

# General Discussion: Has the Business Cycle Changed? Evidence and Explanations

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*Chair: Guillermo Ortiz*

**Mr. Cotis:** First, congratulations for this excellent paper. It puts the right emphasis on the big macro shocks of the 1960s and 1970s. Last year at this symposium, we had an opening paper that failed to account for big supply shocks and, as a result, probably reached complacent conclusions about our ability to improve the quality of forecasting and policymaking. Maybe this time we have moved the other way around and your paper could be a bit too destructive. Perhaps macroeconomic policies did a better job in stabilizing output than you say. This is at least what the OECD research is suggesting. I will illustrate my point with two examples—one from monetary policy and one from fiscal policy, which is somewhat ignored in your paper as a source of macroeconomic stabilization.

Let me start with monetary policy and its capacity to anchor inflation expectations. You say that no such anchoring was visible in the United States until recently. This is probably true, but maybe it is a bit “Americo-centric,” since in most other OECD countries there were early signs of expectation anchoring. To be more specific, in our research at the OECD we looked at the variability of inflation relative to the variability of the output gap. Inflation variability fell in relative terms in most countries from the mid-1980s onward, with the three big exceptions of the United States, Germany, and Italy. So,

in the majority of OECD countries, you may have seen an inward shift in your frontier, partly driven by monetary policy. More generally, the main ambition of monetary policy is to anchor expectations with a view to changing, for the better, the underlying structure of the economy. This is a higher ambition than just improving current outcomes within a given, unchanged, economic structure. On the fiscal side, I think it is very clear that the influence of automatic stabilizers has increased markedly in many OECD countries. This must have played a role, which you are missing in your exercise.

**Ms. Krueger:** Mine is a shorter question, but I wondered to what extent one can take the so-called macro shocks as having been so exogenous. The one that leaps to mind immediately is the oil price increase in 1973-74, which I think of as having come at the end of a commodity price boom—itself a result of the dollar inflation and, for that matter, labor union strikes and things like this, which I think were partly because of uncertainty about relative prices. If so, treating those as macroeconomic shocks that are quite exogenous may understate quite significantly the role of improved policy.

**Mr. Sinai:** A question for Chairman Greenspan. The analytics of your commentary on the risk management approach to monetary policy are persuasive, as you described it in a couple of examples you gave. There are other examples that weren't given, making the risk management framework very persuasive for future policy. If so, how can such an approach be replicated, or approximated, in future practice? Is it generally usable or in as straightforward a way as, say, quantitative inflation targeting, such as is used by other central banks. This is a question for the Chairman.

I have another comment. It is on the paper by Stock and Watson. One of the striking features of monetary policy since about 1987 is preemption. When you look at actions of monetary policy prior to 1987, you see that interest rates were cut only after real GDP growth went negative, and interest rates raised to tighten only after inflation was high and rising. Many think that added a great deal to the pro-

cyclicality of the economy—hence, output volatility. Now, post-1987, you will find (I am sure that you can see this) that starting in 1987 interest rates were raised in advance of rising inflation and in 1989 interest rates were cut before GDP growth went negative. In 1994, interest rates were raised when there was no sign of accelerating inflation in the data. In early 2001, we had very aggressive reductions of interest rates, huge reductions of interest rates, even before GDP growth was found to be negative, which happened long after the fact. Now, I have to believe that in a dynamic system, when you change the timing of an intervention such as I have described between the decades of the 1960s, 1970s, and the 1980s, 1990s, it changes the amplitude and the timing in the dynamics of the business sector. I would argue that the model specifications used in your paper to analyze pre- and post-1984 don't really capture this change. They are very model specific and don't at all capture the essence of the preemption that occurred and what that means for the dynamics of the business cycle. Or, put another way, tell me how the way you tested pre- and post-1984 might account for the kind of criticism I am leveling?

**Mr. Poole:** I have a couple comments. First, I want to try to stiffen Brad DeLong's backbone before he burns his lecture notes. Let me suggest a couple considerations here. First, the Stock and Watson paper underplays the importance of the interaction of inflation uncertainty and output variance. Let me mention two things. Inflation uncertainty tends to propagate shocks through the economy. Anne Krueger talked about that.

The 1973 oil shocks, for example, were certainly propagated by two mechanisms. One was the price controls and the allocations on oil that were themselves a consequence of the inflationary environment, and secondly, the rising inflation expectations that were triggered by the shocks. If you look at more recent oil price shocks, both up and down, and look at the behavior of oil price futures, you can see that the shocks were not built into the longer run expectations. So, there is a lot of evidence that the shock mechanism propagation is quite different now than it was when inflation was high and uncertain.

Secondly, with inflation certainty and expectations well anchored, there is a lot more room for the monetary policymakers to react vigorously. Allen Sinai was talking about that in terms of the most recent experience. That feeds into another comment. If there is a major change in the cyclical behavior of interest rates, historically both long and short rates are classified as coincident indicators. If you go back over U.S. business cycle history, you see that rates turned within a few months of cycle peaks particularly, but also cycle troughs.

During the last two recessions, long-term rates, in particular, turned approximately a year ahead of the cycle peak. That was true in 1990 and again in 2001 cycle peaks. Now, how can that be? It reflects expectations in the market about the course of monetary policy. That was highly stabilizing and it was also one of the major reasons why this time the housing market behaved in such a non-cyclical fashion, so very differently from previous cycles.

**Ms. Rivlin:** This point is implicit in Brad DeLong's excellent comments. I too would like to stiffen his backbone. Isn't there an inherent problem in this Stock-Watson approach in that we know a shock only because it affects output? That is the definition of a shock.

Two points about that: It may be that the world economy has changed in ways that made commodity shocks—the kinds we used to have—less likely. We know that the U.S. economy is less dependent on oil than it used to be and that OPEC probably has less leverage than it used to have. The other point is that we had all these other shocks, as Brad pointed out—the financial market shocks that you cannot identify really in the earlier period and that have not affected the economy. Nobody has mentioned 9/11. That was the shock that might have gone around the world and it didn't.

**Mr. Feldstein:** I think the paper by Stock and Watson is very careful and well done, but it understates the contribution of monetary policy to the recent economic stability. Brad emphasized two reasons for that understatement: the measurement of changes in

policy and the measurement of the shocks. I would add a third and that is the measurement of volatility that they use by focusing on variance of quarterly changes in GDP. What if we are concerned not about those relatively small changes, but about the probability of a really deep and serious recession? By avoiding the high inflation of the late 1970s, the Fed has succeeded in avoiding the major recession of the kind that was necessary to turn that inflation around. Looking ahead, to the extent that the Fed can continue to keep the inflation rate low, it increases the probability of having a more stable—in the sense I have just defined it—performance of the real economy.

**Mr. Freedman:** The point I want to make picks up the point that Marty Feldstein just made. Canada is the outlying country in the Stock-Watson picture. I remember making speeches in the early 1990s, saying that keeping inflation low will not cause the business cycle to disappear. However, if we could avoid the kind of deep cyclical downturns that we had in the early 1980s and early 1990s, that would be an immense contribution. This question of the endogeneity of shocks is very important here. One of the things we have seen in the postwar period was a situation in which we get these enormous overbuilding episodes, for example, in nonresidential construction, which are distortions largely caused by inflation expectations. In the second half of the 1980s in Canada, there was so much overbuilding of office space that there was almost no new building built throughout the 1990s. If you can get inflation down, which we did, then the distortion caused by inflation expectations should be considerably less and you will not get those kinds of sharp downward movements. That is exactly the experience we have seen. How that gets picked up in the kind of metrics that are used Stock and Watson is not clear, but the importance of that endogenous response of behavior to a reduction in inflation is very important.

**Mr. Berry:** I would like to reinforce some of the comments of the earlier speakers. In the 1970s, it is not clear there were ever any physical shortages of petroleum in this country. The price controls, which were there in both oil shock periods, severely distorted distribution.

One key thing that happened was that there was a gigantic shift in inventory from what we could measure into the gasoline tanks of the nation's cars. They went from one-third full to two-thirds full, and suddenly you had an apparent shortage.

What is going on here may be that you are looking too narrowly in this paper at policy changes. We no longer have price controls. You have had a series of episodes—because the market generally has been much freer of regulations of all types—in which the market has been able to handle what in the 1970s were major shocks and turn them into much smaller shocks. For instance, in 1990 I can recall Olivier Blanchard and Bob Hall saying that the reason we had a recession was a spontaneous shift in consumer sentiment triggered by the oil shock. Yet, prices started to come down even before the fighting started in the Middle East. Once you go through that, you see an oil price spike and you don't see that moving into the price level generally and you see that what goes up comes down. You simply get a very different kind of response to what in the past would have been a big shock.

**Mr. Hildebrand:** I was wondering whether the authors could elaborate a bit on the question of why international synchronization has not increased more during the great moderation period. One simple explanation might be that domestic political events, or shocks if you like, continue to be powerful enough to counteract global synchronization trends.

**Mr. Sims:** I figure somebody has to stiffen Stock and Watson's backbone. My impression, based on research I started a long time ago, is that monetary policy in the United States has changed much less than people think. The systematic component of monetary policy is quite stable. Technically, the reason that I get this kind of result and Stock and Watson don't (and much of the other literature doesn't, though there is actually quite a literature that finds little change in monetary policy) is that it is very important pre-1980 to account for the fact that monetary policy reacted not just to inflation but to growth in monetary aggregates. The Taylor principle—that stability

comes from an aggressive enough response to increase in inflation—actually in the presence of monetary aggregates in the reaction function becomes a principle that the sum of the response to money growth and inflation should be large enough. You find the response was large enough if you apply statistical methods.

Brad gave us this great quote from Rudi Dornbusch about the Fed killing expansions. I think that is right. But if you think about it, there was a systematic component of monetary policy before 1980 that involved reacting strongly when inflationary pressure got far enough out of bounds. My view is that, if anything, Stock and Watson have gone too far by saying that the end of the great inflation in the United States was due largely to a change in monetary policy. It was due to monetary policy, but the systematic component of monetary policy as it existed before 1980 would have eventually responded strongly enough to end the inflation. The dynamics would have been a little different, but my own experiments suggested that the overall time pattern would not have been very different.

**Mr. Angelini:** My question is partly related to the point that was raised by the discussant, and that has to do with the G-7 part of the stylized fact that we observe. The point is: Has the great moderation really occurred in all the other countries that are taken into account in the study? If you normalize by the level of the growth rate, and that was what DeLong was mentioning for Germany, the moderation is really not apparent at all in countries such as France, Germany, Italy, and Japan. In other words, in your Chart 1, you see a clear downward trend in the growth rate in these countries and I am wondering whether once you take into account these facts you still observe the stylized fact that you are after?

**Mr. Goldstein:** My question is for Professors Stock and Watson. You tried to estimate the effect of changes in monetary policy on output volatility, but you didn't account either for fiscal policy or exchange rate policy. We had a big shift from fixed to flexible exchange rates. During most of the last expansion, the dollar was rising, which helped

keep output in this country from growing even faster and moderated overheating. I recognize it is harder to model fiscal policy and exchange rate policy than monetary policy. But if you tried to take account of them, would you still get a similar conclusion on the effect of policy versus shocks?

**Mr. Budd:** I just want to make a quick comment about the United Kingdom. The chart shows a quite dramatic fall in volatility in the United Kingdom. The authors date this from 1980. They also have rejected the hypothesis of a regime change. The United Kingdom does seem to be one country in which the regime change story works rather well, because that was the year in which government was elected and specifically for the first time in the postwar period abandoned the objective of full employment and said in the future it would not target full employment, it would target inflation. Now, of course, it made mistakes, as people do when they change the regime. What we have seen since then is the fall in volatility that the chart shows, of course a fall in inflation, and also a steady fall in unemployment. My explanation for this change has been that when the authorities were trying to target full employment, they were targeting a level that was not sustainable. So, we did have boom and busts. They tried to achieve it and they then had to slam on the brakes to prevent the resulting inflation. What has happened instead by targeting inflation is that we have had this low volatility and we have also seen the economy find its natural rate of unemployment, which has proved to be pleasingly low. That is my story and I am sticking to it. I am part of the group that doesn't want to have its story changed. Of course, it is true we have to explain why the same things happened elsewhere and there are obviously common causes. Nevertheless, in the past, the United Kingdom has proved itself perfectly capable of achieving its own output volatility when no one else was. The only final point I should make to this rather good story is that the most recent low level of volatility owes something, it must be admitted, to a change in the way the official statisticians record GDP changes. They do now pre-smooth them, so it isn't quite as smooth as it looks.

**Mr. Fraga:** I just want to add a view from the developing world and a suggestion. There is a lot more variance in our neck of the woods, so I would encourage the authors to check out the action on our side. Here is what I suspect you may find: 1) that the endogeneity of shocks is important. I and a couple of coauthors have just done a study looking at inflation targeting. We come out with that impression after looking at the data. 2) Again, in the developing world, there is no question that changes in macroeconomic regimes and, in particular, in the monetary policy regimes have played a very important role in improving the inflation-output volatility tradeoff or frontier.

**Mr. Summers:** I just want to emphasize the point that Brad attributed to me earlier. I don't think it is plausible to treat as a common phenomenon the reduction in cyclical volatility in the United States, Germany, and Japan. It seems to me that it would be much more right if you want to think about cyclical performance or the quality of aggregate demand management policy as judged by its outcomes to say that we have had substantial success in the United States in the post-1985 period relative to history and substantial failure in Europe and Japan relative to history. So, the approach to distinguishing explanations that is based on the international comparisons seems to me to be unwarranted. I would want to go back to the "common-sense hypothesis" that there was a philosophical change in U.S. monetary policy, dating from the late 1970s. Outcomes were better after it than before it. I am not entirely sure that that is usefully captured by the changes in the structural component of the estimated Taylor model response functions.

**Mr. Watson:** Let me start by discussing one of Brad's comments. It is something that has come up repeatedly, which is our focus on the Taylor rule and, in particular, not on stop-and-go policy that many people have mentioned. It certainly is the case in our formal modeling in the econometric exercises that we did look at that we compared variability of output by changing coefficients in a Taylor rule and not by changing the structure of the systematic responses of interest rates to inflation and output, as you might have thought about doing in some sort of nonlinear stop-and-go policies.

There are some numbers I found particularly compelling when we thought about this. As we report in the paper, the decline in the standard deviation in the United States of output growth post-1984 and pre-1984, you see about a 40 percent decline post-1984 relative to pre-1984. Thinking about stop-go policy, if you take the 1979-83 period and throw it out of the pre-1984 time period, you see a reduction in volatility not of 40 percent but of 30 percent, still quite substantial. So, if you take all of the 1979-83 period and say that monetary policy is responsible for that variability and, hence, only concentrate on the pre-1979 period, you again see a significant reduction in variability. You might say, "Well, wait a minute. What about the period 1973 to 1976?" Well, take 1973 and 1976 and throw it out, and 1979 and 1983 and throw it out, so now you are comparing the 1960s, the beginning of the 1970s, and a little bit of the late 1970s. You see a 20 percent reduction in volatility post-1984 relative to this pre-1979, eliminating the 1974-75 recession period. As Jim mentioned in his comments, the reduction in volatility was not only during the stop periods but also during the go periods. I found that evidence pretty compelling that including stop and go wasn't going to save the monetary policy explanation.

There have been lots of discussions about our focus on systematic policy as being too narrow a view of monetary policy. There is a considerable amount of merit in that criticism. What we have done is to focus just on the systematic response of interest rates to inflation and output. Thinking about policy more generally, thinking about financial deregulation, and thinking about price controls in particular, it seems to me anyway that it is pretty clear that those parts of policy are important in allowing the macroeconomy and the financial markets to digest financial shocks. We saw that. Post-1984, many of the shocks that Brad mentioned were things that were digested very quickly post-1984 and might not have been digested pre-1984 because of the different financial structure and different policies. I am going to stop there in case Jim had something additionally to say.

**Mr. Stock:** I'll make just a couple of quick comments. Much of the discussion is associated with what we interpreted, and perhaps had taken some issues with what we have interpreted, as a structural shift on the one hand, a shock, or monetary policy. The way we tackled monetary policy is, in a way in the context of these models, can account for changes in the timing of monetary policy changes in the way that inflation or price shocks feed through to expected inflation. Those are the sorts of channels, feedbacks, and propagation mechanisms that are handled within these calculations.

A deeper question, and a really interesting one, is that of shock endogeneity. For example, are strikes less likely to occur in a situation with reduced inflation uncertainty? Surely at a level when one thinks of hyperinflations or situations either historically or in economies in great turmoil, the answer is, "Yes. Of course these are endogenous effects." A more relevant question for our calculations is, "Is this a potential endogeneity empirically important at the level of, say, comparing the 1970s and the 1990s? Are strikes in the 1970s a consequence of productivity slowdown and so forth or is it a consequence of inflation uncertainty?"

Some of the issues that were raised have to do with changes in fiscal policy. The short answer on that is those would be incorporated into changes in the coefficients. We were treating those as structural shifts in the economy.

Maybe the most important point here is thinking about what the bottom line conclusion is. We are not saying that monetary policy doesn't matter. We are not saying that things could not have been worse. We could easily imagine that under a different scenario or different leadership, the Fed easily could have bungled many of the circumstances it faced. It didn't do so. Our point is not that the Fed is irrelevant to economic activity. Our point is a much more limited one. We are looking at the volatility of cyclical fluctuations in the 1980s and 1990 versus those in the 1960s and 1970s and asking the narrower question, which is the change in regime that was associated with going from the 1970s to 1980s and 1990s. Should that get the credit for the

volatility reduction? There we say that the answer is no. This is a different statement than saying that the Fed could not have increased volatility had it so chosen to do.

**Mr. Greenspan:** I am just going to follow up on the issue that Jim Stock is raising. How one defines shocks is really crucial to their paper. In a very broad sense, one can imagine a very detailed model in which almost everything is endogenous. Indeed, even the gold finds in the 1890s in Alaska can be formulated as endogenous. The issue, for example, that Anne Krueger raises, is that the 1973 oil price shock is conceivably endogenous if one remembers that, prior to 1970, there was surplus crude oil capacity in the United States suppressed by the Texas Railroad Commission's quotas. Indeed, in many of the periods when world shortages occurred prior to then, the constraints on U.S. oil production were relaxed, markets were flooded, and world prices were determined in the Texas Gulf. By 1970, U.S. oil consumption had essentially absorbed all of our capacity. Within a very short time, the marginal pricing for world oil moved from the Texas Gulf to the Persian Gulf, and has been there ever since, manifesting itself in a huge shock at that time. In somebody's model, that has to be an endogenous event in some form or another. The reason I think this is important is an issue that Alice Rivlin raised as well—namely, that the perception of defining a shock as something that reduces output puts aside the question as to whether the shock, irrespective of its gross size, was absorbed. I would submit that there has been a very significant increase in the flexibility of the American and other economies, to absorb shocks. Indeed, I have been watching economic shocks for 50 years and modeling them. They all look very large to me. I don't perceive that has been any reduction in the 1990s and forward. What has happened is that supply flexibilities have emerged as a consequence of very significant deregulation, starting in the mid-1970s. The obvious major technological changes, which enabled inventory management to be improved and a whole series of other flexibilities, especially in the labor and financial markets, have enabled this country to absorb shocks in ways that I don't believe we would have been able to do in the previous period. If that is indeed the case, one can argue that the perception that there are smaller

shocks is really a measure of the net shock, namely the gross shock minus improved resiliency and flexibility that has been exhibited in the American economy. That is a crucial issue in the determination of where we go in the future. There is no question that September 11 was a shock of monumental importance, but I defy you to find its residual impact after three months. That is an extraordinary event, especially if you observe what was going on in the marketplaces exactly at that time. We have had previous occasions; the LTCM situation I think was mentioned. We had risk premiums rising in riskless securities because they were illiquid. These are events that have very major impacts on output but are not measurable, except in the net sense, and are, in a sense, not measurable except by definition. That is, it is very easy to define a whole series of special shocks, but it is also very easy to define them away by endogenizing them in your models. Choosing four separate econometric models does not solve the model problem, in that all four models have the same problem that concerns me—namely, that none of these models is successful in forecasting the economy without very significant add factoring. If they are not successful in forecasting, it means that their specifications are not fully capturing the dynamics of the real world. Therefore, to employ coefficients of models whose capability of forecasting is dubious and simulating the marginal effects of various different types of actions strikes me as a question that has to be resolved a little more sensibly. I don't perceive, for example, that if you have four econometric models, all structured in a similar manner, that it necessarily substitutes for the reality that we observe.

**Mr. DeLong:** Let me make two points. But first, let me thank everyone who is trying to stiffen my backbone. Nevertheless, I would have bet serious money *ex ante* that Stock and Watson's quantitative calculations would have come out the other way—that there is enough of the policy change in what they call policy that I would have expected it to show up as having a significant effect, and I am still surprised that it didn't.

Secondly—and contradicting this first point—I would like to underscore Mr. Fraga's point that lower shocks, if lower shocks there

be, are not evident in the developing world. If anything, the past decade and one-half kind of makes me worry about how relatively small events, nevertheless, seem to turn into big output shocks in the developing world. In looking back over my career, I see many analytical low points. But my global analytical leader has to be a little memo I wrote for my Treasury boss in the spring of 1994, which said, "Yes, the Bank of Mexico's policy was inappropriately expansionary, but the magnitude of the policy mistake was small and it should not be a serious problem." I still think the Bank of Mexico's sins against the gods of monetarism in 1994 were venial and not mortal. But the fact that such venial sins were followed by such swift and awful punishment has to give you pause about talking about the stability of the system and small shocks.