GENERAL DISCUSSION

ANDREW LEVIN: I'm reading from the English version of equation 1. Unexpected inflation equals news about present value of surpluses or debt. So let me describe two more Michelson-Morley experiments for you apropos of equation 1.

Experiment 1: We're in a situation where the Congressional Budget Office is projecting that the entire government debt's going to be paid off, and I'm actually going into positive government holdings of assets. And then that outlook suddenly changes, and we're now in a situation where the ratio of government debt to GDP stabilizes. So, according to equation 1, I sort of think of that as a surprise, at least to many people in the public. Experiment 2 is a situation where the government debt-to-GDP ratio is stable at 20 to 30 percent of GDP. And it suddenly jumps up to 80 percent of GDP. And I would think of that Michelson-Morley style as, wow, that's going to create a big jump in unexpected inflation.

Well, these two scenarios are not just hypothetical. The first scenario is similar to the CBO's October 2000 outlook, which was then followed by substantial fiscal adjustments in 2001 and subsequent years. As for the second scenario, in 2008–12 the ratio of government debt to GDP jumped from 30 percent to 80 percent, but inflation remained very subdued. So I'm trying to challenge you here a bit with the fiscal theory of the price level, pushing back against your arguments here. Wouldn't those kind of huge fiscal surprises, if the fiscal theory of the price level is right, cause huge changes in unexpected inflation?

JOHN COCHRANE: Thanks for that softball. This equation is an identity. It's a present value relationship, just like price is the present value of dividends. Why can't you go out and measure expectations of dividends, discount them back, and predict stock prices?

Well, we've been trying to do that for forty years. It's not easy! It doesn't mean that price equals present value of dividends is a good place to start when you're thinking about how asset prices work. The fiscal theory works the same way. No, you can't take easily available surplus projections, discount them back, and nail inflation. That doesn't mean it isn't just as useful a framework as price equals present value of dividends.

Those CBO projections are largely, "Dear Congress, here is how the economy explodes if you don't do something," not a conditional mean of what will happen. If the CBO forecasts come true, we have a debt crisis. I think markets rightly believe that America will, once again, after she's tried everything else, do the right thing, which is the point of the CBO projections. I'll remind you that every war has huge deficits and huge debts, and markets understand that you guys are eventually going to pay this stuff off, and there is not huge inflation.

There's nothing as simple in the fiscal theory of the price level as a testable prediction that large debts give you big inflation, or large deficits give you big inflation. That's a good thing too, because the data scream that large debts and deficits are not highly correlated with inflation. Come on, I would not have spent twenty-five years with this if it could be dismissed that easily.

ANDREW LEVIN: But that sounds a little bit like your description of the captain steering between the two sea monsters. You know, the reason inflation didn't jump in either 2001 or 2008–9 is that the markets were already expecting everything that actually happened.

JOHN COCHRANE: It's on my last slide. As you understand, stock price moves largely with changes in discount rates, not with visible expectations of dividends. Likewise, changes in the valuation of government debt are far more important to understand changes in inflation. The amazing feature of the world right now is that the interest rate on government debt is so low. Low discount

rates make government debt valuable, so if there is a puzzle, it is that with *r* very close to *g*, that we don't have *de*flation.

MARKUS BRUNNERMEIER: I would like to come back to John's last point. In his earlier research, especially in his presidential address to the American Finance Association, John Cochrane stressed the importance of time variation of the stochastic discount factor.[5] In other words, asset prices are primarily driven by movements in the stochastic discount factor rather than by cash flow news. In my work with Yuliy Sannikov, "The I Theory of Money," monetary policy is primarily driving the stochastic discount factor and the risk premium rather than primary surpluses.[6] Hence, it is not the expectations about future primary surpluses (the analog to cash flows) that matter most for the current price level (and inflation) but the projection of stochastic discount factor movements. In your presentation, you didn't stress this component much, and I was wondering whether you can you elaborate on that further.

JOHN COCHRANE: Yes! I didn't want to show endless models and equations, but yes, when you have price stickiness, then changes in the real interest rate have an effect on the present value of surpluses. So there is an interesting mix between fiscal and monetary policy there.

EDWARD NELSON: Just as a matter of clarification, I think there are a couple of respects in which the models John was criticizing were more defensible than he was suggesting. In the case of the New Keynesian model, obviously one of the absolutely crucial papers in that literature is the Julio Rotemberg and Mike Woodford paper from twenty years ago, which had the standard New Keynesian equations but modified them with some fairly minor

5. John Cochrane, "2011 Presidential Address," American Finance Association, January 6, 2011, http://www.afajof.org/details/video/2870771/2011-Presidential-Address.html.

6. Markus K. Brunnermeier and Yuliy Sannikov, "The I Theory of Money," NBER Working Paper no. 22533, August 2016.

and transparent timing conventions about spending decisions.[7] And certainly their Figure 1 has an impulse response of the "correct sign" in which a tightening of monetary policy reduces inflation. So I think only a fairly minor modification of the standard New Keynesian model might be needed to get you that result.

On the matter of monetarism, you have to remember that Friedman and Schwartz's *Monetary History* certainly looked at periods in which M2 and the monetary base behaved differently, and whenever those occurred, they regarded money as M2, not the monetary base. And it's basically elementary textbook stuff that reserves aren't money and don't count in the money stock. Certainly big issues have been raised in the last few years about the relationship between the monetary base and M2. But I think there are important elements of the Great Recession period that make looking at M2 of interest. We didn't have a great depression, and in the Great Depression, M2 fell by a third, but it didn't this time. The Bank of England was very explicit in its QE policy, that holding up the money stock was one of the criteria of QE, and there are ways you can do that without relying on the money multiplier mechanism. So I don't think monetarism is going to be dismissed just by saying that the expansion of reserves didn't cause inflation. Remember, Friedman and Schwartz eventually wrote a whole book, *Monetary Statistics*, defending M2 as the definition of money. The variable that John quite rightly says did rise enormously and didn't lead to inflation is the quantity of reserves; and that variable doesn't appear in the money stock, according to the core monetarist literature.

JOHN COCHRANE: Let me briefly address both points, and Marty may want to respond too. The standard New Keynesian model, including Rotenberg and Woodford, generates a negative response to a transitory interest rate. Rotenberg and Woodford

7. Julio J. Rotemberg and Michael Woodford, "An Optimization-Based Econometric Framework for the Evaluation of Monetary Policy," *NBER Macroeconomics Annual* 12 (1997).

do not look at permanent interest rate changes. The standard model also needs to pair a monetary policy shock with a change in fiscal policy, so that you get that jump-down in inflation. If that "passive" change in fiscal policy doesn't happen, the deflation is not going to happen either.

The issue at hand is not whether M2 times V equals PY. The issue at hand is, Are we in a liquidity trap? Can the Fed accomplish anything by running open market operations and increasing reserves? If your view is that we didn't get inflation because reserves didn't leak out into M2, that's exactly my point. The reserves don't leak out into M2, so open market operations don't do anything when you're paying interest on money.

MARTIN EICHENBAUM: I'll repeat that the standard New Keynesian model has no problem getting the nominal interest rate and inflation to move in opposite directions after relatively transitory policy shocks. For persistent shocks, you get exactly what you'd expect. The nominal interest rate and inflation move in the same direction. After all, a permanent shock is the same as a rise in the nominal interest rate target that appears in the Taylor rule. And that kind of change moves the nominal interest rate and inflation by the same amount and in the same direction.

JOHN COCHRANE: But Marty, this does cause a bit of a problem for the standard interpretation of the 1980s, where the standard story is that persistently high interest rates drove inflation down.

MARTIN EICHENBAUM: Larry Christiano points out in his discussion of your *Macroeconomics Annual* paper that Andy Levin has a brilliant paper with Chris Erceg in the 2003 *JME*, which argues that if it took time for agents to believe that the Volcker disinflation was credible, then nominal interest rates and inflation would move in opposite directions during the transition period.[8] This seems plausible in light of the historical record.

8. Christopher Erceg and Andrew Levin, "Imperfect Credibility and Inflation Persistence," *Monetary Economics* 50 (May 2003): 915–44.

JOHN COCHRANE: So you're saying that monetary policy inherently relies on people being systematically irrational. I'd love to see Janet Yellen go to Congress and say, "Look, this is all a confidence game; we cleverly exploit the irrationality of the American people."

MARTIN EICHENBAUM: No, I'm saying that the Volcker disinflation wasn't instantly credible. A wise agent would not have instantly assumed that it was.

JOHN COCHRANE: The only way this thing works is if people are too dumb to know what's happening.

MARTIN EICHENBAUM: Let's be clear. The Erceg-Levin paper assumes that agents have rational expectations.

RICHARD CLARIDA: I enjoyed this session a lot. I actually learned from both. I'm glad you did the point-counterpoint, because I learned a lot. I want to piggyback off something Marty said and reinforce. One thing I've noticed is if you take the Laubach and Williams estimate of r-star, which is done without any reference to financial asset prices, and plot the forward Treasury inflation-protected securities yields, which I've done, it's striking how they come together. I'm not saying the TIPS market's right or Laubach and Williams are right, but I think it is revealing that you had this break in the TIPS implied forward yields at about the same time and the same magnitude as Laubach and Williams.

The second point, which I think is relevant to this discussion, is that it's always convenient to work in silos, but in fact there is a global dimension to real rates, both theoretically and empirically. And we can go back and forth as to whether it's the US factor or a global factor, but empirically, it's a very robust effect. So we don't have 180 countries with a 180 real rates. We've got one factor that's 90 percent of it. But I think that's relevant to stabilization policy, because we've essentially had a global decline in real rates now, and that impacts how much you get in currencies and how much other policies can do.

And the third thing, which is a bit more cynical but relevant to the policy discussion, is that even though I'm in the camp that thinks time-varying r-star is important for policy, it does open up a communications challenge for central banks compared to the world where r-star is a constant, because you get into this potential issue of a central bank that wants to run a very, very gradual, behind-the-curve policy, say, "Well, r-star is low, and we're just following r-star up." And since it's unobservable, or at least poorly measurable, it can complicate communication. But as a practical matter, I think time-variation r-star is very relevant, and there's a global piece as well.

KENNETH JUDD: First, I must say that I'm sure Michelson, Morley, and Einstein are all spinning in their graves. Second, to Marty. You talk about uniquely learnable equilibriums in these dynamic models. I remember Sargent some years ago had some work on learning, where the learning process, I think, created novel dynamics that were more complex than the simple rational expectations equilibrium. I also know that there are many, many learning rules you can use. Are you saying there exists a learning rule that gives you uniqueness, or is this true for all learning rules? Anyway, the claim struck me as surprising.

MARTIN EICHENBAUM: Let me clarify very briefly. I'm not making a global claim with respect to all possible learning rules. I am considering a particular class of learning rules. Within that class, the learning equilibriums converge to a particular rational expectations equilibrium.

JOHN COCHRANE: I have a paper that shows the opposite: the standard New Keynesian model is not learnable, and the fiscal theory model is learnable.

MARTIN EICHENBAUM: That's a different definition of learning. Agents are learning about something entirely different in your paper.

JOHN COCHRANE: There are other learning criteria in which it goes the other way.

MARTIN EICHENBAUM: Nope. The word is the same, but it refers to a different thing.

JAMES BULLARD: Marty's right: you should take learning very seriously. But I just wanted to get a clarification, because this is a little different discussion of neo-Fisherian effects. You're saying you agree that if there's a temporary move in the policy rate, all the other impulse responses we have looked at over the years are right. But if there's a permanent move, then I guess you agree with this, Marty, that you will get higher or lower inflation, depending on which way it goes?

MARTIN EICHENBAUM: Of course. Absolutely.

JAMES BULLARD: So would you say that in Japan, according to our models, what dragged inflation lower is the move from a high to a persistently low nominal interest rate?

MARTIN EICHENBAUM: I'm not an expert on Japan, and I don't pretend to understand all that's going on there. But there are lots of real models that can generate low real interest rates in Japan. Those models rely on low fertility rates, an aging population, and low growth rates of total factor productivity. Granted there's a tension about open economy versus closed economy issues that Rich Clarida points out. But if r-star permanently fell by a lot, the New Keynesian model wouldn't have a problem in generating low inflation and low nominal interest rates for Japan.