

# General Discussion: Inflation During and After the Zero Lower Bound

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*Chair: Erica L. Groshen*

**Mr. Spriggs:** I want to tie your paper with the previous paper because I thought some of what Jon Faust suggested didn't get discussed. In your paper, if we had a higher inflation target, one key thing that happens is there's a quicker bounce-back because you're allowing the economy to re-inflate more rapidly. The previous paper said there are many other things that matter and, I think, one of those things is how quickly we bounce back. So, the amount of time it takes us to get back to previous levels of employment, not unemployment in the U.S., has been going up. So, early in the Great Moderation, we ratcheted up unemployment rates after each recovery. In each successive recovery, we ended at high unemployment rates. Now, we've switched to each successive recovery takes longer. The length of the recovery now has real effects because the length of the time it takes to bounce back no longer is so short term that you might think workers wouldn't experience anything (loss of savings, deterioration of skills or diminished quality of job networks). We're now at five and six years before we get back to the *number* of jobs, not the employment population ratio, just the *number* of jobs. That's a generation of workers. This is their initial entry into the labor market, and it's affected an entire generation. If we don't bounce back quickly, take the current downturn, there was a great

conference last year here in trying to understand the drop in labor force participation. But understand that what we have done is we have ratcheted down the labor force participation rate for women in the United States. It's now equal to that in Japan. I don't think it's easy to think of that as just a cultural thing in the United States, that we've reached some cultural equilibrium. I think it is an economic problem with wages for women. We have ratcheted down the participation of young people in the United States. What we have ratcheted down is the size and the growth of our labor force. We have ratcheted down our potential output. I think the question about looking at long-term trends becomes important, because if you don't bounce back quickly, then each time you ratchet the economy down—we've ratcheted wages down, down, down, down—so, unless there's some policy space for wages to recover, as the previous paper was pointing out, many different things that have been going on, like the declining labor share of income will continue to get smaller. Labor share getting smaller has the direct implication that wages must be going down for a broader spectrum. And the BLS, in its monthly labor report last time, pointed this out for us. Seventy-five percent of the wage distribution is still below real wages at the peak. So, could you talk a little more about the implications of changing the target broader to just the zero bound problem, but to what it means to recovery time and what it means to *other* parts of the economy coming back to normal?

**Mr. Dotsey:** As many of you know, I'm pretty invested in the use of these DSG models and I particularly like the use of the DSG methodology for the questions that Bora Aruoba and Frank Schorfheide are trying to answer. I think this is a nice paper. But I want to sort of echo Lucrezia Reichlin's last comment. A lot of these estimated models seem to prefer indexation in the estimation. But that's just not in the micro data, and I think that's just probably soaking up some misspecification somewhere else in the model. But given how important indexation is in influencing the welfare costs of inflation, for the experiments that you're doing, it might be better not to rely on it and not use it as your benchmark. I think that might give us more reasonable results.

**Mr. Ito:** For the experience of Japan and the behavior of Bank of Japan, I would say that during the deflation time until the QQE was introduced, there was a shift, I think the natural experiment, of the inflation target moving from zero between 1999 to 2012. The behavior and communication gave the impression the BOJ was actually targeting zero. Then, the government forced to adopt 1 percent in January 2012, and 2 percent—this was under different governments—in January 2013, and QQE was introduced in April 2013. So, the intermediate regime of 1 percent, let's forget that, that was a shift from the zero target and no QQE to a 2 percent target and QQE with commitment. So, I think this will be studied, that changing the target would have a significant effect on that. Second is a very quick question. In your handout (Chart 6 in the Arouba/Schorfheide paper), there are two equilibrium, A and B, and the question is whether the deflationary equilibrium is a deflationary trap, meaning that it's a locally stable equilibrium or not. And if I see this kind of graph, I see one of the equilibrium is locally stable, the other one is locally unstable. But we tend to think that the deflationary trap in Japan was self-enforcing, that people have the expectation and you tend to stay there, and little deviation doesn't make an escape. You need a big push to jump from one equilibrium to another. But does your model have that feature that I just mentioned?

**Ms. Mann:** There are many people in this room who will remember this, but I want to take this opportunity because there are so many central banks in the room, to mention a project undertaken by central banks in the late 1980s and early 1990s. It brings together the topic mentioned yesterday by Athanasios Orphanides, the Leeper-Faust paper and the paper that's presented here. That was a multiyear project spearheaded by Ralph Bryant, who was originally from the International Finance Division at the Board of Governors and then ultimately pursued this at the Brookings Institution, and it was a model evaluation project. Many in the Fed and other central banks came with their models, and some of them were forward-looking, some of them were backward-looking, DSGE didn't exist at the time, but today you get the DSGEs in here. The project was to do model evaluation exercises. We took all the models, we were very clear about how the models were similar, how they were different, and we put

rules in, simple rules, discretionary rules, you could have rules with additional secular issues with regard to a trend or a wage share or labor force participation. Then you examine the robustness of achieving objectives against different kinds of shocks. So, that even brings in the first paper yesterday where we had differences in financial shocks versus demand shocks. We could throw a technology shock in there. We could test, and we did test at the time dynamics of adjustment. How long did it take to return to the steady state? What were the implications? You could test symmetry around your targets. I guess what I'm calling for is the central banks in the room, and the OECD to do a model evaluation project like this again. It generated a greater understanding about how rules interacted with objectives and with the rate, the characteristics of economies, and the intersection of interests among the central bank community really brought this together. It was a valuable exercise in understanding the way the economy works, the way models work, and it produced these huge volumes—they were kind of doorstops. But I think it's a way of bringing together all of the topics that have come up in the past two days under different kinds of guises. It's an umbrella project that could be undertaken by the central banks in the room.

**Mr. Hess:** In the model presented, the target inflation rate generates welfare consequences primarily and perhaps exclusively through their probability of hitting the zero lower bound. I think that's been hinted at by some others. Indeed, 6 percent might be a better target than 4 percent in their model, so as to not hit the zero lower bound. However, the paper also cites other work suggesting that there are other broader costs to higher levels of inflation. However, since we're at a policy conference, I'd like the authors to articulate what their policy recommendation is for the appropriate inflation target for a central bank.

**Mr. Blinder:** Two very short questions relating to the extreme austerity of these small-scale DSG models. The first is just a technical question. What if there is no zero lower bound? That takes us back to what we were discussing yesterday. What if in fact you can push below zero—although for reasons that won't be in this model, it gets harder and harder as you push below zero? But there isn't any hard

lower bound; it's sort of smooth there. I'm thinking that the jumping equilibria disappear, but I'm not positive that's right.

Second question: Frank, this has to do with the symmetry of being a little bit above zero or a little bit below zero on the actual inflation rate. The model again leaves out the main reason or reasons why most economists don't think it's symmetric. If Irving Fisher or Ben Bernanke were in the room, they would raise their hand and say something about debt deflation, for example, and bankruptcies when debtors can't pay off their debt, which is different qualitatively from creditors not getting as much back as they thought they would get.

**Mr. Bullard:** I'm going to talk about Chart 6 of the Arouba Schorfheide paper, which has the A and B equilibrium. Elementary consideration from Jess Benhabib, Stephanie Schmitt-Grohé and Martin Uribe suggests that the second steady state is a low-inflation, low-nominal-interest-rate steady state. The thing about this is that it would exist in nearly all models because all you need to get this second steady state is a Fisher relation, which I presume most of you have, a Taylor rule for monetary policy that responds to inflation and the zero lower bound. Or, if you don't like the zero lower bound—like Alan Blinder—you could move it down a little bit. I think you'd still get a second steady state. Arguably, Japan has converged to this B steady state. You can argue about it, but 20 years of zero interest rate policy have not raised inflation very high in Japan. So, a key policy question is: Will Europe and the United States follow Japan to point B in this diagram and miss the inflation target forever? This would be an absorbing state. You're talking about a steady state of a model. The reaction of the policymaking community to this has been to ignore this. But I think it's been one of the top issues in the last five years. I agree with the authors. I like the analysis. They're the top authors as far as trying to estimate this—that the United States is not likely to converge to this point B—although on this issue I would say by the time we have empirical evidence that we have converged to point B, it is going to be way too late. So, you've got to be thinking ahead. I do think we're going to be back at A. I will say on the paper, I'm not sure that raising the inflation target,  $\pi^*$ , is the most natural experiment here. If you just look at this diagram, point A and point B, raising

$\pi^*$  means you're going to move point A further to the right in this diagram, farther and farther away from wherever you are today in this diagram and whatever the dynamics are around these two steady states. So, by moving the targeted equilibrium farther away, roughly speaking, would mean you're less likely to converge to that steady state and more likely to converge to the B steady state. So, at least for this problem and this issue, I don't think raising the inflation target is a natural experiment. For those that are gluttons for punishment, I gave a recent speech on neo-Fisherianism. It's on my Web page, and I talk about some of these issues, some of the data, and my current thinking on this issue. So, I think it's a very nice paper.

**Mr. Aruoba:** Thank you very much to the organizers and everyone here for these great comments. We should start by acknowledging this is a stylized model. It's still somewhat richer than most stylized models, but it's still a stylized model. There's significant uncertainty associated with the things that we say, but we always would like to say, given our model, this is what we find, and we may discuss the qualities of our model. Let me reiterate a few things about our conclusions for Japan. Again, emphasizing that there's uncertainty and we don't have very crystal clear results. The comparison with the United States is quite stark, and that makes us more comfortable with the statements we make. In our companion paper, what we report, at the expense of being a little technical, is this probability of being in one regime versus the other. And that one, unlike the static contour plots that we report here, takes into account the dynamics—the dynamic nature of the relationships—as well as the correlations between variables that we observe. And these two regimes have different implications for these correlations. We are able to distinguish these regimes using these. We find in 1999 there was a significant decline in the probability of being in the targeted inflation regime for Japan. That probability remains very low, sometimes at zero, and sometimes somewhere between zero and 1, but throughout our sample, until 2013 or so, it remains significantly less than 1; unlike the United States, where during the financial crisis there's a little bit of movement. But since then, it's straight at 1. So, that's sort of the stark difference. I'm glad you mentioned 2013. We do see some

of the change in this probability that we extract starting in early in this decade, and that certainly sort of corroborating evidence like you mentioned. So, the 4 percent exercise. It wasn't meant to be, as James Bullard said, a way of getting rid of the deflationary steady state. As he said, it's there in any model that has those three ingredients. What we wanted to do with this exercise was to see if we can improve the outcome for the United States, which was in the targeted inflation regime, by adopting this 4 percent target either going back in history to 1984, or doing it midway in the ZLB episode in 2014. Having said that, Michael Dotsey and Lucrezia Reichlin and others talked about the indexation, so we wanted to give this 4 percent policy its best chance to come up as a good policy. That's why we allowed the price-setters index immediately to 4 percent; this was a credible change by the central bank that everyone accepted. So, if that wasn't the case, even then we found very little support for this type of a policy change. Of course, if you didn't have this indexation and other issues like credibility, then it would be even worse. We wanted to give this policy its best chance.

On your question about if the zero lower bound wasn't a constraint, then this is not an issue. It only becomes an issue as we see in that graph because of the intersection of these lines and the flat part due to the zero lower bound. Certainly, our model extracts from these other costs of deflation that you mentioned, which is important. We try in the paper not to make any clear distinct normative statements. We simply wanted to put, "given our model," which is missing a lot of things, what the various channels are and what we can end up finding with those channels. There are many other channels that we are missing.

**Mr. Schorfheide:** In closing I would like to say that we certainly don't think that all monetary policy statements are sunspots and therefore do not convey information about fundamentals. Whenever you have a model which can generate multiple outcomes, it means there is a mechanism missing in the model that ultimately predicts which outcome will be selected. Our point is that statements made by central banks can possibly influence the selection of these outcomes.

