

**Concluding Remarks<sup>1</sup>**  
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The task of the closing discussant is to identify a unifying theme in the diverse papers presented over the two preceding days. My unifying theme this afternoon will be the Asian financial crisis. 2017 marks the 20<sup>th</sup> anniversary of the crisis, an event that did much to shape the regional and international financial architecture we know today. The papers presented to this conference thus afford us an opportunity to reflect on this legacy. The papers also illustrate how the focus of research has shifted since the crisis. They illustrate how the methods used by economic researchers, specifically those concerned with regional and global financial issues, have evolved over two decades. This points to what I will do in the remainder of my remarks, namely to compare the research presented here with the state of the art 20 years ago.

Consider for example the work of Stefan Advjiev and Galina Hale. This is one of several papers concerned with the cross-border repercussions of U.S. monetary policy. This is not a new topic; in fact, movements in the yen/dollar exchange rate were widely implicated as contributing to the Asian financial crisis in 1997-8. But the mechanism then was different. Asian countries pegged to the dollar, so a stronger dollar translated into a loss of export competitiveness. That Asian countries continue to feel the effects of U.S. monetary policy shocks today, even in the absence of exchange rate pegs, is consistent with Helene Rey's thesis of "dilemma, not trilemma," and more generally with models in which the insulating properties of flexible exchange rates are incomplete.

Advjiev and Hale, in contrast to the emphasis in the 1990s on the impact of exchange rates and monetary policies on export competitiveness, consider the impact of U.S. monetary policy on cross-border bank lending. Economists and others writing in the 1990s did not entirely neglect the impact of U.S. monetary policy on international lending, but the Advjiev-Hale paper is a reminder that there has been a sharp shift in emphasis from the current account to the capital account. Advjiev and Hale also show that the relationship between macroeconomic fundamentals and lending to emerging markets varies with the global financial cycle – it differs between "boom" and "bust" periods. In particular, the authors show that in periods of an appreciating U.S. dollar, the flight-to-quality effect and bank lending channel are especially powerful. This is similar to what some observers thought they detected in the run-up to the Asian crisis, though the authors document the phenomenon more thoroughly using 40 years of data for 114 countries on cross-border bank lending from the BIS (an empirical achievement that would have been beyond the analytical and computational capacity of their predecessors 20 years ago).

The paper by Matteo Iacovietto and Gaston Navarro addresses closely related issues. Inspired in part by the "taper tantrum" in 2013, Iacovietto and Navarro analyze the transmission of higher U.S. interest rates to emerging markets. They too allow the impact to vary with economic and financial conditions. Their treatment highlights the contrast between the advanced

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countries, where transmission depends on trade links with the United States, and emerging markets, where it is more heavily a function of local financial structure. This paper too is consistent with the tendency to place less emphasis on trade links and more on financial conditions. Again, economists 20 years ago would have been impressed by the amount of data the authors are able to mobilize and synthesize, in their case quarterly time series for more than 50 economies spanning more than 50 years.

Another legacy of the Asian financial crisis was a debate, still raging today, over the efficacy of capital controls. Kaplan and Rodrik (2001) famously argued that Malaysia suffered a milder recession than its neighbors because of its resort to controls. It asked the IMF to reconsider the efficacy of controls in light of First Deputy Managing Director Stanley Fischer's earlier proposal that capital account convertibility should be made an obligation of IMF members. The resulting paper for the Board (Eichengreen and Mussa 1998) suggested that capital controls should be seen as a second-best form of macroprudential policy, where first-best policy was capital, liquidity and other regulation directly affecting the stability of the banking and financial system.

Mick Devereaux and his coauthors provide a formally-grounded analysis of these issues. They present capital controls as an alternative to raising interest rates in a crisis. This is in the spirit of Kaplan and Rodrik, who concluded that Malaysia experienced a shallower recession because of a less draconian monetary response to capital outflows. Devereaux et al. show that, under discretion, policy makers will hesitate to regulate capital inflows prior to a crisis (they will not use capital controls to lean against the wind); rather, they will resort to them in the crisis itself. This in fact is what Malaysia did, although it taxed outflows rather than inflows. The authors' exact result is a bit perplexing, as the discussants noted: just why a government should want to tax capital inflows just when capital inflows become hard to access is not entirely intuitive. (The result, as I understand it, is a function of the exact specification of the collateral constraint responsible for the financial friction.) Under commitment, in contrast, optimal policy involves imposing capital inflows taxes prior to the crisis and then inflow subsidies (equivalently, outflow taxes) in the crisis itself. The conclusion, as I infer it, is that the Asian crisis was caused, at least in part, by the inability of Asian policy makers, beholden to various constituencies, to commit to time-consistent policies.

Alexandra Tabova and her coauthors focus on capital flows in the other direction, from emerging markets to the United States. In 1997, research on this topic – had it existed – would have relied on aggregate capital flows, on rare occasion disaggregated by type of instrument (bonds versus equities) and perhaps also source country. Tabova et al. distinguish treasuries, agencies and corporates, each disaggregated by maturity and type of investor. They show that disaggregating by type of bond issuer and holder yields additional insights. Source-country interest rates are important drivers of capital flows into U.S. bond markets, corporate bond markets in particular, consistent with search-for-yield effects (although we also heard alternative interpretations in the discussion). This pattern is especially evident when source-country rates are low. The Fed's low interest policies have been criticized, as everyone in this room will be know, for encouraging excessive risk taking by market participants. Tobova et al. provide a reminder that, the dollar's exorbitant privilege notwithstanding, this phenomenon is not unique to Federal Reserve policy.

Then we have the paper by Xiadong Zhu. A detailed analysis of Chinese financial plumbing would not have been of general interest in 1997-8, China's economy and its financial system in particular not having developing to the extent and eliciting the concern they have more recently. In contrast, much attention has been paid to shadow banking in China in recent years, when the phenomenon and problem have come to the fore. Zhu convincingly argues the need to revise this history: shadow banking in fact began to develop much earlier than suggested by conventional accounts. I would note in passing, however, that this revisionism is also evident elsewhere; it is similarly emphasized, for example, in a recent book by Andrew Collier (2017).

The author contrasts two views of China's shadow banks: a benign view that they are mechanisms through which a relatively efficient formal banking system has sought to circumvent regulatory restrictions on their operation (restrictions on how aggressively they can compete for funds and on what kind of loans they can make, for example), versus a malign view, that shadow banking vehicles were created by local governments and state-owned enterprises to channel funds in politically favored directions.

Zhu's answer to the question of which effect predominated is a resounding "both." But he shows how the balance of effects has varied over time, with the negative effects becoming increasingly dominant recently.

This conclusion makes intuitive sense. With liberalization of the banking system, the need for efficiency-enhancing circumvention of restrictive regulation has diminished. And with pressure from new private-sector competitors on state-owned enterprises and the regional governments that depend on them, the incentive to use shadow banks to channel funds in directions not supported by an increasingly commercialized banking system has intensified further. This conclusion provides an efficiency rationale for the authorities' recent efforts to step up oversight of shadow banking.

Professor Zhou cautions that his evidence is "suggestive rather than conclusive," but even suggestive evidence is useful for shedding light on what otherwise would be a dark corner of the Chinese financial system. That said, I would have liked to see further discussion of the threat to financial stability posed by shadow banking, and a quantitative estimate of how much shadow-bank abuses have contributed to China's corporate debt problem. The author has done much already, in other words, but there is still much more for him to do.

Finally, let me add a few words on President Williams' comments on the evolution of monetary-policy frameworks. The picture he paints is one of increasing efficiency over time, with the evolution from commodity money to the gold standard, to exchange rate pegs and now to inflation targeting. The implicit question he raises is whether there might be further evolution in the future, from inflation targeting to price level targeting, perhaps, or to a higher inflation target, or to elimination of the zero lower bound as a result of the transition to a digital currency.

This issue points up the question of whether changes in the monetary policy framework are more likely to take place in good times or bad. Historically, they have tended to take place in bad times, as a result of a crisis. Think of the movement from the gold standard to managed

floating in the Great Depression, the movement of the Bank of England to inflation targeting as a result of the 1992 crisis, or the movement from pegs to floats by Asian countries as a result of the crisis of 1997-8. But one can argue that the optimal time for such transition is in good times, when the change in framework doesn't perturb the markets. (The analogy would be with when to abandon an exchange rate peg, where authors argue that the answer is in good times, when the rate is apt to appreciate, not in bad times, when it is prone to collapse – see Eichengreen and Masson 1998.) The implication is that now would be a good time to implement the lessons of recent experience – if we could agree on them – and adjust the prevailing monetary framework, but that it might take another crisis to precipitate such a move.

In sum, the papers at this conference remind us of how much progress has occurred in the 20 years since the Asian financial crisis, in terms of both policy and research. This progress is evident in how questions are formulated, in the large amounts of new data used to analyze those questions, and in success at shedding light on dark corners of the regional and global financial systems. I like to think that this progress will continue, and that I'll have the opportunity of delivering another such conference summary 20 years from now.

### References

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