

Commentary on 'Changing Effects of Monetary Policy on Real Economic Activity'

Ralph C. Bryant

Many controversial issues traditionally rear their heads when the focus of attention is the conduct of monetary policy. At past conferences with titles and subjects similar to ours today, participants have vigorously debated the old chestnuts: the pros and cons of different operating regimes (the issues of "instrument choice"); the pros and cons of different types of "intermediate-target strategies," including, of course, the appropriate role, if any, for monetary-aggregate targets in the conduct of policy; the appropriate amount of "activism" in varying the instruments of policy (all the various dimensions of the rules versus discretion debate about the conduct of policy); issues about the information that central banks should (or should not) publicly announce about their policies which, in turn, leads to consideration of the public's expectations about the conduct of policy; interactions between monetary policy decisions and fiscal policy decisions; and, not least important, the constraints and opportunities facing an individual nation's monetary authority because of world economic interdependence, and how the individual nation's authority should cope with them.

The important topic about the conduct of monetary policy that has typically been ignored is the state of empirically **useful** knowledge about how the macroeconomy actually functions, and, in particular, how monetary policy actions are transmitted to the real economy. Too seldom have conference participants focused on the accuracy and reliability of the empirical "models" of the economy available to policymakers. **Nor** has it been popular to **examine** whether such

models have been adequately adapted to institutional and structural innovations in the economy.

Happily, this paper by Ben **Friedman** directly tackles the important empirical topic that usually gets short shrift. It is a pleasure to join **Friedman** in directing attention to these issues.

The paper is thoughtful and its judgments are balanced, as is **typical** of Ben Friedman's writing. I do have some questions, and reservations, about particular details. And I tend to be a bit more agnostic about the status of our empirical knowledge than **Friedman** appears to be in this paper. Nonetheless, Ben proposes generalizations that, on the whole, seem to me plausible. I have had to work fairly hard to do the traditional job of a discussant, namely, to find things to criticize and dispute.

Initial parts of the analysis

The first section of the paper identifies three economic developments of recent years that have presumptively altered the structure of the **U.S.** economy (or, in any event, the way economists tend to model that structure). The overview presented is informative, and there are only a few nuances where I am even tempted to disagree. I, therefore, pass immediately to the section of the paper that discusses "Evidence from Reduced-Form Relationships."

Friedman believes that recent institutional and regulatory changes in the economy's structure have called into doubt, even more than before, the usefulness for monetary policy of aggregate-level relationships based merely on reduced-form equations or simple intermediate-target relationships. I share this view about the **diminished** reliability of such relationships as guides for estimating the impacts of monetary policy. And such relationships were never robust in any case.

On many earlier occasions of this type, both **Friedman** and I have stressed that monetary policy cannot be safely based on simple reduced-form relationships, or on simple intermediate-target relationships.' Perhaps there are only a few individuals at this conference

¹ Friedman's many contributions to the debate include Friedman (1975, 1977, 1983, and 1988). For my views, see Bryan (1980, 1983).

who will want to take exception to Friedman's conclusions on this point.

I can imagine that someone who is persuaded otherwise will not find the sparse additional evidence in Friedman's paper fully persuasive. But I scarcely want to take up the cudgels in disagreement with **Friedman** here. In the last year or so, I have even fondly come to hope that views on many of these old controversial issues have been converging to an unexciting but sensible middle ground.

Because I believe the conclusions stressed in the second section are sound, and by now may even be noncontroversial, I will not linger on the old battlefields. Instead, I go directly to the more interesting and meaty part of Friedman's paper.

Changes in the sensitivity to monetary policy of spending components

As a preface to my comments on the third section of the paper, I first need to summarize the analytical procedures that are followed. **Friedman** focuses on the effects of financial variables on four main components of real spending. He thinks of these effects as the "first-round" consequences of monetary policy (but acknowledges this focus as just partial rather than a full general-equilibrium treatment). He chooses econometric equations from the **1985-vintage MPS** model (of the Federal Reserve Board staff) as a representative characterization of the real spending relationships, and then re-estimates those spending equations, sometimes with minor alterations from the original. When re-estimating, he splits his full sample of data, which begins either in the 1950s or 1960s, into two subsamples; and he then observes how the resulting coefficient estimates differ between the two subsamples. **Friedman** also estimates what might be termed "auxiliary" equations in order to be able to simulate the effects of monetary policy actions per se on the right-hand-side financial variables in his spending equations. He does not split the full sample into two subsamples when estimating these auxiliary equations.

Implicit in Friedman's procedures is a traditional "two-step" approach to thinking about the effects of monetary policy. In step 1, the monetary policy action influences financial sector variables. In step 2, the financial variables then influence real-sector spending decisions.

Finally, **Friedman** uses his different coefficient estimates from the two subsample equations for real spending relationships, combined with the simulations of right-hand-side variables obtained from his auxiliary equations, to suggest how the effects of monetary policy may differ between the "before" and "after" subsamples. Hence the charts on which attention is focused in his section III.

Several questions can be raised about these econometric and analytical procedures. These technical problems need to be identified here, because they bear directly on the trustworthiness of the **section-III** conclusions.

In general, **Friedman's** procedures would be appropriate if the split of his full sample corresponded to the timing of the primary changes in the institutional and regulatory structure of the economy, and if the change in coefficients between the subsamples were a reliable indication of how the actual behavioral relationships have changed. But are these conditions met? I worry that they are not, at least not sufficiently.

One possible difficulty arises right away with the choice of subsample periods. In the paper distributed for the conference, Ben does not indicate why he chose to split the full sample of data as he did. **In fact**, he selected different splits for the four components of real spending.²

These differing choices for where to break the full sample are puzzling to me. I do not find the choices self-evidently compelling as likely dates for changes in behavior for the individual spending components; nor do I understand why the varying choices mesh with the overall analytical purpose of the paper. Take the example of business fixed investment. The years 1976, 1977, 1978, and most of 1979 are included in both subsamples. Why is that overlap included for business fixed investment but not the other components of spending? Or consider aggregate consumer spending, for which the split between subsamples is put at the end of 1969. By the MPS model's identification of credit-rationing periods, which **Friedman** accepts for his

² For residential investment, the two subsamples are 1964-Q1 to 1976-Q4 and 1977-Q1 to 1988-Q4. For business fixed investment, the subsamples are 1958-Q2 to 1979-Q3 and 1976-Q1 to 1988-Q4. For aggregate consumer spending the subsamples are 1955-Q4 to 1969-Q4 and 1970-Q2 to 1988-Q4, while the subsamples for nonagricultural exports and non-oil imports are 1968-Q1 to 1979-Q3 and 1980-Q1 to 1987-Q4.

analysis of home building, the later subsample for consumer spending includes one and one-half out of the three episodes of credit rationing actually observed during the whole sample. It is unclear to me why the **subsamples** for expenditures on housing and expenditures on consumption should be defined so differently.

It would seem a cleaner procedure to split the whole sample of data at the same point for all the components of spending. If the resulting estimates for the individual spending equations fail to look stable or **convincing** when that common split is chosen, then that outcome could well be an indication that the equations, themselves, are not satisfactory on other grounds (for any subsamples) and that the procedure of splitting the sample to look at changes in the coefficients is not a robust procedure. At a minimum, it would be helpful for Ben to make explicit the underlying rationale for his choices and for their consistency with his overall analytical objective.

Another possible source of difficulty stems from Friedman's decision not to split the full sample into subsamples for his auxiliary equations. If asked where behavior might most likely have changed in the economy, might we not say that it has changed within the **financial** sector (where financial innovations and other types of institutional and regulatory changes have been so great) much more than in the real sector? There might have even been a case for splitting the full sample for the auxiliary equations and not for the spending equations; but again, at a minimum, the underlying rationale should be spelled **out**.³

Regardless of the sample or subsamples over which they are estimated, I suspect that the auxiliary equations are somewhat shaky. I conjecture, in other words, that these equations are not accurate (semi-reduced-form) representations of the effects of monetary policy actions on endogenous interest rates. In contrast to the **MPS** specifications for the spending equations, such auxiliary **equations have not** received the same amount of careful study and evaluation.

³ At one level of rationalization, I can sympathize with not splitting the sample for auxiliary equations: Friedman wants to focus on changes in the effects of financial variables on real spending alone, holding other things unchanged. But this procedure for the auxiliary equations—in effect, estimating a whole-sample equation that is a mixture of effects before and after the institutional and regulatory changes—could lead to misleading inferences about the spending equations if there have been even bigger changes in the auxiliary equations themselves, which offset or reinforce the effects in the spending equations.

As a further comment on the analytical procedures used in this third section of the paper, a mention of current disputes in econometric methodology seems appropriate. In particular, try to imagine what an econometrician schooled in the style of David **Hendry** (or Edward Leamer?) might say if commenting on these procedures. Such a critic might well take major objection. He would probably observe that we must try to get at “**deeper**” parameters describing the private sector’s macroeconomic behavior in response to financial variables, where such deeper parameters have not changed. Then, he would say, we should try to obtain more direct estimates of the consequences of the institutional and regulatory changes we believe to be important. The essence of this Hendry-style criticism is that conventional procedures for trying to get at the effects of institutional and regulatory changes—such as those used here by Friedman—are often not robust enough to justify the conclusions based on them. Many types of equation misspecification could lead to the nonconstancy of parameters observed across Friedman’s subsamples. Some of those **misspecifications** could be examined through diagnostic tests. In the absence of such tests, one could incorrectly attribute the quantitative changes of the estimated parameters across subsamples to “institutional” or “regulatory” or “**structural**” changes.

I am no econometric theorist, and certainly cannot credibly articulate the nuanced views of a David **Hendry**. Nor do I wish to push this line of thought too far. The equations in the **MPS** model are thoughtful efforts to capture the effects of macroeconomic behavior; and they embody a long history of research. I think Friedman has appropriately chosen, them as a focus of attention. Nonetheless, the **MPS** equations as re-estimated by **Friedman** are not immune to some of the Hendry-style criticisms. The criticisms may be relevant especially because **Friedman’s** estimates might be substantially different for varying definitions of the subsamples.

I turn now to the substance of the conclusions. By the way, there are two other recent studies that have addressed essentially the same empirical issues. **Friedman** does not mention them, but they are relevant here. They are analyses by M.A. **Akhtar** and Ethan Harris (1987) done at the Federal Reserve Bank of New York and by Barry **Bosworth** (1989) in the most recent issue of the *Brookings Papers on Economic Activity*.

Friedman’s conclusions about the changing effects of financial

variables on real spending relationships can be summarized qualitatively in terms of four propositions:

- (1) Home building is less sensitive to restrictive monetary policy today than in former decades, because of the diminution or elimination of credit-rationing effects.
- (2) Business fixed investment has become more sensitive to financial market conditions.
- (3) In contrast, consumer spending may now be less sensitive to interest rate increases **and** stock price declines.
- (4) The key elements of exports and imports, despite having grown relative to aggregate U.S. economic activity, exhibit less sensitivity to exchange rate changes, and hence presumably to monetary policy actions; than in earlier years.

How much can we trust these conclusions? My own tentative judgment is that two of the generalizations, those about home building and business fixed investment, are broadly valid.

For home building, there seems little doubt that credit-rationing effects in the mortgage market and the related non-interest-rate effects of monetary policy on housing spending are less significant now than several decades ago. Friedman, Bosworth, and Akhtar and Harris all agree on this qualitative conclusion, as do a number of other analysts who have commented on the issue.

The reduced sensitivity of home building to monetary policy actions has probably been offset, at least in part, by increases in the interest sensitivity of other private investment expenditures, particularly expenditures on new plant and equipment. Here, too, there seems to be fairly widespread agreement among those that have tried to look at the question empirically. For example, Akhtar and Harris reach a similar qualitative conclusion. (Bosworth is somewhat more agnostic, worrying that the accounting treatment of computer investment and computer prices clouds the interpretation of recent data.)

I am more agnostic and skeptical, however, about Friedman's generalizations for the other components of spending. The conclusion that consumption spending has become less sensitive to interest

rate increases and stock price declines is not clearly shared by the other recent studies. **Akhtar** and Harris believe they found an increase in the sensitivity of consumer durables to interest rates since the mid-1970s. **Bosworth** again takes a fairly agnostic view, finding it difficult to identify a robust correlation between consumption spending and interest rates for any time period.

I personally tend toward the view that, for consumption spending and even for business fixed investment, we simply do not yet have enough useful new data to pin down the consequences of the big institutional and regulatory changes we have experienced in recent years. Those changes probably significantly altered the effects of monetary policy on domestic expenditures. But we have not had a major enough episode of monetary restraint since the time the changes have been fully in force to be confident of that conclusion; 1979-81 was the last such episode, and not all of the changes were fully in force by then.

I am particularly skeptical about Friedman's conclusions for the export and import components of real GNP. Contrary to Ben's finding about the sensitivity of U.S. foreign trade to financial variables, my own view is that the behavioral effects of exchange rate changes on spending are no less powerful than before. Bosworth's research suggested to him that such effects may not have changed much over time. Akhtar and Harris, though not presenting direct evidence, conjectured that such effects may have increased. Research in the International Division at the Federal Reserve Board—by Catherine Mann, Ellen Meade, Peter Hooper and William Helkie—leads to agnostic and **mixed** conclusions, but not to the view that the sensitivity of trade volumes to exchange rate changes has diminished over time.⁴

Some evidence exists that the sensitivity of trade prices, particularly the implicit deflator for **U.S.** imports, to exchange rates may have been unstable in the 1980s.⁵ Such results, however, like those for investment expenditures, may be inordinately and misleadingly influenced by the NIPA treatment of computer prices. Recent work by Meade (1989) and Hooper-Mann (1989a, 1989b) that uses **fixed-**

⁴ See, for example, Helkie and Hooper (1988, 1989), Hooper and Mann (1989a, 1989b), and Meade (1989).

⁵ See, for example, Richard Baldwin (1988) and Hooper and Mann (1989b).

weight import-price deflators and that studies the business equipment (computer) component of trade separately' from other **manufacturing** goods does not seem to show evidence of significant structural change in the 1980s.

Taking into account the variety of recent research on trade-volume and trade-price equations, I thus doubt that the behavioral sensitivity of trade to financial variables has lessened in the 1980s. Given the quantitatively larger importance of the external sector to the U.S. economy, the overall effects of monetary policy working through the external sector have probably become significantly more important than several decades ago. The sensitivity to interest rate changes of the nominal current account balance as a whole, moreover, is rising over time as the United States goes more deeply into an international net debtor situation.

The bottom line from surveying the available evidence for all the components of spending, it seems to me, is that there has probably been little if any net decline in the power of Federal Reserve monetary policy to influence the U.S. real economy. Friedman, himself, does not seem to want to argue that there has been a net decline either. My differences of judgment with **Friedman** pertain to details about compositional effects, not about the larger issue.

Uncertainty about policy effects

If the means of the effects from Federal Reserve policy actions have not changed much, the variances may have changed appreciably. It seems likely that the transmission effects of monetary policy are at least as uncertain as they once were—probably even more uncertain. This enhanced uncertainty does make the conduct of monetary policy more difficult than it used to be. The importance for **policy** making of this uncertainty, and its implications for further research, prompt me to extend my comments beyond the boundaries that **Friedman** has imposed on himself in the paper.

Consider the research issues first. Can we get acceptable answers to what we want to know about the effects of monetary policy by application of "partial-model" techniques such as those used in this paper? Probably not, I would say. The traditional two-step, **partial-equilibrium** procedure, implying a uni-directional causation for **first-round** effects running from financial variables to real spending, may

not be adequate. Instead, we probably need to go to full-model simulations, and careful attempts within the full models to represent how the institutional and regulatory changes have occurred. (Friedman mentions this problem, but gives it less emphasis than I would.)

Nor is it likely to be sufficient to carry out the research in the context of a full model of the U.S. economy alone. In principle, we should use empirical models that analyze the U.S. economy as part of an increasingly integrated global economy. What, in principle, is required is an empirical measure of changes in the autonomy of U.S. monetary policy, measured as a change in the ability of a given dose of Federal Reserve monetary policy to influence U.S. domestic variables relative to foreign variables (Bryant, 1980, chaps. 11-13). Such a measure in principle requires estimates of final-form multipliers from a full model of the world economy.

But how difficult this is! Analysts must reliably be able to identify changes in full-model final-form multipliers over time. But how could analysts conceivably do that without going back to key "structural" coefficients and how they may have changed over time? That task, in turn, requires dealing appropriately with Hendry-style econometric issues of parameter nonconstancy in the context of very large global models.

We should not underplay the significant uncertainties that exist about the effects of monetary policy, in particular once an effort is made to take international repercussions and feedbacks into account. To give a rough indication of this uncertainty, I have included here a chart that shows the full-model effects of a standardized U.S. monetary policy action on U.S. real GNP, as simulated by a variety of different multicountry empirical models. The underlying model simulations come from a series of collaborative research projects on macroeconomic interdependence in the world economy sponsored in recent years by the Brookings Institution. This chart visually illustrates the diversity in simulated results across different models.⁶

The curves in the chart represent deviations of U.S. real GNP from a "baseline" simulation caused by a simulated expansionary action

⁶ The research projects are described, the participating models are identified, and the main empirical conclusions are reviewed in Bryant, Helliwell, and Hooper (1989); the data plotted in the chart are presented in Table A-3 of the unabridged version of that paper. See also the two volumes of Bryant, Henderson, Holtham and others (1988).

by the Federal Reserve. The data for the specific simulations from the individual models are shown with small dots in the background. In addition, the chart shows two averages (which differ little in this particular case) and two intervals, defined by plus and minus one standard deviation, roughly calibrating the variability in the models' responses.⁷

As the widths of the intervals in the chart indicate, there are very sizable differences across the models, both about the magnitude and the timing of the simulated effects. Some of this model diversity may reflect different approaches in trying to capture recent institutional and regulatory changes. But the diversity can also be traced to even more fundamental differences among modeling groups in the specification and estimation of their **models**.⁸

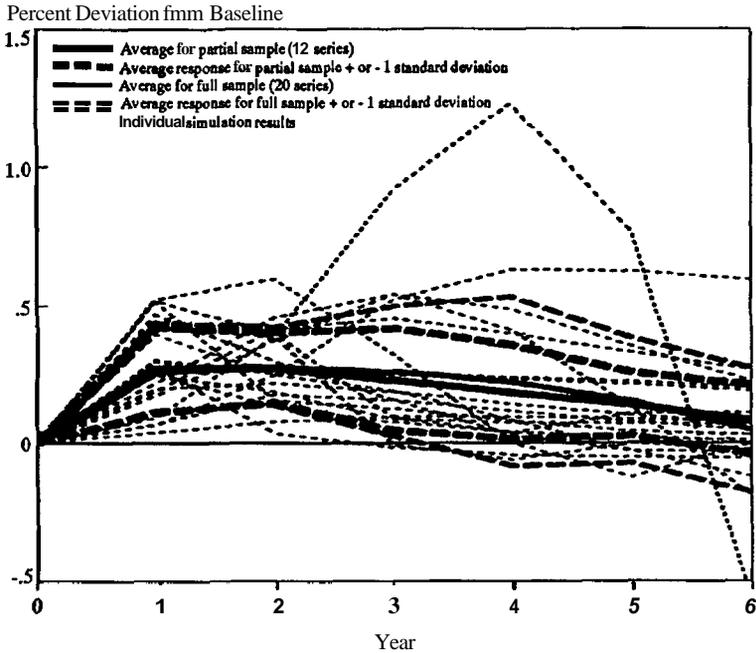
It would not be right, I believe, to infer from the sobering evidence about disagreement among existing models that model uncertainty is very much greater today than in the past. At least with respect to the international dimensions—the macroeconomic interactions among national economies—we are less poorly off with empirical knowledge today than we were several decades ago. Nevertheless, notwithstanding the progress in research achieved during recent years, the economics profession has miles and miles to go before it will

⁷ The baseline (sometimes referred to as "control") simulation is a benchmark set of commonly defined paths for important macroeconomic variables appearing in a model. A policy ("shock") simulation is prepared by changing an exogenous variable by a specified amount from its baseline path and using the model to calculate the alterations in the paths of endogenous variables caused by the policy action. The monetary action illustrated in the chart is defined as the raising of a key **U.S.** monetary aggregate (**M1** or **M2**) above its baseline path by 1 percent throughout the six years of the simulation period. The average curve in the chart shown with a heavy solid line refers to a partial sample of results (from 12 time series of model simulations), while the average with a less prominent solid line pertains to a more complete set of model results (19 time series). As a measure of the variability of the models' responses, the chart also shows with dashed lines the interval defined by plus and minus one standard deviation around the mean. The interval around the 12-series mean is shown with the heavy dashed lines, the 19-series interval less prominently.

⁸ The model simulations included in the chart were generated by models with both rational, **forward-looking** (RFL) and adaptive, backward-looking (ABL) treatments of expectations. Although interesting and in some cases apparently significant, the differences between models with RFL and ABL expectations are **often** less dramatic **than** one might at first expect (especially given the emphasis on this topic in the theoretical literature). Nor do such differences seem to account for the bulk of the variation in results across models. Other types of structural differences among the models seem to dominate the treatment of expectations as the cause of divergent results. For discussion, see Bryant, Henderson, Holtham and others (1988, chap. 3) and Bryant, **Helliwell**, and **Hooper** (1989).

Chart 1

Effects on U.S. Real GNP of U.S. Monetary Expansion



Note: Increase of money stock above baseline by 1 percent, maintained throughout the six years of simulation period.

be possible to place much narrower confidence intervals around the quantitative estimates of the effects of policy actions. This uncomfortable state of affairs still exists for own-country effects in the United States, as is apparent from the chart.⁹ Ranges of uncertainty for

⁹ In the empirical models of the U.S. economy that have not been especially concerned with the international aspects, there remains a very substantial divergence of views about the effects of Federal Reserve monetary policy. See Klein and Burmeister (1976) and Christ (1975) for comparison of U.S.-focused models as of the 1970s. Adams and Klein (1989) report comparisons from recently conducted simulations.

estimates of the cross-border spillover effects (effects of U.S. policies on foreign economies and the effects of foreign policies on the U.S. economy) tend to be even larger than those for own-country effects.

Taking uncertainty into account in policy formulation

I want to conclude with an upbeat observation on how the Federal Reserve seems to be doing in coping with analytical uncertainty about the behavior of the economy and about the transmission of monetary policy to the economy.

Is there major reason to be critical of the Federal Reserve System because somehow it is not sufficiently taking into account the increased uncertainty associated with institutional and regulatory changes, with the increasing openness of U.S. goods markets, and with the increasing cross-border integration of national financial markets? Should the Federal Reserve be proceeding more cautiously, defined in some way or **another**?¹⁰

I cannot, myself, see any grounds for serious criticism. The Federal Reserve System staff continues to re-evaluate existing research and to carry out new research, thereby trying to get as good a fix as possible on changes in the impacts of policy. Both in terms of quality and quantity, that staff research plays a leading role in professional research as a whole.

Moreover, Federal Reserve policy itself appears to give substantial weight to the existing uncertainties. As an illustration, I was struck by the last paragraph of Chairman Greenspan's testimony in this summer's Humphrey-Hawkins hearings. The testimony candidly acknowledged the possibility of a "mistake" due to errors in forecasting the evolution of the economy and the effects on the economy of monetary policy. But it also emphasized that the Federal Reserve will try to steer cautiously between the twin dangers of inflation and recession: "an efficient policy is one that doesn't lose

¹⁰ In this context, the general public (though not the participants in this conference) may need to be reminded that there is not any way that the Federal **Reserve** can somehow set the dials on its instruments merely at "zero," thereby eliminating the effects of policy on the economy. Nor, of course, is there any presumption that some simple **rule** could minimize uncertainty about the effects of monetary policy on the economy.

its bearings, that homes in on price stability over time, but that copes with and makes allowances for any unforeseen weakness in economic activity.’’

That type of cautious discretionary policy, backed up by constant research monitoring of empirical knowledge about the behavior of the U.S. and world economy and about the transmission effects of monetary policy actions, seems to me the best attainable approach the Federal Reserve could pursue. My serious criticisms of U.S. macroeconomic policy have to be directed, not at the Federal Reserve, but at the President and the Congress for their incautious, short-sighted—indeed, outrageous—conduct of U.S. fiscal policy.

References

- Adams, F. Gerard, and Lawrence R. Klein. "Performance of Quarterly Econometric Models of the United States: A New Round of Model Comparisons." University of Pennsylvania, January 1989. (mimeo).
- Akhtar, M.A., and Ethan S. Harris. "Monetary Policy Influence on the Economy—An Empirical Analysis." Federal Reserve Bank of New York *Quarterly Review*, (Winter 1986-87), pp. 19-34.
- Baldwin, Richard E. "Hysteresis in Import Prices: the Beachhead Effect," *American Economic Review*, vol. 78 (September 1988).
- Bosworth, Barry P. "Institutional Change and the Efficacy of Monetary Policy." *Brookings Papers on Economic Activity*, 1989:1.
- Bryant, Ralph C. *Money and Monetary Policy in Interdependent Nations*. Washington: Brookings Institution, 1980.
- _____. *Controlling Money: The Federal Reserve and Its Critics*. Washington: Brookings Institution, 1983.
- _____, John F. Helliwell, and Peter Hooper. "Domestic and Cross-Border Consequences of U.S. Macroeconomic Policies" in Bryant, Currie, and others, eds., *Macroeconomic Policies in an Interdependent World*. Washington: International Monetary Fund, Brookings Institution, Center for Economic Policy Research, September 1989. Unabridged version available as *Brookings Discussion Paper in International Economics* No. 68, Washington: Brookings Institution, January 1989.
- _____, Dale W. Henderson, Gerald Holtham, Peter Hooper, and Steven A. Symansky, eds., *Empirical Macroeconomics for Interdependent Economies*. Washington: Brookings Institution, 1988.
- Christ, Carl F. "Judging the Performance of Econometric Models of the U.S. Economy," *International Economic Review*, vol. 16 (February 1975).
- Friedman, Benjamin M. "Targets, Instruments, and Indicators of Monetary Policy," *Journal of Monetary Economics*, vol. 1 (October 1975).
- _____. "The Inefficiency of Short-Run Monetary Targets for Monetary Policy." *Brookings Papers on Economic Activity* (2), 1977.
- _____. "The Roles of Money and Credit in Macroeconomic Analysis," in James Tobin, ed., *Macroeconomic Prices and Quantities: Essays in Memory of Arthur M. Okun*. Washington: Brookings Institution, 1983.
- _____. "Lessons on Monetary Policy from the 1980s," *Journal of Economic Perspectives*, vol. 2 (Summer 1988).
- Helkie, William L., and Peter Hooper. "An Empirical Analysis of the External Deficit, 1980-86," in Ralph C. Bryant, Gerald Holtham, and Peter Hooper, eds., *External Deficits and the Dollar: The Pit and the Pendulum*. Washington: Brookings Institution, 1988.
- _____. "U.S. External Adjustment: Progress and Prospects." *Brookings Discussion Paper in International Economics* No. 71 (February 1989).
- Hooper, Peter, and Catherine Mann. "The Emergence and Persistence of the U.S. External Imbalance: 1980-87," *Princeton Studies in International Finance*, International Finance Section, Princeton University, 1989a.
- _____. "Exchange Rate Pass-Through in the 1980s: The Case of U.S. Imports of Manufactures." *Brookings Papers in Economic Activity* (1989:1) (1989b).
- Klein, Lawrence R., and Edwin Burmeister, eds. *Econometric Model Performance*. Philadelphia: University of Pennsylvania Press, 1976.
- Meade, Ellen. "Computers and the Trade Deficit: The Case of the Falling Prices." Paper prepared for the conference on International Economic Transactions: Issues in Measurement and Empirical Research. National Bureau of Economic Research and the Conference on Research in Income and Wealth, (November 1989).