Convertibility

It took 12 years from the declaration of official par values by 32 nations in December 1946 to achieve convertibility for current transactions by the major industrial countries, as specified by the Bretton Woods Articles. The Western European nations tried several schemes to facilitate the payments process before establishing the EPU in 1950.[91]

The EPU, established September 19, 1950, by the OEEC countries, initially was to run for two years, renewable thereafter on a yearly basis. It followed the basic principle of a commercial bank clearinghouse. At the end of each month, each member would clear its net debit or credit position (against all other members) with the EPU (the BIS acting as its agent). The unit of account for these clearings was the U.S. dollar. The EPU also provided extensive credit lines. The EPU was highly successful in reducing the volume of payments transactions and provided the background for the gradual liberalization of payments so that by 1953 commercial banks were able to engage in multicurrency arbitrage.[92] On December 27, 1958, eight countries declared their currencies convertible for current account transactions.

The movement to convertibility was aided by the devaluations of 1949. Following a speculative run on the pound in the summer of 1949, the British, 24 hours after informing the IMF, devalued the pound by 30.5 percent. Shortly thereafter, 23 countries reduced their parities by similar magnitudes in most cases.

The devaluations of 1949 were important for the Bretton Woods system for two reasons. First, they, along with the Marshall Plan aid, helped move the European countries from a current account deficit to a surplus, a movement important to the eventual restoration of convertibility. Second, they revealed a basic weakness of the

[90]Solomon (1976).

[91]Kaplan and Schleiminger (1989).

[92]Tew (1988) and Yeager (1976).

adjustable peg arrangement—the one-way option of speculation against parity. By allowing changes in parity only in the event of a fundamental disequilibrium, the Bretton Woods system encouraged the monetary authorities to delay adjustment until they were sure it was necessary. By that time, speculators also would be sure and they would take a position from which they could not lose. If the currency is devalued, they win and if it is not, they just lose the interest (if any) on the speculative funds.[93] The crisis associated with the 1949 sterling devaluation in turn created further resistance by monetary authorities to changes in parity, which ultimately changed the nature of the international monetary system from the adjustable peg intended by the Bretton Woods Articles to a fixed rate regime.

Other developments in the preconvertiblity period included the decline of sterling as a reserve asset and the reduced prestige of the IMF. The IMF by intention was not equipped to deal with the postwar reconstruction problem. Although some limited drawings occurred before 1952, most of the structural balance of payments assistance in this period was provided by the Marshall Plan and other U.S. assistance, including the Anglo-American Loan of 1946. The consequence of this development is that other institutions such as the BIS, the agent for the EPU, emerged as competing sources of international monetary authority.[94]

The fund's prestige was dealt a severe blow by three events in the preconvertibility period. The first event was the French devaluation of January 1948, which created a multiple exchange rate system. The fund censured France for creating broken cross rates between the dollar and the pound. France was denied access to the fund's resources until 1952. France ended the broken cross rates in October 1948 and adopted a unified rate in the devaluation of 1949. Since France had access to the Marshall Plan, the fund's actions had little effect. The second event was the sterling devaluation of September 1949, when the fund, instead of being actively involved in consultation, was given 24 hours perfunctory notice. The third event was the decision by Canada to float its currency in September 1950. Though the fund was highly critical of the action, it was unable to prevent it. The Canadian dollar floated successfully until 1961.

Finally, the fund's resources were inadequate to solve the emerging liquidity problem of the 1960s. The difference between the required growth of international reserves (to finance the growth of real output and trade and to avoid deflation) and the growth in the world's monetary gold stock was met largely by an increase in official holdings of U.S. dollars resulting from growing U.S. balance-of-payments deficits. By the time full convertibility was achieved, the U.S. dollar was serving the buffer function intended by the Bretton Woods Articles for the fund's resources.[95]

THE HISTORY OF BRETTON WOODS: THE HEYDAY OF BRETTON WOODS 1959-1967

With current account convertibility established by the western European industrial nations at the end of December 1958, the full-blown Bretton Woods system was in operation. Each member intervened in the foreign exchange market, either buying or selling dollars, to maintain its parity within the prescribed 1 percent margins. The U.S. Treasury in turn pegged the price of the dollar at $35 per ounce by freely buying and selling gold. Thus each currency was anchored to the dollar and indirectly to gold. Triangular arbitrage kept all cross rates within a band of 2 percent on either side of parity. Through much of this period, capital controls prevailed in most countries except the United States in one form or another, although by the mid-1960s their use declined while increasing in the United States.

The system that operated in the next decade turned out to be quite different from what the architects had in mind. First, instead of a system of equal currencies, it evolved into a variant of the gold exchange standard—the gold-dollar system. Initially, it was a gold exchange standard with two key currencies, the dollar and the pound. But the role of the pound as key currency declined steadily throughout the 1960s. Concurrently with the decline of sterling was the rise in the dollar as a key currency. Use of the dollar as both a private and official international money increased dramatically in the 1950s and continued into the 1960s. With full convertibility, the dollar's fundamental role as

[93]See Friedman (1953).

[94]See Mundell (1969).

[95]See Mundell (1969).

intervention currency led to its use as international reserves. This was aided by stable and low monetary growth and relatively low inflation (before 1965). See figure 1 and table 1.

The gold exchange standard evolved in the post–World War II period for the same reasons it did in the 1920s—to economize on non-interest-bearing gold reserves. By the late 1950s, the growth of the world's monetary gold stock was insufficient to finance the growth of world real output and trade.[96] The other intended source of international liquidity—the resources of the fund—was also insufficient.

The second important difference between the convertible Bretton Woods system and the intentions of the Bretton Woods Articles was the evolution of the adjustable peg system into a virtual fixed exchange rate system. Between 1949 and 1967, very few changes in parities of the Group of Ten countries occurred.[97] The only exceptions were the Canadian float in 1950, devaluations by France in 1957 and 1958, and minor revaluations by Germany and the Netherlands in 1961. The adjustable peg system became less adjustable because the monetary authorities, based on the 1949 experience, were unwilling to accept the risks associated with discrete changes in parities—loss of prestige, the likelihood that others would follow and the pressure of speculative capital flows if even a hint of a change in parity were present.

As the system evolved into a fixed exchange rate gold dollar standard, the three key problems of the interwar system reemerged: adjustment, liquidity and confidence. These problems dominated academic and policy discussions during the period.

The Adjustment Problem

The adjustment issue focused on how to achieve it in a world with capital controls, fixed exchange rates and domestic policy autonomy. Various policy measures were proposed to aid adjustment, including income policies, rescue packages, capital and trade controls, a mix of monetary and fiscal policy, and the injection of new liquidity. Of particular interest during the period was asymmetry in adjustment between deficit countries like the United Kingdom and surplus countries like Germany and between the United States as the reserve currency country and rest of the world.

Both the United Kingdom and Germany ran the gauntlet between concern over external convertibility and domestic stability. The United Kingdom alternated between expansionary policy that led to balance-of-payments deficits and austerity. Germany alternated between a balance-of-payments surplus that led to inflation and austerity.

The United States had an official settlements balance-of-payments deficit in 1958 that persisted, with the notable exception of 1968–69, until the end of Bretton Woods. See figure 14. With the exception of 1959, however, the United States had a current account surplus until 1970. The balance-of-payments deficit under Bretton Woods arose because capital outflows exceeded the current account surplus. In the early postwar years, the outflow consisted largely of foreign aid. By the end of the 1950s, private long-term investment abroad (mainly direct investment) exceeded military expenditures abroad and other official transfers.[98]

The balance-of-payments deficit was perceived as a problem by the U.S. monetary authorities because of its effect on confidence. As official dollar liabilities held abroad mounted with successive deficits, the likelihood increased that these dollars would be converted into gold and eventually the U.S. monetary gold stock would reach a point low enough to trigger a run. Indeed the U.S. monetary gold stock by 1959 equalled total external dollar liabilities and the rest of the world's monetary gold stock exceeded that of the United States. See figure 13. By 1964 official dollar liabilities held by foreign monetary authorities exceeded the U.S. monetary gold stock.

A second reason the balance-of-payments deficit was perceived as a problem was the dollar's role in providing liquidity to the rest of the world. Elimination of the U.S. deficit would create a worldwide liquidity shortage.

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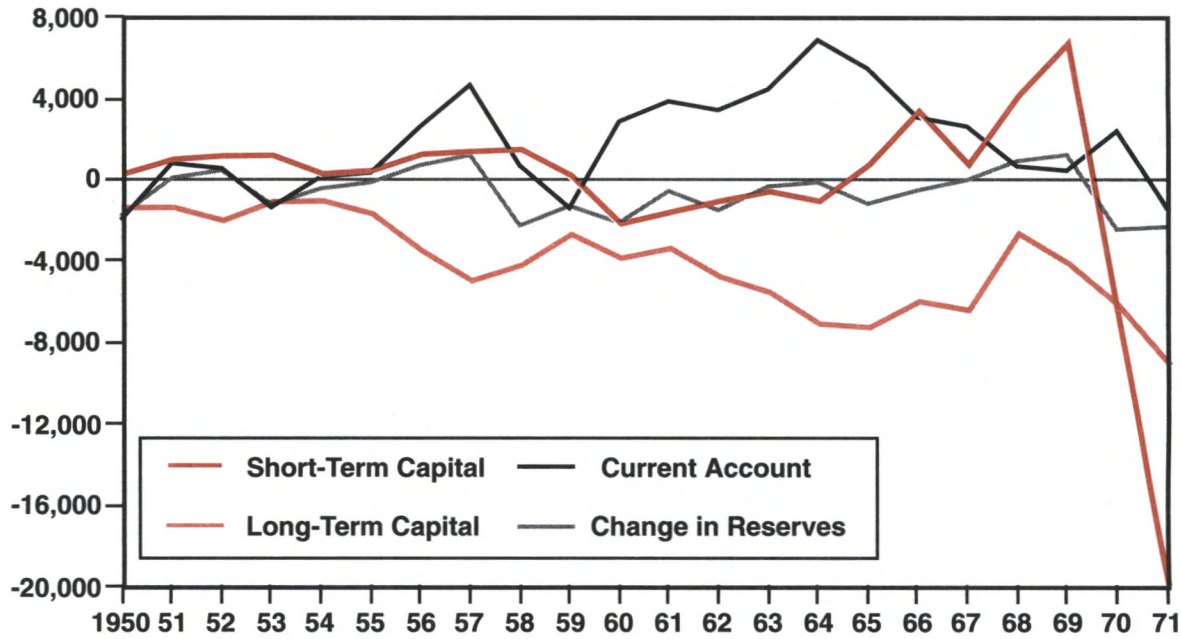
[96]See Triffin (1960) and Gilbert (1968).

[97]The Group of Ten countries were Belgium, Canada, France, West Germany, Italy, Japan, the Netherlands, Sweden, United Kingdom, and the United States. Switzerland was an associate member.

[98]See Eichengreen (1991c).

Figure 14

Balance of Payments: United States, 1950-1971 Millions of U.S. dollars



For the Europeans, the U.S. balance-of-payments deficit was a problem for different reasons. First, as the reserve currency country, the United States did not have to adjust its domestic economy to the balance of payments. As a matter of routine, the Federal Reserve automatically sterilized dollar outflows. The asymmetry in adjustment was resented. The Germans viewed the situation as the United States exporting inflation to surplus countries through its deficits. Their remedy was for the United States (and the United Kingdom) to pursue contractionary monetary and fiscal policy.[99] In fact, U.S. inflation was less (on a GNP-weighted average basis) than that of the rest of the Group of Seven countries before 1968. See figures 1 and 15. The French resented U.S. financial dominance and the seigniorage they believed the United States earned on its outstanding liabilities. Acting on this perception, the French in 1965 began to systematically convert outstanding dollar liabilities into gold. The French solution to the dollar problem was to double the price of gold—the amount by which the real price of gold had declined since 1934. The capital gains earned on the revaluation of the world's monetary gold reserves would be sufficient to retire the outstanding dollar (and sterling) balances. Once the United States returned to balance-of-payments equilibrium, the world could return to a fully functioning classical gold standard.[100]

Some economists argued that the U.S. balance-of-payments deficit was not really a problem. The rest of the world held dollars voluntarily because of their valuable service flow; the deficit was demand determined.[101]

The policy response of the U.S. monetary authorities was fourfold: to impose controls on capital exports; to institute measures to improve the balance of trade; to alter the monetary fiscal policy mix; and to employ measures to stem the conversion of outstanding dollars into gold.

During this period, various solutions to the U.S. adjustment problem were proposed: provision of an alternative international reserve

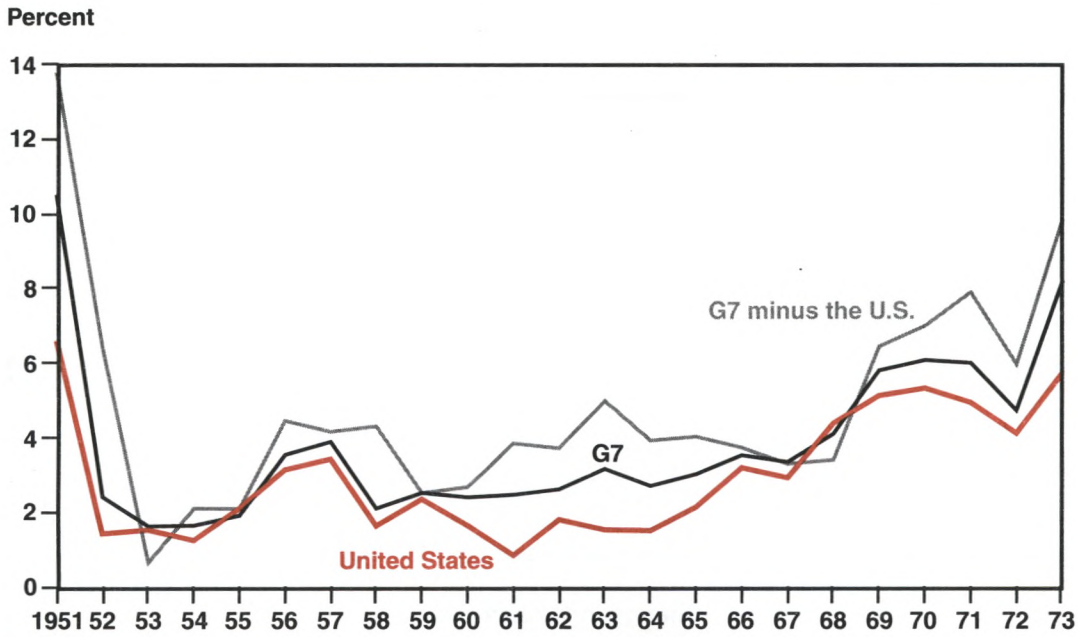
[99]See Emminger (1967).

[100]See Rueff (1967).

[101]See Despres, Kindleberger and Salant (1966).

Figure 15

Inflation Rates in the United States, G7 and G7 Excluding the United States, 1951-1973



media to increase world liquidity; an increase in the price of gold, either unilaterally, which would devalue the dollar against other currencies, or by a uniform change in all parities as under Art. IV; and increased exchange rate flexibility.[102]

The U.S. balance-of-payments policies were in the main ineffective.[103] As long as the United States maintained relatively stable prices, as it did before 1965, the system could be preserved for a number of years. The real problem was that of the gold exchange standard—a convertibility crisis was ultimately inevitable. The twin solutions advocated at the time of an increase in the price of gold and an increase in world liquidity by creation of an artificial reserve asset would not have permanently eradicated the problem.[104]

The Liquidity Problem

The liquidity problem, posed by Robert Triffin and others, evolved from a shortfall of monetary gold beginning in the late 1950s. The real price of gold had been falling since World War II and would eventually reduce world gold production. This happened in the early 1950s but was offset by new sources of production. Gold production declined again in 1966. Moreover, the falling real price would stimulate private demand for gold and it seemed unlikely that Russian gold sales would make up much of the shortfall. The prospect of the world monetary gold stock growing enough to finance the growth of world real output and the value of trade (without deflation) seemed dim. As can clearly be seen in figure 16 for the Group of

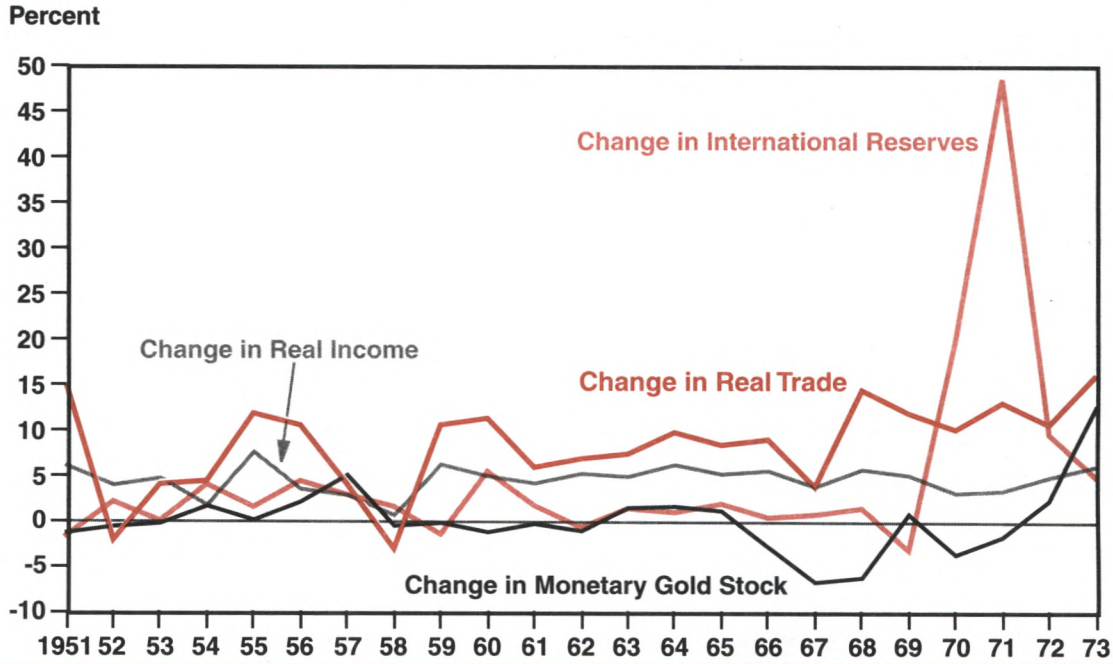
[102]The official view, which was strongly opposed to increased exchange rate flexibility, is in marked contrast to the academic view, which by the end of the decade was solidly in favor of increased flexibility, as evident at the famous Burgenstock Conference. See Halm (1970) and also Johnson (1972a).

[103]See Meltzer (1991).

[104]Even at a higher gold price, world gold production would eventually be inadequate to produce long-run price stability. In the long-run, when account is taken of gold as a durable exhaustible resource, deflation is inevitable. See Bordo and Ellson (1985). Moreover, an increase in world liquidity by an artificial reserve asset, if convertible into gold, would not remove the basic convertibility problem. See McKinnon (1988). Finally as Townsend (1977), Salant (1983) and Buiter (1989) point out, the gold exchange standard as a type of commodity stabilization scheme is bound to collapse in the face of unforeseen shocks. See Garber (1993). According to Meltzer (1991), however, a 50 percent gold revaluation would have succeeded in preserving the Bretton Woods system well into the 1970s had the United States not followed an inflationary policy in the late 1960s.

Figure 16

The Growth of the Monetary Gold Stock, the Growth in International Reserves and the Growth of the Volume of Real Trade and Real Income, G7, 1951-1973



Seven countries, this was the case. A large gap opened in 1958 between the growth of output and the volume of trade and the growth of Group of Seven gold reserves. As can be seen in figure 17, the shortfall for the Group of Seven countries, *excluding the United States*, was made up by a drain on the U.S. monetary gold reserves until 1966.

As Triffin (1960) pointed out, dollars supplied by the U.S. deficit could not be a permanent solution to the impending gold shortage because with continuous deficits, U.S. monetary gold reserves would decline both absolutely and relatively to outstanding dollar liabilities until an eventual convertibility crisis. Should the U.S. monetary authorities close the deficit before such a crisis, however, it would create a massive shortage of international liquidity and the prospect of world deflation. New sources of liquidity were required, answered by the creation of the special drawing rights in 1967. By the time SDRs were injected into the system in 1970, however, they exacerbated worldwide inflation.

The Confidence Problem

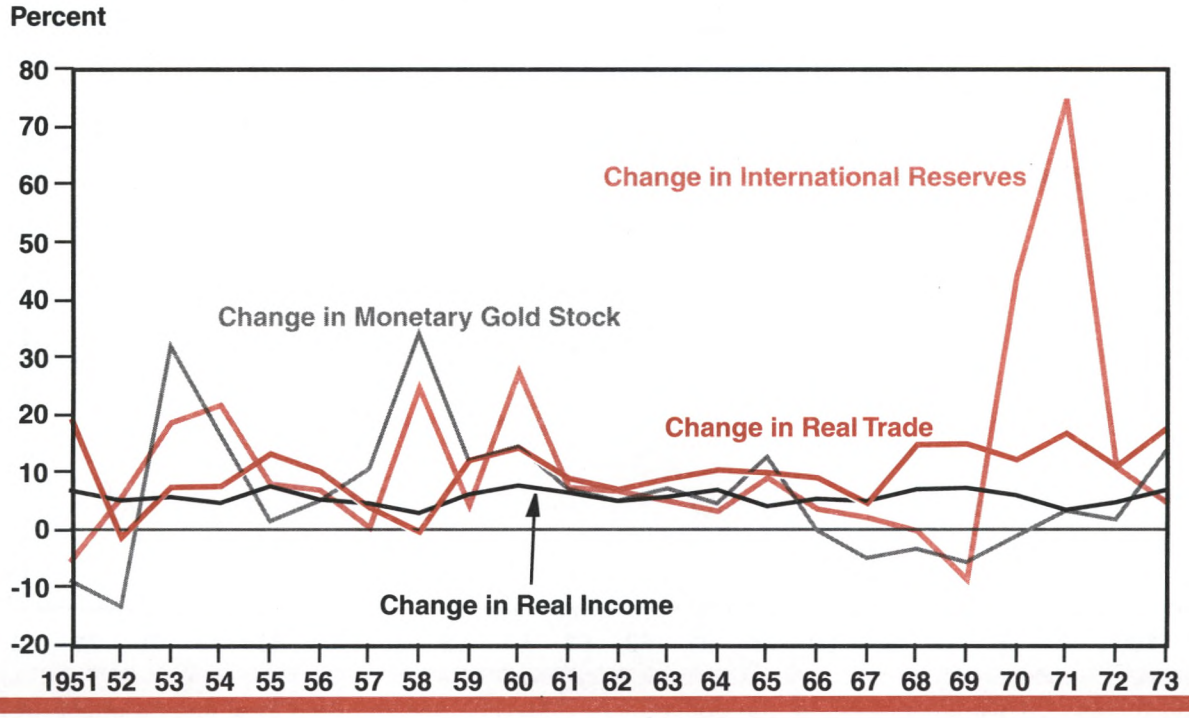
The perceived key problem of the convertible Bretton Woods period was the confidence crisis for the dollar.[105] As argued by Triffin (1960), Kenen (1960) and Gilbert (1968), the gold-dollar system that evolved after 1959 was bound to be dynamically unstable if the growth of the world monetary gold stock was insufficient to finance the growth of world output and trade and to prevent the U.S. monetary gold stock from declining relative to outstanding U.S. dollar liabilities. The pressure on the U.S. monetary gold stock would continue, as growth of the world monetary gold stock declined relative to the growth of world output and trade and as the world substituted dollars for gold, until it triggered a confidence crisis that led to the collapse of the system, as occurred in 1931. At the same time, however, as fears over U.S. gold convertibility threatened the dynamic stability of the Bretton Woods system, gold still served two positive roles.

Gold was the numeraire of the system; all

[105]Although according to Meltzer (1991), there is little evidence in asset markets through the 1960s of a growing loss of confidence in the dollar. Real interest rates did not rise significantly relative to trade weighted real interest rates. Nor did the gold and foreign exchange markets suggest a flight from the dollar.

Figure 17

The Growth of the Monetary Gold Stock, the Growth in International Reserves and the Growth of the Volume of Real Trade and Real Income, G7 Minus the United States, 1951-1973



currencies were anchored to its fixed price through the U.S. commitment to peg its price. Until 1968 gold still served as backing to the U.S. dollar with a 25 percent gold reserve requirement against Federal Reserve notes; the requirement may have served as a brake on U.S. monetary expansion.

The first glimpse of a confidence crisis was the gold rush of October 1960 when speculators pushed the free market price of gold on the London market up from $35.20 (the U.S. Treasury's buying price) to $40. See figure 18. This first significant runup in gold prices since the London gold market was reopened in 1954 was supposedly triggered by concerns over a Democratic victory in the 1960 U.S. Presidential election.

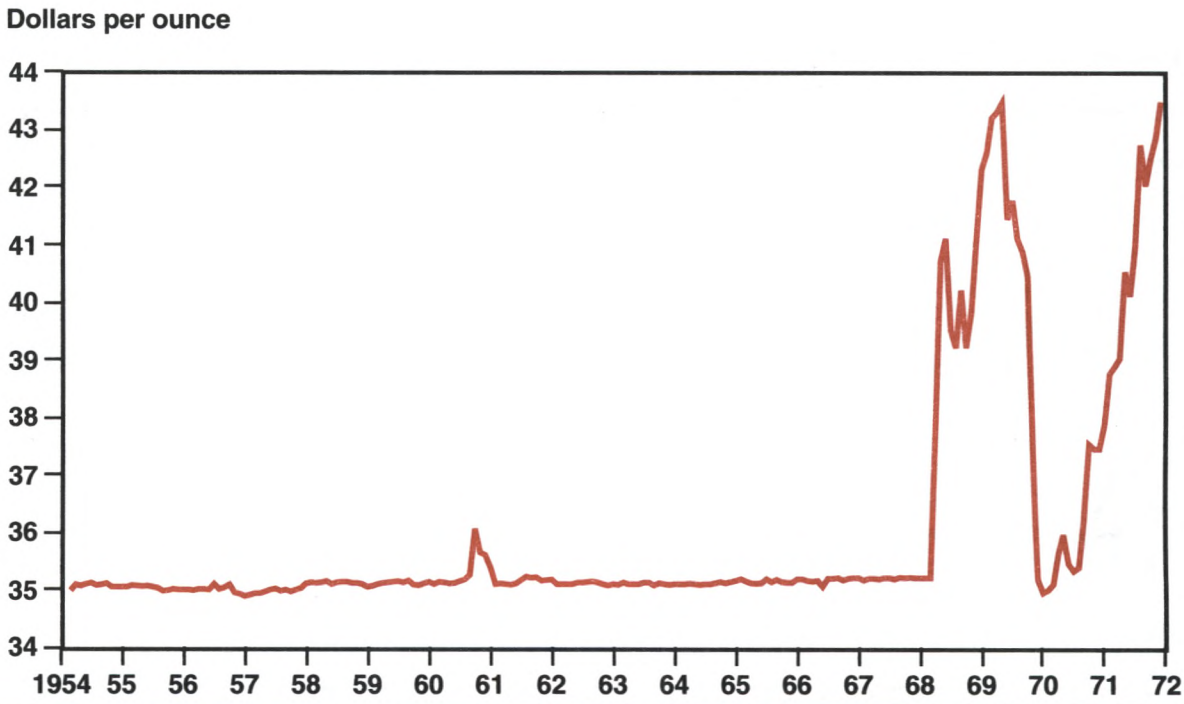
U.S. monetary authorities feared that private speculation in the gold market might spill into official demands for conversion. Consequently, remedial action was taken quickly. The Treasury supplied the Bank of England sufficient gold to restore stability, and the monetary authorities of the Group of Ten countries agreed to refrain from buying gold above $35.20. In succeeding months, the London Gold Pool, which agreed to buy or sell gold to peg the price at $35 an ounce, was formed between the United States and seven other countries. The pool became official in November 1961. For the next six years, it succeeded in stabilizing the price of gold but did not prevent a steady decline in the U.S. monetary gold stock. See figure 13. In fact, though the central banks in the seven other countries supplied 40 percent of the gold required to stabilize the price of gold, they replenished their monetary gold stocks outside the pool by converting outstanding dollar balances into gold at the U.S. Treasury.[106]

During the period 1961–67, the United States made a series of arrangements to protect its monetary gold reserves. These included a network of swap arrangements with other central banks, the issue of Roosa bonds, and moral suasion. France, however, did not go along with these efforts and began its campaign against the dollar in February 1965.

The period was marked by two sets of under-

[106]See Meltzer (1991).

Figure 18
London Gold Price



lying forces that would undermine the dollar's relationship to gold—growing gold scarcity and a rise in U.S. inflation. World gold production leveled off in the mid-1960s and even declined in 1966, while at the same time private demand soared, precipitating a drop in the world monetary gold stock after 1966. Indeed, beginning in 1966, the London Gold Pool became a net seller of gold. Also, U.S. money growth accelerated in 1965, in part to finance the Vietnam War, and inflation began to rise (figures 1 and 15). The current account surplus began to deteriorate in 1964 (figure 14), as did U.S. competitiveness, mirrored in a rise in the ratio of U.S. unit labor costs relative to trade weighted unit labor costs.[107] The balance-of-payments deficit worsened between 1964 and 1966 but was reversed in 1966 by capital inflows triggered by tight monetary policy.

After the devaluation of sterling, which the United States tried unsuccessfully to prevent, pressure mounted against the dollar via the London Gold market. From December 1967 to March 1968 the Gold Pool lost $3 billion in gold, with the U.S. share at $2.2 billion.[108] The immediate concerns of the speculators may have been fears of a dollar devaluation, but according to Gilbert and Johnson, it reflected the underlying gold scarcity.[109] In the face of the pressure, the Gold Pool was disbanded on March 17, 1968, and a two-tier arrangement replaced it. Henceforth, the monetary authorities of the Gold Pool agreed neither to sell nor to buy gold from the market. They would transact only among themselves at the official $35 price. In addition, on March 12, 1968, the United States removed the 25 percent gold reserve requirement against Federal Reserve notes. The key consequence of these new arrangements was that gold was demonetized at the margin. The link between gold production and other market sources of gold and official reserves was cut. Moreover, in the following years, the United States put considerable pressure on other monetary authorities to refrain from converting their dollar holdings to gold.

[107]See Meltzer (1991).

[108]See Solomon (1976).

[109]See Gilbert (1968) and Johnson (1968).

By 1968 the international monetary system had evolved very far indeed from the model of the architects of the Articles of Agreement. In reaction to both developments in financial markets and the confidence problem, the system had evolved into a *de facto* dollar standard. Gold convertibility, however, still played a role. Though the major industrial countries tacitly agreed not to convert their outstanding dollar liabilities into U.S. monetary gold, the threat of their doing so was always present. At the same time, as the countries of continental Europe and Japan gained economic strength relative to the United States, they became more reluctant to absorb outstanding dollars. They also were reluctant to adjust their surpluses by revaluing their currencies, increasingly coming to believe that adjustment should be undertaken by the United States.

The system had also developed into a *de facto* fixed exchange rate system. Unlike the classical gold standard, however, where the fixed exchange rate was the voluntary focal point for both internal and external equilibrium, in the Bretton Woods system exchange rates became fixed because members feared the consequences of allowing them to change. Nevertheless, because of increased capital mobility, the pressure for altering the parities of countries with persistent deficits and surpluses became harder to stop through the use of domestic policy tools and the aid of international rescue packages. Pressure increased from both academic and official sources for greater exchange rate flexibility.

By 1968, the system had also evolved a form of international governance that was quite different from that envisioned at the beginning. Instead of a community of equal currencies managed by the IMF, the system was managed by the United States in cooperation with the other members of the Group of Ten countries. In many respects, it was closer to the key currency system proposed by Williams.[110]

According to Dominguez (1993), the IMF was designed to facilitate international cooperation by serving as a commitment mechanism. It was to use its influence and its financial to enforce the par value system. It did not, however, have sufficient power to prevent devaluations by major countries and its financial resources were too limited to provide adequate adjustment assistance for them. The IMF still had an important role as a clearinghouse for different views on monetary reform, as a center of information, as the principal voice for the countries of the world other than the Group of Ten countries, as these countries' primary source of adjustment assistance and finally as an important partner in the major Group of Ten rescue packages.

In sum, the problems of the interwar system that Bretton Woods was designed to prevent reemerged with a vengeance. The fundamental difference, however, was that the system was not likely to collapse into deflation as in 1931 but rather explode into inflation.

THE COLLAPSE OF BRETTON WOODS

After the establishment of the two-tier arrangement, the world monetary system was on a *de facto* dollar standard. The system became increasingly unstable until it collapsed with the closing of the gold window in August 1971. The collapse of a system beset by the fatal flaws of the gold exchange standard and the adjustable peg was triggered by an acceleration in world inflation, in large part the consequence of an earlier acceleration of inflation in the United States. Before 1968, the U.S. inflation rate was below that of the GNP weighted inflation rate of the Group of Seven countries excluding the United States (see figure 15). It began accelerating in 1964, with a pause in 1966–67. The increase in inflation in the United States and the rest of the world was closely related to an increase in money growth and in money growth relative to the growth of real output. (See figures 19 and 20.) Indeed, a prevalence of excess demand shocks in the mid- and late 1960s is apparent for the United States and other Group of Seven countries in figures 11 and 12.

Darby et al (1983) provided considerable evidence on the transmission of inflation in the

[110]See Williams (1936 and 1943) and Johnson (1972b).

Figure 19

Money (M1) Growth Rates in the United States, G7 and G7 Excluding the United States, 1951-1973

Percent

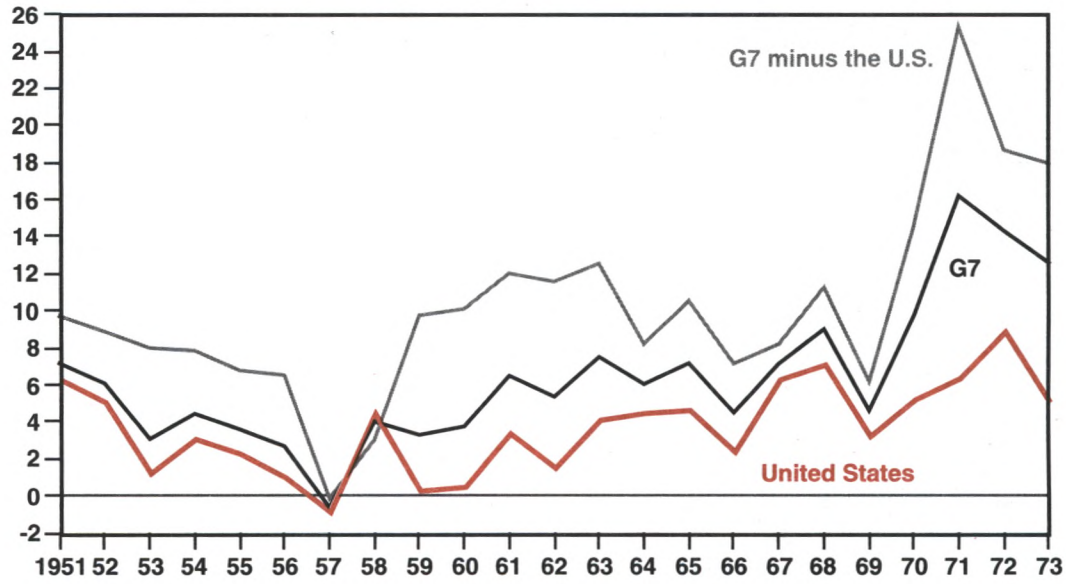
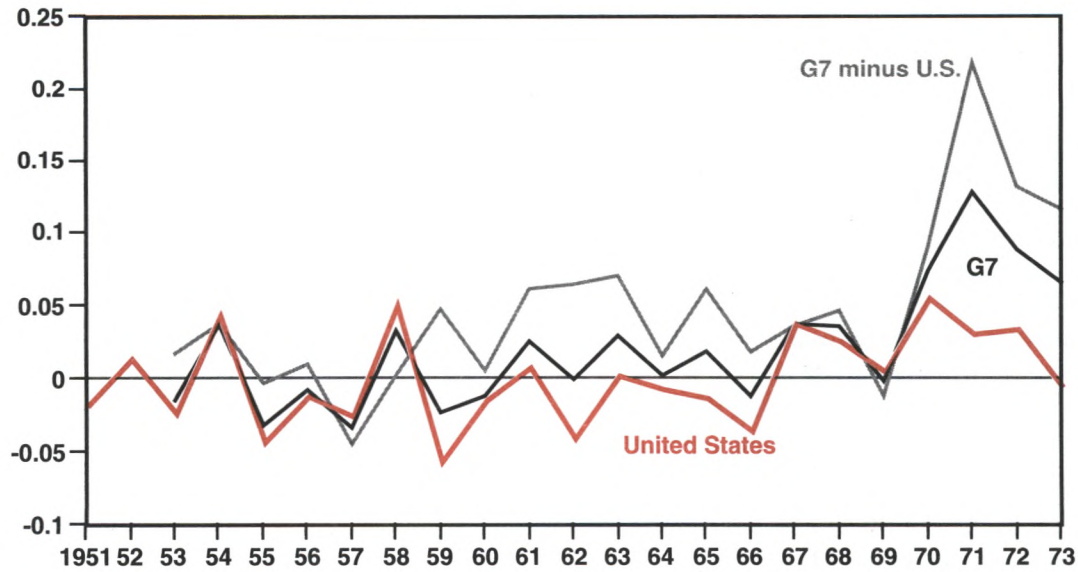


Figure 20

Money (M1) Less Real Output Growth in the United States, G7 and G7 Excluding the United States, 1951-1973

Percent



Bretton Woods system. Their regression analyses led to a number of important conclusions. First, U.S. inflation was caused by lagged U.S. money growth. Second, U.S. money growth was independent of changes in international reserves—the balance of payments had no effect on the Federal Reserve's reaction function. Third, U.S. money growth had strong and significant effects on money growth in seven major countries. These lags were very long—up to four years—and reflected the fact that central banks in the seven countries sterilized reserve flows partially. Finally, money growth in the seven countries explained inflation in these countries with a significant lag.[111]

The key transmission mechanism of inflation was the classical price specie flow mechanism augmented by capital flows. Little evidence for other mechanisms including commodity market arbitrage was detected.[112] According to these authors, the Bretton Woods system collapsed because of the lagged effects of U.S. expansionary monetary policy. As the dollar reserves of Germany, Japan and other countries accumulated in the late 1960s and early 1970s, it became increasingly more difficult to sterilize them. This fostered domestic monetary expansion and inflation. In addition, world inflation was aggravated by expansionary monetary and fiscal policies in the rest of the Group of Seven countries, as their governments adopted full-employment stabilization policies. The only alternative to importing U.S. inflation was to float—the route taken by all countries in 1973.[113]

The crisis mounted from 1968 to 1971. The U.S. current account balance continued to deteriorate in 1968, but the overall balance of payments exhibited a surplus in 1968 and 1969 thanks to a large short-term capital inflow. The capital inflow was activated by events in the eurodollar market. In the face of tight monetary policy in 1968–69 and Regulation Q ceilings on time deposits, deposits shifted from U.S. banks to the eurodollar market. U.S. banks in turn borrowed in the eurodollar market, repatriating these funds. In 1970, as U.S. interest rates fell in response to rapid monetary expansion and Regulation Q was suspended for large certificates of deposit, the borrowed funds returned abroad and the deficit grew to $9 billion, exploding to $30 billion by August 1971 (see figure 14). The dollar flood increased the reserves of the surplus countries, auguring inflation. German money growth doubled from 6.4 percent to 12 percent in 1971, and the German inflation rate increased from 1.8 percent in 1969 to 5.3 percent in 1971.[114] Pressure mounted for a revaluation of the mark. In April 1971 the dollar inflow to Germany reached $3 billion. On May 5, 1971, the German central bank suspended official operations in the foreign exchange market and allowed the deutsche mark to float. Similar action by Austria, Belgium, the Netherlands and Switzerland followed.[115]

In the following months, many began advocating ending the dollar's link with gold. In April 1971, the U.S. balance of trade turned to deficit for the first time and influential voices began urging dollar devaluation. The decision to suspend gold convertibility was triggered by French and British intentions in early August to convert dollars into gold. On August 15 at Camp David, President Nixon announced that he had directed Secretary Connolly "to suspend temporarily the convertibility of the dollar into gold or other reserve assets." The accompanying policy package included a 90-day wage-price freeze, a 10 percent import surcharge and a 10 percent investment tax credit.[116]

The U.S. decision to suspend gold convertibility ended a key aspect of the Bretton Woods system. The remaining part of the system—the adjustable peg—disappeared 19 months later.

The Bretton Woods system collapsed for three basic reasons. First, two major flaws undermined the system. One flaw was the gold exchange standard, which placed the United States under threat of a convertibility crisis. In reaction it pursued policies that in the end made adjustment more difficult.

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[111]See Darby et al (1983)

[112]See Darby et al (1983).

[113]Except for the case of Japan, there is little evidence for the leading alternative explanation for the collapse—that it reflected growing misalignment in real exchange rates between the United States and her principal competitors in the face of differential productivity trends. See Marston (1987) and Eichengreen (1992b).

[114]See Meltzer (1991).

[115]See Solomon (1976).

[116]See Solomon (1976).

The second flaw was the adjustable peg. Because the costs of discrete changes in parities were deemed high, in the face of growing capital mobility, the system evolved into a reluctant fixed exchange rate system without an effective adjustment mechanism.

Finally, U.S. monetary policy was inappropriate for a key currency. After 1965, the United States, by inflating, followed an inappropriate policy for a key currency country. Though the acceleration of inflation was low by the standards of the following decade, when superimposed on the cumulation of low inflation since World War II, it was sufficient to trigger a speculative attack on the world's monetary gold stock in 1968, leading to the collapse of the Gold Pool.[117] Once the regime had evolved into a *de facto* dollar standard, the obligation of the United States was to maintain price stability. Instead, it conducted an inflationary policy, which ultimately destroyed the system.

DID THE BRETTON WOODS SYSTEM OPERATE AS A SYSTEM BASED ON CREDIBLE RULES?

One can view the Bretton Woods system as a set of rules or commitment mechanisms.[118] For nonreserve-currency countries the rules were to maintain fixed parities, except in the contingency of a fundamental disequilibrium in the balance of payments, and to use fiscal policy to smooth out short-run disturbances. The U.S. enforcement mechanism—access to its open capital markets—was presumably its dominant power because the IMF had little power.

For the United States, the center country, the rule was to fix the gold price of the dollar at $35 per ounce and to maintain price stability. If a majority of Bretton Woods members (and every member with 10 percent or more of the total quotas) agreed, however, the United States could change the dollar price of gold. There was no explicit enforcement mechanism other than reputation and the commitment to gold convertibility. According to Giovannini (1993), the Bretton Woods system was an asymmetric solution to Mundell's n-1 currency problem.[119] The United States as the nth country, had to maintain the nominal anchor by following a stable monetary policy. In addition, it had to supply the dollars demanded by the rest of the world as reserves.

The rest of the world had to accept, through its commitment to fixed parities, the price level set by the United States. But because of the adjustable peg, it had the option to change parities. The rule was defective for the nonreserve currencies because the fundamental disequilibrium contingency was never spelled out and no constraint was placed on the extent to which domestic financial policy could stray from maintaining external balance. In addition, with growing capital mobility, the option to change parities became less viable.

For the United States this rule suffered from a number of fatal flaws. First, because of the fear of a confidence crisis, the gold convertibility requirement may have prevented the United States in the early 1960s from acting as a center country and willingly supplying the reserves demanded by the rest of the world. Second, as became evident in the later 1960s, this requirement was useless in preventing U.S. monetary authorities from pursuing an inflationary policy. Finally, although a mechanism was available for the United States to devalue the dollar, monetary authorities were loath to use it for fear of undermining confidence. No effective enforcement mechanism existed. Ultimately, the United States attached greater importance to domestic economic concerns than to its role as the center of the international monetary system.

Thus although the Bretton Woods system can be interpreted as one based on rules, the system did not provide a credible commitment mechanism.[120] The United States was unwilling to subsume domestic considerations to the responsibility of maintaining a nominal anchor. At the same time, other Group of Seven countries became increasingly unwilling to follow the dictates of the U.S.–imposed world inflation rate.

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[117] See Garber (1993).

[118] See McKinnon (1992) for his version of the rules of the Bretton Woods Articles and the dollar standard. Also see Giovannini (1993) and Obstfeld (1993).

[119] See Mundell (1968).

[120] Giovannini's (1993) calculations show that during the Bretton Woods convertible period credibility bounds on interest rates for the major currencies, in contrast to the classical gold standard, were frequently violated.

The failure of the Bretton Woods rule suggests a number of requirements for a well-designed fixed exchange rate system. These include the following:

- that the countries follow similar domestic economic goals (underlying inflation rates);
- that the rules be transparent; and
- that some central monetary authority enforce them.

The recent EMS system was quite successful for a number of years because it seemed to encompass these three elements. Its recent crisis, however, reflected the emergence of some of the same problems that led to the breakdown of Bretton Woods. I discuss these issues below in the following subsection.

POST BRETTON WOODS: MANAGED FLOATING AND THE EMS

As a reaction to the flaws of the Bretton Woods system, the world turned to generalized floating exchange rates in March 1973. Though the early years of the floating exchange rate were often characterized as a dirty float, whereby monetary authorities extensively intervened to affect both the levels of volatility and exchange rates, by the end of the 1970s it evolved into a system where exchange market intervention occurred primarily to smooth out fluctuations. Again in the 1980s exchange market intervention was used by the Group of Seven countries as part of a strategy of policy coordination.[121] In recent years, floating exchange rates have been assailed from many quarters for excessive volatility in both nominal and real exchange rates, which in turn increase macroeconomic instability and raise the costs of international transactions.

The attack cites the favorable experience of the EMS from 1987 to 1991 in producing exchange rate and price stability as a recommendation for a return to a global system of fixed exchange rates. It is argued that recent attempts at policy coordination can be formalized and extended to a more general managed system based on either close policy coordination (to keep exchange rates within specified target zones) or a renewed gold standard. In this paper I do not consider the merits or drawbacks of policy coordination in general, but I examine the EMS briefly as a monetary regime similar to Bretton Woods.[122] Of interest is whether lessons for the international monetary system can be derived from its experience.

The EMS, like the Bretton Woods system, represents an agreement among countries to set exchange rate parities, to manage intra-European Community exchange rates and to finance exchange market intervention. Like Bretton Woods, it is an adjustable peg system.

The origins of the EMS date back to the Bretton Woods period. The case for stable exchange rates within Europe was made in the context of the European Common Market (EEC). In addition to a strong dislike by Europeans for flexible exchange rates—based on their perception of interwar experience and their belief that exchange rate volatility reduces trade in highly open economies—the key motivation for extensive policy coordination to stabilize exchange rates was the common agricultural policy established in 1959.[123]

Food prices in the community are set in terms of a central unit of account (the ECU) but quoted in local currency. Consequently, any changes in exchange rates lead to changes in local prices. During the Bretton Woods era, a system of subsidies and taxes was worked out to insulate the local economy from policy realignments. This led to an asymmetric adjustment between hard currency countries reluctant to lower their agricultural prices and soft currency countries, which allowed their prices to rise. The result was overproduction of agricultural products and an ever-increasing fraction of the EEC budget allocated to subsidize agriculture.

Early attempts to stabilize intra-European exchange rates during the Bretton Woods era were unsuccessful, as was the Snake in the Tunnel agreement of the 1970s. The EMS, established in 1979, was a formal attempt to overcome earlier obstacles to exchange rate stabilization. It was designed to prevent the defects of the Bretton Woods system, including: the asymmetric adjustment mechanism, with the United States as the center, setting the tune for the rest of

[121] See Bordo and Schwartz (1991).

[122] See Feldstein (1988) and Bordo and Schwartz (1989a).

[123] See Giavazzi and Giovannini (1989).

the world; the problems associated with growing capital mobility; and the dramas of parity realignments. Instead, the EMS designed a set of intervention rules that would produce a symmetric system of adjustment; create a mechanism to finance exchange market interventions; and establish a code of conduct for realigning parities.[124]

Like Bretton Woods, the EMS was based on a set of fixed parities called the exchange rate mechanism (ERM). Each country was to establish a central parity of its currency in terms of ECU, the official unit of account. The ECU consisted of a basket containing a set number of units of each currency. As the value of currencies varied, the weights of each country in the basket would change. A parity grid of all bilateral rates could then be derived from the ratio of members' central rates. Again, like Bretton Woods, each currency was bounded by a set of margins of 2.25 percent on either side of parity, creating a total band of 4.5 percent (for Italy, and later the United Kingdom, when it joined the ERM in 1990, the margin was set at 6 percent on either side of parity). The monetary authorities of both the depreciating and appreciating countries were required to intervene when a currency hit one of the margins. Countries were also allowed, but not required, to undertake intramarginal intervention. The indicator of divergences, which measured each currency's average deviation from the central parity, was devised as a signal for the monetary authorities to take policy actions to strengthen or weaken their currencies. It was supposed to work symmetrically.

Intervention and adjustment was to be financed under a complicated set of arrangements. These arrangements were designed to overcome the weaknesses of the IMF during Bretton Woods. The very short-term financing facility (VSTF) was to provide credibility to the bilateral parties by ensuring unlimited financing for marginal intervention. It provided automatic unlimited lines of credit from the creditor to the debtor members. The short-term monetary support (STMS) was designed to provide short-term finance for temporary balance of payments disequilibration. The medium term financial assistance (MTFA) would provide longer term support.

Unlike Bretton Woods, where members (other than the United States) could effectively decide to unilaterally alter their parities, changes in central parities were to be decided collectively. Finally, like Bretton Woods, members could (and did) impose capital controls. These have recently been phased out.

The evidence on the performance of the EMS indicates that it was successful in the latter half of the 1980s at stabilizing both nominal and real exchange rates within Europe, at producing credible bilateral bands and at reducing divergence between members' inflation rates.[125]

Giavazzi and Pagano (1988), Giavazzi and Giovannini (1989), and Giovannini (1989) make a strong case that the success of the EMS was largely due to the fact that it was an asymmetric system with Germany acting as the center country. The other EMS members adapted their monetary policies to maintain fixed parities with Germany. Also, according to the aforementioned writers, the Bundesbank exhibited a strong credible commitment to low inflation and the other members of the ERM, by tying their currencies to the deutsche mark, used an exchange rate target as a commitment mechanism to successfully reduce their own rates of inflation. Evidence for the asymmetry hypothesis is based on the fact that the Bundesbank intervened only when bilateral exchange rates were breached, whereas the other countries engaged in intra-marginal intervention, and on evidence of asymmetrical behavior of interest rates in Germany and the other EMS countries. In the period preceding several EMS realignments, non-German EMS interest rates changed drastically, whereas no change was observed in their German counterpart. Evidence that the Bundesbank's reputation was responsible for the disinflation of the 1980s is based on an out-of-sample simulation of a VAR to predict the inflation rate. Downward shifts in the predicted values of inflation for a number of countries after the advent of the EMS makes the case. That inflation expectations

[124]See Giavazzi and Giovannini (1989).

[125] At its outset, there was considerable doubt that the EMS would be successful at withstanding the strains of greatly divergent money growth and inflation rates among its members. See Fratianni (1980). See Giavazzi and Giovannini (1989); Fratianni and von Hagen (1990 and 1992); and Meltzer (1990).

were significantly reduced only in France and Italy several years after the advent of the EMS (the argument goes) may reflect slow learning or alternatively that these countries used the EMS to justify following unpopular austerity policies.

Fratianni and von Hagen (1990 and 1992) dispute both the asymmetry and the imported disinflation hypotheses.[126] Evidence based on Granger causality tests from a structural VAR suggests that the German monetary base was not insulated from other EMS base movements nor were non-German EMS monetary bases insulated by the German monetary base from external shocks. In this interpretation, the EMS is a coordinated monetary policy system with all members playing a role.

Finally, Fratianni and von Hagen (1990) provide evidence that the EMS has reduced intra-European exchange rate volatility; however, this reduction has been at the expense of increased volatility of non-EMS currencies. Thus they argue that the EMS is on net balance beneficial to welfare because intra-EMS trade exceeds external trade. They also show that although the advent of the EMS has not reduced inflation uncertainty relative to non-EMS countries, it has reduced the effects of foreign inflation shocks on the members.

Despite its favorable performance since the mid–1980s, the EMS was recently subjected to the same kinds of stress that plagued Bretton Woods. September 1992 and November 1992 marked a series of exchange rate crises in Europe that paralleled the events of 1967 to 1971. Precipitated by concerns that French voters would reject the Maastricht Treaty on European Monetary Union in a referendum on September 20, speculators staged attacks early in the month on the Nordic currencies and then later on the Italian lira, the British pound, the French franc, and other weaker currencies. The crisis led to the disabling of the ERM. Both Italy and the United Kingdom left it while Spain, Portugal and Ireland reimposed or strengthened existing capital controls: in November Sweden floated and Portugal and Spain devalued.

The fundamental causes of the crisis, like the crises that plagued Bretton Woods, lay in large part with the exchange rate system. The EMS, like Bretton Woods, is a pegged exchange rate system that requires member countries to follow similar domestic monetary and fiscal policies and hence have similar inflation rates. This is difficult to do in the face of both differing shocks across countries and differing national priorities. Under Bretton Woods, capital controls and less integrated international capital markets allowed members to follow divergent policies for considerable periods. Under the EMS, the absence of controls (after 1990) and the presence of extremely mobile capital meant that any movement of domestic policies away from those consistent with maintaining parity would quickly precipitate a speculative attack. Also, just as under Bretton Woods, the adjustable peg in the face of such capital mobility became unworkable. Thus the difference between the two regimes when faced with asymmetric shocks or differing national priorities was the speed of reaction by world capital markets.

Though the fundamental cause of the crisis was similar in the two regimes, the source of the problem differed. Under Bretton Woods, the shock that led to its collapse was an acceleration of inflation in the United States, ostensibly to finance the Vietnam War, as well as social policies, and to maintain full employment. Under the EMS, the shock was bond financed German reunification and the Bundesbank's subsequent deflationary policy. In each case, the system broke down because other countries were unwilling to go along with the policies of the center country. The commitments to price stability by both the center country and the other members were not shown as credible. Under Bretton Woods, Germany and other western European countries were reluctant to inflate or revalue and the United States was reluctant to devalue. Under the EMS, the United Kingdom, Italy, Spain, Portugal, Ireland and Sweden were unwilling to deflate and Germany was unwilling to revalue. As under Bretton Woods, although the EMS had the option for a

[126]Also, Collins (1988) and Eichengreen (1992d) present evidence that EMS membership may not have been responsible for reducing the inflation rates of EMS countries. Their cross-country regressions show that EMS membership had little effect on inflation performance. Changing public attitudes toward inflation within each country represent a more important determinant. See Giavazzi and Collins (1992).

general realignment, both improved capital mobility and the Maastricht commitment to a unified currency made it an unrealizable outcome.

Thus the lesson from both the EMS and Bretton Woods is that pegged exchange rate systems do not work for long no matter how well they are designed. Pegged exchange rates, capital mobility and policy autonomy just do not mix. During the heyday of Bretton Woods years ago, the case made for floating exchange rates for major countries still holds. This is not to say that if European countries were completely willing to give up domestic policy autonomy, they could not eventually form a currency union with perfectly fixed exchange rates. In an uncertain world subject to diverse shocks, the costs for individual countries of doing so are apparently extremely high.

CONCLUSION

This paper examines statistical evidence on the performance of alternative monetary regimes over the past century. It also examines some aspects of the history of these regimes. Both statistical and historical evidence may help provide answers to the question why some regimes have been more successful than others. They also have implications for current issues in international monetary reform and the ongoing debate over rules and discretion.

The statistical evidence on performance of alternative monetary regimes in the second section leads to the conclusion that the Bretton Woods convertible regime from 1959 to 1970 was by far the best on virtually all criteria, but that the recent floating regime is not much worse. Indeed, it is clear that the performance of the regimes in the post–World War II era is superior to the performance of regimes in the preceding half century. Finally, though the classical gold standard does relatively poorly in terms of the stability of real variables, it performed best on inflation persistence and financial market integration—evidence for the successful operation of gold as a nominal anchor.

This evidence leads to the following question: Why was Bretton Woods so stable yet so fragile and the classical gold standard so unstable and yet so durable? The answer may be due in part to the shocks the two regimes faced. This, however, seems unlikely because the gold standard was subject to both supply and demand shocks that were a multiple of those facing Bretton Woods. It could also be due to greater flexibility of wages and prices and greater factor mobility before World War I, which meant that adjusting to the greater shocks did not have as serious consequences on real activity and employment as later in the twentieth century. Alternatively, political economy factors—such as a more limited suffrage; less concern over the maintenance of full employment; limited understanding of the link between monetary policy and the level of economic activity; and hence loss of an incentive for monetary authorities to pursue policies that would threaten adherence to convertibility—could be responsible. These hypotheses clearly need more investigation.

It also could be due to regime design and especially the incentive compatibility features of the regime. The classical gold standard may have been so successful because of the credibility of the commitment to the gold standard rule of convertibility and because of its near universal acceptance. In turn, the credibility of the gold standard may stem from the origins of gold as money and the importance of Great Britain, the most important commercial nation of the nineteenth century, in enforcing the rules. England's commitment to convertibility in turn was aided by stabilizing private capital flows.

The classical gold standard for the core countries worked as a contingent rule or a rule with escape clauses. As a consequence, it was flexible enough to withstand major shocks. It also enabled governments to finance major wars flexibly, by allowing them to leave the gold standard and temporarily use seigniorage to finance unusual government expenditures. The rule may have endured because the requisite deflation required to restore convertibility after the emergency had passed may not have had severe effects on real variables. This may have been because wages and prices were highly flexible. Alternatively the deflation accompanying resumption may have had significant real effects but no political constituency strong enough to oppose it existed.

The classical gold standard collapsed under the unprecedented shocks of World War I. It was reinstated as the short-lived gold exchange standard. Its brief life reflected the fatal flaws made famous by the Triffin dilemma. But regardless of the weakness of the gold exchange standard, it suffered from the absence of an effective commitment mechanism. There was no cen-

ter country to enforce the rule, just three rivals pulling in different directions. Also, it was the beginning of an era when countries were less willing to go along with the gold convertibility rule because they attached greater weight to the objective of domestic economic stability.

The Bretton Woods system was set up to prevent the perceived flaws of the classical gold standard and the trauma of the interwar period. The Bretton Woods adjustable peg was in some respects similar to the gold standard contingent rule, but it invited speculative attacks, hence compromising its role as an escape clause. Bretton Woods evolved into a gold exchange standard fraught with the adjustment, liquidity and confidence problems of the interwar period. Though the problems of the gold exchange standard could possibly have been corrected by raising the price of gold, as it turned out, it evolved into an asymmetric dollar standard. The United States maintained the credible commitment to a noninflationary policy for only a few years. By the mid-1960s it shifted to an inflationary policy to further its domestic interests. The rest of the world, faced with imported inflation, soon lost the incentive to follow U.S. leadership and the system collapsed in 1971.

The advent of the general floating exchange rate system in 1973 and its longevity suggests that the lessons of Bretton Woods have been learned well. Countries are not willing to subject their domestic policy autonomy to that of another country of whose commitment they cannot be sure in a stochastic world nor to a supernational monetary authority they cannot control. The key advantage of floating exchange rates stressed a generation ago by Milton Friedman and Harry Johnson—the freedom to pursue an independent monetary policy—still holds today. Major countries can design domestic monetary policy rules to achieve domestic price stability without the costs of giving up their policy autonomy.

The experience of the EMS reveals that countries that have similar goals and face similar shocks can establish a regional exchange rate regime. This regime requires both a credible commitment mechanism and the willingness of member countries to give up sovereignty for a higher purpose. As attested to by the events of September and November 1992, however, the durability of such an arrangement seems doubtful, as was the case for Bretton Woods, in an uncertain world subject to diverse shocks where national priorities can change and commitments can be broken. Some have argued that the EMS can be preserved only by precommitment to price stability and fixed exchange rates by independent central banks.[127] Others argue that the only solution is rapid movement to a unified currency enforced by a European central bank.[128] As Feldstein (1992) points out, however, full-fledged monetary union completely precludes the use of domestic monetary policy. To the extent that country-specific shocks dominate common shocks and labor is relatively immobile between European countries, the benefits of permanently fixed exchange rates may not outweigh the cost of increased economic dislocation.[129]

Finally, proposals for monetary reform, such as exchange rate target zones or targeting the real price of gold, though possibly of scientific merit, would work only if nations are willing to give up domestic autonomy and follow credible commitments. The history of international monetary regimes casts doubt on the likelihood that the nations of the world will do so in the forseeable future.

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[127]See Neumann (1991).

[128]See Delors (1989).

[129]See Eichengreen (1992d).

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Appendix Table 1

Supply (Permanent) and Demand (Temporary) Shocks: 1880-1989
Annual Data: Standard Deviations of Shocks (percent); Dispersion of shocks across countries (percent)

	Gold Standard 1883-1913		World War I 1914-1918		Interwar 1919-1939		World War II 1940-1945		Bretton Woods (Total) 1946-1970	
	Demand	Supply	Demand	Supply	Demand	Supply	Demand	Supply	Demand	Supply
United States	2.36	4.77	4.27	9.36	6.73	6.31	2.22	7.10	3.69	5.72
United Kingdom	3.09	3.09	4.34	4.32	6.18	5.23	5.59	7.15	2.42	2.47
Canada	1.01	2.80	6.47	6.45	6.28	8.01	2.31	6.68	2.67	2.73
Italy	4.79	5.00	10.60	8.50	11.56	6.46	18.29	4.92	8.15	5.86
G4	2.23	3.53	4.53	6.82	6.73	4.67	2.49	5.91	2.67	4.33
G4*	2.10	3.30	4.30	6.64	6.35	5.12	1.99	6.30	2.76	4.38
Dispersion	2.69	4.30	3.87	7.03	4.15	5.50	5.62	6.96	2.82	3.40

	Bretton Woods (Preconvertible) 1946-1958		Bretton Woods (Convertible) 1959-1970		Floating Exchange 1973-1989		Post WW II 1946-1989	
	Demand	Supply	Demand	Supply	Demand	Supply	Demand	Supply
United States	5.06	7.76	1.15	1.87	1.46	2.47	2.92	4.55
United Kingdom	2.96	3.18	1.71	1.32	3.27	4.81	2.85	3.49
Canada	3.65	3.55	0.97	1.56	2.41	2.71	2.59	2.75
Italy	10.98	7.95	2.71	2.21	3.43	2.63	6.78	5.01
G4	3.66	5.87	0.95	1.50	1.41	2.56	2.25	3.60
G4*	3.79	5.89	0.90	1.47	1.56	2.37	2.34	3.59
Dispersion	3.93	4.60	1.61	2.10	2.07	2.05	2.57	2.79

G4: G4-aggregate data

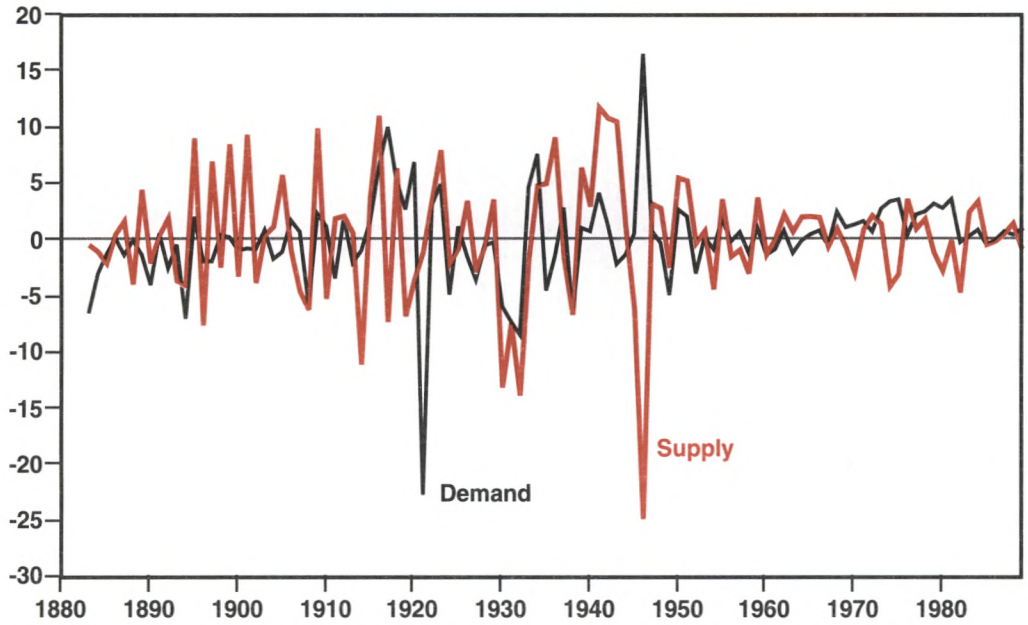
G4*: Weighted average of individual country shocks; the weights are calculated as the share of each country's National Income in the Total Income in the G4 countries, where the GNP/GDP data are converted to dollars using the actual exchange rate.

Dispersion = Σ (weight$_i$(shock$_i$-Σ weight$_i$*shock$_i$)^2)$^{0.5}$ for i = United States, United Kingdom, Canada, Italy.

Appendix Figure 1

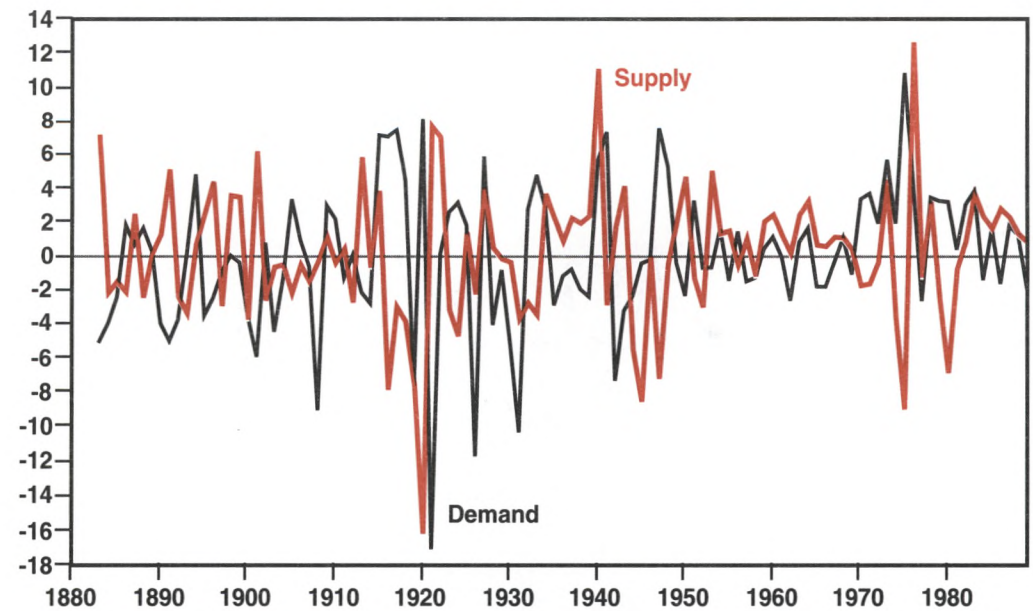
Supply and Demand Shocks: 1880-1989, Including the War Years, Annual Data, United States

Percent



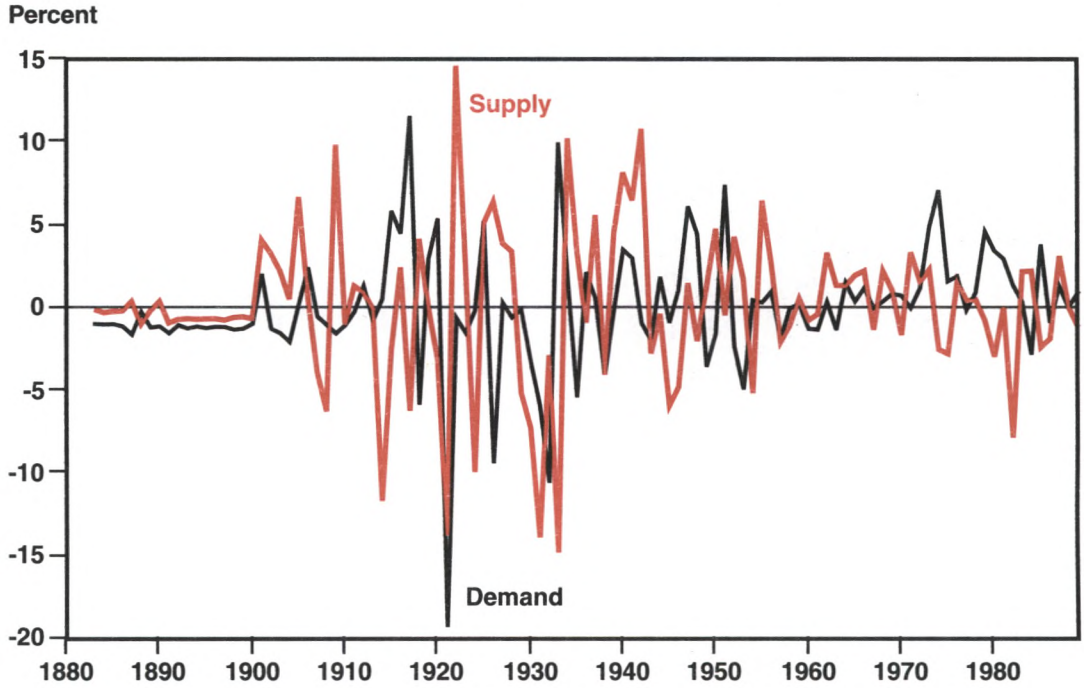
Supply and Demand Shocks: 1880-1989, Including the War Years, Annual Data, United Kingdom

Percent

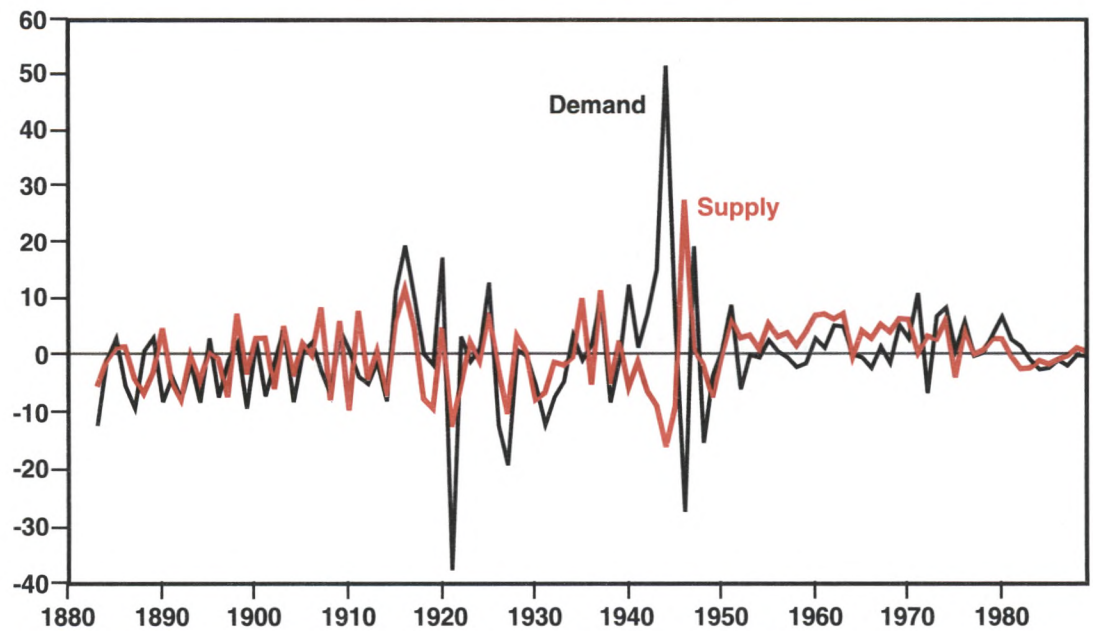


Appendix Figure 1 (continued)

Supply and Demand Shocks: 1880-1989, Including the War Years, Annual Data, Canada

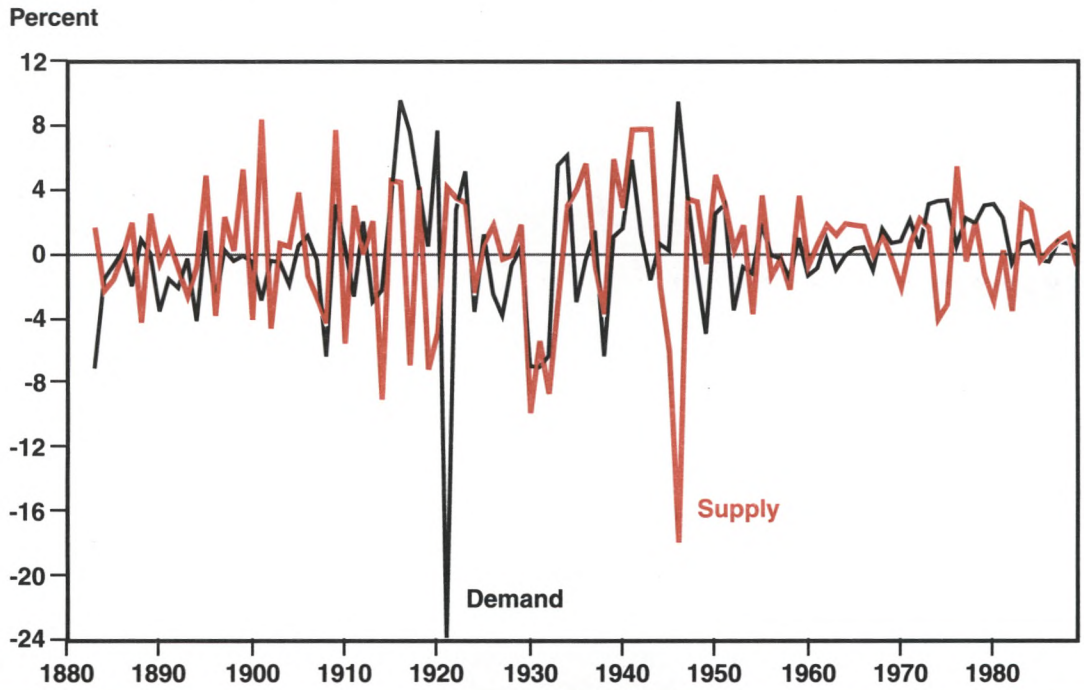


Supply and Demand Shocks: 1880-1989, Including the War Years, Annual Data, Italy



Appendix Figure 1 (continued)

Supply and Demand Shocks: 1880-1989, Including the War Years, Annual Data, G4



Manfred J. M. Neumann

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Commentary

MICHAEL BORDO PROVIDES US with a comprehensive, scholarly study of the history of the three main international monetary regimes: the gold standard, the dollar standard, and the floating exchange rate. He focuses on two important questions. First, which regime provided the best performance with regard to the levels of inflation and real growth? Second, what makes an international monetary regime viable?

Because I am not a historian, I will limit my comments to two areas. I will first discuss the comparative evidence on the performance of the three monetary regimes and use Bordo's statistics to infer a little more information on the role of demand shocks under the different regimes. Thereafter I will concentrate on the important issue of determining a monetary system's credibility. I find Bordo's thoughtful discussion of the issue useful. I should add, however, that sometimes he takes the literature too seriously—especially the affirmative literature on the European Monetary System (EMS). Nevertheless, Bordo forces us to consider which monetary system or standard, if any, can solve the credibility problem in terms of firmly anchoring market expectations about its viability.

WHICH REGIME PERFORMED BEST?

It is natural to evaluate the welfare implications of monetary regimes by asking what different regimes achieve with respect to the level and stability of inflation and real growth. Any monetary regime can be described as a mechanism or device that delivers an average rate of monetary expansion and a variance of money growth. With respect to economic performance, the essential difference is whether a particular regime provides governments with more or less freedom to manipulate the average rate of and the variance of monetary expansion. It follows that regime differences should be reflected in inflation levels and variances of inflation and per capita growth.

Table 1 draws from Bordo's tables 1 and 4. I consider the Group of Seven countries as a whole, the United States, Germany and France and concentrate on the three major periods: the pre-World War I gold standard, the Bretton Woods system of the 1950s and 1960s, and the floating exchange rate in place since the mid-1970s.[1] Note that, in contrast to Bordo, I do not separate out the favorable performance of the Bretton Woods convertible subperiod

[1]The Group of Seven countries are Canada, France, Germany, Italy, Japan, the United Kingdom and the United States.

Table 1

Inflation, Real Growth and Shocks in Different Monetary Regimes for the Group of Seven Countries, the United States, Germany and France[1]

	Gold Standard 1881-1913		Bretton Woods 1946-1970		Floating 1974-1989	
	Mean	Var	Mean	Var	Mean	Var
Inflation						
Mean Group of Seven	1.0	11.56	3.6	21.16	7.1	10.24
United States	0.3	9.61	2.4	6.76	5.6	5.76
Germany	0.6	6.76	2.7	16.00	3.3	1.69
France	−0.0	24.01	5.6	16.81	8.8	10.24
Per capita growth						
Mean Group of Seven	1.5	13.69	4.2	7.29	2.1	5.29
United States	1.8	26.01	2.0	7.84	2.1	7.29
Germany	1.7	8.41	5.0	10.89	2.1	3.61
France	1.5	21.16	3.9	4.41	1.7	2.25
Demand shocks						
Mean Group of Seven		8.64		7.91		6.26
United States		4.12		5.43		2.96
Germany		5.62		8.29		2.76
France		20.98		12.25		3.72
Supply shocks						
Mean Group of Seven		9.27		4.47		5.01
United States		14.52		2.37		3.76
Germany		5.38		7.02		1.93
France		14.06		3.06		2.31

[1]These data come from tables 1 and 4 in the Bordo article in this *Review*. The 1.69 estimated variance of inflation for Germany in the floating exchange rate period is based on a standard deviation of 1.3. This differs from the 1.2 standard deviation in Bordo's table 1 because of differences in rounding of the standard deviation.

The Group of Seven countries are Canada, France, Germany, Italy, Japan, the United Kingdom and the United States.

(1959-1970) in terms of inflation and output. The subperiod looked good on the surface; however, it was in fact the period when the breakdown of Bretton Woods was programmed. More generally speaking, for any regime we might find *ex post* a good looking subperiod.[2]

As Bordo and others have pointed out, the data permit the following observations:

- First, average inflation was negligible under the gold standard and highest under the floating exchange rate.
- Second, the variability of inflation, as well as that of real growth, was higher under the gold standard than under the floating exchange rate.
- Third, the Bretton Woods regime exhibited the highest variability of inflation, whereas output variability was closer to its level under the float than under the gold standard.

The first observation on average inflation performance is well known and understood. It is widely accepted that the classical gold standard prevented the manipulation of monetary expansion by enforcing a direct link between the base money stock, the national stock of gold and the balance of payments. Though devaluation was possible by raising the gold parity in national currency, it was rare. Thus the gold standard delivered the lowest average rate of inflation, given that the available gold stock did not grow much.

[2]As Anna Schwartz pointed out in the discussion, an evaluation of the EMS that bypasses the most recent period, when the EMS came close to collapse, would be seriously misleading.

At the other extreme, fiat money cum floating does not put any external constraint on domestic money production. Thus governments are free to use money production to collect inflation tax and to dampen the business cycle. The additional advantage to governments of the floating exchange rate is that the regime spares them the political cost of negotiating devaluation. In sum, the floating exchange rate is the monetary regime most conducive to inflationary policies. Finally, the Bretton Woods system was in between the gold standard and the floating exchange rate in that it started as a gold exchange standard but was permitted to degenerate into a pure fiat money standard (the dollar standard) during the early 1960s when the United States gold stock fell short of the value of outstanding dollar liabilities.

More interesting than the average inflation performance is the observed difference in the volatility of inflation and output growth among regimes. But to what extent can this volatility be attributed to the operation of the different monetary systems?

EXPLORING THE ROLE OF DEMAND SHOCKS

Apart from determining the level of inflation, monetary regimes differ with respect to nominal demand shock variability. I propose the following conjectures.

First, nominal demand shock variability is highest under the floating exchange rate and lowest under the gold standard. This reflects the differences in the limits to monetary discretion. Because the degree of monetary discretion is close to zero under the gold standard and unlimited under the floating exchange rate, we should observe that the variance of inflation was caused predominantly by nominal demand shocks under the floating exchange rate but by supply shocks under the gold standard.

Second, in a fixed-exchange rate system the system leader sets the floor for nominal demand shock variability. Consequently, for the Bretton Woods period we should observe that nominal demand shock variability was lowest in the United States. Similarly, during the floating rate period nominal demand shock variability should have been lower in Germany than in any other member country of the European snake or EMS.

Checking the empirical validity of these conjectures requires estimating the variance of nominal demand shocks. Bordo's study provides us with some valuable information in this respect. Following Blanchard and Quah (1989) and Bayoumi and Eichengreen (1992) in particular, he has estimated for each country and each monetary regime a bivariate vector autoregression (VAR) for the rates of change of the price level and output. The lower panel of table 1 provides the variances of the estimated aggregate supply and demand shocks.[3] Under the straightforward assumption that the distribution of real demand shocks was the same over the different monetary regimes, differences in demand shock variability can be attributed to the operational differences of the regimes.

The empirical findings are mixed. The data reject our first conjecture. For the Group of Seven countries demand shock variability was highest under the gold standard and lowest under the potentially permissive floating exchange rate regime. The most puzzling aspect is the high demand variability during the gold standard period because not only was monetary policy discretion constrained by the rules of the regime, but also fiscal discretion was negligible, at least by today's standards.

Our second conjecture, in contrast, is confirmed. Demand shock variability was lowest in the United States during the Bretton Woods period and in Germany during the float. Moreover, it can be shown for Germany using an F-test that the demand shock variance of the float differed significantly from its value under Bretton Woods at the 1 percent level of significance. In the United States the level of significance was 10 percent.

Though we have not seen any test statistics of Bordo's VAR estimates, let us assume that the estimates are clean. On this assumption we may use them to investigate the contribution of the aggregate demand shocks to the variability of inflation and output growth under the different monetary regimes. To do so requires a model of aggregate supply and demand to determine the unknown price elasticities of aggregate demand and supply.

Table 2 provides the bare bones of such a model. The model is written in logs and has a

[3]See Bordo's table 4.

Table 2
A Minimal Structure

(1) $y = \theta + \alpha (p - E_{-1} p)$ — Output supply

(2) $y = \beta (m - p)$ — Output demand

(3) $\theta = \theta_{-1} + s$ — Productivity

(4) $m = m_{-1} + d$ — Money stock

Solutions

(5) $$\pi = p - p_{-1} = d_{-1} - \frac{1}{\beta} s_{-1} + \frac{1}{\alpha + \beta} [\beta (d - d_{-1}) - (s - s_{-1})]$$

(6) $$\Delta y = y - y_{-1} = s + \frac{\alpha}{\alpha + \beta} [\beta (d - d_{-1}) - (s - s_{-1})]$$

Variances

(7) $$\sigma_{\pi}^2 = \frac{\alpha^2 + \beta^2}{(\alpha + \beta)^2} \left[\sigma_d^2 + \frac{\sigma_s^2}{\beta^2}\right]$$

(8) $$\sigma_{\Delta y}^2 = \frac{\alpha^2 + \beta^2}{(\alpha + \beta)^2} \sigma_s^2 + \frac{2 (\alpha \beta)^2}{(\alpha + \beta)^2} \sigma_d^2$$

Table 3
The Contribution of Demand Shocks to the Variability of Inflation and Real Growth[1]

	Gold Standard Variance			Bretton Woods Variance			Floating Exchange Rate Variance		
	Actual	Adjusted	Adjusted percentage of actual	Actual	Adjusted	Adjusted percentage of actual	Actual	Adjusted	Adjusted percentage of actual
Inflation									
Mean Group of Four	10.6	4.8	(45)	12.1	5.2	(43)	6.1	2.5	(41)
United States	9.6	3.5	(36)	6.8	4.2	(62)	5.8	2.4	(41)
Germany	6.8	3.3	(49)	16.0	6.0	(38)	1.7	1.5	(86)
France	24.0	12.0	(50)	16.8	7.4	(44)	10.2	2.4	(24)
Real growth									
Mean Group of Four	15.9	8.8	(55)	7.5	4.4	(59)	4.7	2.8	(60)
United States	26.0	13.8	(53)	7.8	6.0	(77)	7.3	4.2	(58)
Germany	8.4	5.2	(59)	10.9	5.8	(53)	3.6	2.6	(72)
France	21.2	13.1	(62)	4.4	2.6	(59)	2.3	0.8	(35)

[1]The Group of Four countries are Canada, France, Germany and the United States. The adjusted variance excludes the contribution of supply shocks.

Lucas-type supply equation and an aggregate demand equation. The evolution of prices and output is driven by productivity and the money supply (both modeled as random walks) with shocks d and s assumed to be independently distributed. The model's solutions [equations (5) and (6)] show that it meets the restrictions used in Bordo's VAR estimates. Supply shocks have permanent effects on the price level and output, whereas demand shocks have no permanent effect on output.

Given the variances of inflation, real growth, demand and supply shocks, equations (7) and (8) can be used to compute the slope coefficients in a p – y plane of aggregate demand, $-1/\beta$, and aggregate supply, $1/\alpha$. Solving by numerical iteration does not yield real solutions in all

cases—notably the Bretton Woods convertible subperiod.[4] Given the estimates of the slope coefficients, we can compute the contribution of the variance of aggregate demand shocks to the observed variances of inflation and real growth in table 3.

Table 3 presents for each monetary regime the measured variances of inflation and real growth, as well as adjusted variances, which exclude the contribution to volatility of the aggregate supply shocks. The numbers printed in parentheses indicate the percentage share in the measured variance of the contribution of the demand shock variance. Note that data from only four of the Group of Seven countries are included; data from Italy, Japan and the United Kingdom had to be removed because it was impossible to compute the slope coefficients for these countries (in at least one subperiod).

Table 3 can be summarized as follows:

- First, for the United States, the leader of the Bretton Woods regime, we find that the variances of inflation and real growth were dominated by the volatility of demand shocks during this period. About 62 percent of the variance of inflation and 77 percent of the variance of output growth can be attributed to the variance of demand shocks. Similarly, we find that for Germany under the floating regime the inflation and the output variance were dominated by demand shocks, which accounted for 86 percent of the inflation variance and 72 percent of the output variance.
- Second, for the four Group of Seven countries we find that demand shocks produced a higher inflation variance under Bretton Woods (5.2) than under the gold standard (4.8) or the floating exchange rate (2.5). The result probably reflects the differential performance of the two leading countries.
- Third, for the four Group of Seven countries as a whole, the variance of demand shocks did not dominate the inflation variance under any of the three monetary regimes. Its contribution never exceeded 45 percent. Thus we find over all regimes that the inflation variance was dominated by the volatility of aggregate supply shocks. This is a little surprising. Are we prepared to accept that systematic differences in the level of demand shock variability are not a characteristic feature of international monetary systems?

We cannot, however, rule out that these findings are statistical artifacts enforced by an inability to separate demand from supply innovations accurately in the VAR estimation. Bordo himself has noted that in some cases the overidentifying restriction (according to which positive supply shocks should permanently raise output and drive down the price level) is not satisfied.

Another indication of a possibly insufficient identification is the estimated change in the slopes of aggregate supply and demand between regimes. Figure 1 presents the average slopes of aggregate supply and demand for the four Group of Seven countries. What effect do we expect monetary regimes to have on these slopes?

Consider the model printed in table 4 which provides more structure than the model in table 2. Because the model is linear in logs, the size of the alpha and beta coefficients depends on the agents' price responsiveness, as well as on the share in output of the respective input in the production function or of the respective expenditure.

Comparing the regimes of the gold standard and Bretton Woods periods, we find that both aggregate supply and demand schedules were steeper under Bretton Woods. I would have expected the opposite on the argument that the economies were generally more open to international trade under Bretton Woods than before.

Comparing the Bretton Woods regime with the float, we observe that the aggregate supply schedule became steeper under the floating exchange rate but the aggregate demand schedule became more flat. The first observation is in line with the model in table 4 because the positive dependence of the nominal exchange rate (its log is denoted by e) on the domestic price level implies a steeper aggregate supply schedule. For the same reason, the demand schedule should also be steeper under floating. However, the data

[4]Of the up to four real solutions for each case, I chose the one which combines a negative slope of aggregate demand with a positive slope of aggregate supply.

Figure 1
G-4

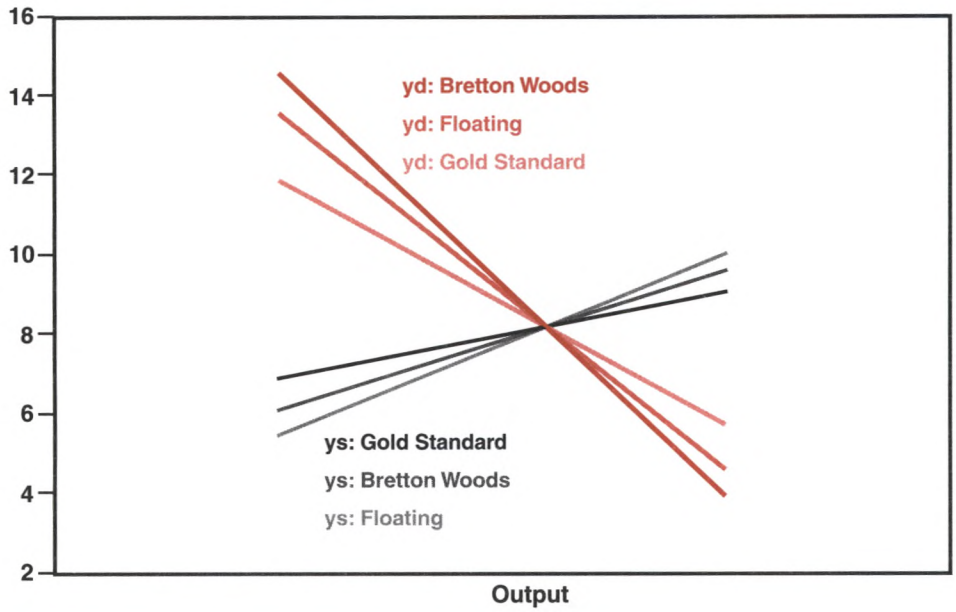

Figure 2
United States

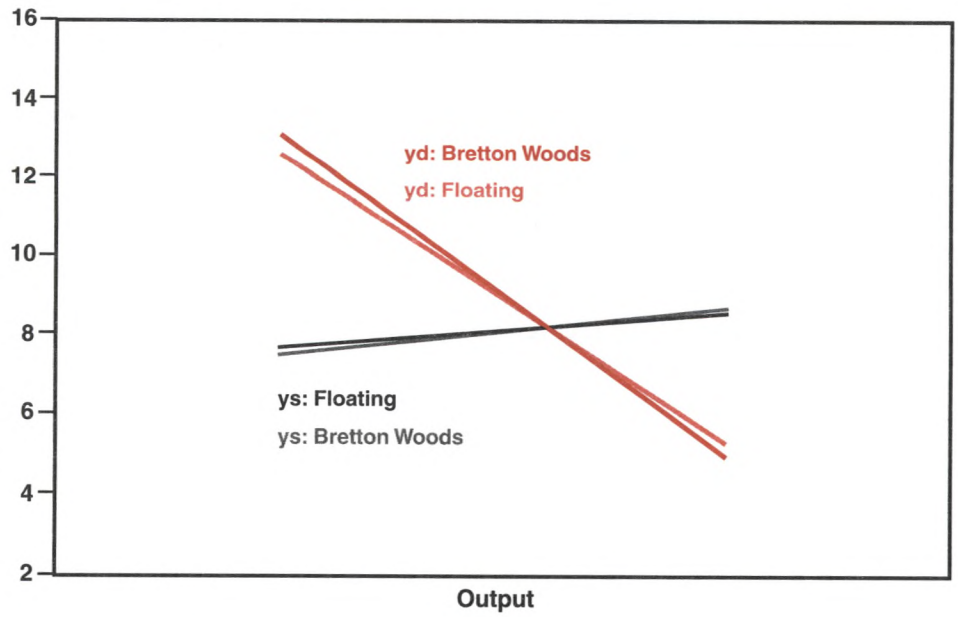

Table 4
Slopes and Exchange Rate Regimes

Model

(1) $y^d = \beta_0 g - \beta_1[i - (E_p{}^c{}_{+1} - p^c)] + \beta_2(p^* + e - p);\ \beta_1 < \beta_2$

(2) $y^s = \theta + \alpha_1(p - E_{-1}p) - \alpha_2(p^{R^*} + e - p)$

(3) $p^c = \gamma p + (1-\gamma)(p^* + e)$

(4) $m = p + y - \lambda i$

(5) $i = i^* + Ee_{+1} - e$

Slopes

Fixed rates: $e = 0$

$$y^d : \frac{-1}{\beta_1\gamma + \beta_2}$$

$$y^s : \frac{1}{\alpha_1 + \alpha_2}$$

Flexible exchange rates

$$y^d : \frac{-1}{\left(\beta_1\gamma + \beta_2\right)\left(1 - \frac{\partial e}{\partial p}\right) + \beta_1 \frac{\partial e}{\partial p}}$$

$$y^s : \frac{1}{\alpha_1 + \alpha_2\left(1 - \frac{\partial e}{\partial p}\right)}$$

do not comply. Also note that the United States data imply that both schedules are more flat under the float. See figure 2.

In sum, I agree with Bordo that his VAR estimates should be viewed with great caution. Moreover, because we are after the differential effect of monetary regimes, a serious drawback is that we cannot differentiate between nominal demand shocks, which we wish to study, and real demand shocks, which are irrelevant because they are not caused by the monetary regime.

Also, I must emphasize that we are studying international regimes, which implies that we cannot treat countries as independent entities. Monetary regime shocks are transmitted internationally. For example, a nominal demand shock produced by the Fed will show up in Germany as a demand shock that raises German output temporarily and German prices permanently. At the same time, however, the shock will show up in Germany as a supply shock, changing the relative price of imported raw materials. This reduces German output permanently and raises German prices permanently. Consequently, the identifying restrictions of Bordo's VAR estimates will classify the nominal demand shock from the United States as a supply shock in Germany.

In conclusion, I believe we have to make another, more sophisticated attempt at investigating the conjecture that international monetary regimes systematically differ with respect to the variability they impose on world economies.

THE CREDIBILITY PROBLEM

I now take up the fundamental question of which international monetary system, if any, can solve the credibility problem in the sense of firmly anchoring market expectations about the viability of the system?

Bordo's careful examination of the history of monetary regimes leads him to conclude that an international monetary system will be stable if its rules are credible. The rules will be credible if the member countries of the system are ready

to honor the rules. And member countries will honor the rules if there is a center country that enforces the rules. Accordingly, the classical gold standard did not break down because the United Kingdom, its center country, was committed to convertibility. In contrast, the United States, as the center country of Bretton Woods, was not committed to convertibility and maintenance of price stability. Bretton Woods consequently broke down. Finally, Germany's commitment to price stability made the EMS a successful and viable system. Unfortunately, the latter prediction held only until last September.

Though Bordo's reasoning makes a lot of sense, it fails to address two essential questions. First, by which means or under what conditions will the center country be able to enforce the rules? Second, and more fundamentally, what conditions are required to make the center country keep its commitment?

In my view, any international monetary system that is based on commitment to rules will be fragile. Commitment should be replaced by precommitment. The game theory reformulation of the pathbreaking analysis by Kydland and Prescott proves that governments cannot commit to price stability.[5] In contrast to commitment, precommitment does not depend on a government's good will or interest. Instead it is created by setting up an external mechanism that effectively ties the hands of current and future governments.

An international monetary regime will be stable and therefore durable if it provides the institutional constraints for a subgame-perfect supergame. The fundamental constraint is effective precommitment by all member governments. There are two types of precommitment: precommitment to price stability at home and precommitment to a fixed exchange rate vis-à-vis another currency. Consequently, we can design two alternative regimes.

A first regime resembles the EMS but commitment is replaced by precommitment. The center country precommits on price stability at home by providing its central bank with the constitutional status of independence from government. Elsewhere I have laid out a sufficient set of institutional elements that provides an incentive-compatible status of independence.[6] The other countries precommit on a fixed exchange rate vis-à-vis the center currency by writing the fixed exchange rate into the country's constitution as Sweden did during the gold standard.

The alternative international regime is created by an agreement that all governments precommit to price stability at home by providing their central banks with constitutional independence.

Which of the two regimes is preferred? The first regime provides price stability for all countries in the medium to long run. The precommitment to fixed exchange rates by n-1 members implies that idiosyncratic shocks will be distributed over member countries at full force, as was the case under the classical gold standard. Because fiscal policy is an important source of idiosyncratic shocks, the regime will hardly be attractive without a (enforceable) rule prohibiting public deficits.

The alternative regime of uniform precommitment to price stability at home might be rejected by some as a nonsystem. But semantics apart, the setup is not to be equated with unconstrained floating. The regime will provide nominal exchange rate stability though not fixity. Depending on the judgment of central bankers, the regime might evolve into an adjustable peg system where up to n-1 central banks unilaterally peg their currencies to the currency of a center country in a flexible manner. This means that in the advent of a sizable country-specific shock at home or in the center country, they will permit exchange rate adjusting. In contrast to the non-precommitted governments in Europe, the independent central bankers will have no interest in defending misaligned exchange rate parities.

In conclusion, the Bretton Woods system and the EMS broke down because both systems were built on unenforceable commitment instead of precommitment.

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[5]See Kydland and Prescott (1977).

[6]See Neumann (1991).

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