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A Summary of the Empirical and Analytical Results and the Implications for International Monetary Policy

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A truly wide array of data, models, and views have been presented in the papers prepared for this conference and in the discussions of the papers. Such a broad scope is entirely appropriate in that the purpose of the conference—the examination of optimal monetary policy for stability in the world economy—requires careful consideration of many different countries, institutions, and events.

But the breadth makes summation difficult. Thanks to the excellent organization of the sessions and the discussions, and the comprehensive statistical analysis prepared by the staff of the Institute for Monetary and Economic Studies, I believe a reasonably coherent and manageable set of results, on which there was considerable agreement, have emerged at the conference. In my view these results deserve careful consideration by policymakers.

I have organized my summary of the results into three main areas. First, I summarize the key *empirical facts of macroeconomic performance* presented in the papers. Second, I summarize some of the *analytical results* developed in the papers and the discussions. Third, I consider the *international monetary policy implications* that might be drawn from the empirical and analytical results.

I. Economic Performance

The conference appropriately began with a presentation of statistical facts about the macroeconomic performance of the major industrial

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countries during the last 30 years. Most of the analysis was focused on the G-7 countries: Canada, France, Germany, Italy, Japan, the United Kingdom, and the United States. There are a number of key characteristics of these data about which there was rather wide agreement at the conference. I will focus on these areas of agreement, but I will also review the areas of disagreement and attempt to resolve some of this disagreement.

A. The Importance of Supply Shocks in the 1970s and 1980s

An important empirical regularity that emerges from the data is that the main source of disturbances in the period since the early 1970s, in comparison with the period of the 1950s and 1960s, has been from the supply side—real, external shocks affecting the macroeconomies. This fact emerges from Michael Parkin's paper, in which he measures such shocks with the ratio of commodity prices to the general price level, and shows their large impact on output fluctuations. The fact also comes out of the Institute for Monetary and Economic Studies' statistical analysis, in which the variability of such external factors as import prices was shown to increase in the 1970s. It is also emphasized in Stanley Fischer's case study approach, in which he focuses on the two supply shocks of the 1970s and early 1980s. Overall, I think that there is wide agreement that a major source of shocks, at least to the major industrial countries during the period of the 1970s and 1980s, has come from the supply side.

Three remarks might be made about this empirical finding. First, the finding that supply shocks have been a key disturbance in the 1970s and 1980s does not imply that monetary policy has played a small role. I think monetary policy and the differences between monetary policy in the different countries has had a big effect on the transmission of these supply shocks within and between the different economies. The importance of supply shocks should not, for example, be viewed as evidence in favor of a real business-cycle theory, in which monetary policy plays no role. On the contrary, the fact that supply shocks have been so large in the 1970s and 1980s is evidence that monetary factors are important. Monetary policy can explain why similar supply shocks have resulted in dissimilar economic behavior in the different countries hit by these shocks. Technically, the explanation can be put in terms of a distinction between impulse and propagation mechanisms. While it is true that the major impulses have been real shocks, the propagation mechanism has been influenced by monetary policy. Monetary

policy has played a role in cushioning the effect of the supply shocks on macroeconomic fluctuations.

Second, there was discussion about whether the productivity slowdowns in the 1970s and 1980s should be included as a significant real supply disturbance affecting economic fluctuations. Jeffrey Sachs, for example, suggested that the productivity slowdown had a big impact on the behavior of the macroeconomies during this period, and may serve as a political-economic explanation for the large deficits and low private saving in the U.S., as people only slowly adjusted their spending to the slowdown in real growth. In the discussion it was argued, however, that the productivity shifts were unlikely to be a source for the large economic fluctuations we have seen, simply because the timing of these shocks has not coincided with the big fluctuations. Stanley Fischer mentioned in particular that the big swings in the 1980s were not related to marked productivity slowdowns.

Third, in the discussion it was mentioned that the real supply shocks may not have been entirely exogenous. For example, the likelihood of oil shocks may increase when the world economy is in a boom. In fact, both oil shocks occurred during boom periods in the world economy.

B. Changes in the Magnitude of Economic Fluctuations

The second main point about which there was substantial consensus was that the size of the economic *fluctuations* in output and inflation have been smaller in many countries—by large amounts in some cases—during the 1970s and 1980s, in comparison with the period of the 1950s and 1960s. The breakpoint for this comparison—the early 1970s—coincides with three big events and therefore interpretation is difficult. The three events are: the change from fixed to flexible exchange rates, the end of the very high growth periods in Japan and Western Europe, and the start of the period when supply shocks were of greater importance.

The most notable reduction in the size of output fluctuations is in Japan. While the Japanese performance is an “outlier,” the decline is also noticeable in West Germany and in France. Under the fixed exchange-rate system, Japan had the largest fluctuations in output among all the countries examined here. Under floating exchange rates Japan had the smallest output fluctuations. Japan moved from the worst, in terms of this measure of performance, to the very best.

This finding emerges from many of the papers presented at the con-

ference. Michael Parkin's paper demonstrates the reduced output variability, as do the statistical time series prepared by the Institute. Allan Meltzer's keynote address emphasized the greater stability in some countries, especially Japan since 1973. Stanley Fischer's study also emphasized the lower output fluctuations in Japan as compared with the other countries, although his study is more of a "cross-country" analysis, looking only at the period since 1973, and not a comparison with the earlier period.

C. Changes in the Variability of the Monetary Instruments

In an effort to make inferences about the cause of the reduction in the variability of economic fluctuations in some countries, several authors examined the variability of the instruments of monetary policy. This examination uncovered the third key observation about which there seemed to be much agreement at the conference: there is evidence that the reduction in macroeconomic variability is associated with the reduction in the variability of the policy instruments—in particular in the reduction in the variability of money growth. Again, the reduction is for the period since 1973 in comparison with the period before 1973. And again, Japan is the best example of this association. The variability of money growth in Japan was lower—much lower—in the post-1973 period than before. The Institute's time series chart on money growth and output growth in Japan shows this most vividly. The association is not so strong in the other countries, but there is no question that the several countries that went from high to low variability of economic fluctuations also tended to go from high to low variability of money growth.

There was some disagreement, however, about whether the evidence showed that *within the same time period*—in particular the 1970s and 1980s—there was an association across countries between the variability of economic fluctuations and the variability of the policy instruments. Stanley Fischer noted in his comparison of four countries that stability of the money supply was not necessarily related to the stability of real output fluctuations. On the other hand, in his paper, Michael Parkin says: "There is a significant relationship across countries between the variability of real output growth and the variability of money growth The only country dummies that are significant are those for Canada and France in the growth rates. None of the country dummies are significant in the innovations." There appears, therefore, to be some disagreement about whether for the period since 1973 there is any evidence that countries that have had more stable monetary

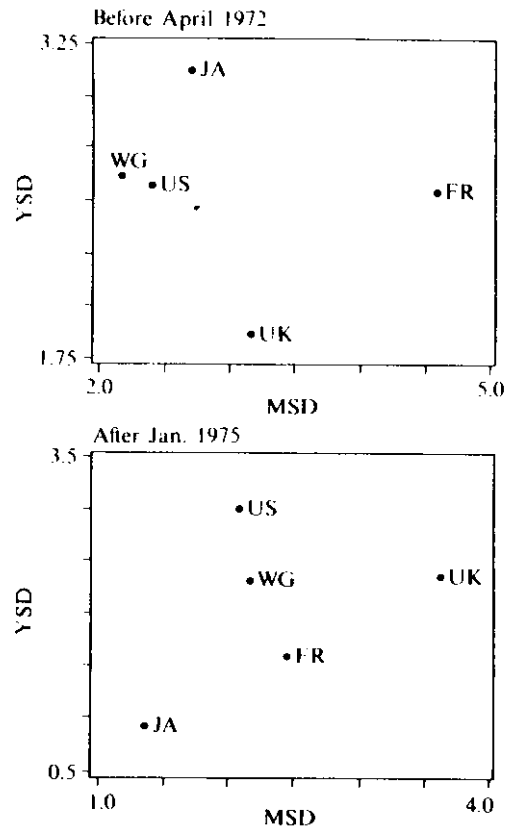


Figure 2.1
Real Output Growth Variability (YSD) and Money Growth Variability (MSD): before April 1972 and after January 1975.
Source: Institute for Monetary and Economic Studies, Bank of Japan. Statistical analysis prepared for this paper.

aggregates have also had more stable economic growth.

In order to examine this question in light of this disagreement, it may be helpful to look at Figures 2.1 and 2.2. Figure 2.1 is based on variability measures prepared by the Institute for Monetary and Economic Studies. Figure 2.2 is based on Michael Parkin's growth-rate variability measure. Both figures are scatter diagrams that illustrate the relation, across countries and for two different periods of time, between the variability of real output growth and the variability of money growth. The two periods are before and after the early 1970s. The top diagram in both Figure 2.1 and Figure 2.2 shows the

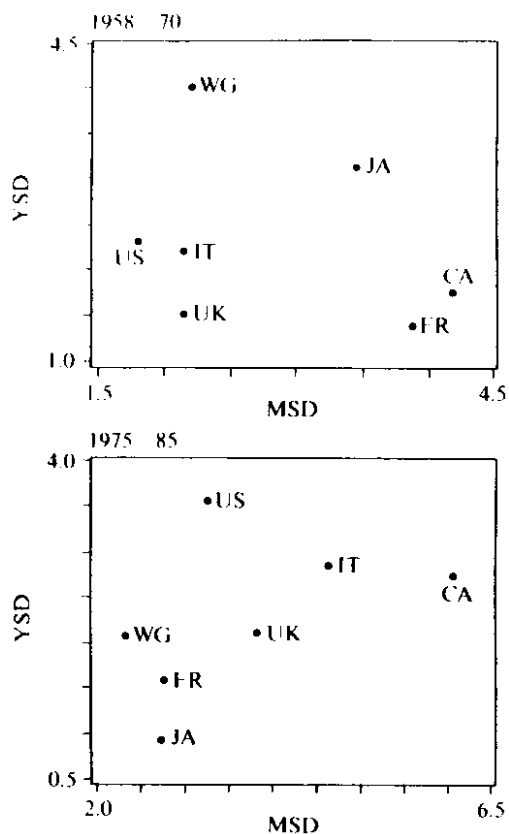


Figure 2.2
Real Output Growth Variability (YSD) and Money Growth Variability (MSD): before 1970 and after 1975.
Source: Michael Parkin's paper in this volume.

variability in the earlier period, while the bottom diagram shows the variability in the later period. Just glancing at these diagrams reveals little systematic relationship between money growth variability and real output variability across the countries during either period. What is evident is the reduction in variability from one period to the next, which I noted above. For example, from the earlier to the later period, the variability both of real output growth and of money growth was reduced in Japan.

Given this visual representation of the data, how then does Michael Parkin come to the conclusion stated above, that "there is a significant

relationship across countries between the variability of real output growth and the variability of money growth?" In order to answer this question, I replicated Parkin's pooled time-series and regression results that lead to this conclusion. The dependent variable in the regression is the variance of real output growth, and the key "independent" variable is the variance of money growth. The other independent variables are dummy variables. The regression is a pooling of data over three periods (1958 through 1970, 1971 through 1975, and 1976 through 1985) and over seven countries. Hence, there are 21 data points. Parkin places six dummy variables in the regression for the seven countries and two time dummies for the three time periods, leaving 11 degrees of freedom.

Parkin's finding of a significant relationship among the countries is evident in the significant coefficient for the variance of money growth that he obtains in this pooled regression. However, while only two of the dummy variables are significant, the dummy variables as a group turn out to play a crucial role in his finding. I believe this role of the dummy variables may reconcile the disagreement I have noted.

To show this, I simply ran Parkin's growth rate regression with the country dummies omitted. Without the dummy variables, the significance of the relationship between money growth variability and output growth variability completely disappears. The coefficient for money variability drops from .41 to .13 and the t-statistic drops to the 1.1 value, indicating that the coefficient is insignificantly different from zero. Moreover, the reduction in the R-square falls from 60% to only 11%, showing that almost all of the cross-country explanatory power of the regression has come from the dummy variables.

My point in looking at the regression without the dummy variables is to reconcile Michael Parkin's data with the results presented by Stanley Fischer. Examined in this way, Parkin's data show no "cross-country" relationship between monetary variability and real output variability. In this sense his data are telling the same story as Stanley Fischer's data. The relationship between money variability and real output variability that Parkin's dummy variable regressions show is simply the same reduction in variability within single countries over time that I have already noted. There is no evidence of a significant association across countries during a given time period.

In my view, the dummy variables in Parkin's regressions are not serving simply to let the intercepts differ in a relationship about which there is independent cross-sectional variation. The variation other than that captured by the dummy variables is created by splitting the sample period into three periods. Put somewhat differently, without

breaking up his data into different time periods, Parkin could not hope to obtain a cross-country association. In one time period, country dummy variables would of course explain everything!

In my view, therefore, the results should not give the impression that simply by using a more stable monetary instrument policymakers can achieve more stable macroeconomic performance. In this sense I would say that the conclusion of Stanley Fischer on this issue is correct.

In any case, there are, of course, problems with drawing inferences from these associations. James Tobin mentioned that the correlation between instrument variability and output variability could be spurious, and Robert Barro pointed out that the deviations of money growth from the stated monetary targets discussed by Stanley Fischer could simply indicate differences in forecasting abilities at the different central banks.

I think the possibility of spurious correlation is greater when the relationship between instrument stability and output stability occurs over two different points in time. There are many different things that could have changed between those two different periods of time. For a cross section of countries at the same period of time, one might at least hope that the shocks and economic environment are similar across countries, and thereby worry less about spurious correlation owing to omitted factors.

D. Relation between Fiscal and Current Account Deficits

The fourth key fact relating to economic performance is the strong relationship between fiscal deficits and current account deficits in the 1980s. Growing current-account deficits in the U.S. have been associated with growing fiscal deficits. Growing current-account surpluses in Japan and West Germany have been associated with shrinking fiscal deficits. These trends in the 1980s come out most clearly in the Institute's statistical calculations and tend to confirm standard theoretical results in most macroeconomic models.

In Jeffrey Sachs's simulation analysis, he examined whether these changes in fiscal deficits could explain the changes in the current account in the U.S. and Japan in the early 1980s. He concluded that the changes in fiscal deficits could explain virtually all of the changes in the current account during this period. However, Sachs's approach was criticized by several conference participants. Edwin Truman criticized the simulation analysis for ignoring everything else but the fiscal deficits. In particular, he suggested that the movements of monetary policy in the early 1980s were a factor that could not be ignored.

Robert Barro criticized the analysis for focusing only on one period, and for not examining how the model would explain the relationship between fiscal deficits and current account deficits in other periods. Both Allan Meltzer and James Tobin commented that the U.S. fiscal deficit was only part of the explanation of the U.S. current deficit.

Hence, from the results presented at this conference one cannot conclude that changes in government fiscal deficits have been the sole cause of the large current account deficits in the U.S. and the current account surplus in Japan and Germany. But fiscal deficits have certainly been a factor, and reducing the fiscal deficit in the U.S. at this time will certainly improve the U.S. current account.

E. Making Inferences from the Empirical Regularities

What inferences do the data permit concerning macroeconomic policy or macroeconomic behavior? Let me consider three questions.

Fixed versus Flexible Exchange Rates?

It seems safe to say that these results demonstrate that the move to flexible exchange rates since the early 1970s did not necessarily worsen macroeconomic performance. In fact, in some countries the macroeconomic performance has improved dramatically. It is just not possible with these data to make the case that flexible exchange rates have made things worse. My reading of the data is that flexible exchange rates have made things better.

Wage-Price Rigidities or Market Clearing Theories?

Michael Parkin mentioned that the empirical results in his paper showed that "the sticky price theory [i.e., sticky wages or labor contracts] of the cycle is rejected while the class of theories based on market-clearing assumptions is not." Some doubts about the statistical analysis on which this conclusion was based emerged from the discussion, however. As John Scadding and Edward Bomhoff noted, making inferences about the dynamic relations between prices and output in Parkin's statistical analysis is questionable with the second differencing that was used. In addition, it was pointed out by James Tobin that the theories Parkin was testing were originally stipulated as explaining the deviations of real output from normal or natural levels, not as explaining the growth rates.

In fact, some people at the conference pointed out that the data are evidence that sticky wages and prices are playing a role. Professor Yōichi Shinkai and Jeffrey Sachs both pointed out that the greater

flexibility of wages in Japan, compared with the U.S. and Europe, could be a reason for the greater output stability in Japan. The importance of nominal wage behavior in explaining why diverse countries performed so differently in the last 10 years was also emphasized in Stanley Fischer's presentation.

In sum, therefore, people seemed to see at least as much evidence in these data in favor of the sticky-wage or -price view of the cycle as against this view.

Do Stable Monetary Instruments Imply Stable Macro Variables?

Finally, as I mentioned above, there was some disagreement about the role of monetary stability in generating real output stability. The reduction in output and inflation variability in Japan is clearly associated with the reduction in monetary variability. But it is clear that merely stabilizing the growth rate of the money supply will not necessarily guarantee stable real output growth. Of the countries examined in the 1970s and 1980s, the U.S. has had one of the best records for small variability of money growth, but one of the worse records for large variability of output growth.

Allan Meltzer emphasized that the lack of a relationship between low variability of money growth and low variability of output growth observed in some countries (in particular the U.S.) must be assessed within a framework that allows for fluctuations in velocity. He also mentioned that the correlation between velocity and money growth was related to the credibility of the central bank. A central bank with high credibility could permit a temporary increase in money growth without consequences for inflation and output.

II. Analytical Results

I will focus on three important theoretical results developed in papers at the conference: the effects of highly mobile capital, the distinction between physical capital mobility and financial capital mobility, and the appropriate policy rule to deal with economies with large "IS" versus large "LM" shocks.

A. The Effects of Highly Mobile Capital

The assumption of nearly perfect international capital mobility has been a key feature of all the theoretical papers presented at the con-

ference, including the Sachs paper, the Niehans paper, and the Fukuda-Hamada paper. There was some discussion about the adequacy of this assumption and its omission of risk premiums. John Makin also noted that there is an asymmetry in arbitrage due to the different tax treatment in the different countries, which is ignored by the perfect capital-mobility assumption. However, no workable alternatives to the simple theoretical assumption of perfect capital mobility were proposed.

What are the implications of the highly mobile capital in these models? Jeffrey Sachs showed in his simulations that monetary policy has a relatively small impact on other countries with highly mobile capital and flexible exchange rates. In his comments on the Sachs paper, Professor Komiya mentioned that this property was also a feature of several other recent studies of perfect capital mobility. I think it is a potentially important result, and have demonstrated it in my own work (Carlozzi and Taylor 1985). It suggests the possibility of conducting monetary policy relatively independently in different countries under flexible exchange rates.

It is also important to note that with perfect capital mobility a fiscal expansion will *appreciate* the currency. This result should be of particular importance now as a large fiscal expansion is being planned for Japan. This result was emphasized by Jeffrey Sachs.

This currency appreciation associated with fiscal expansion was discussed in James Tobin's opening remarks. If in fact there is a fiscal expansion in Japan, then there will be upward pressure on the yen. If this appreciation of the yen is to be avoided, then monetary policy must change. The Bank of Japan can expand, putting downward pressure on Japanese interest rates and the yen, or the Federal Reserve and other central banks can contract. If sterilized intervention has little effect, then these are the only two possibilities. This is the implication of the models. As Tobin said in his keynote address, his view is that the Bank of Japan should expand rather than the Fed contract.

B. Gestation Lags and Physical Capital Immobility

Jürg Niehans's paper began by studying the effects of fiscal policy changes on capital flows, in a model in which physical capital was mobile between countries. However, by introducing gestation lags he also developed results for less-than-complete physical capital mobility caused by the simple fact that capital takes a period of time to build—there is a gestation lag. Niehans also showed that the effects of policy would be quite different if gestation lags were different in different countries. His theoretical analysis focused on a case where one coun-

try has no gestation lags and the other country has gestation lags. This is an extreme characterization, of course, and was used for analytical convenience.

More generally, both countries would have gestation lags but possibly of different lengths. Such differences in gestation lags are a potentially important reason why fiscal and monetary policies may have different effects in different countries. If such differences are large, then imposing similar monetary policies on different countries—as would be implied by a strict version of fixed exchange rates—would be suboptimal. Of course, before drawing such an inference one would need empirical evidence for differences in gestation lags.

C. Optimal Policy Rules to Deal with Shocks

Both the Barro and the Fukuda-Hamada papers delve into the question of how policy should respond to disturbances when there are both IS and LM disturbances. As many people pointed out in the discussion, the issues here are similar to William Poole's famous analysis. In the Barro paper, the focus is on the closed economy and the key result is that when shocks to the LM curve are large it is better to smooth out interest rate fluctuations, but when IS shocks are large it is better to let interest rates fluctuate and thereby smooth out the fluctuations in aggregate demand. There is an important innovation in the Fukuda-Hamada paper, in my view, in that this same type of Poole result is shown to hold when there are country-specific shocks to the IS and LM curves. When there are country-specific shocks to LM curves, they show that smoothing interest rates and exchange rates is a desirable objective. When there are large country-specific shocks to the IS curves, then interest rate and exchange rate variability is desirable.

Since the Barro and Fukuda-Hamada results are so reminiscent of the Poole analysis, much of the discussion related the results back to issues associated with the Poole analysis. There were many useful comments that need to be digested by policymakers in order to interpret the results of these papers. Michael Parkin mentioned that when there are only two types of shocks, things are easy. But when there are also supply shocks, one gets more ambiguous results. It is not so easy to make the Poole classification, and in some cases one needs a social welfare function. James Tobin emphasized that the specific time period one is talking about is very important. Are these months, weeks, quarters? He also mentioned that it is not realistic or necessary to think of the responses as one extreme or the other, but rather as a

compromise which involves some degree of interest-rate smoothing. William White emphasized that the two-country results are artificial in that the countries are not symmetric, as in the Fukuda-Hamada paper.

In his presentation of the Fukuda-Hamada paper, Mr. Hayakawa mentioned that what is needed is an empirical treatment along the lines of the Barro paper. Such an empirical treatment would address some of the issues mentioned by Parkin, Tobin, and White. In other words, what is needed is a joining up of what Fukuda-Hamada have done and what Barro has done. I have been working on such a research project, one which applies an empirical approach to the optimal policy response in an international setting (Taylor 1987). It might therefore be useful for me to briefly summarize the results of that analysis and relate them to these two papers.

The issue is that in order to make some assessments about the desirability of one international policy regime or another using the Fukuda-Hamada approach, one needs an empirical measure of the size of the shocks and the size of the parameters of the model. Barro's approach provides for this in a simple model of a closed economy. Otherwise we will be forever saying, "If this shock is big, do this; if that shock is big, do something else; and don't forget the correlation between the shocks and the relative size of the slopes of the IS and LM curves." An empirical approach gets around this problem in principle by obtaining real-world measures of the parameters and of the shocks.

My empirical approach starts with an quarterly empirical model of the G-7 countries estimated over the last 15 years. I take this model and simulate it over the next 15 years using parameters and the distribution of shocks that I have estimated. I do this simulation over several different regimes, one of which is a flexible exchange-rate regime and another of which is a fixed exchange-rate regime with the U.S. as the center country, and Japan, West Germany, and the other countries using monetary policy to peg the exchange rate.

What I find is that the performance of some countries deteriorates by a large amount under the fixed exchange-rate system in comparison with the flexible exchange-rate system. In particular, the variability of output and prices in Japan and West Germany is much larger under the fixed exchange-rate stochastic simulation than under the flexible exchange-rate simulation. Moreover, one can easily see the reason for this in the simulations: the variability of money growth in Japan and West Germany becomes much larger when exchange rates are fixed with the U.S. as the center country.

These results can explain the empirical observations I noted above,

that the variability of output growth in Japan and West Germany was higher in the fixed exchange-rate period of Bretton Woods. The higher variability of money growth during that period is also explained by the simulations. The empirical results confirm the theoretical possibility that the flexible exchange-rate system has given some independence to the monetary authorities. I think the results are also suggestive of what might come out of a linking together of the empirical and theoretical ideas in the Barro and the Fukuda-Hamada papers.

III. Policy Implications

In Figure 2.3 I summarize, in two simple charts, the macroeconomic performances of the United States, Japan, and Europe in the 1970s and 1980s that we have discussed at the conference. The charts show quarterly observations on the *levels* of real output. Since these are the raw numbers, there should be no issues about the method of detrending, or about the focus on levels, rates of change, or deviations. As was mentioned in the discussion, for the purposes of comparing economic performance the levels are perhaps the most reliable indicator.

The top panel of Figure 2.3 is a pairwise comparison of Japan with the U.S., and the bottom panel is a pairwise comparison of Japan with Europe. The charts illustrate rather dramatically the smooth performance of Japan compared with the volatile performance of the U.S. and Europe. Compared with the United States, output in Japan looks as smooth as U.S. potential GNP! It is hard to project any more stability. The most noticeable difference in the chart since the early 1970s between Japan and the U.S. was the big expansion in the U.S. in the late 1970s, followed by the big slump in the U.S. in the early 1980s. Such fluctuations are not noticeable for Japan. This one large swing alone accounts for much of the difference in variability between the U.S. and Japan as reported in the standard deviations.

The bottom chart uses the same scheme to compare Japan with Europe. Again, compared with Japan there was a big expansion in the late 1970s in Europe, followed by a slump in the 1980s. Compared with the U.S., the recovery in Europe was very slow. That slow growth rate in the 1980s contributes to the poor performance in Europe.

A. What Were the Policy Mistakes?

Let me first ask, with reference to these two charts, what were the

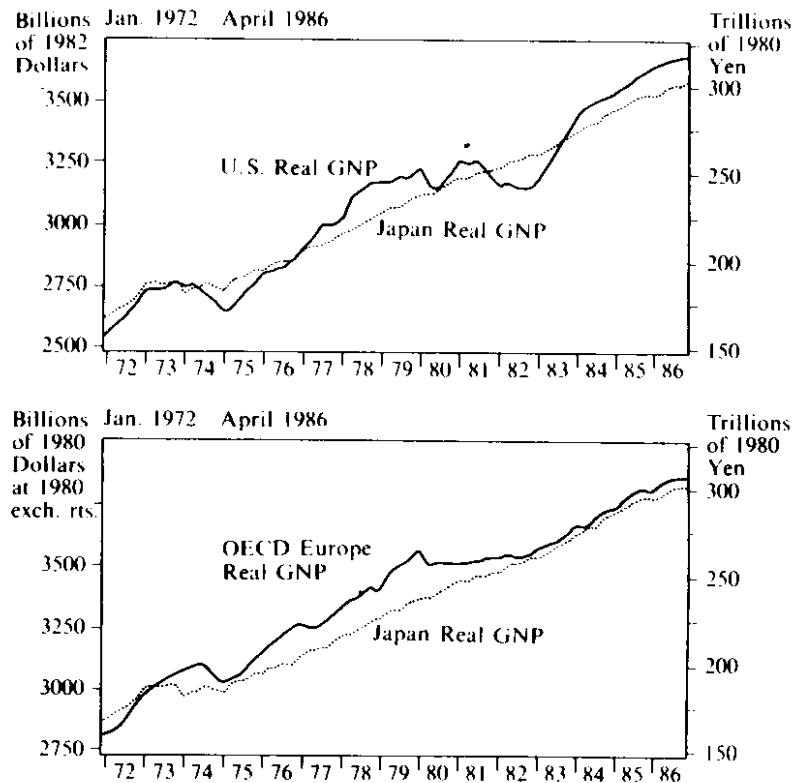


Figure 2.3
 Comparison of Real Output Fluctuations in Japan, the United States,
 and Europe, January 1972 through April 1986.
 Source: *OECD Quarterly National Accounts*, No. 4, 1986.

policy mistakes to which people have pointed at the conference? In his paper, Stanley Fischer points to a mistake associated with the U.S. expansion in the 1980s: "In retrospect, it is clear that U.S. monetary policy in the period between the oil shocks was too expansionary, even though money growth rates did not rise much. Rather, the rapid growth and rising inflation were accompanied by an increasing velocity of circulation (the case of the missing money)." Andrew Crockett also emphasized in one of his remarks that policy was too expansionary in the 1977-78 period, perhaps because the slowdown in the trend of potential GNP was not correctly assessed. Edwin Truman, in criticizing Jeffrey Sachs for not including all the important factors in his ex-

amination of fiscal policy in the 1980s, said, "The failures of the 1980s echo the failure of the 1970s, and any analysis which ignores this is incomplete," by which he meant the overexpansion in the 1970s led to the collapse in the 1980s. Again, the focus is on this same policy mistake. And James Tobin spoke about the courage of Paul Volcker in bringing an end to the overexpansion of the 1970s as well as in ending the recession of the 1980s. In sum, there appears to be considerable agreement that a big monetary policy mistake in the U.S., and perhaps in other countries, was the overexpansion of the late 1970s before the second oil shock.

Another mistake which I would emphasize, although only a few people such as Stanley Fischer have touched on it, is the slow recovery in Western Europe since 1982. If one looks at growth rates, it might appear that the growth rate is fine in Europe during this period. But looking at absolute levels reveals a more dismal picture. Perhaps a more expansionary policy could have been pursued. Of course, one of the problems with that view, and why it may be controversial, is that the level of normal GNP may be lower than implied here, so that a faster recovery would have been inflationary. But my own view is that the recovery was too slow.

B. International Policy Regime

I have already summarized the results pointing toward the advantages of a more flexible exchange-rate regime in permitting monetary independence and allowing for more stable output performance in some countries. A look at the "policy mistake," discussed above, in terms of the simple picture in Figure 2.3 brings home in a simple, intuitive fashion the fact that the flexible exchange-rate system has been an advantage for some countries. With flexible exchange rates, Japan did not have to go through the boom-bust cycle brought on by the overexpansion of the late 1970s. This simple picture, along with the more formal analysis, indicates to me the advantages of a more flexible exchange-rate regime.

C. Which Policy Rule?

There was surprisingly little debate about the rules-versus-discretion issue at the conference. Allan Meltzer opened the conference with the suggestion that we focus on policy rules, possibly active rules, and there appeared to be no dispute registered.

Unfortunately, there was much less agreement on what the form

of the policy rule should be. Allan Meltzer proposed a simple rule for money growth. In my view the rule makes only a very crude correction for velocity, and the fact that it focuses solely on rates of change rather than on levels means that it might generate recoveries that are too drawn out, or booms that are too long or inflationary.

As an alternative, Stanley Fischer mentioned nominal GNP rules. A nominal GNP rule, if strictly adhered to, can also lead to problems if it focuses on rates of change rather than levels. The focus on rates of change in the nominal GNP rule that Fischer attributes to the Bundesbank may have contributed to the slow recovery mentioned above. One needs to have some periods where the nominal GNP growth rate is faster in order to recover adequately from slumps.

I like to think of the ideal policy rule as minimizing the deviations of real output from normal or natural levels, with a correction for inflation. If inflation picks up, the central bank should contract and let the economy drift below normal until inflation dies down. The main difficulty with this rule is determining what is the normal or natural level of output.

D. International Policy Coordination

Finally, what are the policy implications with respect to international policy coordination? If one accepts the view that there are advantages to fluctuating exchange rates, then to some extent the policy coordination issue is easier. There is less need for policy coordination if exchange rates are to fluctuate freely, in the sense that monetary policy can be independent. This doesn't mean, of course, that coordination is not necessary.

Perhaps the most important question for future research is, What should be the appropriate degree of monetary policy coordination when exchange rates are permitted to float? For example, perhaps there should be more systematic coordination with respect to the operating strategies in the different central banks. By operating strategies I simply mean the mechanism through which the short-term interest-rate targets are moved around for the purpose of controlling money growth or nominal GNP. Presently, there is some coordination—occasional telephone calls about changes in the Fed Funds rate—but more could be done on a routine basis. Such coordination would probably require some disclosure of the policy rule that the monetary authorities are aiming for, because the coordination would require statements between central banks about what the policy intentions are.

This coordination would be useful even if policy focused only on

domestic goals. Simply announcing your intention for a change in the funds rate or a call money rate to the other central banks on a routine basis and receiving comments would be an important first step. As a more basic first step, perhaps there could be more coordination of the research activities of the staffs of the different central banks.

Finally, with respect to fiscal policy, none of the results of this conference indicate that the coordination of fiscal policy is not important. On the contrary, the joint easing of fiscal policy in Japan and tightening of fiscal policy in the U.S., in order to help address the current account imbalances, was mentioned by many participants at the conference as just the type of coordination that we need now.

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