

# Commentary: Has the Business Cycle Changed? Evidence and Explanations

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James Stock and Mark Watson's paper challenges things that I thought I knew and tells me that I am going to have to rethink a bunch of issues—going to have to mark my beliefs to market anew once again.

To the extent that there has been a conventional wisdom among economic historians, the extraordinary moderation of the business cycle—the reduction in the size of swings in the unemployment rate and in the variance of annual output growth—has been due to very important learning about how to better conduct monetary policy. Christina Romer has been the most powerful advocate of this line of narrative. And this has been what I have taught my students during the past several years.

The founding of the Federal Reserve brought the possibility of an elastic currency and of avoiding the great liquidity catastrophes that afflicted the United States in the late 19th century: The silver-agitation crises of the 1890s, the great crash of 1873 when British investors grew nervous about the “crony capitalism” of America (a crisis with remarkable similarities to the 1997-1998 East Asian crisis), and the panic of 1907 (mitigated by J.P. Morgan's getting the New York Clearing House to expand the effective money supply via printing clearing

house loan certificates, and then cramming them down the throats by telling banks that they would incur his permanent displeasure if they did not accept them as valid and liquid instruments).

But the Fed's performance in its first two decades was not impressive. The disaster of the Great Depression brought further institutional changes: the coming of deposit insurance to avoid the radical instability of the money multiplier that was such a powerful feature of the Great Depression in America; keen awareness of the dangers of, as Milton Friedman's teachers put it, "unbalanced deflation." The role of fiscal policy when short-term safe interest rates are near their zero nominal bound floor and yet risk, default, and term premiums remain high. Before World War II, an economic disaster of the magnitude of the Great Depression was always a live possibility. With the institutional and organizational changes since World War II a macroeconomic disaster of the magnitude of the Great Depression is—well, before the recent Japanese experience, I would have said impossible.

Nevertheless, when you compare the pre-Great Depression to the post-World War II period, there is less of a reduction in the size of the business cycle than economists hoped or, in the case of Arthur Burns and many others, confidently expected. Improved credit markets allowed households to smooth their spending. Automatic stabilizers meant that incomes varied less than production. In his 1959 presidential address to the American Economic Association, Arthur Burns went as far as to say that deep recession—large spikes in the unemployment rate—were no longer a problem.

He was wrong. Look at 1975. Look at 1982-1983. In Christina Romer's interpretation, Arthur Burns was wrong because he did not recognize the developing stop-go nature of Federal Reserve policy. Most of the time, the Fed worried about achieving maximum purchasing power. Some of the time the Fed worried about achieving price stability. While it was worried about achieving maximum purchasing power, it successfully stabilized production, but at too high a level that allowed inflation to rise. As the late Rudi Dornbusch used to say,

expansions in the United States before 1985 did not die of natural causes: They were killed by a Federal Reserve that had shifted to a mindset in which reducing inflation was job #1. Thus, the first four post-World War II decades saw longer expansions, fewer recessions, but still substantial output variance driven by large inflation-control recessions. And the conventional wisdom has been that the remarkably good performance of the last two decades has been due to the Federal Reserve's greater success at maintaining its balance: at acting preemptively and maintaining an appropriate balance between price stability and maximum purchasing power, rather than careening from one objective to the other.

Now, James Stock and Mark Watson come to challenge this belief. The reduction in output volatility is there, is real, is very large. (Although, as Larry Summers was saying in the shadow of Mt. Moran an hour ago, the fact that Stock and Watson's index of the size of the business cycle has fallen by three-fourths over a time period in which the average German unemployment rate rose from 2 percent to 8 percent makes one wonder whether the variance of output is what belongs on the left-hand side.)

But Mark Watson and Jim Stock look hard and find little sign that reduction in output volatility is due to changes in how the Federal Reserve reacts to economic circumstances. By process of elimination, they conclude that the reduction in the output business cycle is primarily due to luck and not to skill: primarily to smaller shocks hitting the American economy (and the other OECD economies) and only secondarily to better monetary policy.

This surprises me. This is a shock. I would have bet serious money that Stock and Watson's calculations would have come out the other way. I need to mark my beliefs about this to market. Clearly, I have some serious rethinking to do before I give my "macroeconomic stability" lecture to my graduate students at the end of the semester.

But humans are, as cognitive psychology teaches us, really bad at changing their minds. We are really good at finding ways to explain away and ignore new information.

So, let me now try to explain away and ignore Stock and Watson's results: First, their idea of "policy" is limited to "systematic reactions by the Fed that change interest rates in response to changes in inflation and in economic growth rates." This is a very limited definition of "policy." For one thing, it leaves out much of what Christina Romer sees as harmful in pre-1984 policy: the fact that the Fed reacted in one way when it was worried about price stability, and reacted in another very different way to a similar economic situation when it was worried about maximum purchasing power. Stock and Watson's Taylor rule framework can't see this at all. (Now, it is true when Stock looks for evidence that such stop-go policy exists he fails to find it, but our econometric techniques are not very good at picking up such nonlinearities.)

Second, it is not at all clear that the actual shocks to the economy have been smaller since 1984. Before 1984 we have the Vietnam War, the oil shocks of 1973 and 1979, etc. Since 1984, we have had the stock market crash of 1987, the dot.com bubble and then the Nasdaq crash, the extraordinary near-panic of 1998 (which one low-ranking LTCM employee is supposed to have characterized as a nine-sigma shock: The universe will not last long enough for there to be an even chance of even one nine-sigma shock happening ever), September 11, presidential warnings that all Americans are at risk of attack by Iraqi drone aircraft carrying weapons of mass destruction. These shocks are smaller than the earlier shocks in Stock and Watson's framework, but are they really smaller in reality?

Doesn't the swift reaction of the Fed to 1987, to 1998, to 2001—swift reactions that find no place in Stock and Watson's measures of "policy"—play a role in reducing the size of the business cycle? Doesn't the emergence of more private-sector willingness to speculate

on stability as a result of confidence in the Fed reduce the magnitude of what Stock and Watson call “shocks”?

So, my rationalization is that a lot of what Stock and Watson’s framework calls “shock” is actually “policy.” This is not a criticism, really: They have done a very good job. But it is a product of the limitations of our analytical tools.

### **Response to comments**

First, let me thank everyone who has tried to stiffen my backbone. But it is worth remembering that I would have bet serious money that Stock and Watson’s calculations would have come out the other way. And it is still a shock to me that it did not.

Second, let me underscore Arminio Fraga’s point. Any interpretation of recent events that points to a smaller magnitude of shocks to the world economy has to explain why things have looked so different from a developing-country standpoint. Looking back at my career, I see many local analytical low points. But my personal global analytical nadir came in early 1994, when I wrote a memo for my Treasury boss saying that, yes, the Bank of Mexico’s policy was inappropriate and overstimulative, but that the magnitude of the policy mistake was small and there was no reason to expect it to generate a serious problem. Now, I still think the Bank of Mexico’s sins against the gods of monetarism in 1994 were small, were venial, and were not mortal. But the punishment was swift and awful. And that is hard to reconcile with the view of a placid, low-shock world economy.