

General Discussion: Riders on the Storm

Chair: Janice C. Eberly

Mr. J. Taylor: I just want to support what Kristin Forbes said on monetary policy and the exchange rate, which I think is a big issue. If you look at divergence between countries, the effort to stabilize the exchange rate is huge. When you talk to central bankers, they talk about that and when you look at their reaction, it's there. That's just the actual interest rate, and I think it's a little too much to prove from a normative perspective, but it's really there. Also, the same thing is true for unconventional policy. You can see real exchange rate responses, real exchange rate effects, so I think it's another thing in support of the need to go beyond the interest rates to see this divergence.

Mr. Carstens: I enjoyed very much the paper. I have a couple of questions. The key aspect of the paper's argument relies on the decomposition of the evolution of real interest rates into a cyclical component and a trend component. The key finding is the identification of the stark dichotomy between short-run fluctuations in interest rates, which are viewed as driven by monetary policy, and medium-run fluctuations that are governed by forces strictly beyond monetary control. This leaves no room for the potential persistent influence that monetary policy would have on the economy—the role of policy from a medium-term perspective is simply a sideshow. But shouldn't we question at least to some extent this dichotomy, given the dominating role

that monetary policy has played during the last 10 years, as Chairman Powell commented during his speech? And then my less important question is I didn't get why you called the paper "Riders on the Storm." I know the lyrics of that song and it doesn't have anything to do with what we were discussing. So, I am just curious.

Mr. Gourinchas: I thought this was a very rich and very interesting paper. But I have a few comments. I will skip the nautical comments. The first thing is if I look at the main figure, Charts 2 and 3 in the paper that plot your estimate of r^* compared to the actual short rate, what really strikes me is the extent to which they track each other. In fact, your estimates tell us, "Well r^* is just very, very close to what the actual rate happened to be throughout the period." And so I want to sort of turn things around and say, "Well, central bankers have been doing an amazing job, if that is the real r^* , of figuring out where it is and sort of nailing it and being very close to it." Of course, that conditions a number of your results in the sense that once r and r^* are very, very close, what is called the stance cannot play much of a role; there cannot be much of a deviation. And that's going to sort of percolate or trickle down all your results. So that brings back a point that Kristin, I think, rightly emphasized which is we need to understand perhaps better where this proximity between r and r^* is coming from in your estimation.

A second point I think that I picked up from Kristin, which I think is also right, is "where do emerging markets fit into this?" And as I think Kristin hinted at, we would expect a world r^* to include emerging markets and would presumably be higher than the world r^* estimated just on the large four countries that you have. But then it brings into question an old puzzle: your results show that when a country's r^* is above the world r^* , then that country tends to attract capital and to run a current account deficit, as theory would suggest and as you emphasized. But then if the emerging market's r^* is above your estimate of the world r^* for the large four countries, then we should also expect capital to flow to these countries, and current accounts deficits in these countries, which we don't observe. That's sort of an old puzzle of capital not flowing the right way. I want to raise this to say that your estimates seem to be explaining some patterns,

but they don't cover the whole range. And I think that the question of where emerging markets fit into this is important. The last point very quickly, is on the term premium. To the extent that a number of these countries have been conducting nonconventional monetary policy that includes purchases of long-term bonds, you might expect that the term premium in these countries might actually be declining even if the equity risk premium and other asset classes risk premia might not. And I sort of wonder if you have some views on that.

Mr. Jordà: It's truly an honor to be here and thank you so much to the organizers. And Kristin's comments were really educational for me. I think she's right to bring up some of the issues, especially the fact that we've restricted our analysis of spillovers to essentially r^* spillovers. But she's completely correct that, of course, the stance spillovers also matter, and she has provided a really clever way of analyzing those and creating a measure for those.

Let me take general issue with measurement because it's a topic I've heard several times in reference to r^* —that r^* is difficult to measure and therefore perhaps we shouldn't pay as much attention. I would argue that, in fact, all the variables that a policymaker uses are r^* variables. Just think about the employment report that was revised two days ago. We learned that there were 500,000 fewer jobs created last year than initially reported. On top of that, the data that you get from the unemployment report in a given month is actually not data. It's looking through seasonality, to get the pattern of the data itself. So, I don't know that the uncertainty that we have about measurements of employment, about measurements of inflation, about measurements of GDP really are that different from the uncertainty that we suffer in measuring r^* . To me, it seems that it's just part of the job. Part of the job is to deal with uncertainty, and this is just one more variable that we have to deal with.

The takeaway that I hope would come out of the paper that's gotten a little diffused is that there's a great deal in our view of what goes on in terms of policymaking that is about really tracking the underlying forces of the economy domestically and internationally. There's not a whole lot that the central bank can do. It can fiddle around the edges, but those trends that were described in one of the comments

as medium-run trends to longer-run trends, they're just powerful and there's not much the central bank can do.

I think a lot of the suggestions make perfect sense to me, such as including emerging markets data and extending the analysis in a number of dimensions. Of course, we were limited by time constraints. So, thank you. Those are great comments.

Mr. A. Taylor: Well, maybe I'll reply to a few other things that Oscar Jordà didn't cover. Unconventional policies, and should QE be in there. I think when we started out we wanted to try to use the historical data and maybe ask some comparative questions, but what we didn't want to do was change the Laubach and Williams frame by using radically different —terms of modeling in case, you know, you all raised your hand and said, "Well, you've messed with the model and now you're getting a different answer." So, we were cleaving pretty strongly to the standard setup for comparability. But I think it would be interesting to see if you added in measures of the magnitude of QE, does it also drive changes in interest rates and the natural rates at different points along the curve? The intuition might be that that is having an effect, in fact, in terms of dragging slope-star down perhaps. You know, you've only got 10 years of data where that's a factor. It doesn't affect the first 50 years of our sample. But it could be interesting. Is inversion a signal now, or are we just biased toward greater inversions just because in a world of QE you've pulled down the long end so much that you're going to get inversion signals much more often and there will be many more false positives. So, I think that would be an interesting extension to the exercise.

Pierre-Olivier Gourinchas is absolutely right on emerging markets being a different animal. There is a fantastic paper called, "The Allocation Puzzle," which Pierre-Olivier knows very well, because he wrote it, and the flows go the wrong way there, and perhaps deliberately we couched our analysis in terms of the advanced economies perhaps to sidestep that question of whether the r^* mechanisms on average or even dynamically work the same way in EM. So that's a fair point.

And then lastly, Agustín: does monetary policy actually have real effects that could have undermined all of this analysis for everyone doing it? We touched on that in the concluding caveats. There are views out there that monetary policy does affect r^* and its determinates like growth and TFP through various channels. The view in the BIS may be more that you run loose policy, it makes interest rates just go lower and lower, and there you go, and with feedback it persists. The view we found in our recent research with Sanjay Singh actually goes against the BIS view. (Sorry.) Which is that you run tight policy, excessively tight policy, you destroy TFP with a hysteresis effect. That has a medium-term “permanent” effect. And so, 10 years out you’re at a lower level of TFP and you’ve got a lower g and, hence, a lower r^* . But we would agree that more research is needed.

Mr. Obstfeld: Actually, let me echo that this is a very ambitious and very informative paper. I really enjoyed reading it. You know, just two comments. One thing that has troubled me about a lot of this literature is that, particularly in the multicountry framework, you take a group of economies and model them as unconnected, and for each of these economies, you estimate its r^* . And then it turns out that these r^* s are pretty well correlated, they trend together. And so, people look at that and they say, “Well, that’s all good and fine. This is what I expected.” But basically, this is happening because with integrated world capital markets and integrated trade, these aggregates tend to be pretty highly correlated. So, you’re not starting from a framework that imposes capital market integration. You’re taking data that is driven by capital market integration and estimating r^* and it ends up looking OK, but it may be that the underlying model is missing something. There’s a really ambitious paper by Barro and Sala-i-Martin from around 1990, which tried to estimate global saving, global investment, and equate them to get r^* . We probably wouldn’t use the same methodology today, but that’s an example of how you could try to do this. Now, in such a model, of course, you would have to take into account emerging markets, and this would be difficult because of the nature of the capital transfer mechanism being not as frictionless as between advanced economies.

The other point I wanted to make, and this has been touched on by Kristin and John Taylor. We moved to the current system of exchange rates from the Bretton Woods system because we wanted to restore some measure of autonomy for monetary policy. And you see in the real interest parity condition that what is driving the divergences between r^* s in different countries is some measure of expected change in the equilibrium exchange rate, as well as term premia. We don't know that much about how policy affects term premia. So, in principle, these divergences are closely related to exchange rates, which doesn't really come through in the paper. But as John said, if central banks are going to target exchange rates or have fear of floating, then there's going to be much less scope for divergence. But we shouldn't be surprised about some divergence because the system of floating exchange rates is designed to deliver some of that. Of course, how much of that we see will depend on the particular economy that we're looking at.

Mr. Ferguson: I join others in commenting positively on the paper and the comments. I'd like to take us to the point that Alan Taylor made in passing and I think Oscar picked out which is this notion that r^* reflects a number of factors, and we see a general decline in r^* over a period of time, which suggests that maybe we're going back to something that predated a little bit of what we all experienced a few years ago. We're not really at a new normal. The question on my mind is twofold. One is, given how difficult it is to have divergence for reasons that Maury Obstfeld and others have talked about and Kristin talked about with capital flows, and given the fact that r^* globally seems so low, are we going to be in a situation going forward where we tend to have broadly convergent trends, global forces, driving cycles? And secondly, with interest rates so low, are we at a place where financial stability becomes much more important as one of the big issues that central banks have to worry about? Because we have this cycle going on of very, very low interest rates, risks of overleverage, etc. That is a thing that really drives the sort of global trends back and forth, and consequently makes the concept of central banking that much more challenging for all of the emerging and all of the developed market central banks.

Mr. Fischer: It's a very interesting paper. I'm trying to work out, is there any optimality results for countries doing anything in this model? And if there is, who's misbehaving the worst of the four countries? We haven't got China in yet. And what will we use this model for? Is it to tell country X you're way out of line? And then with what? I can't quite figure out how to think through what these results are about.

Mr. Leduc: I was wondering if the role you find for international factors is not in fact a lower bound? I'm saying this because the model you used to filter out the domestic r^* 's is a closed economy one that abstracts from all the interaction across countries, all the flows and capital or trade that would come to impact your domestic gaps and then your domestic policy stance. In that sense, it underemphasizes all the international channels and factors.

Mr. Jorda: I think those are great comments. Maury, it looks like we need to write a paper together. I completely agree with what you said. And the general tenor of the four comments that came about is that there are several limitations to our framework. One, we don't have an optimal response as Stan was asking us about and we're very silent about that. We don't know exactly what that is, and we don't know how that optimal response should incorporate international dimensions of the model which are currently absent, as Sylvain Leduc was pointing out. And I think Roger Ferguson was exactly right. In a world of low r^* , things like fiscal policy and financial stability considerations, the former not being in the hands of the central bank, but the latter clearly is, those matter quite a bit more. So, there's a lot of road to be traveled with our paper. I think that's completely correct.

Mr. A. Taylor: I'm going to add to that except I think I'm going to disappoint Stan Fischer. Whenever I talk to Stan, I feel I disappoint Stan. But seriously, purposefully we tried to avoid putting a policy rule, or anything about optimality and choice, into the setup, partly as pragmatic because our data set overlaps the Bretton Woods period, countries are pegged, there's little capital mobility, and then later on we've got countries then floating and higher capital mobility. If you kind of imposed a uniform policy rule across 50-60 years of data, that might be a bit of a stretch. So, we just left that out. It could easily be added but then you'd be saying where do those coefficients come

from, how do we restrict them, what do they mean, and there'd be a danger of being kind of tautological there of just saying, well it's like you'll just get a revealed preference sense of the, you know, well Germany wanted to have those weights on their policy rule and the United States wanted that. If we had China in there, it would be like, oh China just wants to peg. And then we'd say, well China is doing what it needs to do to peg. Out there, the bears in the woods may not be particularly satisfied with that answer. So, I don't know exactly what traction we would get out of that. But I think it's the right question to ask, but it's tough to answer.

Mr. Blinder: This is a really excellent paper, and I hope this is not just picking at nits. You tell me if you think it is. But I'm wondering about the notion that an economy as big as the United States, and financially we're even bigger relative to the world than our GDP share, is actually a price taker in financial markets. That is, you've got this thing at the end, the world real interest rates, and the maintained assumption is that's impervious to, say, Federal Reserve policy or U.S. policy in general. We don't usually think of the United States as a small open economy. I'm just wondering what you think about that.

Mr. Shambaugh: I also thought this was a great paper and a terrific discussion. I worry a little that the way we're measuring it at the end, we're underestimating how much policy divergence there is for two reasons. First, is this point a couple of people have raised that we've got the world r^* is the average of four things, and then we point out that those four things move with their average. And so, it feels like, by broadening the number of countries in the world r^* , you might see more divergence in r^* s from the world r^* , and so it may look a little less like they just move with the world. But then also, when we focus on stance divergence, it feels like if everybody was doing r equals r^* you get zero stance divergence. But you could have huge monetary policy divergence in the sense that some countries r^* could be 5 and someone else's could be -1. And so, I worry a little bit that you see the divergence basically when they're doing something other than following r^* . So, the biggest divergence point is the MS crisis. So, when they're not focused on the domestic conditions you get

a big divergence. But if everyone focuses on their conditions, they could still have huge divergence, just not in this measurement.

Ms. Reinhart: I very much enjoyed the paper and the discussion. This follows up on some of the earlier comments on the fewer floating. One has to wonder whether central banks, advanced economies included, are now really doing with interest rates what used to be done with foreign exchange reserves during the Bretton Woods era, and that can also help explain why there's so much co-movement across the r s, whether they're r^* s or just observed r s. If one looks, and this is very consistent with a point that Alan made in his presentation, that the interest rate behavior of the 1980s to some degree is the anomalous. The exchange rate vitality also, if one looks at the major currencies, exchange rate volatility was quite high in that period. It has since secularly been declining, which is consistent also with perhaps the reason the r^* s are drifting together, is an unwillingness to tolerate more exchange rate volatility rather than some of the factors that you stress.

Mr. Henry: It's a very interesting paper. I have no problem believing that r^* has fallen, but I have a hard time understanding what we really mean by negative r^* . And I think we're mixing apples and oranges just a little bit in thinking about policy rates versus the real rate of return of capital. Because if the real rate of return on capital is actually negative, we should be seeing decapitalization across the world and we don't see that. So that seems to me that that's not what's going on. So, the question then is, why are we not thinking more about what is the optimal monetary policy stance—that is the appropriate level of the nominal interest rate for the prevailing level of expected inflation—given corporations' prospective return on new investment, going back to Fischer Merton? What we really want to get at is where is the policy interest rate relative to the real rate of return on capital? Why aren't we thinking about things like the earnings price ratio? It's an imperfect proxy for the underlying real rate of return on capital, but it seems to me it's closer to the r^* conceptually that we're trying to capture than what's been estimated here. And on that point, I think Kristin is fundamentally right. We have to think about emerging markets in this context. And to the point about the

allocation puzzle, it's certainly true that on average, capital does not flow to emerging markets. But in specific markets, where the mechanism to actually allow capital to flow exists, emerging equity markets, for instance, we know that there is not an allocation puzzle because capital actually flows from countries that remove restrictions. So, I think there's a lot of heterogeneity in what's being measured, and I think we need to be clear about what it is we're trying to get at, and what r^* is actually measuring.

Mr. A. Taylor: Let me respond to Peter Henry first. I think you're absolutely right that there's a world of difference between the rate of return on risky assets versus the rate of return on safe assets, and some of our other work (e.g., "The Rate of Return on Everything" paper) has highlighted that r is much bigger than g if we're talking about the risky kind of r , whereas r is close to g or sometimes less than g , if you're talking about the safe kind of r . And that's kind of a configuration that we're in today and maybe it's become more stark even in the last 10-20 years. We can discuss why that is. I've got some ongoing work with a former student exploring the role of retirement and demography effects. If you've got a lot of people who want their pensions and their fixed income stream, their annuity or whatever, and that number is piling up, it's probably going to tilt those rates of return. I think there's a story to be told there. But we're in the company of central bankers here, so we're speaking to the kinds of things primarily that central bankers can influence, the safe yields, which is why we're looking at this particular curve and this particular set of assets. But absolutely fair point though. Even then, greater mystery why with those big spreads between safe and risky returns, why isn't investment just going nuts? You can maybe help explain that to me in the coffee break because that's my puzzle.

And maybe I'll refer to Jay Shambaugh's question. Are we underestimating the policy divergence? Yes, we probably should raise the number of countries in the sample. Maybe that can be what we do on the next go-around. But this is kind of an exculpatory paper to say, you know, just central bankers can't help it. If your r^* moves, you then respond, and this again alludes a little bit to the question Stan was raising. I mean, it's not quite a statement of optimality point by

point, but on average, you're going to be pretty close to your local r^* plus inflation target kind of whether you like it or not. And so, if that's moving around, is that something you want to call policy divergence or is that just r^* divergence and countries can end up with different r^* s. I guess our point would be, modulo Agustín Carstens' concerns and our concerns, that the policymakers could screw up and move r^* around in an unintended way. But it's really the stance part that you can only kind of put at the feet of the policymaker.

