PROSPECTS FOR ASIA AND THE GLOBAL ECONOMY

Asia Economic Policy Conference

Sponsored by the

Federal Reserve Bank of San Francisco

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Edited by
Reuven Glick
Mark M. Spiegel

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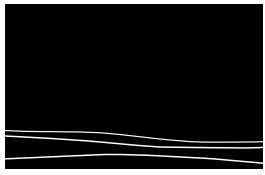

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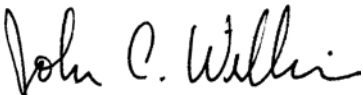
Foreword

The 2013 Asia Economic Policy Conference, titled "Prospects for Asia and the Global Economy," is the third in a series that the Federal Reserve Bank of San Francisco began in 2009 and holds in alternate years. This series is the flagship event of our Center for Pacific Basin Studies, bringing together researchers, private market participants, and policymakers to explore Asia's evolving role in the global economy.

These conferences build on the Bank's long-standing tradition of focusing on Asian developments. They help provide the deep understanding of Asian economies that is very important to the Federal Reserve Bank of San Francisco. Knowledge of the region is important for formulating monetary policy, promoting the stability of global financial markets, and executing our responsibilities in banking supervision.

The program at this year's conference focused on the many challenges faced by policymakers in both advanced and emerging countries in the global economy. What are the channels through which monetary policy in advanced economies has affected other countries, particularly emerging markets? How can monetary policy and macroprudential policy be managed to achieve the objectives of price, output, and financial sector stability? What is the global growth outlook, particularly in Asia? How vulnerable is the global financial system to another crisis?

I appreciate the contributions of all those who took part in the conference, including authors, discussants, panelists, and audience members. My special thanks to Reuven Glick and Mark Spiegel, who organized the program and edited the proceedings, and to Anita Todd and Jeremy Pearce for their assistance with the production of this volume.



John C. Williams
President and CEO, Federal Reserve Bank of San Francisco

Prospects for Asia and the Global Economy: Conference Summary

Reuven Glick and Mark M. Spiegel

The Federal Reserve Bank of San Francisco's Center for Pacific Basin Studies held the third in its biennial Asia Economic Policy Conference (AEPC) series with a program on "Prospects for Asia and the Global Economy" on November 3–5, 2013. The program focused on the challenges faced by policymakers in both advanced and emerging economies as their countries continue to recover from the global financial crisis of 2008–09. Participants considered many ongoing questions, such as what are the channels through which monetary policy easing in advanced economies has affected other countries, particularly emerging markets? How can monetary policy and macroprudential policy be managed to achieve the objectives of price, output, and financial sector stability? What is the global growth outlook, particularly in Asia? How vulnerable is the global financial system to another crisis? Finally, what reforms in financial sector regulation would help mitigate the likelihood of future crises? To explore these issues and others, the conference brought together experts from around the world and commissioned papers and other presentations by distinguished speakers. This chapter highlights the principal issues raised at the conference and summarizes the papers presented.

In opening remarks on "Advanced Economy Monetary Policy and Emerging Market Economies," Governor Jerome Powell of the Federal Reserve Board of Governors spoke on the challenges posed by volatile cross-border capital flows for emerging market economies (EMEs). He acknowledged that accommodative monetary policy in advanced economies, particularly in the United States, has drawn capital into EMEs and put upward pressure on their currency values and asset prices. However, he argued, even if those policies caused EME currencies to appreciate, the consequent drag on their exports has been offset by the positive effects of stronger demand from advanced economies. In addition, Powell said that capital flows depend on other factors as well, such as relative growth prospects and global attitudes towards risk: Capital has been attracted to EMEs by their stronger growth outlook and greater risk tolerance by global investors. Moreover, in his view credit growth and rising house prices in many

EMEs seem to reflect domestic developments more than spillovers from global financial conditions.

Although monetary policy in the United States is likely to remain highly accommodative for some time, Powell acknowledged that financial markets in EMEs may be affected as the U.S. economic recovery continues and the Federal Reserve tapers its pace of asset purchases and moves closer to raising the federal funds rate. However, just as was the case in the most recent financial crisis, EMEs generally should exhibit greater resilience than they did in prior decades, reflecting their more flexible exchange rates, greater international reserve holdings, stronger fiscal positions, and better regulated and managed banking systems. Powell concluded by stating that the Federal Reserve's mandate, like those of other central banks, is focused on the pursuit of domestic policy objectives. Nonetheless, the Federal Reserve takes into account the linkages between the U.S. economy and the rest of the world when conducting monetary policy.

The Asian giants, China and India, have experienced historically unprecedented episodes of growth over the past 30 years. Consensus forecasts for the global economy over the medium and long term call for Asia including China and India to continue to grow strongly. In "Asiaphoria Meets Regression to the Mean," Lant Pritchett and Lawrence Summers of Harvard University coin the term "Asiaphoria" to describe this optimistic forecast, but argue for a more pessimistic outcome. More specifically, they argue that past growth performance is of very little value for forecasting future growth and that there are good reasons to expect that future growth in China and India may be much less rapid than is currently anticipated. In their view, abnormally rapid growth is rarely persistent and "regression to the mean" is an empirically robust feature of economic growth.

Pritchett and Summers also show that the growth process in developing countries is typically characterized by sharp changes or "discontinuities," with very large accelerations or decelerations of growth. Rapidly growing countries are substantially more likely to suffer a sharp downward change in growth than an upward movement; that is, growth declines are more likely to be sudden and large than gradual and small. Moreover, the risks of sharp declines in growth are much higher in countries with weak institutions and policymaking frameworks.

For these reasons their forecasts for Chinese and Indian growth are much more pessimistic than consensus projections. In their view, high levels of state control and limited respect for the rule of law add to the likelihood of a sharp decline in growth in these countries. They refer to prior bouts of Asiaphoria—the

growth projections of Japan from the 1960s and 1970s and the growth booms of the East Asian Tigers of Korea, Taiwan, Singapore, and Hong Kong in the 1980s. Economic growth ultimately declined sharply in all of these economies. Pritchett and Summers predict a similar outcome for China and India.

In "Surprising Similarities: Recent Monetary Regimes of Small Economies," Andrew Rose of the University of California at Berkeley provides evidence on how monetary regimes in small open economies fared during the recent global financial crisis. He classifies monetary regimes into three categories: those that target the inflation rate; those maintaining a "hard" exchange rate peg to some stable foreign currency either by accepting it as separate legal tender or by adopting a currency board arrangement or a conventional peg; and those regimes in the "sloppy center" with crawling bands, adjustable pegs, or monetary aggregate targets.

Rose presents two main results. First, he shows that hard fixers and inflation targeters generally were able to sustain their regimes during the shocks from the global financial crisis. Of the countries that targeted inflation in 2006, almost all were still doing so in 2012; almost three-quarters of the hard fixers also survived over the period. By way of contrast, less than a quarter of the sloppy center maintained the same monetary regime during the crisis and its aftermath. This stability is historically unusual because, as Rose also shows, during prior recessions and crises it was very common for countries to abandon their monetary policy regimes.

Second, Rose finds that macroeconomic performance—whether measured by growth, inflation, fiscal policy, current account, reserve growth, or asset prices—over the period 2007–12 were similar for countries with hard fixed and inflation targeting regimes. In contrast, outcomes for the sloppy center were quite different, particularly on the inflation front, where inflation performance was substantially worse. This result is somewhat surprising, since a hard commitment to a fixed exchange rate seems quite different from the constrained discretion of an inflation target. Hard fixers have severely limited monetary autonomy, while inflation targeters are not directly constrained by exchange rate targets. Thus, hard fixers with open capital markets would seem to have substantially less ability than inflation targeters to insulate themselves from the spillover effects of foreign capital flows.

A possible explanation for Rose's finding is that because the financial crisis was a common shock, creating a deep recession and strong deflationary pressure virtually everywhere, independent inflation targeting central banks as well as the central banks to which hard fixers pegged all aggressively eased monetary policy. Thus, any countries that had pegged to the dollar or the euro

also experienced policy ease because of the actions of the Federal Reserve and the European Central Bank. For most countries very accommodative monetary policy was appropriate during the global financial crisis, and this is what their monetary regimes delivered. This might not be the case for other shocks, such as terms of trade or inflation shocks, where the outcomes may well be very different for hard fix versus inflation targeting regimes.

In a keynote address entitled "The Shifts and the Shocks: Emerging Economies in an Age of Financial Crises," Martin Wolf of the *Financial Times* discussed the origins of the 2007–08 financial crisis and its implications for emerging markets. He views the crisis as the result of the interaction of a vulnerable financial system with a global savings glut after the Asian crisis of the late 1990s, as many emerging economies, including China, pursued economic policies that generated large current account surpluses. This led to large capital flows to the rest of the world, particularly to the advanced economies, including the United States.

In this environment, the Federal Reserve pursued aggressive monetary policies to offset the external drag of growing U.S. current account deficits and concerns about deflation after the stock market bubble burst in 2000. The capital inflows and accommodative monetary policy led to increasing asset prices, particularly in housing, and borrowing in the United States. These effects were amplified by investors "reaching for yield," the proliferation of mortgage-backed assets, and a dramatic increase in leverage within the financial sector. With house prices and credit rising, greater household indebtedness stimulated additional spending by households on consumption and residential investment. When the crisis hit in 2008, the pumped-up demand collapsed. In Wolf's view, the exceptionally aggressive monetary policies in advanced countries since 2008 have been largely ineffective in boosting economic growth and reducing unemployment. He attributes the ineffectiveness of monetary policy to a breakdown in the functioning of the credit mechanism and the effects of the zero lower bound on policy rates.

In discussing the implications for emerging economies, Wolf notes that after the crises of the 1980s and 1990s, emerging economies sought to minimize the risks of crises by adopting more conservative fiscal policies, placing greater reliance on borrowing in domestic currencies, focusing central banks on inflation targeting, allowing their exchange rates more flexibility, and using capital controls when appropriate. These policies generally worked, with emerging economies proving far more resilient to shocks recently than in the past.

Looking forward, Wolf maintained that macroprudential policy through permanently higher capital ratios or by automatic countercyclical adjustments

of leverage ratios could help lower the probability of crisis further. However, he argues that financial crises are inescapable features of greater risk-taking behavior fostered by economic and financial liberalization in a global economy. He suggests that the way to encourage a less crisis-prone world economy involved some reserve accumulation by emerging economies as well as by making more resources available through international financial institutions, such as the IMF.

Reserve accumulation by emerging market economies rose significantly in the aftermath of the 1997–98 Asian financial crisis. This buildup was motivated by efforts to limit currency appreciation in order to maintain competitiveness, as well as by the desire of policymakers in these countries to provide self-insurance against the effects of capital flow reversals. The need for such insurance increases with capital account liberalization, where potential currency mismatches, bank runs, and capital flight can rapidly drain a central bank's foreign exchange reserves.

Reserve accumulation has costs as well. These include the income loss associated with earning interest on reserves that is typically lower than the interest paid on instruments issued by the central bank—typically government bonds or its own liabilities—to finance or sterilize its reserve accumulation. Other costs may arise if sustained reserve accumulation fuels domestic credit booms and asset price bubbles or creates distortions when the banking sector is induced to hold more of these instruments than it would choose to do so voluntarily.

In "Crowding Out Redefined: The Role of Reserve Accumulation," Carmen Reinhart and Takeshi Tashiro of Harvard University emphasize another cost of reserve accumulation in the form of the crowding out of domestic investment in the aftermath of the 1997–98 Asian financial crisis. They show that, for the nine Asian economies in their study, average investment as a share of GDP fell by about 6 percentage points during the period from 1998 to 2012 compared to its average level in the decade before the sustained reserve accumulation. They interpret this dampening of investment in Asia as a form of crowding out, with Asian governments absorbing domestic saving in order to acquire the securities of the advanced economies as reserves.

More specifically, Reinhart and Tashiro show that in the wake of the Asia crisis, many governments in the region redirected their borrowing toward the domestic market because external borrowing was either prohibitively expensive or altogether unavailable. Even in cases where capital market access was not lost, many governments sought to borrow more from "captive" domestic savers, such as pension funds, insurance companies, and domestic banks, to lessen rollover risk. This fostered greater competition for domestic borrowers

in the home market for loanable funds. The resulting higher cost of borrowing curtailed domestic investment through what Reinhart and Tashiro refer to as "conventional" crowding out. To the extent that central banks also funneled domestic saving abroad to accumulate foreign reserves, a broader form of crowding out occurred.

Their analysis suggests that countries with relatively more pervasive capital flow barriers may have a better chance of limiting the crowding out effects of reserve accumulation on investment. This may be because capital controls limit private outflows or capital flight and/or because the magnitude of desired reserve accumulation is smaller, as the controls also insulate the domestic economy from external shocks.

In his address "The Second Phase of Global Liquidity and Its Impact on Emerging Economies," Hyun Song Shin of Princeton University discussed how recent trends in global liquidity have affected the vulnerability of the international financial system. Shin distinguishes two phases of global liquidity during the past decade. The first phase, starting roughly in 2003 and lasting until the 2008–09 crisis, primarily involved the transmission of looser financial conditions across borders through the acceleration of banking sector capital flows. The second phase of global liquidity, which started around 2010, involved the cross-border transmission of financial conditions through the growth of offshore bond markets, particularly involving the debt securities of emerging market corporations.

Shin argues that the second phase of global liquidity has increased the vulnerability of emerging economies for several reasons. First, to the extent that the offshore debt issued by emerging market corporations is denominated in foreign currency, mismatches arise on their consolidated balance sheets, exposing them to exchange rate risk. Second, the increased offshore issuance of bonds has been accompanied by growth in corporate deposits in the domestic banking system, making them more vulnerable to withdrawal in the event of corporate distress. Third, the growing stock of emerging market debt securities has been absorbed increasingly by asset managers, such as hedge funds, that may be more prone to engage in risk-taking behavior in comparison to other investors, such as pension and life insurance companies.

As Shin discusses, most analyses of past financial crises have focused on the banking sector and emphasize leverage or maturity mismatches of banks or other financial intermediaries. In contrast, he argues that future crises are more likely to depend on the activities of the corporate sector and asset managers. Consequently, he argues that the usual indicators of vulnerability, such as bank leverage, may be of limited use in signaling the next crisis. He suggests

several alternative measures of financial system vulnerability that better measure the exposure of emerging market nonfinancial firms to capital flow reversals. These include tracking offshore borrowing as well as corporate bank deposits and other short-term claims of the nonfinancial corporate sector on the domestic financial system.

The second day of the conference began with a paper by Olivier Jeanne of Johns Hopkins University, entitled "Macroprudential Policies in a Global Perspective." Jeanne examines the case for global coordination of macroprudential policies aimed at managing international capital flows. As capital inflows sometimes can be disruptive, countries may unilaterally improve their welfare through macroprudential intervention. However, Jeanne's analysis demonstrates that such policies may involve spillovers to other economies, raising the scope for mutually beneficial policy coordination. In particular, Jeanne presents a theoretical model in which private foreign borrowing can influence the probability of a financial crisis, but this effect is not fully internalized by domestic private agents.

Jeanne finds that there is little scope for mutually beneficial international policy coordination in his frictionless benchmark model, because any international spillovers that may exist manifest themselves through compensating global interest rate movements. The best attainable outcome under such conditions can be reached by each country pursuing its unilaterally preferred domestic macroprudential policy affecting domestic borrowing, rather than implementing controls on capital inflows. In contrast, when nominal frictions—such as wage rigidity—are present, or when countries are large, there is a scope for beneficial policy coordination. For example, policy coordination may be required to avoid pushing one or more countries into a liquidity trap in which the zero-bound constraint on the nominal interest rate makes it impossible to achieve full employment with monetary policy.

Jeanne presents the example of a two-country model of the United States and China, considering macroprudential policy when the United States finds itself in a liquidity trap. He demonstrates that a Pareto-improving outcome that could be achieved through policy coordination would be for the United States to be less aggressive in its pursuit of monetary expansion to escape the liquidity trap, while China lessens its efforts to dampen the impact of U.S. monetary policy on its domestic credit growth by accumulating less foreign reserves.

In "Financial Regulation after the Crisis: How Did We Get Here, and How Do We Get Out?," Gerard Caprio of Williams College reviews the recent regulatory reform initiatives undertaken by the Basel Committee on Bank Supervision and argues that regulatory responses undertaken to date in response

to the global financial crisis need to be reconsidered. In particular, he argues that the main framework underlying the Basel reforms needs to be restarted because of its neglect of the endogeneity of risk to the regulatory structure and the dynamic nature of finance and its regulation. In his view, applying similar risk weights for all banks in all countries has resulted in increased correlation of bank asset returns. This is because similar risk weights induce banks to move into similar asset exposures, which can become more correlated as conditions deteriorate. In addition, Caprio believes that a restart of regulatory oversight is necessary because the response to difficulties in the regulatory framework in the past has been an increase in the complexity of the regulatory system, with regulatory intervention at "an ever more granular level," to forestall undesired behavior by regulated firms.

In order to reverse this pattern of ever-increasing regulatory complexity, Caprio recommends that the risk weights codified under the Basel regulations should be abandoned in favor of a simple leverage rule supplemented by heightened shares of equity finance and conditional convertible debt, commonly referred to as "CoCos." CoCos automatically convert from debt to equity when equity falls below a specified level. His view is that CoCos could induce banks to follow more prudent lending and funding practices, thereby lowering their risk of failure. He also argues for a regulatory approach that focuses on regulator accountability and greater transparency, claiming that regulatory intervention during the crisis was late. He gives the example of Northern Rock in the United Kingdom being allowed to issue dividends shortly before its failure. He therefore argues for the need of a "sentinel" that would monitor regulators against both corrupt and obsolete practices. Such a sentinel would provide public commentary on regulatory practices, forcing the regulator to be more accountable to the public, but have no regulatory power itself. Caprio acknowledges that the creation of such a regulatory sentinel provides no guarantee that regulators will respond to vulnerabilities more vigorously, but he argues that it would increase the odds of a more prompt regulatory response.

In the policymaker panel, Bank of Korea Deputy Governor Woon Gyu Choi discussed "Current Policy Challenges Faced by Emerging Market Economies and Korea." Choi considered policy problems raised for emerging market economies by the anticipated removal of accommodative monetary policies by Western central banks as the recovery from the global financial crisis progresses. In particular, he examined the challenges raised by the Federal Reserve's tapering of its quantitative easing policies.

Choi argues that the anticipated removal of accommodative monetary policy by foreign central banks is analogous to a negative external shock exerting

deflationary pressure on emerging market economies. However, this impact would likely be mitigated by a negative impact on their exchange rates boosting their export competitiveness. Moreover, the appropriate monetary policy response to such a shock is unclear due to possible adverse implications of monetary tightening on domestic economic conditions. Empirically, vulnerable countries, most notably the well-known "fragile five" (India, Indonesia, Brazil, South Africa, and Turkey) experienced capital outflows resulting in exchange rate depreciation and reduced domestic equity values. He echoed the remarks at the conference by Federal Reserve Governor Jerome Powell, arguing that advanced economy monetary policies should account for such international linkages.

Choi claims that the signaling of tapering by the Federal Reserve in May and June 2013 served as a test run of the implications the removal of accommodative Western monetary policies could have on emerging market fortunes. The primary lesson he draws from this experience is that fundamentals, in terms of fiscal space and domestic economic conditions, play a significant role in determining these implications. Moreover, in the event of such policy changes, the scope for adjustment to these external shocks among the most vulnerable emerging market economies is likely to be severely limited.

The Deputy Governor finished his remarks by focusing on the Korean case. Korean fundamentals had improved markedly since the global financial crisis, and this improvement was rewarded by a relatively benign experience during the initial signaling period of Federal Reserve tapering. Still, Korea faces a number of unique challenges, including its extraordinary openness, which results in increased vulnerability to foreign shocks, which could lead to sluggish domestic investment and increasing household debt that could weigh on domestic Korean demand going forward.

The next speaker in the policy panel was David Dollar from the Brookings Institution, who spoke on "Financial and Fiscal Reforms in Support of China's Rebalancing." Dollar began his analysis by noting that, while China weathered the global financial crisis relatively well, it did so through government stimulus that increased already high investment levels. Consequently, China emerged from the crisis in need of structural rebalancing.

Dollar notes that structural reforms are likely to play an important role in this rebalancing. These will include opening up the service sector to competition, easing labor mobility, and most importantly greater liberalization of the financial sector. Financial reforms could allow for gradual Chinese adjustment to a more sustainable growth pattern. Chinese deposit interest rates are exceptionally low, leaving households almost no return on savings. Investment

options outside of the banking sector, such as through equities or securities, are very limited. Under these conditions, a large amount of capital has flowed into the real estate sector, inflating prices.

Some reform has already taken place. Commercial banks are permitted to offer deposit rates up to 1.1 times benchmark rates, and the band may be further widened. Moreover, the shadow banking system, in which higher interest rates prevail, has grown considerably. However, Dollar notes that light regulation in that sector raises vulnerabilities for the Chinese financial system. Further interest rate liberalization in the formal banking sector would therefore be welcomed. On the fiscal side, Dollar argues that a key problem is that revenues are primarily raised by the central government while expenditures typically take place at the local level. Allowing local governments to issue bonds or to raise local revenues through property taxes would address this mismatch.

The final panel speaker was Bank of Canada Deputy Governor John Murray. Murray began his remarks by comparing Canada's current situation with those of the Asian economies. While Canada shares many similarities with Asian economies, most notably its openness and its vulnerability to external shocks from neighboring large economies, there are also notable differences. In particular, while many Asian economies, particularly China, are discussing a rebalancing away from excessive reliance on external demand, Canada is looking forward to greater stimulus from external sources, primarily driven by the U.S. economic recovery. Unlike most Asian nations, Murray argues that the level of reliance on external demand displayed by Canada just before the crisis in 2007 was both sustainable and appropriate. In addition, he notes that, while Canada's maintenance of nominal exchange rate flexibility—which also stands in contrast to many Asian nations—has led to episodes of instability, over the long run it has permitted market signals to feed through to the Canadian economy and allowed for smoother and more timely adjustment.

Murray also reviewed the performance of emerging Asian economies leading up to the crisis, acknowledging that growth rates posted in the region were extraordinary, but also cautioning that the imbalances that accompanied these achievements likely rendered them unsustainable. Thus, in his view, it is not surprising that the export sectors of emerging Asian economies were particularly hard hit during the crisis. Still he argues that the framework crafted by the Group of 20 leaders early in the crisis laid the framework for sustainable recovery.

Nevertheless, Murray acknowledged that the global recovery to date has been disappointing. Moreover, he argues that the pace of recovery achieved was largely due to exceptionally aggressive monetary policies. Further progress

will only be achieved through true structural reform. Along these lines, he points to reform efforts in Asia's two largest economies, China and Japan, as hopeful signs of future meaningful adjustment.

The conference was closed by a broad overview of current issues faced by the Asian region by Barry Eichengreen of the University of California at Berkeley. In his overview, Eichengreen argues that the outlook for the region is first and foremost a question about its two major economies, China and Japan. He was cautiously optimistic about the prospects for what he terms the "great reflation" in Japan. He noted that positive, albeit modest, inflation numbers have begun to show up in Japanese data, a marked change for that economy. He also praised the efforts Japan's government has made to balance its need for short-term fiscal stimulus with that of medium-term fiscal consolidation. However, like many commentators, Eichengreen remains pessimistic about the commitment to the so-called "third arrow" of Japanese adjustment, namely structural reform. He notes the strong opposition that the government has already encountered in its efforts to lower labor adjustment costs.

Turning to China, he characterized its efforts to rebalance its economy as "monumental," noting that household consumption currently only accounts for about a third of the Chinese economy, well below the two-thirds share in many other economies. Since such a rebalancing presumably would be accomplished through greater consumption of services, Eichengreen argues that it would be accompanied by slower output growth since productivity growth in services is lower than that in manufacturing. He therefore wondered whether government authorities would be willing to accept such a growth slowdown in the name of reform.

Eichengreen then turned to the question of the likelihood of another Asian crisis. He highlighted a number of contrasts of the current situation with that of the 1997–98 Asian crisis: Asian countries have more flexible exchange rates, they have a greater share of their debt denominated in local currency, and they are running current account surpluses and have built up substantial war chests of foreign reserves. Still, Eichengreen points to China as a risk, noting that credit broadly defined has boomed in China from 125 percent to 200 percent of GDP. Eichengreen notes that many such credit booms have ended badly.

OPENING REMARKS

Advanced Economy Monetary Policy and Emerging Market Economies

Jerome H. Powell

I appreciate this opportunity to offer a few thoughts on the effects of advanced economy monetary policies on emerging market economies (EMEs)—an issue of great importance for Asia and the global economy.[1] Since the global financial crisis, the Federal Reserve has sought to strengthen the U.S. economic recovery through highly accommodative monetary policy. But my colleagues and I are keenly aware that the U.S. economy operates in a global environment. We understand that America's prosperity is bound up with the prosperity of other nations, including emerging market nations.

Emerging market economies have long grappled with the challenges posed by large and volatile cross-border capital flows. The past several decades are replete with episodes of strong capital inflows being followed by abrupt reversals, all too often resulting in financial crisis and economic distress.[2] Some of this volatility no doubt reflects the evolution of strengths and vulnerabilities within the EMEs themselves.

In recent years, renewed attention has been placed on the role of advanced economies and of common or global factors in driving capital movements.[3] In particular, many observers have singled out monetary policy in the United States and other advanced economies as a key driver. As advanced economies pursued highly accommodative monetary policies and EMEs subsequently received strong capital inflows, reflecting investors' pursuit of higher returns, concerns were expressed that a flood of liquidity would overwhelm emerging markets, drive up asset prices to unsustainable levels, set off credit booms, and thus sow the seeds of future crises. More recently, there have been concerns about potential financial and economic dislocations associated with the advanced economies' eventual exit from highly accommodative policies.

In my remarks today, I will discuss the extent to which monetary policy in the advanced economies—and in the United States in particular—has contributed to changes in emerging market capital flows and asset prices, and I will place this discussion in a broader context of economic and financial linkages

among economies. I will also address the risks that EMEs may face from the eventual normalization of monetary policy in the advanced economies.

The heightened attention to advanced economies' monetary policies and the potential spillovers to EMEs is understandable in light of the unprecedented policy steps taken in the aftermath of the global financial crisis. The severity of the crisis and the challenge of a slow recovery required central banks in the advanced economies and elsewhere to take aggressive action in order to fulfill their mandates. In the United States, the Federal Reserve is bound by its dual mandate to pursue price stability and maximum employment. In following that mandate, the Fed cut the federal funds rate to its effective lower bound in late 2008 and then turned to two less conventional policy tools to provide additional monetary accommodation. The first is forward guidance on the federal funds rate. By lowering private sector expectations for the future path of short-term rates, forward guidance has reduced longer-term interest rates and raised asset prices, thereby leading to more accommodative financial conditions. The second tool is large-scale asset purchases, which likewise increase policy accommodation by reducing longer-term interest rates and raising asset prices.

The Federal Reserve has not been alone in implementing unconventional monetary policies. The Bank of England has also engaged in substantial asset purchases and recently introduced explicit forward guidance for its policy rate. The Bank of Japan, a pioneer in the use of unconventional policy, has recently embarked on an ambitious asset purchase program to combat deflation. And the European Central Bank (ECB) substantially extended its liquidity provision by offering unlimited longer-term refinancing operations. The ECB also purchased some securities in distressed markets, and recently indicated that it expects interest rates to remain low for an extended period. Thus, since the end of the crisis, central banks in the advanced economies have adopted similar policies to promote recovery and price stability.

While a great deal of attention has focused on unconventional policy actions, especially asset purchases, these policies appear to affect financial conditions and the real economy in much the same way as conventional interest rate policy. Indeed, recent research suggests that adjustments in policy rates and unconventional policies have similar cross-border effects on asset prices and economic outcomes.[4] If that is so, then the overall stance of policy accommodation matters more here than the particular form of easing. Moreover, neither conventional nor unconventional monetary policy actions are shocks that come out of the blue. Instead, they are the policies undertaken by central banks to offset the adverse shocks that have restrained our economies. Thus, any spillovers

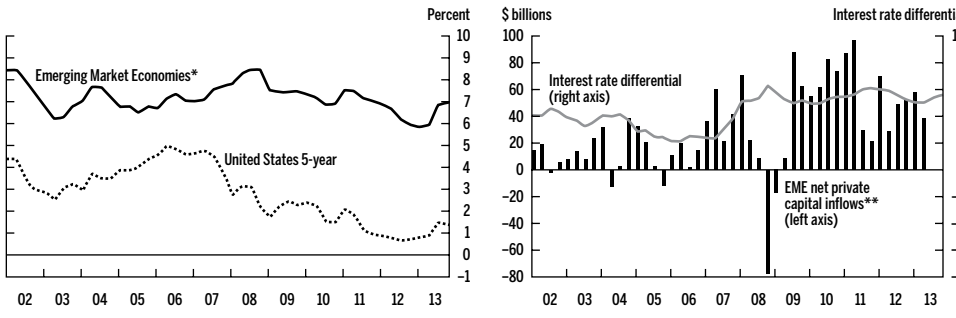
from monetary policy actions must be evaluated against the consequences of failing to respond to these adverse shocks.

In a world of global trade and integrated capital markets, it is natural for economic and financial shocks and policy actions to be transmitted across borders. Spillovers from advanced-economy monetary policies are to be expected.[5] In theory, when advanced economies ease monetary policy in response to a contractionary shock, their interest rates will decline, prompting investors to rebalance their portfolios toward higher-yielding assets. Some of this rebalancing will occur domestically, but some investment will also move abroad, resulting in capital flows to EMEs. In response, EME currencies should tend to appreciate against those of the advanced economies, and EME asset prices should rise. Conversely, a tightening of advanced economy monetary policy in response to a stronger economy should lead these movements to reverse; that is, tightening should reduce capital flows to EMEs and diminish upward pressure on EME currencies and asset prices.

Are these basic relationships apparent in the data? The left side of Chart 1 shows an index of EME local-currency sovereign bond yields along with a roughly similar maturity U.S. Treasury yield. The line on the right is the differential between the two, plotted against net inflows of private capital to a selection of EMEs, shown by the bars. If interest rates were the main driver of capital flows, these two series ought to move in a similar fashion. At times, this is indeed the case: From mid-2009 to early 2011, the interest rate differential and EME capital inflows rose together. But the overall relationship is not

CHART 1

Interest Rates and EME Capital Inflows



*J.P. Morgan Government Bond Index-Emerging Markets, local currency debt.

**Balance of payments data. Includes Argentina, Brazil, Chile, Colombia, India, Indonesia, Korea, Malaysia, Mexico, Philippines, Taiwan, and Thailand.

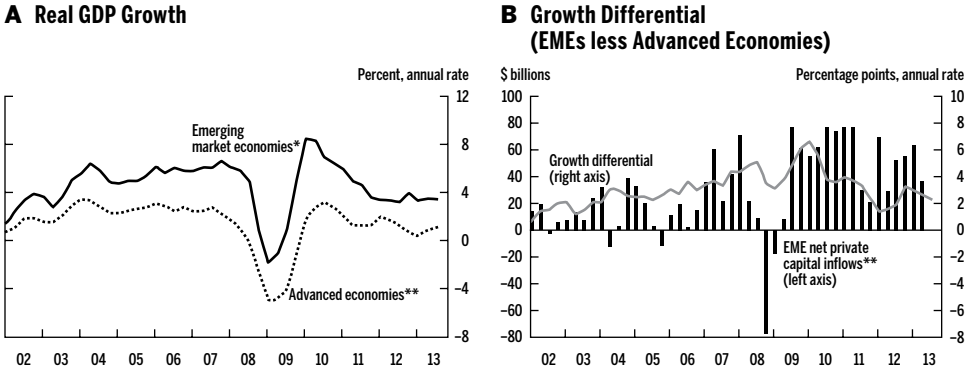
Source: Bloomberg, Haver, IMF *International Financial Statistics*, and J.P. Morgan.

particularly tight. In early 2007, capital flows to EMEs were quite strong even with a low interest rate differential. And in mid-2011, capital inflows stepped down even as the interest rate differential remained elevated. As I will discuss in a moment, the lack of a tight relationship between capital flows and interest rates suggests that other factors also have been important.

Even though interest rate differentials and capital inflows do not always move in the same direction, numerous empirical studies have shown that interest rates do in fact help explain capital flows once other determinants of these flows are also taken into account.[6] In particular, when U.S. rates decline relative to those in EMEs, private capital flows to EMEs tend to rise, consistent with investors rebalancing toward higher-yielding assets. In a similar vein, event studies have shown that the Federal Reserve's policy announcements, including those related to asset purchases, have been associated with capital flows to EMEs as well as upward movements in EME currencies and asset prices.[7] But the role of monetary policy in driving capital flows and the effects of those flows on EMEs should not be overstated. In this regard, I will offer two considerations.

First, many factors affect capital flows to EMEs, not just the stance of advanced economy monetary policy. Differences in growth prospects across countries and the associated differences in expected investment returns are

CHART 2
Real GDP Growth and EME Capital Inflows



*Nominal GDP weighted aggregate of Argentina, Brazil, Chile, Colombia, India, Indonesia, Korea, Malaysia, Mexico, Philippines, Taiwan, and Thailand.

**Nominal GDP weighted aggregate of Australia, Canada, euro area, Japan, Sweden, United Kingdom, and United States.

***Balance of payments data. Includes Argentina, Brazil, Chile, Colombia, India, Indonesia, Korea, Malaysia, Mexico, Philippines, Taiwan, and Thailand.

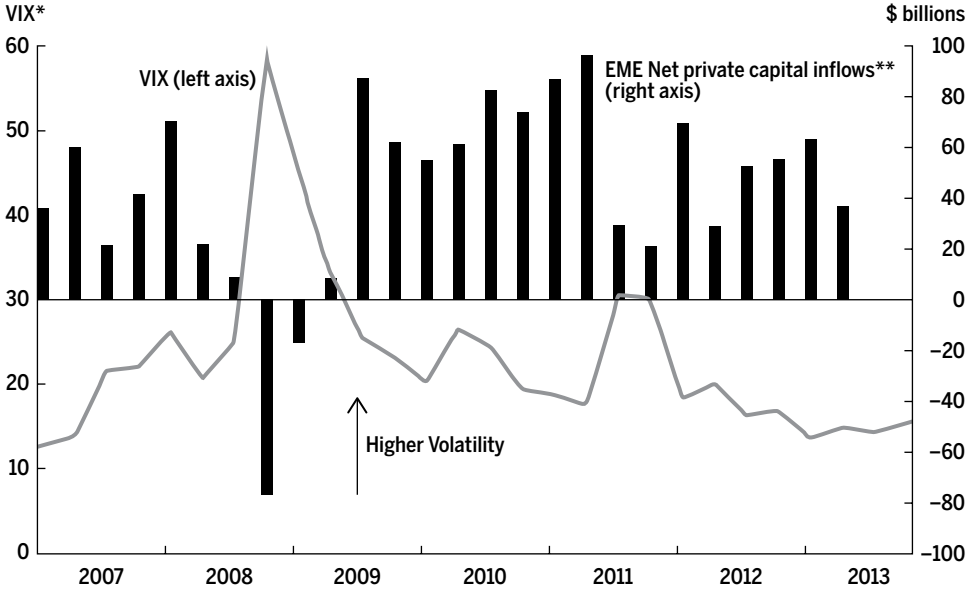
Source: Staff calculations based on data from Haver, IMF *International Financial Statistics*, and IMF *World Economic Outlook*.

important factors.[8] Chart 2 shows the growth rate of real GDP for EMEs and advanced economies. Given their stage of development and demographic profile, EMEs should grow faster than advanced economies on a trend basis. As shown by the line in the right panel, EME growth has, in fact, consistently outpaced that of the advanced economies. In addition, the bounceback of the EMEs from the global financial crisis widened this differential even more, although the gap has diminished more recently as growth in the EMEs has slowed. Moreover, investing in EMEs has become more attractive as many EMEs have improved their macroeconomic policies and institutional frameworks over recent decades; growth differentials may partly be reflecting these improvements. As is evident in the right-hand chart, the relationship between the growth differential and capital inflows to EMEs seems to be quite strong. In particular, the rise in capital flows following the global financial crisis coincided with stronger relative growth performance in EMEs. And in 2011, capital inflows diminished along with the growth differential.

Another key driver of EME capital flows is global attitude toward risk. Swings in sentiment between "risk-on" and "risk-off" have led investors to reposition across asset classes, resulting in corresponding movements in capital flows.[9] Indeed, as shown in Chart 3, the most common measure of uncertainty and the market price of volatility—the VIX—is strongly correlated with net inflows into EMEs. Although the causes of movements in global risk sentiment are uncertain, the ebb and flow of potential crises and policy responses, such as we experienced during the European crisis, are clearly important. Of course, movements in risk sentiment may not be fully independent of monetary policy. An interesting line of research has begun to consider how changes in monetary policy itself may affect risk sentiment. For example, some studies indicate that an easing of U.S. monetary policy tends to lower volatility (as measured by the VIX), increase leverage of financial intermediaries, and boost EME capital inflows and currencies.[10]

A second point to bear in mind when assessing monetary policy spillovers is that expansionary policies in the advanced economies are not beggar-thy-neighbor; in other words, they do not undermine exports from EMEs. In recent decades, some EMEs have successfully pursued an export-led growth strategy, and policymakers in those economies have sometimes expressed concern that their exports will be unduly restrained as accommodative policies in the advanced economies lead their currencies to appreciate. However, as shown in Chart 4, although EME currencies bounced back from their lows during the global financial crisis—when global investors fled from assets they perceived to be risky—for many EMEs real exchange rates have moved sideways or have

CHART 3
Risk and EME Capital Inflows



*The Chicago Board Options Exchange Market Volatility Index (VIX) is a measure of implied volatility of S&P 500 index options.

**Balance of payments data. Includes Argentina, Brazil, Chile, Colombia, India, Indonesia, Korea, Malaysia, Mexico, Philippines, Taiwan, and Thailand.

Source: Bloomberg, Haver, and IMF *International Financial Statistics*.

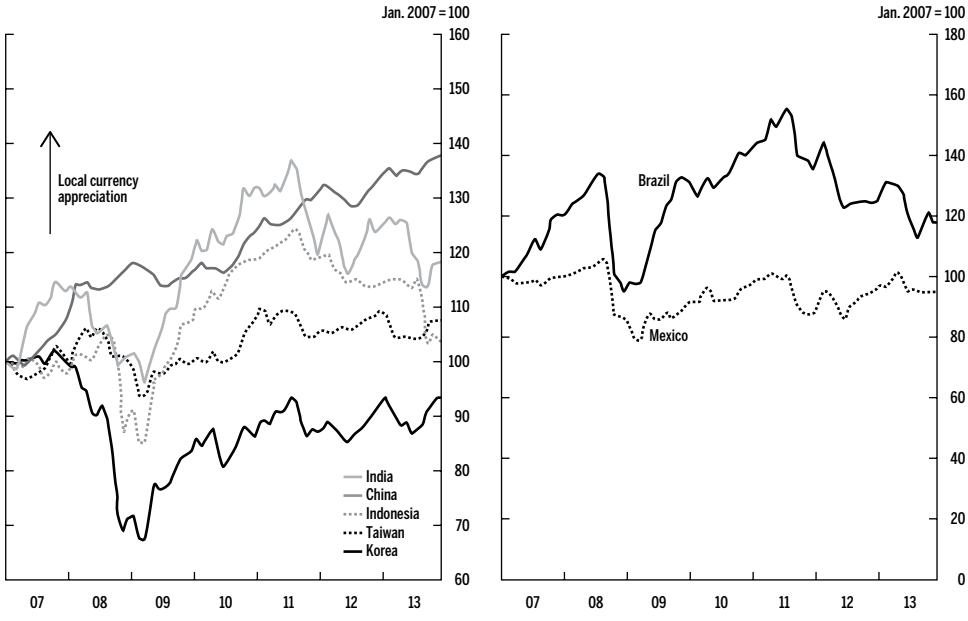
even declined over the past two years. Some of this weakness may reflect the foreign exchange market intervention and capital controls that policymakers used to staunch the rise in their currencies.

But even if advanced economy monetary policies were to put upward pressure on EME currencies, the consequent drag on their exports must be weighed against the positive effects of stronger demand in the advanced economies. According to simulations of the Federal Reserve Board's econometric models of the global economy, these two effects roughly offset each other, suggesting that accommodative monetary policies in the advanced economies have not reduced output and exports in the EMEs.[11] Indeed, this view seems to be supported by recent experience, as the U.S. current account balance has remained fairly stable since the end of the global financial crisis. Over the longer run, advanced economy policy actions that strengthen global growth and global trade will benefit the EMEs as well.

A particularly important consideration regarding spillovers from accommodative monetary policies in the advanced economies is the extent to which

CHART 4

Real Exchange Rates*



*Bilateral vis-à-vis the U.S. dollar, CPI based.

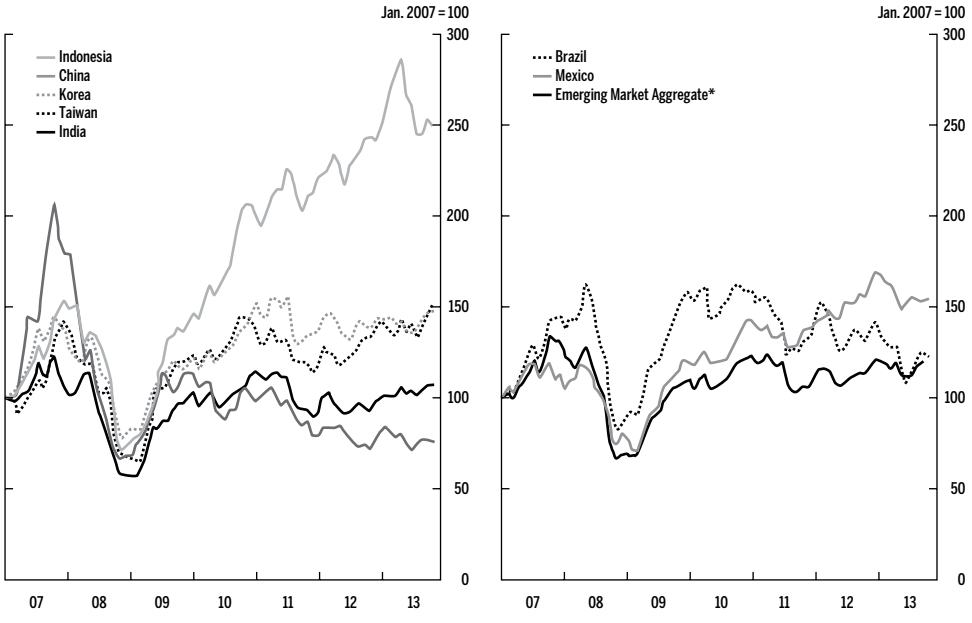
Source: Staff calculations based on data from Bloomberg, Federal Reserve Board, and Haver.

such policies contribute to financial stability risks in the EMEs. Because many EMEs have financial sectors that are relatively small, large capital inflows may foster asset price bubbles and a too-rapid expansion of credit. These are serious concerns, irrespective of the relative importance of monetary policies in the advanced economies in driving these flows. While the picture is a mixed one and some markets show signs of froth, indicators of financial stability do not seem to show widespread imbalances.[12]

For example, EME equity prices, shown in Chart 5, plunged during the global financial crisis, rebounded thereafter, but then generally flattened out or even declined. There are exceptions, of course, such as Indonesia, whose stock market soared until earlier in 2013. But in aggregate, EME stock prices remain below their pre-crisis peak, whereas the S&P 500 is well above its own pre-crisis peak.

Chart 6 portrays the rise in credit to the domestic nonfinancial private sector as a share of GDP from its pre-crisis level. For some EMEs, the rise in credit does not seem out of line with historical trends, but some economies have experienced potentially worrisome increases. Credit growth in China is particularly noteworthy, but this does not seem to be the result of accommodative monetary

CHART 5
EME Equity Prices

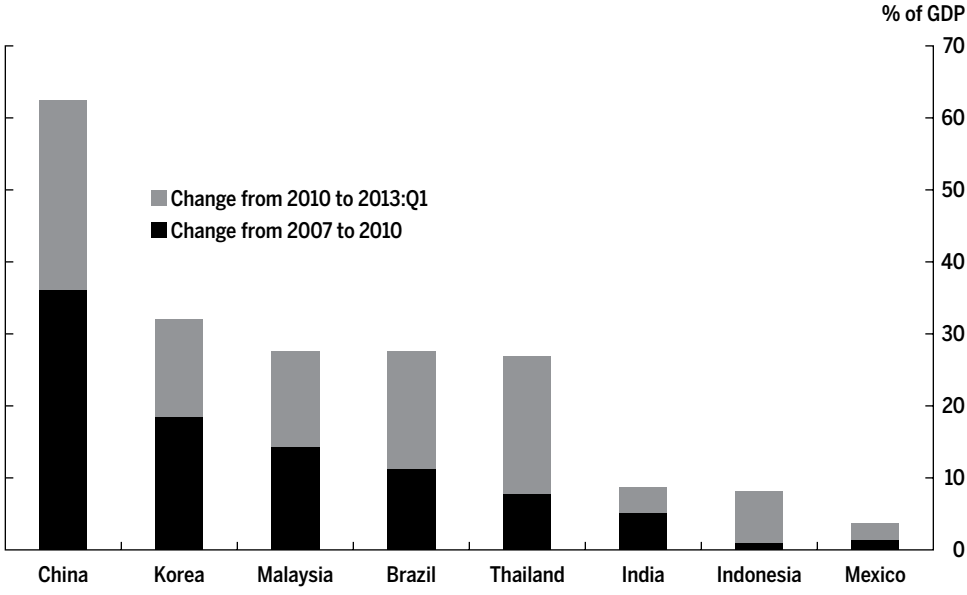


*MSCI EME local currency stock index.
Source: Bloomberg and MSCI.

policies in the advanced economies. Much of the rise took place in the aftermath of the crisis, in large part reflecting policy-driven stimulus to support economic recovery. In addition, China's relatively closed capital account limits the extent to which domestic credit conditions are influenced by developments abroad, including changes in advanced economy monetary policy. Increases in credit in some other economies, notably Brazil, have also been driven to a significant degree by policy actions to support aggregate demand. And, of course, EMEs have policy tools to limit the expansion of credit.

Another area of potential concern is excessive valuations in property markets. Chart 7 displays inflation-adjusted house prices for several Asian economies. The most striking increases have occurred in Hong Kong, which, through its open capital account and essentially fixed exchange rate, is tied most directly to U.S. financial conditions. Of course, the degree of Hong Kong's exposure to U.S. financial conditions is a policy choice, and other factors have also contributed to the run-up in its property prices. House prices have also resumed their rise in China. But, as with credit growth, this rise seems to reflect domestic developments as opposed to spillovers from global financial conditions.

CHART 6
Credit to Private Sector*



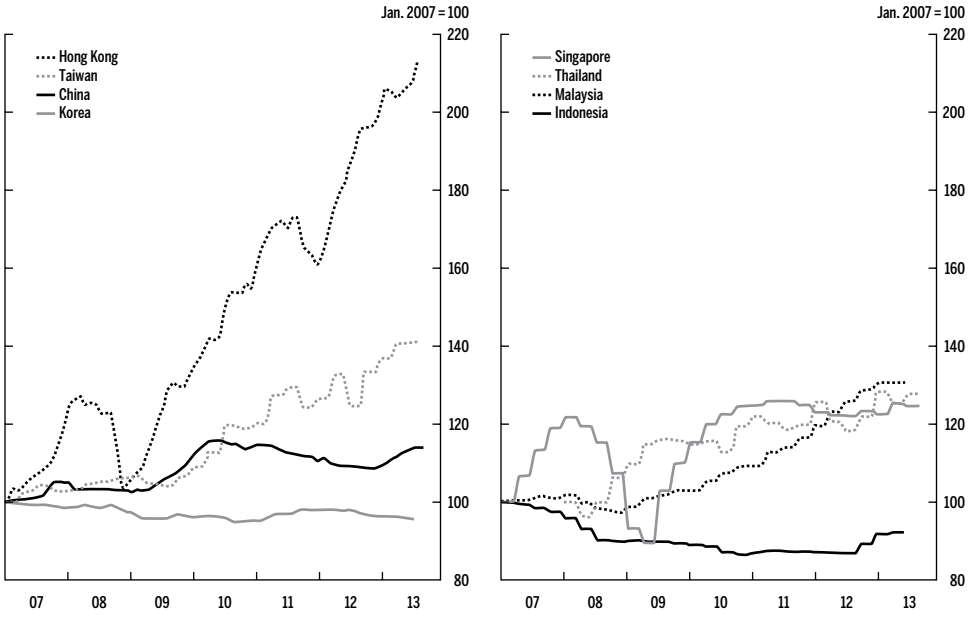
*Total credit to the nonfinancial private sector as a percent of nominal GDP.
Source: Staff calculations based on data from Bank for International Settlements and Haver.

In light of these potential financial stability concerns, it is encouraging that EME policymakers have devoted substantial effort since the Asian financial crisis of the late 1990s to bolster the resilience of their banking systems. Banks in many EMEs have robust earnings and solid capital buffers.[13] Compared with past experience, emerging market banking systems also generally enjoy improved management and a proactive approach by authorities to mitigate risks. Nevertheless, in an environment of volatile global markets, regulators should guard against the buildup of vulnerabilities, such as excessive dependence on wholesale and external funding, declining asset quality, and foreign currency mismatches.

To summarize my discussion so far, EMEs clearly face challenges from volatile capital flows and the attendant moves in asset prices. Accommodative monetary policies in the advanced economies have likely contributed to some of these flow and price pressures, and may also have contributed to the buildup of some financial vulnerabilities in certain emerging markets. That said, other factors appear to have been even more important. Moreover, expansionary monetary policies in the advanced economies have supported global growth to the benefit of advanced and emerging economies alike.

CHART 7

Asian Real House Prices*



*Nominal house prices deflated by CPI.

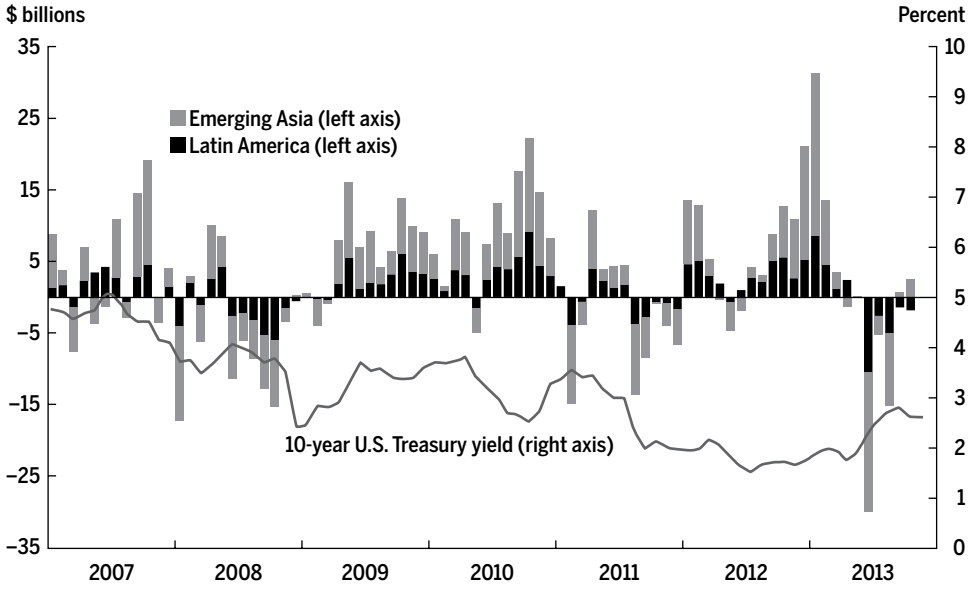
Note: Series indexed to start date, March 2008.

Source: CEIC and Haver.

Turning to the risks and policy challenges going forward, much attention has focused on potential effects in EMEs when recovery prompts the United States and other advanced economies to begin the gradual process of returning policy to a normal stance. As events over the summer demonstrated, even the discussion of such a policy shift may be accompanied by considerable volatility.

As shown in Chart 8, from May through August, U.S. Treasury yields rose substantially as market participants reassessed the future course of U.S. monetary policy. In response, EME bond and equity funds experienced very large outflows, as shown by the bars. EME yields rose as well, in some cases by more than those on Treasury securities, and many EME currencies depreciated. The magnitude of these market responses may have been amplified by the carry-trade strategies that many investors had in place; these strategies were designed to take advantage of interest rate differentials and appeared profitable as long as EME interest rate differentials remained wide and EME exchange rates remained stable or were expected to appreciate. When anticipations of Fed tapering led to higher U.S. interest rates and higher market

CHART 8
EME Bond and Equity Fund Flows*



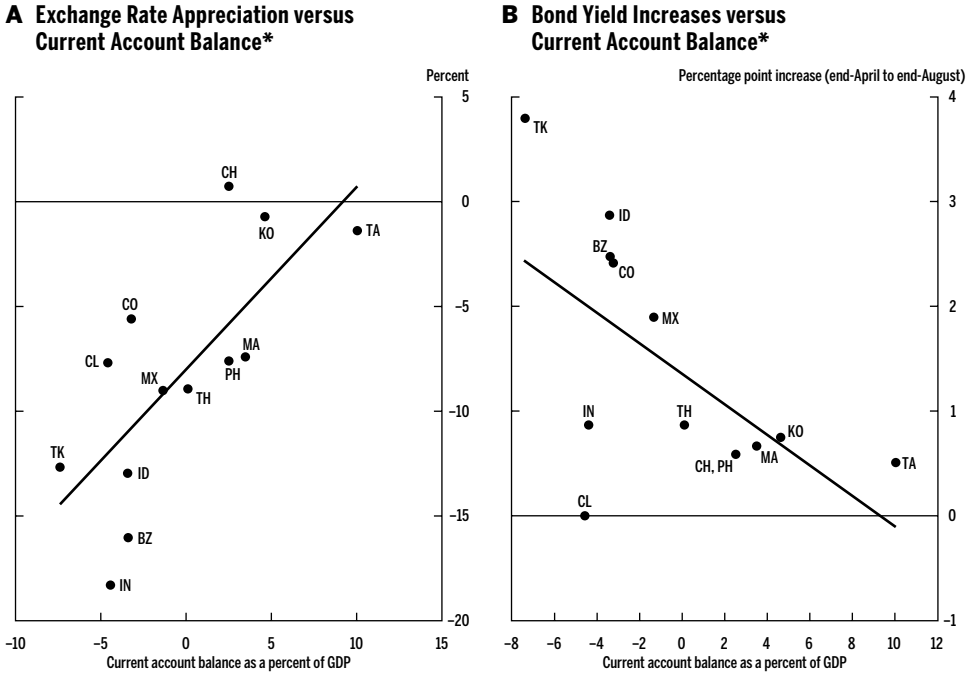
*Flows to EME-dedicated bond and equity funds.
Source: Bloomberg and Emerging Portfolio Fund Research.

volatility, these trades may have been quickly unwound, engendering particularly sharp declines in EME exchange rates and asset prices.

These developments, however, do not appear to have been driven solely by perceptions of U.S. monetary policy. As I noted earlier, GDP growth in many EMEs has fallen from the pace of previous years, which may have led investors to rethink their investment choices. Additionally, it appears that the retreat from emerging markets reflected a change in global risk sentiment, as investors focused on vulnerabilities in EMEs following a period of complacency. Asset prices have fallen considerably more in economies with large current account deficits, high inflation, and fiscal problems than in countries with stronger fundamentals. For example, as shown in Chart 9, changes in EME exchange rates and interest rates since April have been correlated with current account deficits. In general, economies with larger current account deficits experienced greater depreciations of their currencies and larger increases in their bond yields. Thus, while a reassessment of U.S. monetary policy may have triggered the recent retrenchment from EMEs, investor concerns about underlying vulnerabilities appear to have amplified the reaction.

CHART 9

Differentiation across EMEs



*Exchange rate appreciation against U.S. dollar; bond yields are 9- or 10-year local currency bond yields; 2013 current account balance is IMF *World Economic Outlook* projection.

Source: Bloomberg, Haver, and IMF *World Economic Outlook*.

Whatever their source, large capital outflows from EMEs can pose challenges for EME policymakers by simultaneously producing significant currency depreciation, asset price deflation, and inflationary pressures. In such cases, EME central banks are in the difficult position of judging whether to tighten policy at the same time that demand is weakening. It is notable that some central banks with stronger records on price stability have been able to avoid tightening, whereas others have been forced to raise rates to defend price stability in the face of domestic weakness.

Monetary policy in the United States is likely to remain highly accommodative for some time, as our economy fights to overcome the remaining headwinds from the global financial crisis. As our economic recovery continues, however, the time will come to gradually reduce the pace of asset purchases and eventually bring those purchases to a stop. The timing of this moderation in the pace of purchases is necessarily uncertain, as it depends on the evolution of the economy.

While moderating the pace of purchases and the eventual increase in the federal funds rate may well affect capital flows, interest rates, and asset prices in EMEs, the overall macroeconomic effects need not be disruptive. First, tightening will in all likelihood occur in the context of a more firmly established economic recovery in the United States so that any adverse effects on EME financial conditions should be buffered by the beneficial effects of higher external demand. Second, although conditions vary from country to country, on the whole, EMEs exhibit greater resilience than they did in prior decades, reflecting, among other factors, more flexible exchange rates, greater stocks of international reserves, stronger fiscal positions, and better regulated and more conservatively managed banking systems.

EMEs have policy tools to help manage any negative externalities that may arise, and recent developments provide additional rationale for them to redouble their efforts to bolster their resiliency.[14] Reducing vulnerabilities, improving policy frameworks, and safeguarding the financial sector will go a long way toward making EMEs more robust to a wide range of shocks, not just those that may arise from changes in monetary policy in the advanced economies. Global investors should also learn from the experience of this summer, when it became clear that unwinding leveraged carry trades can be difficult in an environment of lower liquidity.

As for advanced economies, policymakers should move gradually to restore normal policies only as their economic recoveries are more firmly established, consistent with their mandates. In addition, policymakers should communicate as clearly as possible about their policy aims and intentions in order to limit the odds of policy surprises and a consequent sharp adjustment in financial markets in response. Indeed, my colleagues on the Federal Open Market Committee and I are committed to just such an approach.

In closing, the Federal Reserve's mandate, like those of other central banks, is focused on the pursuit of domestic policy objectives. This focus is entirely appropriate. Yet, experience has shown that the fortunes of the U.S. economy are deeply intertwined with those of the rest of the world. Economic prospects for the United States are importantly influenced by the course of the world economy, and, by the same token, prosperity around the globe depends to a significant extent on a strong U.S. economy. In order for the Federal Reserve to fulfill its dual mandate of price stability and maximum employment, we must take account of these international linkages. Indeed, the Federal Reserve has a long and varied history of doing so, including our actions during the global financial crisis. There is every reason to expect that to continue.[15]

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NOTES

1 I would like to thank Trevor Reeve for his assistance in the preparation of these remarks.

2 Notable examples of such crises include Latin America in the early 1980s, Mexico in 1994, the Asian financial crises beginning in 1997, Russia in 1998, Argentina in 2001, and Brazil in 2002. See Broner et al. (2013), Forbes and Warnock (2012), Ghosh et al. (2012), and Reinhart and Reinhart (2009) for discussions of large capital flow movements.

3 An earlier literature also examined the role of "push" and "pull" factors in explaining international capital flows. Examples include Calvo et al. (1993, 1996), Fernandez-Arias (1996), and Chuhan et al. (1998).

4 See Glick and Leduc (2013), IMF (2013a), Moore et al. (2013), Rosa (2012), and Wu and Xia (2013). Recent research by Federal Reserve Board staff finds that reductions in U.S. interest rates for any reason—whether caused by monetary policy or other factors—have typically been associated with declines in EME interest rates and appreciation of EME currencies. Moore et al. (2013) document a similar historical relationship.

5 The U.S. economy is affected by spillovers from abroad as well, and these are very much a part of our policymaking environment.

6 See, for example, Ahmed and Zlate (2013), Bluedorn et al. (2013), Fratzcher et al. (2013), Ghosh et al. (2012), and IMF (2011).

7 See Chen et al. (2012), Fratzscher et al. (2013), Hausman and Wongswan (2011), IMF (2013b), and Moore et al. (2013).

8 See Ahmed and Zlate (2013), Forbes and Warnock (2012), Fratzcher et al. (2013), and Ghosh et al. (2012).

9 See Ahmed and Zlate (2013), Bluedorn et al. (2013), Forbes and Warnock (2012), and IMF (2011).

10 See Bruno and Shin (2013) and Rey (2013).

11 See Bernanke (2013).

12 See IMF (2013a,b).

13 See IMF (2013a).

14 See Sanchez (2013).

15 See Eichengreen (2013).

GENERAL DISCUSSION
Advanced Economy Monetary Policy and Emerging Market Economies

Mr. Kashyap: Can you share any of your thinking about where you think risk-on, risk-off investor behavior comes from and to what extent Fed communications have anything to do with that?

Mr. Powell: Well, as you know, there's a good bit of recent research that suggests that monetary policy, including Fed communications, may plan some role in this activity. But clearly other factors—such as developments abroad, particularly in Europe over the period I showed in that chart—have been very important in driving risk-on, risk-off activity, probably more importantly than monetary policy signals.

Mr. McKinnon: I enjoyed your talk very much. I like your charts. But there was a missing chart on commodity prices. After about 2008, when the Fed reduced the interest rate to zero, and in 2002, when it reduced the rate to 1 percent, there were huge outflows of hot money. As you mentioned, emerging markets tend to lose monetary control during these periods. They intervened to prevent their currencies from appreciating, lost control of their money supplies, and collectively inflated. This inflation shows up in primary commodity prices. We had a big primary commodity bubble in 2002 until it crashed in 2008. And then it started up all over when the Fed cut rates again in 2008, and there was another big outflow of hot money. And with the second big outflow there was also a commodity bubble. In 2010 the price of food doubled. I think this is a very serious consequence of zero interest rates in the United States. And the 2010 doubling of food prices explains the Arab spring as a food riot.

Mr. Powell: So let me offer a couple of comments. First, I think that capital flows are volatile, always and everywhere for everybody. The point for emerging market economies is the need to strengthen their institutional structures by a range of policy actions, including floating exchange rates, better fiscal outcomes, more credible central banking, better regulations in the financial markets, and perhaps more openness to two-way flows from domestic investors. You're always going to have volatile flows. It's very important that some emerging market economies have followed this path, because they got better outcomes.

On commodity prices, I just don't agree with you. I think there's more evidence that commodity prices were driven by other macro factors as opposed to monetary policy, particularly the rise of China and the growth of China and developing Asia.

How do we account for commodity prices falling in the last couple of years, when there's been a lot of monetary policy accommodation? But thank you for your question.

Mr. Wolf: I'm very sympathetic to your view. But I'll ask the question in a slightly different way. Effectively, most everybody outside the United States thinks the Fed sets monetary policy for the world. You said completely correctly, the Fed's purpose is to set monetary policy for the United States. In what way and under what circumstances do you think a serious conflict arises between the Fed fulfilling its mandate and what a central bank that is actually running monetary policy for the world would do?

Mr. Powell: That's a good question. The best answer I can give you is that it's reasonable to expect us to be transparent and to move gradually when it is time to withdraw accommodation, and when we begin reducing the pace at which we add accommodation. Also, that we hold to our obligation to only do that as demand strengthens in the United States. Those are the things that we can do and must do and should do. Having said that, we have a domestic mandate. And that is what we have to observe. I'm not going to try to dream up hypotheticals in real time of how that could conflict with global interests. But I'm sure you could. Perhaps you could do that at your lunch presentation. Thanks for your question.

Asiaphoria Meets Regression to the Mean

Lant Pritchett and Lawrence Summers

Consensus forecasts for the global economy over the medium and long term call for a substantial shift of economic gravity towards Asia and especially towards the Asian giants, China and India. While such forecasts may pan out, there are substantial reasons to expect that growth in China and India will be much less rapid than is currently anticipated. Most importantly history teaches that while economic forecasts invariably extrapolate recent growth, abnormally rapid growth is rarely persistent. Indeed regression to the mean is the empirically most salient feature of economic growth, showing far more robustness in the data than, say, the much-discussed middle-income trap. Furthermore, statistical analysis of growth reveals that in developing countries, episodes of rapid growth are frequently punctuated by discontinuous drop-offs in growth. Such discontinuities account for a large fraction of the variation in growth rates. We suggest that salient characteristics of China—high levels of state control and corruption along with low measures of authoritarian rule—make a discontinuous decline in growth even more likely than general experience would suggest. China's growth record in the past 35 years has been remarkable, and nothing in our analysis suggests that a sharp slowdown is inevitable. Still, our analysis suggests that forecasters and planners looking at China would do well to contemplate a much wider range of outcomes than is typically considered.

1. Introduction

The rise of Asia is a story in at least four parts, with the fourth yet to be written. The first is the dramatic rise of Japan before and after World War II, ultimately to a prosperous and productive economy and global leader by the late 1980s. The second is the rise beginning in the 1960s of the East Asian Dragons—led by the four "Asian Tigers" of Korea, Taiwan, Singapore, and Hong Kong and followed by the three larger economies of Southeast Asia, Malaysia, Indonesia, and Thailand. The third is the rise of the Asian giants with populations of over one billion. China and India each have more than twice the population of the other eight East Asian economies combined.

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At least since the 1980s, economic growth accelerated in both China and India and then, surprisingly given usual historical patterns, accelerated again in both countries in the 1990s. That was followed by another acceleration in India in the mid-2000s (Kar et al. 2013). The power of compound interest over long periods at high rates plus their sheer scale in population have led both economies to become global economic powerhouses. In purchasing power parity data (PPP) from the Penn World Tables (PWT) 8.0 (Feenstra, Inklar and Timmer 2013), the three largest economies in the world in 2011 are the United States, China, and India. China's economy is now, again at PPP, roughly three times Japan's and four times Germany's.[1]

The fourth stage of this Asian story, the future, is unknown. Extrapolating a decade or two into the future—based on recent growth rate differentials between China and India, the modest post-crisis growth of the United States, and the even more modest recent growth in Europe—produces an Asiaphoria, the view that the global economy will increasingly be shaped and lifted by the trajectory of the giants. Combined with continued growth in the other large Asian economies that still have low to middle incomes—for example, Vietnam, Indonesia, and Thailand—the vision of the global economic center of gravity shifting even more decisively to Asia becomes destiny.

Asiaphoria has become almost conventional wisdom. *Looking to 2060: Long-Term Global Growth Prospects* (OECD 2012) forecasts per capita growth from 2011 to 2030 for China of 6.6 percent and for India, 6.7 percent. In *China 2030* the World Bank (2012) and the Development Research Center of the State Council of China project output per worker growth rates of 8.3 percent from 2011 to 2015, 7.1 percent from 2016 to 2020, and 6.2 percent from 2021 to 2025. In its official National Intelligence Estimates projected out to 2030, the U.S. intelligence community presents scenarios implying China's share of the world economy will grow from 6.4 percent in 2010 to between 17 and 23 percent in 2030; for India the estimates for the same periods are growth building from 1.8 percent of the world economy to between 6.5 and 7.9 percent. And these are cautious contrasted with Fogel's (2010) prediction that China's GDP will reach US$123 trillion by 2040.

Our principle contribution is a rigorous quantitative demonstration that with respect to economic growth—just as investment firms warn is true about returns—past performance is no guarantee of future performance. *Regression to the mean* is perhaps the single most robust and empirical relevant fact about cross-national growth rates. The lack of persistence in country growth rates over medium- to long-run horizons implies current growth has very little predictive power for future growth. Hence, while it *might* be the case that

China will continue for another two decades at 9 (or even 7 or 6) percent per capita growth, given the regression to the mean present in the cross-national data, where historically the distribution of growth has been an average of 2 percent with a standard deviation of 2 percent, this would be an extraordinary tail event. Similarly, while it *might* be the case that Indian growth continues at 6 percent, this would require India's extended growth, already rare, to persist even longer and become rarer still.

Many of the great economic forecasting errors of the past half-century came from excessive extrapolation of performance in the recent past and treating a country's growth rate as a permanent characteristic rather than a transient condition. Paul Samuelson's textbook predicted in 1961 that there was a substantial chance that the USSR would overtake the United States economically by the 1980s. There was a widespread view right up until the end of the 1980s that Japan would continue to outcompete the world. Or in the opposite direction, consider the pervasive pessimism of a decade ago regarding Africa. Since then, African countries emerged as a majority of the world's most rapidly growing nations.

In addition to demonstrating that past growth performance is of very little value for forecasting the *central tendency* of future growth, we also show that in developing countries the growth process is marked by sharp discontinuities, with very large accelerations or decelerations of growth being quite common. This implies that the explicit (or implicit) confidence intervals in typical forecasts or the range of growth that scenarios consider might dramatically underestimate the actual range of outcomes. The recent crisis has again alerted us to the fact that risks of downside scenarios are often vastly underestimated,[2] just as the fragility in systems is underestimated. Moreover it appears that particular aspects of China's situation—a high degree of government discretion vis-à-vis businesses and an authoritarian regime—add to the likelihood of a growth slowdown.

Our paper is organized as follows. Section 1 presents the basic evidence on regression to the mean in country growth rates and shows how taking account of this evidence leads to forecasts for Chinese and Indian growth that are much more pessimistic than consensus views. Section 2 demonstrates the robustness of the conclusion to a variety of specifications. Section 3 draws on recent work by Kar et al. (2013) that extends work on "stop-start" growth (e.g., Rodrik 1999 and Jones and Olken 2008) and shows the extent to which the growth process is marked by changes in "growth regimes" with large accelerations and decelerations. This is a very different view than the standard trend-cycle model used in industrial country macroeconomics, but it appears to be much more descriptive

of developing countries where "the cycle is the trend" (Aguiar and Gopinath 2007). We show that rapidly growing countries are substantially more likely to suffer a downward discontinuity in growth than an upward movement. Our analysis also suggests that growth declines are more likely to be sudden and large than gradual and small. Section 3 also demonstrates that, in considering China's prospects for continued rapid growth, the much-discussed middle income trap is less a fundamental empirical issue than a simple regression to the mean (if, properly measured, it even exists). Finally, Section 4 considers two qualitative aspects of the Chinese situation—China's high degree of dependence on discretionary policies towards business and its authoritarian character. We show that both make sharp declines in growth more likely. A final section concludes and discusses some implications of the results.

2. The $42 Trillion Question: Will Rapid Growth in China and India Persist?

2.1. Regression to the Mean: The Single Most Robust Fact about Growth

The 1990s saw an explosion of "growth regressions" which placed the growth of gross domestic product per capita (GDPPC) over some period on the left-hand side and everything but the kitchen sink on the right (Wacziarg 2002 and Rodriguez and Shelton 2013).[3] We are not going to characterize what was "learned," as the methodological sensitivity of growth regression findings about particular variables was an issue raised early (Levine and Renelt 1992) and often: Nearly every assertion about correlates (or causes) of growth emerging in any study has been challenged as not robust in a later study.

However, one fact about growth that emerged early—including a paper of ours with Bill Easterly and Michael Kremer (Easterly et al. 1993)—has stood the test of time and new data: There is strong regression to the mean in the growth process, hence very little persistence in country growth rate differences over time, and consequently current growth has a low predictive power for future growth. Although one might have thought that most of the long horizon growth differences were due to the existence of fast and slow growing countries (e.g., Argentina grows slow and Japan grows fast)—the opposite is true and nearly all growth variation is due to differences within countries over time.

Table 1 presents four measures of persistence: the correlation, the rank correlation (to reduce the influence of outliers), the regression coefficient of current growth on lagged growth, and the R-squared of the regression (which is of course the square of the correlation coefficient). We use the PWT8.0 (Feenstra, Inklar, and Timmer 2013) data on local currency real GDP from national

TABLE 1
Little Persistence in Cross-National Growth Rates across Decades

Period 1	Period 2	Correlation	Rank Correlation	Regression Coefficient	R-squared	N
Adjacent decades						
1950–60	1960–70	0.363	0.381	0.378	0.132	66
1960–70	1970–80	0.339	0.342	0.382	0.115	108
1970–80	1980–90	0.337	0.321	0.323	0.114	142
1980–90	1990–2000	0.361	0.413	0.288	0.130	142
1990–2000	2000–10	0.237	0.289	0.205	0.056	142
One decade apart						
1950–60	1970–80	0.079	0.192	0.095	0.006	66
1960–70	1980–90	0.279	0.312	0.306	0.078	108
1970–80	1990–2000	0.214	0.214	0.163	0.046	142
1980–90	2000–10	0.206	0.137	0.143	0.043	142
Two decades apart						
1960–70	1990–2000	0.152	0.177	0.152	0.023	108
1970–80	2000–10	–0.022	0.005	–0.015	0.001	142

Source: Author's calculations with Penn World Tables (PWT8.0) data (Feenstra, Inklaar, and Timmer 2013).

accounts (since we are not yet comparing levels) and population to compute real GDPPC. We compute least-squares growth rates of natural log GDPPC for 10 and 20 year periods for all countries with sufficient data.[4] The results show that the low persistence of growth has been a consistent and robust characteristic across all decades—if anything there is less persistence in the recent decadal growth rates (1990–2000 to 2000–10) than in previous decades.[5] Not surprisingly, the persistence declines over longer periods so that using current growth rates to predict two decades ahead has even less predictive power than predicting one decade ahead.

The results in Table 2 using growth rates over 20 year periods—which smooth even more over "cyclical" fluctuations—are similar in showing strong regression to the mean, low persistence, and low predictive power of current growth for future growth.

For the question at hand—Will the rapid growth rates of the Asian giants continue into coming decades as an engine of global growth?—the most relevant summary statistics are the regressions.

First, knowing the current growth rate only modestly improves the prediction of future growth rates over just guessing it will be the (future realized) world average. The R-squared of decade-ahead predictions of decade growth varies from 0.056 (for the most recent decade) to 0.13. Past growth is just not that informative about future growth and its predictive ability is generally even lower over longer horizons.

TABLE 2
Twenty-Year Periods Show Modest Persistence; Hence Current Growth Has Little Value for Predicting Future Growth

Period 1	Period 2	Correlation	Rank correlation	Regression coefficient	R-squared	N
Adjacent two decade periods						
1950–70	1970–90	0.258	0.318	0.343	0.067	70
1960–80	1980–2000	0.459	0.454	0.494	0.211	108
1970–90	1990–2010	0.327	0.325	0.215	0.107	142
Gap of two decades						
1950–70	1990–2010	0.047	0.015	0.047	0.002	70

Source: Authors' calculations with PWT8.0 data (Feenstra, Inklaar, and Timmer 2013).

Second, if all we knew was a country's current growth rate then what would be the best prediction of the future? The extremes are *extrapolation*, a coefficient of 1, and *exclusion*, a coefficient of zero. Our estimates imply that the coefficients are around 0.3 for decade-ahead predictions and lower if current decades are used to predict further ahead, 0.2 or less.

Essentially what is being asserted here is the equivalent of the *Time* magazine cover curse. It has been observed that public figures who appear on the cover of *Time* often suffer a career reversal soon afterwards. This is just what one would expect with mean reversion and extrapolative expectations. Those who perform best in period t will on average perform much worse than expected in period $t+1$.

At a deeper level, the finding of high mean reversion in growth rates has profound implications for the study of economic growth. If it were the case as many models suggest that some relatively constant feature of countries—their climate, their culture, the quality of their institutions, or their openness to the world as examples—influenced growth, importantly one would expect since these variables persist that growth rates would persist. That growth rates do not persist suggests that factors of this kind should be analyzed as affecting the level but not the long-run growth of incomes. This suggests that, unless a country can either continually improve its policy environment or its governance, even the most favorable conditions will ultimately have diminishing impacts on growth.

2.2. Forecasting the Future Level of GDP in the Giants

What are the mechanical implications for the predicted growth of dollar GDP of China and India of "extrapolation of current growth" versus regression to the mean? By "mechanical" we just mean, what we would *expect* to happen if we did not know anything about China or India and just treated them as if they would follow the statistical regularities that apply to other countries?

We create predictions of growth rates in future decades using regressions that predict countries' growth rates based on their past decades' growth (and their initial levels of income in PPP to allow for convergence). Predictions then just plug China's and India's current growth rates and levels of income into that equation and roll these predictions forward for two decades. The basic idea (on which we experiment with many variants) is to estimate equation (1):

$$g^{i}_{00-10} = \alpha + \beta * g^{i}_{90-00} + \gamma * \ln(y^{i}_{00}) + \varepsilon^{i}, \tag{1}$$

and then predict growth ahead for two decades using the estimated coefficients and the actual values for China and India for the first decade and the predicted growth (and consequent level) for the first decade in predicting the second:

$$gp^{China}_{13-23} = \hat{\alpha} + \hat{\beta} * g^{China}_{00-10} + \hat{\gamma} * \ln(y^{China}_{2010})$$

$$gp^{China}_{23-33} = \hat{\alpha} + \hat{\beta} * gp^{China}_{13-33} + \hat{\gamma} * \ln(yp^{China}_{2023}).$$

Table 3 shows the results of a variety of simple "regression to the mean" regressions, with and without convergence terms, with and without two decades of lags, and for 10 versus 20 year time periods. Not surprisingly given the robustness of weak persistence as a feature of growth rates demonstrated above, all regressions produce coefficients on lagged growth between 0.20 and 0.32.

Because our primary interest is the impact on the global economy, we predict total GDP in dollars (not PPP adjusted) for China and India over the next two decades.[6] To predict population we use the United Nations Medium Fertility projections, which show China's population growth near zero while India's continues to grow about 1 percent per year over the next decade and then slows.

We start the scenarios using the International Monetary Fund (IMF) *World Economic Outlook* 2013 U.S. dollar GDP (which is somewhat a forecast, but, for instance, already includes the depreciation of the rupee in 2013). We compute total dollar GDP for 2023 and 2033 by simply using an assumed growth rate of GDP per capita and then multiplying by population.

The results are at the same time obvious and striking. If one assumes a continuation of current growth rates, the 20 year gain in GDP from 2013 to 2033 in China would be $51.1 trillion (from $8.9 to $60 trillion), which would be a *gain* in GDP more than three times as large as the current U.S. economy. The continuation of current growth rates would make China far and away the world's dominant economy. The gain in India would be smaller (as it begins from a lower base and at a lower growth rate, of 6 percent) but still rises to a substantial $6.8 trillion for a *gain* of $5.1 trillion (the current size of France and Italy combined).

TABLE 3
Regressions of Decade Growth Rates on Past Decade Growth Rates, Allowing for Lagged Level of Income

Dependent variable		Constant	Lagged growth	Second lag of growth	Initial Level of GDPPC	R^2	N
Growth 2000–10	Coefficient	0.023	**0.205**			0.056	142
	t-stat	10.758	2.887				
Growth 2000–10	Coefficient	0.068	**0.329**		–0.006	0.177	142
	t-stat	6.632	4.572		–4.519		
Growth 2000–10	Coefficient	0.074	**0.274**	**0.161**	–0.006	0.222	142
	t-stat	7.227	3.749	2.812	–5.135		
Growth 1990–2000	Coefficient	–0.009	**0.240**	**0.045**	0.003	0.157	142
	t-stat	–0.665	3.561	0.683	1.679		
Growth 1990–2010	Coefficient	0.031	**0.241**		–0.001	0.117	142
	t-stat	3.164	4.272		–1.243		

Source: Authors' calculations with PWT8.0 data.

Even if one assumes growth slows in China to 7 percent its total GDP grows to $36 trillion—more than twice the current U.S. level.

However, it is also obvious that regression to the mean of the ordinary type would reduce these gains massively. Under any of the empirical estimates for "regression to the mean," the level of China's GDP in 2033 would fall to around $20 trillion—which still implies a 20 year increase in GDP of around $11 trillion. Similarly, the gains in India fall from $5 trillion to between $2.4 and $3.3 trillion. It is noteworthy that the forecasts based on past growth and levels predict growth that is closer to the naive expectation that China and India will grow like average countries than to extrapolations of their past growth.

There is some consensus that China will not maintain 9 to 10 percent growth rates, but even the view that China's growth will slow to something like 7 percent assumes substantial persistence (Table 4). The predicted growth over the next two decades using regressions is 3.89 percent (with a coefficient on past growth of 0.24), and the regression standard error of estimation is 1.6 percent, so a continuation of even 7 percent is two standard deviations in the tail, and a continuation of a growth rate of 9 percent is three standard deviations.

Table 5 shows that whether or not China and India will maintain their current growth or be subject to regression to the global mean growth rate is a 42 trillion dollar question. The difference between the "continuation" scenario in 2033, in which the GDP of China plus India gains $56 trillion, and the average of the "regression to the mean" scenarios (which are all quite similar, with total China plus India 2033 GDP between $12 and $15.5 trillion) is $42 trillion dollars. The 7 percent scenario shows a gain of $33 trillion versus $13 trillion of the average of the regression to the mean scenarios.

TABLE 4

Scenarios Predicting Future Growth Rates Using Regressions Allowing for Regression to the Mean and Convergence at 10- or 20-year Horizons

Scenarios		China (2013 GDP=$8,939 bn)		India (2013 GDP=$1,758 bn)	
		2023	2033	2023	2033
Continuation of 2000–10 growth	Growth GDPPC	9.74%	9.74%	6.01%	6.01%
	GDP (billions)	$23,592	$60,034	$3,508	$6,804
Growth at 7 percent	Growth GDPPC	7.00%	7.00%	7.00%	7.00%
	GDP (billions)	$18,329	$36,238	$3,849	$8,188
Falls to 2 percent (full regression to mean)	Growth GDPPC	2.00%	2.00%	2.00%	2.00%
	GDP (billions)	$11,358	$13,915	$2,385	$3,144
Predicted growth, 10 years, one lag, convergence term	Growth GDPPC	5.01%	3.28%	4.24%	3.92%
	GDP (billions)	$15,198	$21,100	$2,963	$4,708
Predicted growth, 20 years, convergence term	Growth GDPPC		3.89%		3.00%
	GDP (billions)		$20,077		$3,820

Source: IMF WEO dollar GDP for 2013 base case, PWT8.0 for 2000–10 growth for China and India, UN Medium variant for population in 2023 and 2033, authors' regressions in Table 4 for predicted growth rates 2013–23 and 2023–33 (or 2013–33).

TABLE 5

The Difference in Cumulative GDP Gains over 20 Years Is $42 Trillion between the "Continuation of Current Growth" and Estimated "Regression to the Mean"

Scenarios	*Gain* in 2033 over 2013		
	China	India	Total
Continuation of current rates (zero regression to mean)	**$51,095**	**$5,046**	**$56,140**
Growth at 7 percent	**$27,299**	**$6,429**	**$33,728**
Regression to 2 percent per year	$ 4,976	$1,386	$ 6,362
Predicted regression to the mean 10 years, no convergence	$10,382	$2,591	$12,973
Predicted regression to the mean, 10 years, with convergence	$12,160	$3,304	$15,464
Predicted regression to the mean, 20 years, with convergence	$11,137	$2,416	$13,553
Average of three predicted "regression to mean" scenarios	**$11,227**	**$2,770**	**$13,997**
Difference in gains to dollar GDP of China and India between the "continuation" and "regression to mean" scenarios	**$39,868**	**$2,275**	**$42,144**

This obviously affects the world growth rate substantially—even in the absence of any feedback effects on the rest of the world's economies. Table 6 shows the evolution of the world total GDP assuming the rest of the world grows steadily at 2 percent, reaching $93 trillion in 2033. If China and India continued at their current rate, they would reach over $66 trillion and hence just mechanically the annual growth rate of world GDP is 3.5 percent and then 4.45 percent in the next two decades (accelerating just because India and China mechanically have a larger share of the total). Conversely, with regression to the mean scenarios for China and India, the global growth rate is 2.48 percent and 2.27 percent.

TABLE 6
Mechanically, If the World Grows 2 Percent per Year and China and India Continue They Are a Larger and Larger Share of Global GDP and Growth of Global GDP Is Higher

Growth	2013	2023	2033
World GDP in Dollars	73,454.49		
Less India and China, 2 percent growth	$62,757	$ 76,500	$93,254
China and India GDP at current growth rates		$ 27,100	$66,838
World with China and India at current growth rates		$103,601	$160,091
Growth rate of global GDP		3.50%	4.45%
China and India level of output with growth rates that show typical regression to the mean		$ 17,325	$ 24,224
World with slower China and India (no linkages)		$ 93,825	$117,478
Growth		2.48%	2.27%

Of course this mechanical calculation underestimates the role of China and India as growth engines by assuming that other country growth rates are not raised by faster growth in the giants. To the extent there are positive linkages, then this mechanical calculation underestimates (perhaps substantially) the impact on global growth of regression to the mean.

We are trying to reverse the default assumptions often made in forecasting GDP, which is that, in the absence of any reason to think otherwise, the current growth rate persists. In this view what has to be justified with argumentation is why the growth rate would decelerate. However, this mode of forecasting or projection or even formulation of scenarios is counterfactual to the single most robust fact about growth rates, which is strong reversion to the mean.

Our argument is that the default prediction/projection/forecast should be that a country's growth rate will be subject to regression to the mean. What has to be justified is why the growth rate would persist at rates higher (or lower) than the world mean growth rate.

For instance, in addressing the current question of whether Asia—and necessarily China and India as part of that—will be an engine of global growth over the future (not the short run of one to three years but the longer run of five to twenty years) our guess is that growth will slow, substantially, in those countries. Why will growth slow? Mainly, because that is what rapid growth does. Our confidence in the prediction that growth will slow is much larger than our confidence in being able to specify why or how or when exactly it will slow.

But this is like all other regression to the mean phenomena. If a hitter has a hot streak with a batting average up 50 points over the past 20 at bats, then we would forecast a return to the average batting average over the next 20 at bats

(perhaps not exactly to the mean, but substantial regression). If pressed to say why the batting average would be lower, one could speculate about why it currently is so high and predict those factors will diminish or predict future events will causally explain the lowering, but mainly, that is just what happens.

One might, at this stage, suspect us of attacking a straw man on two levels. First, no one really ignores regression to the mean in making forecasts. Second, the bullish views of growth in China and India have already softened considerably.

While few agencies explicitly engage in very long-run forecasting, the October 2013 IMF *World Economic Outlook* (WEO) provides forecasts of GDP per capita in constant prices out to 2018 (Table 7). These forecasts reflect the current view that China's growth rate will soften but will remain more than two standard deviations above the historical cross-national averages. Compared with the regression to the mean in the data, this is still substantially higher. In the case of India, the IMF WEO forecast shows almost no regression to the mean.

Of course these are not long-run forecasts as they are only five calendar years ahead, but they reflect substantially more regression to the mean and predictability than actual outturns. Figure 1 shows all of the 185 countries in the IMF WEO data plotted as the geometric average of their reported 2014–18 growth rates and their prior actual growth rates. The lines show no regression to the mean, the actual in the forecasts, and the historical actual regression to the mean. Not at all surprisingly, the forecasts tend to show substantially more persistence and predictability of growth than the historical data over similar periods. The regression of actual growth 2004–08 on actual growth 1993–2002 gives a coefficient of 0.255 (standard error of 0.128) and R-squared of 0.04 (similar to the results above, just adjusted to comparable periods of the forecast for comparison). The forecasts 2014–18 on growth 2003–12 has a slope of 0.481 (standard error of 0.072) and R-squared of 0.263.

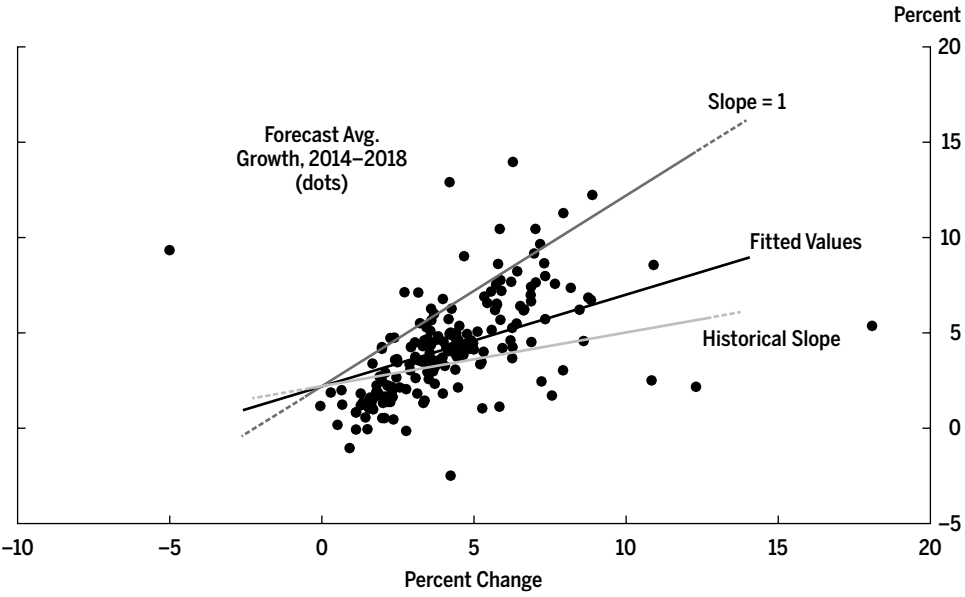
TABLE 7

IMF October 2013 WEO Forecasts of GDPPC Growth for Asian Countries Predict the Continuation of Rapid Growth until the End of the Forecast Period

	2000–11	2014–18
China	9.76%	6.47%
India	5.93%	5.14%
Indonesia	3.87%	4.51%
Vietnam	5.58%	4.38%

Source: Download of data from http://www.imf.org/external/pubs/ft/weo/2013/02/weodata/ of GDP per capita constant prices, national currency. Calculation of geometric growth rate over the periods.

FIGURE 1
IMF Forecasts Show Substantially More Persistence in Growth Rates than Historical Data



Source: Authors' calculations using data from http://www.imf.org/external/pubs/ft/weo/2013/02/weodata/ on October 30, 2013.

One argument against the predictability of long-run growth is that it has in fact been possible to predict the per capita level of GDP far ahead. Suppose all you knew was that Denmark's GDPPC measured in 1990 Geary-Khanis dollars was in 1910 GK$3,891 and that its per capita annual rate of growth during the pre-World War I period of 1890–1916 was 1.90 percent, and someone asked you to forecast GDPPC in Denmark almost 100 years ahead to 2010 using only pre-World War I information. While this might seem pointless, you could venture a guess that it was the simple extrapolation of exponential growth at GK$23,302.[7] Turns out, you would be right, exactly right. Actual GDPPC was GK$23,513. The 94-year-ahead forecast of GDPPC was off by about $200—less than 1 percent. The long-run stability of growth in OECD countries is well-known[8] to all economists, so well-known that it may cause misleading habits of thought. The leading countries have very stable growth rates (averaged over long periods) for a very long time.[9] The high levels of income in the United States and others are the power of compound interest of a modest growth rate sustained over a very long time. However, the apparently reliable prediction of the future is an artifact of growing near the mean growth rate so that extrapolations into the future

and regression to the mean worked together. But in extrapolating growth rates, regression to the mean almost always wins.

3. Robustness of Predicting Future Growth: Years, Levels, Previous Growth, Country Size

The first section has the virtue of simplicity: We compare forecasts with extrapolation to historically observed degrees of regression to the mean in a way that the simple framing of full persistence (extrapolation) is a coefficient of one and no persistence is zero. However, we want to reassure readers that the simple results are robust. In this section we address four issues: (a) whether country predictability either increases with the use of longer past lags in growth as they may produce better estimates of long-run growth, (b) whether predictability has become better over time, (c) whether regression to the mean is asymmetric such that growth booms are more likely to be sustained than growth busts, and (d) whether growth is more predictable in large than in small countries.

3.1. Variation in Growth Predictability over Lags, Leads, and Time

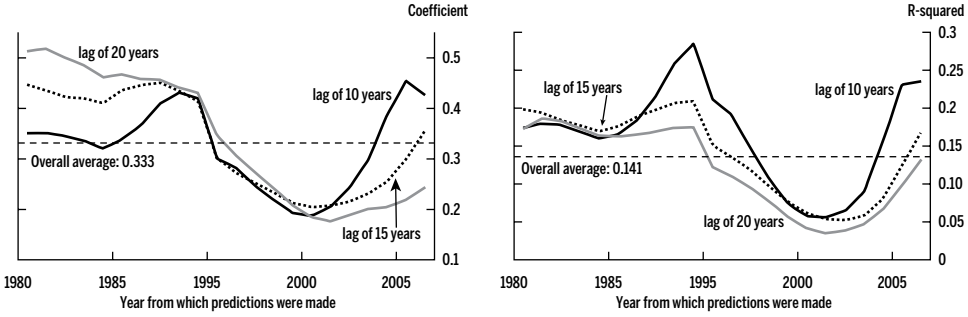
We generalize equation 1 to allow the window of past data (N_b) and the length of the forecast (N_f) to vary.[10] This tests whether the low persistence is an artifact of some particular phase of global growth dynamics or a truly robust feature of the data:

$$g^i_{t,t+N_f} = \alpha_t + \beta_t * g^i_{t,t-N_b} + \gamma_t * \ln(y^i_t). \tag{2}$$

The results of estimating this equation across all available countries (constrained so that the country sample is the same for all lags of N_b) are shown in Figure 2. Averaged over all years and across lags of 10, 15, and 20 years, the regression coefficient for predicting growth 10 years ahead is 0.333. Hence the value of 0.329 for the 10-year-ahead prediction with convergence term in Table 3 is neither atypically high nor low. The predictive power of this simple regression is low, averaging 0.141 and is consistently less than 0.25 so that knowing the present is not that informative about the future.

There is some time variation as growth became less predictable based on previous growth in the first half of the 1990s with some recovery in predictability in the late 1990s/early 2000s. Longer lags (perhaps capturing more permanent features of a country's growth) have more predictive power than 10-year lags in the 1980s but with the opposite result more recently. As detailed in the opening section, regression to the mean or lack of persistence is a robust finding over time and, while it varies, there has been no secular trend to greater

FIGURE 2
Regressions Predicting Future on Past Growth Show Consistently Low Predictability



Source: Authors' calculations with PWT8.0 data.

persistence. Using the same lags of 10, 15, and 20 years to forecast 20 years ahead (which constrains the t to be between 1980 and 1991 to have sufficient data) produces almost exactly the same average persistence coefficient of 0.33.

One feature of these regressions that does vary over time is the coefficient on the level of income. As is well known, *conditional* convergence is a feature of cross-national growth rates—at least if you use the "right" conditioning variables (Barro 1991). But there is also (until recently) absolute divergence (in both levels and natural logs) as the cross-national variance increased over time (Pritchett 1997). Since by design these regressions are conditional only on past growth and initial level of income, the coefficient on lagged income is more like an unconditional than conditional convergence coefficient. Hence during most of this period the coefficient on lagged income was actually small and positive (divergence). As is also well known, the financial crisis and the slowdown in the OECD countries led to more rapid growth in the developing countries. Hence towards the end of the period the lagged level of income actually predicted substantially faster growth in the poorer countries. For 2001, the latest year in which we could run 10-year-ahead regressions, this effect added 1.5 percent to the predicted annual growth of China and 2.0 to India. So, to a very large extent the regression to the mean effects that predict slower growth are offset in the most recent regressions by a historically atypically large unconditional (on policy variables) convergence term.

3.2. Does Economy or Country Size Matter for Persistence?

China and India have continental scale in size and in population. In population, each was three times larger than the United States and twice as large as the

European Union. This leads many to be skeptical as to whether their growth dynamics will be well predicted from cross-national regressions which, even when excluding the tiny economies, contain countries with an average population less than a typical Indian or Chinese province. However, it is far from obvious that rapid growth episodes are more stable in larger economies measured by either total GDP or population; for example Brazil in 1980 and Japan in 1991, both very large countries, had massive decelerations from rapid growth to extended stagnation, not to mention the recent crisis in the United States.

This empirical question is difficult to address because the usual approach of allowing for interaction terms in size has one of two limitations. Either China and India are included in the regressions, in which case they are often influential data points, or they are excluded, which means that predictions from interactions of size have to extrapolate well out of sample. We choose the latter approach and extend our simple equation to allow for an interaction of persistence with size, now pooling across time.

$$(3) \qquad g^i_{t,t+N_f} = \alpha + \beta * g^i_{t,t-N_b} + \gamma * \ln(y^i_t) + \delta * \ln(S^i_t) + \varphi * \ln(S^i_t) * g^i_{t,t-N_b} + \epsilon^i .$$

As a proxy for size we use either total GDP in PPP or population. From samples excluding India and China, the estimated coefficient φ is negative and statistically significant, implying that large countries have less persistence. Using either proxy for size, the predicted annual growth rates for the coming decade for both China and India are in the 3 to 4 percent range (Table 8).

3.3. Asymmetry of Persistence: Do Booms Last while Busts Revert?

The question for China and India is primarily the persistence of an already extended episode of rapid growth. It is possible that the reversion to the mean on average is that countries with busts—that is, episodes of low growth—tend to recover to the mean while episodes of rapid growth are more extended. We explore this possibility with the simple exercise of allowing the regression in

TABLE 8
Predicting Growth Rates Allowing for Interactions of Growth Persistence and Country Size

Proxy for size in equation (3) (ln)	China		India	
	10 yrs ahead	20 yrs ahead	10 yrs ahead	20 yrs ahead
Total PPP GDP	2.78	3.36	3.02	3.07
Population	3.34	4.18	3.68	3.87

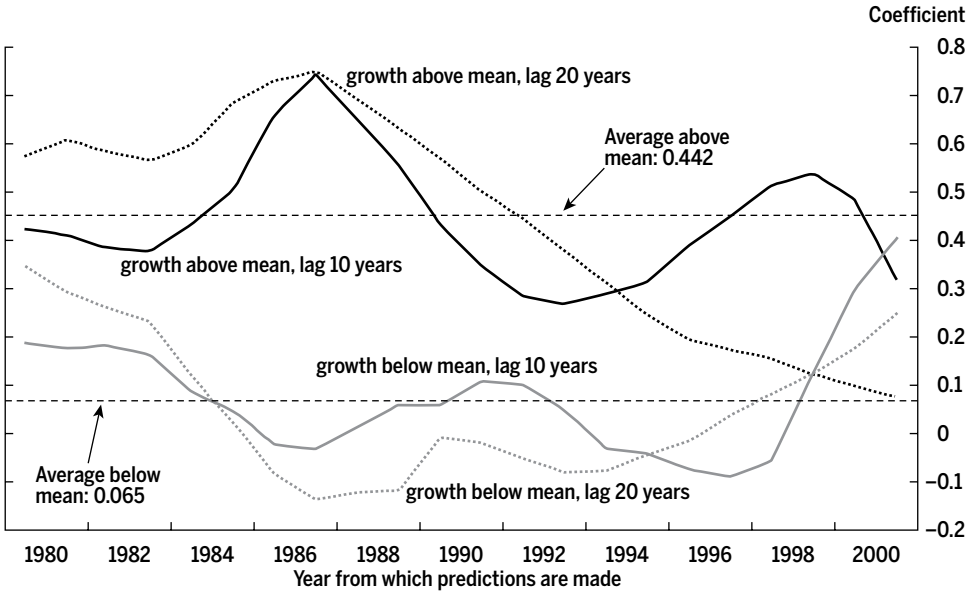
Source: Authors' regressions using PWT8.0 data and coefficients from pooled estimates of equation (3) for years 1990–2001 (for 10 year ahead) and 1990–91 (for 20 year ahead).

each year to have a different coefficient for predicting future growth depending on whether the country's past growth is above or below the mean of past growth. Since China and India obviously have extended booms, we estimate these regressions without those two countries. The results, presented graphically in Figure 3, provide some support for asymmetry. On average for the period 1980–2011 the persistence coefficient was 0.442 for growth above the mean and 0.065 when country growth was below the mean. This suggests that busts were even less persistent than booms—for an extended period the coefficient on past growth was even modestly negative for countries with slow growth, suggesting full regression to the mean.

It is not at all clear how this applies to predicting China's future growth as, using either growth lagged 10 or 20 years, the very most recent results suggest, if anything, the same or even less persistence of a boom. In any case, the highest persistence coefficient one could justify using is the period average of 0.44 for growth rates above the mean, which would still imply, all else equal and with 2 percent world growth, decade-ahead growth predictions of roughly 5 percent for China and 3.8 percent for India—still well below the conventional forecasts.

FIGURE 3

Busts More Rapidly Mean-Reverting than Booms on Average



Source: Authors' calculations with PWT8.0 data.

4. How Long Do Episodes of Rapid Growth Usually Last? How Do They End?

From 1967 to 1980, Brazil's economy grew at 5.2 percent per year. While many people might have identified macroeconomic and structural imbalances putting that growth at risk of a recession or cyclical slowdown, no one in 1980 was predicting that for the next 22 years—from 1980 to 2002—per capita growth would be exactly zero. We conjecture that nearly any assessment of the risk of such an extended slowdown using existing statistical methods for forecasting growth would have found this an extremely improbable outcome. In this section we examine episodes of growth to argue that, while not our modal forecast, the likelihood of a slowdown much larger than the regression to the mean—a so-called sudden stop—has to be considered as a possibility.

The second main point of the Easterly et al. (1993) paper was that, while growth rates have low intertemporal persistence the right-hand side variables of the then-popular growth regressions tended to have high persistence (on the order of 0.6 to 0.8). The obvious consequence is that at most a small part of the observed variation in growth rates could, even in principle, be explained by a linear relationship with an established set of determinants of growth and constant coefficients.

Hausmann, Pritchett, and Rodrik (2005) document the existence of frequent growth accelerations of substantial magnitude (more than 2.5 percent per year) to rapid growth. They show the timing of these growth accelerations are typically not well explained by standard growth determinants (e.g., good policy) or changes in the standard growth determinants (e.g., policy reform).[11]

An alternative to characterizing growth as a smoothly evolving function of linear determinants is to characterize the growth process as episodic, characterized by discrete shifts—accelerations and decelerations—from one growth state to another (Pritchett 2000, Jones and Olken 2008). These discrete shifts in growth states produce large and then persistent changes in growth rates.

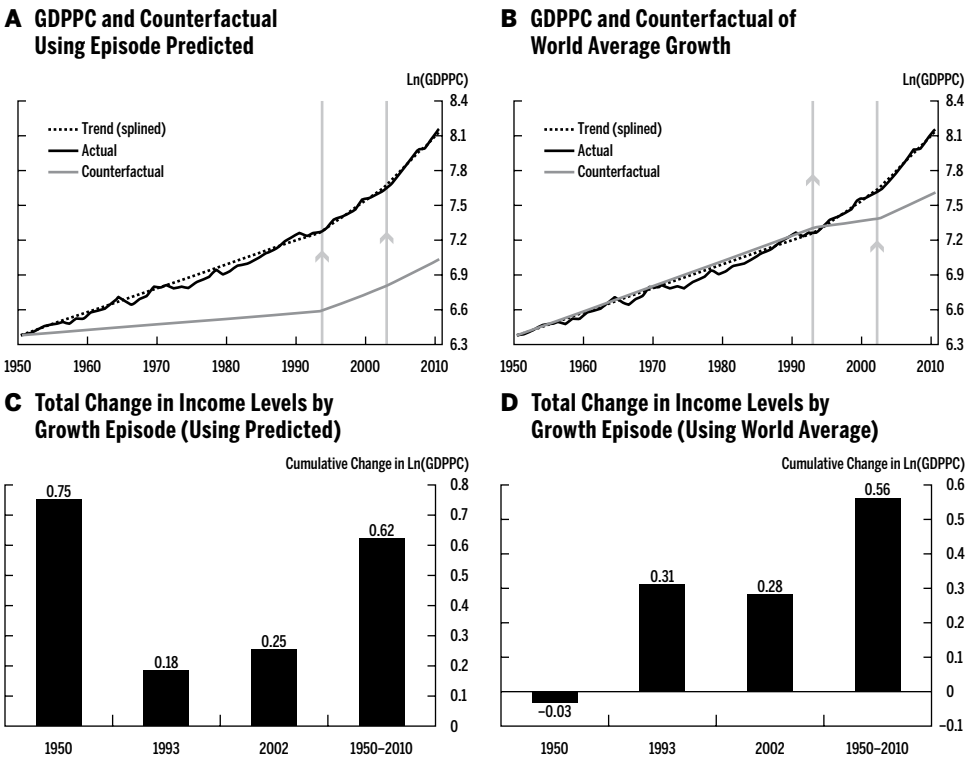
A recent set of papers extended the growth accelerations and decelerations approach to a complete characterization of the growth process of each country into a set of growth episodes (e.g., Kar et al. 2013). The basic procedure was to use the Bai-Perron approach to identify the years that best divided the GDPPC into distinct growth episodes each having a minimum length of eight years. Then a filter was applied to the *magnitude* of each potential Bai-Perron break year to eliminate the potential breaks that were empirically small changes in growth that did not represent substantial change in the growth process.[12] The filter was a 2 percent difference in annual growth rates for the first

potential break; for each subsequent break, if an acceleration followed an acceleration or if a deceleration followed a deceleration then 1 percent was deemed a break, and if an acceleration followed a deceleration (or vice versa) then a 3 percent change was deemed a break. This procedure divides each country's growth experience into a set of episodes from as few as zero (if the country experiences no growth breaks, as is the case for several OECD countries such as France and the United States) to as many as five, if all four possible Bai-Perron breaks pass the filter (as it does for, say, Argentina).

Figure 4 summarizes growth of India's real GDP per capita according to PWT7.1 data.[13] This characterization of India's growth regime is an annual growth rate of 2.09 percent from 1950 to 1993, quite near the world average of 2.15 percent. This is followed by an acceleration of growth to 4.23 percent from 1993 to 2002, then a second acceleration of growth from 4.23 to 6.29 from 2002 to 2010. In this set of episodes India has experienced a period of accelerated growth for 17 years (1993 to 2010) at a pace of 4 percent or higher.[14]

FIGURE 4

Decomposing India's Growth Experiences into Discrete Episodes



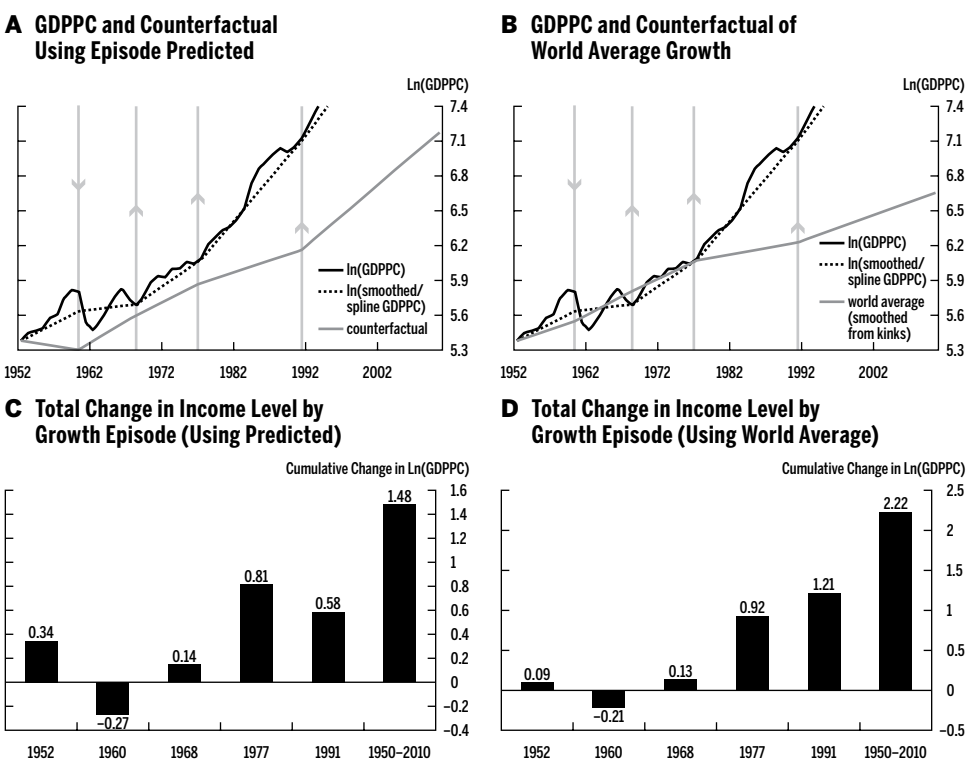
Source: Kar et al. (2013).

In the graphs the solid black line is the actual data, the dashed line is the predicted growth allowing for splines at each of the identified growth episode transitions, and the gray line is the growth if the country had grown at the predicted rate over each episode. In panel A the predicted growth is from a country/episode-specific regression that allows for regression to the mean and (unconditional) convergence. For example, India's growth 1993–2002 is predicted from a regression of growth in all other countries from 2002 to 1993 regressed on their growth over the previous episode of 1950 to 1993 and the level of GDPPC in 1993 and then plugging India's values of growth and level of GDPPC into that regression. This allows for shifts in global growth, duration, and period-specific regression to the mean and convergence (unconditional on anything except past growth). Panel B just uses unweighted world average growth over the episode period as the "predicted" growth.

The same procedure applied to China in Figure 5 produces three accelerations in a row. Growth from 1968 to 1977 was 4.33 percent per year, accelerating

FIGURE 5

Decomposing China's Growth Experience into Discrete Episodes



Source: Kar et al. (2013).

to 7.61 percent from 1977 to 1991 and accelerating yet again in 1991 to 8.63 percent until 2010. (The graph goes off the top scale as these figures are produced for a large number of countries with a common vertical axis range in order to allow visual comparability). China has had growth rates of over 6 percent for 33 years starting in 1977, and this data set ends in 2010.

Speculation about how much longer China's and India's current episodes of rapid growth might last and what might happen after those episodes, a comparison with all other experiences of country accelerations into rapid growth is not dispositive, but it is informative. Table 9 shows all 28 growth recorded accelerations that resulted in episodes of growth higher than 6 percent per year (which is roughly two standard deviations above the cross-national mean). This table reveals how unusual China's (and to a lesser extent India's) current growth experience is, in three ways.

First, episodes of super-rapid growth (>6 percent) tend to be extremely short-lived. The Kar et al. (2013) method of dating growth episodes mechanically does not allow episodes of less than eight years. The median duration of a super-rapid growth episode is nine years, only one year longer than its possible minimum. There are essentially only two countries with episodes even close to China's current duration. Taiwan had a growth episode from 1962 to 1994 of 6.8 percent (decelerating to growth of 3.5 percent from 1994 to 2010). Korea had an episode from 1962 to 1982 followed by another acceleration in 1982 until 1991 when growth decelerated to 4.48 percent—a total of 29 years of super-rapid growth (>6 percent)—followed by still rapid (>4 percent) growth. So China's experience from 1977 to 2010 already holds the distinction of being the only country, quite possibly in the history of mankind, but certainly in the data, to have sustained an episode of super-rapid growth for more than 32 years.

Second, the end of an episode of super-rapid growth is nearly always a growth deceleration. Of the 28 episodes of super-rapid growth, only two ended with a shift to higher growth: Korea in 1982 and China in 1991. So again, China is remarkable in that its acceleration to super-rapid growth in 1977 was followed by another acceleration in 1991.

Third, the typical (median) end of an episode of super-rapid growth is near complete regression to the world mean growth rate. The median growth of the growth episode that follows an episode of super-rapid growth is 2.1 percent per year. So the "unconditional" expectation (or central tendency) of what will happen following an episode of rapid growth, conditional on a shift in growth, is a reversion to not just somewhat slower growth but massive deceleration of 4.65 percentage points. Such a slowdown is more than twice the cross-national standard deviation of growth rates of roughly 2 percent. A deceleration of that

TABLE 9

All Growth Episodes above 6 Percent per Year, with Their Duration and Growth in the Episode Following

Country	Year of acceleration to high growth episode (>6)	Year of end of episode	Duration of episode (so far)	Growth during high growth episode (sorted)	Growth after end of episode	Deceleration (negative)/ Acceleration (positive) to next episode
Trinidad and Tobago	2002	Continuing	8	9.80%		Continuing
Gabon	1968	1976	8	9.26%	–2.66%	–11.92%
Angola	2001	Continuing	9	9.24%		Continuing
Japan	1959	1970	11	8.99%	3.40%	–5.59%
China	1991	Continuing	19	8.63%		Continuing
Korea	1982	1991	9	8.40%	4.42%	–3.99%
Jordan	1974	1982	8	8.18%	–4.35%	–12.54%
Singapore	1968	1980	12	7.94%	4.17%	–3.78%
Malaysia	1970	1979	9	7.66%	1.52%	–6.14%
China	1977	1991	14	7.61%	8.63%	1.01%
Laos	2002	Continuing	8	7.59%		Continuing
Morocco	1960	1968	8	7.25%	3.85%	–3.40%
Portugal	1964	1973	9	7.10%	1.73%	–5.36%
Greece	1960	1973	13	6.98%	1.50%	–5.48%
Taiwan	1962	1994	32	6.77%	3.48%	–3.29%
Malaysia	1987	1996	9	6.69%	2.10%	–4.59%
Botswana	1982	1990	8	6.65%	2.80%	–3.85%
Ecuador	1970	1978	8	6.55%	–0.39%	–6.94%
Thailand	1987	1995	8	6.51%	1.85%	–4.65%
Ireland	1987	2002	15	6.40%	0.37%	–6.03%
Cambodia	1998	Continuing	12	6.35%		Continuing
India	2002	Continuing	8	6.29%		Continuing
Dominican Republic	1968	1976	8	6.29%	1.01%	–5.28%
Korea	1962	1982	20	6.27%	8.40%	2.14%
Chile	1986	1997	11	6.16%	2.79%	–3.37%
Paraguay	1971	1980	9	6.16%	0.66%	–5.50%
Sierra Leone	1999	Continuing	11	6.11%		Continuing
Cyprus	1975	1984	9	6.04%	3.81%	–2.24%
Median			9	6.87%	2.10%	–4.65%

Source: Pritchett et al. (2013).

magnitude would take India's current growth episode of 6.29 to 1.64 percent and China's from 8.63 (in the episode since 1991) to 3.98 percent.

The results in Table 9 are not an artifact of classifying just super-rapid (>6 percent) growth. If we look at all episodes of growth greater than 4 percent (one standard deviation above mean) we would find many more episodes but similar results about duration and deceleration in all three regards. The 70 episodes of growth above 4 percent (inclusive of those above 6 percent) also have a median

duration of nine years. One does find more examples of extended rapid growth at greater than 4 percent—Singapore with 30 years at 4.17 percent from 1980 to 2010, Indonesia with 29 years at 4.71 percent from 1967 to 1996, Thailand with 29 years at 4.91 percent from 1958 to 1987 (followed by an acceleration), and Vietnam with 21 years (and ongoing) at 5.54 percent. But still, other than the combination of Thailand's episodes (the first of which was at much lower rates than China's and the end of which precipitated the East Asian crisis of 1997), none of the episodes of even rapid growth (>4 percent) is of longer duration than China's. In the 70 episodes of rapid growth (>4 percent) there are only four cases in which the episode ended with a growth shift that was an acceleration (China in 1991, Korea in 1982, Thailand in 1987, and Botswana in 1982). Finally, the median growth in the episode following the rapid growth episodes is 1.85 percent. Again, the growth following an episode of rapid growth is, on average, full regression to the mean.

4.1. Are Asian Giant Growth Dynamics Driven by a Middle Income Trap?

In a set of influential papers, Eichengreen, Park, and Shin (2012, 2013) have argued for the existence of a middle-income trap. Their analysis identifies episodes of slowdown in middle-income countries as countries with an episode of growth greater than 3.5 percent followed by a growth deceleration of 2 percent or more, which were also defined as middle income by a level of PPP income of 10,000. Their 2012 paper suggested a mode in the distributions of slowdown around PPP15,000–16,000. Their 2013 update using the new PWT7.1 data with more observations altered both their identifications of the growth breaks, modified some conclusions, and added some insights. First, they find less evidence of a single mode and more suggestion of two modes of slowdowns, one at PPP10,000–11,000 and another at PPP15,000–16,000. Second, they examine the correlates of slowdowns and find that education of secondary and higher levels (conditional on GDP per capita) and high technology exports mitigate the risks of slowdown.

The Eichengreen, Park, and Shin analysis focuses only on those decelerations among countries that are middle income and hence limit their sample to decelerations among countries that are already near middle income. The middle-income trap conjecture almost certainly has no bearing on India, which is and will remain for the foreseeable future a poor country. The PWT8.0 estimate of real GDP (expenditure) per capita is $3755—which is only 8.4 percent of the U.S. level.

We replicate a version of the middle-income trap analysis by taking all structural breaks identified by Kar et al. (2013) and identifying those that are

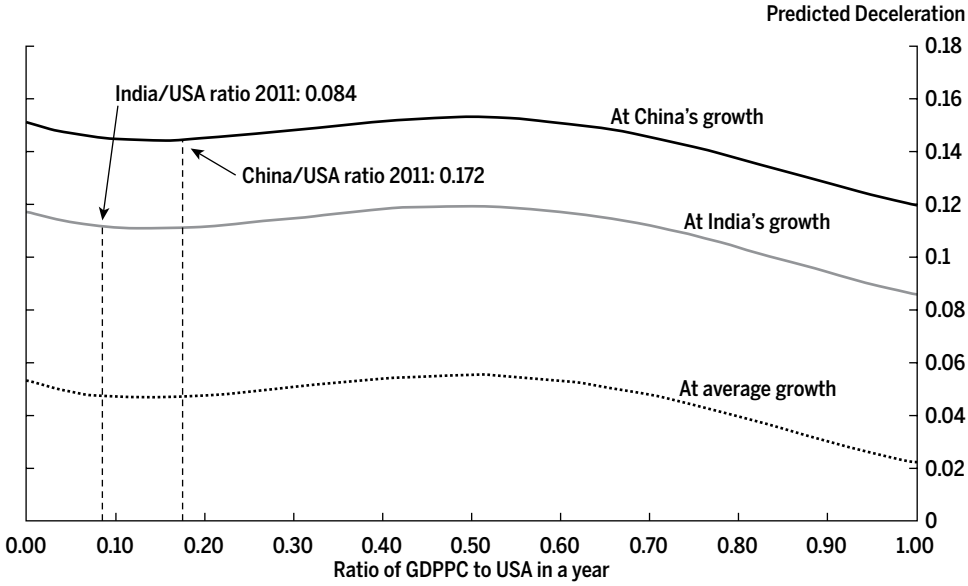
decelerations. We then regress a dummy variable for a country-year pair that was (near) a deceleration on either the country's ratio to the United States (as a proxy for the global leader at the time) or on the absolute level of GDPPC and include the growth in the episode preceding the deceleration. To allow for a flexibly specified relationship, we estimate this simple bivariate relationship using quartic terms in either ratio or level.

Figure 6 (ratio to U.S. GDPPC) and Figure 7 (level of GDPPC) present the predicted probability of a deceleration against either the ratio to U.S. GDPPC or absolute GDPPC. There are three lines showing the relationship with the ratio or level of GDPPC at average growth, at China's growth, and at India's growth.

The graph makes the basic point that the main issue with China and India is that their likelihood of deceleration is currently mainly influenced by the fact that they are growing very rapidly as there is a strong association of pace of growth with the likelihood of deceleration. So, while there may, or may not, be a middle-income trap, empirically the change in the likelihood of deceleration associated with changes in income as countries grow is small compared with the association with rapid growth.

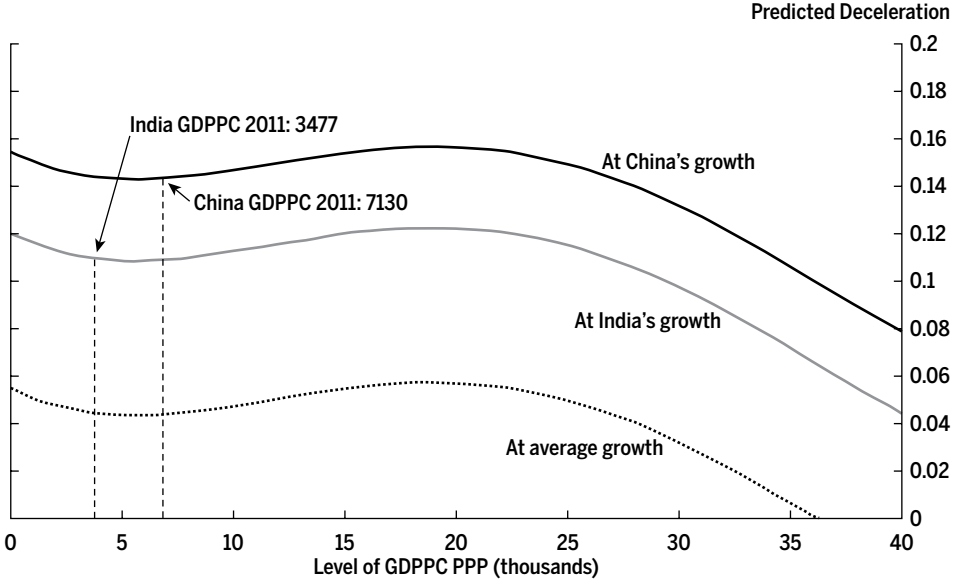
FIGURE 6

Regression of Deceleration on Country/Year Ratio to U.S. GDPPC (Quartic)



Source: Authors' calculations with Kar et al. (2013) structural breaks.

FIGURE 7
Regression of Deceleration on Absolute Level of GDPPC (Quartic)



Source: Authors' calculations with Kar et al. (2013) structural breaks.

Take China. The predicted likelihood of a deceleration of a country with average growth at China's 2011 level of GDPPC of $7,110 (PWT7.1 since the structural breaks were done with that data) is 4.5 percent. At $20,000 (roughly the maximum with respect to level of income) that risk increases to 5.7 percent—so there is some modest middle-income trap dynamic of increased likelihood of deceleration with increased income. By contrast the predicted likelihood of deceleration at China's current level of growth and level of income is 14.4 percent. This increases to 15.7 percent at $20,000. So the increase in the predicted likelihood of a deceleration due to China's much higher than average growth is roughly 10 times larger than the increase from increasing GDPPC from its current level to the biggest risk due to middle-income trap dynamics.

5. China's Challenge: Stable Rule of Power into Rule of Law

So far our discussion of the Asian giants as an engine of global growth has been remarkably free of any discussion of the specifics of the Asian giants. One might have expected the question of whether Asia can be the engine of future global growth, to have been addressed by specifying some relationship between growth and its proximal or causal determinants of the type

$$g = f(x),$$

and then making the case for continued rapid growth or deceleration based on that model and the likely trajectories of the x variables. While we will engage in some country-specific discussion along those lines below, we deliberately choose not to go that direction for three reasons.

First, conditional forecasting of this type is only as good as the forecasts of the conditioning variables. Imagine dividing the potential x's into two types: those easy to forecast because they have high persistence (e.g., size of the country, geography, latitude, nearness to ports) and those with low persistence. Obviously the former are quite easy to forecast, but also cannot be the usual causes of super-rapid growth. Hence they would only be good at predicting the mean that super-rapid growth is likely to regress to, but because they are highly persistent they would have fairly low explanatory power for changes in growth as these variables must, by definition as high persistence variables, have fairly low explanatory power for changes in growth. That is, econometrically, if serial correlation of growth is low then constant determinants of growth cannot have high explanatory power.

The growth determinants with low persistence may be good at forecasting growth but are themselves harder to forecast. Again, by construction, extrapolation of those variables is a bad forecast of x, making a forecast conditional on x a bad forecast. To use this forecast continued rapid growth would then require that we somehow have a good forecast that some important growth determinant is going to change in such a way that growth that otherwise would have decelerated remains rapid. We cannot think of such a thing and, as we argue below, there are several prominent possibilities of just the opposite dynamic.

Second, even if we could reliably forecast the x's we would also have to imagine we had identified a reasonably accurate and long-term stable empirical relationship. This just has not been true to any extent in the domain of economic growth, nor is this unique to economic growth. We have lived through a series of major political, social, and economic events in our lifetime, none of which were widely predicted by experts in the appropriate domain.

A salient recent example is that we are still living in the shadow of the financial crisis in the United States and elsewhere. It is worth pointing out that the depth and severity of the crisis was not only not predicted by academic economists on the sidelines nor, in their assessments of the riskiness of classes of assets, by raters (Silver 2012) nor by policymakers. People who had incredibly high stakes on correct forecasts by having most of their financial wealth at risk (and, unfortunately, leveraged) misforecast the outcomes in the housing market badly—for them. This is not because there was ignorance of a housing bubble; some mainstream economists (particularly Robert Shiller among others)

pointed out the magnitude of the deviation of housing prices from their long-run trends early (at least by 2005) and often. But what was missed was how this would translate into the financial sector and the economy as a whole. Leamer's (2010) demonstration of how the evolution of prices and quantities in the Los Angeles housing market produced enormously different dynamics in different periods, such that the confidence intervals based on past data for future predictions substantially understated the true range of possibility is just one example of the instability of models.

Third, super-rapid growth is due in part to a large residual or unexplained component, which we rarely admit as we overexplain the current reality. While perhaps too much can be made of Taleb's (2007) "Black Swan" arguments that we overpredict reality—that is, we concoct reasons ex post to make it seem as if we understand what happened when we really didn't—conventionally too little is made of it. Taleb's obvious and poignant example that, while Lebanon remained an oasis of multireligious peaceful coexistence and institutional success (aka "the Switzerland of the Middle East") there were many powerful theories why Lebanon's success was overdetermined by observable factors. After Lebanon was engulfed by the general regional instability, it quickly became equally obvious that Lebanon was doomed to instability.

Dramatic changes in perceptions of the Japanese economic system provide another example. During the late 1980s, it was widely believed that Japanese-style industrial policy, Japanese emphasis on corporate linkages through *keiretsu*, and high levels of investment supported by financial repression were keys to rapid growth. A decade later all of these conclusions had been abandoned to be replaced by nearly opposite views in the conventional wisdom.

At an even broader level, it was widely believed in the early 1960s that the Soviet Union would quite likely outstrip the United States economically based on an extrapolation of its recent growth performance. Justifications were even developed for the apparently rapid growth of central Europe as late as 1979, as illustrated by the famous World Bank report of that year on the Romanian economic miracle.

The point can be demonstrated for countries as well. We believe that in the United States there are no known examples since 1950 when the consensus forecast called for recession one year out, even though recessions have occurred on average every five or six years since then and even though they appear to have a permanent rather than a temporary impact on output.

Imagine that in a conference in 2023 we know ex post that in 2014 China's GDPPC growth was 8 percent for the calendar year 2014. However, we also know that in March 2015 China experienced a sudden sharp slowdown in

economic growth that persisted and caused growth to be only 2 percent from 2015 to 2023. Here is the question: In that scenario, what do we think the forecast of growth 2015–23 was in the IMF *World Economic Outlook* for China in October 2014, six months before the slowdown? Our guess is that the 2014 forecast was 8 percent growth and was expressed with substantial confidence.

All that said, we suspect that the reasons slowdowns will come in China and India are similar but will manifest differently given the very different politics. That is, in neither country does investor confidence rely on rule of law. In both countries there are plausible scenarios in which the current political settlement that provides a climate for ordered deals (Hallward-Driemeier and Pritchett 2011) will be disrupted. This disruption of the arrangements that provide settled expectations of investors can easily create processes with nonlinear sudden stops.

As North, Wallis, and Weingast (2009) show, the reason for the low growth *on average* of developing versus developed countries is not the lack of rapid growth—it is the lack of the *persistence* of that growth and the very low growth rates during their periods of negative growth. As we saw with Denmark, the rich industrial countries are rich because they grew at modest rates for very long periods, with little variation and few disastrous downturns—e.g., 84 percent of years in positive growth, and negative growth only falling to –2.33 percent per year. By contrast, current poor countries have failed to converge because they grow much faster when they are growing (e.g., 5.39 percent per year for those in the 2,000 to 5,000 range) then a third of their time have sizable negative growth (averaging –4.75 percent for the same grouping).

Take for example the comparison of the rich countries with those countries with a per capita income between $2,000 and $5,000. Average growth is equal to the average growth when positive times the probability that growth is positive plus average growth when negative times the probability of growth being negative. For both groups, average growth when positive contributes just about 3.6 percent to the weighted average. But for poorer countries, average growth when negative contributes –1.6 percent rather than –0.4 percent, accounting for all the difference in growth (Table 10).

Powerful evidence suggests that high *levels* of output per capita are associated with high *levels* of institutional quality (e.g., Hall and Jones 1999, Acemoglu, Johnson, and Robinson 2001, and North, Wallis, and Weingast 2009) and that, over very long periods, this association is sustained so that institutional arrangements can have very long-lasting effects (including regional evidence within countries as in Dell (2010) showing the persistence on levels of income and well-being today in Peru and Bolivia of the *mita* arrangements in Spanish

TABLE 10
Developing Countries Spend More Time in Negative Growth States than the Advanced Industrial Countries

Per capita income in 2000 (PPP)	Number of countries	% of years with positive growth	Growth rate, when positive	Growth rate, when negative
>20,000 (non–oil)	27	84%	3.88%	–2.33%
"Developing" countries				
15,000 to 20,000	12	76%	5.59%	–4.25%
10,000 to 15,000	14	71%	5.27%	–4.07%
5,000 to 10,000	37	73%	5.25%	–4.59%
2,000 to 5,000	46	66%	5.39%	–4.75%
300 to 2,000	44	56%	5.37%	–5.38%
Average of <20,000			5.37%	–4.61%

Source: Adapted from North, Wallis, and Weingast (2009), Table 1.2.

colonial times). This view can accommodate short-run growth booms and busts driven by exogenous factors like terms of trade or new opportunities that do not challenge existing economic and political interests. However, it is very difficult (but perhaps not impossible) to explain the location, onset, or timing of extended growth episodes in terms of institutions because that component of income explained by events centuries ago (whether that is *mita* or crop endowments or settler mortality or social capital) should not be expected to handle large and rapid *changes* in income.

One conjecture about the source of growth slowdowns is that in countries with weak capability for implementation of policies there is a large divergence between the de jure laws and regulations and the de facto outcomes for specific firms. This can come in the form of arrangements that allow for high and secure profitability for firms without the neutral enforcement of the rule of law. It can even be the case that "closed ordered deals" (Hallward-Dreimeier and Pritchett 2011) that are provided for the favored firms are not only a good investment climate for doing business but a veritable greenhouse—that is, the environment specific to the firm (and its connections to existing power structures) is much *better* than the existing de jure regulatory environment and better than a de facto environment even with good regulations. That is, the favored firms in a closed order deals environment have higher and more secure profitability than the typical firm in an OECD country. As firms either "seize the state" (Hellmann, Jones and Kaufmann 2000) or *are* the state or are chosen by the state (Fisman 2001), the official legal and regulatory environment—or more particularly its implementation—are bended to provide great, if super-local and specific, conditions for growth. That is, growth in closed ordered deals can be much higher than in an institutionally good investment climate.

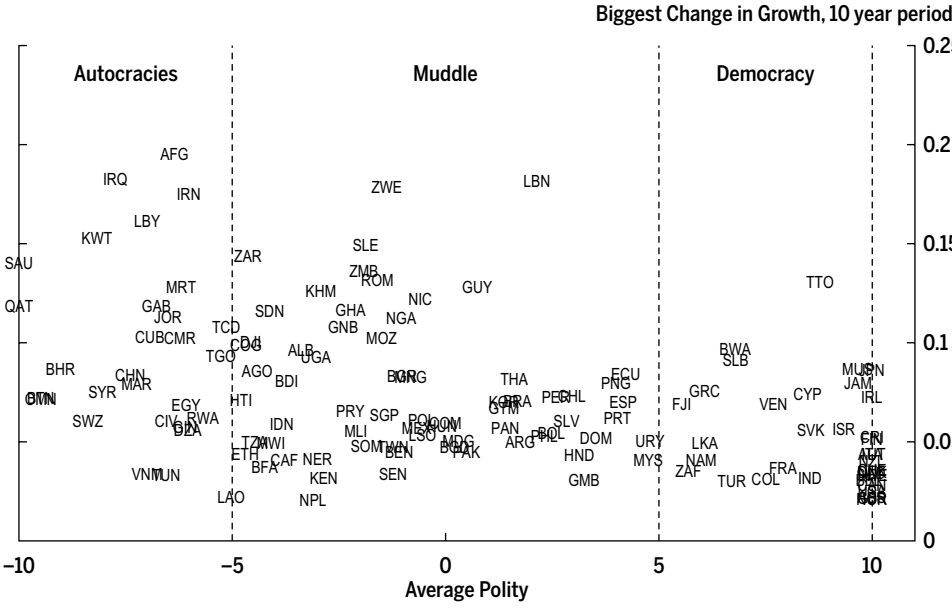
But, the difficulty is the transition. Since investor expectations (both domestic and foreign) are grounded in specific relationships to specific power bases, shifts in power can occasion very sudden stops as investor expectations have to realign to new realities. This can create sudden stops that then can resume as new conditions are established or can persist for a very long time if new institutions have to emerge and have credibility.

This can lead measures of institutions—like those that measure political institutions—to be associated with the *range* of growth outcomes, not necessarily the *level* of growth over medium-run periods. Figure 8 shows the largest *difference* in growth rates over 10-year periods of countries at various levels of the Polity score (which measures autocracy/democracy on a –10 to +10 scale). While autocracies can maintain very high growth rates—even over extended periods—they also tend to have much larger *ranges* of growth outcomes—with booms and busts—than stable democracies.

There is a strong cross-national relationship between the extent to which a country is (or is rated as) a democracy and GDP per capita. This relationship reveals nothing about cause and effect, and certainly we are not going to assert some strong, monocausal, linear dynamic whereby richer countries naturally

FIGURE 8

Lower Polity Scores Associated with Larger Changes in Growth over Time



Source: Pritchett and Werker (2012).

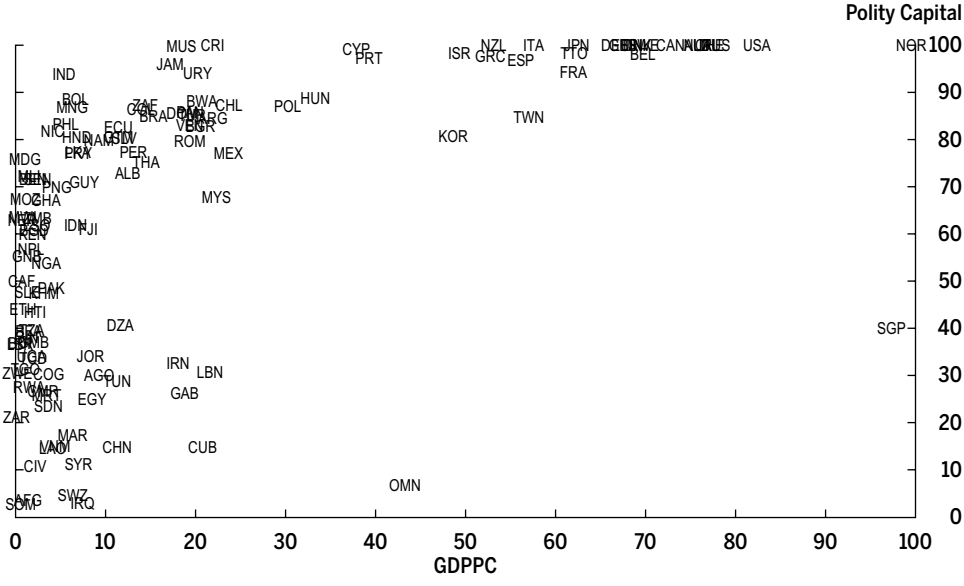
become more democratic. That said, Figure 9 shows the relationship between a stock-like measure of democratic capital index that cumulates the Polity score into a stock (to smooth the transitory fluctuations) which then scales most democratic countries as 100 and least democratic countries as 0.

The obvious point is that there are extremely few exceptions to the tendency for all countries with high levels of GDP per capita (expressed here as an index from 0 to 100) to also have high levels of (measured) democracy. The only two exceptions for a country with GDPPC more than a third of the leader (33 on the index) not having a democracy capital score above 80 are Oman (an oil producer)[15] and Singapore. For countries in China's current range of output (between 10 and 25 on scale of 0 to 100), the complete range of democracy outcomes exist. However, the average for this group is a democracy capital index of 71 with a standard deviation of 25. Already at a score of 14, China is much less democratic than the typical country with its level of output.

For China to continue to have rapid economic growth while maintaining its current level of democracy (as proxied by its Polity score)—a trajectory moving rapidly due east in Figure 9—would make it more and more anomalous. Which is not to say it isn't possible. Singapore (granted, a small city-state of only 5

FIGURE 9

Relationship between GDP per Capita and a Stock of Democracy Capital Index, 2008
(both normalized 0 to 100)



Source: Kenny and Pritchett (2013).

million people) has managed to be nearly the richest country in the world while only having a Polity democracy capital index of 40. But even 40 is more than twice China's current level of 14.

An empirical question is, What, if any, impact might we expect a democratizing period to have on China's growth?

A huge literature examines the association between democracy and growth, and generally finds only a weak and non-robust association between the level of democracy and the pace of growth but that less-democratic countries tend to have much higher volatility of growth. However, a simple analysis of whether democracies grow faster or slower than non-democracies does not capture the possibility that large political transitions may themselves have impacts. In this case, while democracies may be capable of sustaining rapid growth in the long run, the transition itself may create an adjustment period of slow growth. To examine this question, we need to compare countries' growth rates before and after large, rapid political transitions from autocracy to democracy. For this, we need to define what qualifies as "large" democratizing transitions.

Pritchett (2011) searched the Polity data to identify all instances in which a country's index had increased by more than five units in a single year towards less autocracy or more democracy. These were the candidates for a large democratic transition. He then used a decision tree to classify and date these potential transitions, addressing in particular the treatment of countries with multiple transitions. This classification scheme resulted in 52 episodes of large democratic transition. Once large democratic transitions had been identified, the next step was to calculate growth rates before and after the transition.[16] To capture the medium- to long-run dynamics term, he calculated the growth rate for the 10-year period ending three years before the transition and that for the 10-year period beginning one year after the transition (or, if 10 years of data were not available, until the data ended). For instance, in the case of Indonesia, the two 10-year periods would be 1986–96 (the period ending three years before the democratic transition in 1999) and 2000–07 (the period beginning one year after the transition and ending when the PWT6.3 data stop in 2007).[17]

The first result evident in Table 11 is that nearly every country that experienced a large democratic transition after a period of above-average growth (more than the cross-country average of 2 percent) experienced a sharp deceleration in growth in the 10 years following the democratizing transition. Among 22 countries in which episodes of large democratic transition coincided with above-average growth, all but one (Korea in 1987 with an acceleration of only 0.22 percent) experienced a growth deceleration. The combination of high initial growth and democratic transition seems to make some deceleration all but

TABLE 11
Countries with Large Democratic Transitions Starting from Above Average (2 Percent per Year) Growth in GDP per Capita

Country	Year of transition	Magnitude of Polity increase	10-year growth ending 3 years before democratizing transition (%)	10-year growth beginning 1 year after transition (%)	Change in pre-/post-transition growth rates (%)
Greece	1975	7	7.19	0.02	–7.17
Iran	1979	10	7.11	0.11	–7.01
Portugal	1976	6	7.11	1.48	–5.63
Taiwan	1992	8	6.47	3.95	–2.52
Taiwan	1987	6	6.42	5.78	–0.64
Nigeria	1979	7	5.81	–2.44	–8.25
Ecuador	1979	14	5.69	–1.66	–7.36
Congo	1992	6	5.68	0.57	–5.11
Indonesia	1999	11	5.54	3.28	–2.26
Dominican Rep.	1978	9	5.50	1.35	–4.14
South Korea	1987	6	5.36	5.57	0.22
Thailand	1992	10	4.67	0.82	–3.85
Mongolia	1990	9	4.39	2.09	–2.30
Bulgaria	1990	15	4.02	–0.10	–4.12
Panama	1989	16	3.91	1.68	–2.23
Benin	1990	7	3.62	1.30	–2.32
Pakistan	1988	12	3.50	1.32	–2.18
Uruguay	1985	16	3.44	3.16	–0.27
Brazil	1985	10	3.31	–0.34	–3.65
Paraguay	1989	10	2.70	–0.75	–3.45
Bolivia	1982	15	2.37	0.27	–2.09
Romania	1989	6	2.14	0.85	–1.28
Median			5.01	1.08	–2.99
Average			4.82	1.29	–3.53

Source: Pritchett (2011).

inevitable. The magnitude of the decelerations was very large: The median deceleration across the 22 countries was 2.99 percent, and the average deceleration was 3.53 percent.

At least one mechanism that could cause democratizing transitions to decelerate growth has been explored in the case of Indonesia. As Fisman (2001) has shown, the stock market value of firms connected to President Suharto was associated with news about his health, implying that a substantial amount of their value was related to his personal control of the levers or power. While power in China is obviously controlled by much larger and broader regionally competing forces, it nevertheless is not exercised in what one would typically call a democratic fashion nor according to the rule of law, nor are traditionally conceived human rights protected. That said, the fact that China is rated by most indicators as having very low control over corruption and not having

improved over the previous decade has obviously not been an obstacle to rapid growth. This is not surprising as Shleifer and Vishny (1993) have long argued that organized corruption need not be inimical to growth. However it is difficult for corruption to remain organized during a transition in political power.

We are not forecasting that China will move towards democracy nor that this will be what causes China's growth to slow. But we are pointing out the very dangerous shoals through which the Chinese economy is currently sailing very rapidly. While it is possible to envision the transition not happening for some extended time, and while it is possible to envision the transition being made smoothly, neither of these are the outcomes typically observed in the data.

6. Conclusion

Much analysis and forecasting regarding economic growth treats a country's history as the principle information for thinking about its future. It is an empirical question whether this is right or wrong. Someone looking to predict the future health status of a 60-year-old would give some weight to her health history but probably much more weight to the available information on the past populations of 60-year-olds. In the same way, our findings suggest that in forecasting growth rates over the long term, forecasters should give heavy weight to the growth rate of all countries. We believe that most economic forecasting errors historically have come from neglecting this principle and placing excessive weight on a country's recent past in making forecasts. Perhaps this is why official forecasts usually miss discontinuities.

The recovery in the United States is currently slow relative to expectations, and the recovery in Europe is even weaker. Yet, the post-crisis fallout for the global economy has been much less than feared. This has certainly been due in large part to sustained growth in China and India that likely has positive spillovers (e.g., through high commodity prices and trade linkages) to other economies. The hopeful and in many quarters prevailing view is that this represents a decisive shift and that rapid global growth will continue—and perhaps even OECD growth will recover—with Asia as the engine, via the Asian giants and others.

This is certainly one scenario.

However, those around the age of the authors or older remember well at least two previous periods of Asiaphoria. Japan's rapid growth from the 1960s (though decelerated already by the 1970s) led to popular and academic literature explaining why Japan succeeded and would continue to succeed. Although there were some concerns about a bubble in Japanese real estate, almost no one predicted in 1991 that Japan's real GDP per capita would be only 12 percent

higher in 2011 than 20 years earlier (an annual growth rate of only 0.6 percent) and that total factor productivity in Japan, which had *doubled* from 1961 to 1991 would be 6 percent *lower* in 2011 than in 1991 (data and calculations from PWT8.0).

The second Asiaphoria was the growth in the 1990s when Southeast Asian countries—Indonesia, Malaysia, and Thailand—appeared to be booming along with the Four Dragons. A financial and economic crisis spread across East Asia in the late 1990s with sharp contractions in nearly all the booming economies. While most recovered quickly, the annual growth rates for the most recent episode of growth have been only 1.42 percent for Indonesia, 2.1 percent for Malaysia, and 1.85 percent for Thailand, which is a relatively rapid growth from a quite steep contraction. Korea and Taiwan had shorter crises and quicker recoveries, but their growth rates in the more recent growth episode are 3.48 and 3.29 percent, respectively, which is rapid but nothing like the current growth of China or India.

Regression to the mean is the single most robust finding of the growth literature, and the typical degrees of regression to the mean imply substantial slowdowns in China and India relative even to the currently more cautious and less bullish forecasts.

India and, even more so, China are experiencing historically unprecedented episodes of growth. China's super-rapid growth has already lasted three times longer than a typical episode and is the longest ever recorded. The ends of episodes tend to see full regression to the mean, abruptly.

It is impossible to argue that either China or India have the kinds of quality institutions that have been associated with the steady dynamic of growth in the currently high productivity countries. The risks of sudden stops are much higher with weak institutions and organizations for policy implementation. China and India have very different modalities of this risk, but both have tricky paths to continued prosperity.

We would suggest several implications of these conclusions. First, there will be a strong tendency to assume that, if growth slows substantially in China or India, it will represent an important policy failure. This is not right. Regression to the mean in a decade or so is the rule, not the exception. What would require much more explanation would be continued rapid growth, which would be very much outside the general run of experience. Second, those making global projections should allow a very wide confidence interval with respect to growth for countries whose current growth rates are far from the mean. Given the sensitivity of commodity demands in particular to growth rates in Asia, this suggests substantial uncertainty about the medium-term path of commodity prices. In

the same way, forecasts of global energy use and climate change impacts should also recognize the possibility of discontinuities in Asia. Third, much geopolitical analysis has focused on the implications of a rising China, and certainly Chinese international relations theorists have extensively studied past rising powers. Contingency planning should also embrace scenarios in which Chinese growth slows dramatically, presumably bringing with it a range of domestic and international political implications.

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NOTES

1 This, for now, begs the question of whether PPP, which is appropriate for comparisons of living standards, is the right metric for global influence, as international trade obviously happens at actual, not PPP, exchange rates.

2 The recent crisis has again reminded researchers of the distinction between risk and Knightian uncertainty (see, e.g., Greenspan 2013). Silver (2012) argues the models being used by the risk-rating agencies underpredicted the default risk of some bonds not by percentage points or even a single order of magnitude, but by a factor of 200.

3 Barro's (1991) paper has over 10,000 citations in Google Scholar, and Xavier Sala-i-Martin (1997) takes personal credit for four million growth regressions. One suspects the exclusion of the kitchen sink was more a lack of easily downloadable cross-national kitchen sink data than reluctance to use such data in a growth regression.

4 We calculate a growth rate if there is more than 7 years of data for the 10-year growth rates and 14 years of data for the 20-year period (e.g., we include a 10-year growth rate for the 1950s for countries with data starting in 1953). We exclude countries with less than 25 years of data—which removes all of the successor nation-states of the former Soviet Union from calculations in this paper. We also exclude Equatorial Guinea because it has a small population and is frequently a massive outlier.

5 The basic findings in Easterly et al. (1993) Table 1 was a correlation of 0.313 for 100 countries from the 1970s to 1980s (compare 0.337 in the current results) and 0.212 from the 1960s to 1970s (compare 0.339 for 108 countries in the current results).

6 On one level this choice of official versus PPP is inconsequential for our basic point as the proportionate changes would be the same whatever base we use, since we are not assuming any changes in exchange rates. Obviously by using the base of official exchange rates we make the Chinese and Indian economies look relatively smaller to the U.S. (or OECD) economies, and hence PPP calculations would make the relative sizes even more dramatic. However, to do PPP we would have to do something about the relationship between level of income and PPP prices over time, which complicates our calculations in a not useful way.

7 GK$23,302=exp(ln(3891.25)+(.0190*(2010–1916)).

8 A figure showing the long-run stability of growth for the United States has been the cover of Charles Jones's textbook on economic growth.

9 Of course, as also known at least since DeLong's critique of Baumol's assertions of convergence, the argument is somewhat circular that what it *means* to be a leading country is that it maintained a high growth rate. Argentina's GDPPC in 1890 was about the same as Austria, France, or Germany and much higher than Italy, Norway, Sweden, or Spain.

10 One additional difference in this section is that we eliminate from all (a) one small country (Equatorial Guinea) that has unusual growth dynamics driven by oil, (b) five countries whose growth dynamics are driven by conflict (Liberia, Sierra Leone, Iraq, Democratic Republic of Congo, and Kuwait), and (c) all countries with population less than 600,000. We think a reasonable case can be made that the parameters used in predicting growth in the giants of China and India should not be overly influenced by the growth dynamics of small or atypical outliers.

11 Although Rodrik (1999) does a better job of explaining growth decelerations as a product of negative shocks and weak social ability to cope with shocks. Breuer and McDermott (2013) recently argue that the onset and timing of depressions are better empirically explained by bad policies than growth accelerations are by good. This is consistent with the argument of Easterly that many of the robust findings of the first generation of growth regressions were actually due to nonlinearities of sufficiently bad policy (e.g., high black market premium) producing very bad growth outcomes.

12 That is, we did not use the Bai-Perron tests of statistical significance to identify which potential breaks were true breaks as the statistical power varies so widely from country to country.

13 The PWT8.0 was only recently available and the entire procedure has not been repeated with the new data, either using national accounts or PPP-adjusted data.

14 There has been a great deal of debate over the dating of India's growth acceleration, and the results are sensitive to the data and method used. In particular, the choice of the length of an episode determines how a recent growth acceleration will affect the dates as no acceleration can be near than the fixed length from the end of the coverage of the data. While many date the acceleration near the adoption of the liberalizing reforms during and after the incipient macroeconomic crisis of 1990–91, Rodrik and Subramanian (2004) date the growth acceleration to the early 1980s—well before the onset of those reforms. For our purposes the question is whether the recent acceleration to super-high growth rates, which clearly took place in the 2000s—will persist.

15 The nature of the data selection process used also excluded most other oil countries that would have had high income but low democratic capital because they lacked sufficient data.

16 The comparative data on PPP-adjusted real GDP per capita are taken from PWT6.3 compiled by the Center for International Comparisons at the University of Pennsylvania. See http://pwt.econ.upenn.edu/php_site/pwt63/pwt63_form.php.

17 These timing assumptions are not innocuous. Often a political transition is preceded by a large fall in GDP per capita, sometimes as the result of the chaos surrounding the transition itself. If one then calculated the growth before the transition to include this fall (which could be the result of the transition itself), then it would look as though the political transition had accelerated growth. That is why we go back some years before the transition, so that the pure disruption effects are not counted as part of the pre-democratic period. Rodrik and Wacziarg (2005) obtain similar results overall: Of the nine countries they identify with democratizing transitions begun from above 2 percent growth, the average deceleration is 3.53 percent, which is *exactly* what we find in Table 9. But in some countries timing differences produce different results.

COMMENTARY

Asiaphoria Meets Regression to the Mean

Chang-Tai Hsieh

I will present the message of the paper through three graphs. Figure 1 presents a scatterplot of GDP per worker of a country relative to the United States in 1980 vs. GDP per worker (again relative to the U.S.) in 1960, where the solid line is the 45-degree line. What I like about this figure is that we can read off both the level of income as well as the growth rate. That is, a country that has converged relative to the United States will lie above the 45-degree line, and a country that has diverged relative to the U.S. will lie below the 45-degree line, and the magnitude of the convergence or divergence is reflected in the distance of the country from the 45-degree line. From looking at this figure, it is evident that the growth miracles over this period are the widely recognized cases of Taiwan and Korea. But another country that grew just as quickly as Taiwan and Korea over this time period is Brazil. This of course was the Brazilian growth miracle that seems almost a mirage these days. One way to see this is to look at the same scatterplot from 1960 to 1990 (Figure 2). As can be seen, Taiwan and Korea continued to grow in the 1980s. On the other hand, the growth miracle in Brazil came to an end by the early 1980s.

Figure 3 plots GDP per worker from 1990 to 2010. As can be seen, the growth miracle in Korea and Taiwan came to an end after 1990. Both countries continued to grow of course, albeit at more modest rates. Nonetheless, starting in 1960 with GDP per worker about 15 percent that of the United States, Korea and Taiwan ended up in 2010 with GDP per worker about 75 percent that of the U.S. And Brazil never recovered and ends up in 2010 with a GDP per worker relative to the United States that is almost the same as what it was in 1960. So although it is clear that high growth in Taiwan, Korea, and Brazil from 1960 to 1980 was mean reverting, there is a big difference between mean reversion in Taiwan and Korea vs. mean reversion in Brazilian growth rates.

The second thing one can see in Figure 3 is the growth story in the past 20 years in China and, to a lesser extent, India. So the central question in this paper is whether growth in China and India will slow down. The paper suggests that, based on the statistical evidence of mean reversion of growth rates, it is

FIGURE 1
Relative GDP per Worker: 1960 vs. 1980

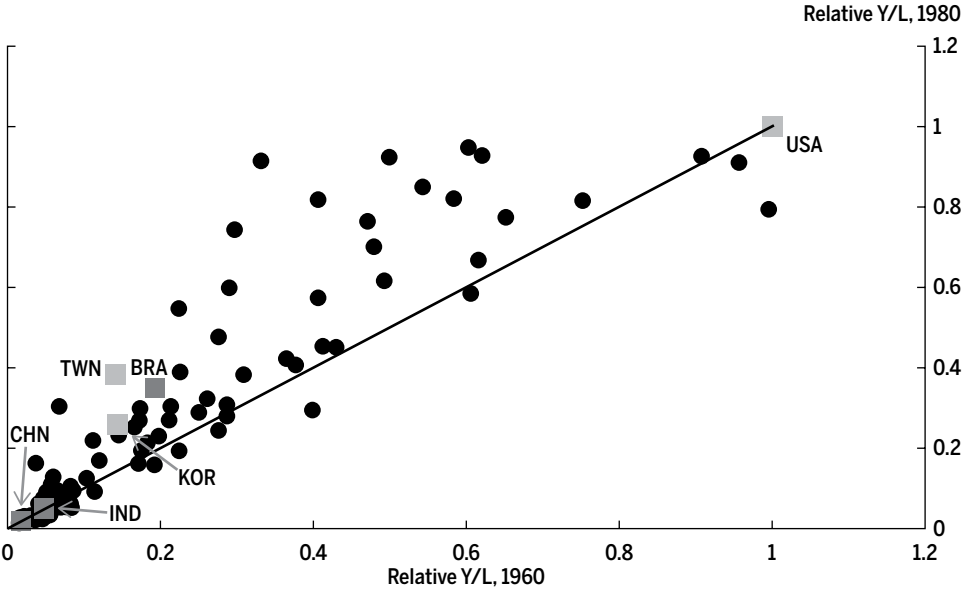


FIGURE 2
Relative GDP per Worker: 1960 vs. 1990

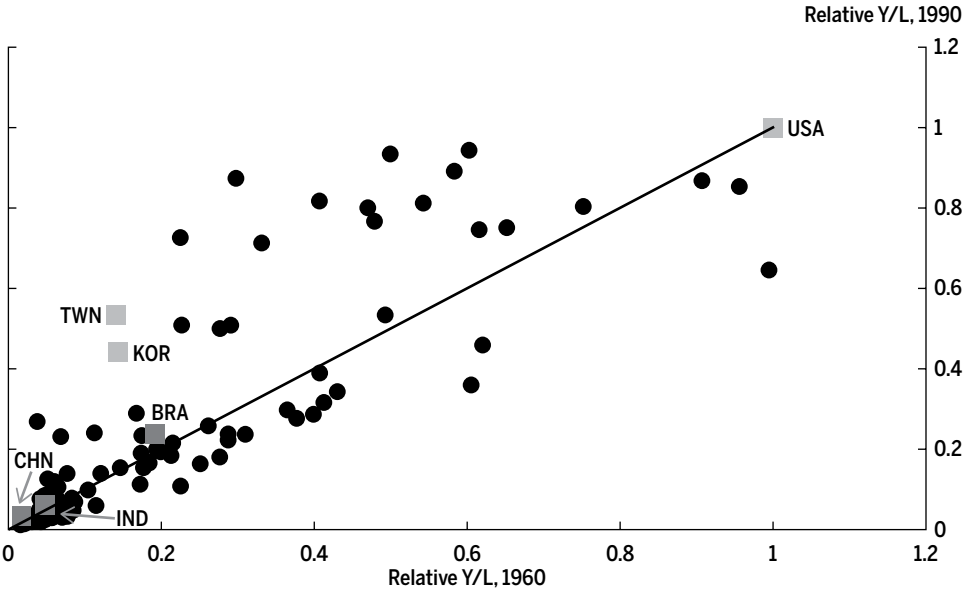
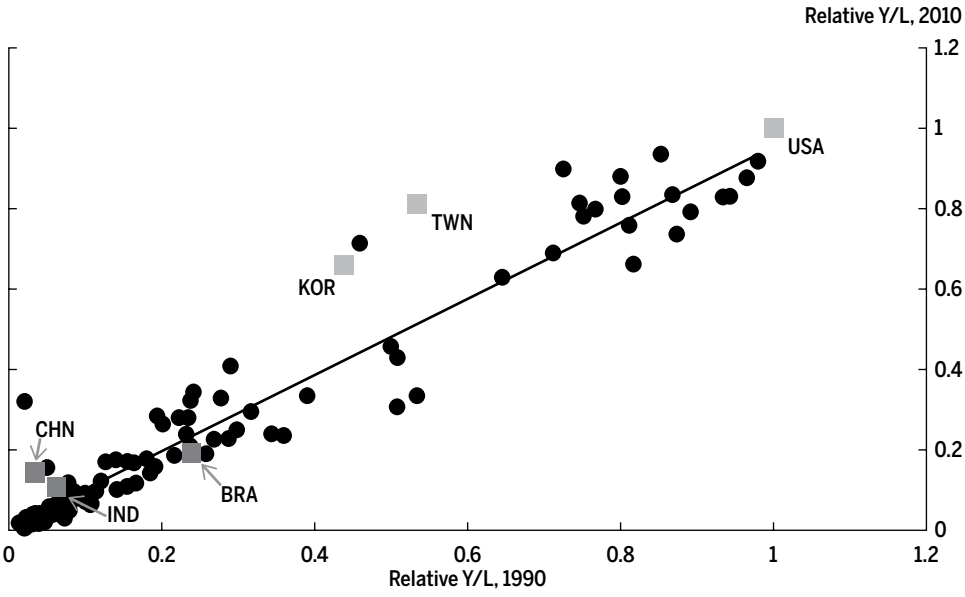


FIGURE 3
Relative GDP per Worker: 1990 vs. 2010



very unlikely that growth rates in China and India will continue to be as high as it has been in the past few years.

There are three points to think about in assessing whether the conclusion of the paper is correct. First, even if one believes that growth is mean reverting, it makes a big difference whether growth in China and India will revert to the mean as in Korea or Taiwan or whether it will revert as in Brazil. Even if growth in China and India slows down, it would be a growth miracle of epic proportions if GDP per worker in China and India end up at 75 percent of the United States. Second, although I am sure that the authors do not believe that growth is simply the outcome of a statistical process, parts of the paper read as if this were the case. Statistics are fine if one wants to summarize data, but if the goal is to make predictions, then they need to impose some structure.

For example, in a canonical neoclassical growth model, a key variable determining growth rates is the country's distance from the frontier. The further away the country is from the frontier, the higher the potential growth rate and the longer the episode of high growth. Viewed in this way, a key fact that seems important is that even after 30 years of high growth, GDP per worker in China in 2010 is still only 17 percent that of the United States, which is roughly where Taiwan and Korea were in 1960. And after 20 years of high growth, GDP per

worker in India in 2010 is still only 10 percent that of the United States. Viewed through the lenses of a neoclassical growth model, it appears that both India and China still have at least several decades of high growth ahead.

However, even when augmented with a minimalist neoclassical framework, there are serious limitations in what the statistics can tell us about future performance. After all, there is no history of a country as large as India and China that has grown as much as they have. There is also no history of countries that were as poor as India and China were 20 years ago that have grown as fast as they have. So without additional information, I think the reasonable answer to the question, "Will growth rates in China and India continue to be high?" is that we simply do not know.

The only way to answer this question with more confidence is to dig deeper into the underlying determinants of growth. For example, what exactly was it that drove the past two decades of high growth in China? We know that China's growth was not due to the adoption of better "institutions" for business, at least as measured by, say, the widely used World Bank's Doing Business indicators. For example, China ranks 91st in the world in the overall Doing Business rankings, which puts China in the same league as Guatemala and Zambia. When measured by the "ease of entry," China ranks 151 in the world, roughly the same as the Congo. So if the improvement of institutions, at least in the way that we think about institutions, were not behind China's growth, what was it then?

One answer is what I call the Arthur Lewis/labor surplus story. Here, the story is that China's growth has been driven by the reallocation of rural workers from where their marginal product was zero (or very low) towards nonagricultural sectors where their marginal product was high. The growth of the nonagricultural sector in China then was due to the fact that they were able to grow since they faced a nearly elastic labor supply curve. According to this explanation, growth in China will come to an end once China hits the Lewis turning point, when the labor supply faced by the nonagricultural sector is no longer elastic. The problem with this story is that it implies that wages in China must have remained roughly constant or increased very little. And although it is true that the labor share has fallen in China, real wage growth has averaged over 7–8 percent per year over the past two decades. Real wage growth of such magnitude over two decades is not supportive of a Lewis reallocation mechanism driving growth.

This paper's preferred story of China's growth is what I would call the Pritchett/Suharto model of growth. Here, the mechanism driving growth is simply crony capitalism where the cronies of the political elite get benefits, and everyone else has to follow the (bad) rules. This seems like the correct

characterization of the Chinese institutional environment in the past two decades, but the question is how crony capitalism can generate such enormous growth. After all, the fundamental problem with crony capitalism is that the gains to the cronies are almost always outweighed by the losses suffered by those who are on the outside. So the central question is whether the Chinese version of crony capitalism is like that we see in countries such as Indonesia under Suharto, where growth was high for a short period of time but eventually came to an end, or whether there is something different about the crony capitalism that we see in China. I do not think we know the answer to this question, but understanding this, and not cross-country evidence of mean reversion, is the key to whether China will continue to experience high growth in the future.

COMMENTARY

Asiaphoria Meets Regression to the Mean

Robert C. Feenstra

The question this session raises is whether Asia will be a growth engine for the United States and world economy. This topic is very much in the news lately, and here is a typical quote:

> *. . . the continuing strength of China's economy in a difficult global situation present[s] opportunities for Asia to keep its economic engines strong. China will continue to become increasingly important in shaping the world's development in the years to come.*

Does that sound familiar? Well, I have actually changed a few words, and have also not been truthful in describing this as a *recent* quote. The original version is:

> *. . . the continuing strength of China's economy in a difficult global situation present[s] opportunities for Asia to keep its economic engines strong. China will continue to become increasingly important in shaping* the region's *development in the years to come. [emphasis added]*
>
> —Report of the World Economic Forum, *East Asia Economic Summit*, Hong Kong, 2001

In fact, in the years just after 2001, the policy discussion in Hong Kong was whether tourists from the mainland would be able to prop up the local economy. I recall being in Hong Kong at that time, and admit that I was quite skeptical as to whether that high-income economy could count on the boost coming from what I perceived to be low-income Chinese tourists. I could not have been more wrong, and in the years after 2001 there was a huge inflow of tourists from China. Among the 18 million people who visited Hong Kong in 2004, for example, 12 million were tourists from China, and they brought in US$7 billion of business in that year.[1] In a short 10 years since that time, we have gone from asking whether China can prop up the Hong Kong economy to now asking whether China can prop up the *world* economy.

I am sympathetic to the views expressed in this paper that we should be very cautious about making predictions about future growth in China based on

its strong past performance: Very few economies have ever kept up the growth performance that China has maintained in the past decade. But I know how wrong I have been in the past, so I will avoid making predictions about future growth in China. Instead, I would like to list a number of pro and con factors for why we might expect China to keep up its current growth rates (about 7.5 to 8 percent per annum) for some time, or not.

Let me begin by listing three reasons to be skeptical that China can keep up its current growth rate. The first reason is that a large portion of China's aggregate demand comes from investment, and it seems doubtful that China can keep this up. We are all familiar, for example, with the so-called ghost cities that have been built in China but are mostly unoccupied. These types of wasteful investment can stimulate growth in the short term, but cannot be expected to provide sustainable growth over the long term.

Second, the pension system in China is ad hoc and does not provide the long-term benefits that the population can rely on. In the absence of economic security in their old age, the population will save more when it is young and middle-aged, so that it is difficult to increase the share of consumption in GDP.

Third, an argument has been made recently that the state-owned enterprises that are dominant in the upstream sectors of the Chinese economy extract rents from the downstream firms, and therefore from the workers employed in those firms (Li, Liu, and Wang 2012). According to this argument, the low share of labor in overall gross domestic product is explained at least in part by this rent extraction. That low labor share also acts to limit consumption in the Chinese economy. Together with the difficulty of keeping up investment over the long term, this means that both potential sources of growth cannot be relied upon.

But there are also some reasons to think that growth in China may be maintained at its current rate or close to it for some time into the future. One reason is that, while pollution is high, it is also the case that pollution *abatement* is a luxury good whose share in spending grows as a country becomes wealthier. There is every reason to expect that to be the case for China. To be sure, the pollution levels at present are unacceptably high, not just to observers from abroad but to the local population. For that reason we can expect spending on green technologies to increase and be maintained at a high level for the foreseeable future.

The final factor I would like to stress is that prices in China are high—surprisingly high, in fact—which suggests that China can become an important market for goods made in the United States and elsewhere, providing some boost to global growth. The high prices found in China have led to a substantial reduction in estimates of real GDP per capita. This can be seen by considering

estimates from the World Bank *World Development Indicators*, which in turn relies on prices collected by the International Comparison Program (ICP). The ICP first included China in 2005, so earlier estimates of real GDP in that country were based on extrapolations of prices from other countries. For China, the 2005 benchmark estimates from the World Bank show that real GDP per capita for China was 40 percent smaller in 2005 than real GDP for the same year based on extrapolations. As Deaton and Heston (2010, p. 3) report, "the 2007 version of the World Development Indicators (WDI) . . . lists 2005 per capita GDP for China as $6,757 and for India as $3,452, both in current international dollars. The 2008 version . . . which includes the new [2005] ICP data, gives, for the same year, and the same concept $4,088 for China and $2,222 for India. For comparison, GDP per capita at market exchange rates is $1,721 for China and $797 for India."

The fact that the ICP prices for China in 2005 were higher than expected led to the reduction in real GDP for that country. The ICP sample for China has been criticized for being too heavily weighted towards urban areas, with higher prices. For that reason, when constructing the Penn World Table (PWT) version 7, Alan Heston has reduced the ICP prices of consumption and investment goods for that country by 20 percent in 2005, leading to an increase in the estimates of real GDP. This "20 percent solution" is also adopted by PWT version 8, which has now been taken over by the University of California, Davis, and the University of Groningen.[2]

The high prices paid in China suggest that this economy can become a purchaser of goods from the United States over the long run. A prime example of this comes from the number of students from China, some 200,000 currently, who are able to afford an education in the United States. It is remarkable that in a country whose real GDP per capita was only $4,088 in 2005, citizens who are described as middle class can afford to send their children to the United States at full tuition. This represents a tremendous sacrifice for them. Surely, these people do not literally come from the middle of the income distribution but are among the more affluent wage earners. As wages in the rest of the country rise, we can expect that the large mass of population *below* that income level will also aspire to buy goods and services at the prices prevailing elsewhere in the global economy. If quality in China remains low and safety standards poorly enforced, that will lead to demand for foreign goods (of which demand for a university education is a prime example). For these reasons, we should not be surprised to find that Chinese consumers will indeed provide some boost to global demand and growth.

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NOTES

1 "How Chinese Tourists Changed the Retailers in Hong Kong," *United Daily News*, May 30, 2005.

2 See http://www.rug.nl/research/ggdc/data/penn-world-table.

GENERAL DISCUSSION

Asiaphoria Meets Regression to the Mean

Chair: Reuven Glick

Mr. Glick: So let's take questions from the floor. Please keep your questions on point. We'll start with Martin Wolf.

Mr. Wolf: I'd like to talk about corruption and growth, because it seems to me that, Lant, you and your two discussants have all brought it up. First, Lant, you talked about how corruption and GDP are related, but surely there's another interpretation for this relationship. Developing countries can grow at 10 percent and occasionally they do, and developed countries can't grow at 10 percent and they never do. If corruption tends to be higher in developing countries, corruption will appear to be correlated with GDP. But this result need not have anything to do with corruption. For as long as rich countries never grow at 10 percent and emerging countries which have high corruption occasionally can, you'll find a correlation. This point also relates to Professor Hsieh's discussion, and perhaps Rob Feenstra's comment. You're presuming there are competing rent extractors in developing economies. It sort of reminds me of Mancur Olson's stationary bandits and roving bandits. You're saying that they're all stationary enough to do things that will raise the GDP level rather than just extract. But why aren't they just exploitative rent extractors in the way that so many Indian local bosses are? They don't develop anything. There's something in the system that forces the exploitative bandits to be productive, but what? That wasn't clear to me from your story.

Mr. Hooper: Well I thought this was a very interesting statistical analysis that came to a relatively negative conclusion that China's growth will revert to the mean. China's growth over the last two decades of course has been 9 percent plus. My question is, what is the mean in China's case? And as an observation, you noted that this was a statistical analysis. And you admit you aren't taking into account much of the specifics of what is going on in China's economy. Absent that knowledge, you make a very appealing argument for an inevitable slowdown in China. But we do know some things about China. For example, coming up is a potentially seminal event, the third party plenum. And the sorts of things we're hearing out of China that could come up suggest a dramatic move towards

privatization of the economy, including financial liberalization, including awarding thousands of new banking licenses to deal with this shadow banking system. Chang-Tai Hsieh mentioned this. Certainly, freeing up interest rates and allocating credit away from the state-owned enterprises, which Rob Feenstra mentioned, tends to be a drag on the economy, but encouraging credit expansion in 70 percent of the economy could potentially add substantially to growth. Other items up for consideration include demographic shifts, like moving from a one-child to two-child policy over time, which could add significantly to growth for the next two decades. And perhaps most important, there is the potential for major land reform, such as giving farmers title to their land. This would create a dramatic shift, a massive reallocation of wealth, moving agriculture to a more privatized system into the hands of folks who could then begin to invest and increase the productivity in a sector that is way behind. As Rob notes, the reason per capita income is so low in China is that it has an agricultural system that is way behind the times. We've seen many civil unrest events in China in recent years caused by the corruption that Martin talks about. But I think maybe your point would be moot if, indeed, we get this land transfer. The bottom line is that there is the potential for privatization giving us another decade of near double-digit growth in China. So reverting to the mean might be the case if there weren't something going on, but we know that things are happening.

Mr. Glick: Okay, one more question for this round. David Dollar.

Mr. Dollar: Thank you very much. I enjoyed all the discussion. Actually, this is really more of a comment. Chang-Tai and Rob, I actually thought you helped Lant make his case very nicely because I really took his main point as being that there's tremendous uncertainty looking ahead to any country's future growth, China and India included. And Chang-Tai and Rob, I thought both of you had good stories that helped us understand that uncertainty. Chang-Tai, I like your model of growth in China in terms of 3,000 Suharto-like crony-capitalist bosses competing for business and success. It may answer part of Martin's question, but what you didn't add is that local officials are promoted based on the growth performance of the economy. That's the mechanism Deng Xiaoping set up, to start as a local head of a little fief and then move up to be in charge of a provincial fief by generating growth. So they've created a system where there are powerful incentives for local governments to make things grow and lots of rent-seeking opportunities. But you didn't have enough time to get into how that all ends. You could easily imagine this mechanism eventually winding down with some very messy political transformation, which would fit in nicely with some of Lant's negative outcomes.

Rob, I take your point about Chinese students coming to the United States. I think there are 200,000 Chinese students now studying in the U.S., and that reflects a tremendous sacrifice for parents who desire that their children get a great education at places like UC Davis. But it also, frankly, represents hedging on the part of middle class, upper middle class, and wealthy families in China. They want their children to come to the United States, get educated here, buy property here, maybe have a baby here, and maybe get a U.S. passport. If you get the kind of political event that might come out of Chang-Tai's story, you could very well have a lot of the more well-off people leaving China. So it's easy to see how you could get some very negative scenarios of the sort Lant has suggested that we had better be aware of. The fact that China grew well for a long time does not at all ensure that it's going to continue to grow well.

Mr. Pritchett: In response to Martin's question, I did not mean to say anything about corruption per se. I could have used any number of other indicators of institutional strength and would have gotten the same result, that good institutions are not necessarily associated with higher growth. The difficulty is that a lot of the dominant discourse suggests that having good institutions is a major driver of long-run prosperity. That's a great story, and I think it helps explain why you're rich or poor, but it doesn't explain growth, that is, how fast you're headed towards being richer or poorer. I view corruption as a symptom of a weak institutional environment and don't put a lot of weight on it as a barrier to growth. What we see is a huge upside for poor countries, which means they can grow at 10 percent, whereas if you're at the frontier of total factor productivity, you can't catch up as rapidly. My point is that the dynamics we see are not the result of any advantages of economic backwardness. Gerschenkron told the story that, if you're behind and you're poor, that means you're going to catch up very quickly because it's easy to import productivity that exists in the world into your domestic economy. But what we've learned is that for very extended periods there has been zero convergence in per capita incomes. In fact, there's been massive absolute and some relative divergence in incomes, which means if you have weak institutions then you tend to persist in having weak institutions. And so what I am arguing is that countries with weak institutions have very volatile growth, which means having rapid growth is less predictive of better future growth if your institutions are weak. So I'm taking off the table this natural convergence argument, that because you're poor you're likely to grow fast.

Which moves to my response to Peter's argument. It is possible that China will do the right thing and so continue its super rapid growth for another 10

years. But we should center our expectations on the experience of lots of other countries that have had extended periods of growth. Keeping a period of rapid growth going requires active motion forward, which very few countries have been able to sustain. So I'm not discounting the possibility that the third plenum will go well and they will initiate reforms. To make an analogy, consider the path a thrown baseball follows, but you didn't see who threw it. If I threw it, the baseball would have a gravity trajectory that would tail off embarrassingly quickly, but if it were thrown by a professional baseball player, its trajectory might continue much longer than mine. But it would have to go a long ways before you would say that the law of gravity has been repealed. So I'm just saying the gravity trajectory of China should be our default trajectory. They may be stronger and better and more clever in keeping it going than we think, but that's just one scenario. In my mind, it shouldn't be the default scenario, given how hard it's been in so many other countries to keep that going.

Finally, let me go back to David's comment on the view of Chinese growth based on competition of 3,000 Suharto-like bosses. I have a paper where I look at the World Bank's Doing Business indicator, which shows how hard it would be for you to start a new firm if you followed the law. Well, that may or may not be relevant to how long it would take you to start a business in China, right? We use firm-level new activity data and the Doing Business indicators, and find that they're completely uncorrelated across countries. So the environment for doing business in China is, in fact, terrific. People want to do business in China, they just don't do business following the law, and that's the tension that creates measures of corruption. But corruption is not the primary phenomenon. The primary phenomenon is, why is it good to do business in China when you don't, in fact, have any assurance from the rule of law that will continue. That's what creates the tricky transition that China has to confront sometime. Maybe not this year, maybe not in two years, but sometime in the coming decade things will come to a head politically.

Mr. Hsieh: I'll say two things in response to Martin and David. In thinking about corruption in China, I would say it's the major source of social tension in China. In Chinese blogs it is one of the main things that people talk about. If you think about the Suharto model of growth, it has also been the engine that has made the system work. In many places of the world, corruption is what I call inefficient in the sense that these places have grown the fastest. Shanghai, for example, is massively corrupt, but in a way that basically turns the local party secretary into the residual claimant. In fact, there's been a dramatic transformation in the city of Shanghai in the last 15 or 20 years. All of the

transformation was driven by land expropriation. All of the Pudong area was expropriated from the farmers. Two-thirds of the people who used to live in the traditional downtown business district were expropriated and moved out to the suburbs. What are the consequences for efficiency, that is, what was the marginal product of land in Pudong 20 years ago relative to now? It seems clear that it's higher. Now who got the rents from that? In other societies it's the people who formerly owned the land. In the case of China, it's not.

Regarding David's comment, I think he's right in some cases, that the desire by officials to pursue policies that help growth often depends on the probability of promotion. But my sense is that this is something that only works for the people at the very top tier. The guys in these obscure Chinese cities of 200,000 people, they know they have no chance of getting promoted. In places like that, it's all about wealth, and future growth depends on enhancing efficiency and redistributing assets acquired by wealth creation practices. In other places, there has been wealth redistribution with very little efficiency consequences. Where China's overall wealth distribution is going to go, I just don't know.

Mr. Hope: I'm Nick Hope, from the Stanford Center for National Development. I've heard a lot of provocative stuff. Lant, you couldn't make a safer bet than that Chinese growth will slow despite the massive investment they continue to indulge in. I think you're right, we're all waiting with bated breath until November 9 to see whether they'll put the reforms in place that will allow future growth to be higher rather than lower. But you can take it to the bank that growth this year will be 7½ percent. President Xi guaranteed it. It might be 7.6 percent. It'll be a brave statistician who would say only 7.2 or 7.3 percent.

I want to take on Chang-Tai because I simply think that Suharto is being libeled. The model that Suharto supported was not everything for his family and friends and nothing for anybody else, it was actually something for everybody and more for his family and friends. Suharto remained in power for many years because he paid great attention to the fact that all boats should rise. He came from a poor farming family in Central Java, and he conducted policies that we didn't really think too much of. He subsidized pesticides. He subsidized irrigation. He subsidized seeds. He subsidized fertilizer. But when he lost power, it was largely because incomes that had been going up at 5 percent a year for virtually everybody suddenly went down by close to 10 percent a year during the East Asian crisis. There was a 15 percent change in income among the poorest, and people not unreasonably objected. Why do I raise this issue? I think this is the situation in China. I think the Communist party rules because it's got this explicit social contract that we do what we want, and we get away with

it because everybody's material well-being is increasing. So the central government sweats all the time about rising inequality and that, if growth slows too much, they can't provide rising incomes. They're trying their best to do it through wage increases, which seem to be extending now from formal wage increases that we can measure into the informal sector, such as the construction industry, where for a long time the Lewis growth model was more appropriate than it was in the big state enterprises or the government. This is a major distinction between China and India. India doesn't seem to have the constraint that they have to do something for the very poor, but China does. And this is where we could run into a Suharto problem in China. If something goes badly wrong and that social contract is broken, then I look for the Chinese growth rate to slow dramatically and quickly.

Mr. Williams: I have a question on one of your tables. The way you set up the analysis, is to look at an episode of high growth and then see what happened when that episode ended. It seems the question people really are asking is a little different. That is, if you have had an episode of high growth of, say, eight years, what will your growth rate be on average for the next 10 years? Another way to put it is, what's happened in China so far is eight standard deviations from what your analysis predicts because it's essentially a third of a century when you put the two episodes together. That would actually get at the question of, if you've been having high growth, what is the probability for high growth in the future. That would also include all the amazing stories of China, Taiwan, Korea, and others that obviously did have decade-after-decade episodes of ongoing growth.

Mr. Ostry: Lant, I think development economists and growth economists owe you a great debt for having focused on the discontinuities in developing countries' growth performance and how they differ fundamentally from the smooth performance of industrial countries. I was wondering what you thought about an approach that focused primarily on the duration of growth episodes. I say that not only for self-serving reasons but also because I want you to comment on something that hasn't come up much in the discussion so far. If you look at growth duration you highlight the quality of institutions and a number of Washington consensus variables as being very important, like openness to trade. But what comes out clearly as very important for developing and emerging market countries as well as industrial countries is inequality. We don't yet fully understand what all the channels are, but they probably relate to the environment for investment, both of physical and human capital, political instability, and so on. Do you think the rise of equality in China might undercut longer-term growth

prospects, as others like Rajan and Stiglitz have argued has been the case for industrial countries?

Mr. Glick: Two more questions and I'll have to close the list.

Mr. Choi: This is Woon Gyu Choi from the Bank of Korea. I have a comment associated with the case of Korea. Korea's real GDP is now 65 percent of U.S. GDP and is expected to stay around the same level through 2030 if the population aging problem is not well addressed. But if labor market issues and the population aging problems are tackled appropriately then we could rise to 85 percent of U.S. GDP by 2025. This suggests that there is still room for progress by strong emerging markets to move towards the frontier.

Mr. Glick: And the last question goes to Kei-Mu Yi.

Mr. Yi: Kei-Mu Yi from the Minneapolis Fed. This is a question for Lant but it also bears on the first part of Chang-Tai's discussion. As you know, there's a widely used empirical framework for thinking about long-run growth, starting with Barro and Mankiw, Romer, and Weil and refined over the years. That framework is grounded in economic theory, the Solow growth model adjusted and refined to include things like institutions and policies. That framework basically implies conditional convergence controlling for institutions and policies. All else equal, countries that have higher per capita income grow slower in the future. There's been a lot of empirical work in the 1990s and part of the 2000s using aggregate data that is fairly consistent with this theory. I personally consider it one of the successes in macro in terms of empirical work and theory working together. So why didn't you take the state-of-the-art conditional convergence framework and plug in China's numbers? That framework, of course, would predict that China's growth rate will slow down over time. That's the main lesson, that as China gets richer, all else equal, if its institutions and policies don't change it should slow down. As a benchmark, you can get a prediction of what its growth rate will be 10 years from now, 20 years from now, et cetera, and maybe compare it with the work that you've done.

Mr. Pritchett: Let me comment on that and work backwards. The reason I didn't do what you suggest is that I don't believe that research is very useful. I've written a lot of papers about why, but I actually think it's mostly ad hoc. It's not theoretically well grounded and doesn't, in fact, have a good track record of predicting future growth. If you used the best kind of growth regression model of the 1990s and predicted who was growing fast now, it doesn't work very well, and if you did that for the 1980s it doesn't work very well. So the variables that

are thrown in on the right-hand side morph to fit the current data. I don't like that literature at all. I've been fighting against it for 10 to 15 years and that's why I didn't start with it. That said, lots of people would do that and maybe that would work.

The second problem though is, if I'm going to count on conditional convergence then I have to forecast the conditioning variables. So if I'm going to use a relationship of $Y = \beta X$ to forecast what happens to Y in the future, I've got to know X in the future. Well, X's break down into two kinds, those that I can't predict into the future and those that I can predict, usually only because they tend to be stable over time. So it's most useless when we want it to be the most useful. Second, coming back to Korea, it happens to be, along with Taiwan, one of the few countries that has successfully managed this institutional transition from authoritarian and, frankly, guided by networks of relationships that one might call corrupt if one weren't being careful, into more transparent rule of law-based and politically sustainable democratic institutions. But the fact that only two of the 120 countries have managed to go from 5 to 65 percent growth and simultaneously manage the transition to democracy suggests it's really hard. And, hence, we should expect few countries to do it. Again, it's not impossible that India and China could do it, but our default expectation should be they're not going to do it, or they will probably perform more like the average than the outliers Korea and Taiwan. I don't doubt that Korea and Taiwan can continue further growth now that they've made this transition, but are they a good model for what will happen in India and China or not? I don't know, and I don't assume that just because parts of Korea used to touch parts of China they're similar in the relevant dimensions.

Which gets to Nick's question, what really was the Suharto model of development? With weak institutions and no clear rules, the economy depends on being able to make good "deals." Our analysis of the enterprise surveys from the World Bank versus the Doing Business surveys says if you want to know how many days it will take you to get a license to open a factory, it's not where you are, it's who you are. You can create favored firms and investors , but they still have to pay off to be profitable for you. If you can create a favorable environment that provides secure profitability expectations for investors conditioned not on their stealing money from other people but rather by being productive even with protection of the market, that can be a terrific environment for growth. That has nothing to do with having good rules, it's all about fostering good deals.

But the difficulty with deals is making the transition from one set of institutions to another, which gets to Jonathan's point. Jonathan's done great work showing that a lot of the persistent growth differentials we observe over time

reflect how long countries stay in good states versus shifting into bad states. And if we know there's a strong long-run relationship between institutions and GDP levels, then to continue on a growth path your institutions have to be improving. Because if you're not on this kind of dynamic, sooner or later your growth path will be cut short, often in a very dramatic and messy way, resulting in negative growth or long periods of stagnation. Managing this transition is strongly related to reducing inequality—not generic inequality in the sense of the Gini coefficient, but in the sense that can you make attractive, productivity-enhancing deals available to more and more investors in an orderly way. So the important question is whether policy brings more and more people into the favorable growth environment, such as Suharto was able to create for a large number of investors, not just for himself, or do the dynamics of inequality feeding back into the politics and institutions make that circle of investors narrower and narrower. The Suharto development model stopped when, even among the set of the military, largely ethnic Chinese conglomerates, and a few favored others, the set of favored investments began to narrow, inequality kicked back and blew the system up. So is inequality increasing because your cronies are capturing more of the favored deals, garnering the resentment of others outside that can torpedo the institutions? Or, as Peter just suggested, is the set of investors subject to these favorable deals getting wider and wider to the point it can morph into essentially being a new set of rules? This is the key question I've been trying to work on. It's very hard to model, to get data around, and to think about. But the upshot is, this transition is hard, and when inequality starts to spiral as it has in China, and almost as much in India, it's a sign that you're probably not on a sustainable trajectory towards better institutions because fewer other people are capturing more of the gains, rather than more people capturing more of the gains. That's not a path to the kinds of institutions that will sustain the kind of prosperity that Korea was able to achieve.

I do think that is the right point, and in the current paper, we're in-between the two points. Regression to the mean just shows you growth calculated average to decade to average to decade, whereas what you really want to know is, conditional on having a growth episode of a duration as long as China has experienced so far, what's the unconditional forecast that the episode will come to an end. I think doing the calculation you suggested is a good idea.

Mr. Glick: Okay, I want to thank Lant, Chang-Tai, and Rob for their presentations.

Crowding Out Redefined: The Role of Reserve Accumulation

Carmen M. Reinhart and Takeshi Tashiro

It is well understood that investment serves as a shock absorber at the time of crisis. The duration of the drag on investment following the global financial crisis, however, has been perplexing. For the nine Asian economies we focus on in this study, average investment as a share of GDP was about 6 percentage points lower during the period from 1998 to 2012 than its average level in the *decade* before the crisis; if we exclude China and India the estimated decline exceeds 9 percent. We document how in the wake of crisis home bias in finance usually increases markedly as public and private sectors look inward when external financing becomes prohibitively costly, altogether impossible, or undesirable from a financial stability perspective. Also, previous studies have not made a connection between the sustained reserve accumulation and the persistent and significantly lower levels of investment in the region. Put differently, reserve accumulation involves an official institution (i.e., the central bank) funneling domestic saving abroad and thus competing with domestic borrowers in the market for loanable funds. We suggest a broader definition of *crowding out*, driven importantly by increased "liability" home bias in finance and by official capital outflows. We present evidence from Asia to support this interpretation.

1. Introduction

The literature on early warnings of financial crises generally singles out overvalued currencies, widening current account deficits, large capital inflows, rising leverage, and low and declining international reserves as precursors to disaster.[1] These patterns have been prevalent in innumerable emerging market crises and (with the exception of depleted international reserves) were also prevalent in most of the recent financial crises in advanced economies. Indeed, as Gourinchas and Obstfeld (2012) document, the most notable difference between the emerging markets and advanced economies is that output declines during a currency crisis are larger in emerging markets, while other patterns are qualitatively similar.

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Faced with a sudden stop à la Calvo, (1998), current account deficits can no longer be financed by borrowing from abroad—at least not to the same extent.[2] Sharp output declines, private and sometimes public debt overhangs, and the scarcity and high cost of finance combine to produce dramatic swings in the current account balance, most often from deficit to surplus. The brunt of this adjustment usually falls on investment.[3]

The Asian crisis of 1997–98 was most acute in Indonesia, Korea, Malaysia, the Philippines, and Thailand. Yet in 1998, investment as a share of GDP fell across the board in the major Asian economies, from wealthy Japan and Singapore to lower-income India and China, which were relatively shielded from the crisis. In Indonesia, the investment-to-GDP ratio was nearly halved, falling from 32 percent in 1997 to 17 percent the following year.

The decline in investment–GDP ratios among the advanced economies since 2007 has been no less draconian, if somewhat more spread out. From 2007 to 2012, Ireland's investment–GDP ratio fell 15 percentage points. Even countries with continued access to international capital markets, such as the United Kingdom and the United States, have recorded declines of around 3½ percentage points over that time frame.

While it is not difficult to understand the role of investment as a shock absorber at the time of crisis, it is perplexing why it takes so many years after the crisis to recover—if it recovers at all. For the nine Asian economies we focus on in this study, average investment-to-GDP is about 6 percentage points lower during the period from 1998 to 2012 than its average level in the *decade* before the crisis; if we exclude China and India the estimated decline exceeds 9 percent.[4] Over the same pre- and post-crisis sample the decline in growth is 2.5 percent for all countries and 3.3 percent if China and India are excluded.[5]

A prolonged investment slump is not a new phenomenon following a deep crisis. Kaminsky and Pereira (1996), who focus on explaining the poor growth performance of Latin America vis-à-vis Asia during the crisis of the 1980s, show that public and private consumption as a share of GDP rose more than 5 percentage points in Latin America from 1982 to 1988, while for the Asian economies the comparable ratio fell by almost 2 percentage points. With public and private saving rates falling sharply in Latin America, current account deficits were closed by even larger declines in investment. The evidence they provide supports the earlier observation by Sachs (1989) that the significant income inequality in Latin America, coupled with rising political instability, were at the root of Latin American governments' inability to implement austerity measures at the time of crisis. The same factors also made downward adjustments

in private consumption less likely. In that episode, at least, investment was importantly crowded out by a rising share of public and private consumption.[6] Though it was not discussed by these authors, we would add that the decline in measured domestic saving was also significantly exacerbated by *leakages* to the domestic system in the form of private capital flight, which escalated in the region to record highs.[7]

Unlike Latin America in the 1980s (or modern-day United States), domestic saving rates in post-1997 Asia were high to very high. The significant economic inequality issues that continue to plague Latin America are not a feature of the region (notwithstanding China's increasing troubles on that front). There is little to suggest private capital flight has been an issue in the past decade. Hence, it is unlikely that many of the factors that drove the investment slump during Latin America's lost decade have been at work in Asia on a similar scale in the past 15 years. And yet, countries in both regions turned inward for financing sources in the post-crisis era (in the case of Latin America driven more by necessity).

In this paper, we aim to shed light on some of the factors that may account for the sharp and sustained decline in investment as a share of GDP in many Asian countries since the events of 1997–98. While the evidence does not support one-size-fits-all explanations, the topic may have broader resonance within and outside Asia. China and India (for different reasons) may be on the cusp of a significant investment correction; other large emerging markets (Brazil, South Africa, and others) may be similarly placed; and much of Europe and the United States are entering their fifth or sixth year of an investment slump.

It is not our goal to offer a comprehensive model of the determinants of investment, nor do we compare actual investment ratios to some optimal benchmark. We do not explore supply-side hysteresis effects of financial crises. Young's (1995) hypothesis that the East Asian growth miracle may well have been "primarily the result of one-shot increases in output brought about by the rise in participation rates, investment-to-GDP ratios, and educational standards and the intersectoral transfer of labor from agriculture to other sectors (e.g., manufacturing) with higher value added per worker" may be a primary explanation for the slowdown. But the fact remains that the Philippines and Japan—which were at very different phases of the development cycle—have experienced sustained and sharp declines in investment as well. As we discuss here, all of these countries also experienced an important shift in policy that significantly altered the allocation of domestic saving.

We focus on finance and examine trends that affect the availability of domestic funding for investment and the allocation of the pool of domestic saving. The

old concepts of *leakages* and *crowding out*, which we redefine here to be more encompassing than the conventional definitions, are central to our analysis.

In the wake of crisis, home bias in finance usually increases markedly as governments look inward when external financing becomes prohibitively costly or altogether impossible. Even in milder cases, when capital market access is not lost, governments may seek the relative stability of captive domestic audiences, such as pension funds, insurance companies, and domestic banks to lessen rollover risk.[8] This post-crisis pattern is not unique to Asia; it was prevalent in acute form in Latin America during the 1980s, and it is also a feature of the post-crisis landscape in Europe, especially (but not exclusively) in the periphery economies. In cases where the government is running substantial deficits and/or rolling over a large stock of debt, these activities would fall under the conventional definition of crowding out. Japan and India, in different dimensions, are examples where this channel may be at work. However, most of the remaining Asian economies do not have particularly high levels of public debt, nor are they financing large sustained fiscal deficits. This leads to a second (related) financing *leakages* channel.

The rise of home bias in Asia has not been symmetric. Acquiring liabilities to the rest of the world is avoided while acquiring assets (reserves) from the rest of the world is actively pursued. But the acquisition of assets is selective, favoring safety and low yields; the accumulation of these assets is not left in the hands of the private sector but orchestrated by the official sector via the central banks. The desire to keep a tight lid on current account deficits and encourage surpluses has translated into an accumulation of foreign exchange reserves on an unprecedented scale. A good deal has been written on the subject, but on the motives for holding reserves, we broadly concur with the interpretation offered in Obstfeld, Shambaugh, and Taylor (2010):

> *A primary reason for a central bank to hold reserves is to protect the domestic banking sector, and domestic credit markets more broadly, while limiting external currency depreciation. The need for such protection increases given the multiplication of risks in more financially open economies, where potential currency mismatches and a combination of internal drains (runs from bank deposits to currency) and external drains (flight to foreign currency or banks) can place extraordinary demands on a central bank's foreign exchange reserves.*

Supporting this view, we sketch in this paper a slice of Asia's history with credit events (or near-credit events) and quantify the lack of adequate reserve cover at these critical moments, which (not surprisingly) has given rise to the

insurance motive described above.[9] However, we would add that *fear of floating* is not asymmetric, and leaning against the wind of an appreciation has been an important driver of reserve accumulation since the 2007–09 financial crisis.[10]

A related literature has attempted to model reserve accumulation via a precautionary motive on the part of households or investors (Caballero and Krishnamurthy 2009 and Carroll and Jeanne 2009) but this misses the point that the acquisition of foreign assets both in timing and magnitude are determined by the central bank (i.e., the official sector). A framework closer to the motivation of a central bank concerned about possible runs is offered in Aizenman and Lee (2007). As of mid-2013 according to Federal Reserve data, about 72 percent of the U.S. Treasuries held abroad were in the hands of official institutions. Furthermore, a careful study of this phenomenon by Warnock and Warnock (2009) points out that the reported figures significantly understate actual official holdings, as oil exporters and other central banks and governments purchase U.S. Treasuries through offshore centers. The dominant role played by official entities in uphill capital flows is also extensively documented in Alfaro, Kalemli-Ozcan, and Volosovych (2013).

Some studies have stressed the social costs associated with this policy, which is closer to our theme in this paper. Rodrik (2006) focused on the income loss associated with acquiring assets that deliver a lower yield than the interest cost of borrowing abroad. Mohanty and Turner (2006) went further, suggesting that sustained reserve accumulation fuels domestic credit booms and asset price bubbles and introduces distortions in the banking system. Filardo and Yetman (2012) provide further evidence on those concerns and suggest compelling reasons why the taxes often associated with sterilized intervention can funnel activity into the riskier and nebulous world of shadow banking.[11] Jeanne (2012), who connects the accumulation of reserves in a financially repressed economy (closed capital account) to forced saving, lower consumption, and welfare losses (he is interested primarily in the case of China) comes closer to the cost of reserve accumulation we are interested in investigating. To our knowledge, these and other related papers have not made a connection between the sustained reserve accumulation and the persistent and significantly lower levels of investment in the region.[12] Put differently, in past literature reserve accumulation has involved an official institution (i.e., the central bank) funneling domestic saving abroad and thus competing with domestic borrowers in the market for loanable funds. We suggest a broader definition of *crowding out*, driven importantly by official capital outflows, that is applicable to most Asian economies (and a significant number outside Asia) to varying degrees. In principle, reserve accumulation could also crowd out private consumption (as in the case

of China) or public consumption. Our focus on total (private and public) investment is driven by the large and persistent declines we have already alluded to.

The paper proceeds as follows. The next section examines the evolution of the current account and investment in a 26-year window around the Asian crisis of 1997–98; for comparative purposes, we present the corresponding macroeconomic data for Europe and the United States from the 10 years prior to the 2007–09 crisis to the present. In Section 3, we use our historical data on the level and composition of debt to document the rise of home bias. Section 4 presents and reinterprets the evidence on reserve accumulation as a source of crowding out. The hybrid Asian experiences, which encompass high debt with current account surplus in Japan and low debt with current account deficit in Indonesia, are discussed in the context of the home bias-crowding out framework. Concluding remarks focus on further research and speculate to what extent elements of the Asian experience are present elsewhere.

2. Crises and Investment: The Long Aftermath

The literature on sudden stops (see Calvo 2012) and capital flow surges (Reinhart and Reinhart 2009) has documented the sharp reversals of the current account that take place at times of crisis, including the Asian crisis episode of 1997–98. In this section, we document current account reversals that *persisted* well after the financial crisis was over. Linked by an accounting identity (briefly discussed below), we connect the current account reversal to a change in investment; the corresponding exercise for growth is presented in the Appendix. To set the stage for the discussion on the connection between domestic and external debt and reserves, we review episodes of Asia's brush with default and restructuring (or near default).

2.1. Basics

The simple rules of double-entry accounting ensure that, excluding statistical discrepancies, the capital account surplus or net capital inflow (denoted by KA) is related to the current account surplus (denoted by CA) and to the official reserves account RA of the balance of payments through the identity,

$$CA + KA + RA \equiv 0 .$$

Notice that $RA < 0$ implies an accumulation of reserves by the monetary authority.

A property of the current account is that it measures the economy's net foreign wealth. A country that runs a current account surplus must have a capital account deficit (private capital outflow) or an increase in reserves (an official outflow). Another related identity is that the current account surplus equals the difference between national savings (S) and national investment (I),

$$CA \equiv S - I\,.$$

2.2. External Balances

Table 1 presents selected developments for the current account for two groups of countries: the nine Asian economies we focus on (China, India, Indonesia, Japan, Korea, Malaysia, Philippines, Singapore, and Thailand); and a comparison group composed of Europe and the United States. For Asia, we compare 1987–97 (the 11-year period in the run-up to the Asian crisis) with 1998–2012—the 15-year aftermath. We avoid extending the comparison further back, as Asia was also in crisis in the earlier part of the 1980s. For the Europe and U.S. sample we compare the 1997–2007 pre-crisis with the five years after (2008–12). Table 1 reports the peak deficit level and year it was recorded from 1980 to 2012. The memorandum item calculates the pooled means for the relevant periods before and after the crisis.

Starting with Asia in 2012 (last column), there are two groups, India and Indonesia, with current account deficits and all others with surpluses of varying magnitudes—an issue we will address again in Section 5. Of the core Asian crisis countries (Indonesia, Korea, Malaysia, Philippines, and Thailand), all except Indonesia show a deteriorating current account in the 11 years ahead of the crisis and an improvement in the subsequent 15 years (first two columns).[13] Indeed, if we had ended the exercise in 2011 Indonesia would also be showing a surplus and subsequent improvement. A more meaningful comparison than point-to-point is the pooled means. For 1987–97, the average current account deficit is 0.1 percent; during the 15 years after the crisis the average turns to a surplus of 5.4 percent (the 5.5 percent difference is statistically significant at all standard levels of confidence).

For the Europe and U.S. sample, which involves a mix of debtor and creditor countries, a common pattern in the crisis countries (marked by an asterisk) is the swing from a worsening deficit to a post-crisis surplus, as in Asia. The most dramatic turnaround shown in Table 1 is Iceland, which records a deterioration of 14 percent, followed by an improvement of nearly the same magnitude. As of 2012, Ireland, Portugal, and Spain had shifted into impressive surplus territory.

TABLE 1

Current Account Balance/GDP, Levels and Changes: Selected Countries, 1980–2012

Country	Change over:		Peak deficit: 1980–2012		
	1997–2007	2007–2012	Level	Year	2012
Europe and United States[a]					
Austria	6.0	–1.7	–5.2	1980	1.8
Belgium	–3.6	–3.5	–4.1	1981	–1.6
Denmark	0.8	4.2	–6.0	1986	5.6
Finland	–1.3	–6.0	–5.4	1991	–1.8
France	–3.7	–1.2	–2.2	2012	–2.2
Germany*	7.9	–0.5	–1.7	1980	7.0
Greece*	–9.4	11.2	–14.9	2008	–3.4
Iceland*	–13.9	10.8	–28.4	2008	–4.9
Ireland*	–8.8	9.8	–13.6	1981	4.4
Italy*	–4.1	0.5	–3.6	1980	–0.7
Netherlands*	0.1	3.4	–1.0	1980	10.1
Norway	6.2	1.7	–6.1	1986	14.2
Portugal*	–4.3	8.6	–14.6	1981	–1.5
Spain*	–9.9	8.9	–10.0	2007	–1.1
Sweden	5.3	–3.4	–3.3	1980	6.0
Switzerland	–0.7	2.6	–0.6	1980	11.2
United Kingdom*	–2.1	–1.6	–4.6	1989	–3.8
United States*	–3.3	2.2	–5.8	2006	–2.7
Average	*–2.2*	*2.6*	*–7.3*		*2.0*
Memorandum items:	*1987–1997*	*1998–2012*	*Difference*		
Average level	0.7	0.3	–0.3		
No. observations	198	90			

Country	Change over:		Peak deficit: 1980–2012		
	1987–1997	1997–2012	Level	Year	2012
Asia					
China*	3.8	–1.5	–3.7	1985	2.3
India	0.4	–3.5	–4.8	2012	–4.8
Indonesia*	1.3	–1.0	–7.5	1983	–2.7
Japan[b]	–1.2	–1.2	–1.0	1980	1.0
Korea*	–9.1	5.4	–8.3	1980	3.8
Malaysia*	–14.2	12.0	–13.4	1982	6.1
Philippines*	–5.3	7.6	–6.9	1980	2.9
Singapore	16.0	3.1	–13.1	1980	18.6
Thailand*	–1.3	2.1	–8.3	1990	0.0
Average	*–1.1*	*2.6*	*–7.4*		*3.0*
Memorandum items:	*1987–1997*	*1998–2012*	*Difference*		
Average level	–0.1	5.4	5.5		
No. observations	99	135			

Sources: International Monetary Fund (2013c) and Reinhart and Rogoff (2009).

a An asterisk denotes a banking crisis in the "common crisis year"; the common crisis years for the advanced and Asian economies are 2007–08 and 1997–98, respectively. The years refer to the start of the crisis.

b Japan's financial crisis began in 1992.

Notes: The difference in pooled means tests are significant at standard confidence levels.

2.3. Investment in the Shadow of Sudden Stops

Table 2 presents the investment–GDP ratio in the same format as Table 1, replicating the same coverage of countries and time. The table shows the phenomenon we are trying to understand. With the exceptions of China, India, and Indonesia, the second post-crisis column stands out by showing the large declines in investment following deep crises. The top panel showing Europe and the United States is even more uniform in this regard, as not a single country records a higher level of investment in 2012 than in 2007. As with the current account, we place more weight on the pooled means reported as memorandum items. These show a 6.2 percent decline in investment for the full Asian group. If China and India are excluded, however, the decline is 9.4 percent. The standard difference in means tests yield significant results at all standard levels of confidence.

Five years into the aftermath of what began as the subprime crisis in the United States in the summer of 2007, the European economies and the United States seem to be on a similar track. The pooled means point to a decline of 2.6 percent after the onset of the crisis. Furthermore the magnitudes of the declines in a number of the periphery countries match and surpass the investment in Asia.

The investment slump unfolds during a period of lower average growth. Appendix Table A1 replicates the format used in Tables 1 and 2. The pooled estimates show mean growth in the 15-year span after the crisis as 2.5 percent lower for the nine-country Asian sample and 3.3 percent lower if China and India are excluded.

Having shown that current account surpluses and lower investment ratios and growth are the post-crisis "new normal," we now turn to the crisis episodes that cemented the policies of self-insurance via large-scale reserve accumulation. The motivation for governments to hold reserves is well established in the literature, as discussed in Obstfeld, Shambaugh, and Taylor (2010) and Calvo, Izquierdo, and Loo-Kung (2012). The sketch presented here of past crises only serves to highlight the gap between then and now for international reserve "covers."

2.4. Disasters and Near Disasters

Table 3 presents a list of external credit events in the form of outright default, debt restructuring, or "near train wrecks" in that the country was on the verge of default. This list does not include banking, currency, and inflation crises; the dates for these can be found in Reinhart (2013). While the intent of

TABLE 2

Investment–GDP Ratios, Levels, and Changes: Selected Countries, 1980–2012

Country	Change over: 1997–2007	Change over: 2007–2012	Peak: 1980–2012 Level	Peak: 1980–2012 Year	Level 2012
Europe and United States[a]					
Austria	–1.9	–0.5	31.72	1980	22.68
Belgium	2.0	–2.0	25.93	1980	21.00
Denmark	2.6	–6.3	23.37	2007	17.06
Finland	3.7	–3.1	30.44	1989	19.75
France	5.1	–2.2	23.36	1980	19.80
Germany	–2.1	–2.0	28.16	1980	17.26
Greece	5.9	–13.1	26.72	2007	13.59
Iceland	9.3	–14.5	35.61	2006	14.55
Ireland	4.7	–15.3	28.16	2006	10.86
Italy	2.4	–4.5	27.59	1980	17.62
Netherlands	–1.8	–2.9	23.96	1989	17.53
Norway	2.4	–0.7	31.34	1986	25.05
Portugal	–3.4	–6.1	37.99	1982	16.70
Spain	8.9	–11.2	30.98	2007	19.75
Sweden	4.1	–1.5	23.99	1989	18.84
Switzerland	–0.1	–1.6	30.44	1990	20.98
United Kingdom	0.8	–3.5	21.99	1989	14.65
United States	0.0	–3.3	25.08	1984	19.05
Average	**2.4**	**–5.2**			**18.15**
Memorandum items:	*1997–2007*	*2008–2012*	*Difference*		
Average level	21.85	19.23	–2.6		
No. observations	198	90			

Country	Change over: 1987–1997	Change over: 1997–2012	Peak: 1980–2012 Level	Peak: 1980–2012 Year	Level 2012
Asia					
China	0.9	10.9	48.85	2012	48.85
India	3.3	9.8	38.11	2007	35.62
Indonesia	–10.0	3.6	47.71	1989	35.32
Japan[b]	–0.4	–7.5	32.53	1990	20.60
Korea	4.6	–7.9	40.06	1991	27.55
Malaysia	19.9	–17.2	43.64	1995	25.77
Philippines	7.6	–8.9	32.84	1983	18.46
Singapore	1.0	–10.2	46.95	1984	27.00
Thailand	5.8	–3.9	42.84	1991	29.74
Average	**3.6**	**–3.5**			**29.88**
Memorandum items:	*1987–1997*	*1998–2012*	*Difference*		
Average level	33.36	27.14	–6.2		
No. observations	99	135			

Sources: International Monetary Fund (2013c) and Reinhart and Rogoff (2009).

a An asterisk denotes a banking crisis in the "common crisis year"; the common crisis years for the advanced and Asian economies are 2007–08 and 1997–98, respectively. The years refer to the start of the crisis.

b Japan's financial crisis began in 1992.

Note: The difference in pooled means tests are significant at standard confidence levels.

TABLE 3
Credit Events (Restructuring, Default, and Near-Default), Debt, and International Reserves: 1958–2002
(Debt as a Percent of GDP)

Country	External credit events *Italics* = near defaults	Total external debt (public plus private)	Central government debt			Reserves/ external debt
			External	Domestic	Total	
India	1958	*12.7*	2.3	12.7	15.0	n.a.
	1969	*15.2*	2.5	15.2	17.7	8.1
	1972–1976	13.8	2.5	24.3	26.8	9.1
	1989–1990	25.2	8.0	15.7	23.7	5.1
Indonesia	1966–1970[a]	46.9	n.a.	n.a.	n.a.	3.4
	1998–2000	158.7	56.5	10.5	67.0	15.0
	2002	65.5	35.0	35.3	70.3	24.2
Korea	*1979–1980*	34.9	7.6	3.8	11.4	12.9
	1997–1998	26.5	3.0	7.0	10.0	14.9
Philippines	1983–1992	72.9	22.8	11.3	34.1	3.1
Thailand	*1997–1998*	72.7	9.5	0.7	10.2	23.9[b]
Memorandum item:		*Average 2013 reserve–external debt ratio for India, Indonesia, Korea, Philippines, and Thailand*				*85.5*

Sources: Bloomberg, International Monetary Fund (2013b,c), League of Nations, *Statistical Abstract* (various issues), Park (2005), Reinhart and Rogoff (2009), Reinhart (2010), United Nations (1948) and *Yearbook*, various issues, World Bank (2013), World Bank, *Quarterly External Debt Statistics*, (QEDS), http://web.worldbank.org/WBSITE/EXTERNAL/DATASTATISTICS/EXTDECQEDS/0,,menuPK:1805431~pagePK:64168427~piPK:64168435~theSitePK:1805415,00.html

a As no data is available for 1966, we report 1970.

b The amount of reserves reported by the Bank of Thailand did not net out U.S. dollar reserves borrowed in the forward market; nonborrowed reserves were significantly lower.

this discussion is not to evaluate the indicator properties of debt or reserves, two features of Table 3 are noteworthy. First, with the exception of Indonesia's external debt in 1998, none of the debt ratios at the time of crises seem obviously high—certainly not by advanced economy standards. In effect, Indonesia's external debt–GDP ratio in 1997 was 63.2 percent, an implosion in the rupiah and a sharp decline in GDP drove the debt ratio higher by nearly 100 percent in less than a year. In seven of the 11 episodes shown external debt levels would have met the Maastricht criteria—indeed, Reinhart, Rogoff, and Savastano (2003) show that in more than one-half of the defaults since 1970 that also would have been the case. Second, as the last column reveals, while external debt might not have been alarming in its own right, the central bank did not have the reserves to back even a quarter of the outstanding debt stock at the time the crisis broke out. In nearly half the cases, the reserve backing amounted to less than 10 percent of the hard currency debt. The point that the common practice of focusing on reserves-to-import ratios was not especially useful to convey vulnerability to financial crises and that reserves should be compared to the potential stock of liabilities these must back (such as M2 or external debt, or at

least the short-term component of debt) was forcefully made by Calvo and Mendoza (1996).

The crisis experience sets the stage for both a policy that redirects government borrowing toward the domestic market and a central bank that strives to build a foreign exchange war chest as a financial stability policy tool. To what extent private external borrowing was discouraged through macroprudential measures or more explicit capital controls has varied considerably across countries and across time.

In the next section, we document the evolution of home bias in public finance and provide some markers on the magnitudes of the accumulation in foreign exchange reserves.

3. Home Bias

Home bias in finance has many dimensions, so it is impossible to measure it by a single indicator or even a handful of indicators. We do not use the term as it's used in the original work of French and Poterba (1991) to describe low shares of foreign equity in domestic portfolios or low shares of foreign bonds in private domestic portfolios. We focus on "liability home bias," which we define as a preference for borrowing domestically (and usually in the domestic currency) after the 1997–98 crisis. In what follows, we focus primarily on the internal-external composition of government debt as well as the evolution of external total debt (public plus private) in comparison to domestic credit to the private sector.

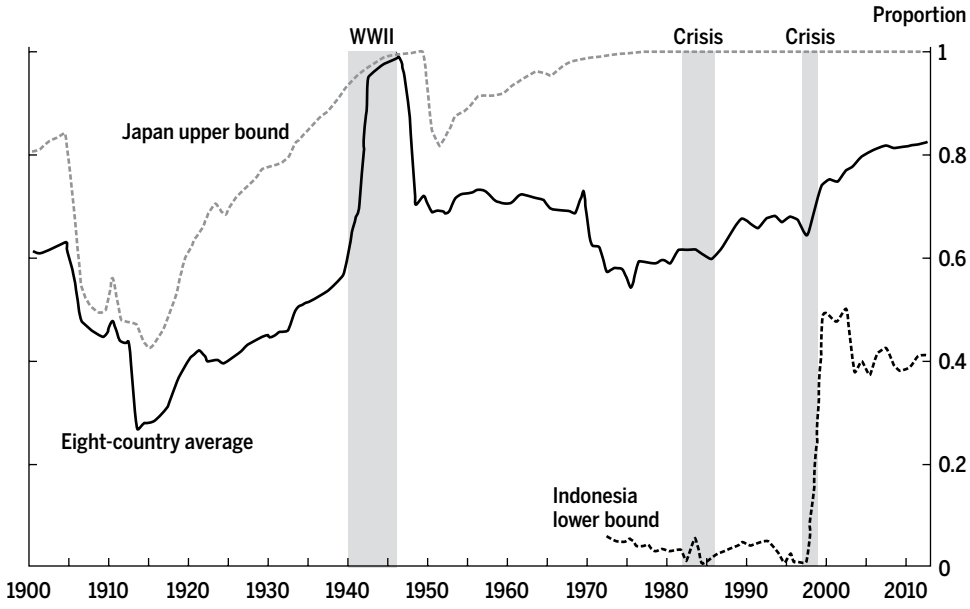
3.1. Domestic and External Public Debt

The analysis here builds on Reinhart and Rogoff (2009, 2011), who trace out the long (and forgotten) historical evolution of domestic public debt. The debt series begins as early as 1835 for "British" India and 1872 for Japan; for Korea and Thailand the starting date is 1913; for Malaysia and the Philippines it is the 1940s and so on. The data for China are the least comprehensive in every dimension, both in terms of time frame and coverage. The Data Appendix available online (see Reinhart 2013) provides the details. Figure 1 traces the share of domestic central government debt; as such the range of variation is bounded by zero and one. The solid line is the average for eight of the nine economies (excluding China); the gray dashed line plots the ratio for Japan, which is the country with the highest share of domestic debt for the most extended period of time, while the black dashed line is the time series for Indonesia, which recorded the most significant dependence of external borrowing right up to the 1997 crisis (and even subsequently).

FIGURE 1

Share of Domestic Debt in Total Central Government Debt

Seven Asian Economies, 1900–2012: The Rise of Domestic Debt



Sources: Detailed sources for each country are provided in Reinhart (2013).

Notes: The shaded areas encompass WWII and year in which three or more of the seven Asian economies included experienced systemic banking crises; these two episodes span 1982–85 and 1997–98.

On the basis of the eight-country average, it is evident that, with the exception of the financial autarky imposed by World War II, Asian governments relied heavily on external financing. In the period of financial repression and capital controls during Bretton Woods, home bias was higher than in the more liberal international capital markets of the late 1970s and beyond. During the multiple crises in Asia from 1980 to 1986 (these were not as synchronous as 1997–98) about 40 percent of government debt was external. After that crisis the pendulum began to swing toward domestic debt and the share of domestic debt edged higher still. The average shows the share of domestic debt hovering around 80 percent, but that largely reflects that Indonesia and the Philippines still rely far more heavily on external debt than the others. For Japan and Singapore domestic debt is the whole story, while for India, Korea, Malaysia, and Thailand domestic debt currently accounts for 90 percent or more of the total.[14]

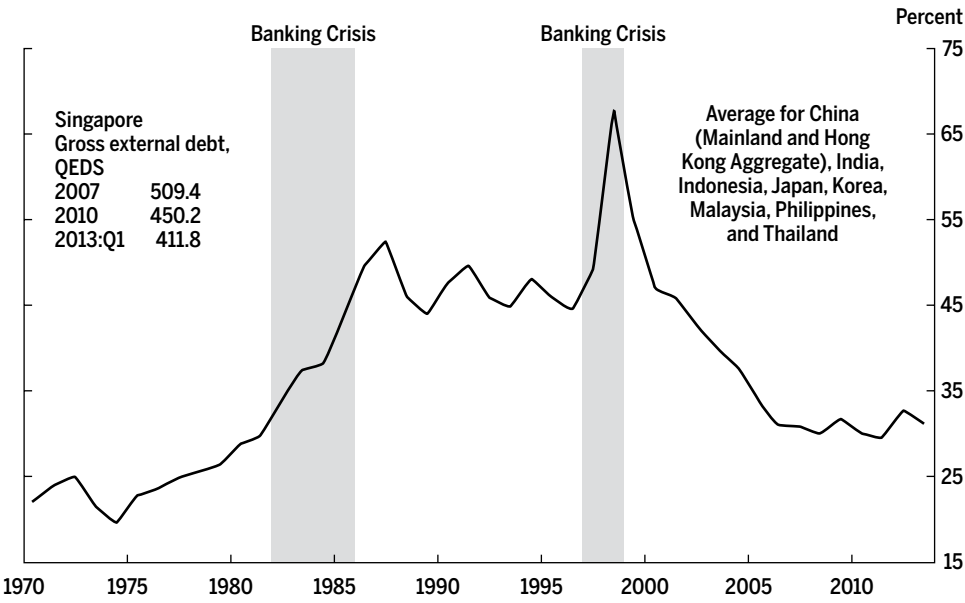
3.2. Total (Public and Private) External Debt

The preceding evidence is confined to the central government. Historically a substantive part of external borrowing has come from other sectors of government, such as enterprises and, of course, the private sector. To ascertain how total public and private external indebtedness has evolved, we plot the average external debt-to-GDP ratio for eight of the nine economies from 1970 to 2013:Q1. Singapore, which ranks fourth in the *Global Financial Centres Index* (Z/Yen Group 2013), is the obvious outlier and is excluded from the average shown. Singapore's gross external debt, which is a multiple of GDP, is shown in the inset of Figure 2. Of the 69 countries that participate in providing quarterly gross external debt data in the joint IMF-World Bank Quarterly External Debt Statistics (QEDS) exercise, only four report higher external debt ratios than Singapore: Iceland, Ireland, Luxembourg, and Mauritius. The latter two are offshore centers and the first two were in the past. For China, we report the aggregate external debt of Hong Kong and the Mainland relative to the aggregated GDP.

FIGURE 2

Total (Public Plus Private) External Debt: Selected Asian Economies, 1970–2013:Q1

(Percent of GDP)



Sources: Detailed sources for each country are provided in Reinhart (2013).

Note: Shaded areas encompass years in which three or more of the seven Asian economies included experienced systemic banking crises; these two episodes span 1982–85 and 1997–98.

In line with the home bias redirection, total external debt has been halved since 1987 and hovers around 30 percent. While deleveraging usually follows a deep crisis, the debt reduction, as we shall see, has been far more pronounced for external debt. Indeed, several countries in the region are concerned about an internal credit boom.

3.3. Hidden Debts, Contingent Liabilities, and Private Credit

> *"There are four things every person has more of than they know: sins, debt, years, and foes."* —Persian proverb

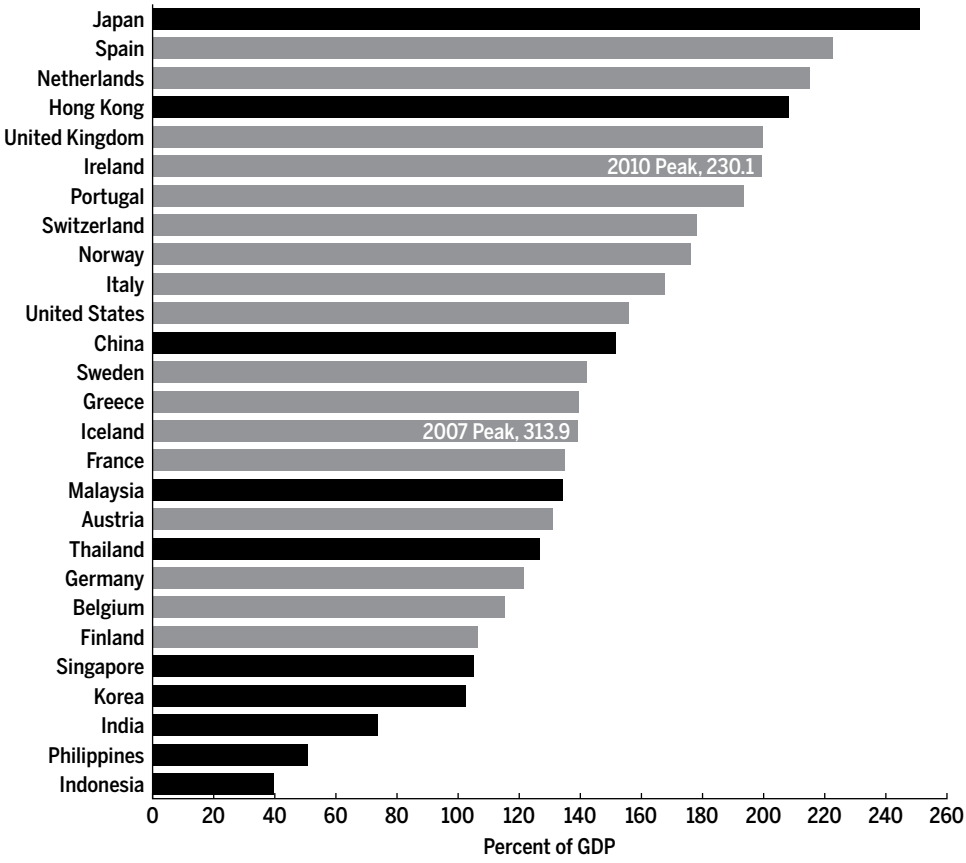
The broad debt picture would not be complete without a discussion of domestic credit to the private sector, with its history of past booms and busts and where significant vulnerabilities have re-emerged in a number of the countries studied here.[15] The ratio of domestic credit to the private sector outstanding to GDP as of the second quarter of 2013 is shown in Figure 3 for the Asia, Europe, and U.S. sample.[16] As with the composition of public debt and the extent of external private and public indebtedness, there is considerable cross-country variation within our sample, but the general prevailing pattern (with the exceptions of the Philippines and Indonesia) is that in the remaining seven countries domestic levels are high (if not necessarily at peak levels) by their own historical standards, if not by a broad cross-country comparison (as shown in Figure 2).

Japan's ratio is the highest (slightly above 250 percent) and it hovers near its 1996 peak. The banking crisis in Japan began in 1991 and, unlike in most severe banking crises where debt ratios begin to decline usually two years after the crisis, deleveraging was slow and partial, as the debt ratio never dips below 215 percent (see Reinhart 2013). Korea's drastic external deleveraging after the crisis has no obvious domestic counterpart, as the domestic credit ratio continued to climb. At 103 percent as of mid-2013 the credit–GDP ratio is only slightly below the 109 percent peak in 2009. Thailand and Malaysia have also experienced a marked rebound in domestic private credit, especially connected to household debt. China, while still classified as a low-income country, has a domestic credit ratio that is on par with advanced economies; these data reflect the growth of domestic credit in the "formal" banking sector. Hong Kong's average annual increase in credit–GDP ratio during 2007–13 exceeded Ireland's during the decade before the crisis (1997–2007) and has only been surpassed by Iceland's credit boom over the pre-crisis decade (Figure 4). Credit ratios are at all-time highs for Hong Kong, India, Singapore, and the group as a whole (Figure 5).[17]

A missing component in this analysis is domestic securitized debt, which varies in importance with the size of the domestic bond market across these

FIGURE 3

Domestic Credit to the Private Sector Outstanding as a Percent of GDP, 2013:Q2



Sources: Board of Governors of the Federal Reserve System (2013) and International Monetary Fund (2013b).

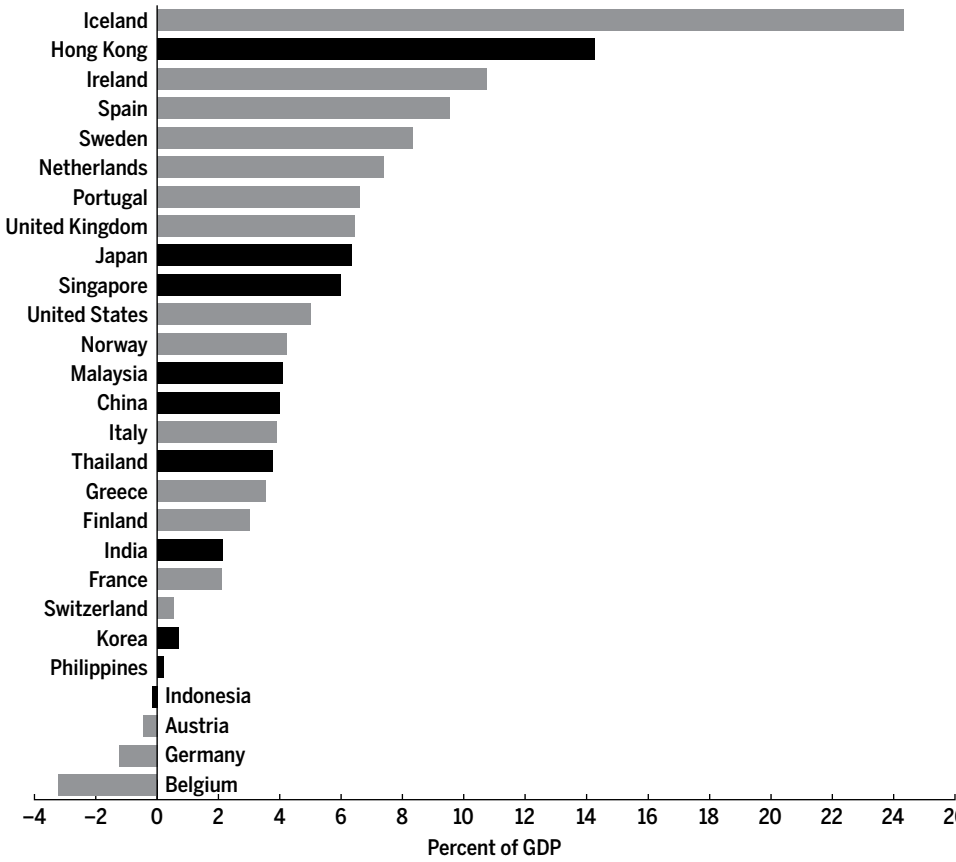
Notes: Black bars are for Asian countries, and gray bars are for Europe and the United States. Data for Korea, Norway, and Philippines are through 2012. For the United States, debt outstanding of the nonfinancial private sector is used in lieu of domestic bank credit.

countries. Also, important hidden debts for the case of China are provincial liabilities often contracted through the burgeoning shadow banking sector in that country and directly tied to real estate activity. To the extent that central banks issue their own debt to facilitate open market operations or to sterilize the effects of large purchases of foreign exchange reserves, this type of domestic debt is also not quantified here.[18] In Malaysia, there are ongoing discussions of hidden debts in the form of an assortment of off-balance-sheet expenditures involving government enterprises.

FIGURE 4

Average Annual Change in Domestic Credit–GDP Ratio, Asia, Europe, and the United States, 1997–2013:Q2

(as a Percent of GDP)

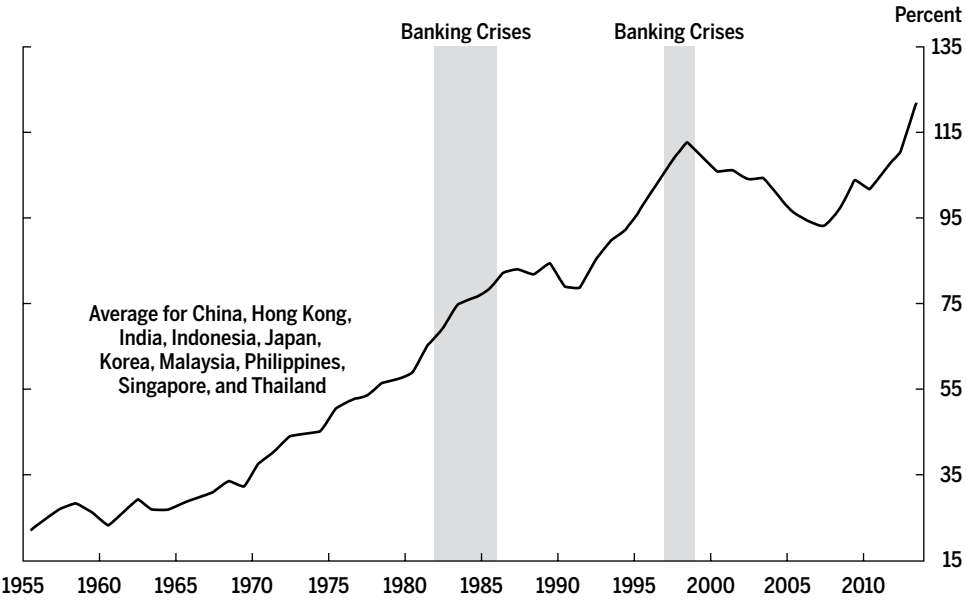


Sources: Board of Governors of the Federal Reserve System (2013) and International Monetary Fund (2013b).

Notes: Black bars show average annual change for Asian countries, 2007–13. Gray bars show average annual change for Europe and the United States, 1997–2007. Data for Korea, Norway, and Philippines is through 2012. For the United States, debt outstanding of the nonfinancial private sector is used in lieu of domestic bank credit.

We have not yet discussed public debt as whole, but we observed that these have become increasingly domestic, as external debt levels have declined and the share of domestic debt in the total pie has risen. The preceding discussion highlighted the growing private domestic debts. Taken together, the implication is that, for Asia's largest economies, domestic leverage is an issue of some concern. Banking crises need not have an external dimension.

FIGURE 5
Domestic Credit, Restructuring, and Banking Crises, 1955–2013:Q2
(End-of-period as a Percent of GDP)



Sources: International Monetary Fund (2013b), Reinhart and Rogoff (2009) and sources cited therein.
Note: Shaded areas encompass years in which three or more of the seven Asian economies included experienced systemic banking crises; these two episodes span 1982–85 and 1997–98.

3.4. Original Sin and Debt Intolerance

The discussion that follows revolves around seven of the nine sample countries, excluding Japan and Singapore. On the surface, the preceding discussion would suggest that the challenges posed by "original sin" in Eichengreen, Hausmann, and Panizza (2005) have been overcome in this sample. Before jumping to that conclusion, we note that the full original sin phenomenon, as described by the authors, had two dimensions: first, the inability of governments to borrow domestically (in the domestic currency) for the long term at fixed rates; second, the inability of governments to borrow in their own currency abroad (i.e, debt issues under international law).

We have presented evidence that governments turned inward in their funding pattern but there is considerable variation within the group. China and India have historically had a significant domestic debt market, (understandably given their size and inward development strategy for many decades). Korea, Malaysia, and Thailand, which relied extensively on external funding (had both dimensions of original sin) have shifted overwhelmingly to domestic debt

fulfilling the criteria described above. Indonesia (see Figure 1) and the Philippines have also shifted markedly to domestic funding, but external debt still accounts for about half of central government debt.

We have also documented the more generalized (encompassing the private sector and other layers of government) pattern of declining external debt and rising domestic debt. This trend is silent, however, on the external dimension of whether these governments are able to place domestic currency debt in international capital markets, as we have not examined the currency composition of debt issues under external law. In sum, the domestic strand of original sin has significantly diminished, but that is about all that can be said.

Our analysis is equally silent on whether *debt intolerance* has been overcome or not, as external debt levels are low by historical standards. Table 3 is a reminder that crises often occurred at low (and sometimes extremely low) levels of external debt, which is the essence of debt intolerance. At present, the Indian rupee together with Brazil's real, China's renminbi, South Africa's rand, and the Turkish lira, have been dubbed the fragile five.[19] Since the spring of 2013, the central banks of India and Indonesia have lost a substantive amount of reserves in efforts to stem a slide in the currency; India has introduced measures to limit capital flight. As of the first quarter of 2013, India's and Indonesia's total external debt as a percentage of their GDP amounted to 19.8 percent and 26.8 percent, respectively.

4. Crowding Out Redefined

In this section, we revisit the conventional definition of the old concept of crowding out as it applies to Asia. We move on to redefine and broaden the concept of crowding out to the official sector at large, which includes central banks. In light of this broader definition of the official sector, we ask whether the record reserve accumulation that took root at the time of crisis in much of Asia is related to the persistently lower levels of investment since 1997–98. Put differently, we ask whether central bank reserve accumulation has been crowding out private investment in the past 15 years.

4.1. Conventional Crowding Out

Crowding out is usually understood as the process through which increased government borrowing displaces investment spending. If the government is competing with the private sector for a limited supply of loanable funds, then the higher public borrowing crowds out private investing. This crowding out can occur via the rising cost of borrowing for firms, or it can occur without rising interest rates if the government receives preferential access to the supply

of loanable funds. Financial regulation can (and often does) tilt the balance in favor of government debt; financial repression does this more explicitly, often through directed credit.[20] Crowding out is typically a pressing policy concern when the public and private sectors' access to international capital markets is limited or non-existent, when government new financing needs are large, and when the government has to roll over large debts on a continual basis. To this list we would add the obvious point that, in cases where foreign saving cannot be tapped, the smaller the pool of domestic saving (all else equal), the greater the problem of crowding out.

As with other indicators, the range of variation in public debt profiles within the region is vast, both in terms of debt levels and their composition.[21] Table 4, which provides a snapshot of the public debt in selected countries in the region, highlights this diversity. Public debt-to-GDP levels range from around 20 percent for Indonesia and China (for the latter it is likely an understatement) to Japan's record (a multiple of 12) near 240 percent. These extremes support the obvious point that the fiscal policy challenges faced by these countries are of a very hybrid nature. Our intention is not to have a discussion of the complex regional fiscal panorama but to focus on two cases where the conventionally defined challenge of crowding out is most applicable, Japan and India.[22]

4.1.1.Japan

Since 2009, Japan's general government debt-to-GDP ratio has surpassed 200 percent (Figure 6). According to the Reinhart and Rogoff (2009) historical public debt database, few sovereigns have recorded comparable levels. Of the seven episodes we identify of a debt–GDP ratio above 200 percent (France 1921–23;

TABLE 4
Central Government Debt: Selected Asian Economies as a Percent of GDP

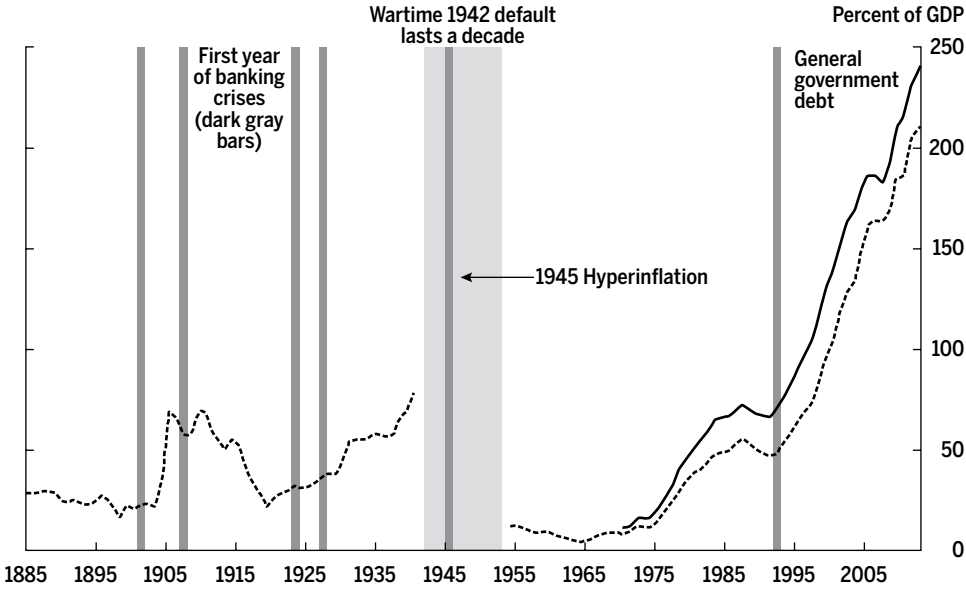
	China*	India	Indonesia	Japan	Korea	Malaysia	Philippines	Singapore	Thailand
Start of coverage	1984	1835	1972	1872	1913	1949	1948	1963	1913
Average, all years	13.0	28.8	35.6	50.0	15.2	44.6	35.4	67.6	17.2
Peak year	2010	1945	2000	2012	1938	1987	2004	2012	1986
Peak level	33.5	79.5	95.2	209.7	35.8	106.0	74.4	111.4	40.1
Average, 1980–2013	13.0	38.0	41.7	100.5	19.4	57.8	55.3	82.7	24.7
Change, 2007–2013	3.3	0.2	–10.0	45.4	3.4	12.2	-6.5	21.1	6.3
2013	22.9	40.3	20.9	208.7	33.1	52.3	47.4	108.4	30.3
Memorandum item: General government									
Start of coverage	1984	1991	2000	1970	1990	1990	1994	1990	1996
2013	22.9	67.2	26.2	243.5	35.7	57.0	41.2	107.8	47.1

*For China, General Government Debt is used in lieu of Central Government Debt.

Sources: Detailed sources are provided in Reinhart (2013).

FIGURE 6

Japan: Central and General Government (Domestic Plus External) Debt, Default, and Banking Crises, 1885–2013



Sources: Financial Bureau, Ministry of Finance, Japan; League of Nations, *Statistical Abstract*, various years; Reinhart and Rogoff (2009) and sources cited therein; Reinhart (2010); United Nations (1948); *Yearbook*, various issues.

Note: Dark gray vertical bars show first year of banking crises.

Germany 1944; Greece 1894–97; Netherlands, 1821–53; New Zealand 1932–34; and the United Kingdom 1813–25 and 1945–48), only two lasted longer than five years.[23] In three (Germany, Greece, and New Zealand) of the seven cases, the country was in full default or undergoing a restructuring. Still, in the four episodes that did not involve a default or restructuring, the debt was intimately connected with a war or a series of wars; France in the aftermath of World War I, the United Kingdom in the wake of the Napoleonic War and World War II, and the Dutch, who fought two wars in what is now Indonesia in 1821–37 while engaged in the Belgian fight for independence (1830–39) at home.

To say history offers little guidance on how the debt is unwound in these more extreme cases is an understatement. On the surface, the two longer episodes involving the United Kingdom and the Netherlands in the 1800s are of greater interest. Both countries played a prominent role as international financial centers at the time, and as the high level of public debt coexisted with high levels of private saving, both were creditors to the rest of the world (like Japan). Even so, the comparison is a stretch, as the two countries enjoyed a stream of

substantial transfer of resources from their colonies that modern-day Japan cannot count on, and in both cases the tapering of war efforts played a role in debt reduction that is strikingly at odds with the concerns of an aging population.[24] It is noteworthy that the post-1900 episodes (even in less extreme cases than those listed here) often involved substantial debt erosion through inflation or financial repression.[25]

More to the point of this paper, since its peak shortly before the 1991 crisis (Table 2), Japan's investment ratio has declined by 12 percentage points while household consumption as a share of GDP has risen by about 6 percentage points over that period (not a surprise in light of the country's aging population). Private debt has remained above 210 percent of GDP, where it has oscillated since the mid-1980s. Government debt, which was 47 percent of GDP in 1991 on the eve of crisis, has multiplied by a factor of five. In light of these combined developments, it may be possible in an in-depth case study to fully account for the observed investment slump. However, as the next section discusses this is not the full picture; a significant share of Japan's savings was channeled abroad through reserve accumulation (official capital outflows) during this period. Foreign exchange reserves (excluding gold) relative to GDP rose from about 2 percent of GDP in 1991 to 24 percent in mid-2013.

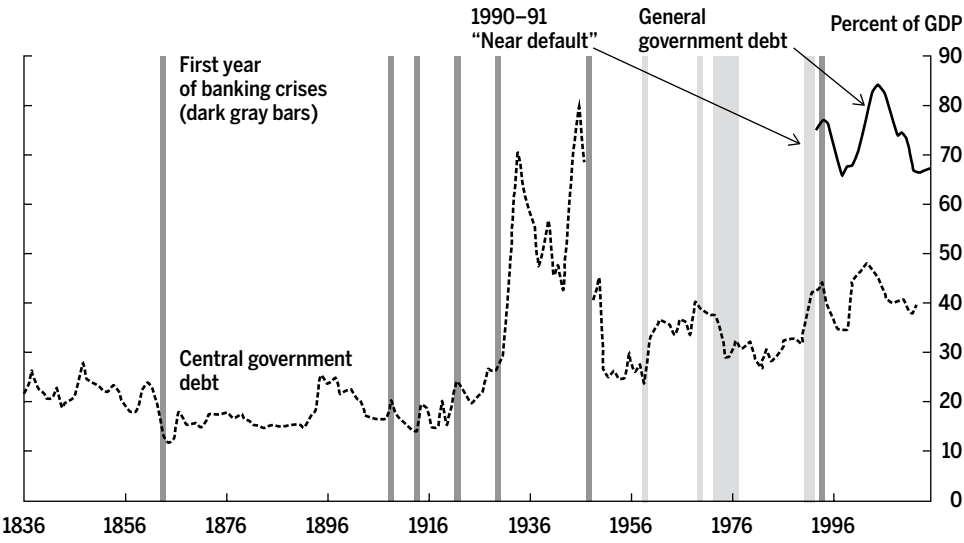
4.1.2.India

If Japan's predicament is rare, a more common pre-crisis pattern of vulnerability is recently visible in India. The troublesome combination involves relatively high public debt (especially evident in general government, Figure 7); large budget deficits (about 8.5 percent of GDP); a widening current account deficit (despite what are still low levels of external debt); slowing growth; and private domestic credit at a historic high. Added to this list are accumulated past mistakes or missed opportunities. Frankel, Végh, and Vuletin (2013) recently reexamined the issue of fiscal procyclicality. Their analysis, which controls for the endogeneity of institutions and other determinants of procyclicality, shows that, over the last decade, about a third of the developing world has been able to escape the procyclicality trap and actually become countercyclical. Unfortunately, this uplifting finding does not apply to India's fiscal policy, which they find to be procyclical as in the past. With growth rates consistently above 5 percent in the past six years, fiscal deficits have oscillated between 8 and 10 percent of GDP; one can only imagine what fiscal finances will look like with slower growth. If the past is any guide, India will rely on financial repression and a negative real interest rate as one of the tools for debt erosion. Reinhart and

FIGURE 7

India: Central and General Government (Domestic Plus External) Debt Restructuring, Near-Default, and Banking Crises, 1835–2013

(as a Percent of GDP)



Sources: League of Nations, Statistical Abstract (various issues): International Monetary Fund (2013c); Ministry of Finance; Reinhart and Rogoff (2009); Reinhart (2010); United Nations (1948) and *Yearbook*, various issues; and World Bank (2013). Additional sources for debt, exports, and GDP: 1835–1839, Brahmananda (2001); 1840–1920, *Statistical Abstract Relating to British India*.

Note: Dark gray vertical bars show first year of banking crises. Light gray vertical bars show debt restructuring.

Sbrancia (2011) estimated the financial repression tax as amounting to 2 percent of GDP a year.[26]

India along with China showed no evidence of an investment slump after 1997 as investment has risen by about 9 percentage points since that time, while household consumption has fallen by about 6 percentage points. India is an outlier in this group in that investment has been partially financed by foreign saving (India is the only country in the Asian group that has recorded consistent current account deficits since 2004). In 2012, the country's current account deficit widened to 5 percent of GDP (Table 1), its peak level since 1980. If external finance falters, India's strong investment could quickly reverse.

4.2. Central Bank Crowding Out

We discuss some understudied aspects of what is a well-documented and researched phenomenon—the buildup of foreign exchange reserves in much of Asia. This process began immediately after the Asian crisis of 1997–98 but

became especially marked since 2000–01, when China accelerated its purchases of foreign exchange to an unprecedented scale. The premise explored here is an extremely simple one and connected to the balance of payments identity and the definition of the current account.

We have presented evidence that most governments in the region have, to varying degrees, shifted from external financing to domestic financing and that the private sector, especially households, have entered domestic credit markets forcefully. Both public and private sectors were relying on foreign saving prior to the crisis. Taken together, these observations suggest that there are more players competing for domestic saving in post-1997 crisis Asia. These trends by themselves would, perhaps, have more modest implications for domestic investment, if it were not coupled with a macroeconomic policy objective of building an insurance war chest of foreign exchange reserves and avoiding current account deficits (or fickle foreign saving) if at all possible. Possibly, *fear of floating* combined with *fear of current account deficits*—a very understandable prudential reaction to severe crisis.

A reserve buildup is an official capital outflow, funneling domestic saving abroad. The decision to intervene or not and at what pace to accumulate reserves is determined by an official institution (the central bank) and is distinct from the process of the private sector's allocation of saving. Accordingly, we broaden the definition of crowding out to include the central bank under the umbrella of the public sector. This broader definition might not have made much difference over large tracts of history. Ironically, under fixed exchange rates, annual changes in reserves relative to GDP were smaller (except in the immediate vicinity of a crisis) than those observed under floating exchange rates since 1997.[27,28] Furthermore, under fixed exchange rates reserve changes were more symmetric, with years of reserve losses alternating with reserve accumulation, than post-1997. If reserve purchases are sterilized to some degree, as is most often the case, it is done by increasing reserve requirements or by open market sales of government or central bank bills (or bonds).[29] In the narrower conventional definition of crowding out, the government is issuing more debt; in the more encompassing definition, the government need not be issuing more debt—the central bank is, either by selling its holdings of government debt or by selling its own sterilization bonds, and a key point is that the central bank is doing so persistently over an extended period of time.

In the debt crisis of the 1980s in Latin America, private capital flight funneled domestic saving abroad, to the detriment of investment in the region. We do not suggest that from a *macroprudential* and *signaling* standpoint the process of a central bank accumulating reserves and capital flight are comparable.

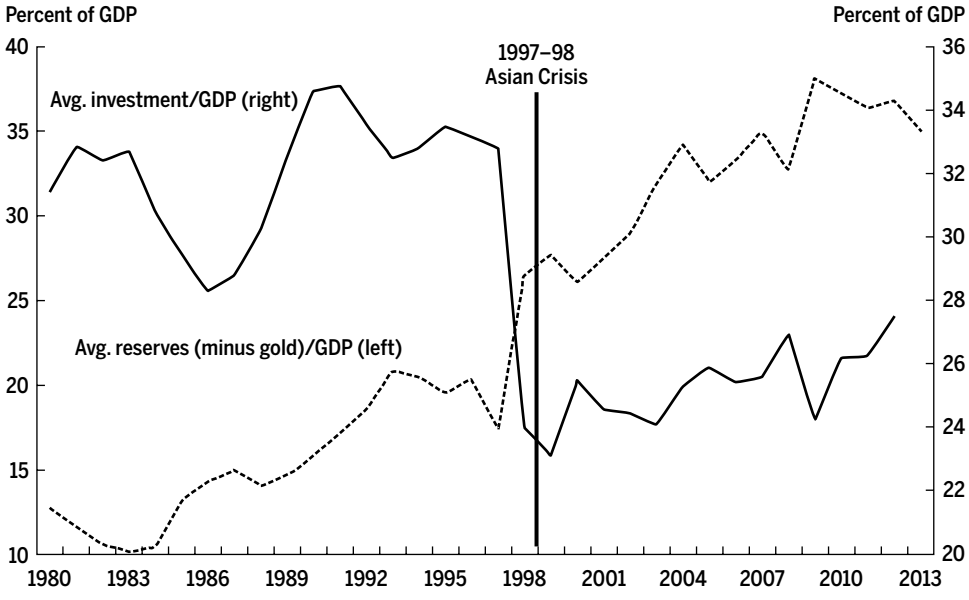
Foreign exchange purchases create a backing for foreign currency debt in times of stress and, more generally, for the monetary aggregates (M2), as in Calvo and Mendoza (1996) and Obstfeld, Shambaugh, and Taylor (2010). Large current account deficits (capital flow bonanzas, Reinhart and Reinhart 2009) are precursors of crises, so avoiding them has a distinct financial stability objective. But the fact remains that whether the outflows are official or private, a slice of domestic saving is directed to the purchase of foreign assets in lieu of domestic investment. In the case of capital flight, this wealth held outside the country is difficult or impossible to tax; in the case of reserves, given the low yield of the assets purchased and domestic foreign interest rate differentials, quasi-fiscal losses have often been significant.[30]

4.3. Not All That Glitters Is Gold

Table 5 presents some summary statistics quantifying the reserve buildup in the nine Asian economies in the sample plus Hong Kong, which we report both separately and combined with Mainland China. As scale variables, we use two domestic and two external variables. Both the stock of and annual change in reserves are expressed as a share of GDP to facilitate magnitude comparisons with investment, saving consumption, and the current account. Since a major impetus to reserve accumulation is to provide insurance, particularly (but not exclusively) in the event that foreign currency debts (public and private) have to be immediately repaid, we also report reserves relative to these external debts. To gauge the magnitude of the reserve buildup (stocks and flows) from the perspective of the United States, which is a major recipient of official flows, we present the data relative to U.S. GDP and the level of marketable U.S. Treasury debt. The evolution of reserves over the period 1980–2013 is also traced in Figures 8 and 9.

Starting with the averages for the region, which conceal cross-country variation (in magnitudes, not direction), reserves on average rose around 4 percent of GDP; Singapore and China are on the upper end and Indonesia on the lower end. On average, as of August 2013 the ratio of reserves to GDP was 36 percent, which is very close to the total amount of public and private external debt outstanding. At 93.1 percent, reserves-to-external debt is indeed a contrast to the reserve-to-debt ratios reported in the last column of Table 3, where reserve ratios in times of crises were uniformly less than 25 percent and frequently less than 10 percent. As shown in Table 5, the magnitude of official outflows (reserve accumulation), relative to the size of the economy are on a scale that can potentially help account for an important component of the decline in investment simply from the narrow vantage point of balance of payments accounting.

FIGURE 8
Investment and Reserves, Eight Asian Economies, 1980–2012
(as a Percent of GDP)



Sources: Bloomberg, International Monetary Fund (2013b,c).

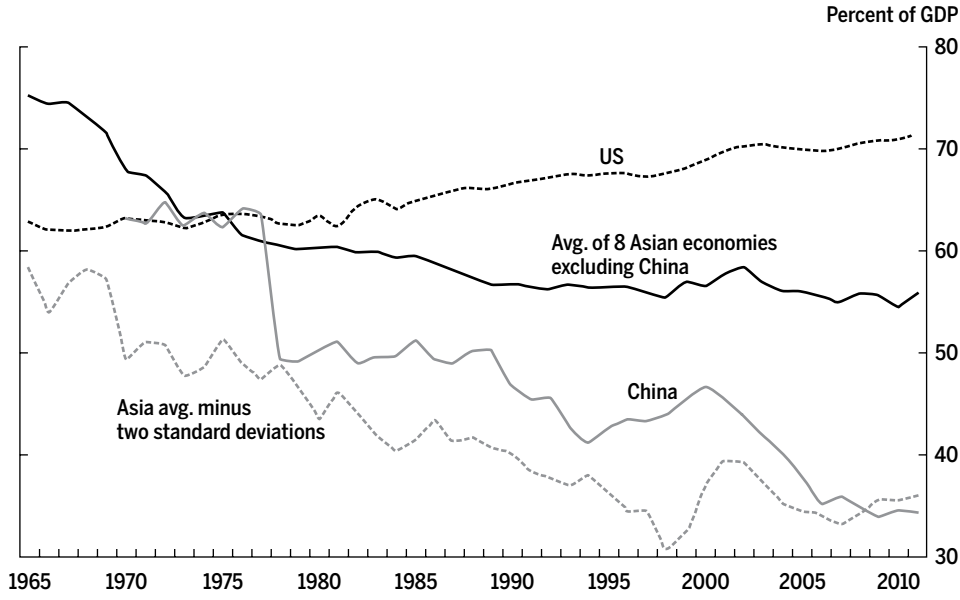
Notes: Eight economies include India, Indonesia, Japan, Korea, Malaysia, Philippines, Singapore, and Thailand. Correlation of investment and reserve ratio is –0.76. The standard error on the correlation coefficient of the investment and reserve ratio reported in Figure 8 is 0.073; if China and India are included the correlation is 0.66 and the corresponding standard error is 0.098. Both sets of correlations are significant at the standard confidence levels.

The magnitude of the reserve buildup in these nine countries combined from the U.S. vantage point is no less substantial, although it may somewhat overstated by Table 5, which presents total reserves minus gold, not just Treasury securities or government-sponsored enterprises debt. That line of inquiry, however, is beyond the scope of this paper.

Table 5 covered the period of most intense reserve accumulation post-2001 but Figure 8 traces the evolution of reserves since 1980, thus covering before and after the 1997–98 crisis. It is important to remember that while 1997–98 was the most severe crisis (by a number of metrics including investment) and certainly the most synchronous, it was not the only crisis in Asia since 1980. Indeed from 1980 to 1985, in addition to the Korean debt crisis (no default or restructuring ensued) of 1979–80 and the default by the Philippines in 1983 (Table 3), there were systemic banking crises in Korea in 1983, Malaysia in 1985, Philippines in 1981, Singapore in 1982, and Thailand in 1983.[31] These crises did not produce as sharp a change in public policy attitudes toward the

FIGURE 9

Household Consumption as a Share of GDP: Eight Asian Economies, China, and the United States, 1965–2011



Sources: World Bank, *World Development Indicators*.

external sector as did the later crises, specifically as regards the danger of current account deficits and the desirability of a generous pool of foreign exchange reserves. So, while there is a sharp downturn in investment (which falls 5 percentage points between 1983 and 1986), the post-crisis recovery in reserves is neither as sharp nor sustained as post-1997.[32] The simple correlation between the foreign exchange reserve and investment ratios over 1980–2013 is –0.76. In interpreting the correlation, it is important to remember that these are linked via the balance of payments accounting identity. An increase in official outflows (reserve accumulation), however, could just as plausibly translate to a reduction in the consumption-GDP ratio (public, private, or both) as in China post-2001. While reserve intervention or accumulation is a policy choice, it is probable that it is connected to a policy reaction function of one form or another, a point made by Obstfeld (1982).

The obvious question is, of course, China, the country with the most significant buildup in reserves where average investment ratios are almost 3 percent higher after 1997. If China is included so that it is a nine-country average the correlation drops to –0.66. The space for simultaneous reserve accumulation

TABLE 5
International Reserves minus Gold, Stocks, and Flows for Selected Asian Countries, 2001–August 2013

	Average annual change in reserves over 2001–13 relative to:		Reserves as of end of August 2013 relative to:*			
	Domestic GDP	U.S. GDP	Domestic GDP	External debt	U.S. GDP	U.S. marketable debt
China, Mainland	*7.12*	*1.81*	*39.12*	*467.28*	*20.91*	*29.35*
China, Hong Kong	*7.03*	*0.10*	*108.67*	*29.38*	*1.82*	*2.55*
China, Combined	7.01	1.92	41.23	210.12	22.72	31.90
India	2.04	0.13	14.66	66.08	1.54	2.16
Indonesia	0.99	0.03	10.72	36.73	0.56	0.78
Japan	1.46	0.53	24.78	43.90	7.42	9.95
Korea	2.29	0.14	27.73	80.95	1.99	2.79
Malaysia	4.95	0.06	43.20	134.17	0.81	1.13
Philippines	2.65	0.03	27.30	125.84	0.44	0.62
Singapore	8.12	0.10	91.05	22.11	1.57	2.20
Thailand	4.34	0.07	41.34	118.04	0.99	1.39
Sum		3.01			38.04	52.93
Average	3.76		35.78	93.10		

*China (Mainland) does not participate in the joint International Monetary Fund and World Bank Quarterly External Debt Statistics (QEDS) exercise nor the Special Data Dissemination Standard (SDDS). As such, the most recent external debt data are year-end 2011. Hong Kong data are available through 2013:Q1 in the QEDS database; the aggregates reported for the Mainland and China, Combined end in 2011.

Sources: Bloomberg, Board of Governors of the Federal Reserve System (2013), International Monetary Fund (2013b,c), and World Bank QEDS.

and higher investment in the case of China was largely accommodated by a sharp decline in household consumption (Figure 9 and Appendix Figure A1) that leaves the consumption share of households in 2011 at around 35 percent of GDP, roughly one-half the U.S. share and more than two standard deviations lower than the Asia average. Lardy (2008, 2012) has connected the decline in household consumption (and household income) share of GDP in China to the large reserve buildup post-2000 in combination with financial repression. Jeanne (2012) formalizes this connection. Massive purchases of foreign exchange reserves (ranging from 9 to 13.5 percent per year from 2004 to 2009) coupled with partial sterilization helped fuel a credit boom and accelerating inflation. With interest rates on deposits capped, real ex-post interest rates fell from around 1.5 percent to about –7 percent during this period. The financial repression tax on households depressed incomes and consumption, as shown in Figure 9.

5. Concluding Remarks

Much has been said about the macroprudential rationale for holding sufficient foreign currency reserves to cover short-term liabilities. There is less

agreement on how broad that coverage should be, especially if there is the potential for hidden debts and implicit guarantees on private sector liabilities. As a consequence, the concept of an optimal level of reserves is subject to debate. As reserve accumulation picked up momentum around 2000–01, a number of studies have also emphasized that there is no free lunch. Reserve accumulation carries costs. Calvo's (1991) perils of sterilization, written well before the Asian crisis, focused on the higher nominal interest rates that result from the central bank's effort to sterilize reserve accumulation.[33] Other perils, including creating distortions in the banking sector, fueling credit booms, and impairing central bank balance sheets, have been considered.

The point emphasized in Bussière et al. (2013) that reserve accumulation and capital controls may be best viewed as complements rather than substitutes has resonance to the analysis presented here. They found the greatest resilience to the global shock of 2008–09 was among countries with high reserves and less-than-open capital accounts. In the longer-horizon focus of this paper, the parallel would be that countries with relatively more pervasive capital account barriers may have a better chance of limiting the crowding out effects of reserve accumulation (official outflows) on investment. This may be because the controls themselves limit private outflows or capital flight—a leakage (to the extent, of course the measures are effective) because the magnitude of the desired reserve accumulation is smaller (as the controls also insulate the domestic economy from external shocks), or a combination of the two.

The global consequences of this reserve buildup have been debated under various headings, including Bernanke's saving glut and the risks it poses to capital-importing countries like the United States. Bernanke (2005) argued that interest rates in advanced economies were held down by a glut of saving from Asian economies. To this we would add that Asia's investment has been held down because Asian governments have been absorbing domestic saving to purchase the securities of the rest of advanced economies.

Then there is the eternal quest in the international finance literature for how to measure capital mobility.[34] For one, the reserve buildup drives a large and variable wedge between domestic saving and investment. Thus a test of the mobility of capital in the spirit of Feldstein and Horioka might conclude capital flows freely because national investment is not constrained by saving. Actually, domestic investment may be crowded out from using domestic saving because of the government's decision to build reserves. Interest parity conditions are silent on the volume of official-to-official versus private international capital flows, a phenomenon convincingly documented in Alfaro, Kalemli-Ozcan, and Volosovych (2013) and our discussion of rising post-crisis home bias.

This paper is silent on the global implications of official capital outflows from Asia, but it adds another dimension to the policy dilemma of capital exporting countries—when are high and rising levels of reserves too much of a good thing? Since the 1997–98 crisis, investment ratios in Asia have not recovered outside of China and India, and now those two countries may be on the cusp of a correction. We have hypothesized here that the persistent and quantitatively important official outflows orchestrated by central banks have crowded out investment—and not necessarily just private investment but public as well. Since 1997, growth has slowed significantly in the region, even when China and India are included in the calculus.

We are not aware of other studies addressing this particular trade-off between the size of the security blanket and the price in terms of the medium-term growth consequences it may carry. This is to say that this is a fruitful area for policy research. It is relevant for Asia, not just because China and India are now part of the fragile five, but also because several of the other countries have their own challenges, ranging from Japan's gargantuan public debt to signs of internal household credit booms in some of the other former crisis countries. Perhaps Asia's investment slump has been largely overlooked because, after all, Asia's investment ratios are still among the highest worldwide—but their post-crisis average is more than 9 percentage points below average in the decade before the crisis. If there were another round of turbulence in Asia with its usual attendant impact on investment, that gap could narrow further.

Of course, the problem of dwindling investment is also a compelling policy challenge for many of the advanced economies facing large public and private debt overhangs. Indeed, it is an acute problem in the cases of periphery Europe, where capital market access remains limited at best and an ongoing credit crunch unfolds. In much of Europe, finance has turned inward, and banks, pensions, and insurance are largely in the business of buying domestic government bonds and evergreening significant levels of moribund private debt. Unlike Asia, however, the leakage draining domestic saving is not coming from central bank purchases of foreign assets as a rainy day fund is built. As Eichengreen et al. (2013) convincingly illustrate, much of Europe's post-crisis experience to date aligns more closely with Latin America's lost decade—to their analysis we would add that, like Latin America in the 1980s, capital flight from the periphery remains a drain on its domestic saving.

Appendix

TABLE A1

GDP Growth for Selected Countries, 1980–2012

Country	Change over:		Peak: 1980–2012		Level
	1997–2007	2007–2012	Level	Year	2012
Europe and United States[a]					
Austria	0.6	–1.4	4.35	1990	0.87
Belgium	1.4	–4.0	4.72	1988	–0.28
Denmark	2.9	–3.6	5.53	1994	–0.38
Finland	2.7	–7.0	6.20	1997	–0.83
France	–0.2	–2.2	4.67	1988	0.01
Germany	0.3	–0.9	5.72	1990	0.90
Greece	5.9	–10.0	5.94	2003	–6.39
Iceland	–3.6	–3.3	8.55	1987	1.64
Ireland	7.6	–11.1	11.27	1997	0.16
Italy	–1.3	–4.2	4.19	1988	–2.37
Netherlands	2.4	–5.5	4.68	1999	–1.25
Norway	3.6	–2.4	5.90	1984	3.02
Portugal	–3.2	–7.6	7.86	1990	–3.24
Spain	–1.8	–5.5	5.71	1987	–1.64
Sweden	–0.7	–1.8	6.56	2010	0.95
Switzerland	0.5	–1.0	5.11	1980	1.05
United Kingdom	–0.8	–4.2	5.57	1988	0.17
United States	1.0	–1.7	7.26	1984	2.78
Average	**1.0**	**–4.3**			**–0.27**
Memorandum items:	*1997–2007*	*2008–2012*	*Difference*		
Average level	3.10	–0.38	–3.5		
No. observations	198	90			

Country	Change over:		Peak: 1980–2012		Level
	1987–1997	1997–2012	Level	Year	2012
Asia					
China	–2.3	–1.6	15.20	1984	7.70
India	0.1	–0.8	10.55	2010	3.24
Indonesia	–0.2	1.5	9.88	1980	6.23
Japan[b]	–2.5	0.4	7.15	1988	1.96
Korea	–6.5	–3.7	12.27	1987	2.04
Malaysia	1.9	–1.7	10.00	1996	5.64
Philippines	0.9	1.6	7.63	2010	6.82
Singapore	–2.3	–7.2	14.78	2010	1.32
Thailand	–10.9	7.9	13.29	1988	6.49
Average	**–2.4**	**–0.4**			**4.60**
Memorandum items:	*1987–1997*	*1998–2012*	*Difference*		
Average level	7.17	4.69	–2.5		
No. observations	99	135			

Sources: International Monetary Fund (2013c) and Reinhart and Rogoff (2009).

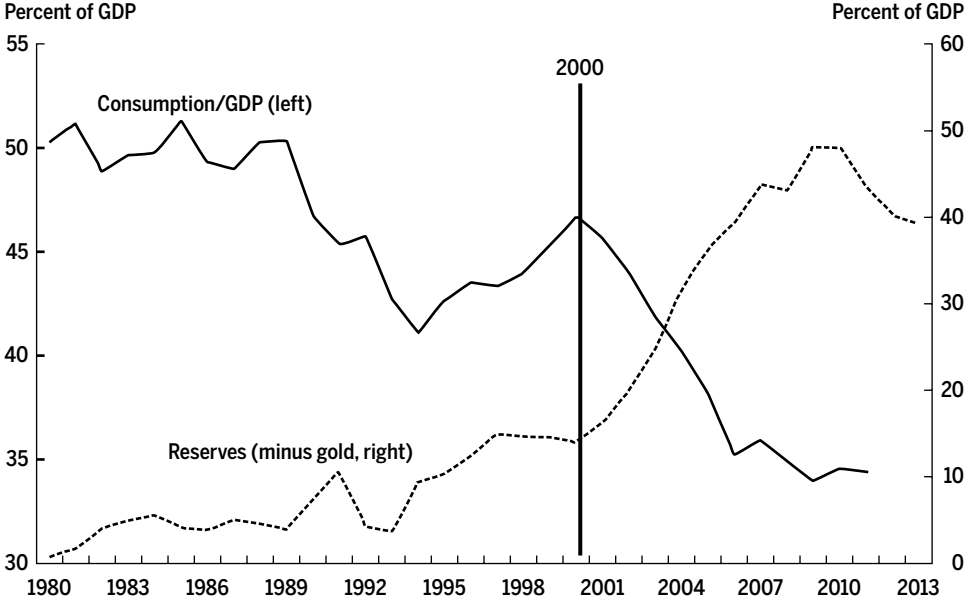
a An asterisk denotes a banking crisis in the "common crisis year"; the common crisis years for the advanced and Asian economies are 2007–08 and 1997–98, respectively. The years refer to the start of the crisis.

b Japan's financial crisis began in 1992.

Notes: The difference in pooled means tests are significant at standard confidence levels.

FIGURE A1

Household Consumption and International Reserves: China, 1980–2012



Sources: Bloomberg, International Monetary Fund (2013b,c).

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NOTES

1 See Bussière et al. (2013), Frankel and Saravelos (2012), and Gourinchas and Obstfeld (2012) for new contributions to the analysis of early warnings as well as the comprehensive discussions of the existing literature therein.

2 See also Calvo (2012) on the dynamics, incidence, and time profile of sudden stops.

3 Recall the current account balance equals saving minus investment.

4 The countries are China, India, Indonesia, Japan, Korea, Malaysia, Philippines, Singapore, and Thailand.

5 This growth comparison showing lower growth during the era of high reserves is not at odds with the finding in Bussière et al. (2013) that emerging markets with higher reserves performed better (in terms of output) during the global financial crisis of 2008–09. The period we are comparing spans 26 years (11 years through 1997, and 1998 to 2012); our focus is on the long-term growth performance.

6 In the Kaminsky and Pereira (1996) sample, increases in public and private consumption (as a share of GDP) were of comparable magnitudes.

7 See Claessens (1997) for a comprehensive analysis of the capital flight magnitudes involved.

8 See Reinhart and Sbrancia (2011).

9 See Aizenman and Lee (2007) and Calvo, Izquierdo, and Loo-Kung (2012).

10 Calvo and Reinhart (2002); Aizenman and Lee (2007) find the "mercantilist" motive statistically significant but quantitatively small relative to self-insurance, but their sample does not extend to recent post-subprime years. Jeanne (2012) also emphasizes the fear of floating or, in this particular instance, "fear of appreciation" motive (see Levy-Yeyati and Sturzenegger 2007 for compelling documentation of this tendency in emerging markets for the pre-crisis period).

11 On the use of reserve requirements to sterilize capital inflows see Reinhart and Reinhart (1999) and Cordella, Végh, and Vuletin (2013).

12 This is not to suggest that some declining investment ratios have other important drivers relating to technological change and the transition to a more mature stage of development. We would observe that, by 1991, Japan was considered a mature economy and that Singapore did not lag far behind.

13 If we use a narrower window around the crisis of three or five years, this pattern is even more pronounced and uniform.

14 The individual country series are plotted in Reinhart (2013).

15 For historic dimensions of these credit cycles, see Schularick and Taylor (2012)—Japan is in their sample; for the interaction between capital flows, credit, and crisis (including the nine Asian economies covered here, see Mendoza and Terrones (2012).

16 Denmark is excluded as there is a substantive break in the credit series reported in *International Financial Statistics* in 2000, which needs to be sorted out.

17 In Figure 5, Hong Kong is not combined with Mainland China, as for external debt in Figure 2.

18 For example, the issue of including central bank debt in public sector debt statements is under discussion in Korea (*Korea Herald* 2013).

19 Badkar (2013).

20 Reinhart and Sbrancia (2011).

21 Reinhart (2013) presents a pictorial history through the time series for all the categories of debt for which data are available.

22 See International Monetary Fund (2013a).

23 While the United Kingdom public debt data are available from 1692, the nominal GDP data prior to 1830 is of a more tentative nature (see Hills and Thomas 2010); as such the debt ratios immediately following the Napoleonic Wars are to be interpreted with care.

24 The *Statistical Abstract Relating to British India*, various volumes spanning 1840 to 1920, quantify the transfer of treasure to the United Kingdom, while Bos (2007) chronicles the use of revenue from Indonesia directed to reduce Dutch public debt.

25 See Reinhart and Rogoff (2009) and Reinhart and Sbrancia (2011).

26 As is discussed in that paper, the estimate is a conservative lower bound.

27 Commodity price booms and busts were also associated with larger-than-normal fluctuations in reserves.

28 Standard textbook definitions of floating exchange rates do not involve foreign exchange market intervention, and a common simplifying assumption is that reserve changes are zero.

29 See Reinhart and Reinhart (1999) and Cordella, Végh, and Vuletin (2013).

30 See for instance, Mohanty and Turner (2006) and Rodrik (2006).

31 See Kaminsky and Reinhart (1999) and Park (2005) for a comparison of the two Korean crises and Athukorala (2010), who reviews the mid-1980s and 1997–98 crises in Malaysia and the turbulence of 2008.

32 The decline in investment by 5 percentage points in a period of three years is not far off from the 5.4 percent decline for the United States and Europe in the five years after the crisis (Table 2).

33 This has not been the case in the most recent period of heavy financial repression in China, as discussed in Lardy (2008).

34 Obstfeld (1995) provides a comprehensive tour of this literature and Obstfeld, Shambaugh, and Taylor (2010) also deal with some of these issues.

COMMENTARY

Crowding Out Redefined: The Role of Reserve Accumulation

Alan M. Taylor

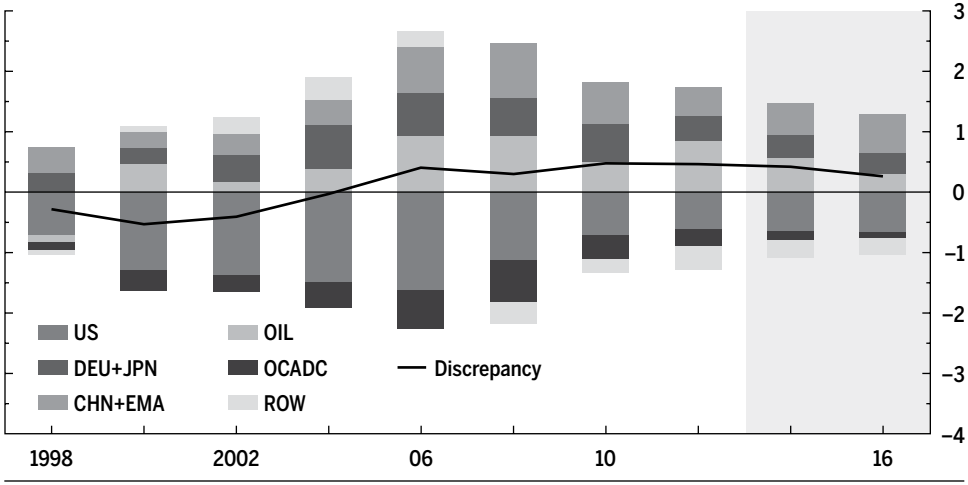
Thanks to Carmen Reinhart for the fine paper. I always learn a great deal from reading her work: It is always original and provocative and suggestive, and there is much here to dig into. I am going to be doing the discussant's job of trying to make some constructive criticisms and suggestions for how to go forward. So let me focus on a few issues.

First I will give a contextual preamble using recent and historical data to get some perspective. I will go from there to summarize what I think are the main points of the paper. Next, I am going to have five comments, one big and four small. And then I will end with five parting questions.

Starting with the contextual preamble, I want to focus on the big fact about the world we live in, and the world we have been living in for the past decade or two. This is the asymmetry between emerging markets and developed markets, which I think is the key fact in international macroeconomics that we have to contend with, that we have to teach our students about, and that policymakers have to worry about as well. So let me look at some aspects of that.

First I turn to global imbalances, their emergence, and the putative rebalancing that we are now going through. Figure 1 from the International Monetary Fund (IMF) *World Economic Outlook* shows the surpluses and deficits of major countries and regions, many of them familiar. On the positive side, the oil-exporting countries have been earning surpluses for three or four decades; China with its precautionary savings or mercantilism, whichever you prefer to call it, of many years; and Germany, which is a new and upcoming mercantilist if you believe recent chatter. On the negative side of these bars you see the deficit countries, the most prominent of course being the United States. This sets up an important context for where we have gotten to, although it may be that imbalances will recede. It is a measure of flows, but it has some stock implications. And if we look a little deeper, moving from net positions to gross positions and how they have been evolving over time, we get to asymmetry fact number two.

FIGURE 1
Global Imbalances
(Percent of GDP)



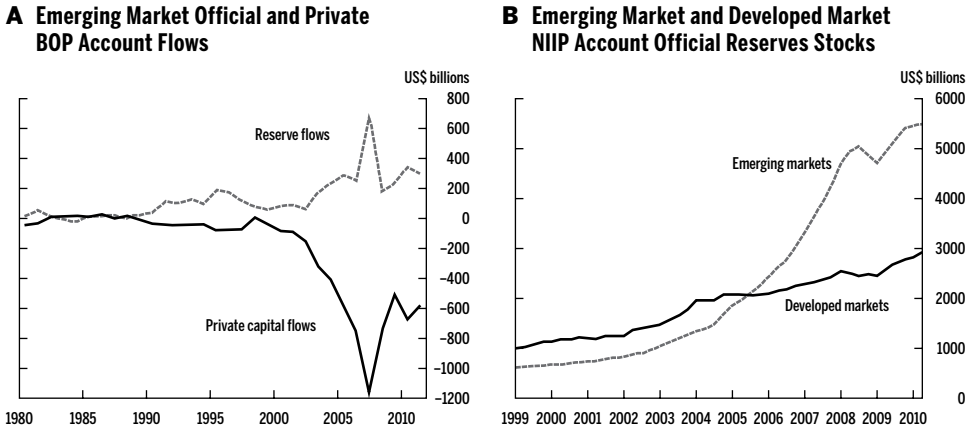
Sources: CPB World Trade Monitor; Haver Analytics; and IMF staff estimates.

Note: CHN+EMA = China, Hong Kong SAR, Indonesia, Korea, Malaysia, Philippines, Singapore, Taiwan Province of China, Thailand; DEU+JPN = Germany and Japan; IP = industrial production; OCADC = Bulgaria, Croatia, Czech Republic, Estonia, Greece, Hungary, Ireland, Latvia, Lithuania, Poland, Portugal, Romania, Slovak Republic, Slovenia, Spain, Turkey, United Kingdom; OIL = oil exporters; ROW = rest of the world; US = United States.

Figure 2 shows two things: in panel A on the left, official and private balance of payments items for private and official flows to emerging economies, and in panel B on the right, the stock of reserve assets. On the left-hand side is flows and on the right-hand side is stocks. The left-hand side says, in a nutshell, that whatever you learned about the Lucas paradox of uphill capital flows was mostly wrong. It is correct for the aggregate flow, but really the Lucas paradox is about private investor decisionmaking. But when you look at private capital flows, they have in fact been going downhill from rich countries to poor countries for the past 20 or 30 years. So there hasn't been capital flowing in the wrong direction in that neoclassical sense. It is just that if you look at the official flows in the figure, they are large and offsetting—and large enough to *more than offset* in the net balance of payments the downhill flow of private capital into the emerging economies. And what form has emerging country capital accumulation principally taken? It has taken the form of the reserve accumulation that Carmen talked about and we are all familiar with. This is shown in panel B chart in the accumulation of vast stocks of reserves in the emerging markets, going from about $1 trillion to $6 trillion over that period, and growing much faster than their GDP or even their trade flows. And of course much,

FIGURE 2

Private and Official Assets: Flows and Stocks

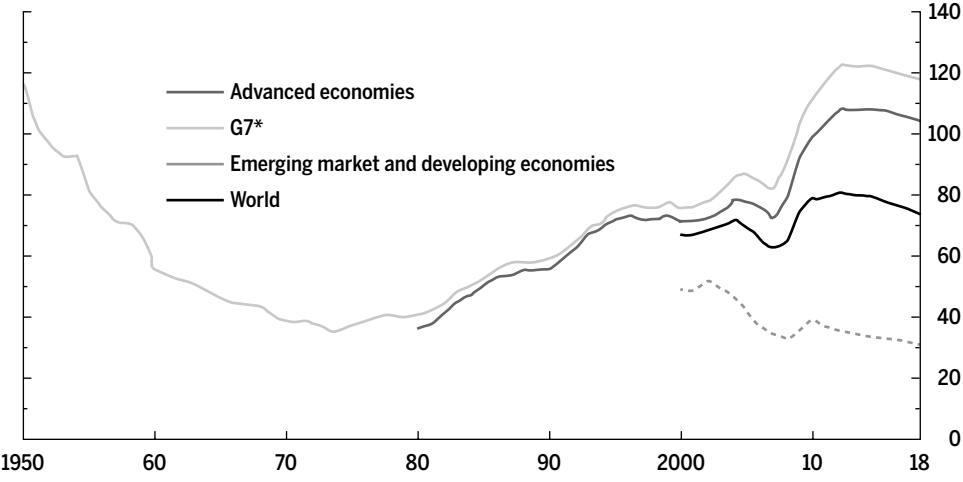


Source: Taylor, Alan. 2012. "The Great Leveraging." BIS Working Paper 398. http://www.bis.org/publ/work398.pdf

much faster than in the advanced economies, where reserve accumulation has been fairly flat relative to economic growth.

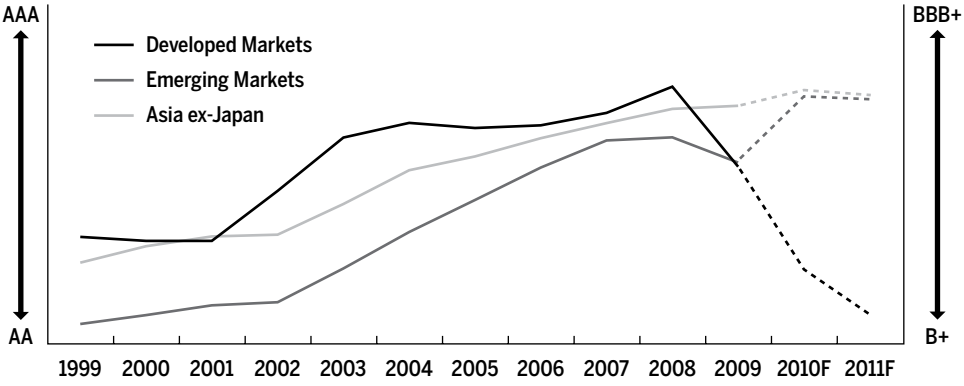
Fact number four concerns public debt, and we can refer to Figure 3. Remember how we used to lecture the emerging markets about their irresponsible fiscal policies and how they were overindebted? But that was then, and the shifts are clear when we look at a long time series for the advanced economies, the Group of Seven, and the emerging economies. It is striking how things have changed since the 1990s, with the emerging markets and developing economies getting their fiscal act more in order and lowering their public debt-to-GDP ratios. At the same time, the advanced economies have gone in the opposite direction. So not only have emerging economies been piling up assets for precautionary or other reasons, they have also been reducing their liabilities. They have been piling up official assets and reducing public liabilities. What's been the payoff? Well, if you believe investment ratings then there has been some payoff in the recent crisis. Traditionally, going back to the 19th century, as economic historians well know, whenever the advanced countries sneeze the emerging countries catch pneumonia. But this recent crisis episode has proven to be an exception. Emerging economies have—remarkably—escaped virtually unscathed, without any crises occurring on their own territory even as the advanced economies have gone through so much turmoil. That achievement is reflected in these observed credit ratings as well, shown in Figure 4. The crisis in the advanced world has brought a great deal of fiscal stress and strain and a lot of economies have been downgraded. Obviously the scales are different.

FIGURE 3
Public Debt
(Percent of GDP)



Source: IMF staff estimates.
*G7 comprises Canada, France, Germany, Italy, Japan, United Kingdom, and United States.

FIGURE 4
Sovereign Credit Ratings



Source: Viktor Hjort, *Reverse Contagion: Sovereign Crisis Implications for Asian Credit*, 12, February; Morgan Stanley Research.

On the left axis you've got the advanced economies in AA through AAA range. On the right axis, you've got the emerging countries in the B to BBB range. We don't see much of a blip in emerging sovereign credit ratings, but we see a strong downward trend in the credit ratings of the advanced sovereigns.

Fifth, let me make a final contextual point to conclude my survey of the asymmetry between emerging and advanced economies by looking at private credit. What has been happening there? We all know there has been a credit crunch. Or at least there has been in the advanced economies. But there has not been one in the emerging economies. In fact, emerging economies have been beset by worries in the past year or two regarding whether they are having too much of a credit boom. Again, this is a striking difference between the two groups of countries. The United States seems to be emerging from that credit crunch with growth rates of credit gradually creeping upwards in Figure 5, panel A. Of course the euro zone, beset with the double whammy of a financial crisis and then a lot of sovereign stress, is still seeing zero to almost negative growth rates of credit. But in the emerging economies the growth rate of credit is robust and is supporting high, but maybe (according to arguments in Carmen's paper) *not high enough* rates of investment.

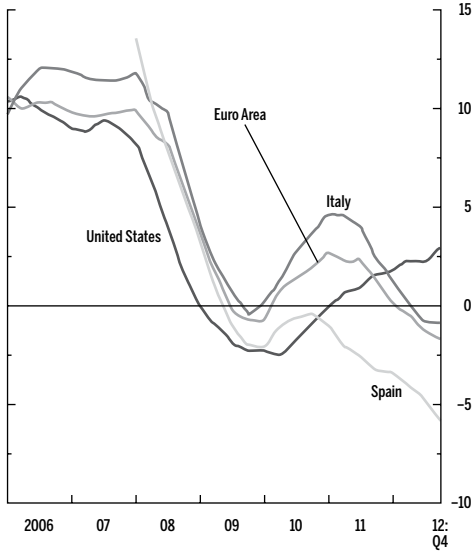
So that is a tour of the landscape. Let me now come back to what the paper is saying and what it is trying to do. I see the paper as drilling a little bit deeper into this emerging market asymmetry. It looks at investment after financial crises for the emerging market countries in the 1990s and tries to make some parallels with the euro zone today. And it asks whether the massive reserve accumulation I just described has constituted a form of crowding out, and whether the government balance sheet, even if it is safer, represents a diversion of resources that may incur a growth or investment penalty.

Now some aspects of these claims are completely understandable and uncontroversial. We know that investment-to-GDP is highly procyclical, so when there are big crises or big recessions we expect investment to go down hard and stay down for some time. So I think the question here is not really about the short run, at the business cycle frequency. It is really medium to longer term: Can we discern anything reliably at that horizon? Can we tease out evidence from these few recent macroeconomic events?

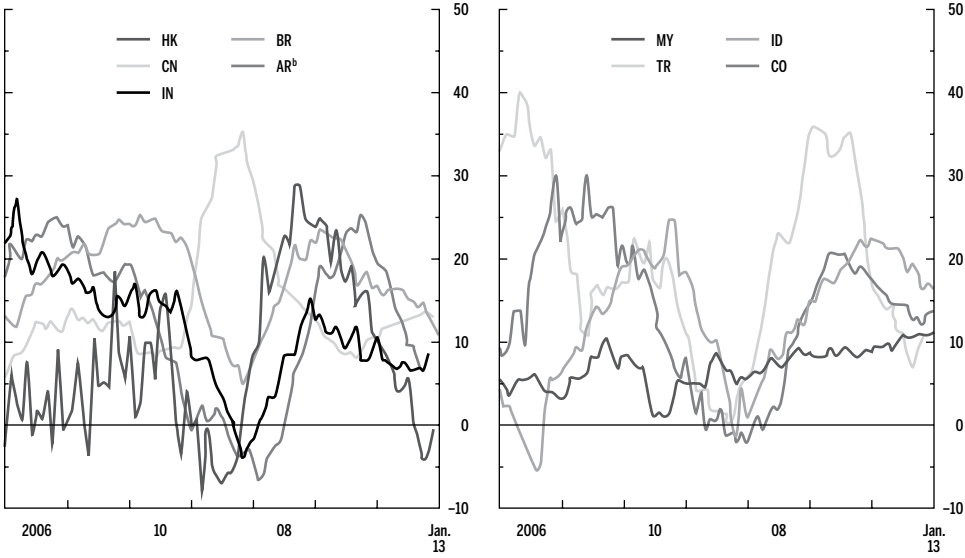
Let's begin not with theory or evidence, but something beyond dispute: accounting identities. The basic story is running through the balance of payments identity, current account equals saving minus investment. And we know from the data that crises tend to be associated with the current account moving sharply in the positive direction, out of deficit towards surplus. As an accounting matter, that means saving must be rising or investment must be falling, or

FIGURE 5
Private Credit

A Nonfinancial Firm and Household Credit Growth[a] (year-over-year percent change)



B Real Credit Growth (year-over-year percent change)



Sources: Haver Analytics; IIF Emerging Markets Bank Lending Survey; IMF, *International Financial Statistics*; and IMF staff calculations.

Note: AR = Argentina; BR = Brazil; CN = China; CO = Colombia; HK = Hong Kong SAR; ID = Indonesia; IN = India; MY = Malaysia; TR = Turkey.

a Flow of funds data are used for the euro area, Spain, and the United States. Italian bank loans to Italian residents are corrected for securitizations.

b Nominal credit is deflated using the IMF staff's estimate of average provincial inflation.

some combination of the two. Looked at this way, the overarching questions in the paper boil down to tracing balance of payments shifts: How large? For how long? Which components? What's the causal story? And is reserve accumulation an important part of that story?

I always look through a paper for the crucial quotes and I think there are two crucial quotes in this paper. The first is, "The crisis experience sets the stage for both a policy that redirects government borrowing toward the domestic market and a central bank that strives to build a foreign exchange war chest as a financial stability policy tool."

The paper is asking us to think about the possible downsides to this response in the emerging economies, and possibly now in the euro zone today. Maybe one can view this in some sense as a rational macroprudential or "financial repression" policy, but in addition to putative benefits it may also incur some costs. This ties into the much broader debate about finance and growth. If you have a freewheeling financial sector, does that give you more rapid growth in the long term? But does it also come at the cost of higher volatility, higher frequency of crises, and deeper crises?

The second crucial quote is this: "The fact remains that whether the outflows are official or private, a slice of domestic saving is directed to the purchase of foreign assets in lieu of domestic investment."

I ask myself, is this a priori true? And I think the answer is clearly no. And it gets to the question of what is the counterfactual. Whether it is true is going to depend on the responses of other items in the national income accounts and the balance of payments accounts. What is going to happen in the counterfactual to private savings? What is going to happen to the current account? And so what I think this paper forces us to do is to think about the substitutability of the private and official holdings of foreign assets in the actual and counterfactual worlds, and possible side effects operating through other channels such as risk and volatility, which I'll come to in a moment. But you have to have all of those ingredients in place for this policy change—if it is that—to lead to the big counterfactual being declining investment. And there are some scenarios in which it might conceivably go in exactly the opposite way.

A crucial figure in Reinhart's paper (Figure 8) shows that the reserve accumulation is going up quite steadily but investment does kind of jump down at the 1997 crisis turning point. My question is, can we get a bit more detail here? Maybe we need to dig into the balance of payments items or look at flows versus flows rather than just flows versus stocks to get a sense of which components are doing the work. Is it private savings moving around, or is it current accounts moving, and which elements are in motion? Knowing that could provide a little

more granularity and detail to clarify the story. And perhaps going deeper, doing some statistical analysis country by country, item by item, would help.

I now turn to my main comments, of which the first is the most important.

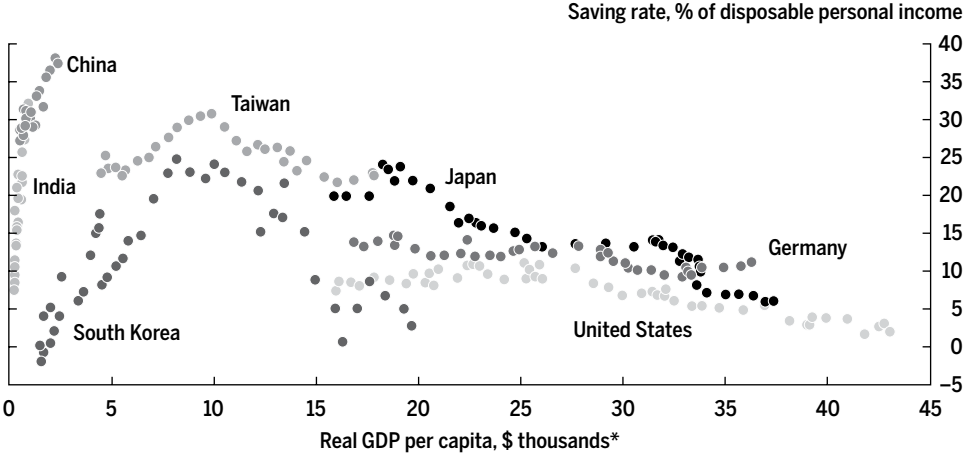
Comment 1: This recapitulates a point made by Lant Pritchett in the first session of this conference, which is that all fast-growing economies are going to hit some kind of a slowdown. Maybe it's predictable, maybe it's not, and I don't want to get into that argument. But even if you take the standard neoclassical model, you're going to have growth slowing down. In the standard model, growth deceleration is monotonic and linear; in reality of course it jumps around. I took what Lant was saying as basically consistent with the observation by Daron Acemoglu et al., that not only do higher quality institutions lead to higher levels of output per capita, but they also lead to less volatile output per capita growth. Thus, in the left-hand tail where you have low quality institutions, things are going to be jumping around more. That is why you get more reversals and accelerations in the bottom end of the institution and income distribution. We would therefore expect countries that have been developing and emerging and traveling up that income escalator to eventually slow down, but it may not be linear, they may hit some inflection points.

Over the very long run, the data do suggest that, beyond a certain point, for maturing economies, growth and investment steadily decline together. Figure 6 from a McKinsey report shows savings rates (which are approximately equal to investment rates in the long run). This goes back into 19th century data using very long time series. What you see is that at very low levels of per capita income, near a subsistence constraint, savings can rise pretty rapidly, but they hit a peak. Economies have hit that peak and growth just chugs on and on, through the 5, 10, 15, 20 thousand dollar range, and saving and investment start to track downward as economies mature and returns diminish. (Germany, of course, is the exception.) The data in the paper are quite consistent with some long-run historical patterns. You see big negative changes in investment to GDP in the pre- and post-crisis samples that are quite evident. If you dig into the appendix, you also see that these are transitions associated with high to low growth. Growth slowed in the countries in which investments slowed. So for the moment we can only say this is correlation and not causation. It's quite a challenge to move forward and say something causal and get some identification that we can test.

But that kind of correlation may not be simply a reflection of a slow, long-run shift; it could be a deep, cyclical, but protracted response to a financial crisis. In my work with Òscar Jordà and Moritz Schularick, we see the same empirical

FIGURE 6

Household Saving Rate and GDP per Capita for Selected Countries, 1960–2008



*At constant 2005 prices and exchange rates.

Source: Bank of Japan; Bank of Korea; Directorate-General Budget Accounting and Statistics, Republic of China; Global Insight; Reserve Bank of India; U.S. Bureau of Economic Analysis; World Development Indicators, World Bank; McKinsey Global Institute analysis. Exhibit from "Farewell to Cheap Capital? The Implications of Long-Term Shifts in Global Investment and Saving," December 2010, McKinsey Global Institute, www.mckinsey.com/mgi. McKinsey & Company. Reprinted by permission.

regularity in 140 years of data for 14 economies. After financial crises there are big slowdowns in growth, but also collapses in investments and sharp moves in the current account toward surplus. It is a pattern we have been seeing for a long time: Major crisis events are followed by big and lasting macroeconomic shifts.

Comment 2: I worry a little about the interpretation of what emerging governments and central banks are up to when accumulating all of these reserves. Carmen spoke to this, and I think that was well put. Before all of the crises we were telling emerging economies that they were taking too many risks: We told them they had foreign currency mismatch, too much borrowing, and that was all incredibly risky and they should do something about that, which they did. That suggests all of their reserve accumulation and prudential stuff was great, but there can be too much of a good thing. Now the concern is that they may have a new kind of distortion, perhaps even financial repression, which could be bad. Carmen has written on both sides of this point. But if we stand back, there must be a middle ground where we can ask what the supposedly optimal position is for these economies. Then we can ask if they have really transgressed beyond that.

Comment 3: This takes us to the observation that this reserve accumulation wasn't unintentional. We can go back and profitably reread the 1990s literature by Martin Feldstein on self-insurance for emerging markets, and more recent papers in the 2000s by Lawrence Summers and Dani Rodrik on the opportunity costs of reserve accumulation. We know there is likely to be a cost of reserve accumulation: You are parking your resources in very low return—now near-zero return—assets; and you are not doing alternative things such as consuming, or investing in real positive-return projects and creating real capital in your economy, or even outside of your economy. So one fundamental issue to confront first is, is this just an income effect? You have just sacrificed a return on a portfolio, but that's a price you are willing to pay as an insurance premium.

If it's just an income effect, then it might be hard to make an argument that this is necessarily harmful for domestic investment and growth. Why? Imagine the perfectly elastic, frictionless model of capital accumulation in a neoclassical world in a small open economy. You have decided to buy a lot of reserve assets but you can still access the world capital market at a real interest rate r^*; your domestic interest rate or return on capital is presently $r > r^*$, so you can still just borrow whatever you want, until the two equalize. You have just increased your gross position, but the net effect on your economy needn't be significant at all, it could be zero. Official flows leave, but private flows come in to take their place. So presumably to get some traction you have to break that kind of simple neoclassical assumption. How? You have to argue that something else is going on. That is where we get back to the point I made earlier, that you may need to look at individual balance of payments and asset accumulation items to try to figure out what is going on. Why are these changing? And are they leading to changes in private savings, the current account, and investment outcomes that you can identify as being really different?

Comment 4. A potential problem is that this could all go the other way. A well-known example, widely discussed in recent years, is the so-called Bretton Woods 2 argument of Michael Dooley et al., and their so-called total return swap view of capital flows. Their argument is that, when an emerging country accumulates all these official reserves, it does not lead to the outcome of lower investment, but rather higher investment—because the reserves have made the economy a safer investment bet for foreign capital. It is as if the economy is posting a bond or forming some kind of collateral that makes global investors more likely to put money into the economy, say, via a lower risk premium, thus allowing the country to invest more and grow more rapidly than it otherwise would. So there are theoretical arguments out there, though they haven't

necessarily been empirically tested, that could have these mechanisms operating in exactly the opposite direction of what Carmen proposes.

Comment 5. Carmen speaks of the rise of home bias, which may be perplexing or confusing to some in the audience who use that term or read about it in other settings. That's because she is talking about something slightly different than we typically mean when we use the term. Gross foreign asset positions are massively up for emerging and advanced economies, and we know that is a long-run trend. More foreign assets, more foreign liabilities: We see that in all the charts churned out by the IMF and others. In conventional parlance that doesn't necessarily sound like a world of increasing home bias, does it? Furthermore, we know there has been a shift away from currency mismatches, as work by Lane and Shambaugh have shown, so some of the more disaggregated features of home bias have been diminishing. But I believe Carmen has in mind a different use for the term home bias (and it may be a bit of a stretch), that is, the composition asymmetry between public and private portfolios. For an emerging country there is clearly a foreign bias in assets, via its accumulating all these official reserves, so there is bias in that part of the portfolio. My one thought is that it takes two to tango. If there is home bias in liabilities and foreign bias in assets for emerging markets then that's got to be counterbalanced by exactly the opposite for the rich countries or at least for the rich reserve countries. Which reminded me of the work by Gourinchas and Jeanne. To get this outcome you have to have some venture capitalist countries on the other side of the transaction, willing to be long emerging risky assets and also supply the safe assets that the emerging countries want to buy. So does that make them home biased too, in some different way? It is home bias in an unfamiliar shape, and I worry that it is potentially perplexing terminology. Maybe there is a different, and less confusing, way to describe it.

My five parting questions:

- Great paper, good description but could we get more formal empirical evidence? I think that's going to be hard because it is a small sample, but it could be feasible if we slice up the data more.
- Can we get causal inference? I think that's going to be harder still; I don't know if we can go beyond correlation and get to causation.
- Can we discriminate between this story and the Bretton Woods 2 story, which is the main counter-example of reserve accumulation encouraging investment rather than discouraging it.
- Is sterilization a key part of the argument, for example when central banks borrow in local currency by issuing sterilization bonds? Can we get more data on that? This is another piece of granular data that might

help cement the argument. For China that is a big part of the story. It might be nice to relate that to the large debate on sterilization. Does it depend on capital controls and other features of the policy environment?

- Last, can we say something about the political economy? We talk in macro about these countries having purchased insurance and gotten rid of currency mismatch, but we are talking about the entire national balance sheet here. I think an important issue, particularly if—or should I say when—we get an emerging market crisis is that all the insurance is on the official balance sheet, but many risks are on the private sector balance sheet. Who gets access to the insurance and who doesn't? Is it going to be banks or firms or households in the private sector, and which ones? There are large political economy questions, and these could be the next disruptive events for the emerging economies: moving from how to accumulate reserves in good times to how to dole them out in the bad times.

COMMENTARY

Crowding Out Redefined: The Role of Reserve Accumulation

Brad DeLong

Let me second what Alan Taylor said. This is another high-value paper by Carmen Reinhart and Takeshi Tashiro. And it shares the four traditional high-quality characteristics of Carmen Reinhart papers:

1 It uses data that we have not looked at before or that we have not looked at in this way before.

2 It presents the data in a very interesting and thoughtful manner that makes us think very hard about important questions.

3 It does not focus on either the trend or the cycle exclusively, but looks hard at the interrelationships between them—interrelationships between the cycle and the trend that are traditionally ruled out, or at least not at the forefront of, our back-of-our-envelope first-cut.

4 It does not bow to current theoretical perceptions, but attempts to focus our attention on what the important and interesting features of the economy are.

This paper, in brief, is about the long and dark shadow cast by the cycle on the trend. In this case, the cycle is the 1997–98 Asian Pacific Rim financial crisis. The trend is what that crisis has meant for the development of Asia's Pacific Rim since. The at least partial motivation for this paper is that, right now, the European periphery is going through something somewhat similar to the sudden stop experienced by Asia's Pacific Rim in 1997–98. The point is to look at the European periphery today in the Asian Pacific Rim 1997–98 mirror, to see what we see. What do we see?

The origin of the sudden stop is that something bad happens to foreigners' willingness to invest in a particular region. Something bad happens to foreigners' willingness to hold assets located in a particular country. The financial system has to deal with this disruption to the previous pattern of financial intermediation. And the economy then has to deal both with the changed financial situation and with the real side concomitants of that shift in foreigners' preferences.

When we back up and assume the economist's 30,000-foot perspective, there is no reason why a shift in foreign investors' preferences and even a disruption of financial intermediation should materially disturb the real economy of production, employment, and investment. The market—or the central bank or the government—should be able to build, should have already built, firewalls to guard against financial disruptions of real production, employment and investment. The marginal rate of time preference hasn't changed. The marginal profit of capital hasn't changed. The marginal product of labor hasn't changed. All that has happened is that foreigners have suddenly lost their taste for buying assets denominated in a particular currency, located in a particular place. And we have a price—the value of the currency—that is supposed to smoothly match demand and supply in the market for assets located in a particular place, and markets are well designed to deal with both the partial and the general equilibrium adjustment to preference shifts.

Markets deal with such a shift in preferences by lowering the price. In the case of demand for Asian Pacific Rim or European periphery assets, the market's natural adjustment path is to bounce the value of the currency down. Bounce the value of the currency far enough that speculators think its next move is at least as likely to be slightly up as it is down. There is then no reason for safe interest rates in the region to rise. The situation stabilizes. And you have, at worst, a short, sharp, V-shaped downturn followed by an export boom. And yet that is not what happened. On the Asian Pacific Rim in 1997–98, the fact that so much of the region's debt was denominated in dollars meant that bouncing the value of the currency and thus of domestic production down far enough raised universal and valid fears of bankruptcy, and sharply raised risk premia. The Asian Pacific Rim thus had to, to a certain extent at least, defend its currency. And in Europe's periphery, nations are tied by treaty, by the deep and close technical integration of the financial system, and by hopes for a united and peaceful European future in the euro zone. Thus when the crisis comes both regions must generate rapid adjustment of the current account: a sudden stop.

The problem is general. There are lots of reasons why the natural market's bounce-the-value-of-the-currency-down adjustment mechanism will not work. Overwhelming reasons to maintain a fixed parity. High levels of harder-currency debt. A tight coupling of import prices to domestic inflation and a belief that the costs of accepting domestic inflation are unacceptable—*cough cough*, why we all today feel sorry for Raghu Rajan. In any of these cases, when the crisis comes you must generate a rapid adjustment in your current account, and the easiest and the most straightforward way to do this is via domestic

investment collapse. This is the first failure of the veil of the financial system to be merely a veil—the first coupling of financial distress to destructive real economic consequences.

But this should be temporary. This should produce a V. What Carmen and Takeshi impressively document is that investment does not come all the way back. We have a long-run 6 percentage point of GDP delta in the investment share in the post-financial crisis period relative to the pre-crisis normal on the Asian Pacific Rim. We have a long-run 2.5 percentage point per year delta in real GDP growth. We do not have a V. We have an L. And there does not appear to be anything going on in terms of exogenous breaks in long-run trends that would lead us to say that the trend break was going to come anyway, and that the financial crisis disruption and according depression was not the cause but the consequence of the trend break.

Now when I was young I was taught that the trend was the trend and the cycle was the cycle, and that sometimes breaks in the trend caused cycles, but not vice versa—or at least not vice versa enough that we needed to worry about it. When I was young I was taught that Say's law held in the long run, even if not in the short run, because the interruptions of Say's law that caused demand cycles were driven by sudden excess or deficient demands for the stock of liquidity, and those had a natural end because flows accumulated to make up stocks.

Furthermore, when I was young I was taught that central banks were large and powerful enough to make Say's law roughly true in practice even though it wasn't true in theory even in the relatively short run—that the short run of aggregate demand shortfalls, and the durations of V's, was limited to two or at most three years. There could be surprises, and long and variable lags. Those blocked offsetting demand shocks immediately and instantly, but the duration of the surprise was limited to the period of predetermined prices.

And I was taught that capital should flow downhill; that at the level of international economics, governments really, really were agents of the citizens so we could view the country as a whole; and that we could expect to find conditional convergence of living standards and productivity levels across nations—convergence conditional on getting good institutions, that is.

Yet these do not appear to be so. The fallout from the 1997–98 financial crisis has been very large, very persistent. This is even more puzzling because, as Alan Taylor said, the post-crisis policy reaction of building up reserves should not be a drain on national savings at all.

The way such situations were supposed to be handled was that when there was, in the words of the Articles of Agreement of the International Monetary

Fund (IMF), a "fundamental disequilibrium," the IMF was supposed to step in. The IMF was there to take the blame for getting the country to do the things that had to be done in the long run to balance resources against commitments—to take the political fallout, and thus induce politicians to do what was necessary rather than kicking the can down the road while they hoped for a miracle. The IMF was there to get the country back onto its proper long-run trend growth path. And the IMF was there to provide bridge financing to make the process of adjustment as painless as possible.

In this case, the Asian reserve accumulation after 1997–98 is best viewed as a recognition by the Asian periphery that they really did not believe that they could trust the IMF to do its proper job.

In 1997 and 1998, from the Asian Pacific perspective at least, first-world international speculators suffered a great failure of nerve, an irrational panic, and fled the region. This panic was irrational: Those that held on did fine, and those that bought into the crisis did enormously well. But in the crisis the IMF did not do its job—did not provide enough funding fast enough with appropriate conditionality. And the sovereigns of the Asian Pacific Rim reacted to this by deciding to build up their own reserves so they would never be forced to rely on the IMF again.

That decision should create added confidence. That you can now invest in Malaysia or Korea or Indonesia without worrying about any kind of financial disruption because its own central bank stands ready to smooth adjustment and does not need the IMF cavalry should lead to a higher investment share, not a lower one. Thus, relative to what is my and what I suspect is Alan's counterfactual, the investment gap is even larger than Carmen makes it sound.

Could the investment gap possibly be crowding-out by China's production—the idea that there is a niche in the world economy for export-oriented rapid development by some emerging market countries with low-valued currencies, but that in 1997–98 it became clear that China was going to occupy that niche and bigfoot everyone else out of it? In response to that recognition, the argument would go, the returns to investment in the Asian Pacific Rim would drop, and so investment would drop. I do not believe so, largely because I have sat at the feet of Chang-Tai Hsieh and learned from him how very tightly coupled the Chinese export sector is to the other economies of Pacific Asia. More exports from China mean more work and more profits elsewhere on the Asian Pacific Rim. China actually getting its act together and taking up a position as the supplier of the low-wage component of the value chain seemed and seems to me more likely to raise the marginal product of the capital in the rest of the Asian rim rather than lower it.

Could it be that there was a negative shock to the expected pace of globalization in the aftermath of 1998? Again, I find this hard to credit. If anything, globalization has proceeded faster since 1998 than anyone had previously imagined it could before. Before 1998, we were gradually realizing that the moving of industrial production out of the North Atlantic core to the periphery was a thing that was now possible. We were realizing that containerization had produced another great downward leap in costs of transport. But we had little idea then how much difference the coming of modern telecommunications would make in tying the world together.

Thus I find myself puzzled when I try to think of how 1997–98 could have seen a shift in trend that then caused the cycle. I cannot see any exogenous change in the trend that would validate either the post-1998 slowdown in growth and investment or the financial crisis itself as a reaction to bad news about future growth in Asia.

Thus I am left hunting for other explanations. One of them is definitely Carmen and Takeshi's: that this particular 1997–98 financial crisis is casting an extraordinarily long shadow on the economy. This may in some way be tied up, in a manner I do not understand, with the astonishing role that the dollar has played over the past 15 years. There is an economist in the front row who, back in 1979 and 1980, made me read at extended length about Robert Triffin and the Triffin dilemma. And at the time I thought that this was a waste of my time—how could we ever again get into a situation in which it would be useful to characterize the world economy as suffering from a dollar shortage—a shortage of dollar-denominated and U.S. property- and taxing capacity-backed liquid assets? Yet, now, we have seen not just Bretton Woods II but the return of Robert Triffin at a scale that even back in 1998 I would never have believed possible in a thousand years. The bottom line, I think, is that we have a difficult task before us. Our old belief that you have a trend, and you calculate the trend; that you have a cycle, and you argue over whether the cycle falls below the trend or fluctuates around it; but that you can carry this discussion along two separate and largely disconnected tracks—that belief looks to be simply wrong. Carmen and Takeshi's paper here is another nail in the lid of its coffin, another demonstration that by trying to think in such ways over the past generation we have not done ourselves or the world a very good service.

GENERAL DISCUSSION

Crowding Out Redefined: The Role of Reserve Accumulation

Chair: Reuven Glick

Mr. Glick: We're going to give Carmen a chance to respond first.

Ms. Reinhart: Very quickly, I want to thank both discussants. This is a first pass at this topic, so I welcome all the suggestions. Let me deal with just a couple of things. On the question of stocks versus flows of foreign assets and liabilities, there is actually quite a bit of analysis on flows. Reserve accumulation as a share of GDP averages about 4 percent a year, though with a lot of variation across countries. A natural next step would be to further expand that analysis and include flows of private savings, consumption, and public saving—to give a broader picture. But some of that is already in the paper. Regarding the concern that this might be a bizarre way to characterize home bias: We tend to think that because gross flows are large we have a good measure of capital mobility. If you look at the savings–investment correlation measure by Feldstein and Horioka you would actually find very low correlations, but it would not necessarily be because capital is mobile. It's because reserve accumulation introduces a wedge between saving and investment. I take the point that perhaps we should be more careful on how we talk about home bias. But in the end it's very asymmetric. Emerging market countries are holding less external debt and much more external assets.

Brad's comment on whether Asian economies experienced a V or an L-shaped recovery is an excellent point. I need to look at that more. The story I am telling here is definitely an L story when it comes to investment, yet the export picture post-crisis is a V-shaped story in Asia. I don't know what the labor market story is, but that may also help determine whether it's a V or an L shape. Another point that Brad made on China is the big foot of China somehow accounting for the regional investment decline. I haven't looked at the issue of substitutability and whether that has been a drain.

On Bretton Woods II, Alan I would say if indeed the benchmark is such that reserve accumulation generates confidence in higher investment, then there's a

bigger gap to explain. But let me stop there. I think these are wonderful comments on a very early stage of this research project, so I thank you.

Mr. Glick: Okay, we have time for three or four questions, so we're not going to be able to recognize everybody. I'm going to start with Joshua and then Barry.

Mr. Aizenman: I enjoyed the session very much but I have a question and maybe a suggestion. Your focus seems to be on the 1997–98 crisis. Maybe we could switch to the present crisis and the future. Globally, we know that the sum of saving equals the sum of investment. There seems to be a difference in the global configuration after the 2008–09 crisis versus the 1997–98 crisis. My question is, to what degree might the future differ from the adjustment that you are portraying after 1997–98?

The logic is simple: After 1997–98 the OECD countries were growing reasonably fast in terms of their historic metrics and unemployment or low employment was not an issue. You ask what the source of reserve accumulation was for most of Asia after 1997–98; I believe more than 60 percent was due to balance-of-trade surpluses.

At that time it was fine because of the Clinton-era U.S. economic boom and the continuation of sensible growth of the OECD. But now we are in a new universe. My sense is that the global budget constraint of aggregate saving equaling aggregate investment is going to bite much more now politically. At the beginning we heard about the rumble of mercantilism in Germany. Chances are that if global growth doesn't pick up substantially, we are going to hear much more about this. The sheer size of China implies that the old regime is over. In my work with Nancy Marion we find that in the last two or three years, China's reserve ratio peaked around 50 percent, but then declined marginally. A lot of this is associated with a rapid increase from a low base of outward-oriented foreign direct investment (FDI) from China. So my uneducated hunch is that in the next 10 years the terms of this topic will differ from the 10 years after 1997–98. The trends in China are indicative of this. By default the success of China's economy growing so large is constraining its ability to continue doing what it did after 2001.

Mr. Eichengreen: The previous paper by Lant Pritchett told us there's reversion to the mean in growth rates. Can there be a reversion to the mean in investment rates? Investment is a source of growth. If you remember the skyscrapers in Bangkok and the expansion binge of the chaebol, you would think the two go together. How do we know that the main thing going on here is not simply

an inefficiently high level of investment before the crisis rather than reserve accumulation?

Mr. Ostry: I really like this paper and the discussion. I have a question about whether our policy advice may not be behind some of the things we observe in the data. To wit, when money is flowing into emerging market countries, we condone the use of intervention to moderate the upward pressure on currencies, both out of concern about competitiveness and also presumably for financial stability reasons—namely, that we want the reserves there when the cycle turns around. But when the cycle does turn around, we tell emerging markets to only intervene to guard against disorderly conditions, whatever those mean in the foreign exchange market. Don't use the reserves to moderate the downward pressure over a longer period on the exchange rate, which presumably was part of the reason why they built up reserves in the first place. Could that asymmetry in our policy advice be behind some of the things we observe?

Mr. Glick: Mr. Choi and then Ron McKinnon. We'll have to close it there.

Mr. Choi: It's great to see the link between foreign asset accumulation and investment. I have researched this issue, and the impact of reserve accumulation on investment is substantial for Asian economies. It is noteworthy that when foreign exchange intervention is sterilized, the negative impact of reserve accumulation on investment will be pronounced. Now turning to what other factors could explain the low investment rate. It seems that investment could also have been affected by excess capacity after the Asian crisis. Another possible factor is an increase in policy and economic uncertainty after the global financial crisis. And for recent years, some emerging economies including Korea might have experienced a fall in foreign direct investment abroad that might have had the effect of lowering the domestic investment rate. Thanks.

Mr. Glick: Last question is from, Ron.

Mr. McKinnon: This is a short question. Carmen, I liked your paper very much, but couldn't you have a simpler description for the buildup of foreign exchange reserves? As a matter of fact, the words foreign exchange didn't enter any of these speakers' comments. I would suggest that the buildup of dollar reserves earning nearly no interest is just an accident. It's not deliberate policy on the part of my Korean friend or Chinese friend. Rather, it's policy made in Washington because interest rates are so low that you get huge hot money flows into developing countries. They intervene desperately to prevent appreciation, and they create excess money. And then, Carmen makes a very good point, the

sterilization of this money creation might crowd out domestic investment. But this is not a conscious portfolio choice, it's an accident coming out of very low interest rate policies in the United States.

Ms. Reinhart: I would like to thank everyone for their comments and questions. Let me very quickly try to hit some of the highlights.

Joshua, I do think that over time country profiles are changing. We are seeing and have seen massive current account reversals in Europe post-crisis, noticeable in the periphery. Irish investment-to-GDP is off 15 percent since the crisis—big swings in the current account. But this makes the question for Asia all the more compelling. Fifteen years after the crisis, they have surpluses with a special focus on avoiding current account deficits because those got everyone into trouble in the past. The policy of avoiding current account deficits persists. Further declines in investment may have to be engineered unless someone comes up with very creative ways of increasing saving, which would be difficult given how high saving rates in some countries already are. But I believe that the forward-looking part is definitely not an extrapolation of the post-crisis experience. I looked at the 15 years since the 1997–98 crisis because, as Brad pointed out, I wanted to consider not just the immediate aftermath but also the longer consequences.

Barry, I don't have an answer to your question. It's quite possible that one would get much of the decline in investment whether you accumulated reserves or not. If you look at the previous presentation by Lant Pritchett, a lot of the growth spurts identified in that paper ended much earlier. The growth spurts in Japan, Korea, and Singapore all ended before the Asian crisis. The ones in Thailand and Malaysia ended much closer in time. So if one takes the logic of that paper for Malaysia and Thailand you could probably explain a lot of the decline in investment as mean reversion. I don't consider this in my paper. My message is the case that foreign reserve accumulation is an official outflow—that represents saving that could potentially finance higher investment as well as higher consumption.

Jonathan, I do think that the pattern of reserve accumulation in emerging markets is intimately connected to the policy advice. The thrust of the paper is not to say that we had it all wrong about reserve accumulation. There are good reasons for past policy, and I hope my table in the presentation highlights that. Some countries had major debt crises not because their debt was high but because there wasn't adequate coverage because their debt was in a foreign currency. I do believe there is an asymmetry between dealing with inflows and outflows. But I wanted to, for lack of a better term, internalize in the policy

discussion the fact that there also may be side effects in the medium term—not necessarily related to vulnerability in the next six months or in the next year. Again, I want to emphasize that this is a first pass in this area, but I think the issue of separating sterilized from nonsterilized intervention is very important. In sterilized intervention, the central bank either sells its existing stock of government securities—in which case you're back to more traditional measures of crowding out. Or the central bank actually issues its own securities to sterilize—which is also within the realm of crowding out—and then it gets more unclear whether it's nonsterilized intervention. It's still a drain, but it gets more unclear.

Ron, in the paper I really don't talk about the drivers of capital flows, which is what you're talking about—you know, the old question of push versus pull. Are the drivers external factors or domestic factors? I start from the premise that the policy had been reserve accumulation. Here it goes back to something Alan Taylor said. Alan, I don't agree with what you said about the process of reserve accumulation going back to the 1990s. One striking thing about the data is that, despite the fact that we supposedly had more fixed exchange rates back then, before 2001 you see periods of both reserve accumulation and deaccumulation. But since 2001 it's all going in one direction, towards more accumulation. You've raised bigger issues, Ron, than what I've tackled here

Mr. Glick: That's the last word. So, I want to thank our participants: Carmen, Alan, and Brad.

LUNCHEON KEYNOTE ADDRESS

The Shifts and the Shocks: Emerging Economies in an Age of Financial Crises

Martin Wolf

> *"My view is that improvements in monetary policy, though certainly not the only factor, have probably been an important source of the Great Moderation. In particular, I am not convinced that the decline in macroeconomic volatility of the past two decades was primarily the result of good luck, as some have argued, though I am sure good luck had its part to play as well."*
>
> —Ben Bernanke, Federal Reserve Board Governor (2004)

The past is a foreign country. In a celebrated speech on what economists hubristically called the "great moderation," Ben Bernanke talked about what now seems a different planet—a world not of financial crisis and long-term economic malaise, but of outstanding stability and superlative monetary policy.[1] This may seem exaggerated. But look at what then-Governor Bernanke (2004) said: "improved monetary policy has likely made an important contribution not only to the reduced volatility of inflation (which is not particularly controversial) but to the reduced volatility of output as well." This seems quaint.

The economics establishment failed. It failed to understand how the economy worked, at the macroeconomic level, because it failed to understand financial risk, and it failed to understand financial risk because it failed to understand how the economy worked at the macroeconomic level. The work of economists who did understand these sources of fragility was ignored because it did not fit into an imaginary world of rational agents that the professors Pangloss had made up.[2]

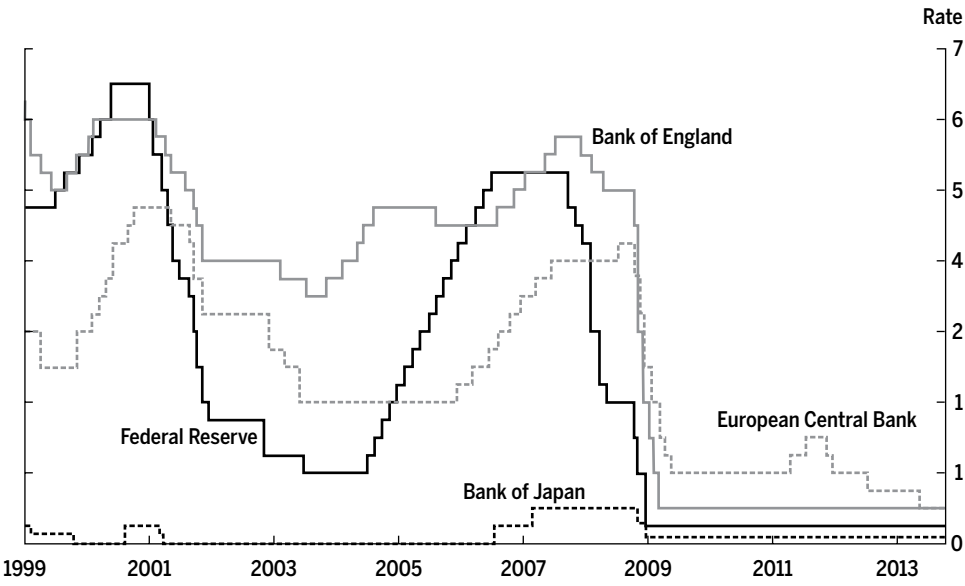
In what follows, I intend to address four questions: Where are we? How did we get here? What are the global implications? What are the implications for emerging economies?

Where Are We?

Sometimes, indeed, I have to pinch myself. Since 2008, the high-income countries have been in a "contained depression." What are the symptoms? They are the combination of exceptionally aggressive monetary policies with weak economies, high unemployment, and low inflation. The Bank of Japan's official intervention rate has been close to zero since 1995. The Federal Reserve's rate has been near zero since October 2008. The Bank of England's reached half a percent in March 2009. The European Central Bank (ECB), the advanced economies' most conservative central bank, has been at half a percent since May 2013. But it reached 1 percent four years earlier, then made a ridiculous effort to raise rates in 2011, and was driven back down to 1 percent by December. Nobody but the ECB can imagine its monetary policies are not now too tight. Moreover, beyond nearly free money, these central banks have all engaged in huge expansions of their balance sheets (Chart 1).

Nevertheless, of the six largest high-income economies, only the United States and Germany were larger in the second quarter of 2013 than they had been at their pre-crisis peak. More striking is how far economies had fallen below pre-crisis trends. In the third quarter of 2013, the euro zone economy was 13 percent below its 1995–2007 trend, which was already far from dynamic; the

CHART 1
Central Bank Policy Rates



U.S. economy had fallen 14 percent below its 1980–2007 trend (Chart 2); and the U.K. had fallen 18 percent, again below its 1980–2007 trend (Chart 3).

A principal reason for this contrast between policy and effectiveness also seems clear: The credit machine broke. Measures of broad money have been stagnant and, to the extent that they have not been, that was because of quantitative easing. The evidence supports the view that, in the aftermath of a huge financial crisis, monetary policy is not that effective (Chart 4).

The zero lower bound bites. But willingness to use fiscal stimulus, which is the direct means of lowering excess desired savings, was, alas, limited. Politicians and the public suffered from "sticker shock" when they saw the huge fiscal deficits of 2009 and 2010 and chose austerity instead, leaving the macroeconomic policy burden on the frail shoulders of central banks.

How Did We Get Here?

The crisis is, in my view, the result of the interaction of a global savings glut (as suggested by Bernanke as chairman of the Federal Reserve), with a fragile financial system. The link between the two was forged by inflation targeting monetary policy, especially in the United States, but also inside the euro zone. When what some call the "Minsky moment" hit the financial system, the result

CHART 2

U.S. GDP against Trend to 2007:Q4

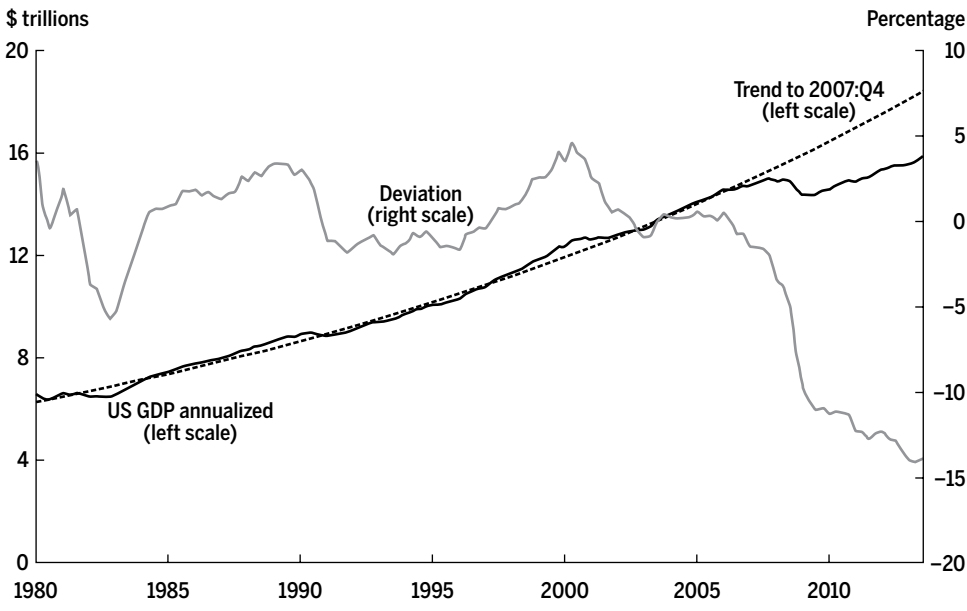


CHART 3

Euro Zone GDP against Trend to 2007:Q4

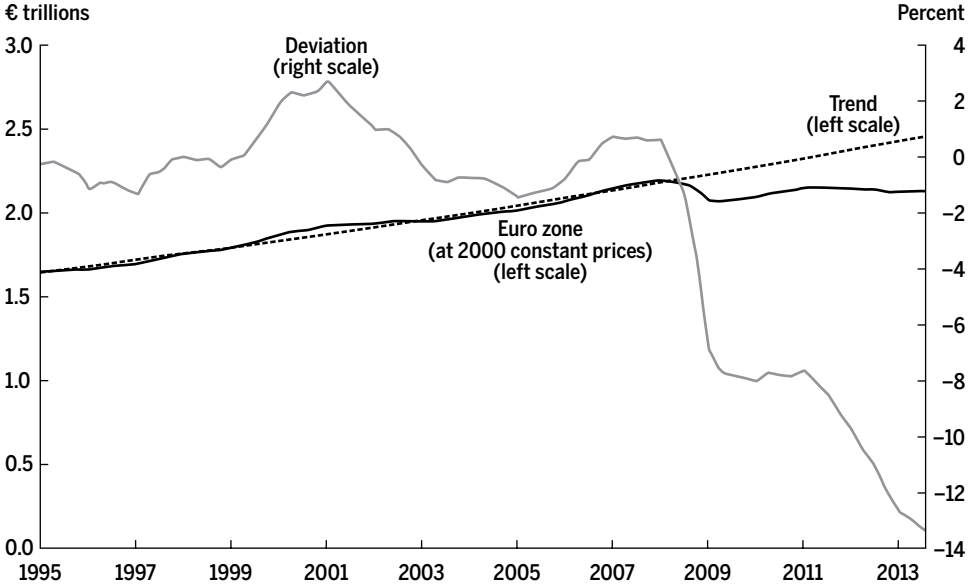
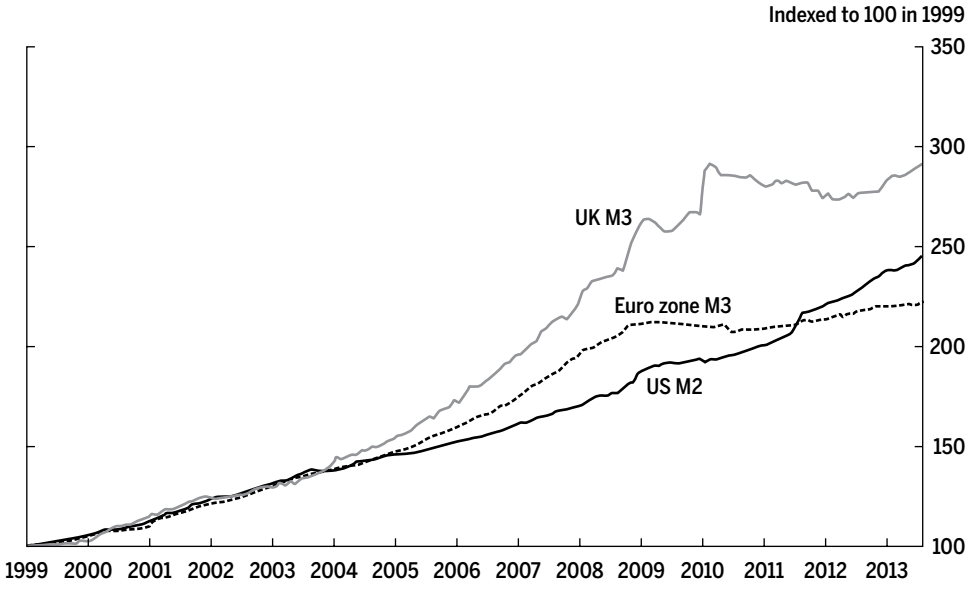


CHART 4

Money Supply in the Great Recession



was a huge financial crisis, a decision by states to back the entire financial system of the high-income countries and the aggressive monetary policies we have seen.

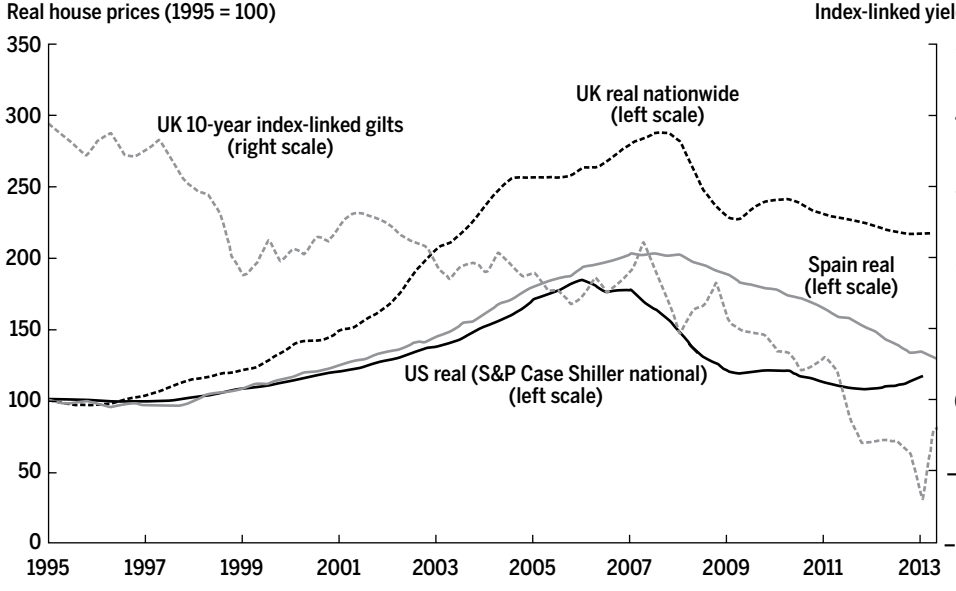
The Asian financial crisis coincided with a sharp fall in global real interest rates, which is indicated by evidence from index-linked government bonds. The fall in U.K. index-linked bonds was from close to 4 percent before mid-1997 to close to 2 percent afterwards. This fall coincided with a beginning of house price inflation in developed economies with elastic supply of credit. Causality is hard to prove. But it is hard to believe these are mere coincidences (Chart 5).

In the aftermath of the Asian financial crisis, many emerging economies, including China, decided to pursue exchange rate, monetary, and other policies that generated large current account surpluses. How far these surpluses were intended is unclear. They were a natural reaction to the Asian crisis—"never again" became the motto—and a natural consequence of sterilized interventions in foreign exchange markets. But they were also supported by important structural shifts towards profits, particularly in China. These policies contributed to the emergence of large "global imbalances."

Three important capital exporting regions emerged: China and other emerging East Asian economies; the oil exporters; and some old industrial

CHART 5

Real House Prices and Real Index-Linked Yields



countries, particularly Germany and Japan. At the same time, two important capital importing regions emerged: the United States and peripheral Europe. The latter, without exception, went into financial crisis in 2008–10. Was this yet another coincidence? I suggest not (Charts 6 and 7). An important feature of this world was that the fast-growing economies with, one must presume, the best opportunities, decided to lend huge quantities of capital to slow-growing economies with poor investment opportunities. The plausibility of the latter view is supported by another important fact: In the high-income economies, nonfinancial corporate sectors mostly ran financial surpluses: Their retained earnings exceeded investment. This was partly because of the exceptional buoyancy of profits. It was also because of the weakness of corporate investment after the stock market bubble burst in 2000. Thus, the domestic counterparts of the capital flows into the high-income countries were, again almost without exception, fiscal and household financial deficits, the latter associated with rising real house prices and booms in residential construction.

Meanwhile, in an environment of depressed returns on safe assets, we saw the "reach for yield," the financial sector fabricated pseudo-triple-A, mortgage-backed assets in huge quantities. These were indeed "fool's gold," as my colleague Gillian Tett (2009) called it. The financial sector also leveraged itself up

CHART 6

Global Imbalances as Percent of World GDP

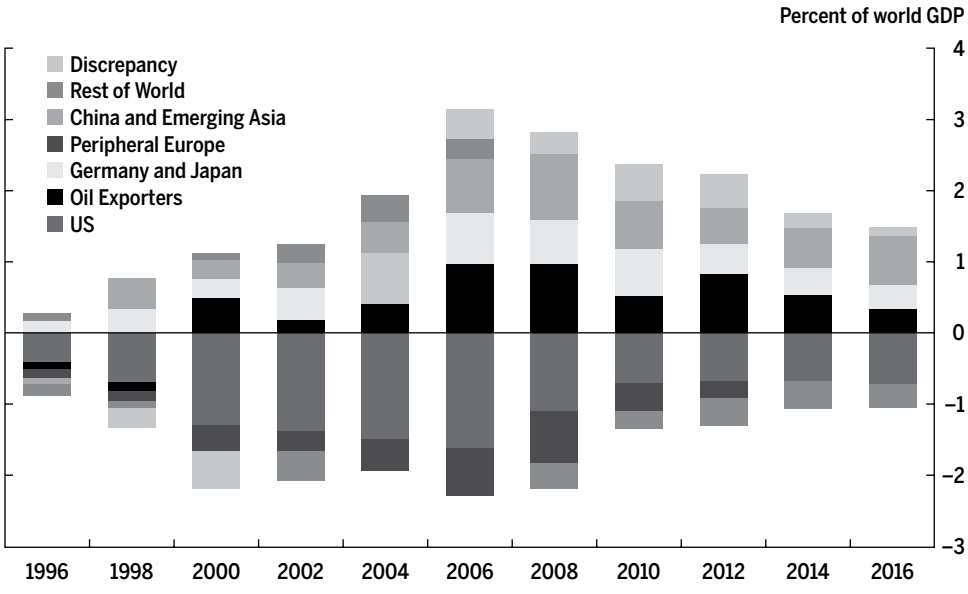
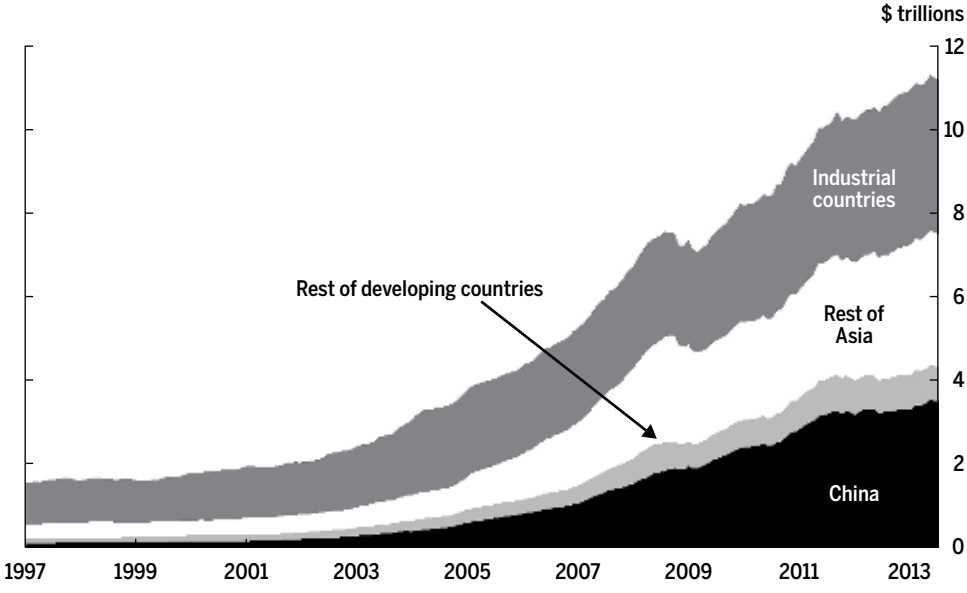


CHART 7

Foreign Currency Reserves



dramatically in order to boost its return on equity (unadjusted for risk) and pay its management and staff huge bonuses: In the United Kingdom, for example, the median leverage of the banking sector rose from 20-to-1 to 50-to-1 during the first decade of the 2000s, only to collapse after the crisis was over. Furthermore, there was a huge expansion of the shadow banking sector, which increasingly took on the risks of a traditional banking sector, but without comparable oversight or insurance. Should someone steeped in the history of financial crises have been surprised? Not really. It was just a more imaginative and higher-technology version of previous excesses, supported by the illusion that our sophisticated modern financial sector understood so well how to manage risk that it needed essentially no capital at all.

The Federal Reserve played a central role in all of this. As a result of policies pursued elsewhere, the U.S. external imbalance increased rapidly in the late 1990s and 2000s. This was (and remains) of no direct concern to the Federal Reserve. But it is a disinflationary force, tending, all else equal, towards underutilized capacity, rising unemployment, and falling inflation. As an inflation targeting central bank, the Fed's job was to offset the external drag. It did that with its aggressive monetary policies of the early 2000s. But how do the monetary policies work on the economy? The answer is that they work either

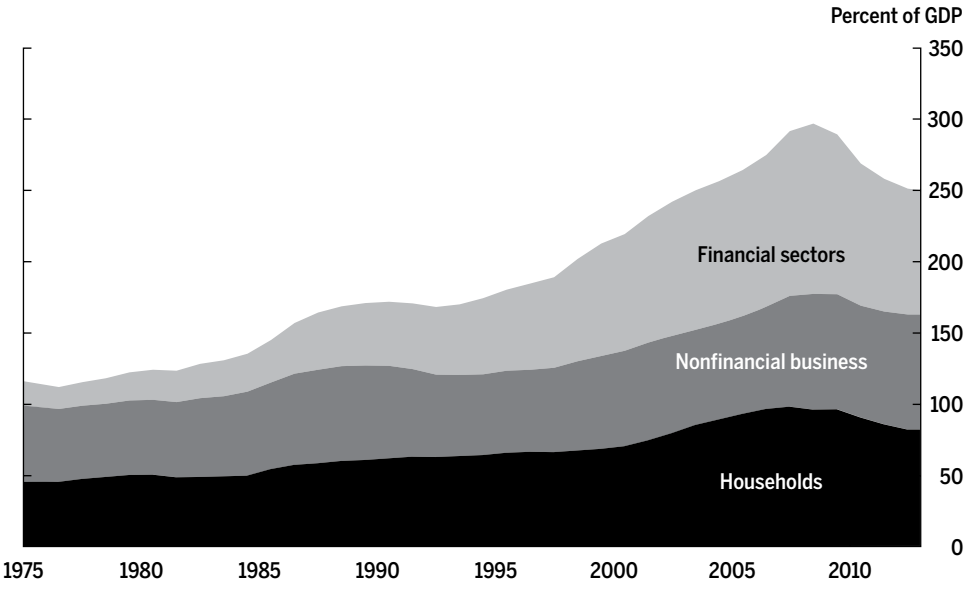
through changes in asset prices or through changes in borrowing, or, more usually, both. In this case, it was both. It worked, above all, through house prices and the associated lending and borrowing. Moreover, as the supply of credit grew (created by the financial system itself) it went out into the world looking for better returns than those available in the United States. The United Kingdom was directly affected.

The ECB accommodated a similar process inside the euro zone. Partly as a result, overheating in credit-elastic peripheral euro zone economies suffering from "interest-rate illusion"—the confusion of nominal with real rates natural in economies that had never enjoyed such low rates before—offset the extreme weakness of demand in the creditor countries, particularly Germany. The euro zone average, which the ECB targeted, consisted of a part of the economy that was much too cold and a part that was much too hot, with capital flowing on an enormous scale from the former to the latter.

In brief, with house prices soaring and credit exploding, households and financial sector gross debt expanded rapidly relative to GDP (Chart 8). This combination of asset price inflation with a huge rise in gross indebtedness was sufficient to stimulate additional spending by households on consumption and residential investment. This then balanced economies that had huge current

CHART 8

U.S. Cumulative Private Sector Debt over GDP



account deficits and so the world economy as a whole. But it did so only until the crisis hit. Then the pumped-up demand collapsed, leaving policymakers the painful job of trying to pump it up again.

What Are the Global Implications?

What we might call the "advanced countries crisis" is merely the biggest, but far from the first, large financial crisis of the past four decades. It was preceded by the debt crisis of the developing countries in the 1980s, the Tequila crisis of the mid-1990s, and the Asian financial crisis of 1997–98. Also important, in a host of other crises, was the one that hit Japan in the 1990s, which first brought the liquidity trap and the zero lower bound back to economists' attention.

This history of devastating crises and particularly this last crisis, which took place in the world's most sophisticated financial systems and most advanced economies, raises some very big questions. Here are five, with some brief and (I hope) provocative answers.

First, are such crises inescapable features of a liberal global economy and financial system?

The answer is: yes. The question seems to be how big and how often, with the horrible possibility that the less often they come, the bigger they will be. Stability breeds risk-taking, which then generates instability.

Second, was the notion that inflation targeting would bring economic stability a feeble-minded delusion?

The answer is: yes, it was. The critics were correct. Stable inflation may be a necessary condition for financial and economic stability (though one can debate even that), but it is clearly not sufficient.

Third, what is the role of an unconstrained credit system in generating instability?

The answer is: fundamental. Financial crises are either fiscal or private. We know, more or less, what causes a fiscal crisis. Private crises are created by the ability of an elastic financial system to generate unlimited increases in credit, until solvency concerns bring the party to a halt. But the elasticity of credit itself postpones concerns about solvency, thereby guaranteeing bigger crises in the end. This is why liquidity crises are ultimately solvency crises.

Fourth, will regulation allow us to enjoy the benefits of inflation targeting without risking huge financial crises?

The answer is: probably not. Macroprudential policy will frequently find itself fighting monetary policy. This tension is more or less inevitable since it is precisely when monetary policy is most expansionary that regulators are most likely to worry about financial sector misbehavior. The least bad option is

probably to force much higher capital ratios permanently, with some semiautomatic countercyclical adjustments.

Finally, is it important that there is no global authority capable of mitigating the causes of crises and managing them when they hit?

Yes and no. It would have helped if the obvious "adding up" problems of policymaking in the 2000s had been made still clearer. But the links between global macroeconomics and what was happening in the financial sector were obscure and, to many, remain so. It is highly unlikely that if the International Monetary Fund (IMF), for example, had been far more powerful than it was, it would have been able to prevent the crisis. Similarly, as the euro zone experience shows, the presence of a shared central bank does not prevent crises and may well worsen them.

What Are the Implications for Emerging Economies?

After the emerging economy crises of the 1980s and 1990s, the emerging economies sought to minimize the risks of crises. In general, their chosen solutions included:

- Conservative fiscal policies;
- More reliance on borrowing in domestic currencies;
- Inflation targeting central banks;
- Floating or deliberately suppressed and so "undervalued" exchange rates; and
- Exchange controls.

By and large, these policies have worked. Emerging economies have proven far more resilient to shocks than they used to be (Charts 9 and 10). This was proven dramatically true in 2009, except in central and eastern Europe, which had fallen into the trap of easy borrowing. This is partly because their policies were better. It is also because China proved able to respond to the crisis by expanding investment demand strongly. Nevertheless, changes in policies in advanced economies, especially the United States, affect them all. That was true in 2008–09 and again over the summer of 2013, in the aftermath of discussion of tapering by the Federal Reserve. Indeed, it is striking how large a jolt that announcement of a possible reduction in the rate at which the Fed expanded its balance sheet turned out to give.

Would that be different if the renminbi became a global reserve currency? It is hard to see any reason why it should, unless one thinks China's policies would be less domestically focused than those of the United States, which seems entirely implausible. Would it be better for emerging economies to have a choice

CHART 9

Net Capital Flows and Volatility

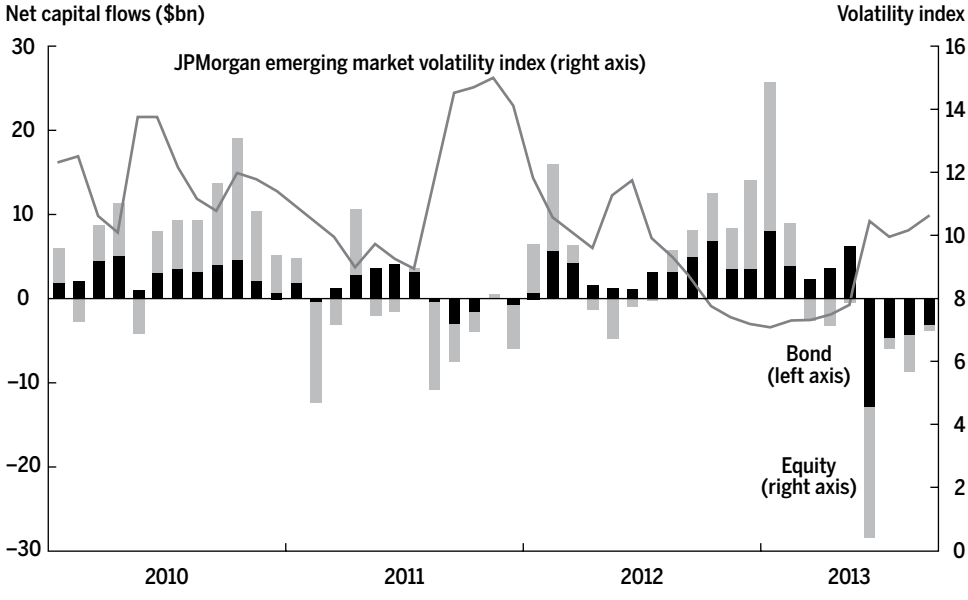
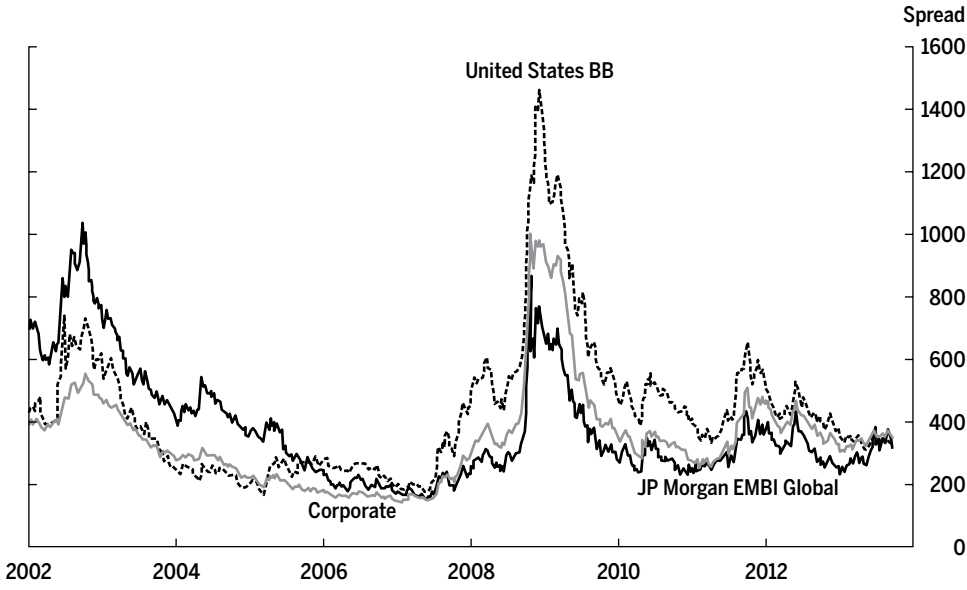


CHART 10

Spreads over U.S. Treasuries



of reserve currencies? Possibly. But that might also exacerbate instability among the major currencies.

It would be different for emerging economies if the incidence and scale of shocks abated. I can see little reason to expect that. So the main global reform seems to be greater insurance, in place of the colossal investments in self-insurance we have seen over the past decade. A much larger IMF would be one possibility. It would even be possible for emerging economies to pool their reserves separately from the IMF for this purpose. But that does not seem to be on the table.

Another and perhaps more plausible possibility might be an extension of swap lines from the core central banks, such as the Federal Reserve, to a growing number of approved central banks of emerging economies. This would be a carrot for reform in emerging economies. It would also reduce the risks of "sudden stops" and so encourage net import of capital by emerging economies, which the advanced economies should definitely want. It could also be justified as a way for the central banks of the core countries to reduce the risks of instability that then affect their own economies. In the absence of better insurance, I believe emerging economies must control capital inflows—both net and gross.

Conclusion

I leave with five conclusions.

First, the world economy is now in a very strange place. We should not forget how strange and disturbing it is.

Second, we should be devoting a huge effort to understanding why we have ended up in the world of the zero lower bound and the liquidity trap.

Third, there is no reason to be confident we have eliminated the danger of doing this all over again.

Fourth, emerging economies have learned quite well how to cope with this volatility. But that may be largely because they have avoided large net capital imports.

Finally, the way to encourage a better balanced and less crisis-prone world economy is partly via greater insurance of emerging economies against liquidity risk. That should, in turn, encourage capital to flow the way it should, from countries with poor investment opportunities to countries with good ones. Using the world's surplus capital to build houses in rich countries that nobody needs is silly. Let us not do anything as silly as that again.

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NOTES

1 Stock and Watson (2003) coined the term "great moderation."

2 Foremost among the economists whose views were widely ignored were the late Hyman Minsky and Charles Kindleberger. See, for example, Minsky (1986) and Kindleberger and Aliber (2011).

GENERAL DISCUSSION
The Shifts and the Shocks: Emerging Economies in an Age of Financial Crises

Mr. Wolf: I'm happy to take questions. I cannot imagine that I haven't enraged most people here.

Mr. Eichengreen: So Martin, that's a very complex narrative. You can't be accused of offering a monocausal explanation of the crisis. However, it did seem that you attached priority in your explanation to the mysterious decline in real interest rates around the turn of the century that set on foot housing booms and the associated financial excesses. That suggests that you really think the fundamental causes of the crisis were first China and the big inflows from China to the advanced economies. And number two, the euro, which was responsible for the decline in real rates in the European periphery. Without China and without the euro, does it follow we would not have had a crisis of this magnitude?

Mr. Wolf: I think that what was going on in the western nonfinancial corporate sector was also an important and interesting part of the story. It seems to me there were a number of forces coming together that were leading to the savings glut, investment at the global level, and the associated shift in real interest rates. But I do think that an important aspect of the monetary response, and therefore what I think affected the financial sector's role in this, was the way monetary policy evolved in the developed world and what it was trying to cope with. It does seem to me that the evolution of the current accounts, the savings surpluses, what Carmen Reinhart said about reserve accumulations, and the nature of the portfolio preferences of these countries had an important effect on the monetary policy response and on the financial sector consequences. Now, my view at the moment is that it is interesting to consider what would have happened if all our economies had been closed and we had simply liberalized their domestic financial systems. Would that have been enough on its own to have had these consequences? We can't run this counterfactual, so I think the answer is, we don't know. But I find it rather difficult to believe that would have been enough to create this degree of excess. Some of it would have happened anyway. I'm sorry I can't produce a simple monocausal explanation because I don't really think there is one.

Ms. Reinhart: You rightly point out that in the era of the Great Moderation these kinds of financial crises were unthinkable in the advanced economies. These things were supposed to only happen in emerging markets. And emerging markets often dealt with debt overhangs and debt restructuring. Could you comment on your view of restructuring issues in the periphery and debt restructuring in general?

Mr. Wolf: I think the observation is correct that developed countries have generally found restructuring of debt unthinkable. There's been quite a lot of restructuring of private debt in the United States. And in Europe, there's generally not even been much of that. My belief is that substantial amounts of debt restructuring in the public and private sectors in Europe including the United Kingdom are nearly certain to occur. The only question is when. Up to now, the assumption had been that, because we were able to operate zero interest rate policy, we can finance our governments relatively easily. That obviously didn't apply to the periphery. But it applied to the core. We hoped that we could get back reasonable nominal GDP growth sooner than we have—clearly we didn't expect still to be here, instead the assumption was that we would be able to grow our way out of this. The right relationship between nominal GDP and nominal interest rates would allow that to happen, because the assumption was that we would have sufficient control over both sets of variables. But it's now clear that in the periphery of Europe that isn't working and won't work, and the current policies clearly won't work. So debt restructuring seems to me pretty well inevitable. It is perfectly possible that we will end up in, say, the United Kingdom, having to do very substantial mortgage restructuring. And that depends largely on how well the economy grows and what happens to house prices and employment. But in the periphery of Europe—in Italy, Spain, and so forth, given their growth projection—given the likely growth rates and interest rates, I can't see any way they can get out of debt restructuring. I regard that as a necessary condition for a return to growth. It's not a sufficient one, because their economies remain deeply uncompetitive. And they're operating in a currency union, which has absolutely no demand for growth and which is falling into deflation. Therefore there seems to me no satisfactory way out of this, so debt restructuring is a necessary condition for getting back to growth. But it's a very long way from sufficient.

Surprising Similarities: Recent Monetary Regimes of Small Economies

Andrew K. Rose

In contrast to earlier recessions, the monetary regimes of many small economies have not changed in the aftermath of the global financial crisis of 2007–09. This is due in part to the fact that many small economies continue to use hard exchange rate fixes, a reasonably durable regime. However, most of the new stability is due to countries that float with an inflation target. Though a few countries have left to join the euro zone, no country has yet abandoned an inflation targeting regime under duress. Inflation targeting now represents a serious alternative to a hard exchange rate fix for small economies seeking monetary stability. Are there important differences between the economic outcomes of the two stable regimes? I examine a panel of annual data from more than 170 countries from 2007 through 2012 and find that the macroeconomic and financial consequences of regime choice are surprisingly small. Consistent with the literature, business cycles, capital flows, and other phenomena for hard fixers have been similar to those for inflation targeters during the global financial crisis and its aftermath.

1. Introduction

The global financial crisis (hereafter "crisis") of 2007–09 began and was felt most keenly in the rich Northern countries. Nevertheless, much of its effect was felt abroad: The Great Recession of 2008–09 was a global affair. Small economies were indirectly affected as the shock waves spilled out from New York and London, most dramatically in the form of contractions in the international flow of capital and trade. My interest in this paper is comparing how the outcomes for small economies varied by their choice of monetary regime.

Author's note: *This is a shorter version of NBER Working Paper 19632; the data set, key output, and a longer version of this paper are freely available at my website, http://faculty.haas.berkeley.edu/arose/. For help and comments, I thank Joshua Aizenman, Woon Gyu Choi, Joe Gagnon, Reuven Glick, Mojmir Hampl, Michael Hutchison, Anil Kashyap, Frederic Mishkin, Ashoka Mody, John Murray, Jonathan Ostry, Carmen Reinhart, Mark Spiegel, Alan Taylor, and conference participants at the 2013 Asia Economic Policy Conference. I thank Kristin Forbes and Frank Warnock for providing their data set. I also thank the National University of Singapore for hospitality during the course of this research.*

I am particularly interested in contrasting two monetary regimes: hard fixed exchange rates and floating exchange rates with an inflation target. Both are well-defined monetary regimes widely used by small economies around the world. The two regimes are also quite different, potentially providing a sharp contrast. The question I raise is, Did one monetary regime provide more insulation from the crisis than the other?

The Great Recession associated with the crisis was the most dramatic macroeconomic event in generations; as Imbs (2010) convincingly demonstrates, it was also the first truly global recession in decades. Historically, recessions have frequently caused monetary upheaval; change in monetary regime has been strongly countercyclical. Yet this time was different, at least for the two monetary regimes of concern here. Most countries with hard fixed exchange rates in 2006 (before the onset of the crisis) still retained them in 2012. More striking though was the performance of inflation targeters. While the tactics of flexible inflation targeting regimes have varied with quantitative easing and forward guidance, the fundamental monetary strategy has not: No country abandoned inflation targeting.[1]

Interest in academic studies of currency crises (typically when a fixed exchange rate is abandoned) has greatly diminished over the past 15 years. A number of small economies whose experiences spawned important academic research are now sufficiently stable as to be boring, including Brazil, Chile, Korea, Mexico, Sweden, Thailand, and Turkey. The common element in the transition to stability is the adoption of a floating exchange rate monetary regime with an inflation target. While before 2007 there were legitimate questions about the durability of inflation targeting, it has now withstood a substantial trial by fire.[2] Between the hard fixers and inflation targeters, most of the international monetary system has withstood the pressures of the crisis and its aftermath in at least one critical aspect: It has preserved itself.

My analysis in this paper is broad in the sense that I analyze a number of macroeconomic phenomena for more than 170 small economies. My focus is also narrow: I am most interested in the period since 2006, and I am interested in the effects of the *monetary* regime, primarily the way international capital flows were handled.[3] My quantification of the monetary regime relies on a comprehensive classification of de facto behavior, gathered by the International Monetary Fund (IMF).

I have two major results. First, monetary regimes have remained stable and unchanged during the crisis and its aftermath for a large number of countries that were hard fixers and inflation targeters. The recent finding of monetary stability contrasts with earlier periods; historically, countries have

switched their regimes countercyclically, that is, especially during recessions. Since small economies now have two reasonably stable monetary regime options that appear to be starkly different, it is natural to ask which has performed better, especially during the turbulent period since 2006. In practice this question is hard to answer: While both hard fix and inflation targeting countries have experienced, for instance, lower inflation than other countries, the behavior of business cycles, capital flows, current accounts, government budgets, real exchange rates, and asset prices do not seem to vary significantly between the two regimes. Thus my second major result is that the recent macroeconomic and financial performance of small countries with hard fixed exchange rates is similar to that of countries that float with an inflation target. At first blush, this seems surprising, since a hard commitment to a fixed exchange rate seems quite different from the constrained discretion of an inflation target. However, the result is actually quite consistent with the literature, which has generally been unable to find strong consequences of inflation targeting regimes, except for exchange rate volatility.

2. A Broad Data Set on the Monetary Regime

One of my goals in this work is to be as comprehensive as possible. I begin with the entire sample of countries available in the World Bank's *World Development Indicators* (WDI). In all, I have at least some data for 214 countries, though there are many gaps.[4] However, the focus of this study is on small economies; accordingly, for much of the following analysis I define small as "not large" and simply remove all large economies.[5] Adopting the taxonomy of the IMF's *Spillover Report*, I exclude from the sample the five systemically important economies of China, the euro zone, Japan, the United Kingdom, and the United States.[6]

One key variable of interest missing from the WDI is the national monetary regime. In the past, researchers have resorted to using the formal de jure exchange rate regime as declared by the national monetary authorities. This information was provided in the IMF's *Annual Report on Exchange Arrangements and Exchange Restrictions* (AREAER), and was thus available for all members of the Fund. It is now widely accepted that de facto measures of what national authorities actually do are of greater relevance; Rose (2011) provides more details. Two of the most popular de facto classifications are those of Levy-Yeyati and Sturzenegger (2003, hereafter LYS), and Reinhart and Rogoff (2004, hereafter RR). One issue with both classifications is the limited span of the data set. RR has now been extended through 2010, giving one year of data for the aftermath of the Great Recession; LYS has only been extended

through 2004. In any case, there is a more serious problem; both LYS and RR are *exchange rate regime* rather than *monetary regime* classifications. While a fixed exchange rate constitutes a well-defined monetary policy, a float does not. If the central bank doesn't fix the exchange rate, it has to do something else, but what?

This problem has long been recognized, and can be solved by classifying countries by their monetary regime. Stone and Bhundia (2004, hereafter SB) propose a taxonomy of monetary regimes by the choice and clarity of the nominal anchor. I use their classification below; it covers 85 countries, though unfortunately only from 1990 through 2005.

To augment this, I need some way to classify countries by monetary regime in the aftermath of the crisis. To its credit, the IMF long ago switched to a de facto classification of monetary regimes in AREAER. The Fund provides an official series available back through 2001 for each of its members; I use this classification through 2012.[7] The IMF divides country-years into an exhaustive taxonomy with 44 cells that vary by exchange rate rigidity, the orientation of the fix (most countries peg their currency to either the dollar or the euro), and the objective of floating rate regimes (countries target either inflation or a monetary aggregate, though some also use other frameworks). I use this monetary classification extensively below; for sensitivity and historical analysis, I also employ the LYS, RR, and SB schemes.

3. Monetary Regimes During and After the Global Financial Crisis

Monetary regimes have remained remarkably stable from the run-up to the crisis through its aftermath. This paper focuses on the experiences of small economies during this period. I contend that the monetary regimes of small economies —like those of large economies—have exhibited stability since before the crisis. This stability is new and contrasts with the historical countercyclicality of monetary regime switches.[8] It also is remarkable compared with the size of recent macroeconomic and financial shocks.[9]

Table 1 groups the small economies by monetary regimes in 2006 (the calm immediately before the beginning of the financial crisis in 2007–08) and 2012 (the most recent period available for most data). I focus on two extreme monetary regimes of particular interest. In 2006, 26 countries were classified by the IMF as floating exchange rates with an inflation target; only one country had switched regimes by 2012, that is, Slovakia left to join the European Monetary Union (EMU).[10] Clearly, inflation targeting has shown its resilience through a trying period of macroeconomic turmoil; it is manifestly a durable monetary regime. No country has ever dropped out of an inflation targeting regime under

TABLE 1

Small Economies by Type of Monetary Regime, 2006–12

A. Countries with Inflation Targeting

Continuous, 2006–12 (25)

Armenia	Australia	Brazil	Canada	Chile
Colombia	Czech Republic	Guatemala	Hungary	Iceland
Indonesia	Israel	Korea, Republic	Mexico	New Zealand
Norway	Peru	Philippines	Poland	Romania
South Africa	Sweden	Switzerland	Thailand	Turkey

Targeted in 2006, exited by 2012 (1), by country (type of regime)

Slovak Republic (euro)

B. Countries with Hard Fixed Exchange Rate

Continuous between 2006 and 2012 (60)

Antigua/Barbuda	Aruba	Bahamas	Bahrain	Barbados
Belize	Benin	Bhutan	Bosnia/ Herzegovina	Brunei
Bulgaria	Burkina Faso	Cameroon	Cape Verde	Central African Rep.
Chad	Comoros	Congo, Rep.	Côte d'Ivoire	Denmark
Djibouti	Dominica	Ecuador	El Salvador	Equatorial Guinea
Eritrea	Fiji	Gabon	Grenada	Guinea-Bissau
Hong Kong	Jordan	Kiribati	Latvia	Lesotho
Libya	Lithuania	Mali	Marshall Islands	Micronesia
Montenegro	Morocco	Namibia	Nepal	Niger
Oman	Palau	Panama	Qatar	Samoa
San Marino	Saudi Arabia	Senegal	St. Kitts/Nevis	St. Lucia
St. Vincent/ Grenadines	Swaziland	Timor-Leste	Togo	United Arab Emirates

Both 2006 and 2012, but not continuous (3)

Kuwait	Turkmenistan	Venezuela

Fixed in 2006, exited by 2012 (20), by country (type of regime)

Azerbaijan (float, other)	Belarus (float, other)	Cyprus (euro)
Egypt (float, other)	Estonia (euro)	Honduras (soft fix)
Lebanon (soft fix)	Macedonia (soft fix)	Maldives (soft fix)
Malta (euro)	Mauritania (float, other)	Pakistan (float, mon. target)
Seychelles (float, mon. target)	Slovenia (euro)	Solomon Islands (float, other)
Syria (soft fix)	Trinidad/Tobago (soft fix)	Ukraine (float, mon. target)
Vanuatu (soft fix)	Vietnam (soft fix)	

stress; the only exiters have adopted the euro. This holds true using classifications other than the IMF's. Mishkin (2008) lists five components of inflation targeting: a medium-term numerical target for inflation; an institutional commitment to price stability as a primary goal of monetary policy; an information-inclusive strategy to set instruments; central bank transparency; and central bank accountability. Mishkin's criteria lead to the same conclusion.

TABLE 1 (CONTINUED)
Small Economies by Type of Monetary Regime, 2006–12

C. Countries with Various Other Regimes: Sloppy Center

Continuous between 2006 and 2012 (32)

Afghanistan	Algeria*	Botswana	Burundi*	Cambodia*
Congo, Dem. Rep.*	Costa Rica*	Gambia, The	Guinea*	Haiti*
India	Iraq*	Jamaica*	Kenya	Kyrgyz Rep.*
Lao PDR*	Liberia*	Madagascar	Malaysia	Mauritius*
Mozambique	Myanmar*	Nicaragua	Papua New Guinea*	Paraguay*
Singapore*	Somalia	Sudan*	Tanzania	Tonga
Uganda	Zambia			

Both 2006 and 2012, but not continuous (21)

Angola	Argentina	Bangladesh	Bolivia	Croatia
Ethiopia	Guyana	Iran	Kazakhstan	Malawi
Mongolia	Nigeria	Russia	Rwanda	Sierra Leone
Sri Lanka	Suriname	Tajikistan	Tunisia	Uzbekistan
Yemen				

Sloppy Center in 2006, exited by 2012 (9), by country (type of regime)

Albania (inflation target)	Dominican Republic (inflation target)	Georgia (inflation target)
Ghana (inflation target)	Moldova (inflation target)	Sao Tome/Principe (hard fix)
Serbia (inflation target)	Uruguay (inflation target)	Zimbabwe (hard fix)

*Indicates switched IMF de facto monetary regime between 2006 and 2012.

By way of contrast, 83 small economies maintained a hard fix in 2006. I define a hard fixer as a monetary regime with either (a) no separate legal tender, (b) a currency board arrangement, or (c) a conventional peg.[11] Of these hard fixers, 60 were maintained continuously through the end of the sample in 2012 and thus proved to be durable; I address these in more detail later.[12] The 2012 monetary regimes for the other 23 countries are listed in Table 1; a number left to join the euro, but most switched to less rigorous monetary regimes.[13]

The last group of countries collectively maintains a variety of other monetary regimes. These include (a) soft fixers (the IMF lists a number of variants such as stabilized arrangement, crawling peg, crawling band, and pegged exchange rate within horizontal bounds); (b) floating with a monetary target (variants include crawl-like, managed, or free floats); and (c) floating with an "other" framework (like their large counterparts the EMU, Japan, and the United States). I will refer to these other regimes collectively as the "sloppy center." Between 2006 and 2012, 32 countries were in this category; of the countries that began in the sloppy center, 30 had switched out at least once by 2012. Even this overstates the degree of stability in the sloppy center, since it is a coarse, ill-defined grouping, containing dozens of finer IMF de facto monetary

regimes. Seventeen of the 32 countries that remained in the sloppy center throughout switched among variant monetary regimes between 2006 and 2012.

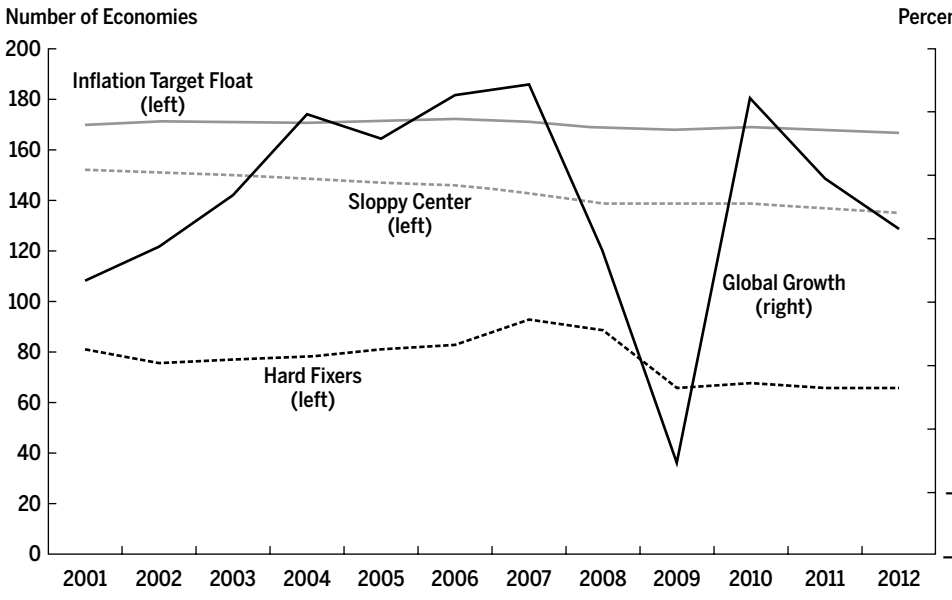
Simply counting countries understates the stability of monetary regimes through this period of time (see Figure 1). While the number of inflation targeters is considerably smaller than those in other regimes, they are, on average, larger and richer. In 2011, inflation targeting countries represented some 20 percent of global output.[14] By way of contrast, the more numerous stable fixers are small, poor, or both; they represent only 4 percent of 2011 global GDP, while the sloppy center constitutes some 7 percent of the world's output. This is clearly visible in Figure 2, which portrays the fraction of global GDP in each of these three monetary regimes. It is striking that inflation targeting regimes make up such a large and stable fraction of global GDP, even through the crisis and its aftermath.

Table 2 summarizes the stability of the monetary regimes for small countries between 2006 and 2012. Of the countries that targeted inflation in 2006, 96 percent were still doing so in 2012; almost three-quarters of the hard fixers also survived. By way of contrast, less than a quarter of the sloppy center maintained the same monetary regime during the crisis and its aftermath.

FIGURE 1

Monetary Regimes by the Numbers: Counting Countries

Grouping Small Economies by Monetary Regime



Note: Large economies excluded: China, EMU, Japan, United Kingdom, and United States.

FIGURE 2

Monetary Regimes by the Numbers: Sizing Up the Economies

Global GDP of Small Economies by Monetary Regime

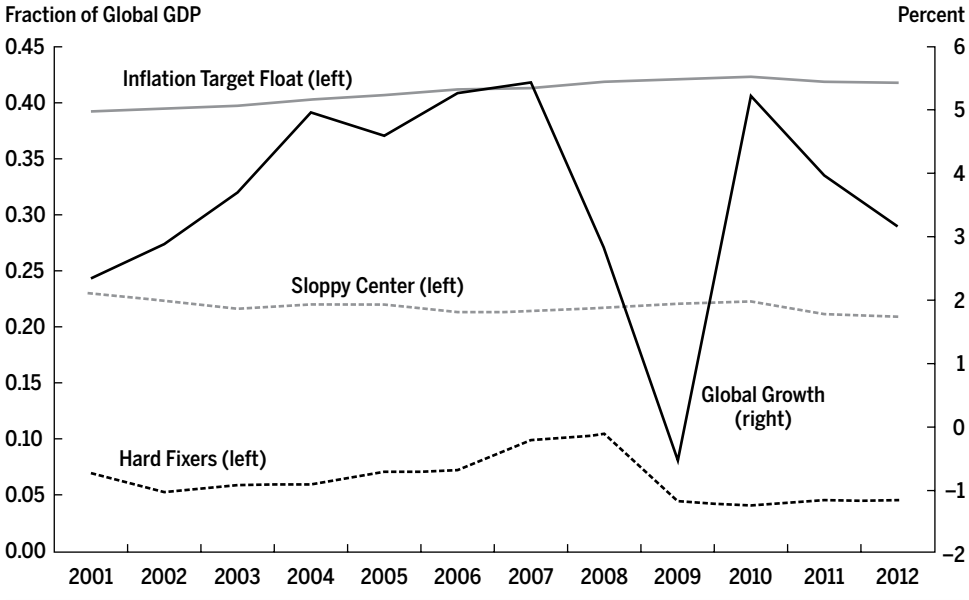


TABLE 2

Durability of Monetary Regimes, Small Economies

	Inflation targeting	Hard fix	Sloppy center
Monetary regime in 2006	(26)	(83)	(62)
Monetary regime in 2012			
Inflation targeting	25	0	7
Hard fix	0	63[a]	2
Sloppy center	0	16	53[b]
EMU entrants	1	4	0
Percent continuously in same regime since 2006	96%	72%	23%
Percent of 2011 global GDP	20%	4%	7%

Note: Excludes China, European Monetary Union, Japan, United Kingdom, and United States.

a Three of these countries both started and ended in hard fixes but strayed between 2006 and 2012. The Netherlands Antilles exited the sample upon its split.

b Of these countries, 21 both started and ended in the sloppy center but strayed between 2006 and 2012. Of the 32 countries that remained continuously in the sloppy center, 18 changed their de facto IMF monetary arrangement, leaving only 14 in the same monetary regime throughout.

4. The Countercyclical Nature of Monetary Regime Shifts: Historical Evidence

Monetary regimes for many economies, both large and small, have been stable through the crisis and its aftermath. This stability is a relatively new phenomenon. Historically, turnover in monetary regimes has been frequent during recessions. The monetary turmoil during the Great Depression of the 1930s is well known and helped motivate the creation of the postwar Bretton Woods system.[15] Still, it is hard to quantify this cyclicality because there are no long-lived measures of monetary regimes. The SB classification only goes back to 1990. Both LYS and RR go further back in time, but they classify *exchange rate* rather than *monetary* regimes.[16]

Table 3 presents historical data analogous to Table 2, but for two important historical episodes: the global slowdowns of the early 1970s and the early 1980s. Where Table 2 compares *monetary* regimes six years apart (2006–12), Table 3 compares *exchange* rate regimes six years apart (for 1970–76 and 1980–86); both tables exclude large economies, although the definition of these countries changes somewhat over the two periods. Just over half of small economies remained in the same exchange rate regime continuously between 1970 and 1976 according to RR, though these constitute only one-eighth of global GDP.[17] Since all of the large economies switched their exchange rate regimes during this period, the 1970s are appropriately remembered as a period of international monetary turbulence. Small economies experienced more stability in the 1980s according to the RR classification, with over one-quarter of global GDP being produced in small economies with stable monetary regimes. However, the LYS analog is lower by a factor of three. Both schemes agree that at least a quarter of fixed exchange rate regimes failed and that the stable fixers produce little global output.

TABLE 3
Durability of Monetary Regimes, Small Economies: Historical Evidence

	All countries	Fixers
Percent continuously in same Reinhart-Rogoff regime, 1970–76	55%	59%
Percent of 1976 global GDP	12.3%	6.4%
Percent continuously in same Reinhart-Rogoff regime, 1980–86	60%	75%
Percent of 1986 global GDP	28.4%	3.3%
Percent continuously in same Levy-Yeyati-Sturzenegger regime, 1980–86	53%	58%
Percent of 1986 global GDP	9.1%	9.1%

Note: Excludes Germany, Japan, United Kingdom, and United States.

I conclude from this analysis that monetary regime transitions have historically been countercyclical. The stability of national monetary regimes is not only a recent phenomenon; it is unexpected given the size of the Great Recession.

5. Effects of Monetary Regimes

5.1. Statistical Evidence

I next examine the recent consequences of monetary regimes for small economies. One might reasonably expect floating with an inflation target to be diametrically opposed to a durable hard fix, especially for handling the shock waves that spilled out from the large economies as a consequence of the crisis. How did actual performance under the two regimes differ?

For comparison, I split my sample into three groups: (a) inflation targeters such as Brazil, Korea, Mexico, and Canada; (b) the 60 small economies that maintained hard fixes continuously from 2006 to 2012, including Saudi Arabia, Hong Kong, and Denmark; and (c) the observations from the remaining sloppy center, including India, Russia, and Iran.[18] The first two of these monetary regimes are long-lived and durable, often predating the crisis significantly. It is difficult to find systematic determinants of the monetary regime. Beyond the persistent effects of size and democracy, monetary regimes seem to be almost random. Accordingly, in this analysis I initially treat the choice of monetary regime as plausibly exogenous to estimate the effects of the monetary regime on outcomes of interest without further econometric hassles. I address two questions: Should we care about which monetary regime a small country chooses? And has the monetary regime made a substantial difference to the macro-economies of small economies in the period since the onset of the crisis?

Tables 4 and 5 contain regression evidence for 2007–12. Each row presents results from a panel regression of the regressand (first column) on dummy variables for both inflation targeting and hard fix regimes. The omitted regime is the sloppy center so that the coefficients in the inflation targeting column represent the difference between inflation targeters and the sloppy center. Test results for two hypotheses of interest are tabulated at the right: (a) the hypothesis that the hard fix and inflation targeting regimes have the same effect compared with the sloppy center, and (b) that the two regimes have no effect. The equations are estimated via least squares with fixed time- and random country-specific effects.[19] While there is little reason to believe that hard fix and inflation targeting regimes are chosen endogenously because of their relevance to the variables of interest, I address this issue more directly in the longer version of this paper with two more sophisticated econometric techniques.

TABLE 4
Effects of Monetary Regimes 2007–12: Regression Evidence

Regressand	Inflation Targeting	Hard Fix	IT = H Fix? (P-value)	IT = H Fix = 0? (P-value)
BK-filtered GDP	0.006	–0.003	0.04*	0.10
	(0.004)	(0.004)		
HP-filtered GDP	–0.002	–0.004*	0.13	0.04*
	(0.001)	(0.001)		
CF-filtered GDP	–0.02	0.00	0.77	0.76
	(0.02)	(0.04)		
Demeaned growth	–1.9*	–1.4	0.56	0.04*
	(0.8)	(0.8)		
Time-detrended GDP	–0.04	–0.08**	0.16	0.01**
	(0.03)	(0.02)		
Gross capital inflows	3.2	–4.1	0.90	0.57
	(3.2)	(6.4)		
Gross capital outflows	–0.0	–3.2	0.61	0.87
	(3.2)	(6.7)		
Net capital flows	3.2	0.8	0.03*	0.09
	(1.9)	(1.6)		
Std dev capital inflows (c/s)	5.5	5.5	1.0	0.38
	(4.2)	(6.9)		
Std dev capital outflows (c/s)	5.1	7.0	0.82	0.36
	(4.2)	(7.4)		
Current account	1.6	3.4	0.73	0.49
	(1.4)	(5.5)		
Export growth	0.01	0.00	0.70	0.85
	(0.01)	(0.01)		
Import growth	–0.00	0.00	0.76	0.94
	(0.01)	(0.01)		
Chinn-Ito capital mobility	–0.1	–0.5	0.41	0.24
	(0.4)	(0.3)		
Financial freedom change	0.01	0.00	0.16	0.16
	(0.01)	(0.01)		
Investment freedom change	0.03**	0.01	0.01**	0.01**
	(0.01)	(0.01)		
M2 growth (% GDP)	–0.01	0.00	0.18	0.41
	(0.01)	(0.01)		
International reserve growth	–0.4	–0.5	0.26	0.44
	(0.4)	(0.4)		
Government budget	0.3	0.7	0.70	0.74
	(0.8)	(0.9)		
Change in budget	0.5	–0.4	0.30	0.57
	(0.7)	(0.5)		

Notes: Coefficients displayed for monetary regime dummy variables on regressand; default regime is sloppy center. Standard errors in parentheses; coefficients significantly different from zero at 0.05 (0.01) marked with one (two) asterisk(s). Each row estimated by panel least squares estimation with fixed time and random country effects (except for cross-sections). Annual data span 2007–12, 167 countries (with gaps).

Using a number of variables, I examine a range of consequences of capital flows from large economies. I look at output consequences, the capital flows themselves, and the mechanisms through which a small economy can adjust to capital flows.

TABLE 5
Effects of Monetary Regimes 2007–12: Regression Evidence

Regressand	Inflation Targeting	Hard Fix	IT = H Fix? (P-value)	IT = H Fix = 0? (P-value)
CPI inflation	–4.4**	–5.2**	0.15	0.00**
	(0.7)	(0.6)		
GDP inflation	–4.7**	–5.2**	0.41	0.00**
	(0.8)	(0.7)		
Real effective exchange rate	–15.0	–20.1*	0.13	0.05*
	(9.8)	(9.6)		
Change in real effective exchange rate	–3.9	–5.4	0.06	0.07
	(3.4)	(3.5)		
Growth in stock prices	–4.5	–11.8**	0.01**	0.00**
	(3.5)	(3.3)		
Bond yields	–1.0	–1.0	0.96	0.43
	(0.8)	(1.0)		
Growth in property prices	2.3	–1.1	0.35	0.63
	(4.8)	(5.1)		

Notes: Coefficients displayed for monetary regime dummy variables on regressand; default regime is sloppy center. Standard errors in parentheses; coefficients significantly different from zero at 0.05 (0.01) marked with one (two) asterisk(s). Each row estimated by panel least squares estimation with fixed time and random country effects (except for cross-sections). Annual data span 2007–12, 78 countries (with gaps).

The top of Table 4 lists business cycle effects as measured by real GDP detrended in the five ways discussed earlier; this is one of the most important consequences of policy choice. Since this paper is concerned with the effects of monetary regimes on small economies through the tumultuous period of the crisis, it is also important to examine capital flows.[20] I use the series constructed by Forbes and Warnock (2012) to examine gross capital inflows and outflows, as well as net capital flows.[21] Since the volatility of capital flows is of interest, I also construct the country-specific standard deviation of both inflows and outflows (over time) to examine the effect of monetary regimes on the cross-country variation of capital flows.[22]

When capital starts to flow into a small economy, it can be handled in a variety of different ways.[23] These include (a) encouraging an offsetting change in the current account; (b) restricting capital inflows or promoting outflows; (c) accumulating reserves, possibly implying an increase in the money supply; (d) fiscal contraction; or (e) real exchange rate appreciation. Real appreciation, in turn, can be achieved via nominal exchange rate if the latter is flexible, or inflation induced by a monetary expansion.[24] Since I am interested in how economies in different monetary regimes have reacted to the capital flows since 2007, I examine measures for each of these channels. I include the current account and the growth of exports and imports (all relative to GDP). Capital inflows can be countered by capital controls, so I look at the Chinn-Ito measure of capital

mobility as well as measures of financial and investment freedom taken from the *Economic Freedom of the World* data set. Near the bottom of the table, I also look at different measures of policy: the growth of international reserves and broad money, the government's budget position relative to GDP, and how the budget has changed. Table 5 is analogous to Table 4, but examines prices. I include two conventional measures of goods and services domestic inflation—consumer price inflation (CPI) and GDP inflation—as well as the real effective exchange rate and its change. The effect of the monetary regime on asset prices is the subject of much recent debate. Accordingly, I examine three important assets: the yield on long-term bonds and growth in both stock and property prices.[25] Jointly, these variables cover a wide range of potential responses to international capital inflows.

What do the data show about the consequences of monetary regime choice? Very little. Perhaps most importantly, Table 4 shows that the magnitude of the business cycle does not seem to have varied significantly between inflation targeters and hard fixers over the period since 2007; there is weak evidence that countries in both regimes suffered somewhat worse than the sloppy center. I do not dwell on this result since it does not stand up to further econometric scrutiny, as I show below. This weak result is consistent with the fact that capital flows and their volatility seem not to vary across monetary regimes; the exception is that inflation targeting regimes received larger net capital flows. Neither the current account nor the growth of either exports or imports varies consistently with the monetary regime. Inflation targeting regimes increased the ability of their residents to invest freely, but the other two measures of capital mobility show no significant differences across regimes. Perhaps most strikingly, there are also no significant differences across regimes in the growth of international reserves, the money supply, or broad measures of fiscal policy.

It turns out that the weak results in Table 4 do not stem from the methodology or the fact that the data set is limited to six annual observations (admittedly for up to 167 countries). As Table 4 shows, both CPI and GDP inflation are about 5 percent lower for *both* hard fixers and inflation targeters compared with the sloppy center, an economically and statistically significant result.[26] Since one of the chief tasks of a monetary regime is to deliver low inflation, this is an important and comforting result. Interestingly, both the level and the rate of change in real exchange rates over this period are lower (more depreciated) for *both* hard fixers and inflation targeters compared with the sloppy center, though these results are only on the verge of statistical significance. Stock prices have fallen more for hard fixers than the sloppy center. Growth rates for property prices and bond yields are insignificantly different across monetary regimes.

5.2. The Visual Story

A visual version of the weak results from Tables 4 and 5 is presented in Figures 3 to 5. These are quantile plots, which compare the distribution of some of the most important variables from Tables 4 and 5 for hard fixers and inflation-targeters.[27] In Figure 3, panel A graphs the quantiles of real GDP growth for fixers since 2007 (on the vertical axis) to growth over the same period for inflation targeters on the horizontal axis). For reference, a diagonal line shows where the data would be plotted if growth were distributed similarly across the two regimes. With the exception of a few outliers at both ends of the distribution, most of the data are scattered close to the diagonal line, consistent with the notion that growth for most hard fixers is similar to that for inflation targeters (though hard fixers experience more outliers, both positive and negative). The pattern for CPI inflation (panel B) and the government budget (panel D) are similar, while the distribution of current accounts (panel C) is more extreme for hard fixers. In general though, the distributions for key variables in Figure 4 seem similar across monetary regimes for capital inflows (panel A) and outflows

FIGURE 3
Key Differences across Monetary Regimes
Quantile Plots for Small Economies, 2007–12

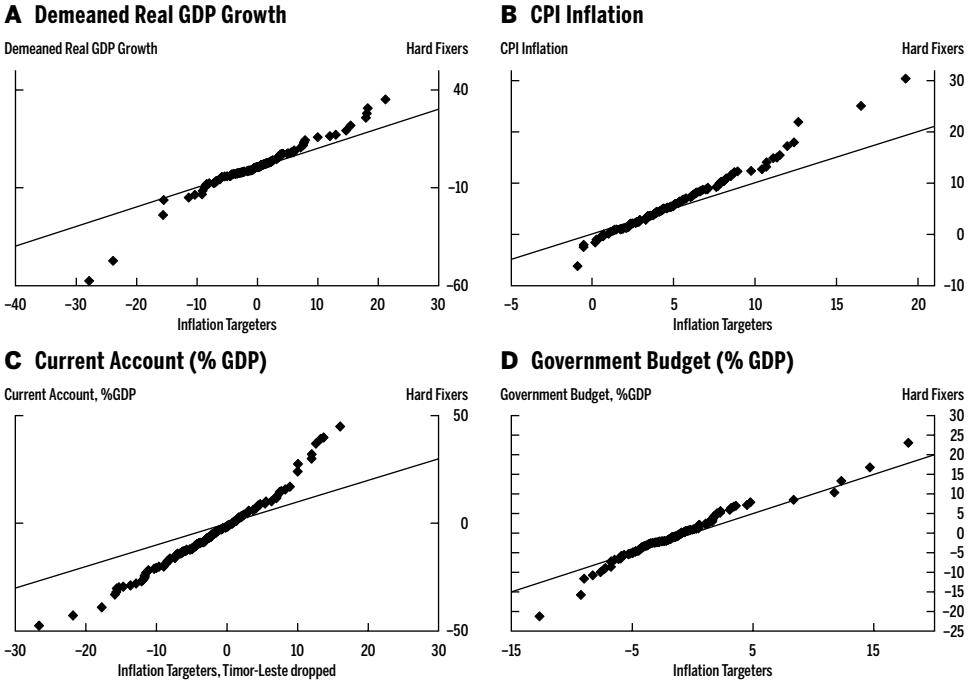
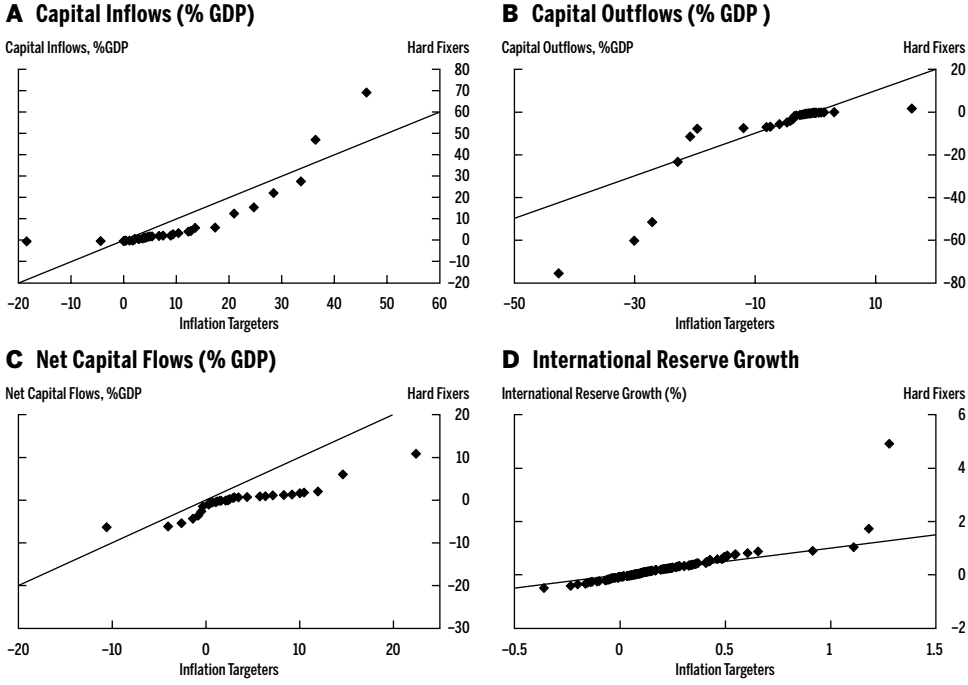


FIGURE 4

Key Differences across Monetary Regimes

Quantile Plots for Small Economies, 2007–12



(panel B), and international reserve growth (panel D), and in Figure 5 for the change in the real effective exchange rate (panel A), and asset price changes (panels B, C, and D). One exception is net capital flows, shown in Figure 4, panel C, which are systematically higher for inflation targeters.

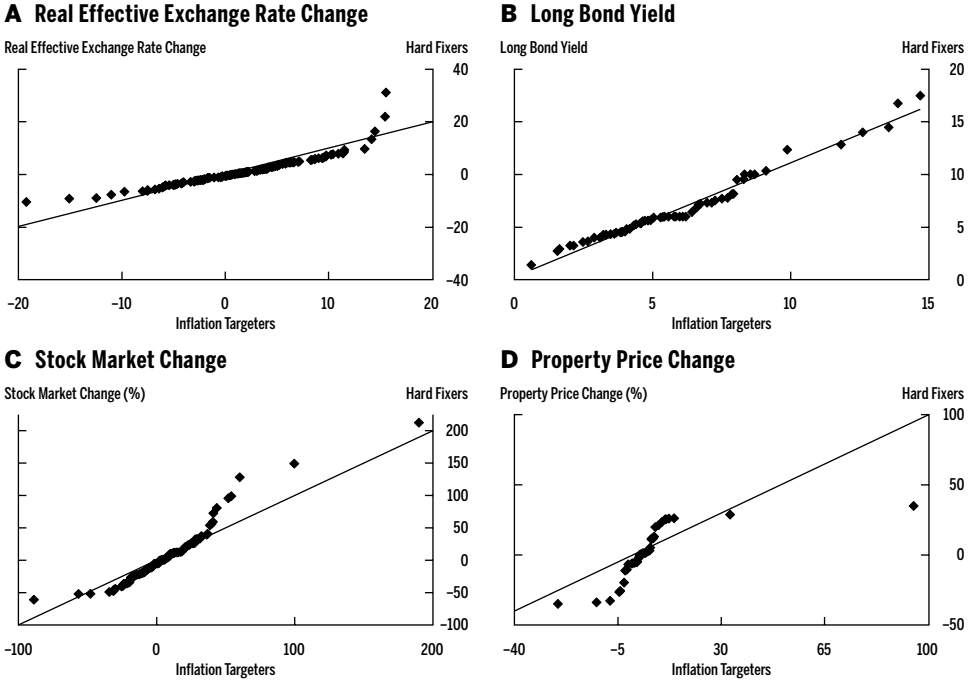
5.3. Summary

There is rarely any significant difference between the effects of inflation targeting and hard fixes for the variables I examine. Three exceptions are net capital flows, the change in investment freedom, and stock prices. This is a striking result that essentially runs throughout the statistical analysis. Initially it seems implausible; after all, these monetary regimes differ radically. Hard fixers have severely limited monetary autonomy, while inflation targeters are not directly constrained by the exchange rate. Hard fixers with open capital markets would seem to have substantially less ability than inflation targeters to insulate themselves from the spillover effects of foreign capital flows. Still, this finding is actually quite consistent with the literature, which has been unable to find strong

FIGURE 5

Key Differences across Monetary Regimes

Quantile Plots for Small Economies, 2007–12



effects from the monetary (or, more commonly, the exchange rate) regime. Rey (2013, pp. 19–20) recently wrote (italics added for emphasis):

> Analyses suggest monetary conditions are transmitted from the main financial centre to the rest of the world through gross credit flows and leverage, *irrespective of the exchange rate regime*. This puts the traditional "trilemma" view of the open economy into question. *Fluctuating exchange rates cannot insulate economies from the global financial cycle*, when capital is mobile.[28]

The notion that the monetary regime matters surprisingly little is not new; see, for example, the recent book by Klein and Shambaugh (2010) and references therein.[29] The abstract of Baxter and Stockman (1989) concludes, "Aside from greater variability of real exchange rates under flexible than under pegged nominal exchange-rate systems, we find little evidence of systematic differences in the behavior of macroeconomic aggregates or international trade flows under alternative exchange-rate systems."[30] While an absence of any large detectable

differences across monetary regimes might seem bizarre to a monetary economist, it is almost folk wisdom inside international finance.

To summarize, small economies that float with an inflation target have, in many respects, behaved similarly to hard fixers over the post-bubble period. This might be an artifact of the econometric methodology I have employed or the size of the data set. But that seems unlikely. The literature has been unable to find many significant differences across monetary regimes; perhaps there simply are few. It seems that the trade-offs between hard fixers and inflation targeters lie more in the operation of monetary policy than in their manifestations in real economic outcomes.

I conclude that small economies interested in stable monetary regimes now have a real alternative to a hard fix. Floating with an inflation target seems to have few quantifiable macroeconomic or financial trade-offs for small economies compared with a hard fix, and it is at least as durable.[31]

6. Conclusion

Bulgaria is a small open emerging market, with membership in the European Union, reasonable and improving institutions, and GDP per capita of around $12,000. Its neighbor Romania is roughly comparable in size, income, institutions, and openness. Bulgaria prides itself on having rigorously maintained a fixed nominal exchange rate since 1997 through its currency board arrangement. Romania, on the other hand, has operated an inflation targeting regime with a flexible exchange rate since 2005. Manifestly, similar economies choose different approaches to monetary policy. Denmark has stayed fixed to the euro (earlier, the deutsche mark) at the same rate since 1987; Sweden has changed its regime a number of times since then, and installed an inflation targeting regime with a flexible exchange rate in 1993. Yet Denmark and Sweden are broadly comparable in size, income, institutions, and openness. The examples are legion: Ecuador, El Salvador, Côte d'Ivoire, and Bosnia-Herzegovina are hard fixers while their neighbors Colombia, Guatemala, Ghana, and Albania are similar in many respects but target inflation. Roughly similar countries are happy to maintain radically different monetary regimes. In this paper, I have found that this decision has been of little consequence for a variety of economic phenomena, at least lately. Growth, the output gap, inflation, and a host of other phenomena have been similar for hard fixers and inflation targeters during and since the global financial crisis. That is, the "insulation value" of apparently different monetary regimes is similar in practice. Since the international finance literature has found few substantive macroeconomic differences across monetary regimes, I expect this result to be banal for some. Since this stylized fact

is not well known outside international economics, I expect it to seem implausible to others.

For small economies interested in monetary stability, there are now two options: a hard fixed exchange rate and inflation targeting. The alternative to the rigors of a hard fix used to be limited, essentially consisting of muddling along in a sloppy center of crawling bands, adjustable pegs, monetary targets, and considerable discretion. But two monetary regimes have withstood the rigors of the global financial crisis and its aftermath. The fact that the constrained discretion of inflation targeting poses no quantifiable trade-off vis-à-vis a hard fix is a theoretical puzzle, but it is quite consistent with the literature.

It is natural to think that a big shock—like the global financial crisis and the Great Recession—will put the system to the test and reveal which is the best monetary regime. We've now had the big shock and it appears that now—as opposed to during the Great Depression or the early 1970s—the current system can indeed survive a serious crisis. That said, the shock has not provided any clear guidance as to which monetary regime is preferable for small economies. One caveat is that I've only examined one shock, even if it was a monster. More importantly though, the experiences of countries in hard fixes during and after the crisis have been similar to those of inflation targeters.

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NOTES

1 Except to enter the European Monetary Union; I discuss that caveat below.

2 This is consistent with much of the analysis in Reichlin and Baldwin (2013) who agree with Charles Wyplosz, that flexible inflation targeting has survived the test of a major financial crisis well.

3 As Svensson (2010) argues, "financial-stability policy and monetary policy are quite different, with different objectives, instruments, and responsible authorities, the latter with considerable differences across countries. This does not mean that there is no interaction between them."

4 For the purposes of this paper, I use "country" interchangeably with the more precise and appropriate term "economy"; some of the economies in my sample are territories, colonies, or special administrative regions without full political sovereignty.

5 The fact that I can do so has much to do with my Canadian identity.

6 I exclude the countries inside the EMU from my analysis as they are parts of a large economy, so this work has essentially nothing to say about the euro crisis.

7 AREAER is published by the Fund in the autumn.

8 An easy comparison is provided by the 1990–2005 Stone-Bhundia data set, in which approximately 10 percent of monetary regimes change each year.

9 My argument is consistent with the contention I made some years ago that much of the new stability in the international monetary system derives from the emergence of inflation targeting. In Rose (2007), I described the emerging "Bretton Woods reversed" system, driven primarily by inflation targeting administered by independent and transparent central banks. These countries place few restrictions on capital mobility and allow their exchange rates to float. This system was not planned and does not rely on international coordination. In 2007 I argued that there was no role for an anchor country (a claim I would now weaken, given the success of the swap lines provided by the Federal Reserve), the IMF, or gold. Succinctly, it is the diametric opposite of the postwar system; Bretton Woods, reversed. My central claim concerned the durability of the system; in contrast to other monetary regimes, no country has been forced to abandon an inflation targeting regime. The crisis has now provided the experiment to put Bretton Woods reversed to the test, and the system has proved, at least thus far, resilient.

10 While the IMF classifies Slovakia otherwise, I follow conventional wisdom and classify the national bank of Slovakia as an inflation targeter in 2006; http://www.nbs.sk/_img/Documents/MPOL/mprog/2008a.pdf

11 The inclusion of countries that the IMF classifies as "conventional peg" may raise the eyebrow here. Examples of these countries include Caribbean peggers (Aruba, Bahamas, Barbados, and Belize), Euro peggers (Denmark and Latvia), Gulf peggers (Bahrain and Saudi Arabia), the CFA franc zone (Benin and Burkina Faso), and South African peggers (Lesotho and Namibia). The vast majority of these pegs were in fact quite hard, making it inappropriate to place them in another bin.

12 Of the hard fixers in 2012, most had been hard fixers for many years. It is hard to be definitive, since there is currently no continuous measure of the de facto monetary regime available historically, as discussed earlier.

13 Much of this analysis compares the features of the 60 durable hard fixers with the inflation targeters.

14 The last year for which I have a broad sample of comparable real GDP data is 2011.

15 Eichengreen and Sachs (1985) is one important paper in a large literature.

16 Masson and Ruge-Murcia (2005) analyze the determinants of exchange rate regime transitions. Note also that the considerable literature on choice of exchange rate regime (as opposed to transitions between regimes), rarely focuses on business cycle events; see, for example, Poirson (2001).

17 The LYS classification only begins in 1974 and is hence unusable for this purpose.

18 Since some countries in the last group are in hard fixes for some of the period (and thus not in a sloppy center monetary regime each year), I use this taxonomy at the risk of some confusion.

19 Since the *countries* in the hard fixes and inflation targeting regimes are chosen because of their durability, country fixed-effects would render regime effects inestimable.

20 An alternative strategy would be to follow the methodology of Klein and Shambaugh (2013) and directly examine the strength of interest rate linkages across monetary regimes.

21 I thank Kristin Forbes and Frank Warnock for providing me with their data set.

22 For the cross-sectional analysis, I do not include either country or time effects.

23 Montiel (1998) provides a convenient taxonomy.

24 I ignore intervention that is effective, permanent, and sterilized; Engel (2013) writes in his recent survey, "Very few studies have found significant evidence of a sustained effect of sterilized intervention on the level of the exchange rate."

25 Series on the bond yield and stock index are taken from the IMF's *International Financial Statistics*, while property prices are taken from the Bank for International Settlements.

26 Zimbabwe is a serious outlier because of its recent hyperinflation and has been excluded from CPI inflation estimation.

27 Quantiles are points taken at regular intervals from the cumulative distribution function of a random variable. Dividing ordered data into q essentially equal-sized data subsets is the motivation for q quantiles; the quantiles are the data values marking the boundaries between consecutive subsets.

28 It should be noted that Rey does not actually test the relevance of the exchange rate regime.

29 My earlier paper (Rose 2011) concludes, "The fact that similar economies make completely different choices might lead one to despair; as a profession, we have collectively made little progress in understanding how countries choose their exchange rate regimes. Still, before panicking, one should first remember that such choices often seem to have remarkably little consequence. Exchange rate regimes are flaky: eccentric and unreliable."

30 In their survey, Frankel and Rose (1995, p. 1,706) write, "The more general point is that the volatility of macroeconomic variables such as money, output, and prices (appropriately parameterized) does not vary much across exchange rate regimes, certainly not enough to rationalize the large cross-regime differences in exchange rate volatility." These negative results are also consistent with those in related literature. For instance, in its 2012 *Spillover Report*, the IMF uses three approaches to pin down spillover effects (event studies, examination of U.S. portfolio flows, and vector autoregressions) and sums up the findings as indicating that the "results do not permit any easy generalization about advanced country monetary policy as the main driver of asset price pressures in emerging markets."

31 A number of countries that engage in hard fixes have characteristics—the critical ones being size and polity—similar to those of inflation targeters, including Bulgaria, Republic of Congo, Denmark, Ecuador, Panama, and El Salvador. It seems reasonable to expect more such countries to adopt inflation targeting in the decades to come, and the stability of the international monetary system to expand accordingly.

COMMENTARY

Surprising Similarities: Recent Monetary Regimes of Small Economies

Frederic S. Mishkin

Overview

Andy Rose's paper provides important evidence on the impact on how monetary regimes fared during the recent global financial crisis. The paper has two basic results.

First, it shows that the two monetary regimes, hard fixing and inflation targeting, have become surprisingly durable and were able to withstand the stress of the shocks from the global financial crisis. Before the global financial crisis, monetary regime changes were countercyclical, that is, switches in regimes occurred more often during bad times, and particularly when countries were hit by major crises. The response of hard fix and inflation targeting regimes did not display this countercyclical pattern because countries stuck with them in the aftermath of the global financial crisis, even in the face of huge negative shocks. This was especially true for the inflation targeting regime: Not one of the inflation targeting countries dropped this monetary regime, unless they dropped it to enter the euro zone, and the reasons for entering the euro zone had little to do with the success or lack of success of inflation targeting. Countries that dropped inflation targeting to adopt the euro did so to become more fully part of the European project, and this was a political decision not a monetary one.

The second result in Rose's paper is that from 2007–12 outcomes in terms of many macroeconomic variables were surprisingly similar for countries with hard fix and inflation targeting regimes, while outcomes for the regime he calls the sloppy center were quite different, particularly on the inflation front, where inflation performance was substantially worse.

The empirical analysis is well done and pretty convincing and is not bizarre, at least to this monetary economist. But how should we interpret the evidence? Does it suggest that the monetary regime doesn't matter? I will argue that the

Author's note: *The views expressed here are my own and are not necessarily those of Columbia University or the National Bureau of Economic Research. Disclosure of my outside compensated activities can be found on my website at http://www0.gsb.columbia.edu/faculty/fmishkin/*

answer is a strong no. However, I will also argue that the evidence in the paper suggests that a key feature of a monetary regime is not whether it is a fixed versus a flexible exchange rate regime.

Empirical Issues

Before discussing the above, I do want to raise some concerns about the empirical analysis in the paper as is my duty as a discussant.

The first concern has to do with the classifications of the regimes. Inflation targeting regimes are pretty well defined because most countries adopting inflation targeting have converged to similar best practices. However, this cannot be said for the hard fix classification that Rose adopts, because it includes exchange rate pegs with possibly very different degrees of "hardness." Exchange rate regimes that have a strong statutory framework are clearly hard fixes. These hard fixes are of two types: full dollarization or a currency board. A currency board has the domestic currency backed 100 percent by a foreign currency, and the note-issuing authority, whether it be the central bank or the government, fixes a conversion rate to this currency and stands ready to exchange domestically issued notes for the foreign currency on demand at this rate. A currency board is a hard fix because the commitment to the fixed exchange rate has a legal (or even constitutional) backing and because monetary policy is, in effect, put on autopilot and completely taken out of the hands of the central bank and the government. Full dollarization is an even harder fix because it involves a country dropping its own currency entirely and instead adopting a foreign currency (the U.S. dollar, but it could be another currency, such as the euro or the yen) as legal tender.

The hard fix classification that Rose uses includes conventional pegs and, as Rose mentions in his footnote 11, this "may raise the eyebrow here." My eyebrows certainly did rise because conventional pegs just involve a government announcing an exchange rate peg, which it can abandon at any time. Nonetheless, conventional pegs, even though not written into law, can sometimes be quite hard. Denmark immediately comes to mind because it is so integrated with the euro zone, both economically and politically, and so, though not statutory, the commitment to the peg is very strong. This was similarly true in the past with a country like Austria that sustained its peg with the deutsche mark for 20 years until it adopted the euro. However, many conventional pegs may not have strong commitments behind them, and history has taught us that, in those cases, the pegs are easily abandoned and so were not strong.

Deciding on whether a conventional peg is hard or not is not at all easy to do, thus I do not have a recommendation for Rose on what he should do about

this. Nonetheless, given his expertise, I would like him to think more about this issue to see if there is some way to differentiate conventional pegs into hard versus soft pegs. Doing so would make the empirical work of the paper even more convincing.

The second comment on the empirical analysis is not a criticism, but the recognition that the empirical work in the paper is purposely very narrow. The empirical work finds that the hard fix and inflation targeting regimes have similar outcomes for only one type of shock: that is, a major financial crisis. It doesn't tell us that outcomes would not be very different for hard fix versus inflation targeting regimes from other shocks, in particular, terms of trade or inflation shocks.

The academic literature suggests that flexible exchange rate regimes can cushion terms of trade shocks. Australia's experience immediately comes to mind. With minimal exchange rate intervention, commodity price booms have led to appreciations of the Aussie dollar, which put a brake on the resulting economic expansion of the Australian economy because it led to expenditure switching. Similarly, the contraction of the economy was cushioned by the flexibility of the exchange rate when there were commodity price busts, which led to a depreciation of the Aussie dollar, thereby increasing the demand for noncommodity Australian goods and services. When you talk to Australian policymakers, they argue that the Australian economy became much more stable when they learned that it was a mistake to intervene to keep the Aussie dollar stable and so adopted a more flexible exchange rate regime. Indeed, the Australian move to a flexible exchange rate with inflation targeting is viewed as one of the great success stories of monetary policy regime change in the past 20 or so years.

A large literature (some of which I have contributed to) has documented that inflation targeting has proven to be very successful in containing inflationary shocks, especially for emerging market countries (e.g., see Mishkin and Schmidt-Hebbel 2007). The period of the global financial crisis that Rose examines experienced a worldwide, highly contractionary shock, rather than an inflationary shock. Hence the paper's evidence tells us nothing about whether, in an environment where inflationary pressures are the problem rather than a worldwide economic contraction, inflation targeting might produce better outcomes than conventional pegs.

What Defines a Good Monetary Regime?

One way that the paper might be read is that monetary regimes don't matter much because hard fix and inflation targeting regimes had similar outcomes during the global financial crisis. However, this is not the lesson of the paper at

all, and this is why I did not find its empirical findings bizarre, as Rose suggests might be the case for many monetary economists. Instead it shows that monetary regimes matter a lot, but the key feature of a monetary regime is not fix or flex, as Miguel Savastano and I pointed out in a paper we published in 2001 (Mishkin and Savastano 2001).

Over the past 30 or so years, the academic literature has developed what has been dubbed the science of monetary policy (Clarida, Galí, and Gertler 1999), whose principle policy recommendation is that an effective monetary policy regime must have a strong nominal anchor, that is a commitment to keep inflation stable with a target for a nominal variable, whether it be the inflation rate as in inflation targeting, an exchange rate as in a hard fix, or the money supply as in monetary targeting. Indeed, as I point out in Mishkin (2011), nothing that occurred during the global financial crisis weakens the intellectual underpinnings for this conclusion from the science of monetary policy. In contrast to statements by some economists and media pundits, the events since 2007 strengthen the support for having a strong nominal anchor, because policies to counter the contractionary impact of financial disruptions require a strong nominal anchor to ensure that they do not lead to an unhinging of inflation expectations that could be very harmful to the economy.

The key feature of both hard fix and inflation targeting as monetary regimes is that they both embody a strong commitment to a nominal anchor. Indeed, as Rose's evidence indicates both of these regimes were able to keep inflation low and stable during the global financial crisis, with inflation 5 percentage points lower than was true for countries whose monetary regime was the squishy center.

However, there are two other key features required to make these monetary regimes successful. First is a regulatory system that ensures that financial institutions are safe and sound. If there is a failure of the regulatory system such that financial institutions take on excessive risk, even minor shocks can cause the financial system to seize up, resulting in a banking or general financial crisis. The impairment of financial intermediation during a financial crisis causes lending to contract, and thus leads to a fall in investment spending that causes a contraction in economic activity. In addition, the losses on bank and other financial institution balance sheets during a financial crisis can make it impossible for a country to defend its exchange rate. As a result, a currency crisis ensues, with a collapse of the value of the currency that can lead not only to a surge in inflation but also to further destruction of balance sheets, making the financial crisis even worse (see Mishkin 2006).

The second additional feature necessary for a successful monetary regime is strong fiscal institutions to ensure that government budget deficits do not get out of control. When weak fiscal institutions lead to an explosion in government debt, there are two possible outcomes. One is the possibility of sovereign debt default that causes domestic interest rates to surge, which helps produce a sharp contraction in the economy. The other is described as *fiscal dominance*, because the monetary authorities will no longer be able to pursue monetary policies to keep inflation under control because they will be forced to purchase (monetize) the government debt, leading to higher inflation (referred to by Sargent and Wallace (1981) as "unpleasant monetarist arithmetic").

Note that there are possible strong interactions between weak financial institutions and fiscal dominance. Weak financial institutions during a financial crisis can lead to large government bailouts, as occurred in Ireland during the global financial crisis, that lead to huge budget deficits. On the other hand, fiscal dominance which leads to a sovereign debt crisis can lead to large losses on financial institutions' holdings of government bonds, thereby destroying these institutions' balance sheets, as occurred in Argentina during its 2001–02 crisis.

A tale of three countries—Greece, Spain, and Germany—during the global financial crisis illustrates that having a strong nominal anchor as in a hard fix or an inflation targeting regime is not enough to assure good outcomes. These three countries had very different experiences during the global financial crisis. Greece's woes stemmed from its weak fiscal institutions that led to a sovereign debt crisis that has devastated its economy. Spain, on the other hand, did not run large budget deficits, but did have financial institutions that took on excessive risk during the boom period before the financial crisis. When its real estate market collapsed, its banks booked large losses, leading to a sharp contraction in lending and a deep recession, with unemployment rising to over 25 percent. Germany on the other hand, not only was fiscally responsible before the global financial crisis but also had financial institutions that were sufficiently strong to withstand the losses resulting from the global financial crisis. The result was that the German economy fared much better than many others in Europe.

Conclusion

Andy Rose's paper provides important empirical work that shows that the debate over monetary regimes should not be over fix versus flex, but rather should be how a monetary regime can be designed to have three key features: (1) a strong nominal anchor, (2) strong fiscal institutions, and (3) strong financial institutions.

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COMMENTARY

Surprising Similarities: Recent Monetary Regimes of Small Economies

Anil K Kashyap

I want to thank the organizers for giving me the chance to comment on this paper and to participate in what has become a premier conference. I will separate my comments into three parts. First, I will summarize the main findings of the paper. Next, I will explain why the results are surprising. Finally, I will offer my best explanation for the findings and pose a couple of questions that I believe the analysis raises.

1. Summary of the Main Findings

Andrew Rose's assignment for this paper was to explore the outcomes for different types of monetary regimes in the wake of the global financial crisis. He excludes the five dominant economies and focuses on smaller economies. His analysis is very straightforward and he convincingly demonstrates two main results. The first is that most countries with hard exchange rate pegs or inflation targeting policies both sustained their monetary regimes during the crisis. This stability is historically unusual because, as he also shows, during past recessions it was very common for countries to abandon their monetary policy regimes. Second, he shows that a wide range of economic outcomes across the two regimes were very similar. The variables he considers include both aggregate macroeconomic indicators and financial measures.

The data he uses are all available on his website, and I did some analysis myself cross-checking the findings. My conjecture in re-examining the data was that the choice of a peg instead of inflation targeting depends on the characteristics of a country. So perhaps controlling for those characteristics directly would uncover some differences. I describe a specific example of this below, but ultimately I did not find any systematic patterns that overturn his conclusions.

Author's note: *All errors are my own and the views expressed are not necessarily shared with the institutions with which I am affiliated. I thank Yongcong Tang for excellent research assistance.*

2. Are These Results Surprising?

Andy writes, "while an absence of any large detectable differences across monetary regimes might seem bizarre to a monetary economist, it is almost folk wisdom inside international finance." Let me side with the monetary economists and offer four reasons why these results are surprising and almost troubling.

First, a hard fix is a monetary policy rule, while flexible inflation targeting gives the monetary authority discretion. There is a vast literature on the merits of adopting rules versus exercising discretion in policymaking. Indeed, Mervyn King, who is arguably the father of inflation targeting, routinely argues that the whole point of inflation targeting is to implement constrained discretion (see, e.g., King 2004). I would guess with high confidence that most of the people who have supported inflation targeting did not think that they could have achieved the same outcomes by simply adopting an exchange rate peg.[1]

Second, we do not think that monetary regimes are chosen randomly. As Rose demonstrates, prior to the crisis we often saw countries switch monetary regimes during downturns. One would expect that the countries that had been driven to a peg got there after experimenting with other monetary policy arrangements. Moreover, we would expect that the rules would emerge in specific circumstances. In some cases, the currency unions or pegs might have evolved because of historical accident relating to colonial arrangements. But outsourcing your monetary policy to another country via a fix would make the most sense when the country abandoning flexibility had institutional weaknesses that limited the benefits of retaining flexibility. Crudely put, if a country cannot find a competent central banker or cannot avoid interfering with the central bank, then that country might wind up with a peg. In that case, however, you would think that the underlying problems would still lead to bad economic outcomes during crises (when compared with countries that are organized enough to run a partially discretionary policy).

A third reason that these results are surprising is that countries that do adopt hard pegs often do not seem to be part of an optimal currency area with a country to which the peg is set. I doubt that most economists would suggest that interest rate policy for the West African countries that belong to the CFA should be set in Frankfurt. Yet with support from France these eight countries have pegged their currency to the euro. Likewise, Hong Kong and many countries in the Middle East have hard fixes to the dollar, despite the limited commonalities between their economies and the United States.

Finally, there is a vast amount of evidence that bad monetary policy matters for economic outcomes. So having your monetary policy set by a central

bank that pays no attention to your economy would seem to be a very risky policy to pursue.

Thus, while I recognize that Rose is correct in saying that past research has found results that are similar to what he reports, I do not think we should take the findings as self-evidently obvious. If anything, I would say that as a general proposition most economists would expect that inflation targeting and pegging would be expected to deliver dissimilar outcomes.

3. How Can We Explain the Findings?

After my first read of the paper, I was convinced that it must be the case that many of the countries that adopted hard pegs had problems with corruption or the rule of law. Thus, if we simply redid the analysis and controlled directly for these factors, we could isolate the countries with pegs for these reasons as the ones that have underperformed. Corruption is higher and standard proxies for rule of law are weaker on average in the countries with fixed exchange rates. But controlling for these factors did not overturn the conclusion that most of the economic outcomes that Rose considers look the same between the inflation targeters and the countries with hard fixes.

Upon further review, I switched to a different consideration. The crisis was not only a deep recession that brought strong deflationary pressure, it was also felt virtually everywhere. So in this particular case, for most countries a competent, independent central bank would have aggressively eased monetary policy. The inflation targeting countries were free to pursue such a policy. But for any country that had pegged to the dollar or the euro, they also saw policy ease because of the actions of the Federal Reserve and the European Central Bank. So during the crisis it seems that the risk of a peg delivering an inappropriate monetary response was not an issue. Under this interpretation the crisis is a special case from which we might not want to generalize.

One way to see this is to look at the experience of the oil-dependent countries in the Middle East (Bahrain, Jordan, Qatar, and Oman) which had pegged their currencies to the dollar. As Table 1 shows, oil prices from 2002 through 2007 had nearly tripled, and through the summer of 2008, oil prices were still rising. The U.S. economy had been slow to recover from the 2001 recession and inflation was contained, so the Federal Reserve only began raising interest rates in 2005. Consequently U.S. monetary policy was not likely to be the one that these oil-dependent countries would have chosen. As the table also shows, inflation in the Arab countries was consistently rising, and in 2008, when the Fed had already responded to the onset of the U.S. recession by cutting interest

TABLE 1
Inflation, Interest Rates, and Oil Prices

Year	Federal funds rate (%)	US CPI inflation (%)	Bahrain, Jordan, Oman, Qatar average CPI inflation (%)	Brent oil prices (US$)
2002	1.67	1.6	0.5	24.99
2003	1.13	2.3	1.8	28.85
2004	1.35	2.7	4.2	38.26
2005	3.21	3.4	5.0	54.57
2006	4.96	3.2	6.7	65.16
2007	5.02	2.9	7.5	72.44
2008	1.93	3.8	11.2	96.94
2009	0.16	–0.4	–0.9	61.74
2010	0.18	1.6	1.5	79.61
2011	0.10	3.2	2.0	111.26
2012	0.14	2.1	3.1	111.63

Sources: World Bank Development Indicators and Federal Reserve Bank of Saint Louis FRED database.

rates, we see that inflation exceeded 11 percent. As the crisis raged, oil prices dropped, and in 2009, inflation in the Arab countries also retreated.

I read this evidence as saying that monetary conditions in the crisis still mattered. For most countries super loose monetary policy was appropriate. But in the rare case, like these four countries, where this was not the best policy, the usual problems arose. So I do not think we can conclude that monetary arrangements are simply irrelevant or that success of the hard fixers is inevitable.

Instead, I wonder if the findings in this paper would be obtained in more normal circumstances. Suppose global conditions are not synchronized so that, for instance, the Federal Reserve and European Central Bank interest rates are moving differently. Will the countries that have pegged fare equally regardless of which of the two they have pegged? Another way this could manifest is if the major central bank decisions about when to begin normalizing policies from the extraordinary ones that are in place are asynchronous. Will that have benign effects?

Coincidentally, we may soon get an out-of-sample test of the premise of the paper. The aforementioned members of the West African currency union have announced their intentions to move to a new currency, the eco, which will no longer be tied to the euro and will eventually be enlarged to include a number of other countries.[2] Nigeria is slated to be one of the new members of the full union. Nigeria's GDP is three times larger than the current members of the bloc, and its heavy oil dependence has meant that its business cycle historically has been disconnected from the others. I am betting with the monetary economists that if this comes to pass, the hard fix will lead to hard times.

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NOTES

1 Of course, the analysis in this paper sets aside the five large economies. So perhaps one might conclude that if most of the large economies tried to peg to one of the others, the results would not be good. But, I still believe that the proponents of inflation targeting would expect it to deliver superior outcomes relative to a peg for most small countries.

2 See "Currency Unions in Africa: Ever Closer," *The Economist*, December 7, 2013. http://www.economist.com/news/finance-and-economics/21591246-continent-mulls-merging-currencies-ever-closer

GENERAL DISCUSSION
Surprising Similarities: Recent Monetary Regimes of Small Economies

Chair: Reuven Glick

Mr. Glick: Andy, I'll let you answer the discussants first, and then we'll take some questions.

Mr. Rose: I'll be brief. Ric Mishkin raises the point about whether conventional pegs make a difference. That was something I was a little worried about. Appendix 1 of the paper, available on my website, shows it really doesn't make any difference in practice. But he raises a legitimate point. Anil raised a point about whether hard fixers are different because they have institutional weaknesses, and he's right, they are different. You can see that very clearly in Figure 3. Hard fixers are systemically smaller, but more importantly they have much worse democratic outcomes. There's no doubt about it, he's absolutely right.

Ric and Anil both raised the point that there is only one shock in my analysis. I'm going to blame this on my marching orders, which were to focus on small countries since the global financial crisis.

But I do think that the main message of my paper is true in a much larger context, because it appeals to a literature going back at least to Mussa and Baxter and Stockman, that finds little differences in the behavior of real variables across exchange rate regimes.

Mr. Glick: Okay, let's open to questions. Alan Taylor.

Mr. Taylor: A question for Andy about why this time might be different. I guess this follows up from Anil. If we were teaching this material to undergraduates, we'd use your favorite econ blogger's IS-LM view, and we'd say, well, if the IS curve shifts and if you're on a peg where you can't move the LM curve, you have to import the interest rate because of the trilemma, so the domestic interest rate equals the exogenous foreign interest rate. But if you're floating, you can lower your interest rate and you end up with different domestic and foreign interest rates. But in the post-2008 world, the foreign interest rate is zero for all the relevant partner countries you're pegging to. That collapses the range

of interest rate outcomes you can get in the non-anchor countries. That leads to a very different world, where you may not have the interest rate variations to identify differences in the effects of currency policies, in contrast to, say, Argentina versus Brazil, or Britain versus France in earlier episodes where the policies in these countries diverged.

So I was wondering if you could empirically look at nominal interest rates or policy rates during this episode to see if there's any evidence of a risk premium?

Mr. Glick: Jonathan Ostry?

Mr. Ostry: Congratulations to all of you on respecting the slide limit. I have one question and one comment. The question is whether we are asking the right question. It seems that we need to know more about how countries are managing their economies when they're in different regimes. It seems to me that we should be worried about the buildup of financial vulnerabilities and about how countries are managing the exchange rate under these different regimes. Could you comment a bit about how they're managing things?

The comment is that the one thing I remember about looking at the behavior of countries under different regimes during the global financial crisis is the staggering difference, between, say, the Baltics, which were among the worst performers in terms of output, and Poland, which was among the best performers and allowed its exchange rate to move a lot. I'm not sure if the charts show these outliers, but it certainly seems to be a telling story there.

Mr. Glick: Joshua.

Mr. Aizenman: The most popular regime seemed to be what you are calling the sloppy center. So I have two questions. First, why are we not focusing on comparing the sloppy center with the other two options of hard fixers and inflation targeters? Second, is there any deeper selectivity story about why some countries chose to be in the sloppy center, whether it matters? If you take a horizon of 20 years, I believe that it matters. And being in the sloppy center for emerging markets seems to be the winning regime. But even if you are focusing only on the effects of the recent global crisis, the sloppy center appears to be the most popular regime, since more than 60 countries are now in the sloppy center.

Mr. Glick: Carmen, and then we'll let Andy answer before I take a second round of questions.

Ms. Reinhart: So I'm not entirely surprised by this. About 10 years ago, Andy, as you know, I did the paper with Ken Rogoff on the modern history of exchange

rate arrangements. After going through all the trouble to classify countries by their regime, one of the things that really jumped out was the difference between functional and nonfunctional economies. When you looked at GDP and inflation behavior, there wasn't that much difference whether the country had a hard peg or a floating regime. We didn't control for inflation targeting because a decade ago those regimes were just getting on their feet. But the real difference was between the dysfunctional economies with high inflation—what we called "freely falling"—and everybody else. Related to the question on the sloppy center, I wonder, is there a direct mapping to a longer history in your results, or are we overstating the impacts of the monetary arrangement?

The other thing, though, is when you say it's surprising we had so few transitions between regimes. I think this is a moment where you have to say the opera ain't over till the fat lady sings. What we've had in that window you're looking at is a period of both high commodity prices and low international interest rates. One thing I would love to see is how your exercise would play out at a time when external fundamentals turn nastier for emerging markets. Under this circumstance, how much longer will the inflation targeters stick to their regime than the fixers?

Mr. Glick: Why don't you answer this round of questions, then we'll continue with more questions after.

Mr. Rose: Okay. So Alan Taylor raised the point of using nominal interest rates to differentiate among regimes. He makes the point quite correctly that differences may not be very apparent. But in my statistical work I'm comparing inflation targeters with hard fixers, and in particular the hard fixers that remained fixed throughout the entire period. The inflation targeters typically allow their exchange rates to move a lot. You would have imagined that would lead to very different outcomes, especially for capital flows when compared with the hard fixers. So he's right, and I'm happy to look at nominal interest rates.

Jonathan Ostry asked whether I should take more into consideration, that economies vary on many different dimensions and not just on the exchange rate regime. There's a long literature comparing various different outcomes—for instance, output volatility, output gaps, or inflation—by the exchange rate regime. I recently wrote a review of that literature for the *Journal of Economic Literature* and cited papers, including some by Jonathan.

Now these conditions may vary a lot, and you can include conditioning variables to control for these considerations. But historically this literature has found almost no success in looking across exchange regimes for outcomes

independent of what you're conditioning on. Maybe it hasn't been done historically correctly, but it just strikes me as being a stretch.

Joshua raised the point that many countries are in the sloppy center and in some sense that's deliberately chosen. That may well be true. I think of it as, many countries just don't have the institutional capacity to stick to a monetary regime for whatever reason. They can't stick to a hard fix, and they don't have the capacity to deliver a credible inflation target. So they're just moving back and forth between blocs. The duration of monetary regimes for countries in the sloppy center is almost always less than two years. So I don't think of it as a very well-defined monetary regime, which is why I focus on hard fixers and inflation targeters. Here I've got to correct Ric. The policy choice is not fixed versus float, because a float is not a well-defined monetary regime. If your exchange rate floats, you have to say what you're going to do otherwise. And so I think inflation targeting is a well-defined monetary regime, but not all countries that float have an inflation target.

Mr. Mishkin: Right, we're in complete agreement on this. That's what I'm saying: The monetary regime is not fixed versus float. . . .

Mr. Rose: The regime has to have a nominal anchor. Many countries in the sloppy center float, but they don't say what they do in terms of an anchor. So I just think that's an ill-defined center.

Carmen is exactly right. Certainly, one of the things that I've used repeatedly is the Reinhart and Rogoff data on exchange rate regimes, and the regime often doesn't matter. Now, it may well be the case that you need a longer history to find a big difference between, say, inflation targeters and hard fixers, and that the global financial crisis and its aftermath is too short a period of time. However, historically whenever there's been a really serious recession, monetary regimes fall like tenpins. There's huge regime turnover, especially during bad times. If you have any doubt about it, think about the Great Depression. The global financial crisis and the Great Recession were enormous. You would assume that there would be massive turnover after the recent crisis and there just hasn't been.

So it may be too soon, I'm not denying it. But it's still striking. This is the presence of absence thing.

Mr. Glick: Turning to my list, I've got John Murray, Sarah Calvo, Mark Spiegel, Ashoka Mody, Peter Hooper, Deputy Governor Choi from Korea, and Michael Hutchison, and that'll close the book. So, John Murray.

Mr. Murray: Just a quick point. I may be misremembering, Andy, but at one point in your presentation you concluded by saying that small countries had an alternative. They could go from a hard fix to a floating exchange rate. Now, I'm a believer in floating exchange rates, obviously, so take what I'm about to suggest with a grain of salt. I thought the optimum currency area logic would throw that on its head, because the standard argument for a flexible exchange rate is to have a different macro outcome, but you realized you'd be sacrificing some efficiency gains by giving up fixed exchange rates. So wouldn't your question be better posed if you really believed the macro outcomes were the same? Why don't all of these countries go to a hard fix?

Ms. Calvo: Thanks. A reaction more than a question to Ric. Ric highlighted the case of Australia in praising how well inflation targeting has been working, and I wondered if it has really been tested. During the mid-2000s, Australia received substantial portfolio inflows and their foreign exchange liabilities are quite high, like 60 percent of GDP. And then Lehman Brothers collapsed. So I wonder if in fact the inflation targeting regime in Australia has been really tested.

Mr. Glick: Okay. Mark Spiegel.

Mr. Spiegel: Thanks. I wanted to disagree a little with Anil's characterization about the policy options available under the two regimes during the crisis. In particular, that being pegged to a country that was at a zero rate was essentially equivalent to an inflation targeter cutting the rate to zero. I think even though you're pegged to a country that's at a zero rate, an additional policy option that was available to the hard pegger was a discrete devaluation. I would think a discrete devaluation of a hard peg as being quite a different animal than, say, an inflation targeter who then adopted some kind of a devalued peg or some completely different regime. Indeed, in the paper, Andy, you mention somewhere that a couple of the hard-peggers you identify actually moved their exchange rates during the crisis. I'm not sure how you treated those, but it suggests that policy option was available to the hard peggers that would manifest itself in a very different way than to the inflation targeters.

Mr. Glick: Ashoka Mody.

Mr. Mody: I don't think you answered Jonathan's question on the Baltics versus the non-Baltics. Hungary was in very acute financial distress, but did have a floating regime and did not go into the tailspin in terms of output loss that the

Baltics experienced. Also, your comments in Table 4 said that the durability of the regimes was more or less the same, but there's a lot more exit from the hard fix in Table 4 than your comment suggested.

My third point is that, as Martin Wolf said at lunch, this was a North Atlantic crisis, and a vast swath of the world was not affected by this crisis. To that extent, a large number of countries in your sample did not bear the brunt of the crisis. So drawing the inference that this crisis did not have a material effect on countries irrespective of their regime is not, I think, a very interesting conclusion, because many countries were not directly affected by the crisis.

Finally, at times it looked like you were suggesting that this extended not just to this crisis but was also true of the past. But surely the durability of fixed regimes has been in question for a long time. There is a paper by Obstfeld and Rogoff that talks about the mirage of fixed exchange rate regimes. So I don't think the claim that fixed regimes are as durable as nonfixed regimes is empirically correct.

Mr. Glick: Next we have Peter Hooper.

Mr. Hooper: I was just going to comment that I thought Anil's observation, that this was not the right shock to test this particular question, certainly resonated. I'm thinking back to empirical macro modeling work from a couple of decades ago by Ralph Bryant and others at Brookings that ran a number of large and small economy macro models through their paces. We found there were very sizable differences between fixed exchange rate regimes and money targeting as opposed to inflation targeting, which I presume would be somewhat transferable, although I guess Carmen's observation about earlier evidence might question that.

Mr. Glick: Okay. Mr. Choi?

Mr. Choi: I have a question about the possible role of nonlinearities when comparing the two different regimes. The regression results suggest the two regimes are very similar, but looking at Figures 3, 4, and 5, we can see some kinked curves or curves with inflection points. For example, capital inflows are much larger for hard fixers. If we look at the high growth in reserve accumulation, there tends to be greater accumulation for hard fixers.

We also can see that hard fixers experienced much larger stock price surges than did inflation targeters. Taking this all together then, there tends to be more volatility in economic outcomes for hard-fixers than other regimes. Thank you.

Mr. Glick: The last question is from Michael Hutchison.

Mr. Hutchison: I'm afraid to ask this question because Andy will usually say he's already addressed it somewhere in an appendix, but I'll go ahead anyway. It looks like 83 countries started with the hard fix, and of those, 60 survived, which means more than 25 percent dropped out. What bothers me a little is that you want to know at the beginning: Did the countries that began with the hard fix perform similarly to your floating, inflation targeting group? Because in some sense, looking at the countries that dropped out, my guess is they're very, very different. So I would argue that you may have a survival bias here in the selection.

Mr. Rose: I'll just respond quickly to the questions that were directed to me. John Murray raises a completely appropriate point, which is, why isn't the default to use a hard fix, if the outcomes are the same? I think, not being a central banker myself, that people might respond that this is only one type of shock and there are other shocks. But it's a legitimate point.

Ashoka Mody chastized me a little bit for not answering Jonathan's point about the Baltics versus Poland and he added Hungary. I don't look at individual countries in my analysis. I don't do anecdotes because for every anecdote that one person can come up with, I can come up with one on the other side. I do everything. Really, I'm not switching. Jonathan is right, the Baltics did much worse than Poland. But, if you look at the entire spread of the data I displayed, there have to be cases on the other side as well. That's the reason why I try to include all of them.

You also raised a point I think is right, that there's a lot of exiting from hard-fixes. Mike Hutchison raises the same point. There's no doubt about it, I'm the first to say countries that say they're fixed don't stay fixed forever. In my statistical analysis I focused on the countries that have hard fixes all the way through. As Mike says, I could have used the ones that started as hard-fixers. I don't believe things would change very much, but I haven't done it, at least not in the version that's reported. I certainly don't want to claim that all hard fixes are durable because they're not. But I would say that's the case so far for inflation targeters.

It was pointed out that the figures suggest there may be nonlinearities in the relationships. Yes, if you look at the tails in the graphs, there is a difference between the behavior of the hard fixers and inflation targeters. I would say that those are simple bivariate graphs, and they're there to give you a flavor of the data. But everything I do, I try to reinforce with more rigorous methods. Now, if you're going to look at the tables, the best thing to do is to match every observation from a hard fixer with an observation from an inflation targeter. In the

Appendix on my website, that's what I did in Tables 13 and 14. You want to do it in a nonparametric way because the tail distributions are different. Tables 13 and 14 reinforce that impression in exactly the same way, but it's more rigorous. The last comment I want to make is that Mike Hutchison makes an extremely good point. I actually did the analysis but it's not reported in the table in this paper. So I'll add another appendix table online. Thank you all very much for your comments. I really appreciate them.

Mr. Mishkin: There are two other things. Sara, you raise this issue about whether Australia's been tested. Actually I talked about Australia just because I love Sydney so much. But Canada is also very similar in this regard. And I would argue that it's true, in a sense, that they weren't tested in terms of having internal financial crises. But that's because they basically got it right in addressing the criteria for successful monetary regimes. That is, they both had done nominal anchors through inflation targets with floating exchange rates. In addition, their regulatory systems worked very well by being principle-based. Both countries have about five big banks that regulators sit down with and, if there's something going on in the bank that they don't like, they say, nudge nudge, wink wink, we don't want you to keep doing it. That's versus rules-based regulation, in which the rules can frequently be stretched. As a result they did not permit much overly risky behavior. There's also an element of luck. Both Canada and Australia had commodity booms and great investment opportunities in their own countries, so they never got involved in a lot of the crazy derivative securities like Germany and France did, because they actually have good places to lend their money. So there may be a luck element besides the fact that they did a good job in terms of regulation.

On the fiscal issue, again, both of these countries are paragons of virtue in this regard. Canada actually used to have very bad fiscal policy, but in the 1990s somehow a magic wand hit them and they figured it out. And, in fact, in Australia there was a debate about whether they had too low a debt-to-GDP ratio. I think that these examples show they had all three elements of the monetary regimes and, in that sense, when this crisis hit they did very well. And I think it was a test. But you're right, if there are different shocks because they screwed up on their financial sector or fiscal sector, the fact that they had an inflation targeting regime wouldn't have helped them. They would have been in deep doo-doo like the rest of us.

Mr. Kashyap: To Mark's point, I think Ashoka Mody and Mike Hutchison already answered. You can say I'm doing a one-time devaluation, then you do a two-time devaluation, and then the next thing you know you're not fixed

anymore. You can do that a little bit, but you had better have a good story about why you're doing it. If you go into it saying I'm kind of fixed, I expect to stay fixed, I really mean it but I might unfix—it's not going to work so well.

Mr. Glick: Let's thank all of the speakers for an excellent discussion.

EVENING KEYNOTE ADDRESS

The Second Phase of Global Liquidity and Its Impact on Emerging Economies

Hyun Song Shin

The term "global liquidity" is often invoked by emerging market policymakers to denote the global factor that drives cross-border spillovers in financial conditions and credit growth. The term is often used in connection with monetary policy spillovers from advanced economies.

However, global liquidity is not a term that would receive universal acknowledgment among researchers as being a meaningful concept. The vagueness of the word "liquidity," as well as its intellectual baggage associated with past academic disputes concerning the role of monetary aggregates in macroeconomics, means that many listeners have already erected barriers to whatever comes next in the conversation. That said, the recent Bank for International Settlements report on global liquidity (BIS 2011, known as the "Landau report") and the International Monetary Fund's work on the topic, both at the behest of the Group of 20, have put the term "global liquidity" into the titles of official documents, and so it does appear that the term is here to stay.[1]

For the benefit of defining the issues more clearly, it is useful to distinguish between two phases of global liquidity. The first phase, starting roughly in 2003 and lasting until the 2008 crisis, had global banking at its center, and the central theme was the transmission of looser financial conditions across borders through the acceleration of banking sector capital flows. The global factor that explains comovements in financial conditions across geography and sectors in this context is the leverage of the global banks. This topic has been covered extensively (especially in the context of the European crisis), and so I will not dwell on it today.[2]

More relevant today is what I would classify as the second phase of global liquidity, which started around 2010. In this second phase, the main stage is the bond market, especially the market for emerging market debt securities that

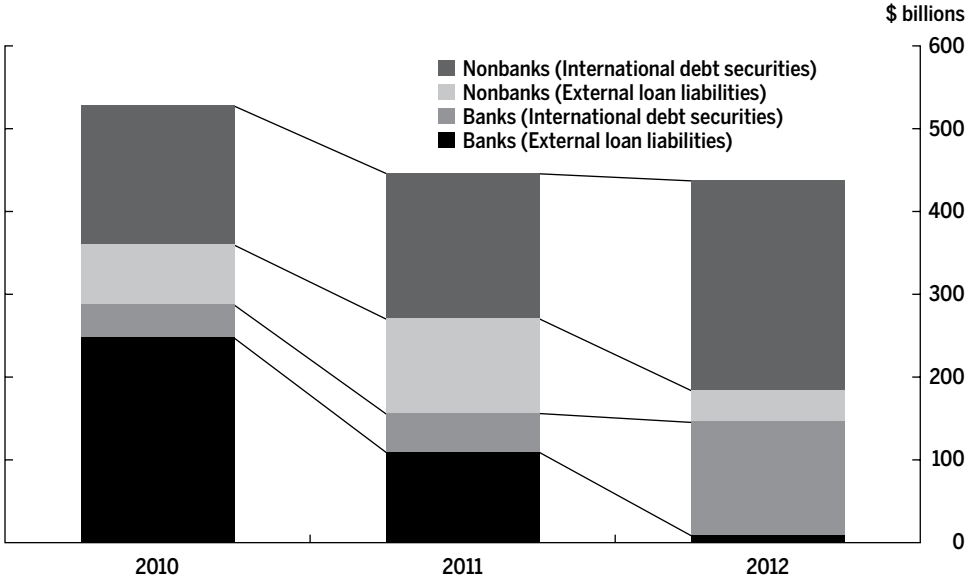
Author's note: *I thank Claudio Borio, Dietrich Domanski, Ingo Fender, Masazumi Hattori, Dong He, Philip Turner, and Jing Yang for comments.*

are open to international investors. As for the main players, the global banks have increasingly given way to asset managers and other "buy side" investors who have global reach. The transmission of financial conditions across borders has taken the form of "reaching for yield," the decline of risk premiums for debt securities and the explosion in issuance of international debt securities that has ensued in order to satisfy the demand.

Figure 1 summarizes the shift from banks to the bond market since 2010. The chart uses BIS banking and securities statistics and is taken from Turner (2013). The bottom two sections of the bars both refer to borrowing by emerging market banks. The top two sections of the bars refer to borrowing by nonbanks. The numbers are net financing amounts each year, and hence denote increases in the amounts outstanding. Notice how the bottom black section shrinks rapidly across the years, indicating that the capital flows from global banks to emerging market banks have slowed to a trickle. In its place, emerging market banks have increased their debt securities issuance. For nonbanks, the growth in net issuance of international debt securities has been even more dramatic.

Notice that in the legend for Figure 1, the word "external" is in quotation marks. This is because the international debt securities numbers in Figure 1 are based on the *nationality* of the borrower, rather than the usual practice

FIGURE 1
Net "External" Financing of Emerging Economies



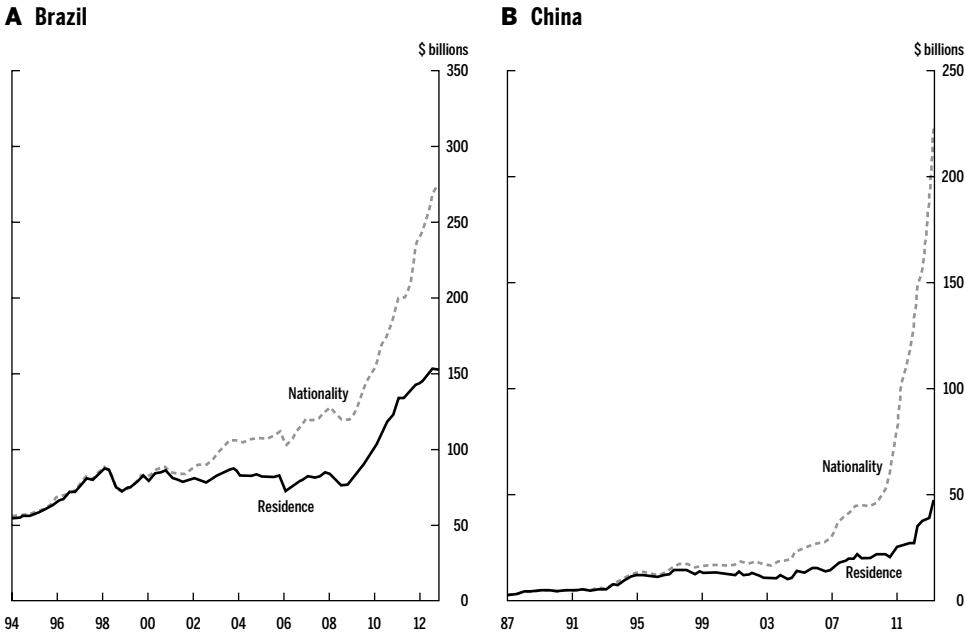
Source: Turner (2013).

of basing the classification on the *residence* of the borrower. If an emerging market corporate borrower issues U.S. dollar-denominated bonds through its London subsidiary, the usual locational definition would treat the bonds as the liability of a U.K. entity. However, the emerging market company will manage its finances by reference to its consolidated balance sheet. Thus, in order to explain the behavior of the emerging market company, it is important to consider the consolidated balance sheet and take account of debt securities issued offshore.

The offshore issuance of debt securities by emerging market firms has proceeded at a great pace in recent years, as documented in the recent *BIS Quarterly Review* (McCauley, Upper, and Villar 2013). As an illustration, Figure 2 plots the international debt securities outstanding of borrowers from Brazil and China, plotted by residence and by nationality. The difference between the nationality and residence series is accounted for by the offshore issuance of international debt securities. The difference remained small until after the global financial crisis, but since has widened dramatically. We can also see from

FIGURE 2

International Debt Securities Outstanding (All Borrowers) by Residence and Nationality of Issuer



Source: Bank for International Settlements securities statistics Table 11A and 12A.

the scale of the charts that the outstanding amounts are large. McCauley, Upper, and Villar (2013) note that most of the offshore issuance has been in U.S. dollars, so that emerging market corporates have become much more sensitive to U.S. interest rates and the fluctuations in exchange rates vis-à-vis the U.S. dollar.

The weight of corporate bond issuance in offshore locations sheds light on a recent puzzle. The challenge has been to reconcile what appears to be the small net external debt position of many emerging economies (measured in the usual residence terms) with the apparently disproportionate impact of tighter global monetary conditions on their currencies and financial markets.[3] One piece in the puzzle may be the role of nonfinancial firms that operate across borders. When corporate activity straddles the border, measuring exposures at the border itself may not capture the strains on corporate balance sheets.

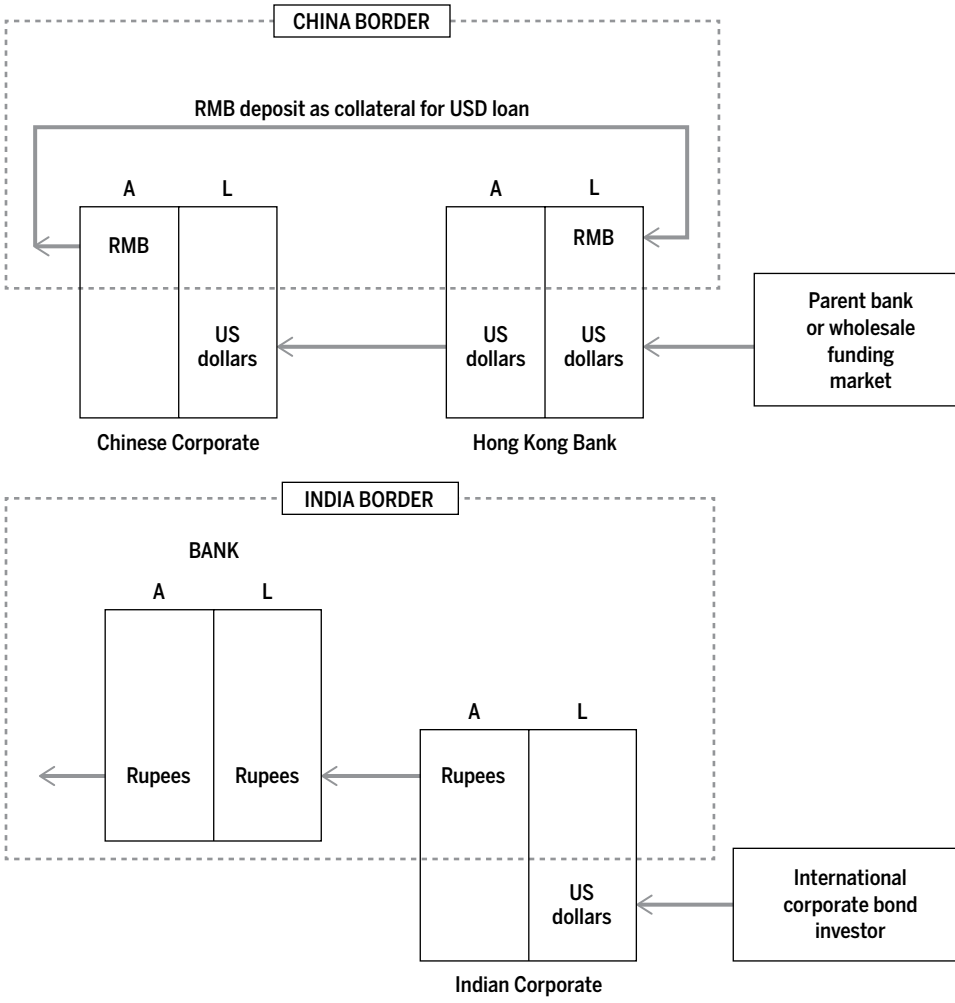
Figure 3 depicts two instances in which the true external exposures of firms with cross-border activities may not be captured in the residence-based statistics. The top panel shows a Chinese corporate with a Hong Kong office that borrows in U.S. dollars from a Hong Kong bank, and deposits renminbi (RMB) in the China office of the bank as collateral. This is just like the old London Eurodollar currency swap transactions of the 1960s and '70s, which work like a straight collateralized loan. The bottom panel shows an Indian corporate that borrows in U.S. dollars through its London subsidiary and defrays the group's costs using the dollars, but that then accumulates rupees instead at headquarters. The rupees are then held as time deposits in a local bank in India. In both instances, the firm has engineered a currency mismatch. In effect, the firm has taken on a carry trade position, holding cash in local currency financed with dollar liabilities.

One motive for taking on such a carry trade position may be to hedge U.S. dollar receivables. Alternatively, the carry trade position may be motivated by the prospect of financial gain if the domestic currency is expected to strengthen against the dollar. Whatever the motivation, the corporate treasurer who takes the consolidated balance sheet into account will care about fluctuations in the exchange rate as well as the U.S. dollar borrowing costs.

In this way, the second phase of global liquidity has resulted in a combination of forces that has increased the vulnerability of emerging economies to a reversal of permissive financial conditions. There are three elements:

- Yields on emerging market debt securities in local currency have fallen in tandem with those of advanced economies and have shown increasing tendency to move in sync with those of advanced economy bonds (Miyajima, Mohanty, and Chan 2012, and Turner 2013).

FIGURE 3
Straddling the Border through International Transactions



- Offshore issuance of corporate bonds in foreign currency has resulted in currency mismatch on the consolidated balance sheets of emerging market firms. Accompanying the offshore issuance has been growth in corporate deposits in the domestic banking system, which are vulnerable to withdrawal in the event of corporate distress.
- The growing stock of emerging market corporate debt securities has been absorbed by asset managers whose main reason for buying them has been the perception of stronger economic fundamentals of emerging markets.

The reversal of all three elements during the summer of 2013 put emerging economy financial markets under severe stress. When the current lull in global financial conditions is eventually broken by tighter U.S. dollar funding conditions due to Federal Reserve monetary tightening, the vulnerabilities are likely to be exposed once more. Given the elements that have underpinned the second phase of global liquidity, the crisis dynamics in the emerging economies would then have the following elements:

1. Steepening of local currency yield curve,
2. Currency depreciation, corporate distress, and runs of wholesale corporate deposits from the domestic banking system,
3. Decline in corporate capital expenditures feeding directly into a slowdown in economic growth,
4. Asset managers cutting back positions in emerging market economy corporate bonds citing slower growth in the emerging economies, and
5. Back to step 1, thereby completing the loop.

The distress dynamics sketched out here have some unfamiliar elements. We normally invoke either leverage or maturity mismatch when explaining crises, and the usual protagonists in the crisis narrative are banks or other financial intermediaries. In contrast, this scenario has asset managers at its heart. We find this unsettling, as long-only investors are meant to be benign, not create vulnerability. They are routinely excluded from the list of "systemic" market participants.

However, the distinction between leveraged institutions and long-only investors matters less if they share the same tendency toward procyclicality. Asset managers are answerable to the trustees of the fund who have given them their mandate. In turn, the trustees are themselves agents vis-à-vis the ultimate beneficiaries. In this way, asset managers lie at the end of a chain of principal-agent relationships that may induce restrictions on their discretion to choose their portfolio. Frequently, the trading restrictions are based on measures of risk used by banks and other leveraged players. As such, their behavior may exhibit the same type of procyclical risk-taking that banks are known for. The uncomfortable lesson is that asset managers may not conform to the textbook picture of long-term investors, but instead may have much in common with banks in amplifying shocks.

In addition, the large weight of the asset management sector in the financial system will ensure that any tendency toward procyclicality will be felt more broadly. The recent report by the U.S. Treasury's Office of Financial Research (OFR 2013) estimates that the top five asset managers (BlackRock, Vanguard, State Street, Fidelity, and Pimco) have combined assets under management of

$12 trillion, while the top 10 have $18 trillion. As large as these figures are, they may underestimate total exposures to risk assets in that "assets under management" refers to equity, not total assets of these entities. Not much is known about the effective leverage of the asset management sector, but the leverage may be expected to be modest in the aggregate.

Given the potential for procyclical actions and the sheer size of the asset management sector, the usual indicators of vulnerability that were designed and back-tested for past crises (many of which are bank-driven events) will no longer be very useful. In particular, the crisis indicators that were developed by reference to the first phase of global liquidity will be of little use during the second phase. For instance, it would be easy for some policymakers to be lulled into a false sense of security by seeing that banking sector leverage is lower now than it was before the Lehman Brothers bankruptcy. As always, the challenge should be to anticipate the next crisis rather than look back to the past crisis, but accountability exercises usually address known past weaknesses, rather than asking where the new dangers are.

What then are the useful signals for vulnerability during the second phase of global liquidity?

Tracking the amounts outstanding of corporate bonds and the yields on such bonds would be a good first step. Tracking offshore issuance by emerging market borrowers may be particularly informative in gaining a sense of the currency mismatch on the consolidated balance sheet.

There is one further idea, which harks back to the classic theme of measuring global monetary aggregates. This brings us back full circle to "global liquidity" in the title. The key insight is that any corporate bond issuance activity will leave an imprint on the domestic banking system. Since the firm will be issuing more debt during periods of permissive financial conditions in international capital markets, increased borrowing in international capital markets will coincide with greater holdings of cash as deposits in the banking system or short-term instruments in the shadow banking system.[4]

Thus, an indirect way to track the activity of corporates who straddle the border is to examine the fluctuations in a monetary aggregate consisting of the corporate deposits and other claims of the nonfinancial corporate sector on the domestic banking system.

In recent work with some coauthors,[5] I have examined the properties of such an aggregate by constructing a global monetary aggregate that consists only of claims of nonfinancial corporates. The procedure is as follows. For each country j, we take the deposits of nonfinancial corporates in the banking system from the information that is used to compile the IMF's *International Financial*

Statistics (IFS). Having obtained corporate deposits L_j for each country, we convert the sum into U.S. dollars and then add up across countries. The resulting series is dubbed *GL* for global liquidity. In other words, *GL* is defined as follows:

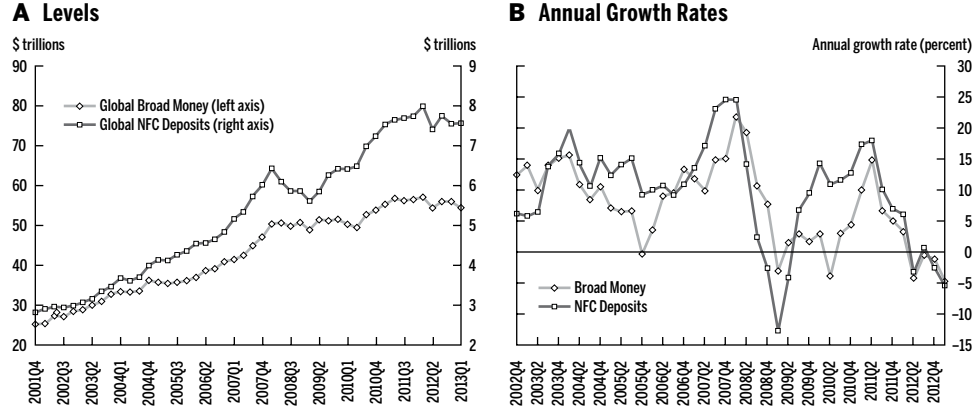
$$GL = \sum_j \frac{L_j}{\text{Price of U.S. dollars in currency of country } j}.$$

The study of global monetary aggregates echoes the project outlined by McKinnon (1982), but with a very different rationale. McKinnon proposed a global monetary aggregate in a monetarist framework with stable demand for global money due to the possibility of substitution between currencies. For us, the role of the money stock serves as an indirect indicator of global credit conditions when the cross-border activity of nonfinancial firms makes the direct measurement of corporate credit through standard locational measures of external indebtedness less meaningful.

We see from Figure 4 that the global liquidity measure displays a highly procyclical pattern, tracking the upswing before the global financial crisis, the sharp decline with the onset of the global financial crisis, and then the subsequent recovery afterwards.

In Figure 4, the sharp fluctuations in the global liquidity measure reflect, in part, the exchange rate movements of the U.S. dollar vis-à-vis other currencies. The sharp decline in the global liquidity measure during the 2008 financial crisis is explained in part by the rapid appreciation of the U.S. dollar that coincided with the deleveraging pressures that hit borrowers around the world. In

FIGURE 4
Global Broad Money and Global Liquidity



Source: Chung et al. (2014), data from IMF *International Financial Statistics*, 2002:Q4–2013:Q1.

turn, the bounceback in the global liquidity measure reflects, in part, the appreciation of emerging economy currencies in the aftermath of the crisis. By using the U.S. dollar as the numeraire, the fluctuations in *GL* due to exchange rate changes move in the same direction as the local currency quantities. So, the global liquidity aggregate reflects the reinforcing interaction of the exchange rate and the local currency monetary aggregates.

Chung et al. (2014) show that the global liquidity aggregate *GL* comoves strongly with global activity indicators, such as global exports, imports, and GDP growth. Further investigations may reveal how much *GL* tells us about vulnerability to crises.

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NOTES

1 See also the speeches on the subject by Caruana (2013a, b) and the IMF working paper by Chen et al. (2012).

2 See, for instance, the report of the Committee on International Economic Policy and Reform (CIEPR 2013). I have previously characterized the first phase of global liquidity as a "banking glut" (Shin 2012). Bruno and Shin (2013) identify the leverage of global banks as the single global factor that drives financial conditions worldwide during the first phase.

3 See, for instance, Krugman (2013).

4 An example is Japan in the 1980s. Hattori, Shin, and Takahashi (2009) show that the rapid increase in broad money in Japan in the 1980s was due to corporate time deposits of large manufacturing firms recycling capital market funding.

5 Chung et al. (2014).

GENERAL DISCUSSION
The Second Phase of Global Liquidity and Its Impact on Emerging Economies

Ms. Reinhart: So let me see data-wise if I understand what you are saying. There's a pocket of relatively well-documented activity in the domestic banking sector, there's a pocket of relatively well-documented activity in what one would classify as classic external debt, but there's also this quasi-domestic securitized debt market that's a claim on reserves, that's sort of in the netherworld, and that is where you think the real action is going to come from.

Mr. Shin: I like this comment. So we care about what happens at the border because that's a very natural unit for thinking about decisions of various actors. What I'm claiming is that now that's no longer a very useful unit in thinking about decisions, because corporate balance sheets now straddle the border. As you rightly point out, these actions are now a claim on reserves. This is one of the first things that Raghuram Rajan said we need to look at when he went to the Reserve Bank of India. We have to look at this larger universe. How you track it is going to be much more difficult. So one of the things you do is look at Bank for International Settlements data on cross-border claims. The other way is to track the money stock, because it gives you a sense of what the claims of the very active players are doing in the domestic financial system. Now what you do about it is a very difficult question. How on earth are you going to deal with this? These are nonfinancial firms. With banks you have a chance, with nonfinancial firms it's just much more difficult. So I don't claim to have an answer on what to do.

Mr. Plosser: I just want to follow up on why you think this change is taking place. One way of thinking about it is that the actions of policymakers around the world have set incentives for this type of change in financial activities to take place. So, is there a way to think about whether or not this is endogenous or exogenous? And if it's endogenous, why, and what do you think is driving it?

Mr. Shin: Well various people have done surveys of these nonfinancial corporations. For example, the Hong Kong Monetary Authority (HKMA) has conducted surveys of these financial firms and asked them, Why do you borrow so much? And the answer is, it's a great time to borrow, the conditions are so

good. So essentially the answer is that they borrow because they can, and we don't normally ask what they do with the money. What this suggests is that we should be spending a lot more time asking what on earth these firms do with the money.

Mr. Plosser: But it's also important to ask why this is such a good time to borrow money.

Mr. Shin: Well, if you just look at the sums, before May 22, 2013, there was insatiable demand for any kind of emerging market paper, and it's aided and abetted by various market players. There are sales-side people, but it's the asset managers who are just loading up on this in huge amounts.

Mr. Plosser: Does this have anything to do with monetary policy?

Mr. Shin: I suppose interest rates do have some effect. Reaching for yield has consequences, I would say that.

Mr. Gourinchas: So I guess my question is going to follow on the previous one. It seems that for a long time we were worried about emerging market economies borrowing in foreign currency and about original sin. In recent years we've convinced ourselves, perhaps wrongly, that original sin was not as much a problem as it used to be because there has been a development of local currency bond markets, so we don't see as much of a currency mismatch. And you're telling us that if we look more closely, there might be just as much currency mismatch, not on the bank balance sheets but on the nonfinancial corporate balance sheets. We didn't have good models of what was at the root of the original sin in the first place, so why would countries borrow in foreign currency when they would be exposing themselves to these balance sheet effects if there were a crisis? I'm connecting a bit to Carmen's paper presented earlier today—it might not make sense for a corporate that's borrowing nowadays to borrow in foreign currency. Instead, if it were hit by an aggregate negative shock, something like U.S. tapering, it would be easier for a borrower to think if they are sitting on billions of dollar reserves, that it's going to be easy for them to have dollar liquidity so they don't have to worry about currency risk.

Mr. Shin: I think that would be a subsidiary concern. These are nonfinancial firms, so they would not be the first in line to receive dollar liquidity. Although in India, exceptionally, they did do that. But I think the primary reason is that this is a great way to beef up your bottom line. If you have a currency mismatch and you're sure the renminbi is going to appreciate, what better way than to use currency movements to make a fast buck? For exporters, as well, you could give

a more benign story, that they're trying to hedge their currency exposures if the invoicing is in U.S. dollars. But, as you know, the line between hedging and speculation is very blurred.

Mr. Wolf: It's late at night and I've been up a long time, but I am very confused. The two examples you gave seem to suggest that these are ways around exchange controls. It has been obvious for some time that exchange controls are porous, and one of the ways companies are getting around them, obviously with the tacit connivance of their authorities, is to set up subsidiaries in London. But is that a central part of the story, or since you gave Brazil in there, is it not? And the second question I had . . .

Mr. Shin: Brazil is also one of these cases.

Mr. Wolf: I thought Brazil didn't operate exchange controls in the same way.

The second question I had is, what's the systemic risk here? Okay, lots of nonfinancial corporates are doing bank speculative-type things, they borrowed bonds that are relatively long term. If this goes wrong there may be a lot of corporate bankruptcy, depending on how leveraged the corporates are. So we have lots of corporate bankruptcies in emerging economies and the asset managers who are not leveraged will lose money. They will report to the people who put money with them that things didn't go as well as hoped. So what? Tell me why I should care.

Mr. Shin: That's exactly the kind of reasoning that I wanted to push back against.

Mr. Wolf: I knew I was wrong.

Mr. Shin: We tend to think of leverage as being dangerous because it leads to failures but I would say that leverage is especially dangerous when the leveraged players don't fail. So how on earth did they avoid failing? It's because they were cutting back. I think the mechanism is much more direct than through banking sector credit contractions. Here it's going straight to the yield curve, and as soon as the yield curves steepen, capital expenditures drop like a stone. So you're going to see investment cut off, and growth will just plummet. And so we care about financial stability not because it's an interesting and topical issue. We care about financial stability not only because of failures, but because it's about economic outcomes. It's about the macro economy. This is saying we can just bypass the banks and go straight to the economic outcomes. And you can see the shades of this also in the United States. When mortgage rates jump by 100 basis points, it has an economic impact. That was one of the reasons the

Federal Open Market Committee gave for thinking twice about the tapering in September. So anything that happens here, you can think of this being magnified several-fold in emerging economies.

This is not a scenario that's very familiar. These are very unfamiliar problems, and I think it's more important than ever that we have a very clear mind about what really matters and what the mechanisms are. We have developed rules of thumb to help us, but some of the rules of thumb are not very useful. So if we just look around for leverage in the banking sector, that's exactly the wrong thing to look at because that is not the mechanism I claim is going to happen.

Mr. Wolf: But it is leverage in the nonfinancial corporate sectors.

Mr. Shin: Yes, but as you say . . .

Mr. Wolf: If they had borrowed very little we wouldn't worry.

Mr. Shin: Well remember, it's what they do that we care about. So they're cutting back. Yes, they are leveraged, and it's in the form of corporate bonds rather than bank loans. But they're not 30 times leveraged, as banks are. Their leverage is much lower, but they would nevertheless behave in ways that would actually amplify these kinds of stress.

Mr. McKinnon: I like your analysis. You say that phase one began with a cut in interest rates in the United States to 1 percent in 2002, and phase two with the cut in December 2008 to zero percent, and you document very well that both set off waves of hot money. But the same thing happened in the 1970s with the threat of the Nixon shock in the form of a dollar depreciation, there were massive hot money outflows from the United States in 1970 and '71 that caused the great inflation in 1973–74. And then it happened again, there was a phase two when the Jimmy Carter Administration tried to talk the dollar down against the yen. Then there was another big flood of hot money that caused a second wave of international inflation in 1979–80, until Paul Volcker had to come in and rescue the situation. So there was the same thing, two phases then just as there are now.

Mr. Shin: I did read your 1982 paper. But by the way I should say that the paper and the charts are on my home page. And the ones that I've posted have fewer typos.

Mr. Yen: So my question is, why do government and nonfinancial corporates hold so much in liquid foreign assets and also borrow externally? After the

Asian crisis, emerging market economies turned themselves from debtors to creditors, and they have managed liquidity by accumulating foreign reserves that are typically dollar-denominated. So when the exchange rate changes, they have a currency mismatch problem. So to balance their assets and debts in terms of currency, many governments and firms borrowed more abroad in dollars, particularly when interest rates were low, in order to achieve better currency matching.

Mr. Shin: I think John is a bit restless because of the time. Time for one more question.

Mr. Calvo: I wonder if I can take you back to monetary policy. When you look at what happened in developed markets, the central banks thought they were facing regular conditions and pushed the interest rate down to zero. After doing that, they looked around and said, Let's do something else because this is not enough. What they learned was that this was not a standard depression or recession, but a financial crisis that hit the credit market in the face. So my question has to do with monetary policy looking forward, if you are faced with this kind of credit shock. Reducing interest rates plays two roles in this situation. One role is the standard one of lowering the cost of borrowing. The second role is that lowering the interest rate on very liquid assets creates incentives to search for yield. I don't like that expression because it's not a search for yield in my mind, but rather for things that are very liquid. So by pushing the interest rate down to zero and doing something that we know little about, central banks created the incentives for the sort of things that you're now describing. The alternative would have been, when you notice that the brake is not working in your car, you use the handbrake, right? But we waited until we crashed. And after we crashed, then we used the handbrake. That's the way I think about this in intuitive terms. So what lessons do we get from this for the future? Shouldn't we attack the credit market more directly and maybe use quantitative easing from the beginning? Just go directly to where the problems are and buy up toxic assets and so on?

Mr. Shin: Like credit easing.

Mr. Calvo: Yes.

Mr. Shin: Anil Kashyap and I are working on a project right now that addresses some of these issues. I think one way to phrase it is to ask, what are the trade-offs? The trade-off would be stimulus—if you had stimulus today, what is the trade-off? What's the cost? The cost is, when eventually you have to exit, you're

going to have to face a tougher problem. It's trading off something tangible and direct today with a potential issue down the road. So I think that's a very difficult thing to convey and to actually work out. But let's finish with that.

Macroprudential Policies in a Global Perspective

Olivier Jeanne

This paper analyzes the case for the international coordination of macroprudential policies in the context of a simple theoretical framework. Both domestic macroprudential policies and prudential capital controls have international spillovers through their impact on capital flows. The uncoordinated use of macroprudential policies may lead to a "capital war" that depresses global interest rates. International coordination of macroprudential policies is not warranted, however, unless there is unemployment in some countries. There is scope for Pareto-improving international policy coordination when one part of the world is in a liquidity trap while the rest of the world accumulates reserves for prudential reasons.

1. Introduction

One legacy of the global financial crisis is the emergence of macroprudential policy as a new policy tool towards financial stability. The policymakers in charge of financial stability missed the mark before the crisis because they failed to perceive and contain the financial vulnerabilities that were building up during the boom. Macroprudential policy fills this gap—retrospectively and hopefully looking forward—by restraining the factors of systemic risk in the balance sheets of the banking and real sectors before the crisis. To the extent that it succeeds, macroprudential policy will allow monetary policy to continue to focus on its traditional objectives.

This paper is about the nexus between macroprudential policies and international capital flows. This nexus is important because international capital flows play a key role in generating the financial vulnerabilities that macroprudential policy tries to remedy. There is evidence that inflows of private capital to emerging market economies help generate domestic credit booms that often lead to financial crashes (Obstfeld 2012). Emerging market economies

Author's note: *A preliminary version of this paper was presented at the BOJ-IMES Conference, "Financial Crises and the Global Financial System" at the Bank of Japan, May 29–30, 2013. I thank the participants at these conferences for their comments, especially my discussants Guillermo Calvo, Jonathan Ostry, and Eric Santor. I also thank Anton Korinek for his comments on a previous draft.*

have responded to surges in capital inflows by accumulating large stocks of foreign reserves and with prudential capital controls.[1] International capital flows pose macroprudential challenges for advanced economies too. For example, it has been argued that the rest of the world's appetite for U.S. "safe assets" was an important factor behind the U.S. credit and asset price boom and the subsequent crisis (Bernanke et al. 2011).

The relationship between macroprudential policies and international capital flows goes both ways. Not only do macroprudential policies respond to capital flows, they also affect capital flows, and they do so in a way that may generate undesirable international spillovers. For example, the accumulation of large stocks of reserves may have prudential motives from the perspective of emerging market economies, but it may have had a destabilizing effect on the U.S. economy. At a conceptual level the existence of such spillovers is not surprising. In a globally integrated financial market, a macroprudential restriction in one part of the world deflects financial flows toward the rest of the world, which must then deal with the consequences for its own financial stability.[2]

While there is a long line of literature on the international spillovers generated by monetary policy (and to a lesser extent fiscal policy), we would like to know more about how macroprudential policies interact in the global economy and whether there is a case for international rules or mechanisms of coordination in this area. This issue is discussed in Jeanne, Subramanian, and Williamson (2012), Ostry, Ghosh, and Korinek (2012), and Korinek (2012), but there remains scope for more theory to inform the policy discussions. This paper contributes to fill this gap.

For this purpose, I present a simple framework to analyze the international consequences of macroprudential policies. The model is in line with the recent theoretical literature that motivates the role of macroprudential policies by the need to address certain financial externalities, as reviewed in Section 2. The model assumes that certain financial contracts generate negative externalities because they increase the risk of a systemic debt crisis. The role of macroprudential policy is to correct the distortions induced by these externalities. I adopt here a broad view of macroprudential policy, that includes but is not limited to banking regulation and also covers measures such as prudential capital controls on inflows or the accumulation of international reserves.

I then look at the international spillovers generated by macroprudential policies. The key result is that macroprudential policies are *strategic complements*, to use game theory terminology. A macroprudential restriction in one country deflects capital flows toward the other countries, leading them to restrict their own macroprudential policies. In the uncooperative equilibrium,

all countries implement macroprudential policies that are more intense because of these spillovers, a situation that might be reminiscent of an inefficient arms race.

In spite of these spillovers I find that there is little scope for international coordination of macroprudential policies to improve global welfare. The reason is the same as in Korinek (2012), who derived this result earlier. The spillovers induced by macroprudential policies are not true externalities because they are mediated through a competitive price, the global interest rate. There is no more reason to coordinate macroprudential policies than, say, to coordinate competitive producers and consumers in a general equilibrium model because their supply and demand affect the prices of goods.

The fact that macroprudential policies tend to lower the global interest rate can become problematic when there are nominal frictions, however. Macroprudential policies tend to depress demand, an effect that monetary policy may be unable to offset because of the zero-bound constraint on the nominal interest rate. I present a Keynesian extension of the model in which the uncoordinated use of macroprudential policies can push some or all countries into a liquidity trap with a positive level of unemployment. In such a situation, there is scope for Pareto-improving coordination of macroprudential policies. The countries with unemployment benefit from a coordinated relaxation of their macroprudential policies that raises global demand.

Finally, I study the scope for the international coordination of monetary policy and macroprudential policy. I present a specification of the model in which one country (the United States) is in a liquidity trap with unemployment while the rest of the world (China) attempts to mitigate the effects of the U.S. monetary stimulus by a prudential accumulation of reserves. I find that there is again a case for international coordination, leading both countries to be less aggressive in the pursuit of their objectives.

The paper is structured as follows. Section 2 presents a selective review of the literature. Section 3 presents the model and compares domestic macroprudential policies and prudential capital controls. Sections 4 and 5 look at the case for the international coordination of prudential capital account policies, respectively assuming full employment and less than full employment. Section 6 concludes with a brief discussion of the policy implication of my analysis for the international community.

2. Literature

This paper belongs to a rapidly growing literature on financial externalities and regulation. One important part of this literature focuses on the banking

sector, where there was a shift in emphasis from the microprudential regulation to the macroprudential regulation of banks (see Hanson, Kashyap, and Stein 2011 and Galati and Moessner 2013).[3] In a nutshell, macroprudential regulation focuses on how the collective behavior of banks makes the financial system riskier, whereas microprudential regulation focuses on individual banks' risk of insolvency, taking their financial environment as given.[4]

From a theoretical perspective, the shift toward macroprudential regulation has been justified by the need to address certain *externalities* that lead to financial amplification in a crisis. Two externalities have received most of the attention in the literature.[5] The first one is related to the interconnectedness between financial institutions that stems from the network of claims and liabilities across institutions. A shock to a given institution may propagate itself to a large number of other institutions through a domino effect, including those that are not directly linked to the bank at the origin of the shock. Banks do not internalize their contribution to the propagation of systemic risk when they contract with other banks, which leads to a network that may be excessively fragile.

The second externality is related to the fire sales that occur when all banks try to deleverage by selling the same assets at the same time. Ex post (in the crisis), banks do not internalize that selling an asset drives other banks into insolvency by depressing the asset's price. Ex ante, they do not take into account the contribution of their own leverage to systemic risk induced by fire sales.

In theory, the most direct and natural policy instrument to address an externality is a Pigouvian tax. Some analyses of macroprudential banking regulation indeed take Pigouvian taxation as a theoretical benchmark, and some measures that were recently implemented or proposed take the form of taxes on certain banking activities. For example, Shin (2010) and Perotti and Suarez (2011) proposed using a tax on banks' noncore liabilities as a tool for prudential regulation, and such a tax was introduced in Korea in August 2011. But overall, the macroprudential regulation of banks relies on the traditional quantity-based instruments of banking regulation.[6]

Macroprudential policy is often taken to mean the macroprudential regulation of *banks*, especially in central banking circles, but it is important to realize that the externalities that justify the use of macroprudential policies work not only in the banking sector but also are relevant in the real sector. For example, the evidence in Mian and Sufi (2009) suggests that one important reason behind the large and persistent fall in U.S. demand after the banking crisis was excessive leverage in the *household* sector.

The recent theoretical literature on Fisherian "debt deflation" has studied how the type of externalities that have been invoked to justify macroprudential

regulation of banks can also lead to excessive leverage in the real sector. For example, in a residential real estate bust the fact that households are credit-constrained puts further pressure on house prices. The feedback loop that this generates is very similar to the fire sale mechanism in the banking literature. This mechanism is analyzed in the three-period model of Lorenzoni (2008) and more dynamic quantitative contributions can be found in Jeanne and Korinek (2010b) and Bianchi and Mendoza (2010).

Another transmission mechanism involves aggregate demand. For example, the model in Jeanne (2013a) features an economy in which firms produce inputs that are complementary in the production of the consumption good. As a result default may be contagious. Sectoral shocks that make the producers of certain inputs insolvent lower the price of complementary inputs and may draw the producers of those other inputs into default. There is excessive borrowing under laissez-faire because each firm does not internalize the impact of its debt on the default risk of other firms.

Similar arguments can be developed in the open economy. A boom in capital inflows is associated with a real appreciation of the domestic currency, which increases the internationally acceptable collateral on the basis of which domestic agents can borrow abroad. The problem is that booms in capital inflows are often followed by "sudden stops" à la Calvo (1998), in which exactly the same amplification mechanisms work in reverse. The sudden capital outflow is associated with a depreciation of the currency and a decline in the foreign-currency price of domestic assets.

One strand of recent theoretical literature examines whether prudential capital controls are desirable from the perspective of improving the overall domestic welfare of an emerging market economy when there are booms and busts in capital flows (Korinek 2010, 2011, Jeanne and Korinek 2010a, and Bianchi 2011). The optimal policy is a Pigouvian tax on capital inflows that makes private market participants internalize their contributions to systemic risk in order to restore the efficiency of the decentralized market equilibrium.[7]

Consistent with the recent theoretical literature, I adopt in this paper a broad view of macroprudential policy which is not limited to banking regulation. I define macroprudential policy as a system of Pigouvian taxes (or equivalent quantity-based measures) that aim at reducing excessive leverage in a boom, whether it takes place in the banking sector or the real sector. In the open economy, macroprudential policy can be implemented through the management of international reserves.

Unlike for trade or monetary policies, where the welfare benefits of international cooperation have been studied extensively, there has been relatively

little research on the international coordination of macroprudential policies. In a recent contribution, Korinek (2012) shows in a model similar to the one presented here that international cooperation is not justified if small countries use prudential capital controls to redress domestic externalities. In another, Bengui (2012) studies the scope for international coordination in an open-economy version of the Hölmstrom and Tirole (1998) model of public liquidity provision. He finds that the uncooperative equilibrium between national regulators is inefficient as national regulators do not internalize the benefits of their country's provision of liquidity to the rest of the world.[8]

Let me conclude the discussion of the literature by emphasizing two things that this paper is *not* about.

First, this paper does not address the effectiveness of macroprudential policies when the private sector attempts to circumvent them. There is evidence that the private sector makes such efforts, but the empirical literature suggests that they are not entirely successful—although they may constrain the set of effective policies. Existing empirical research finds that the macroprudential regulation of banks has been effective at least in some ways. Based on aggregate data, Lim et al. (2011) and Dell'Ariccia et al. (2012) find evidence of some macroprudential policies being effective in reducing the procyclicality of credit and leverage.[9] Similar results have been obtained in the empirical literature on capital controls.[10] I ignore problems related to the avoidance of macroprudential measures.

Second, this paper does not discuss the international coordination required to close the gaps that come from international arbitrage between regulators. Traditional arguments for international coordination of banking regulation are the need to maintain a level playing field for banking competition and to avoid regulatory races to the bottom. These arguments also apply to the macroprudential part of banking regulation. But the fact that booms and busts are often country-specific generates a new tension because macroprudential regulation may have to be restricted in some countries and not others. A problem arises when the macroprudential regulation of banks is used to contain excessive leverage in the real sector. In a financially integrated world, borrowers who see the cost of borrowing from the domestic banking sector increase because of a macroprudential restriction can borrow from foreign banks, either directly (for the largest corporate borrowers) or through their domestic branches (if they are not subject to domestic macroprudential regulation).[11] This problem is especially salient in the euro area, where country-specific macroprudential regulation is more important than elsewhere to fulfill the stabilizing role that monetary policy can no longer play at the national level, and at the same time

banking integration is an explicit objective. However, in this paper I will consider this problem solved by assuming that the domestic policymaker can tax borrowing by domestic agents irrespective of the residency of the lender.[12]

3. Macroprudential Policies in a Small Open Economy: A Simple Model

The key concept in the literature reviewed in the previous section is that of externality. There is excessive borrowing in a boom because debt has social costs that are not internalized by the borrowers. The uninternalized social costs of borrowing can be modeled in several ways, but they all boil down, in reduced form, to the existence of a wedge between the private return and the social return on borrowing. I present in this section a model, based on Jeanne (2013b), that captures this idea in a simple reduced-form way. Note that the model is not specifically about banking, although one could view it as a model of banking by interpreting the borrowers as bankers who make loans to the real sector. Thus the macroprudential policies discussed in this section are not limited to the macroprudential regulation of banks.

3.1. Assumptions

The model has two periods. Lending and investment take place in the first period and repayment takes place (or not) in the second period. The model is completely real (there is no money) and it features one single good which is used for both investment and consumption.

The assumptions about the lenders are simple and standard. The country has a mass of identical lenders who are endowed with the country's GDP, denoted by Y, in the first period. The lenders maximize their utility, U, which is the sum of a concave function of their first-period consumption, C, plus the expected value of their second-period consumption, C'

$$U_l = u(C) + E(C').$$

The lenders lend their saving, $S = Y - C$, at the riskless interest rate, r. If capital is perfectly mobile this interest rate is equal to the world riskless interest rate, r^* (taken as exogenous for now). In general, r could be higher or lower than r^* because of restrictions to international capital mobility.

The lenders save until the marginal benefit of saving is equal to the marginal cost, $u'(Y - S) = 1 + r$, which implies that saving is an increasing function of the real interest rate,

$$S = S(r), \quad S'(\cdot) > 0.$$

The mass of lenders is normalized to 1, so that S represents both the saving of an individual lender and the country's aggregate saving.

The borrowers are identical atomistic entrepreneurs (or firms) who need funds to finance investment projects. A given entrepreneur invests a quantity I of good in the first period in the hope of receiving a quantity $f(I)$ of good in the second period. There are decreasing marginal returns to investment, i.e., function $f(\cdot)$ is concave. The investment is risky because the payoff $f(I)$ is obtained with a probability p that is in general lower than one. With probability $1-p$ the investment yields nothing. Although this is not crucial for the results, I will assume that this risk is perfectly correlated across firms, i.e., there is a "good" aggregate state in which all firms have a high payoff and a "bad" aggregate state in which they all have a zero payoff. The bad state will lead to a systemic debt crisis.

The borrowers have no funds in the first period, implying that the investment is entirely financed with debt, $D = I$. If the borrowers are unable to repay their debts in the second period because of a zero payoff, they default and the lenders receive nothing. Because of this risk the borrowers must pay a default risk premium: They promise a repayment of $(1+r)D/p$ to the lenders.

The borrowers consume in the second period only (for example because the agency cost of debt deters them from borrowing to finance first-period consumption). The borrowers, thus, simply maximize the expected level of their second-period consumption,

$$U_b = E(C').$$

Similar to lenders, the mass of borrowers is normalized to 1. Domestic welfare is the sum of the welfare of lenders and borrowers, $U = U_l + U_b$.

The following assumption is key in generating systemic risk. I assume that the expected payoff of an investment is a decreasing function of the aggregate level of debt,

$$p = p(D), \quad p'(\cdot) < 0. \tag{1}$$

Note that in this expression D is the *aggregate* level of debt rather than the debt of an individual entrepreneur. This assumption generates the externality leading to systemic risk: Individual borrowers do not take into account the impact of their borrowing on the risk of default for the other borrowers. It can be viewed as a reduced form for the microfounded model of contagion in systemic debt crises presented in Jeanne (2013b). In that model, as mentioned in the previous section, entrepreneurs produce inputs that are complementary in the production of the consumption good. As a result default may be contagious because of a

demand externality. Sectoral shocks that make the producers of certain inputs insolvent lower the price of complementary inputs and may draw the producers of those other inputs into default. Assuming that the probability of default of a given entrepreneur is a function of the aggregate level of debt, as we do in equation (1), is a simple reduced form for this mechanism.[13]

Essentially, the model captures the idea that some expenditures generate negative externalities because they are financed by debt. There is nothing essential to the assumption that productive investment is debt-creating whereas consumption is not. In the following one can think of I and C as notations for expenditures that are financed relatively more by debt and by cash, respectively.

3.2. Domestic Macroprudential Regulation

It is easy to see how laissez-faire may lead to overborrowing. Since the representative borrower repays $(1+r)I/p$ with probability p, his expected repayment is $(1+r)I$ and his ex ante utility is given by,

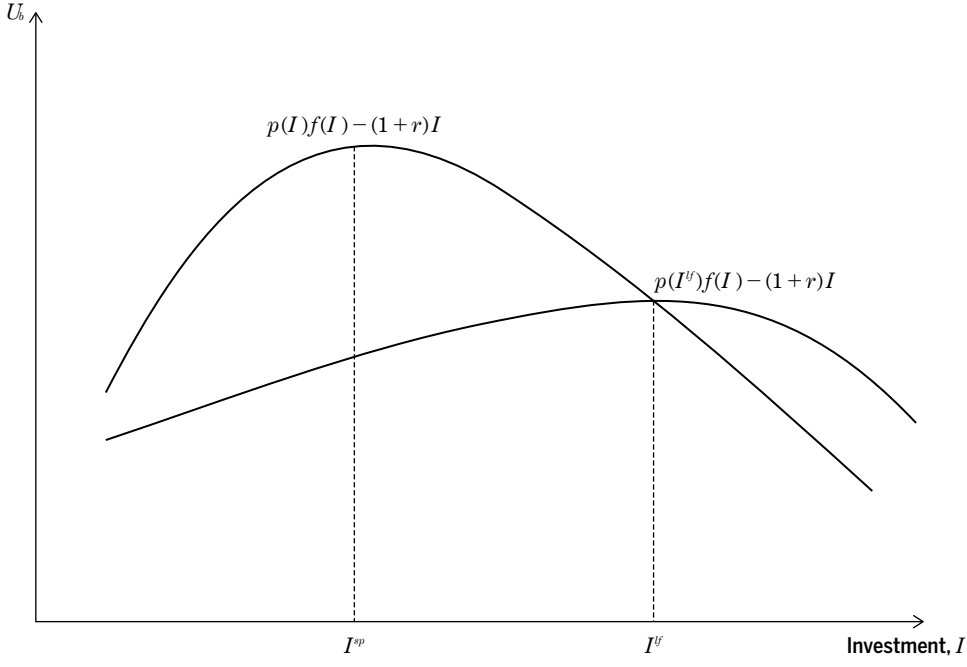
$$U_b = pf(I) - (1+r)I.$$

The entrepreneur borrows until the marginal benefit is equal to the marginal cost of borrowing, $pf'(I) = 1+r$, which implicitly defines the demand for loans as a decreasing function of the real interest rate. The equilibrium level of debt under laissez-faire satisfies $p(I^{lf})f'(I^{lf}) = 1+r$.

The level of borrowing is excessive under laissez-faire because individual borrowers do not internalize that the probability of a systemic debt crisis depends on the aggregate level of debt. A benevolent social planner, by contrast, would take this effect into account and maximize $p(I)f(I) - (1+r)I$. The difference between laissez-faire and the social planner solution is shown in Figure 1. At the laissez-faire equilibrium level of debt and investment, $I = I^{lf}$, the ex ante welfare of borrowers is increased by marginally reducing the investment level in order to reduce the probability of the state in which all the borrowers default (a systemic debt crisis). The social planner, thus, would pick a level of investment, I^{sp}, that is lower than under laissez-faire. This is also the level of investment that maximizes ex ante domestic welfare, $U = U_l + U_b$, since in this simple model the welfare of lenders is not affected (ex ante) by the risk of a systemic debt crisis.[14]

Figure 2 shows the Metzler diagram for this simple economy. The figure shows, on the horizontal axis, the level of investment and saving, and on the vertical axis, the gross marginal gain from investing and the gross marginal cost of saving. Under perfect capital mobility and laissez-faire both the marginal

FIGURE 1
Borrowers' Welfare under Laissez-Faire and a Social Planner

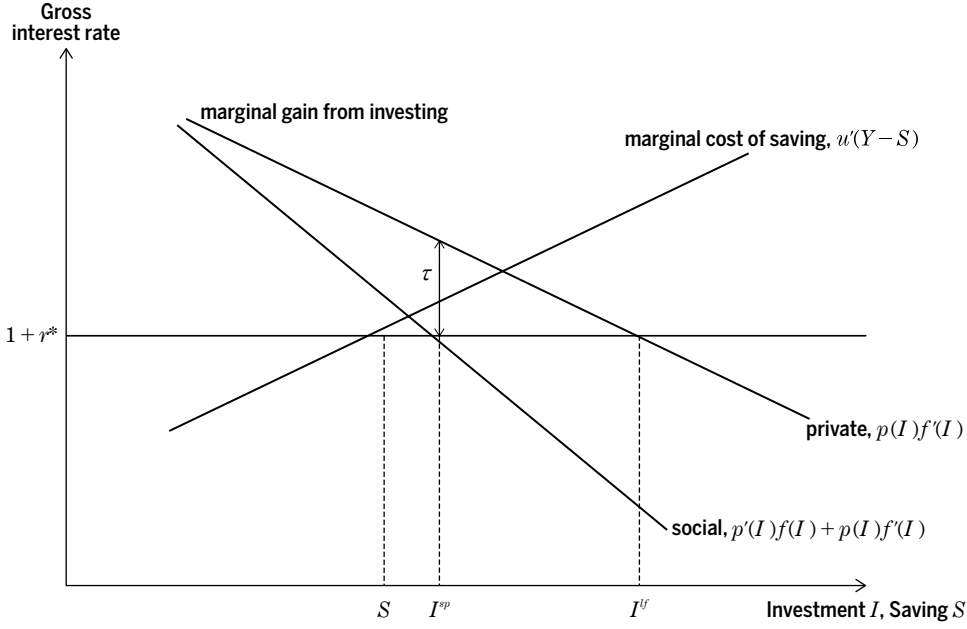


gain from investing and the marginal cost of saving must be equal to the gross cost of external borrowing, $1+r^*$. The difference between domestic saving and domestic investment, $S-I$, is the country's current account balance.

The main difference with the textbook Metzler diagram is that in the presence of systemic debt externalities, the social marginal gain from borrowing is lower than the private marginal gain. The difference, $p'(I)f(I)$, reflects the impact of aggregate debt on systemic risk. As a result the social planner would like to reduce domestic investment below the laissez-faire level, which means—domestic saving being unchanged—that the country's current account balance must increase. The figure illustrates the case where the social planner reduces a current account deficit that remains positive. But in general, the intervention of the social planner could also reverse the sign of the current account balance and transform a capital-importing country into a capital exporter.

What policy instrument can the social planner use to achieve the optimal level of borrowing and investment? The most direct policy instrument is a Pigouvian tax on domestic borrowing equal to the wedge between the private return and the social return (labeled τ in Figure 2).[15] The proceed of the tax can

FIGURE 2
Metzler Diagram with Systemic Debt Externalities



be rebated in such a way that both the borrowers and the lenders benefit. To the extent that the tax is imposed on domestic borrowing irrespective of the residency of the lender, this policy should be interpreted as domestic macroprudential policy rather than a capital control.

More formally, let us assume that the tax increases the riskless cost of borrowing from r^* to $r^*+\tau$.[16] The level of debt and investment in the decentralized equilibrium is now given by

$$p(I)f'(I) = 1 + r^* + \tau.$$

This coincides with the level of debt and investment chosen by the social planner,[17] which satisfies $p'(I^{sp})f(I^{sp}) + p(I^{sp})f'(I^{sp}) = 1 + r^*$, if the tax is set at

$$\tau = -p'(I^{sp})f(I^{sp}).$$

That is, the optimal Pigouvian tax on domestic borrowing is equal to the marginal loss in expected output from the systemic risk caused by a marginal increase in aggregate debt.

How does the optimal domestic macroprudential tax vary in the cycle? For simplicity I will consider the case where the cycle is induced by variations in

the cost of foreign borrowing, r^*.[18] As can be seen from Figure 2, a lower cost of external borrowing is associated with more investment both under laissez-faire and under the social planner. It also leads to an increase in the optimal Pigouvian tax on domestic borrowing if the difference between the private marginal gain and the social marginal gain from investing increases with the level of investment, that is, if $-p'(I)f(I)$ is increasing with I. In this case the domestic macroprudential policy is countercyclical, in the sense that it is used to smooth investment, domestic borrowing, and capital inflows against variations in the cost of foreign borrowing.

Domestic macroprudential regulation leans against the ebbs and flows of international capital movements, whether they are caused by variations in global interest rates or in risk premia. Hereafter we will assume that this is true by making the following assumption:

Assumption 1. *The marginal cost of the systemic risk increases with the level of debt, i.e., $-p'(D)f(D)$ is increasing with D.*

3.3. Prudential Capital Account Policies

If the economy is receiving capital inflows, another way that the social planner can reduce lending to the socially optimal level is by imposing a tax on *external* borrowing, i.e., on lending from nonresidents to residents. Because the tax is differentiated by the residency of the lender, it is a capital control of the type, for example, that Brazil has been using since 2009. The tax on external borrowing raises the (riskless) interest rate at which domestic borrowers can borrow from r^* to $r^*+\tau$ and so has exactly the same impact on domestic borrowing and investment as the domestic macroprudential tax considered in the previous section. However, the capital control tax also increases the interest rate for domestic savers. At the margin, domestic borrowers can borrow from domestic lenders, and they will do so until the same interest rate is paid to domestic and foreign lenders.

More formally, let us consider a capital-importing country, i.e., a country for which the autarkic interest rate r^a (defined as the level of interest rate that equalizes domestic saving and domestic investment) is higher than the external cost of borrowing r^*. As long as the tax on capital inflows τ is lower than the difference between the autarkic interest rate and the external cost of borrowing, the country imports capital and increasing the tax on capital inflows raises the domestic interest rate one-for-one. When the tax rate reaches $r^a - r^*$, however, the country is in financial autarky and raising the tax further has no impact on the domestic interest rate, which remains equal to the autarkic level r^a. If the

social planner wants to increase the domestic cost of borrowing to a level r that is above r^a, he must *subsidize* capital outflows at rate $\tau = r - r^*$. In the following, τ is a wedge that will be interpreted either as a tax on capital inflows or a subsidy on capital outflows depending on the sign of the current account balance.

The use of subsidies on capital outflows does not seem to be widespread in the real world, but the capital account policies of countries such as China illustrate how the same outcome can be achieved with policies that affect quantities rather than prices (see Jeanne 2013a).[19] To simplify, China's capital account is closed to most capital inflows except foreign direct investment (FDI) whereas most of the accumulation of foreign assets takes the form of foreign exchange reserves at the central bank. As a counterpart to the purchase of foreign reserves the domestic banking sector, which is largely controlled by the government, produces domestic assets that must be purchased by residents since nonresident investors do not have access to these assets. The domestic interest rate, thus, must adjust to the level that makes resident investors willing to hold the domestic assets backing up the reserves. Essentially, the Chinese authorities divert a fraction of domestic saving—which would otherwise be lent domestically through the banking system—into the accumulation of foreign exchange reserves (Jeanne 2013a).

In the context of my simple model, the impact of Chinese-style reserve accumulation can be captured in a simple way by assuming that the capital account is closed, so that the current account balance $B = S - I$ is equal to the accumulation of reserves by the authorities. Then in equilibrium the domestic interest rate r has to adjust to a level such that the domestic lenders are willing to save B in excess of the domestic demand for loans,

$$B = S(r) - I(r).$$

If the level of reserve accumulation B is higher than the level of net foreign assets that would be observed in the absence of capital account restrictions, this policy raises the domestic interest rate above the world level, $r > r^*$. The equilibrium is effectively the same as if the authorities had imposed a subsidy $r - r^*$ on capital outflows.

How do prudential capital account policies compare with domestic macroprudential regulation in terms of welfare? Since the underlying externality affects domestic investment, not domestic consumption, it is inefficient to change the levels of both consumption and investment. Capital account policies affect all expenditures alike, including those that do not generate externalities. The impact of a tax on external borrowing has a welfare-enhancing

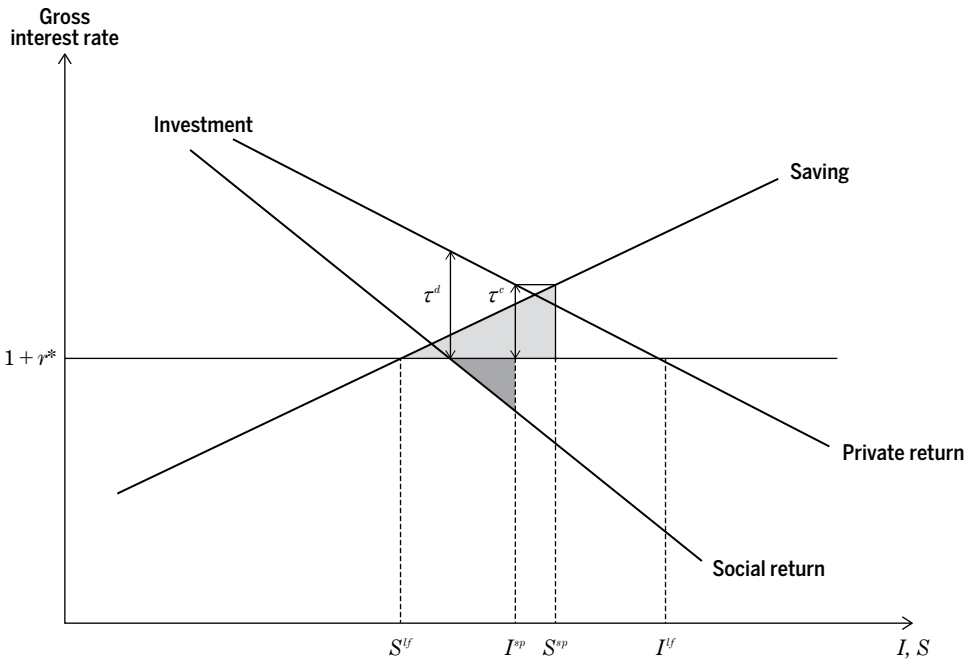
effect on domestic borrowing, but its impact on domestic saving is distortive. In this model, thus, prudential capital controls are a second-best instrument—the first-best instrument is domestic macroprudential regulation.

As a result, the optimal tax on capital inflows is lower than the optimal domestic macroprudential tax. To distinguish between the two types of taxes, let us denote by τ^c the tax on capital inflow, as opposed to τ^d the domestic macroprudential regulation tax. Figure 3 shows the impact of the capital control tax on the equilibrium. Unlike the domestic macroprudential tax, the capital control tax raises the level of saving. This implies that a given level of tax has a larger impact on the current account balance if it applies to external borrowing rather than domestic borrowing. Figure 3 illustrates a case where the optimal capital flow tax transforms a capital-importing country into a capital-exporting country, whereas the optimal tax on domestic borrowing would not.

In addition, the figure shows the welfare loss from excessive borrowing (the lower triangle) as well as the welfare loss due to the distortion of saving (the upper triangle). The tax on external borrowing τ^c is at the optimal level when it minimizes the total welfare loss (the sum of the areas of the two triangles).

FIGURE 3

Lending and Saving in the Open Economy: The Case of Capital Controls



It is easy to see that the optimal tax on external borrowing is lower than the optimal tax on domestic borrowing. If τ^e were set at the same level as τ^d, the lower triangle would disappear but the upper triangle would be much larger. Then there would be a first-order gain (in terms of higher consumption) but a second-order cost (in terms of higher crisis risk) from marginally reducing τ^e below τ^d. Intuitively, capital controls should be used less aggressively than domestic macroprudential regulation because they come with a collateral cost: They distort non-debt-creating expenditures at the same time as they correct debt-creating expenditures.

The cyclical properties of the optimal tax on external borrowing are not necessarily the same as for the tax on domestic borrowing, but one can ensure that the optimal capital controls are countercyclical at the cost of an additional assumption. To understand this, it is useful to introduce the country's total expenditures,

$$E = C + I. \tag{2}$$

Using a tax on foreign borrowing implies that the marginal utility of consumption must be equal to the gross private marginal return on investment,

$$u'(C) = p(I)f'(I). \tag{3}$$

This constraint implies that investment and consumption are positively related in equilibrium: With capital controls it is impossible to reduce investment without also repressing consumption. Together equations (2) and (3) make it possible to write consumption and investment as a function of total expenditure, *C(E)* and *I(E)*. Then it is possible to show that the optimal tax on external borrowing is countercyclical (i.e., smooths the domestic cost of borrowing against variations in the cost of external borrowing) if and only if the following assumption is satisfied.

Assumption 2. *The marginal cost of systemic risk increases with the level of domestic expenditures, i.e.,* $-p'(I(E))f(I(E))I'(E)$ *is increasing with E.*

This assumption is the analog of Assumption 1 for the case of capital controls. The social planner targets the *total* level of expenditures because he can no longer target the level of debt-creating expenditures (investment) separately. Assumption 2 ensures that the optimal tax on external borrowing varies inversely with the external cost of borrowing. Assumptions 1 and 2 are independent (neither one implies the other), but it is not difficult to find specifications of the model in which they are both satisfied. Appendix B presents a quadratic specification of the model in which both assumptions are satisfied and closed-form expressions for the main variables can be derived.

Our main results are summarized below.

Result 1. *Consider a small open economy in which domestic borrowing may be excessive because of a systemic risk externality. The first-best policy instrument is a macroprudential tax on domestic borrowing. A second-best instrument is a macroprudential tax on external borrowing. Under Assumptions 1 and 2, both taxes should be used in a countercyclical way to smooth the domestic cost of borrowing against variations in the external cost of borrowing.*

Proof. See Appendix A.

This analysis raises the question of why governments should ever use prudential capital controls since the first-best instrument is domestic prudential regulation. There are several possible answers to this question.[20]

First, discriminating between the transactions involving residents and nonresidents may be justified if nonresident investors contribute more to systemic risk than resident investors in a crisis. For example, short-term debt could be systemically more dangerous in the hands of nonresident investors if they have a stronger tendency to rush out of a crisis than resident investors. There is evidence that this was the case in the 2008 crisis, when investors tended to retrench on their own countries' assets (Forbes and Warnock 2012). These factors are not explicitly captured by my simple model but they may be important in the real world. In the model, systemic risk is determined by the level of D irrespective of the residency of the debt holders. But one could decompose total debt into the component held by residents (D^h) and the component held by foreigners (D^f) and assume that p is more sensitive to D^f than to D^h.[21]

Second, the appropriate domestic macroprudential taxes may not be available as policy instruments. As discussed in the introduction, the externalities leading to systemic risk do not necessarily all take place inside the banking sector. Thus the scope of macroprudential regulation may be too narrow if it is limited to banks. Although broader macroprudential taxes can in principle be used, they are determined in the context of a political process that makes it unlikely that they will be used according to Pigouvian principles.[22] Capital controls may be the only broad tax-like instruments that are somewhat sheltered from the political process.

Finally, policymakers might have to rely on a wide range of instruments (including second-best ones) because exclusive reliance on a narrow set of instruments may encourage avoidance and circumvention efforts by the private sector. In this case, there could be a maximum level for τ^d, above which there will be excessive avoidance, and at the margin τ^c must be used.

For these reasons, there might be a case for using prudential capital controls as a second-best instrument. In the rest of the analysis, thus, I will assume that countries use capital controls and domestic macroprudential policies.

4. International Spillovers and "Capital Wars"

I now consider a world composed of a large number of small open economies like the one described in the previous section. The countries are indexed by $j \in J$. The global capital market finds its equilibrium for an interest rate r^* such that

$$\sum_{j \in J} S_j(r^* + \tau_j^c) = \sum_{i \in J} I_j(r^* + \tau_j^d + \tau_j^c),$$

where τ_j^d and τ_j^c are country j's taxes on domestic borrowing and external borrowing, respectively. This equation endogenizes the equilibrium global interest rate, r^*, as the level for which global investment is equal to global saving.

It is easy to see that prudential taxes on domestic or external borrowing have international spillovers. All else equal, raising the domestic macroprudential tax in country j lowers the global demand for investment and so the global interest rate. Raising the capital control tax by the same amount lowers the global interest rate even more since it raises the global supply of saving at the same time as it lowers the global demand for investment. In both cases, the other countries respond to the lower global interest rate by increasing their tax rates on domestic or external borrowing. Intuitively, raising the macroprudential taxes in a given country deflects capital flows to the other countries, inducing them to raise their own macroprudential taxes. Macroprudential policies are strategic complements.

This raises the question of the efficiency of the equilibrium that is reached when all countries set their prudential taxes in an uncoordinated way. To answer this question in the context of the model I assume that countries belong to two groups that use different policy instruments: the countries in the first group use the tax on domestic borrowing ($j \in J^d$) whereas the countries in the second group use the tax on external borrowing ($j \in J^c$). The equilibrium global interest rate, then, satisfies

$$(4) \qquad \sum_{j \in J^d} S_j(r^*) + \sum_{j \in J^c} S_j(r^* + \tau_j^c(r^*)) = \sum_{j \in J^d} I_j(r^* + \tau_j^d(r^*)) + \sum_{j \in J^c} I_j(r^* + \tau_j^c(r^*)),$$

where $\tau_j^d(r^*)$ and $\tau_j^c(r^*)$ are the optimal tax responses discussed in the previous section.

The equilibrium level of the global interest rate is lower, and could be much lower than in the absence of macroprudential policies.[23] The Nash equilibrium

in tax policies may thus give the impression of a "capital war," in which countries are engaged in a self-defeating effort to export capital to the rest of the world. As first shown by Korinek (2012) in a similar context, however, the impression that the uncoordinated equilibrium is inefficient is misleading. The Nash equilibrium in macroprudential policies is efficient, as stated in the following result.

Result 2. *Consider a world composed of many small open economies such as the one analyzed in the previous section. Countries mitigate their systemic debt externalities using a macroprudential tax on either domestic borrowing or external borrowing. Then the Nash equilibrium in which each country independently sets its macroprudential tax yields the same allocation as the equilibrium in which all the taxes are set by a global social planner who maximizes global welfare.*

Proof. See Appendix A.

There is no need for the international coordination of macroprudential policies (whether purely domestic or involving the capital account) since the Nash equilibrium between domestic policymakers is Pareto-optimal. The capital war, in other words, is efficient.[24]

The reason for this result is that the international spillovers associated with the use of capital controls (or domestic prudential policies) do not constitute a true international externality. The spillovers countries impose on each other are mediated through a price (the real interest rate) in a perfectly competitive market so that the first welfare theorem applies to the decentralized equilibrium between countries in the same way as it applies between consumers in a general equilibrium model. Each domestic social planner is like a small agent in a competitive market.

An important caveat to this result will be presented in the next section when we look at the case with Keynesian unemployment. But before we proceed with a Keynesian version of the model, other caveats are in order.

First, the results would be different in the presence of cross-country systemic debt externalities. Going back to the microfoundations of the model, one could assume that the consumption good is produced with production inputs from different countries, making default contagious across countries and not only across firms in a given country. This would make it optimal to coordinate national social planners to internalize the cross-country externalities. The point made by Result 2, from this perspective, is that it is not enough to point to cross-country spillovers to justify international policy coordination: One must show that the spillovers involve a true externality.

Second, the uncoordinated use of capital controls increases global welfare less than the uncoordinated use of domestic prudential policies. In fact, it is

easy to construct an example where the uncoordinated use of capital controls does not change global welfare at all. Assume that all countries set their taxes on external borrowing in the same way, i.e., they have the same tax response function $\tau^c(r^*)$. Then the global interest rate must satisfy,

$$\sum_j S_j(r^* + \tau^c(r^*)) = \sum_j I_j(r^* + \tau^c(r^*)).$$

It follows that the equilibrium cost of borrowing, $r^* + \tau^c(r^*)$, must be the same as the level of the interest rate that would be observed in the equilibrium without macroprudential taxes. The uncoordinated use of capital controls, thus, is self-defeating in the sense that it leads to exactly the same allocation (and the same level of welfare) as if no capital control were used. If the use of capital controls entailed some administrative cost on the side of governments, or costly circumvention effort on the side of the private sector, there would be a case for international coordination to reduce or save these costs, as noted by Ostry, Ghosh, and Korinek (2012).

Third, we have assumed so far a large number of countries. With strategic interactions between a small number of countries, the results are different. In the two-country model of Costinot, Lorenzoni, and Werning (2011), the borrowing country can raise its welfare relative to the laissez-faire level by restricting its borrowing and in this way lower the interest rate that it must pay to the lending country. Conversely, the lending country will want to restrict its lending to raise the world interest rate. The Nash equilibrium of this game leads to a Pareto inefficient "capital war" in which both countries see their welfare decreased. This is essentially the transposition to intertemporal trade of the classical optimal tariff argument for free trade.

5. Capital War in a Global Liquidity Trap

The previous section showed that a capital war was efficient even though it depressed the level of the global interest rate. The model, however, may have missed an important reason why depressing the real interest rate is problematic in the real world: the existence of a zero bound on the nominal interest rate that prevents the real interest rate from reaching the full-employment level. Could a capital war be costly and inefficient because it leads to unemployment in a global liquidity trap?

One cannot study this question without making significant changes in the model. Instead of looking at a real endowment economy, I now introduce money and nominal rigidities and make output endogenous. With the new model in hand, I re-examine the case for international coordination of macroprudential policies. Finally, I look at the scope for international policy coordination in

an asymmetric world with one country (interpreted as the United States) that attempts to implement a monetary stimulus in a liquidity trap whereas countries in the rest of the world set their macroprudential policies to limit the spillovers from the monetary stimulus.

5.1. A Keynesian Model

The model is made Keynesian in the most simple way possible. In particular, I consider a world with only one currency to focus on the interactions between national macroprudential policies rather than between monetary policies. This could also be interpreted as a world with fixed exchange rates (not necessarily a bad approximation if one thinks of the policy interactions between, say, the United States and China.) The nominal price of the good in terms of the global currency is denominated by P.

One key difference with the model used so far is that first-period output is now endogenous and can be demand-determined. Each country produces output with labor according to the production function $Y_j = g_j(L_j)$ where $g_j(\cdot)$ is increasing and concave. The demand for labor from the first-order condition,

$$g_j'(L_j) = \frac{W_j}{P},$$

where W_j is the nominal wage in country j and P is the nominal price of the good (the same in all countries by the law of one price).

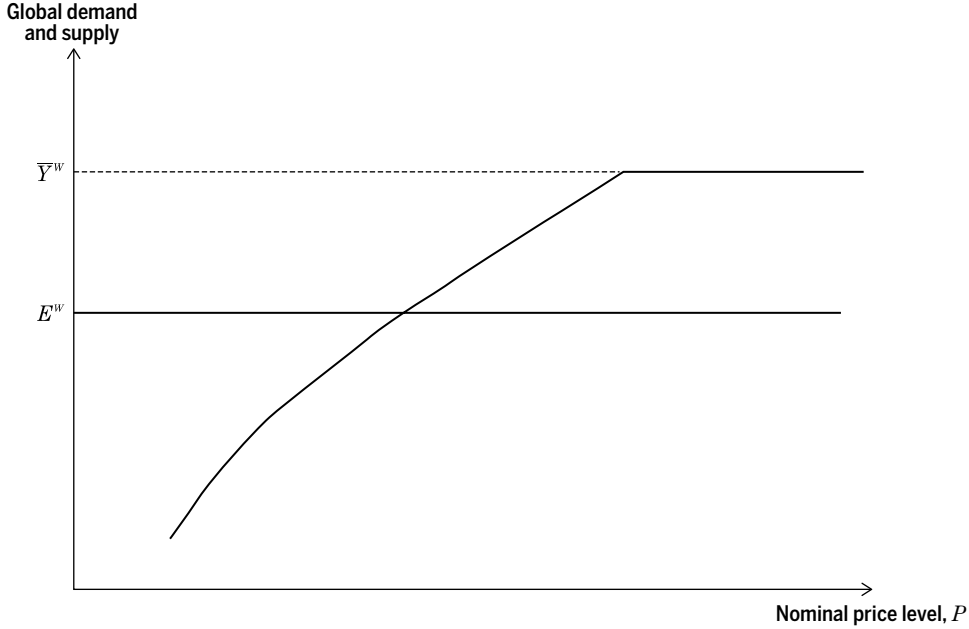
I assume that the nominal wage is rigid downward in the same way as in Schmitt-Grohé and Uribe (2012). The total quantity of labor used in the economy cannot increase above a level corresponding to full employment, whereas the nominal wage cannot fall below a level that is predetermined for each country,

$$L_j \leq \overline{L}_j, \quad W_j \geq \underline{W}_j.$$

A given economy can then be in two regimes. Either there is full employment and $W_j = g_j'(\overline{L}_j)P$, or there is less than full employment and $W_j = \underline{W}_j$. Which regime the economy lands in depends on how the global price of the good compares with the country's nominal wage. Country j has full employment if $P \geq \underline{W}_j / g_j'(\overline{L}_j)$. Once full employment is achieved, increases in the world price level are reflected one-for-one in domestic wages because wages are flexible upward.

Figure 4 shows how global supply $Y^W = \sum_j g_j(L_j)$ varies with the nominal price level. An increase in the nominal price level raises supply by lowering the real wage, like in the textbook model of aggregate supply and aggregate demand. When the nominal price level falls below a certain threshold, there is unemployment in some countries. In general, the unemployment could be

FIGURE 4
Global Supply and Global Demand



spread across all countries (this would be true in the symmetric case where all countries are identical) or it would be concentrated in a few countries that have high nominal wages relative to their productivity.

The figure also shows the level of aggregate demand,

$$E^W = \sum_{j \in J^d} [C_j(r^*) + I_j(r^* + \tau_j^d)] + \sum_{j \in J^c} [C_j(r^* + \tau_j^c) + I_j(r^* + \tau_j^c)],$$

where $C_j(\cdot)$ gives consumption in country j as a function of the interest rate. Global demand is represented by a horizontal line in Figure 4 because it does not depend on the nominal price level. It is determined instead by the real interest rate as well as the macroprudential taxes, and it is decreasing in these variables. Global demand is equal to the full-employment level of global supply when the real interest rate is at the Wicksellian level.[25] Importantly, the macroprudential taxes depress global demand and thus lower the Wicksellian interest rate.

Finally, we need to specify how the real interest rate is determined. I assume that a global monetary authority sets the nominal interest rate i^*. The expected rate of inflation, denoted by π^*, is taken as exogenous, for example because it results from a credible inflation targeting mandate. Variations in the

nominal interest rate, thus, are reflected one-for-one in the real interest rate, $r^* = i^* - \pi^*$. In addition monetary policy is constrained by the usual zero bound on the nominal interest rate. Thus, one can think of the monetary authority as setting the real interest rate subject to the constraint,

$$r^* \geq -\pi^*.$$

5.2. The Benefits from International Coordination

We now return to the Nash equilibrium between the domestic social planners in charge of macroprudential policy. Like before I assume that each domestic social planner uses one macroprudential tax, which is either on domestic borrowing or on foreign borrowing. There is now one more player in the game between policymakers: the global monetary authority. For simplicity I assume that the global monetary authority maximizes global employment conditional on the inflation target π^*. In the next section I will address a case where the monetary authority maximizes the welfare of a particular country.

An equilibrium, then, is characterized by a set of macroprudential taxes on domestic borrowing, τ_j^d ($j \in J^d$), and on foreign borrowing, τ_j^e ($j \in J^e$), as well as a real interest rate r^* such that:

(1) the domestic social planner of each country j sets his macroprudential tax (τ_j^d or τ_j^e) to maximize domestic welfare, taking the macroprudential taxes of the other countries and the global real interest rate as given;

(2) the global monetary authority sets the real interest rate r^* to maximize global employment subject to the constraint $r^* \geq -\pi^*$ taking the countries' macroprudential taxes as given.

It is easy to see that now, a capital war can decrease the welfare of all countries. This is clear in the special case where all countries are identical and use capital controls. Then as we saw in Section 4 in the case of an endowment economy without nominal stickiness, the capital war decreases the real interest rate without changing the allocation. If it decreases the real interest rate from a level that is above $-\pi^*$ to a level that is below $-\pi^*$, the capital war will lead, in the presence of nominal stickiness, to a global liquidity trap with unemployment in all countries. All the countries would then benefit from an international agreement not to use capital controls, which would restore full employment.

The case for the international coordination of macroprudential policies is more general than that. It arises as soon as there is unemployment in some countries, as stated in the following result.

Result 3. *Assume that there is unemployment in some countries in the Nash equilibrium between macroprudential policymakers. Then a coordinated*

reduction in the macroprudential taxes of the countries with unemployment raises the welfare of those countries without affecting the welfare of the countries with full employment.

Proof. See Appendix A.

There is scope for Pareto-improving international coordination of macroprudential taxes in a world with unemployment. The intuition is that the countries with unemployment do not internalize the impact of their macroprudential taxes on global demand. Global demand creates a true international externality because each country, by imposing taxes on domestic or external borrowing, reduces the demand for the output of other countries in a way that is not mediated by a competitive price. At the margin, countries with unemployment suffer a second-order loss from lowering their prudential taxes but a first-order gain from an increase in global demand that raises their employment level. As for the welfare of countries with full employment, it does not change since the global real interest rate stays at the same level (it is equal to minus the inflation target because the zero-bound constraint is binding).

Importantly, the scope for policy coordination does not include the countries with full employment. This is because (realistically) we have not allowed the countries that lose from a change in macroprudential policies to be compensated by international transfers. Otherwise the countries with unemployment would find it optimal to pay the countries with full employment to reduce their macroprudential taxes. The countries with full employment would suffer a second-order loss from slightly reducing their taxes, whereas the countries with unemployment would have a first-order gain from increasing their employment. But in the absence of transfer (or any other type of reward), there is no way that the countries with full employment can be induced to reduce their macroprudential taxes below the uncooperative level.

5.3. U.S. Monetary Stimulus vs. Chinese Reserve Accumulation

I now consider an application of the model to the equilibrium between monetary policy in one part of the world and prudential reserve accumulation in the rest of the world. In the wake of the Great Recession the monetary authorities in most advanced economies, after lowering their policy rates to levels close to zero, have resorted to unconventional forms of monetary stimulus such as quantitative easing or forward guidance. This induced global capital to move towards emerging market economies, which in response accumulated foreign exchange reserves and in some cases imposed restrictions on capital inflows. Does the model support the view that there is scope for efficient policy coordination in such a situation?[26]

The question can be addressed by specializing the model as follows. Two countries in the model are labeled "U.S." and "China." The global real interest rate is set by the U.S. to maximize its welfare. The capital account of China is closed except for the accumulation of foreign assets (reserves).[27] The accumulation of foreign reserves by China is denoted B_C. We consider a Nash equilibrium between the U.S. setting the interest rate r^* and China accumulating reserves B_C.

For simplicity I focus on equilibria in which there is full employment in China but not in the United States. There is less than full employment in the U.S. because of the lower bound on the real interest rate,

$$r^* \geq -\pi^*.$$

China accumulates reserves to contain the growth in domestic credit caused by U.S. monetary stimulus. For simplicity I assume that there is no debt externality in the United States.[28]

Given that there is full employment in China, its output is equal to $Y_C = g_C(\overline{L}_C)$. Setting foreign reserves B_C is equivalent to setting the level of domestic expenditures $E_C = Y_C - B_C$. Increasing Chinese reserves by one dollar reduces Chinese domestic expenditures by the same amount. Thus Chinese consumption and investment can be written as functions of reserves in the same way as in section 3.3., $C_C(B_C)$ and $I_C(B_C)$. Both consumption and investment are decreasing with B_C since reserve accumulation reduces domestic expenditures. The problem of the Chinese social planner can be written,

$$U_C = \max_{B_C} u_C(C_C(B_C)) + p_C(I_C(B_C))I_C(B_C) + (1 + r^*)B_C.$$

It then follows from the envelope theorem that the partial derivative of Chinese welfare with respect to the real interest rate is equal to the level of Chinese reserves,

$$\frac{\partial U_C}{\partial r^*} = B_C.$$

An increase in the interest rate raises Chinese welfare by increasing the return that it receives on its reserves.

As for U.S. welfare, it is given by,

$$U_{US} = u_{US}(C_{US}) + p_{US}I_{US} - (1 + r^*)B_C, \tag{5}$$

where we have used the fact that Chinese foreign assets are U.S. foreign liabilities. The probability of a high payoff on the investment does not depend on the level of debt because we have assumed away the debt externality in the U.S.

The U.S. social planner's problem, thus, is rather simple. If China accumulates a positive level of reserves ($B_C > 0$), the right-hand side of equation (5) is maximized when the real interest rate is minimized. It is thus optimal for the U.S. social planner to set the real interest rate at the lowest possible level subject to the zero-bound constraint, $r^* = -\pi^*$. At the margin, any increase in U.S. consumption or investment is "free" since it is produced by unemployed U.S. labor.

Given that the first two terms on the right-hand side of equation (5) do not depend on B_C, U.S. welfare is decreasing with Chinese reserves,

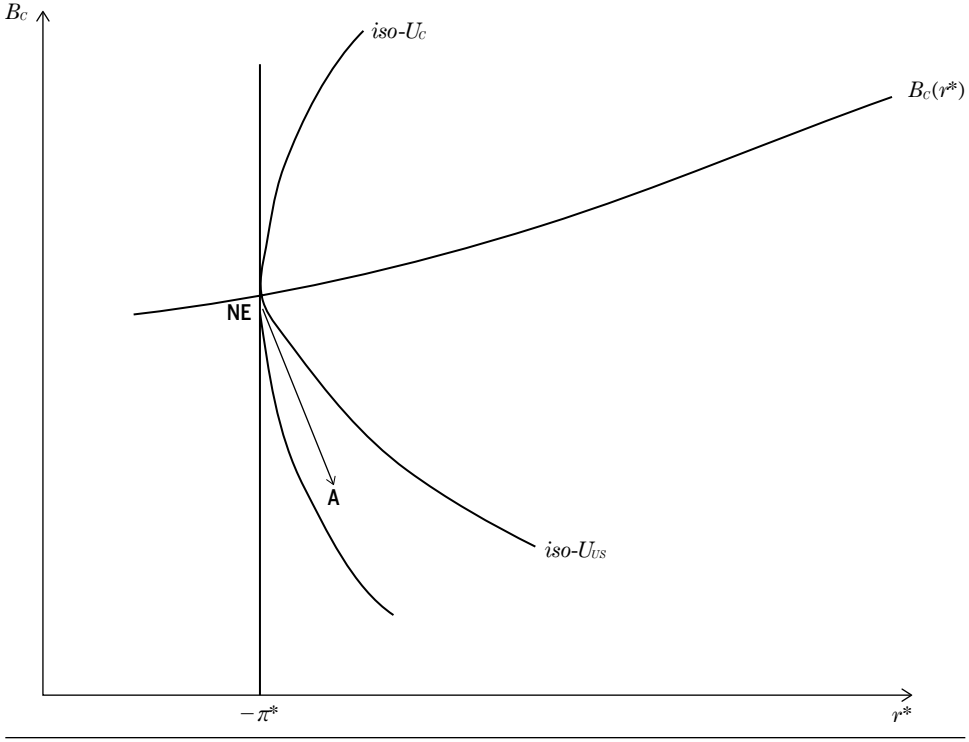
$$\frac{\partial U_{US}}{\partial B_C} = -(1 + r^*).$$

An increase in Chinese reserves lowers global demand and U.S. production by the same amount (since Chinese production does not increase at the margin). The gross interest rate appears on the right-hand side of the expression above because the U.S. borrows from China to cover the fall in its first-period income.

We are now ready to look at the case for international coordination. Figure 5 shows the policy instruments of the U.S. on the horizontal axis and China on the vertical axis. The curve labeled $B_C(r^*)$ shows how China's optimal level of reserves increases with the real interest rate. The vertical line corresponds to the optimal U.S. policy, which sets the real interest rate at the minimum level irrespective of what China does. The Nash equilibrium is at the intersection of the two countries' best response curves (the point labeled NE in the figure). As a condition for optimality the iso-welfare curve of China must be tangent to the vertical line. Finally, the figure shows the U.S. iso-welfare curve that passes through the Nash equilibrium. This curve is downward-sloping since an increase in the real interest rate that reduces U.S. welfare must be offset by a decrease in Chinese reserves that raises the demand for U.S. output.

The figure shows that the Nash equilibrium is not Pareto-optimal. Welfare for both the U.S. and China are increased by moving from the Nash equilibrium to a point such as A, where the U.S. sets a higher interest rate and China accumulates less reserves than in the uncooperative equilibrium. Then China benefits from receiving a higher return on its reserves, whereas the U.S. benefits from a higher level of Chinese demand. The U.S. suffers from raising its own interest rate, but it is always possible to make this cost smaller than the benefit that it receives from larger Chinese demand. This is because the cost incurred by China for raising its own demand is second-order since Chinese welfare was at its maximum in the Nash equilibrium. Thus China can be compensated for

FIGURE 5
Monetary Stimulus and Reserve Accumulation



increasing global demand by a very small (second-order) increase in the U.S. interest rate.

Result 4. *Assume that the model has two countries. One country (the U.S.) sets the global interest rate and has some unemployment because of the zero-bound constraint. The other country (China) has full employment and accumulates foreign reserves for prudential reasons. Then in the Nash equilibrium there is scope for Pareto-improving policy coordination in which the U.S. raises its interest rate and China lowers its reserve accumulation at the margin.*

Proof. See discussion above.

6. Conclusion

I have presented a simple framework that allows us to compare the welfare effects of domestic prudential policies and prudential capital account policies in a small open economy; analyze the general equilibrium effects of the

uncoordinated use of these policies; and explore the case for the coordination of macroprudential and monetary policies at the global level. The main conclusions are as follows: (1) Domestic prudential policies are generally preferable to capital controls, but realistic constraints on the use of the former may justify using the latter. (2) The fact that these policies have international spillovers does not per se imply that they should be subject to international rules or coordination. However, (3) International coordination may be justified if there is a global demand shortage.

The bottom line, thus, is that a case for the international coordination of macroprudential policies can be made, but it is not as robust or generic as one might expect. The case for coordination cannot be based merely on the existence of international spillovers and depends on the circumstances of global demand.[29] The case for coordination is stronger in a bust—when global resources are underutilized—than in a boom. This suggests that coordination should be run on an ad hoc basis and, when circumstances require, perhaps under the auspices of the Group of Twenty. Furthermore it will be difficult to involve the countries that have full employment in the coordination effort. Basic theory does not suggest that the international oversight of prudential capital control policies should be supported by the kind of permanent institutions that exist for international trade, such as the World Trade Organization.

APPENDIX A

Proofs

Proof of Result 1. The claims in Result 1 have been proven in the text except for the statement that the optimal tax on external borrowing is countercyclical under Assumption 2. Domestic welfare is equal to the utility that lenders derive from their first-period consumption, $u(C)$, plus the expected second-period income of lenders and borrowers, which is equal to the payoffs from the domestic investment and from the foreign assets,

$$U = u(C) + p(I)f(I) + (1 + r^*)(Y - C - I).$$

If the domestic social planner uses capital controls, consumption and investment can be written as functions of the level of domestic expenditure, E, with $C(E) + I(E) = E$. Differentiating domestic welfare with respect to the level of expenditure gives,

$$\frac{dU}{dE} = p(I(E))f'(I(E)) + p'(I(E))f(I(E))I'(E) - (1 + r^*).$$

To derive this expression we have used the first-order condition $u'(C) = p(I)f'(I)$ as well as $C'(E) + I'(E) = 1$. The first term on the right-hand side is the private marginal utility gain from increasing total expenditure, which by the envelope theorem is the same as if the marginal expenditure were spent on investment. The second term on the right-hand side is the social cost from increasing total expenditure, equal to the marginal increase in the probability of a systemic crisis due to higher debt.

Figure A1 is similar to Figure 2 but with total expenditures instead of investment on the horizontal axis. The marginal gain from increasing total expenditures is lower from a social perspective than from a private perspective. A social planner would choose a level of expenditures E^{sp} such that the social marginal gain is equal to the gross external cost $1 + r^*$. This can be achieved by a Pigouvian tax on external borrowing equal to

$$\tau^c = -p'(I(E^{sp}))f(I(E^{sp}))I'(E^{sp}).$$

As shown by Figure A1, a lower cost of external borrowing r^* increases total expenditures and, if Assumption 2 is true, also increases the optimal tax on external borrowing.

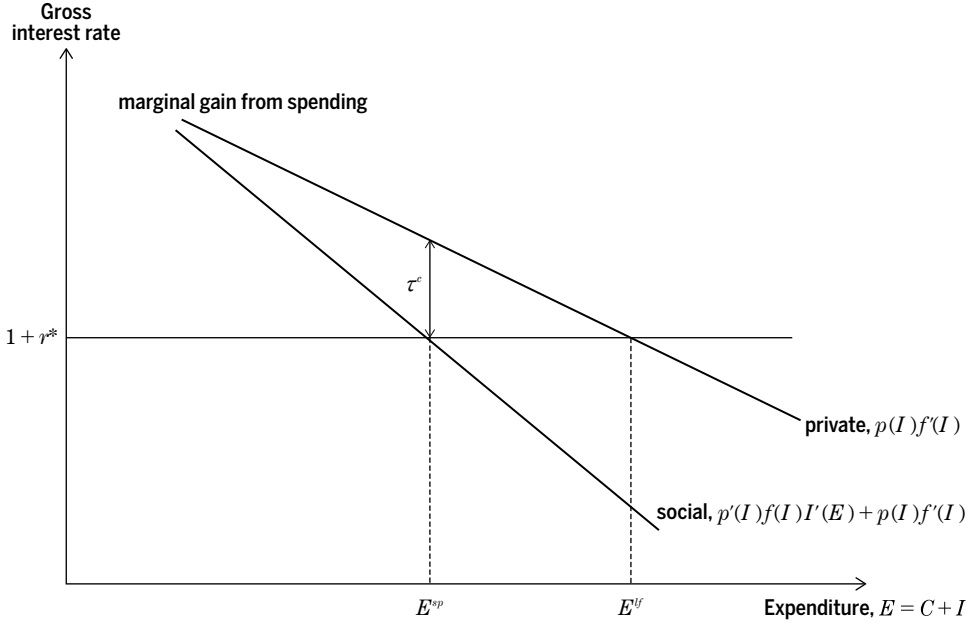
Proof of Result 2. The global social planner's problem is

$$\begin{cases} \max \sum_j [u_j(C_j) + p_j(I_j)f_j(I_j)] \text{ s.t.} \\ \sum_j (C_j + I_j) \leq \sum_j Y_j, \\ u_j'(C_j) \geq p_j(I_j)f_j'(I_j) \text{ for } j \in J^c. \end{cases}$$

The global social planner maximizes global welfare, which is equal to the sum of the utilities that lenders derive from first-period consumption plus the expected levels of second-period output. The first constraint is the global resource constraint. The second constraint is that the marginal utility of consumption cannot be smaller than the private marginal return on investment for the countries that use capital controls.

For the countries that use domestic macroprudential policies ($j \in J^d$), the first-order conditions are

FIGURE A1
The Optimal Level of Tax on Capital Inflows



$$u_j'(C_j) = \lambda,$$
$$p_j'(I_j)f_j(I_j) + p_j(I_j)f_j'(I_j) = \lambda,$$

where λ is the shadow cost of the global resource constraint.

For the countries that use prudential capital controls ($j \in J^c$), the first-order conditions are:

$$u_j'(C_j) = \lambda - \mu_j u_j''(C_j),$$
$$p_j'(I_j)f_j(I_j) + p_j(I_j)f_j'(I_j) = \lambda + \mu_j[p_j'(I_j)f_j'(I_j) + p_j(I_j)f_j''(I_j)],$$

where μ_j is the shadow cost of the constraint on the marginal utility of consumption.

The first-order conditions and the constraints are the same as in the Nash equilibrium with independent national planners, with $\lambda = 1 + r^*$. Hence a global social planner who maximizes global welfare chooses the same allocation as the one obtained in the Nash equilibrium between national social planners.

Proof of Result 3. We consider a Nash equilibrium with unemployment in some countries. In such an equilibrium the zero-bound constraint is binding, $r^* = -\pi^*$.

Let us assume that all the countries with unemployment ($j \in U$) reduce their macroprudential tax by a small (first-order) amount $d\tau$. This increases global demand by,

$$-\sum_{j \ni U \cap J^d} I'(r^* + \tau_j^d(r^*))d\tau - \sum_{j \ni U \cap J^c} [C'(r^* + \tau_j^c(r^*)) + I'(r^* + \tau_j^c(r^*))]d\tau = dE^W > 0.$$

The increase in global demand is matched by an increase in supply from the countries with unemployment. In equilibrium, there is a small increase in the nominal price of the good ($dP > 0$), which raises supply in all the countries with unemployment by $dY_j > 0$ in such a way that $\sum_{j \in U} dY_j = dE^W$. Since there is still some unemployment left after this first-order change, the global monetary authority keeps the nominal interest rate at the zero bound and there is no change in the real interest rate r^*.

The welfare of a country that uses domestic macroprudential regulation is given by,

$$U_j = \max_{C_j, I_j} u_j(C_j) + p_j(I_j) f_j(I_j) + (1 + r^*)(Y_j - C_j - I_j).$$

The welfare of a country that uses prudential capital controls can be written in the same way, with the additional constraint $p_j(I_j) f_j'(I_j) = u_j'(C_j)$. By the envelope theorem, the change in country j's welfare is,

$$dU_j = (1 + r^*) dY_j > 0.$$

Thus all the countries with unemployment have a positive welfare gain, whereas the countries at full employment see their welfare unchanged.

APPENDIX B

Linear-Quadratic Specification of the Model

Let us assume that output and systemic risk vary linearly with investment and debt respectively,

$$f(I) = (1+\rho)I, \quad p(D) = 1 - D/\overline{D},$$

where ρ and $\overline{D}$ are exogenous parameters. We assume $\rho > r^*$ to ensure that investment is profitable when there is no systemic risk. Under these assumptions the laissez-faire level of debt and investment satisfies $p(I^{lf})(1+\rho) = 1 + r^*$, implying

$$I^{lf} = \frac{\rho - r^*}{1+\rho}\overline{D}.$$

The first-best level of investment maximizes $(1 - I/\overline{D})(1+\rho)I - (1+r^*)I$. It is equal to one-half of the laissez-faire level of investment,

$$I^{fb} = \frac{\rho - r^*}{1+\rho}\frac{\overline{D}}{2}.$$

This level of investment can be achieved using a macroprudential tax on domestic borrowing $\tau^d = -p'(I^{fb})f(I^{fb}) = (1+\rho)I^{fb}/\overline{D}$ or

$$\tau^d = \frac{\rho - r^*}{2}.$$

As a result the net domestic cost of borrowing is $(r^* + \rho)/2$. The optimal tax smooths out one-half of the variations in the external cost of borrowing.

The utility for consumption is quadratic,

$$u(C) = \alpha C(\overline{C} - C/2),$$

where $\overline{C}$ is the satiation level in consumption. It follows that saving is a linear function of the real interest rate,

$$S(r) = Y - \overline{C} + \frac{1+r}{\alpha}.$$

When the social planner uses a tax on external borrowing, he optimizes under the constraint $u'(C) = p(I)f'(I)$, or

$$\alpha(C - \overline{C}) = (1+\rho)(1 - I/\overline{D}).$$

This constraint, together with the definition of total expenditures, $E = I + C$, can be used to derive how consumption and investment increase with total expenditures,

$$C = \frac{\overline{C} - \beta\overline{D} + \beta E}{1+\beta},$$

$$I = \frac{\beta\overline{D} - \overline{C} + E}{1+\beta},$$

where $\beta \equiv (1+\rho)/(\alpha\overline{D})$. The optimal tax on external borrowing is

$$\tau^e = \frac{\rho - r^*}{2+\beta}.$$

It is smaller than τ^d, the optimal tax on domestic borrowing. One can show, finally, that the current account balance is larger with the optimal tax on external borrowing than with the optimal tax on domestic borrowing.

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NOTES

1 For example, Brazil introduced a tax on all capital inflows except direct investment in October 2009. Prudential capital controls have been viewed with more sympathy than in the past by the official sector (IMF 2011, Ostry et al. 2011). In its Seoul Action Plan (following the 2010 Group of 20 summit in Seoul), the G-20 endorsed the use of "carefully designed macro-prudential measures" to deal with excessive volatility in capital flows to emerging market economies. See Williamson (2005) for a pre-crisis exposition of the merits of prudential capital flow management for emerging market economies.

2 There is evidence that capital controls deflect capital flows between emerging market economies. For example, Forbes et al. (2011) find that capital controls in Brazil caused investors to increase the share of their portfolios allocated to other Latin American countries, possibly shifting vulnerabilities from one country to another.

3 See Borio (2003) for a pre-crisis discussion of macroprudential policy.

4 Hanson, Kashyap, and Stein (2011, p. 3) differentiate microprudential and macroprudential regulation as follows: "A microprudential approach is one in which regulation is partial-equilibrium in its conception and is aimed at preventing the costly failure of individual financial institutions. By contrast, a macroprudential approach recognizes the importance of general-equilibrium effects, and seeks to safeguard the financial system as a whole."

5 See De Nicolo, Favara, and Ratnovski (2012) for a discussion of the externalities that underpin the macroprudential regulation of banks. See Stein (2012) and Acemoglu, Malekian, and Ozdaglar (2013) for models of systemic risk in the banking sector based on these externalities.

6 Of the 10 macroprudential instruments reviewed by Lim et al. (2011), none takes the form of a tax.

7 Benigno et al. (2013) compare the use of ex ante prudential capital controls and ex post interventions in a small open economy with collateral frictions.

8 The international coordination of capital control policies has also been studied in models where these controls are not implemented for prudential reasons. Farhi and Werning (2012) look at the macrostabilization benefits of capital controls for economies with a fixed exchange rate or a common currency in the context of a New Keynesian framework with nominal stickiness, and find a very limited need for coordination. Costinot, Lorenzoni, and Werning (2011) find that international cooperation may be warranted if countries are large enough to influence their intertemporal terms of trade (the world real interest rate).

9 Claessens, Ghosh, and Mihet (2013) find similar results based on disaggregated data on more than 2,000 banks in 48 emerging market and advanced economies.

10 See IMF (2011) for a review of this literature, and Ostry et al. (2012) for a recent study. Klein and Shambaugh (2013) find that capital controls must be broad-based in order to be effective.

11 For example, Aiyar, Calomiris, and Wieladek (2012) find that U.K.-owned banks and resident foreign subsidiaries reduce lending in response to tighter capital requirements, but this effect is partially offset by an increase in lending from resident foreign branches.

12 Basel III allows domestic regulators to require foreign regulators to impose higher capital standards on domestic lending by foreign banks, which may reduce future leakage.

13 In the microfounded model the probability of a systemic debt crisis depends on the level of debt repayment in period 2, $(1+r)D/p$, rather than on the level of debt issued in period 1, D. This complicates the model in ways that are interesting in some respects (for example by generating multiple equilibria à la Calvo 1988) but that are not essential for the analysis in this paper.

14 This is because the lenders are risk-neutral and appropriately compensated for the risk of default in equilibrium. This ignores the fact that the lenders could suffer from a debt crisis through other channels, for example if they receive a wage income from the productive sector. In this case the social planner has to take into account the impact of prudential policies on agents other than the borrowers, but the essence of our results carries through.

15 A tax on domestic borrowing is a tax paid by all domestic borrowers irrespective of the residency of the lenders or the jurisdiction of issuance.

16 The promised debt repayment must be $(1+r^*+\tau)/p$ if the tax is not paid when the borrower defaults. Like the interest rate, the ex ante tax rate is increased by a default premium.

17 For simplicity I assume that $p(I)f(I)$ is a concave function of I so that the first-order condition is sufficient for optimality.

18 Similar results hold if the cycle is induced by a change in domestic productivity that changes the private and social marginal gains from investing by the same factor.

19 This is not to suggest that the motive for reserve accumulation in China is primarily prudential—as opposed to, say, maintaining a competitive exchange rate.

20 See Ostry, Ghosh, and Korinek (2012) for a related discussion of this issue.

21 Another consideration is that in models where the pecuniary externality involves the real exchange rate, such as Korinek (2010), the repayment of foreign currency debt has a larger systemic impact on the domestic economy if it involves a transfer to foreign creditors.

22 The evidence suggests that even taxes that are explicitly designed to address externalities are heavily influenced by other considerations—see for example Barthold (1994) for the case of environmental taxation in the United States.

23 The strategic complementarity between macroprudential policies does not lead to equilibrium multiplicity. As $r^*+\tau^c(r^*)$ and $r^*+\tau^d(r^*)$ are both increasing in r^*, global saving and global investment are respectively increasing and decreasing with the interest rate so that the global loanable funds market has one unique equilibrium.

24 The fact that the uncoordinated use of macroprudential policies raises global welfare does not mean that it raises the welfare of all countries. The welfare of capital-exporting countries may be reduced by the lower return on their foreign assets.

25 It is impossible, in this simple model, to lower the real interest rate below the Wicksellian level because labor cannot be increased above the full employment level. Any attempt to do so would result in an unbounded increase in P.

26 Blanchard and Milesi-Ferretti (2012) argue that when the global economy is in a liquidity trap, the negative impact of certain policies such as reserve accumulation on global demand could justify international coordination.

27 This does not suggest that the Chinese accumulation of reserves is made primarily for prudential reasons as opposed to resisting the appreciation of the currency. The results in this section would remain valid, however, if China accumulated reserves because of a positive externality related to currency undervaluation, as in Aizenman and Lee (2010). The analysis in Korinek (2012) also encompasses this case.

28 This assumption is not restrictive as it is in general optimal for the U.S. to set the domestic macroprudential tax to zero if there is unemployment in the U.S.

29 There may be other reasons for having international rules of good conduct for capital account policies, e.g., reducing stigma for appropriate policies—see Jeanne, Subramanian, and Williamson (2012). I have focused here on the rationale for coordination based on international spillovers and externalities.

COMMENTARY

Macroprudential Policies in a Global Perspective

Jonathan D. Ostry

Olivier Jeanne's paper is elegant and makes a number of important points regarding the appropriate policies to mitigate financial risk and the possible benefits of policy coordination in the realm of capital controls and macroprudential policies.

Olivier's first result is that when the externality relates to total borrowing rather than external borrowing, the correct intervention is a macroprudential tax. This is surely right. He proves that a capital control, which discriminates against transactions between residents and nonresidents, is distortive rather than purely corrective; this is in contrast to the macroprudential tax, which is purely corrective. A capital control distorts saving, which is not the source of any distortion to begin with, so it is a second-best policy in Olivier's setup.

I worry though that the arguments made in the paper could be used to suggest that there is a general superiority of macroprudential taxes over capital controls. I do not believe there is a general argument for capital controls being second-best and macroprudential policies, which don't discriminate against nonresidents, being first-best. I believe that the model could be recast to consider a situation in which resident–nonresident transactions are the riskier ones, and that in such a model the first-best intervention would be a capital control. Foreign lenders could in principle be flightier than domestic lenders: In such cases, the marginal foreign loan might engender more systemic risk and deserve to be taxed more heavily.

More generally, I believe that macroprudential policies of the type in Olivier's model have their limitations. If the risky flows bypass the regulated financial sector, or if agents move outside the regulated financial sector to bypass the taxes, the macroprudential taxes will be ineffective. If there is direct borrowing between residents and nonresidents that is particularly risky, macroprudential policies geared to the regulated financial sector will lack traction

Author's note: *These views are personal and should not be construed as representing the views of the International Monetary Fund.*

to address the problem. Capital controls might be the preferred instrument in such cases. Beyond this, sometimes the risks to the economy are different from the ones highlighted by Olivier's model: They may relate specifically to overvaluation rather than financial stability risks as such. Capital controls may be more appropriate in such circumstances as well. These issues are fleshed out more in Ostry et al. (2011, 2012).[1]

Olivier's second result relates to the need for policy coordination. The paper makes the point that a Nash game in which countries independently choose the level of their macroprudential or capital control tax may be globally efficient. I think this is an important point because, in other papers, it is sometimes alleged that the mere existence of spillovers from countries' capital control policies generates a need for coordination. Olivier's point, also made by Korinek (2012), is that capital control wars—in which country A's actions engender higher capital controls in country B—may not indicate any global efficiency cost or need for coordination. The spillovers from each country's policy may not be externalities in the relevant sense, but merely the normal functioning of a market system in which the cross-border effects of policies are intermediated through the relevant prices.

My issue though is that, while the point is valid as far as it goes, it seems quite likely that countries will not be able to costlessly inoculate themselves against the cross-border effects of capital controls or macroprudential policies in other countries. In a world in which using the policy instrument is costly—and I think this is the world in which we live—the cross-border spillovers will have implications for global efficiency. Playing Nash in such circumstances will not be efficient and there will be gains from coordination. The costs from using the capital controls or macroprudential instrument may be bureaucratic or, more importantly, result from the imperfect targeting of the flows. In aiming to limit hot money flows, for example, there will inevitably be some collateral damage in which other, more beneficial flows are also impeded. This is a point my colleagues and I make in some related work (Ostry, Ghosh, and Korinek, 2012).

The third result in the paper concerns the benefits from coordination in a situation of liquidity traps and unemployment. In such a case, the Nash equilibrium is shown to be inefficient because countries do not internalize the spillover effects from their macroprudential policies in reducing global aggregate demand. This is a genuine externality because of the zero lower bound constraint on monetary policy. By coordinating the macroprudential taxes, it is possible to obtain a first-order employment gain at only a second-order cost of additional financial instability. I agree with the argument. However, I worry that there is a potentially serious domestic coordination issue that needs to be

confronted. The macroprudential regulator may not care much about the benefit from additional employment but will be held accountable for the increase in financial instability that a move from Nash to coordination implies. My second comment relates to whether generating additional aggregate demand would not more logically fall to fiscal policy than to macroprudential policy. While the result of course holds with macroprudential policy, thinking in terms of targets and instruments would pull one more toward fiscal policy in my view.

Olivier's final result concerns the nature of the Nash and coordinated equilibrium between a large surplus country with an undervalued currency and a large deficit country in a liquidity trap with deficient demand. The paper shows that a coordinated equilibrium in which the liquidity trap country actually tightens monetary policy and the surplus country reduces its dollar purchases is Pareto improving. The logic is that, while there is a first-order loss to the liquidity trap country from higher interest rates, this can be more than compensated for by the additional demand from a reduction in foreign exchange intervention (or equivalently, more total expenditure, including on foreign goods) in the surplus country. For the surplus country, reduced intervention only engenders a second-order loss. But the tighter monetary policy in the liquidity trap country engenders a welcome reduction in capital inflows, and thus a first-order gain. A Pareto improved outcome is the result. There is logic to this story, and perhaps it mirrors the calls for a faster exit from unconventional monetary policy to reduce financial spillovers to other countries. It would be good if the paper discussed more how the coordinated equilibrium affects the *composition* of aggregate demand in the surplus country, and in particular how it relates to the narratives that call for more internal rebalancing—involving more consumption and less investment in such countries. It would also be good if the paper said more about the implementation, specifically the likely extent of needed monetary tightening in exchange for decreased reserve buildup that the coordinated equilibrium envisages, and the split of the gains between the two countries.

I will conclude my comments with a couple further points. On the case for coordination over capital control-macroprudential policies, one should keep in mind that the purposes of these policies may not always, or even typically, be to reduce financial-stability risks (Jeanne 2012, Ostry et al. 2011, and Jeanne, Subramanian, and Williamson 2012). Such policies may be deployed to prevent warranted external adjustment, to exploit market power (terms-of-trade gains) or as a second-best response to production externalities. In all such cases, the use of capital controls may be problematic from a multilateral point of view. But the scope for coordination may be very small. Instead, rules of the road to limit multilaterally problematic behavior may be called for.

Finally, I believe much of the analysis in Olivier's paper can be usefully understood in terms of an older literature on policy coordination, which highlighted that gains from coordination depend on there being a deficit of instruments relative to policy targets. In Olivier's first coordination example, there is only one target—financial stability—and one instrument gets the job done without a need for coordination. In his examples with liquidity traps, there is a demand management target but no instrument to meet it because of the zero lower bound. Coordination becomes useful because of a dearth of policy instruments.

One important issue we need to confront is why, despite obvious gains from coordination, policymaking is more typically unilateral than multilateral. One salient point in this regard is the role of uncertainty and disagreement about the magnitude, and even the sign, of cross-border spillovers, which may indeed be an impediment to negotiating and sustaining coordinated policies. The international community needs to think harder about how to overcome the impediments to coordination. Would a neutral assessor help bridge the differences about the nature of cross-border spillovers? Are guideposts needed when policies give rise to palpable cross-border spillovers, but coordination is not in the cards?

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NOTE

1 Another issue is the relative cost of prudential and capital control instruments in terms of access to credit. It is possible that, if the goal is to maintain credit access for small and medium enterprises (SMEs) that rely on bank financing rather than direct borrowing abroad, capital controls may be less costly than prudential taxes that may cause banks to curtail lending to SMEs.

COMMENTARY

Macroprudential Policies in a Global Perspective

Guillermo Calvo

I enjoyed reading this paper. The models are simple and focus on important issues. The approach assumes the existence of externalities and discusses Pareto-improving policies along Pigouvian lines. A basic assumption is that, beyond a certain point, credit expansion carries with it the virus of financial crisis which, however, is not internalized by individual agents. Then, it proceeds to discuss Pareto-improving borrowing taxes and other forms of government intervention.

The first part of the paper shows that if the dominant distortion resides at home, then *domestic* macroprudential policy, and not controls on capital inflows, are in order. This is a straightforward result in terms of the model, but it is a real eye-opener for those in the policy arena, where there are numerous instances in which policymakers implement controls on capital inflows without paying much attention to domestic distortions. The model is extended to account for several countries facing domestic financial distortion. In this setup, one country's macroprudential policy can have a negative effect on the others. Hence, there may be room for international policy coordination. However, if countries are small, the "invisible hand" is still capable of working its magic: the paper shows that if each country pursues macroprudential policies that are individually optimal, the equilibrium outcome is Pareto optimal! This result is far from obvious, and provides a benchmark case that should make policymakers think twice before engaging in policy coordination initiatives prompted by international financial externalities. To be true, some countries may feel shortchanged, but the fact that the uncoordinated equilibrium is Pareto optimal is likely to make global alternative agreements hard to achieve. For instance, achieving alternative agreements may call for a nontrivial subset of underdogs to get together and threaten to take autarkic actions. However, this is unlikely to pay off since these initiatives are likely to be riddled with hard implementation problems.

The second part of the paper shifts attention to macro waters infested with wage stickiness and binding zero interest rate bounds. It abandons the world of atomistic economies in order to center on a world dominated by two large

blocks, called China and U.S. In this scenario, which can be employed to portray currency wars, the paper shows that there is room for policy coordination in which, for example, China commits to slow down the accumulation of international reserves and use the proceeds to increase aggregate demand, while U.S. jacks up its policy interest rate. This is a model-specific result but it helps to show that policies that are on the table for these two blocks could be Pareto-improving relative to the outright currency war that seems to be playing out these days. There is no doubt that much work is needed before these insights become operative, but the paper succeeds in showing that there is no blind alley ahead. In my opinion, this is macroeconomic *theory* at its best.

The paper addresses a slew of interesting and policy-relevant issues, as illustrated by these examples. It is an ideal tool to introduce economists to a discussion that so far has mostly been left in the hands of policy experts and policymakers, without solid analytical underpinnings. Moreover, it has the great advantage of showing that some issues can be addressed in Econ 101. However, there is still a large distance between these models and the scenarios faced by policymakers who are keen on cushioning the economy from the effects of capital flow volatility. Even if one agrees with the basic assumptions in these models, policy implementation becomes a very challenging issue. The Pigouvian methodology, for example, necessitates knowledge of how credit booms, say, eventually translate into credit busts. Without specific knowledge of these issues, the policymaker is left with only the vague notion that she should be doing something to pare down the boom. Unfortunately, knowledge about the specifics of credit booms and busts is extremely limited, and the economics profession displays a wide variety of opinions. The spectrum of opinions ranges from those who believe that these boom/bust episodes boil down to financial frictions that are subject to rigorous statistical estimation, to those who claim that these episodes reflect the existence of multiple equilibria and, therefore, there are sharp discontinuities or strong nonlinearities that are very hard to assess. Part of the problem here is that financial distortions are much harder to assess than the ones emphasized in the global warming controversy, for example. To be sure, global warming is still strongly debated but, at least, one hopes that basic science—which is aeons ahead of macro-finance—will help to identify and quantify some key relationships. This is bad enough news but, in addition, one has to reckon with the fact that when taxation is involved, the whole government gets drawn in, and the issue becomes highly politicized. These problems are even thornier in the context of international coordination.

Moreover, there are a slew of pedestrian implementation problems that must be taken into account. A prominent case in point is tax avoidance. For instance,

many countries have imposed taxes on short-maturity inflows to keep hot capital at bay. The effectiveness of those controls appears to be limited: While taxing short-term capital inflows lengthens the maturity of those flows, the effect on the current accounts may be negligible (Magud and Reinhart 2007). This may result from the fact, pointed out in Garber (1998), that investors can easily resort to off-balance-sheet tricks (such as interest rate swaps) to bypass capital-inflow taxes, changing reported debt maturity without significantly affecting total capital inflows.

The models employed in the present paper do not distinguish between *gross* and *net* credit flows, and center entirely on net flows. But, in practice, it is hard to tell gross from net borrowing. For instance, a firm may extend credit to its customers and simultaneously borrow from its suppliers. A literal reading of the paper implies that the government should tax borrowing and subsidize lending. However, this is not how controls on capital inflows work in practice. Until recently, for instance, Brazil imposed stiff taxes on gross capital inflows without countervailing subsidies on outflows. Is this a major distortion? I think the issue is worth pursuing because, as far as I can tell, it has also been ignored in the outstanding literature. I suspect, though, that if countervailing subsidies on outflows are allowed, it will make it even easier for the financial sector to find new ways to avoid paying taxes on net borrowing.

Finally, the paper ignores *quantitative* controls, possibly combined with taxation. Quantitative controls are especially relevant when policymakers know little about the market's reaction to a financial tax but have a much better idea about excessive credit growth. An example, with some practical relevance, is bank credit. It may be easier to assess if bank credit is growing too fast than figuring out the tax rate that would stop banks from exceeding the speed limit.

In summary, this paper helps to address central questions related to controls on capital flows and offers a clear-cut framework that helps to see the big picture. Future extensions addressing some of the issues raised here will certainly increase the policy relevance of this research project.

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GENERAL DISCUSSION
Macroprudential Policies in a Global Perspective

Chair: Mark Spiegel

Mr. Spiegel: Olivier, would you like to take about five minutes to respond to these questions?

Mr. Jeanne: Yes. Some papers try to think outside the box and some papers tend to stay more inside the box. This paper perhaps belongs more to the second category. There is a huge literature about international spillovers and the benefits of international coordination of monetary policy and fiscal policy. These policies are the first two pillars of macroeconomic stabilization. There are a lot of debates now about a third pillar, macroprudential policy. But we know very little about the international coordination of macroprudential policies.

What my paper is trying to do is provide a textbook analysis of this question. It is inside-the-box thinking in the sense that it is pretty close to the textbook. That being said, the results are not all obvious—for example, the fact that the lack of coordination between macroprudential policies could be efficient even when it seems mutually destructive.

Moving to more specific points, Jonathan said that in some cases capital controls could actually be the first-best instrument, and I completely agree. First, as I say in the paper, this would be true if foreign investors tend to rush for the exits faster than residents in a crisis. This could be true too in models where debt is in foreign currency and repaying foreigners tends to depreciate the domestic currency more than repaying residents. Thus, in some circumstances capital controls could be the first-best instrument.

Guillermo invited me to think more outside the box, and I agree with a lot of what he said. There are big questions about the extent to which macroprudential capital controls can be circumvented. The impact of these policies has not been identified very well in the empirical literature. This paper does not have much to say about that.

Mr. Spiegel: We have time for a few questions. First, from Anil Kashyap.

Mr. Kashyap: Olivier was very clear and measured in what he said, but I find myself at these conferences becoming the language police. The word

"macroprudential" to me means something very different in a model where there is something clearly negative like pollution and you're trying to tax it to reduce it. I think macroprudential is fundamentally about dealing with financial issues, which are only interesting if there's the possibility of default. And the possibility of default is all about market incompleteness. And when you have market incompleteness, there is no planner, there's no first-best solution, and there's probably no second-best. I think we should agree among ourselves that if we're going to try to talk to policymakers about macroprudential policy, we've got to map the landscape about what happens when there are incomplete markets. As soon as you get into market incompleteness, I believe there's a typology that's helpful. One set of issues arises if market incompleteness results in some people trying to exploit the possibility of default to make themselves better off—like the too-big-to-fail narrative where the incentives are to privatize the gains, socialize the losses. Then there's a second set of issues, explored in the Diamond-Dybvig literature on runs, which says that when the financial system collapses, activity collapses. We should map all of this discussion into whether one is worried more about too big to fail, or about runs and collapse.

The macroprudential discussion is totally different depending on which of those two boxes you get into. So as soon as I hear that the planner showed up, I leave the room. Because the planner isn't ever going to go to Basel. We need to get away from that box, because that intuition isn't helpful. It is possible that, in the third-best case, you get to some Pareto outcome. But then the whole question is your starting point. Is there overinvestment because you're too big to fail and you're basically gambling? Or is there underinvestment because the run collapses the economy? I believe that's a more helpful way to go ahead.

Sorry, that's not even a question, so you can ignore it if you want, but if you have thoughts, I'd be curious.

Mr. Jeanne: Well, I believe your viewpoint about macroprudential policies is a bit too restrictive. It's not only about too-big-to-fail or run-and-collapse. One can think of other frictions. I can imagine a world without banks, without bailouts, without bank runs, some agents in the real sector overborrow. The U.S. mortgage crisis and its aftermath are about excessive leverage in the household sector as much as, perhaps even more than, it is about excessive leverage in the banking sector. To me there is a need for a broader view of macroprudential policy, because financial frictions exist outside the banking sector too.

Mr. Spiegel: Okay, I have John Murray and then Carmen Reinhart, and then I think we're going to have to close this session.

Mr. Murray: Just an observation that ties into a question aimed more at Jonathan than Olivier Jeanne. There is a form of social planning, certainly coordination, that is trying to be conducted through Basel and the Financial Stability Board on the macroprudential side. With regard to the international monetary system, of course, the International Monetary Fund is at the center and wants to set itself up perhaps or be even more firmly embedded in that role. But that leads to my question about Jonathan's reference to the need for rules of the game. You observed in passing that they are already spelled out pretty clearly in the IMF articles. It's just that nobody obeys them. Do you have any thoughts on enforcement, since it's very difficult without some sort of enforcement mechanism. I realize this might be a little off topic, but that seemed to be the answer from your perspective to many of the problems we were talking about. Might it fall to the World Trade Organization (WTO) to impose trade penalties to deal with misbehavior regarding currency and capital controls and exchange rate manipulation? Is this the enforcement mechanism for more effective resolution of externalities?

Mr. Ostry: Thanks, John, for that. Of course let me emphasize that I'm speaking for myself. What we have in mind in the paper I mentioned in my discussion is something much softer than what you articulated in your question. The idea is that multilateral surveillance, using tools like in the IMF's external sector report and the spillover report, would have some words of caution about external balances or current account balances or capital account balances, including their composition, that are engendering negative spillovers or risks in other countries. You're perfectly correct that in some sense we've had these things on the books for a long time.

We have some discussion in our paper about the extent to which the IMF already has been playing this "neutral umpire" role over the past 20 years. My sense is that we haven't really done it in the way that I laid out. But you're correct, it would take much more if these rules were going to be hard. And I don't think the international community has the appetite for hard rules at this point.

Mr. Spiegel: Carmen Reinhart has the last question.

Ms. Reinhart: My question is related to some of the points that Jonathan and Guillermo raised about connecting Olivier's analysis to reality. Taxes and domestic borrowing are not a hypothetical. Part of the sterilization of capital inflows has involved big increases in reserve requirements. Now, depending on who pays that tax, it's not the tax exactly that you have in mind, but it is a tax that ultimately is passed on either to the depositor, to the borrower, or to a

combination of the two. So my question is related to Guillermo's point about distinguishing, especially in times of stress, between large borrowers and small borrowers.

In the paper that I did about 15 years ago on precisely the issue of who pays the reserve requirement tax, one of the things we found was that, when the tax is passed on to borrowers, small firms are the ones left in the domestic banking system and large firms go abroad to borrow. This raises the issue of adverse selection. And it raises the question of whether, if you introduce a tax on domestic borrowing, does that immediately imply you're going to have to do something about external borrowing as well? Otherwise, you may wind up taxing the small and medium-sized firms while the big firms go abroad.

Mr. Jeanne: So, by going abroad you mean . . . ?

Ms. Reinhart: Borrowing.

Mr. Jeanne: I'm sorry, I have not explained well what I mean by a tax on domestic borrowing. The criterion is not whether the loan is issued domestically or abroad. By domestic borrowing I mean borrowing by domestic residents, whether they do it at home or abroad. In principle the tax would apply to what you are calling foreign borrowing. When you say foreign, what you have in mind is foreign jurisdiction, right?

Ms. Reinhart: The legal term.

Mr. Jeanne: Right. So what I'm calling a tax on domestic borrowing is a tax on debts issued by domestic borrowers, whether the debt is issued at home or abroad.

Mr. Spiegel: Any last comments from Guillermo?

Mr. Calvo: Just a comment complementary with what Carmen said. The experience is that big firms go borrow outside the country when there's an episode of capital inflows. And when there is a sudden stop or some problems in the international capital market, they turn around and fund themselves in the domestic market. Because they are prime borrowers, they tend to drive out the small borrowers, which probably are the ones that are engaged in more labor-intensive activities. So it can have a huge effect on nontradables output, which tend to be labor-intensive, and on unemployment that has no comparison with what you see in the aggregate about credit. Credit may not change much and still there may be a very big impact on the domestic economy.

My other point is related to what Kashyap said about the Diamond-Dybvig model. I think you can go outside the banking sector to have this type of result.

You can generate shadow banks that are completely outside of the banking system and still have the phenomenon that they produce some sort of liquidity that suddenly crashes and has an immediate effect on credit. Using taxes in that context is very tricky. After all, remember that Greenspan kept saying that he was afraid of pricking the real estate price bubble. If you really want to prick a real estate bubble when prices are growing at 20 percent per year, then what interest rate do you have to set in order to dissuade that kind of activity? You're going to hurt the whole economy.

Mr. Spiegel: Jonathan, do you have a comment?

Mr. Ostry: Just very briefly, on Carmen's question. So there is a literature that documents the harm capital controls do to small and medium-sized enterprises (SMEs). If you're debating between using macroprudential tools and capital controls, you also want to consider the fact that SMEs rely more on banks, and large firms rely more on direct borrowing from abroad. So it's not obvious to me that, if you want to help SMEs, you would opt for macroprudential tools over capital controls. You might choose just the opposite.

Mr. Spiegel: Great. Please join me in thanking the panel and Olivier for an excellent session.

Financial Regulation after the Crisis: How Did We Get Here, and How Do We Get Out?

Gerard Caprio, Jr.

Following the global financial crisis of 2007–09, regulatory authorities either are or should be engaging in a fundamental reconsideration of how they approach financial regulation and supervision. This paper briefly summarizes the present international consensus on regulation as embodied in the Basel framework. It looks at how we came to be in such a situation, and proposes a restart of the process organized by the Basel Committee on Bank Supervision. The paper reviews the flaws of that framework and concludes that its weaknesses are fundamental, in its neglect of the endogeneity of risk to the regulatory structure, and of the dynamic nature of finance and thus of its regulation as well. Neither a static rule book nor an increasingly complex one will ever provide financial safety and soundness. I make specific recommendations, starting with abandoning risk weights and adopting a simple leverage rule, supplemented by conditional convertible debt and some simple rules. More radically, I urge a different approach, one that focuses on the oversight and accountability of regulators and greater transparency for both banks and the regulatory process.

I come to bury Caesar, not to praise him. The evil that men do lives after them; the good is oft interred with their bones; so let it be with Caesar. —William Shakespeare, *Julius Caesar*, Act III, Scene 2

1. Introduction

In the wake of one of the worst financial crises in history, governments in the United States and Europe are moving in fits and starts to adjust financial regulation, albeit in increments far smaller than virtually anyone with advance knowledge of the dimensions of the crisis might have imagined. Just as barn doors tend to be shut after a horse has escaped, banking crises routinely are followed by new and "tougher" regulation. Regulatory change and tougher

Author's note: *James Barth, Charles Calomiris, Stijn Claessens, James Hanson, Takeo Hoshi, Ross Levine, Ashoka Mody, and participants at the conference provided helpful comments. The conference organizers and Kevin O'Rourke posed questions that inspired the focus of the paper. The author would like to thank Brian McNamara for excellent research assistance. As usual, responsibility for what lies herein rests with the author.*

enforcement of existing rules have been the norm following most modern crises, notably in the Great Depression, the U.S. savings and loan (S&L) crisis, and many emerging markets since the 1980s. Yet crises have shown no signs of abating or diminishing in severity, and their fiscal cost has exploded.

Discouragingly, many researchers—including the author—believe that reforms after the 2007 crisis fall far short of what is needed to reduce the likelihood and severity of future crises.[1] There is no consensus yet on policy recommendations, such as more and differently defined capital, higher liquidity requirements, conditional convertible debt (known as CoCos), narrow banking, or criminal prosecutions. Any solution that is effective will reduce the availability of credit from what it was in the extreme years during the run-up to the crisis, but despite the unwillingness of politicians to make that point, better-allocated credit would be a boon to societies. Just as the tech bubble saw investments financed that should not have been (e.g., so-called dark fiber, or fiber optic cables that still have not been utilized), the credit bubble in the 2000s featured unproductive investments in housing and a variety of consumer goods that left societies with high unemployment, a debt overhang, and little else, save some empty houses, the regrets of the borrowers, and the enlarged wealth of many in the financial sector. Nonetheless, bankers are protesting that the response in the pipeline will produce financial disintermediation, denying credit to many and thus reducing growth.

As the title suggests, this paper looks at where the formerly[2] advanced countries are in terms of financial regulation. It then suggests how countries might escape the current situation of a massively complex regulatory apparatus that is not producing a banking sector that is both safe and yet still contributes to prosperity. Section 2 begins that task, focusing on how the current approach to regulation came to be the new norm. Section 3 reviews some lessons from the recent crisis, focusing on the issues where my views differ from the majority position in the literature. Section 4 then looks at Basel as a regulatory model; I conclude that its fundamental shortcomings played a role in recent crises, and that its approach to regulation requires "rebooting." Some possible ways ahead on the regulatory front are offered in Section 5. Changing bank regulation and supervision is an arduous task, and as suggested by Calomiris and Haber in a forthcoming book, politics not policy advisors dominate the decisionmaking process. Erstwhile reformers therefore should know that their task will seem, and perhaps be, Sisyphean.

One of the paper's key conclusions is that the Basel approach to bank regulation and supervision is choking on it own complexity as it attempts to tackle three jobs: keeping the banking system safe, leveling the playing field for banks,

and being responsible for risk management at the individual bank level. Just as Soviet planners found that they had to intervene at an ever more granular level to avoid market participants adjusting in undesirable ways, the Basel Committee has responded to the failure of each of its Accords with an ever more complicated version. But beyond complexity, the Basel Committee has neglected the endogeneity of risk: Its attempts to level the competitive field for banks have increased the covariance of banks' exposures, which should be anathema to bank regulators but instead has received little attention. Furthermore, there is no reason to think that harmonized policies will work the same in different institutional environments, which might be one reason why cross-country empirical studies cannot find any consistent effect of tighter capital regulation or increased supervisory powers (Barth, Caprio, and Levine 2006, hereafter BCL). The upshot of these points—increasing complexity, endogeneity, and the differences in countries' institutional environments—means that the Basel Committee is playing a game with market participants, locked in a strategic battle that it cannot win and should not be attempting to play. Temporary wins will not only be short-lived, they will necessarily entail a loss someplace else. I discuss the implications for what a new Basel Committee might do, beginning with abandoning the risk weighting of capital (and liquidity) requirements and adopting a meaningful leverage, or unweighted capital, rule. I suggest additional specific measures for a proposed newly reconstituted committee with new membership to consider.

As has long been known, thanks to the seminal work of Ed Kane (1981) and Buser, Chen, and Kane (1981), finance is dynamic, responding to and innovating around regulation. Indeed, this point was clear at the dawn of modern banking, when fledgling bankers used simple innovations to evade limits on usury despite the seemingly stiff penalty (eternal damnation). The Basel answer to this problem of evasion has been ever-increasing complexity and ever-growing numbers of supervisors. But if finance is dynamic, then so too must be its regulation. The failure of regulators to use the powers they had during the crisis calls for greater accountability. But beyond that, the dynamic nature of finance, by suggesting either that legislators must constantly reconsider financial legislation (a scary thought) or that more discretion for regulators is warranted, also demands more accountability, because power without accountability is unsustainable in a democracy. We must consider creative ways of disclosing more information, and this paper offers one proposal related to compensation and risk management. No doubt people will disagree with many of the proposals, but the key points are (1) that the current framework, like that of Soviet attempts to replace market forces with diktats, is doomed, and (2) that a different approach

focused on simple rules (that would actually be enforced), disclosure of information, and monitoring and accountability of regulators is long past-due.

It is useful to delimit this paper. Humans' tendency to search for explanations—even of random events—tends to be equalled by our belief in single causes, or "silver bullets." Yet, in my experience, most complex phenomena have diverse causes, the crisis of 2007 being a clear example. A global savings glut, integrated international capital markets with macroeconomic policies that fueled large capital flows, easy monetary policy, resulting in lowered interest rates and credit spreads, easy loan standards, a boom in toxic financial innovations, greedy bankers, and an unsustainable explosion of credit, have all been cited as explanations of the crisis, and no doubt these factors played a role. My favorite explanation is the "perfect storm" theory, namely that the timing of many of these factors coincided, which no one could have anticipated. This explanation might better be labeled the "perfect excuse," as such a complicated set of factors coming together supposedly made it impossible to anticipate or predict the crisis.[3] This paper will not revisit that discussion. Rather, I focus on the inefficacy of the current approach to regulation. As much as international capital flows and macroeconomic policies may have played a role, I wish good fortune to those who would reform the international financial system or find a way to guarantee better macro policy. Also, I don't intend to let bankers and others in the financial sector off the hook for responsibility. One can only applaud the efforts, unsuccessful in the United States thus far, to encourage prosecution, but this paper won't dwell on that subject.

I must provide an important clarification on terminology. I use the term "regulation" as shorthand for regulation and supervision, and "regulators" to mean regulators and supervisors. This will no doubt bother some readers. However, regulatory agencies frequently shift which personnel work on regulations or supervision, so it seems fair to use a similar flexibility in this paper. More importantly, what most people care about is an effective regulatory framework, which results from a combination of the regulations themselves and how they are enforced. Rules without enforcement are tantamount to no rules at all. If the act or process of supervision (or supervisors) is the focus, I will use that term.

2. Where We Are, and How We Got Here

Before getting to the story of how we arrived at the current approach to bank regulation, a few facts about the shape of the banking system around the world are warranted.

The financial world in North Atlantic countries now, compared with that a decade ago, is markedly different in many respects as a result of the crisis and the policy efforts to deal with it. As of 2011, we still see tremendous differences around the world in banking (and more generally, financial sector) development, shown in Figure 1 by the ratio of credit to the private sector relative to GDP. These differences reflect dramatic differences in institutional economics in terms of the rules, laws, customs and other country characteristics that affect behavior. This figure then is a handy beginning, because a reminder of the substantial differences in countries' institutions and stages of financial development raises caution for any attempt to impose the same regulatory systems in every country.

Banking itself, once considered outmoded, expanded rapidly from the 1990s (Figure 2) relative to GDP, using the broadest measure of global banking available from the Bank for International Settlements (BIS). Bank concentration has been an increasing worry since the crisis, and Figure 3 shows how far it has advanced as of 2012. Although a high degree of concentration may reflect in

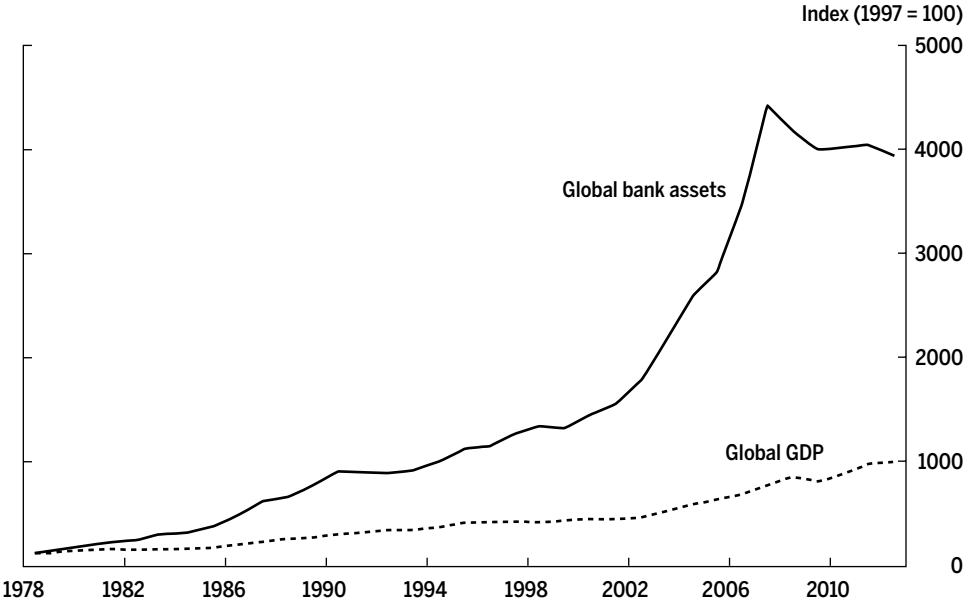
FIGURE 1

Financial Depth around the World

Bank Credit to the Private Sector as a Percent of GDP

Source: Barth et al. 2013.

FIGURE 2
Global Banking Assets (BIS Reporting Banks)



Source: Bank for International Settlements.

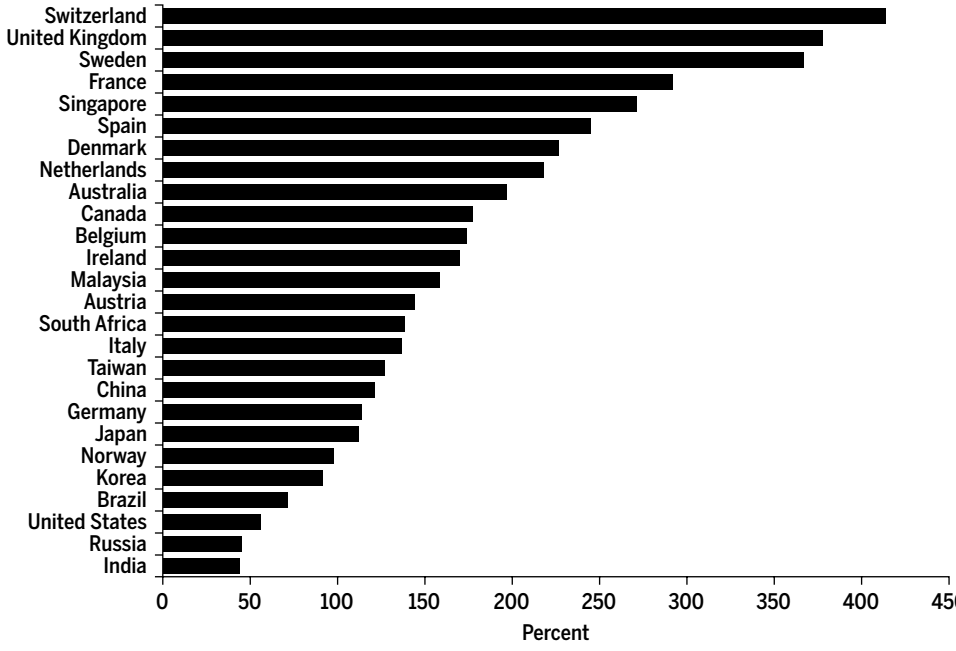
part changing technology that allows greater capture of economies of scale and scope, the boom in bank mergers (discussed in more detail later) emerged from the ending of U.S. restrictions on branching and the European Union's (EU) move to a single banking market. Another significant change (BCL 2013) is an increase in the share of banking assets held by foreign banks in many countries since the late 1990s. In areas such as derivatives, a handful of institutions essentially are the market.

These developments in the sector, featuring greater interconnectedness, larger size, a smaller share of state banks, and more dominant big banks, suggest that the regulation of the banking system is more important than ever.

Against this background, what was happening with regulation? Until the early 1980s, prudential regulation and supervision was relatively simple: Many countries around the world relied on reserve, liquidity, and portfolio requirements, along with controls on interest rates, to keep the banking system safe. Some countries had more highly segmented financial sectors (notably the United States, Japan—reflecting the U.S. influence in the immediate postwar period—and the United Kingdom), while others relied on more universal banking systems; however, I would be willing to bet that no industrial country

FIGURE 3

Assets of Top Five Banks as Percent of GDP



Source: Barth et al. 2013.

had either budgets in real terms or staff devoted to prudential regulation and supervision equal to 10 percent of the current total.[4] With the decontrol of interest rates and decreased intervention on the asset side of banks' balance sheets, regulatory agencies, at first gradually, ramped up their efforts to substitute indirect prudential regulation and supervision for more direct controls. This effort was advanced in developing and emerging markets with the assistance of the World Bank and International Monetary Fund (IMF), whose loans in the 1980s and 1990s often required deregulatory steps—in some cases, even where banks were insolvent!—and offered technical assistance on the transition to less direct controls. Direct controls had broken down as a result of the much greater volatility of the post-Bretton Woods era, the recycling of petro-dollars, and the increased financing needs of many governments. Financial disintermediation of the banking sector was a driving force of change.

The Basel Committee on Banking Supervision (BCBS) was formed in the 1970s as policymakers realized, at least from the 1974 failure of Herstatt Bank, the complexities associated with banks' cross-border exposures to one another, especially in the case of bank failure. The move away from direct controls was a

key factor in expanding their work.[5] This effort soon focused on creating a level playing field for internationally active banks, and although established by the Group of 10 (G-10) central bank governors, who might have been more concerned with systemic issues, it quickly came to be dominated by a focus on individual banks. It is not clear which was cause and which was effect, but the Committee took a microprudential focus, that is, looking at the banking sector on a bank-by-bank basis and in turn thinking about bank safety loan-by-loan. The 1982 debt crisis, the subsequent failure of Continental Illinois, and the expansion of Japanese banks in the 1980s seemed to be factors in the emphasis on capital. By definition, banks that fail have insufficient capital to cover their obligations; Japanese banks were among the top banks in the world and threatened to continue their expansion, thanks in part to an asset boom that boosted their balance sheets including the market value of their capital. Thus, attention to the definition and minimum amount of capital seemed warranted. Also, bank capital holdings had fallen from their much higher pre-Depression levels, reflecting both the extension of the safety net through expanded deposit insurance and a more widely accepted activist approach for lenders of last resort (Herring 2011).

The 1988 Capital Accord, or Basel I, adopted the approach of a minimum risk-weighted capital ratio, justified presumably by the reasoning that banks differ in their degrees of riskiness, and that it was unfair to require banks with relatively low risk portfolios to hold as much capital as those with much greater risk. So the Committee agreed to a set of arbitrary risk weights, or risk buckets. The original Accord only covered credit risk, setting minimum capital as equal to 8 percent of risk-weighted assets. An agreement on some market risk was reached in 1996. Two important features of the risk weighting of Basel I were first, that government debt was accorded a zero risk weight and second, that the weight for most residential mortgages was 50 percent, whereas mortgage-backed (and other) securities—a bundle of presumably diversified mortgages—carried a 20 percent risk weight.[6]

Not surprisingly, financial markets continued to evolve, driven in part by the Basel Accord itself. Asset securitization took off in the 1990s, reflecting several factors, notably the differential risk weights in Basel I and an increasingly quantitative approach to risk management, which in turn drew on the continuing steep declines in the costs of computing and communicating, as well as advances in finance itself. The result was that banks shed assets with higher risk weights to economize on their capital. The merger boom likely fed this process as well. In addition to the changing environment, the Basel Committee's recognition of the need to amend its Accord also responded to the criticism of the arbitrariness of the weights and in particular that the 100 percent risk

category included many disparate assets with different risk attributes. Moreover, the Committee's first Accord was quickly adopted, at least in name, by most countries around the world.[7] Thus, in addition to a revision of the risk weights, the Committee, after repeating for years that its first Accord was not intended to apply to developing countries, appeared to be motivated by achieving a more general agreement. The Committee expanded its membership and the group with which it consults (see Tables 1 and 2). After a lengthy search for a new basis for assigning risk and eventually a new compromise, Basel II was published in 2004.

Basel II was based on three pillars: minimum capital requirements, supervisory review, and market discipline. However, it was immediately noted that the pillars were unevenly developed. The bulk of the Accord was devoted to the first pillar, with the fewest pages—15 of the 239—dedicated to market discipline. Bank supervision was the focus of 17 pages, but this material was supplemented by many documents on the Basel website, as the BCBS had already issued its Core Principles on Bank Supervision in 1997 and much of the Committee's intervening work had been devoted to this area. Basel II's first pillar on capital (the focus of the revised Accord) also was distinguished by four variants: simplified standardized, standardized, internal ratings-based (foundation), and advanced internal ratings-based. What were the key differences? Risk weights in the first variant essentially were those of Basel I, except that the risk categories of export credit agencies could be used for sovereign risk; the second featured a few more risk categories and allowed the setting of weights according to the risk ratings of the export credit agencies or the rating agencies (Fitch, Moody's, Standard & Poor's); the third allowed banks to use their internal models to estimate their loss given a default; and the last allowed banks to go even further in using their own models to decide on their own risk weights.[8] Low- and middle-income country authorities complained that the rating agencies were pressuring them to adopt more sophisticated variants of Basel II than appropriate for the stage of development of their banking system. Private consulting firms rushed to sell models and technical assistance for their implementation—and even offered to run the models with U.S. data when countries did not have sufficient data of their own!

Most higher-income and some middle-income countries were adopting Basel II in the 2004–08 period. But then the process was hit by the financial crisis. This event was especially jarring for the Committee because a number of the countries, such as the United Kingdom, the United States, and the largest EU countries, whose regulatory and supervisory systems under Basel were essentially the model for others, were the ones most seriously affected. And although

TABLE 1
Membership in the Basel Committee on Bank Supervision

Argentina	Central Bank of Argentina
Australia	Reserve Bank of Australia Australian Prudential Regulation Authority
Belgium	National Bank of Belgium
Brazil	Central Bank of Brazil
Canada	Bank of Canada Office of the Superintendent of Financial Institutions
China	People's Bank of China China Banking Regulatory Commission
France	Bank of France Prudential Supervision and Resolution Authority
Germany	Deutsche Bundesbank Federal Financial Supervisory Authority (BaFin)
Hong Kong SAR	Hong Kong Monetary Authority
India	Reserve Bank of India
Indonesia	Bank Indonesia
Italy	Bank of Italy
Japan	Bank of Japan Financial Services Agency
Korea	Bank of Korea Financial Supervisory Service
Luxembourg	Surveillance Commission for the Financial Sector
Mexico	Bank of Mexico Comisión Nacional Bancaria y de Valores
Netherlands	Netherlands Bank
Russia	Central Bank of the Russian Federation
Saudi Arabia	Saudi Arabian Monetary Agency
Singapore	Monetary Authority of Singapore
South Africa	South African Reserve Bank
Spain	Bank of Spain
Sweden	Sveriges Riksbank Finansinspektionen
Switzerland	Swiss National Bank Swiss Financial Market Supervisory Authority FINMA
Turkey	Central Bank of the Republic of Turkey Banking Regulation and Supervision Agency
United Kingdom	Bank of England Prudential Regulation Authority
United States	Board of Governors of the Federal Reserve System Federal Reserve Bank of New York Office of the Comptroller of the Currency Federal Deposit Insurance Corporation
Basel Consultative Group	(See Table 2 for full list of jurisdictions and institutions)
Secretariat	Bank for International Settlements
Observers on the Basel Committee:	European Commission European Central Bank European Banking Authority International Monetary Fund Financial Stability Institute

Source: Basel Committee on Banking Supervision, http://www.bis.org/bcbs/membership.htm

TABLE 2
Basel Consultative Group

Chairman: Karl-Friedrich Cordewener, Switzerland **Chairman:** Bryan Stirewalt, United Arab Emirates	
Austria	Austrian Financial Market Authority Group of Banking Supervisors from Central and Eastern Europe
Bulgaria	Bulgarian National Bank
Chile	Banking and Financial Institutions Supervisory Agency
China	China Banking Regulatory Commission
Cote d'Ivoire	Commission Bancaire de l'Union Monétaire Ouest Africaine
Czech Republic	Czech National Bank
Germany	Deutsche Bundesbank Bundesanstalt für Finanzdienstleistungsaufsicht
Hong Kong SAR	Hong Kong Monetary Authority
Hungary	Hungarian Financial Supervisory Authority
Isle of Man	Financial Supervision Commission Group of International Finance Centre Supervisors
Japan	Bank of Japan
Korea, Republic of	Bank of Korea
Malaysia	Central Bank of Malaysia Islamic Financial Services Board (IFSB)
Mexico	Association of Supervisors of Banks of the Americas
Netherlands	Netherlands Bank
New Zealand	Reserve Bank of New Zealand
Norway	Finanstilsynet
Philippines	Bangko Sentral ng Pilipinas
Poland	Polish Financial Supervision Authority
Qatar	Qatar Financial Centre Regulatory Authority
Saudi Arabia	Saudi Arabian Monetary Agency
Thailand	Bank of Thailand
Tunisia	Central Bank of Tunisia
United States	IBRD/World Bank International Monetary Fund
Switzerland	Bank for International Settlements

Source: Basel Committee on Banking Supervision, http://www.bis.org/bcbs/membership.htm

Ireland was not necessarily a model—in fact its banking system had been dangerously expanding for a decade with no regulatory check on its growth—it was given a very positive review in the Financial Sector Assessment Program Update (IMF 2006, p. 5):[9]

> *The Irish financial sector has continued to perform well since its participation in the Financial Sector Assessment Program (FSAP) in 2000. Financial institution profitability and capitalization are currently very strong, with Irish banking sector profits amongst the highest in Western Europe. Reflecting their good performance, the major Irish banks receive upper medium- to high-grade ratings from the international ratings agencies.*

Although the report notes some risks associated with housing and its possible adjustment, it concludes, "the financial system seems well placed to absorb the impact of a downturn in either house prices or growth more generally" (p. 6). This suggests that both the yardstick and diagnostic capabilities were flawed, in particular given the problems with Irish supervision in the years leading up to the crisis.

The latest variant by the BCBS is Basel III, agreed to in 2010 and subsequently revised.[10] It features a redefined and higher capital requirement, a liquidity requirement, likely a leverage requirement (the commenting period on this proposal ended in September 2013) and still greater complexity. In a speech on Basel III, Andrew Haldane (2011), a critic of the complexity of Basel, describes Basel I as having seven risk categories and requiring seven calculations, whereas he rates Basel II/III as having more than 200,000 categories with more than 200 million calculations, though the latter apply to the advanced model approaches, and it is unclear how one arrives at a minimum or maximum for either. However measured, it is undisputed that the complexity of regulation has increased, as reflected in the establishment of a Basel Committee task force on simplifying regulation. According to the press release issued with a discussion paper by the task force (BCBS 2013), Mr. Stefan Ingves, Chairman of the Basel Committee and Governor, Sveriges Riksbank said, "The Committee is keenly aware of the current debate concerning the complexity of the current regulatory framework. For that reason, the Committee set up a Task Force last year to look at this issue in some depth. The Committee believes that it would benefit from further input on this critical issue before deciding on the merits of any specific changes to the current framework."[11]

Although the Committee is to be commended for recognizing the criticism of the enormous complexity of Basel III, much of the history of Basel has been a relentless march to ever-greater complexity—and now it has spawned another task force. It is not clear that the process can be stopped without a fresh start and fresh perspectives in the group. In fact, the BCBS focus on risk-adjusted capital ratios, the key source of the complexity of its approach, is unabated.

By any metric then, banking regulation seems as complex as it is has ever been. Compared with the Federal Reserve Act (only 31 pages) and the Glass-Steagall Act (37 pages), it would be an arduous task even to count the pages or terabytes of regulations and interpretations for the Basel Committee, not to mention the Dodd-Frank Wall Street Reform and Consumer Protection Act (2319 pages, plus requirements for 330 rule-making provisions and more than 60 studies, BCL 2012, p. 172), the Vickers Report for the United Kingdom (a mere 26 pages), and the report of the Liikanen group (153 pages) for

the European Union and supporting studies and rules. Bank regulatory agencies in the United States, the United Kingdom, and the European Union are said to be substantially increasing the number of supervisors. According to the Basel Committee, as of their survey published in August 2013, about 100 countries either had implemented or were in the process of implementing Basel II that year, and about 72 were in the process of implementing Basel III (this definitely involves double-counting, as all 27 BCBS countries are listed in both categories). The BCBS is not solely responsible for this situation, as this list reflects; Dodd-Frank for example was driven by the crisis and domestic politics. Still, the approach to regulation that has been taken by Basel is a significant contributor to the present state of bank regulation.

3. What Are the Lessons of the Recent Crisis?

A popular canard is that the crisis that began in 2007 was "made in America," with other industrial countries affected by financial contagion. One explanation for why some countries were affected more seriously was their greater exposure to securitized assets that were largely generated in the United States. Yet authorities in the countries that suffered the most in that crisis—Iceland, Ireland, and the United Kingdom—have since put out multiple reports arguing that their crises were homegrown, in the sense that they would have happened even without the events following the demise of Lehman in September 2008.[12] The first two countries had only a minor degree of financial innovation, and like most crisis countries, none of them had any separation of commercial and investment banking—no Glass-Steagall—to repeal, which are two popular explanations for why the U.S. mold did not fit these crises. What they did have in common with the United States were incredible lending booms and, in the case of the United Kingdom, the expansion in mortgage lending was largely backed by short-term funds. Northern Rock failed because of the outrageous extent to which it played the yield curve—not exactly the first time in history that banks have gotten into trouble in this fashion, and not due to securitization, which was less prevalent in the United Kingdom compared with the United States. Official reports in all three countries conclude that the warning signals, notably the high double-digit growth of balance sheets, were clear in advance and that the crisis represented a failure on the part of regulators.

The general phenomenon that characterized crisis countries was the failure by the regulatory authorities to enforce their powers, notwithstanding the ludicrous risk-taking that was occurring. There is no dearth of examples. The stratospheric expansion of Icelandic banks, whose "assets" grew to an order of magnitude greater than the size of the economy, failed to attract much

supervisory resistance; supervisors there were singled out in a Special Investigative Report of the Icelandic Parliament (2010, chapter 21, pp. 98–104) as being understaffed, excessively meek in pursuing corrective actions, and willing to tolerate flagrantly risky behavior with little or no response. Or consider the slower but still suicidal expansion of the Anglo-Irish Bank's loan book (nearly 40 percent per year for a decade), which only elicited a letter of concern from the Irish Regulator and no follow-up for 2½ years. As detailed by the Central Bank of Ireland's 2010 annual report (2011, chapter 4), there were numerous examples of supervisory laxity on the part of the then-separate Irish Financial Regulator, notably that banks were frequently violating their own lending criteria without facing a response from the regulator. Like many regulatory agencies, the adequacy of staff resources was an issue. The 2010 report details (p. 62) how staff resources for bank supervision declined to a mere 13 percent of the regulator's total, but also the unfortunate fact that a group almost as large—11 percent of staff resources—was devoting its time to Basel Committee and EU affairs. The Irish report notes a similar issue was cited in the U.K. FSA's report (Financial Services Authority 2008, pp. 2–3) on its oversight of Northern Rock.

So rather than "sticking to the knitting" of bank supervision, staff among other activities were spending time dwelling on the complexities of Basel—another common factor that, while not limited to the crisis countries, was particularly intense there. Bureaucratic sport or mismanagement also played a role: Responsibility for supervising Northern Rock was kicked around the FSA like a football—three different lead supervisors in the two years before its failure. It is worth repeating the verdict from the UK report (p. 34) as quoted in BCL (2012):

> *The FSA did not supervise Northern Rock properly. It did not allocate sufficient resources of time to monitoring a bank whose business model was so clearly an outlier; its procedures were inadequate to supervise a bank whose business grew so rapidly. We are concerned about the lack of resources within the Financial Services Authority solely charged to the direct supervision of Northern Rock. The failure of Northern Rock, while a failure of its own Board, was also a failure of its regulator. As the Chancellor notes, the Financial Services Authority exercises a judgment as to which "concerns" about financial institutions should be regarded as systemic and thus require action by the regulator. In the case of Northern Rock, the FSA appears to have systematically failed in its duty as a regulator to ensure Northern Rock*

would not pose such a systemic risk, and this failure contributed significantly to the difficulties, and risks to the public purse, that have followed.

While the Irish and Icelandic reports have similar language criticizing their own regulators, the U.S. Financial Crisis Inquiry Commission was less harsh with both U.S. regulators (notwithstanding the examples below) and the banks. Apparently, it is more popular on this side of the Atlantic to say that mistakes were made than to detail who made them. These examples do not mean that new regulatory measures are unnecessary, but they do suggest that the lack of attention to enforcement and regulatory oversight is a grave shortcoming of many recommended responses to the crisis. And to the extent that new regulations are needed, regulatory officials were not known to be complaining in the run-up to the crisis that they needed more resources or powers. Indeed, the U.S. Securities and Exchange Commission (SEC) went on record in testimony to assure Congress that it was on top of its job of supervising the investment banks. For commercial banking, the revolution in risk management and in the sophistication of thinking about bank regulation, as embodied in Basel II, were regarded as a source of strength. Potential reasons why regulators were not more active in protecting the public's interests are discussed later.

Regulatory laxity also was a clear concern in continental Europe, where a devotion to Basel was perhaps most intense. Banks there, along with other financial intermediaries, notably insurance companies, were buying securities with higher rates of return than other securities in their risk class. The claim that European and U.S. regulators were trusting the ratings on the securities is hardly a defense; given the strong positive correlation between risk and return, vigilant regulators would have been asking whether these higher return securities were as safe as those with comparable high ratings (e.g., comparing AAA-rated collateralized debt obligations (CDOs) with AAA-rated corporate bonds). There are no reports that those questions were raised.

Regulatory laxity of course was an important factor behind the U.S. crisis. BCL (2012) cite numerous examples, including the following: the Fed's late-1990s decision to allow banks to lower their capital by buying certificates of deposit from entities that the Fed did not oversee, and thus depend on the rating agencies' views; the Fed and the Treasury's Office of the Comptroller of the Currency ignoring information on widespread fraud in mortgage markets in the early 2000s and other incontrovertible evidence (e.g., widely advertised NINJA loans) of heightened risk in banks; the Federal Deposit Insurance Corporation's failure to act promptly even to intervene in the case of small banks (this from its

own Material Loss Reviews); and numerous and flagrant instances of the SEC defaulting on its regulation of investment banks and ratings agencies, both of which affected commercial banks.

An important and easily observable factor in the crisis was the sea change in compensation in some countries, well documented in the U.S. case by Philippon and Reshef (2012), which began slowly in the 1980s and then accelerated in the mid-1990s. Although it is difficult to get data on compensation in the financial sector on a cross-country basis, pay packages that favored returns and did not adequately, if at all, weigh risk seem to have been pronounced in the crisis countries and especially in the banks that were most in need of support. As in the United States, pay in a number of European institutions emphasized returns and growth (BCL 2012, chapter 5), and these changed incentives seem to explain how separate units in banks such as UBS could play a role both in generating assets that were said to contain "toxic waste" and yet be on the buying side as well for these instruments. Staff in both parts of that bank clearly, at least to their auditor, were being paid for returns, without regard for risk.[13] Such behavior is in line with Akerlof and Romer's (1993) framework, as this type of compensation scheme is a form of looting, with the only uncertainty being how long it will take before the institution fails and those responsible escape.

How did this change in incentives in the financial system take place? After all, many U.S. and European countries had gone for years without a systemic financial crisis, notwithstanding the turbulence of the 1970s, 1980s, and 1990s. One change that likely played a critical role is the merger movement in the United States and Europe, the former in response to the ending of limits on interstate branching in the 1980s and 1990s (Strahan 2003), and the latter as a result of the drive to a Single Banking Market that picked up speed in the late 1980s (Kleimeier and Sander 2007).[14] As mergers take off, banks tend to focus on the business of growing themselves—partly based on survival, partly because bank executives discover that it is much more remunerative—not to mention more flattering to one's ego from more media attention or potentially more political power—to be the CEO of a large bank than of a smaller one. When senior bank management assigns top priority to the growth of their institutions, they tend to base compensation more on returns; they are also less concerned about financial risk, since there is also a risk to slow growth, namely being taken over and even pushed out of a job. Except in recessions and outright bank crises, markets tend to value expanding banks higher relative to their sluggish competitors. And of course this type of pay structure is consistent with the Akerlof-Romer looting story. Bankers know that when they hold

stock or stock options, they get the upside of their risk-taking. To the extent that these risks pay off in the short run, the longer-run consequences are less relevant. Volume-based compensation models, which first took root with the "2 and 20 percent" formula[15] for pay in hedge funds, spilled over to investment and even commercial banks, as business lines blurred. BCL (2012) even note that rating agencies—unbelievably—adopted a similar model, with pay based on the volume of securities rated. Compensation packages that generously rewarded returns or the volume of business permeated the sector in part due to overlapping labor markets—some people moving from commercial or investment banks to hedge funds, or from rating agencies to a bank—but even more to the emphasis in the sector on growth. The sharp expansion of banks internationally (BCL 2013) was part of the boom in the size of U.S. and European banks.

The consequences—more highly rated securities and more risk—were surprising only to those not paying attention. Risk taking was most pronounced where it was least regulated, which is why the large investment banks were the ones that failed or had to get access to the Fed's support by becoming commercial bank holding companies. They had survived for years with more prudent pay practices as partnerships, where the partners had a truly long-term interest in the firm's health (no put option there), but thanks to financial globalization and increased competition—including more mergers or takeovers in their sector—they went public, thus making their senior management agents for the shareholders rather than the principals of the firm.

This view on compensation is not without some controversy. Although Bebchuk, Cohen, and Spamann (2010) and Bebchuk, Cremers, and Peyer (2011) find support for it, Fahlenbrach and Stulz (2011) contradict it, arguing among other things that the most senior management (top five executives) of Bear Stearns and Lehman lost a significant amount of money when their firm failed. However, this latter view ignores that in the Akerlof-Romer looting framework, the stock price might merely be the tool for holding up the bank: An inflated stock price—and the excessive risk taking that fueled it—was the instrument by which senior management cashed out hundreds of millions of dollars in the years leading up to the crisis (as Bebchuk et al. showed). Might they have cashed out more without speeding the collapse of their firm's stock price? That is not clear, and it is evident that the amounts that they extracted were considered, even by bank executives, to be a fortune. Moreover, the study of the compensation of the top five executives of banks—all that is permitted by the data—necessarily ignores that many other executives were paid extraordinarily well for earning higher returns without regard to risk, as was well documented in the case of Lehman and UBS.

Those who view the crisis as an American affair, spread by contagion to a group of innocent bystanders, often argue that it was the repeal of Glass-Steagall that caused the crisis. In addition to the point above (no Glass-Steagall there) on countries such as Iceland, Ireland, and the United Kingdom, this view has trouble explaining why some countries saw a serious financial crisis while others did not.[16] To be sure, one factor in the severity of the crisis was the drying up of liquidity, but this was an event that was truly global. If the channel were primarily portfolio links, it does not appear to have been the case that the three hardest hit crisis countries in Europe were particularly large purchasers of CDOs. Instead, these countries had their own domestic lending bubbles that were set to explode, they were fed by incentive systems that favored risk, and they were largely unrestrained by regulation and supervision. Ireland even adopted procyclical fiscal policies that worsened their eventual crisis and adjustment problems (Lane 2003).

Many assumed that the adoption of extreme compensation models and the pursuit of growth at all costs—what would be regarded as irresponsible behavior on the part of management—would not occur; well-governed financial institutions were supposed to have the incentive to look after their business, and the fact that so many institutions engaged in the above types of compensation and took on absurd risks likely was part of what Alan Greenspan meant when he famously testified that his model of the world failed.[17] However, it has long been acknowledged in the corporate governance literature that shareholders with limited liability tend to favor greater risk, compared with creditors, as only the former benefit from the upside of risk taking, whereas excessive risk jeopardizes the promised returns for the latter. Similarly, although a well-governed institution presumably was thought to be one in which management answers to all shareholders, the difficulty of successfully addressing this principal-agent problem also is discussed in many texts.

Another at least debatable conclusion from the crisis is that higher capital ratios based on equity alone will insulate the economy from banking crises. Although at least ex post it is true that banks with more capital would have fared better, it is not clear that higher risk-weighted minimum capital requirements would have left them with more capital or less risk. In addition to the theoretical literature suggesting that the impact of higher capital requirements is ambiguous (Koehn and Santomero, 1980; Buser, Chen, and Kane, 1981), with one reaction being to take on more risk, some empirical evidence also is relevant. BCL (2006), using a large cross-country database, found no robust impact of tighter capital requirements, given the variation in those requirements as of the late 1990s, and Laeven and Levine (2009) showed that the impact of regulation,

including capital requirements, varies with ownership structure. Banks with more concentrated ownership tend to take on increased risk with an increase in capital requirements.

A limitation of these empirical studies is that the data are from the late 1990s, a period when capital ratios alone fluctuated within a relatively narrow range, compared with its historical variation since the mid-19th century, though the definition of capital requirements utilized includes factors that capture related requirements affecting the stringency of the definition of capital, which varies widely.[18] Still, these studies should be interpreted as suggesting that modest variations in capital requirements might have little impact, whereas a substantial increase—say to 20 or 25 percent—is outside their sample, so that their empirical findings might not apply. This appears to be a plausible point, because the possibility that banks with higher capital requirements might indulge in greater risk taking depends on that behavior not being observed by the market or by regulators. Although even a doubling or tripling of capital requirements could induce greater risks on the part of banks, the increased risk taking might be thought to be so large that it would be obvious to all. However, the absence of a regulatory response in the run-up to the recent crisis, which saw a substantial increase in leverage and risk in banking, belies this reasoning. Many who hope that higher capital requirements will lead to safer banking systems point to times in the late 19th and early 20th century when capital ratios were much higher and bank failures less costly (Calomiris and Gorton 1991); however these earlier times differed in a number of dimensions (e.g., the many U.S. states and countries that had double or higher liability limits).

A final and misunderstood lesson of the crisis is the impression that the market supposedly missed it, and that therefore ever-greater reliance on official supervision and regulation is needed. In addition to the profits made by those who were vigorously shorting the housing market, some simple market ratios (Tobin's *q*) showed that equity markets were distinguishing between the crisis banks that had to be bailed out or merged with others, compared with stronger banks, several years before the crisis (Haldane 2011).[19] Unfortunately, this information, embedded in equity pricing, did not lead to any regulatory action, and some of the troubled banks, such as Northern Rock, even were allowed to adopt the advanced internal ratings-based approach (a regulatory blue ribbon) and increase dividends shortly before they failed. The supervisory community's interpretation of the crisis as demonstrating that market monitoring does not work, and therefore that supervisors must step up their efforts, is ill-founded. Indeed, BCL (2012) show that this response was similar to those following earlier crises: more rules, with little attention to information and enforcement.

Instead, the conclusion might be that supervisors should spend less time on risk management and more time mastering—and disclosing—information that is in the market.

4. Basel as a Regulatory Model

Perhaps the best-known drawback to the Basel approach is its complexity. Haldane and Madouros (2012, p. 4) argue that "the more complex the environment, the greater the perils of complex control." They also contend, as do Blundell-Wignall and Atkinson (2008), that banks were using Basel I and Basel II to reduce capital, returning funds to shareholders by reducing their higher risk assets. The latter notes that Citi's holdings of assets not requiring capital rose to close to half its overall balance sheet. Some riskier assets were being moved to off-balance-sheet entities. We do not know—thanks to the confidentiality of supervisory information such as that embedded in bank examination reports—if regulators ever considered that these assets might come back to the originating bank's balance sheet, or if they worried about it daily.

Much of the complexity associated with Basel is a result of the attempt to gauge the risk of banks, and the BCBS shows no sign of backing away from this orientation.

In fact, it is clear that the Basel Committee wants all banks to limit the variation in risk weights for the same or similar assets. In the press release for the "Report on the regulatory consistency of risk-weighted assets in the banking book issued by the Basel Committee" (2013) Stefan Ingves, Chairman of the Basel Committee and Governor of Sveriges Riksbank, said:

> *While some variation in risk weightings should be expected with internal model-based approaches, the considerable variation observed warrants further attention. In the near term, information from this study on the relative positions of banks is being used by national supervisors and banks to take action to improve consistency. In addition, the Committee is using the results as part of its ongoing work to improve the comparability of the regulatory capital ratios and to enhance bank disclosures. The Committee will be considering similar exercises to monitor consistency in capital outcomes and assess improvement over time.*

In other words, the BCBS has no intention of jettisoning its risk weights, and its mission seems to be to have every bank assess risk in the same fashion. It is as if the BCBS sees itself as overseeing risk management in banks.

Basel's approach to risk weights and risk models results from a confusion of regulators' responsibilities with those of the market. Communist governments failed at the same task, trying to micromanage firms rather than allow prices and the profit motive to send the signals, and discovered instead that not only does replacing market forces require a large bureaucracy but is ineffective as well. As documented in older editions of economics texts, Soviet planners found that they had to go beyond specifying the feet or pounds of glass output in order to avoid market participants adjusting in undesirable ways, such as producing glass either in sheets so thin as to guarantee a high rate of breakage, or so thick as to be opaque. In other words, people adjust to regulation based on their incentives, and those designing regulatory interventions need to factor this tendency into how they operate. This does not suggest that bank regulators should not have rules—some are suggested below—but that if bank management is not doing its job and corporate governance is not working, then they should look to the deeper causes and avoid complex approaches to regulation.

This Basel approach has been a key contribution to financial crises since the late 1990s. Although the BCBS treats risk as an exogenous characteristic of assets, in fact it is endogenous. Persaud (2000) and Danielsson et al. (2001) made this point early on when Basel II was still under discussion, but the BCBS has not effectively responded. Whether it is requiring banks to have the same risk weights (Basel I) or to use the same or similar models (Basel II and III), the Committee's assumption is that risk is an exogenous property of various assets and that it can be estimated. However, the act of encouraging all banks to look at risk the same way and to reward them when they increase the proportion of low risk assets in their portfolio increases the fragility of the banking sector. First, it increases the funds that are available to the asset classes that are claimed to be low risk, even though these estimates are based on a time when those assets had less funding available. Increased funding by the banks (and other sources) changes this important fact. Second, it increases the demand for assets that can be labeled as low risk, which in turn creates incentives to boost the supply of such assets. Basel's approach to risk weighting, along with the U.S. approach to sanctioning certain rating agencies and the passive acceptance of these ratings by regulators generally, led to an explosion in the revenues of these firms and a fundamental change in their internal incentive systems (BCL 2006).[20] The billions of dollars in commissions that were available to those creating complex securities at least in part were the result. Third, it ignores that a given risk exposure entails different risk for different banks to the extent that their portfolios differ and therefore the given exposure's correlation with that

portfolio will differ. This goes back to an original sin of Basel: Risk weights were assessed one asset at a time, rather than recognizing that capital should be held based on the likelihood of unexpected losses for the portfolio as a whole. Basel II and III try to make ad hoc adjustments to take account of this consideration, but Ingves's quote and the BCBS drive to make risk modeling consistent suggests that it remains unappreciated.

Danielsson et al. (2001) argue that ignoring the endogeneity of risk is innocuous in normal times but deadly in a crisis, because it encourages a simultaneous run for the exit, that is a simultaneous dumping of assets and drying up of markets for these assets as only sellers are to be found. The authors point to the Russian crisis of 1998 as an example of the impact of similar trading strategies on bringing about a crisis. However, now there is more evidence of the effects of the Basel approach to risk in recent crises. These events should make clear that ignoring the endogeneity of risk is dangerous even in normal times, because these are the times when exposures are built up and risk is changing. By rewarding banks for holding highly rated securities, Basel helped create the immense rewards that were to be had for manufacturing these securities, and for the buildup in banks' exposure to so-called highly rated instruments, such as mortgage-backed securities and CDOs. Yet the rating methodologies were long known to be faulty (BCL, 2006, pp. 68–73). The rating agencies' models were recognized as flawed, they paid little attention even to diversifying the credit risk of the bundled loans, they ignored the changing population of borrowers and the fact that by representing financing at the same point in time, these securities shared interest rate and credit risk, even though diversifying this risk was the justification for securitization.[21] Moreover, a reliance on a similar approach to modeling ignored model risks: The model might be estimated with limited data, and any data set is inadequate since the adopting of modeling changes the world by increasing the covariance between banks' risk profiles. Thus a similar approach to risk contributed to the changed incentive system in banking and finance more broadly, and to the massive buildup of exposures to real estate and other forms of risky debt (e.g., Icelandic paper, which was bundled in some CDOs).

In addition to helping explain the severity of the 2007 crisis, the Basel approach also is a culprit in the European crisis, though with many coconspirators. Basel I assigned risk weights of zero to all sovereign debt in Organisation for Economic Co-operation and Development (OECD) countries.[22] Although Basel II allowed for a more varied treatment, the European Union assigned a risk weight of zero percent for "exposures to Member States' central

governments . . . denominated and funded in the domestic currency of that central government (EU Directorate-General 2010, p. 6)." As a result, capital flowed to the periphery. Funding to Greece, for example, might have been in ample supply as a result of its membership in the euro and the assumption by euro-area banks that those governments would stand behind Greece. Still, it is likely that part of the capital flow resulted from this approach to risk weighting. The European Union shares in the blame, but once a principal of zero risk for a government is established, it seems unrealistic to expect that any government could say that it was a higher risk than others. A system that relies on governments to commit political suicide in order for that system to work is not well conceived. And of course had the European Union instead insisted either that risk models or ratings from rating agencies be used for sovereign risk, the aforementioned problems apply: The models failed to account for their simultaneous adoption by others, and ratings of sovereign debt are notoriously lagging indicators.

A legacy of both of these crises is a debt overhang and years of misallocated capital, both of which are contributing to lower growth. It is impossible to rerun history to see whether banks would have indulged in the same risk taking—after all, there were banking and real estate related crises long before the Basel Committee existed, and as noted in the previous section and the literature cited, there was no shortage of factors behind the crisis of 2007. Similarly, the Euro crisis was well anticipated by economists who pointed out that a fiscal and banking union were essential prerequisites for monetary union, and who knew well the lesson of Bretton Woods, namely that a fixed rate system without these prerequisites and missing symmetric pressure on deficit and surplus countries is doomed. However, it is reasonable to view the virulence of these crises as in part a direct result of the Basel approach. And barring a change in that approach, it will exacerbate the next one.

Returning to the issue of complexity, an additional consequence is that it makes it incredibly difficult to hold regulators accountable. Regulatory accountability already is made difficult by the confidentiality of information—an issue that needs revisiting—because it is impossible for the public or legislators to find out what the supervisors knew and when. BCBS guidelines on supervision focus on the information that banks are required to make available to the supervisor, not to the public, nor do they have standards for supervisory disclosure.

Complexity also favors big banks—they have the large staff to deal with an increasingly cumbersome and costly approach to regulation, and thus can exacerbate the issue of excessive size and concentration in the sector. Thus

Basel's approach to regulation may have been a factor in the consolidation of the sector, noted above, though it is difficult to quantify its importance.

5. Lessons

Paul Krugman has said that the past year or two has seen a remarkable change in the conventional wisdom on a number of macro issues—at least in his view.[23] What is remarkable is that, following one of the most wrenching financial crises in history, the approach to financial regulation is essentially more of the same—a bit higher but still complex capital ratio, supplemented by a liquidity ratio and possibly a low leverage ratio. This review of where we are argues that it is time for a change. What guidance then might be offered, in particular given the focus of this conference, for Asian countries? Some conclusions are suggested by the above arguments as well as by recent research.

Recalling the opening quotation, no one would mistake this paper as an attempt to praise Basel. Burying it and starting over is an attractive proposition, as changes in orientation and clear thinking are demanded. A new committee, perhaps with a different meeting place, dedicated to looking at regulation and supervision from a systemic or macroprudential vantage is long past due. If Asian governments and emerging market authorities more generally were interested in increasing their role in what has been the Basel Committee, this change in direction should be a first priority. This new group—perhaps the Bali Committee—should be composed of those with responsibility for macroprudential regulation and should reach out to the researchers who are active in this area, rather than basing their work on the least common denominator approach of the Basel Committee. The recommendations here apply wherever the group meets—even if it is a very different group meeting in Basel.

An early accord by the Bali Group should feature an abandonment of risk weights and an adoption of a simple, unweighted capital or leverage ratio. This would not only end the gaming of the system but remove an important source of increased covariance in banks' exposures. Members of the current Basel Committee might argue that this would allow some banks to price their loans below other banks, and thus create an unfair competitive advantage—a "tilted" playing field. The point is, to the extent that their exposures differ, variations on loan pricing should be accepted, as long as banks meet other regulatory requirements. To the extent that this loan pricing reflects excessive risk taking, there are better ways to deal with it than a horde of supervisors and a mass of complex rules. Banks with a large exposure in one area (a given sector, or their home market) would benefit from diversifying into another, but would meet with

more competition in the former area from banks with the opposite portfolio. Stability would no longer be sacrificed on the altar of leveling the playing field.

Second, however high the capital ratio, and partly because not only is it impossible to know how high to raise this requirement but also in all likelihood the regulatory community will adopt a ratio too low, it is important to impose a conditional convertible debt requirement (CoCos), along the lines proposed by Calomiris and Herring (2013). Well-designed CoCos would provide a more continuous cushion to protect taxpayers from having to inject funds into banks and importantly would serve as a check on banks that attempted to increase their risk even in the face of high capital levels. CoCos only would be effective if debt is not bailed out, though even the uncertainty of a bailout would encourage monitoring through this market, and would discourage greater risk taking and loan underpricing. CoCo holders would not care about how banks price a given loan in a single market, but rather would monitor their overall pricing and risk management strategy. There are objections to some plans for CoCos, but the Calomiris-Herring plan meets them, and in effect functions like a gun pointed at the heads of managers, set to go off well in advance of bank failure.[24] Similar to proposals calling for mandatory subordinated debt, it would be important to make sure that the holders of CoCos have an arm's-length relationship to the bank, a job that supervisors, freed from worrying about risk weights, could fulfill. CoCos also could be used to improve the incentives for bank managers; as proposed by the Squam Lake Group (2010), banks might be compelled to hold CoCos in their bonus pool, with the requirement that their bonds convert to equity before those of other CoCo holders, effectively insuring that they would take a loss.

Little has been said here about the issue of "too big to fail." Even those who insist on the need to downsize banks have no analytical approach to determine where to draw the line. CoCos would help deal with this problem, as Calomiris and Herring point out, provided of course that governments are not there to bail out debt holders before the conversion takes place. The other key elements of their proposal—setting the trigger so that conversion occurs well before insolvency, and making sure that existing equity holders are subject to a painful dilution—are critical to improve the monitoring of large, complex banks. Indeed, such banks might find it so painful to sell CoCos that they would downsize on their own.

Third, consideration should be given to some simple rules. Claessens, Ghosh, and Mihet (2013) find that measures to discourage excessive borrowing, such as limits on debt-to-income ratios, loan-to-value ratios, and overall limits on credit

growth and foreign currency lending, could be effective to limit booms during their expansion phase. Of all these measures, restrictions on loan-to-value ratios for mortgages seems like the most promising, and also useful not just for prudential reasons but also for consumer protection—as some who purchased homes with no down payment near the peak of a housing cycle discovered.

More radical still, it is past time for a different approach to regulation and supervision. The orientation of the Basel Committee has been to focus on the information available to the supervisor, and has seen the supervisor almost as a risk manager for the banks they oversee. Yet BCL (2006) found no evidence that supervision works in contributing positively to the development of the financial system or its resiliency to crises, and recent crises show that supervision was ineffective.

Thus I suggest two further key changes. First, whatever regulators and supervisors do, they must face some credible accountability. Finance is dynamic; so too must be its regulation. Most static rules are possible to evade, implying that regulators must be given some discretion to respond. However discretion demands close accountability, otherwise regulators could become (even more) direct agents for banks, and the poor performance of regulators in crises requires effective monitoring as well. BCL (2012) argue that like sports referees, regulators were biased. While standard models of regulatory capture might apply, it is plausible that psychological capture is at least as important. In sports, it has been convincingly argued (Moskowitz and Wertheim 2011) that the key explanation of home field advantage—the fact that in all refereed sports, home teams win more games than visitors—is the influence of the fans on the referees. Perhaps the most convincing evidence is from baseball, where electronic cameras—before their presence was known to the umpires—showed that the strike zone when the visiting team was at bat was significantly larger than for the home team.[25] Numerous other examples of referee bias were found, even though the referees maintained that they were doing their job in an unbiased fashion. Moskowitz and Wertheim note, however, that humans have a psychological need to be liked, and that the home team advantage has decreased in sports that have adopted instant replay technology.

BCL (2012) suggest that in banking, the bankers play the role both of the home team as well as the fans sitting in the plush box seats near the field. The public sits far up in the stands (in the nosebleed seats), so far removed from the action that they cannot see what is going on and even have trouble understanding the game. BCL argue for the creation of a sentinel, a watchdog group that would have access to all of the information regulatory agencies collect and would have the job of publishing a regular report on the key systemic risks in

the banking sector and what the regulators were doing about them. The goal is to instill greater regulatory accountability; the sentinel would have no regulatory power whatsoever, just the power to interpret and reveal nonproprietary information. BCL also discuss some of the operational issues with making their proposal effective, including the need to offer compensation sufficient to offset severe limits on private sector employment. By revealing the key systemic issues in banking and what the regulators are or are not doing about them, the sentinel serves as a type of instant replay that has worked in sports to reduce home field advantage. Thus a sentinel might have flagged that the Irish regulators were not stopping the 40 percent growth rate of Anglo-Irish Bank, or that this bank was violating its own lending guidelines in an alarming proportion of its loans; that the Fed was not acting despite its information about mortgage fraud; or even that overall leverage in several economies was increasing to alarming levels, calling for increased oversight. A sentinel will not guarantee that regulators will act, but it should increase the odds that they will.

Second, with risk weights ended, an important focus of regulation should be increasing the transparency of the banking system. Holders of CoCos want the best possible information, and supervisors' jobs could center on compelling banks to disclose more information, ensuring that this information is accurate, and assessing penalties for inadequate or misleading disclosures.[26] As seen in the last crisis, although many knew of the lavish compensation in the financial sector, it was not well known how salaries were determined, and more disclosure in this area would be quite helpful in serving as a check on potential looting behavior. Regulators now regularly assess banks' risk management systems. Indeed, how risk is rewarded, including board oversight, is and should be the most important determinant in this assessment. Merely publishing these scores would not violate anyone's privacy and yet would send a signal to bank creditors and shareholders about which were excessively risky compared with those more prudently managed (those paying out much of profits as current rather than deferred compensation, compared with those paying bonuses deferred to the future, with claw-back features, or with debt). This disclosure is appropriate for any financial intermediary. If CoCo holders, other creditors, and shareholders had more information on how compensation was being awarded at Lehman or AIG, as well as at WaMu, Northern Rock, or Anglo-Irish, their unhappiness likely would have been revealed in the prices of debt and equity.

An attractive feature of this approach is that markets and regulators would in effect be working together to support one another: More information, reviewed by supervisors, would improve monitoring by those with funds at risk, and clearer signals from the market (e.g., it would be difficult to ignore the

signal when CoCos are triggered) would tell both management and supervisors when banks need to be wound down. A sentinel or some substitute group, by holding regulators more accountable, would contribute to the quality both of regulation and the information available in the market.

With the end of risk weighting, it would be useful also to end the encouragement or requirement to hold highly rated instruments in other parts of the financial sector (e.g., for pension funds or insurance companies) and to end the category of Nationally Recognized Statistical Rating Organizations (NRSROs). Before these changes, rating agencies were tiny, because they added little value (Partnoy 1999 and Sylla 2001). The SEC has repeatedly shown that it exerts no effective regulation over the NRSROs, and the existence of this category, along with legal requirements or inducements to hold highly rated paper, makes it difficult for those harmed by these ratings (e.g., the pensioner who suffers when his pension fund buys highly rated paper that plummets in value) to seek legal redress. Without the comfort of these ratings, institutions will hesitate before buying complex securities, which is exactly what regulators who care about protecting their citizens should desire. National authorities should not wait for U.S. actions, as misleading ratings have contributed to the perversion of incentives in the financial system.

Final areas for consideration are the most challenging, having plagued financial regulation since medieval times when usury restrictions were circumvented. Goodhart (2010) has emphasized that as a result of boundary issues (the ability of regulated entities to shift prohibited activities to unregulated domains, whether in another part of the financial system or another location), it is better to think of controls as continuous variables rather than on-off switches, to lessen these concerns. The recommendation on CoCos is an application of his point; rather than attempt to draw a line that prohibits activities or constrains size, CoCos should work to gradually raise the cost of undesirable attributes of banking, such as excessive complexity. Similarly, a binding ceiling on pay would just drive risk taking on a wholesale basis into a less regulated part of the sector; greater disclosure of compensation practices might encourage some shift but would act as a countervailing force to the pressure of competition from other parts of the financial system to force an imprudent reward of risk in banking. Boundary issues are difficult, should be an important consideration in regulatory design, and are yet another reason to give regulators discretion. For example, allowing bank regulators to define a bank would give them the power to extend reserve and other requirements to money market funds, which owe their existence entirely to regulatory arbitrage.

These recommendations, some of which would mark a sharp departure for bank regulation, presume that regulatory decisions are driven by results. Unfortunately, as Calomiris and Haber (2014) contend, political factors likely are the real driving force of what countries do, international politics included. Thus regulatory failures in their view (for example, the limits on branching earlier in U.S. history) are the result of political coalitions forming to secure the adoption of rules that benefit them. In this interpretation, large banks (along with regulators and perhaps even the hotel and restaurant industry in Basel) have been the primary winners from a complex risk weighting system and have outmaneuvered the general public, which suffers from crises. Merely moving the meetings from Basel to Bali will not change this dynamic, even though the assertion of a greater role by Asian countries and other emerging markets will upset this process. That is precisely why a sentinel, meaning some oversight of regulators, is so important, as it would at least tip the scales a bit less against consumers and taxpayers in the battle over regulation by exposing the action of regulators. We have tried regulation without accountability and oversight and seen its sorry results. Is it not now time for a change?

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NOTES

1 Though hardly an arbiter of effective regulation, *Time* magazine's September 23, 2013, cover captured the concern, "How Wall Street Won: Five Years After the Crash, It Could Happen All Over Again."

2 The word "formerly" is used because, as will become clear, the much vaunted regulatory best practices that were the model for the rest of the world, and the supposed state of the art in bank supervision, were part of the problem that caused the crisis.

3 See Besley et al. (2009). Interestingly, the perfect storm explanation was first seen in a number of letters that hedge fund managers sent to their clients in fall 2007 explaining the unusual losses incurred. Some went so far as to state or imply that even if the history of the universe were rerun a number of times, events seen that summer still would not have been anticipated. An alternative explanation—that their models were wrong, in part by ignoring the increased correlation of risk—was not voiced until much later.

4 This statement reflects the perspective of someone who was working at the Federal Reserve Board in the late 1970s and early 1980s, as the adjustment was beginning, and at the World Bank in the late 1980s and 1990s as deregulation was spreading around the world. Unfortunately, although the World Bank's first survey on Bank Regulation and Supervision attempted to get comparable data going back in time on budgets and staffing, it was not possible to do so.

5 Goodhart (2011) provides the definitive history of the Basel Committee. The BCBS website contains many gigabytes of documents on the Basel process, but Goodhart benefited from some unpublished material as well.

6 As developing countries moved to adopt Basel I, they allowed their banks to use a zero risk weight for their own sovereign exposure, even though there is no evidence that the Basel Committee ever intended this application. Initially the committee's focus was on the largest internationally active banks in OECD countries.

7 For those not up to Goodhart's encyclopedic coverage, BCL (2006, chapter 2) contains a shorter description of the Basel I and II era).

8 Banks' expected loss can be written as the probability of default times the loss given default times the exposure at the point of default. Thus the foundation internal ratings-based approach allowed banks to use their models to estimate the second of these three elements, and the advanced approach permitted model estimation of the latter two. Powell (2004) presents a nice description of Basel II aimed at a developing country audience, reflecting the desire of authorities in many countries to move to that system.

9 The FSAP is a joint program of the World Bank and IMF, with the IMF having chief responsibility for assessments of higher income countries and the Bank correspondingly a lead role in developing countries.

10 Basel 2.5, like Windows ME, can best be passed over.

11 This statement is available on the BCBS website: http://www.bis.org/press/p130708.htm

12 See BCL, chapter 5.

13 See UBS (2008).

14 See BCL (2012, chapter 3) for more details on these merger movements.

15 According to this formula, pay would equal 2 percent of the assets under management plus 20 percent of the return above some benchmark, though with no sharing of losses. Since investors often rush into top-performing funds, it pays managers thus remunerated to take significant risks, have a good year, and cash in. Even though this strategy might lose over the longer run, without any claw-back provisions in contracts, the managers get to keep this inflated reward.

16 The link between the repeal of Glass-Steagall and the crisis is not always clear. For some it suffices to note that the period in which the Act was in force was free of systemic crises; for others, it might be that its repeal marked an increase in compensation in the financial sector. The latter arguably was due to the merger boom and, for investment banks, to the change in their ownership from partnerships to public companies, which made their former principals agents of the shareholders. Globalization likely was at least as important a factor in the need to go public as competition from commercial banks, and investment bank management certainly exploited the change to reward themselves lavishly. To the extent that these arguments hold, reinstating Glass-Steagall without a change in incentives will have little effect on the stability of the system.

17 Greenspan also famously said, "Through all of my experience, what I never contemplated was that there were bankers who would purposely misrepresent facts to banking authorities. You were honor bound to report accurately, and it never entered my mind that, aside from a fringe element, it would be otherwise. I was wrong." See Vaughan and Finch (2013).

18 The capital regulation index includes variables related to how tough provisioning requirements are, the sources of capital, and how authorities verify those sources. Thus although many countries adopted an 8 percent minimum risk-weighted capital ratio, they varied widely in the stringency of their provisioning rules, and lax provisioning standards leads to an overstatement of capital.

19 To be sure, other market indicators, such as credit default swap (CDS) prices, missed the crisis, but then some of these markets clearly were "polluted" by skewed compensation models—clearly at work in the writing of CDS contracts, for example.

20 Regulations encouraging or requiring other financial intermediaries (insurance companies, pension funds, etc.) to hold highly rated instruments also contributed to the increase in demand for these assets and the rewards for those who could create what appeared to be safer assets.

21 The most straightforward case is for a security that contained a number of adjustable-rate mortgages with initial low interest rates that were exposed to the risk of rising rates. Mortgages that had low or zero down payments or that were lacking verified information on borrowers shared greater credit risk, which was not factored into models used by the ratings agencies. Similarly, mortgages taken out when real estate prices were high both relative to historical values and to incomes, were assumed to have the same risk characteristics as those originated when prices were much lower. BCL (2012) note that statements of former senior managers at rating firms reveal that they were aware of shortcomings of their models and processes, but also that arguing for greater prudence not only was not rewarded in their firms but actually was punished.

22 And even though the risk weight was set at 100 percent for non-OECD countries, outside the OECD, governments regularly allowed their domestic banks to adopt a zero risk weight for lending to their home government.

23 He observes a change in professional opinion on structural unemployment, that fiscal austerity is expansionary, and more. See Krugman (2013).

24 It is not only possible to write a paper just on CoCos, many have. See Calomiris and Herring (2013) and the literature they cite.

25 And in soccer it was found that when the home team was behind (ahead) in a close game, there were more (fewer) penalty minutes, whereas there was no bias when the game was not close. The authors creatively show that times when the fans' influence on the players—the leading view of why the home crowd matters—is at its peak (e.g., when a basketball or soccer player is taking a foul shot or penalty kick)—there was no influence of the crowd—the percentage of foul shots/penalty kicks scored was identical for home and visiting teams.

26 Charles Calomiris pointed out to me that CoCos would reduce the tendency of bank managers to try to use differential risk weights to lower their capital, because of the threat of being replaced. This certainly might be the case for banks that are closer to having CoCos convert, but it would seem that banks far from conversion, and with existing shareholders who want dividends, might well respond. And risk weights still are endogenous. I would rather depend on CoCo holders to monitor the risk of the bank, as they will have every incentive to do so as long as they never expect to be bailed out.

COMMENTARY

Financial Regulation after the Crisis: How Did We Get Here, and How Do We Get Out?

Takeo Hoshi

This paper makes very controversial points. Jerry Caprio argues that the attempts to improve banking regulation in the last 40 years have been totally misguided and we need to "reboot" the process. The paper focuses in particular on the Basel capital ratio regulation and argues that the regulation has grown more complicated without making the financial system any safer. Worse, the Basel's regulation was at least partially responsible for the global financial crisis. The approach to apply the same risk weights or the same risk management model for all the banks in all the countries ended up increasing correlation of banks' exposures. These problems intensified in Basel III, which makes the financial regulation more complex and less transparent. So Caprio recommends rolling back the Basel regulation and replacing the risk-weighted capital regulation with simple leverage ratio regulation supplemented by conditional convertible debt, or CoCos.

Caprio also points out some regulatory failures during the global financial crisis. The British Financial Services Authority did not intervene in Northern Rock in time and on the contrary allowed them to increase dividends shortly before the failure. The regulators in the United States did not act on widespread fraud in mortgage markets in the early 2000s or on the clear signs of more risk-taking by banks (such as NINJA loans). But the regulators do not seem to be held accountable. So Caprio recommends creation of a sentinel, which collects and disseminates information on financial regulation and supervision, so that the market can discipline the regulator. The idea is more fully discussed in Barth, Caprio, and Levine (2012).

Unfortunately, the analysis and recommendations in this paper have not become a consensus view. Basel III implementation is still going forward. Indeed the U.S. regulatory authority has just announced the final implementing regulation for the liquidity coverage ratio, which is a new part in Basel III,

for large U.S. banks. I have not heard that any country plans to create a sentinel, either. So the points made in this paper remain controversial.

But I completely agree with the author. Indeed I would go further and argue that what he suggests in this paper—getting rid of risk weights, requiring banks to issue CoCos, and making regulators accountable—are just the beginning. We should consider a lot more.

I divide my comment into two parts. The first part points out some financial regulations other than the capital ratio regulation that should also be reconsidered. The second part points out some recent attempts to improve financial regulations other than CoCos that may actually be useful and can be salvaged. So I argue the recent efforts by many including the people in this room were not a total waste.

First, let me point out some financial regulations other than Basel capital ratio regulation that should be reconsidered.

The first one is actually a part of the Basel III regulation: the liquidity coverage ratio regulation. Under this regulation, banks would be required to hold a large enough amount of high quality liquid assets to survive 30 days during a stress. The liquidity regulation has problems similar to the capital ratio regulation. In calculating the denominator of capital ratio, each asset is assigned a "risk weight" and the risk-weighted assets are calculated as the risk-weighted amount of assets. Similarly, the numerator of the liquidity coverage ratio is calculated as the sum of various liquid assets weighted by the "haircuts," which can be considered the liquidity weights of assets. For example, Level 1 assets such as cash and U.S. Treasury bills are given zero haircuts or 100 percent liquidity weight and Level 2A assets such as mortgage-backed securities guaranteed by the government-sponsored enterprises are given 15 percent haircuts or 85 percent liquidity weights. Just as the Basel capital ratio regulation made banks hold assets with the highest expected returns among assets with the same risk weights, the new liquidity coverage ratio regulation will make banks hold assets with the highest expected returns among the assets with the same liquidity weights, and hold assets with the highest liquidity weights among assets with similar risk-return profiles.

The liquidity ratio regulation also increases the correlation of asset holdings across banks, similar to the capital ratio regulation; this will actually lead to a more serious problem because the liquidity of many assets depends on the existence of a well-functioning secondary market. If all banks rely on the same type of asset to secure liquidity during a stress situation, all banks will try to sell the same type of asset during a stress, leaving no one on the other side of

the market, which makes the asset illiquid. In this case, the liquidity disappears exactly when it is needed most. In this sense, the problem is very similar to that of counting deferred tax assets as a part of capital, which became obvious during the banking crisis in Japan. The deferred tax assets disappear exactly when we need bank capital as a buffer.

So, it is better to replace the liquidity coverage ratio regulation with something less distortionary and simpler. One such candidate is the requirement to just disclose a simple liquidity indicator without arbitrary assumptions on haircuts and run-off ratios, as Shadow Financial Regulatory Committees of Asia, Australia-New Zealand, Europe, Japan, Latin America, and the United States (2013) proposed.

To reboot the Basel approach as Caprio suggests, we would need to go back 40 years, but to roll back the liquidity ratio regulation, we only need to go back four years at most, so it is worth consideration.

My next suggestion would take us back more than 40 years. An important reason we need any regulation on bank leverage is because monitoring by debt holders, which would limit leverage in many other industries, does not work in banking. In many countries, deposits and often other bank liabilities are insured or otherwise protected by governments, so the debt holders are indifferent toward bank risk-taking. Since the equity holders welcome risk-taking by banks, especially when the amount of equity is small, banks end up overleveraged in the absence of regulations that would limit leverage.

Protection of depositors, especially small deposits, is usually justified on the grounds that uninformed depositors may run on a solvent bank and create a self-fulfilling bank failure. But, in today's world, where developments in information and communication technology have substantially reduced the cost of acquiring information, I think we should at least question the traditional assumption that depositors are uninformed. If it is not too much to ask depositors to be informed about the financial health of the banks they deal with, then we may be able to get rid of deposit insurance and protection of bank liabilities.

Even if it turns out that it is still expensive for all depositors to be reasonably well informed, there are other mechanisms such as narrow banking that limits the amount of deposits to be protected. At least, there seems to be no justification for protection of large bank creditors, which is still observed in many countries.

The third area for reconsideration is that financial regulation often tries to achieve multiple goals. Caprio points out that the Basel capital ratio regulation tries to pursue three objectives at the same time: "keeping the banking system

safe, leveling the playing field for banks, and being responsible for management at the individual bank level." Then, he recommends that capital regulation focus on the first objective: keeping the banking system safe.

More generally the governments in many countries use the financial regulation to try to achieve various, sometimes conflicting goals. In the United States, the financial regulation has been used to promote homeownership (through mortgage financing system with Fannie Mae and Freddie Mac), to address spatial and racial inequality (mainly through the Community Reinvestment Act), and to achieve foreign policy goals (through the Office of Foreign Assets Control), just to name a few. Sometimes pursuing these objectives compromises the safety of the financial system, as was shown by the recent financial crisis that was partially caused by a housing boom fueled by long-identified problems with Fannie Mae and Freddie Mac.

I suggest we should consider freeing financial regulation from these social, foreign, and other policies. This suggestion comes out of my own research on zombie firms in Japan (Caballero, Hoshi, and Kashyap 2008). There, the financial regulators often encouraged the banks to continue lending to zombie firms so that they could continue employing the workers.

Moving on to the second part of my comment, although I agree with Caprio that the refinement of the Basel regulation following the global financial crisis is mostly in the wrong direction, there are some regulatory developments that could be useful in making the financial system safer.

The first one is stress testing, which many countries have used since the global financial crisis to determine the amount of capital that each bank needs to be well capitalized during a stress. The inspection of banks in Japan before the second round of public capital injections in 1999 was also a stress test, and it was useful in stabilizing the financial market at least temporarily. Hoshi and Kashyap (2010) include a more detailed discussion on this.

Unlike the capital ratio regulation, which looks at the current level of capital and is static in this sense, a stress test is dynamic and asks how much capital would be lost during a stress. Since we have often observed well-capitalized banks quickly become undercapitalized in a stressful economic condition, a stress test can provide additional useful information. A more important function of stress tests is to force banks to imagine future stress scenarios, which would help them and the regulators prepare and respond better when a stressful situation actually arises.

From this point of view, a "reverse stress test" in which a bank is asked to come up with a stress scenario that would make the bank insolvent is especially useful. Such a scenario is not easy to come up with, but the effort to come up

with an unthinkable but possible scenario where the bank would fail is really useful to prepare for a crisis.

A related regulation forces each bank to file a resolution plan. This is also worth keeping. One of the major problems in the latest financial crisis in the United States was the inability to close down a large financial institution without bringing down the whole financial system with it. The United States had a very efficient system to close down failed small banks, but the framework to deal with large troubled bank holding companies or investment banks did not exist. Thus, when Lehman Brothers failed in 2008, financial markets all over the world stopped functioning.

We observed similar problems for Japan in the late 1990s. Japan did not have a framework to deal with large failing banks, either. When large international banks such as the Long-Term Credit Bank of Japan (LTCB) and the Nippon Credit Bank (NCB) got into trouble, the Japanese regulators initially tried hard to prevent the failure. The Japanese government explicitly mentioned that they should not let a failure of Japanese banks destabilize the global financial system. We should give credit to the Japanese regulators back then for at least understanding the danger of letting internationally connected large banks fail. Eventually, Japan created a mechanism to nationalize large failed financial institutions temporarily and used it to nationalize LTCB and NCB in late 1998, which started the end of the crisis situation.

The fact that the regulators in advanced countries (or "formerly advanced countries") are now serious about coming up with resolution mechanisms and at the same time asking each financial institution to prepare a resolution plan (also called a living will) is promising. Although there are many remaining issues, such as how the national regulators should coordinate in a resolution of large multinational financial institutions, the attention being paid to the resolution mechanism is warranted.

In summary, the efforts for regulatory reform since the global financial crisis have not been a total waste of time. There are some promising developments. But, I also share Jerry's concern that the increasingly complex regulatory framework represented by the expansion of the Basel framework has been in the wrong direction. It would be a good idea to roll back the Basel guidelines and reconsider the financial regulation's foundation.

Finally, let me end my comment by reiterating the importance of politics which Caprio points out and Calomiris and Haber (2014) discuss in more detail. Most of our discussion of financial regulation, including my comment, is just economics. But politics is what determines financial regulation in the end. Jerry's paper argues that creation of a sentinel that would collect and publicize

the information on financial regulation and supervision could be the first step toward empowering taxpayers, who end up paying the cost of financial crises, but I am not optimistic. After all, taxpayers and the general public are more dispersed than other stakeholders and face serious collective action problems.

Moreover, the problem of financial regulation entangled with social policy is even harder to solve, because a significant portion of the general public believes that they benefit from the social policy aspects of financial regulation. Some of them would actually claim that financial regulation should be strengthened to promote those social goods. More research into the politics of financial regulation is very important and urgent.

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COMMENTARY

Financial Regulation after the Crisis: How Did We Get Here, and How Do We Get Out?

Ashoka Mody

In this important paper, Jerry Caprio argues that the reforms of the Basel bank regulatory framework may, in fact, have deepened the vulnerabilities that brought on the global financial crisis. He goes further: Basel cannot be reformed, and attempts to do so distract from the task at hand while financial fragilities continue to rebuild. Caprio proposes a new regulatory structure, which in its philosophy and packaging offers a fundamentally new way forward. This is a paper by an expert, but written with a passion that emphasizes the urgency of change—a change in world view as much as in regulatory design.

Stepping Out of the Cognitive Bubble

Caprio starts by asking a simple question: What should public policy expect of the banking sector? He laments that the focus continues to be on the greater availability of credit to businesses and households. This preoccupation is manifest today in the constant drumbeat of "credit crunch" and the many efforts to increase credit supply. In practice, distinguishing between a decline in credit demand and supply is hard; many analysts are concerned that pushing supply could be counterproductive.

In addition, the longer-term vision of an ever-expanding financial sector remains the dominant model in much policy thinking. For example, Mark Carney (2013), the Governor of the Bank of England, visualizes a world in which global banking "increases in line with historical norms." In that world, with the United Kingdom maintaining its global share, he says, "U.K. bank assets would exceed nine times GDP," a ratio matched recently by Cyprus and Iceland. Governor Carney sees the task of financial regulators as that of making banks more resilient and crisis-proof rather than questioning the model of financial growth on steroids.

Author's note: *I am grateful to Michael Bordo for our ongoing discussions on these themes and particularly for his permission to let me draw on his ideas on the politics of learning.*

The global financial crisis was the product of a cognitive bubble. It visualized an ever-growing role for the financial sector. This cognitive bubble supported a financial bubble. Together, they reinforced each other. While some voices questioned the increasing share of finance in GDP, even these were mainly concerned by rapid bursts of financial growth, the "credit booms." The notion that when finance grows disproportionately large relative to the real economy, it may become impossible to tame was never seriously examined.

Caprio questions this single-minded pursuit of banking and financial growth. Regulatory reform, he says, must start from the goal of slower-growing but better-allocated credit. In his words,

> *Any solution that is effective will reduce the availability of credit from what it was in the extreme years during the run-up to the crisis, but despite the unwillingness of politicians to make that point, better-allocated credit would be a boon to societies. . . . the credit bubble in the 2000s featured unproductive investments in housing and a variety of consumer goods that left societies with high unemployment, a debt overhang, and little else, save some empty houses, the regrets of the borrowers, and the enlarged wealth of many in the financial sector. Nonetheless, bankers are protesting that the response in the pipeline will produce financial disintermediation, denying credit to many and reducing growth.*

The risk is clear. With signs of economic recovery, policymakers and bankers are recommitting to a model that almost brought the world economy to its knees.

Caprio's fire is directed at the Basel framework, which, in the new guise of Basel III, he regards as a continuation of a discredited bank regulatory system. Within Basel, the culprit remains the system of risk-weighting of assets. Caprio argues that when assets are weighted by their perceived risks, regulation is compromised on two counts. First, bankers and regulators are drawn into a game of allocating assets to risk categories, a game that bankers typically win. Second, more seriously, the procedure creates systemic risk. Banks invest disproportionately in asset classes designated to be low risk. But assets do not stay as low risk. They may have been misclassified in the first place, if their credit ratings are too optimistic. Or the economic and financial conditions may change: In that case, because several banks have invested in these assets, they suddenly become exposed to correlated risks.

Instead of stepping back from the mindless complexity and hazards of risk weights, the Basel process has determined that the complexity was insufficient.

The reaction, therefore, has been to double the bets. Basel evidently could not prevent the crisis; it must, therefore, be reinforced with greater complexity and intrusiveness.

Caprio's call is for stepping out of the cognitive bubble and starting afresh.

The "Bali" Framework

To mark a radical departure, Caprio proposes that a new group constitute itself as the "Bali Committee," rather than the Basel Committee. The framework he proposes for their consideration attempts to balance the need for simplicity with the hydra-headed nature of finance. Thus, while he favors simpler rules, he is sensitive to the concern that bankers will quickly find ways to subvert the rules. Identifying the sweet spot where the rules are simple but not simplistic is the perennial challenge.

Caprio's proposal has four elements: more equity capital relative to unweighted assets; contingent convertible debt; some hard-wired ratios; and a sentinel, who provides informed commentary to limit the risk of new cognitive bubbles.

The case for a higher equity-to-unweighted assets ratio has been generally accepted. It is a case most forcefully made by Anat Admati and Martin Hellwig (2013). They ask for ratios much higher than currently visualized, perhaps, as high as 25 percent. They are not impressed by the claim that Basel has made progress in this direction; their assessment is that Basel would allow as much as 97 percent of assets to be financed by borrowing. Caprio is clearly sympathetic to this way of thinking, although he does not propose specific equity benchmarks.

The second element of Caprio's proposal is greater use of convertible debt: debt that would automatically convert into equity when the equity ratio risked falling below the desired level. These so-called CoCos have been more controversial. Some, such as Admati and Hellwig, are concerned that convertible debt instruments will remain prone to the destabilizing character of debt—the event of a conversion could create panic in financial markets. But this is really a matter of design, which needs to ensure that the conversion is smooth, incremental, and automatic.

Caprio refers his reader to a paper by Calomiris and Herring (2013) as having proposed a credible design for CoCos. Indeed, that paper offers an elegant trigger for conversion. It proposes that the conversion occur when the 90-day moving average of the ratio of the bank's market equity value to the sum of its market equity value and face value of debt falls below 4 percent. The moving average disregards temporary market moves, and the buffer of 4 percent

implies that the conversion does not wait until the problems have become stark and unmanageable.

With proper design, CoCos offer the prospect of not just adding equity at critical moments. They potentially improve banks' incentives for prudent action. Calomiris and Herring (2013), as well as Kashyap, Rajan, and Stein (2008), are concerned that excessive equity will allow the banks' management to take imprudent risks. In contrast, bondholders are more vigilant. Thus, CoCos can provide the monitoring associated with bondholders but, by creating a smooth conversion to equity, they can minimize the inevitable drama associated with discontinuities of debt restructuring and default.

In this regard, drawing on Goodhart (2010), Caprio makes an important observation about the philosophy of regulation. Regulatory design should avoid on-off solutions inherent in benchmarks and thresholds: The regulated banks have an incentive to work around such boundaries. For this reason, Caprio is disinclined to set limits on executive compensation. Rather, he suggests that the compensation formulas be made public so that the stakeholders are aware of the incentives driving bank management. Nevertheless, Caprio seems torn on this theme. He concedes that there may be a need for a third element in regulatory design—in addition to more equity and CoCos—banks will probably need some hard limits, such as loan-to-value ratios on home lending.

The fourth and final element of Caprio's proposal is the creation of a sentinel. With his long-time coauthors, James Barth and Ross Levine, Caprio is in search of the best way of making regulators more accountable. Even when regulators are not corrupted, they can be sucked into outdated assessment criteria and procedures. A sentinel could provide commentary on a regulator's decisions and thereby force the regulator to be more publicly accountable. Pointing to the failures of Irish regulation and supervision in the years before the crisis, Caprio wonders if the presence of a sentinel may have prevented the regulatory complicity in fostering egregious lending behavior.

The concept of a sentinel is attractive and deserves serious consideration. But it should be adopted with the knowledge that the sentinel may itself be sucked into the cognitive bubble. In the Irish case, the closest process similar to that of a sentinel was the International Monetary Fund's Financial Sector Assessment Program (FSAP), administered in Ireland in 2006. The FSAP's verdict was that the Irish banking sector was in good health, a judgment that has added to the list of failures of international economic and financial surveillance. The staff of the FSAP team is comprised of international experts and clearly has no skin in the game. Yet, prevailing norms do influence even the experts.

In concluding his paper, Caprio recognizes that regulatory processes have much inertia and, despite the evident need for change, the response is slow—and even injurious. He recognizes that politics often trumps good sense. But he does not delve into the political dynamics of Basel. This intractable issue deserves more attention, not least for the success of Caprio's sentinel, who risks being captured by the same political forces that have stymied regulatory progress. The rest of my comment is devoted to sketching a taxonomy of the politics of institutional learning.

The Politics of Institutional Learning

It is conventional wisdom that a crisis triggers reforms. The vested interests lose their grip and those that were disadvantaged by the earlier system gain new voice to promote change. These plausible dynamics, unfortunately, do not always materialize. Jared Diamond (2011) documents how societies often choose to fail. On a less sweeping scale, in Abiad and Mody (2005), we find that while balance-of-payments crises do generally create a constituency for reform, the more complex banking crises evoke a weaker response.

Successful reform requires, as Diamond (2011) highlights, a shift in group decisionmaking, which in turn appears to require many ingredients. In the midst of the Great Depression, President Franklin Roosevelt was able to push through the New Deal, which fundamentally changed the social contract in the United States. A crisis was met by leadership but was also made possible by a favorable political configuration: Roosevelt had the two houses of Congress behind him. Together, they were able to exploit the critical juncture.

The willingness to learn must also be present. Diamond (p. 439) refers to the contrast in handling the two crises between Cuba and the United States. The Bay of Pigs invasion in 1961 is widely regarded as a disastrous decision. The groupthink that led to the decision was marked by a "premature sense of ostensible unanimity" and President John Kennedy's discouragement of disagreement. That disaster did induce learning. The Cuban missile crisis about 18 months later evoked the opposite response. On this occasion, President Kennedy encouraged dissent and contrary views among his advisors.

Learning in politically autonomous institutions does occur provided a technocracy is in place. But the risk is that such learning can be backward-looking, solving the previous crisis even as new challenges unfold. Because the response focuses on the parameters of the previous crisis, such learning may be characterized as "least-squares" learning. Rotemberg (2013) describes the evolution of the Federal Reserve in those terms. Some may argue that the IMF is also capable of such technocratic, least-squares learning; others remain concerned

that the IMF's ability to learn is constrained by political influence (Thacker 1999 and Barro and Lee 2005).

Complexity, groupthink, and politics act most adversely in coordinating a large number of actors, each with some veto power. The Basel process comes closest to meeting these criteria. The past compromises to achieve the Basel consensus have made it a clumsy system in which different parts resulting from delicately negotiated agreements don't fit together well. With veto authority held by many, forward movement becomes difficult. This leads to a form of "Groundhog Day" learning, with glacial progress. As Caprio remarks, "following one of the most wrenching financial crises in history, the approach to financial regulation is essentially more of the same." In his discussion of the Caprio paper, Takeo Hoshi points out that the system of risk-weights is also being adopted for the new liquidity regulations. The conundrums in assigning such weights have not been a deterrent.

It is, therefore, remarkable that despite these constraints, Switzerland has made progress in its regulatory approach. While still working off Basel, the Swiss authorities have moved more rapidly with demands for larger equity buffers and CoCos. The equivalent of a sentinel does not quite exist, but the Swiss National Bank adds its macroprudential voice to the deliberations of the financial regulator, the Financial Market Supervisory Authority. The risks to Switzerland arising from the fragility of its big banks have focused all the minds. The private sector can normally be expected to push back on reforms—and there is much evidence worldwide of efforts to roll back even the reforms that have been put into place—but the Swiss lesson is that private actors do eventually learn to live with new structures. The policy task is to demonstrate the feasibility of a new approach and change the incentives to make that approach operational.

Conclusion

With his alternative framework, Caprio has called on regulators to change course or risk facing another humbling and costly crisis. It is a thoughtful alternative that challenges the core philosophy of the current regulatory system. Under his proposed system, the financial safeguards required by banks would be large enough to rein them in, and the balance would shift from rules to higher quality information. In setting out his regulatory vision, Caprio is aware that the politics will push back. But perhaps there is a way forward. Rather than attempting adoption of a new framework in one fell swoop (even if the Bali location helps a meeting of the minds), a few more examples of pragmatic advance, such as the Swiss initiative, will act to diminish the inertia of groupthink.

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GENERAL DISCUSSION

Financial Regulation after the Crisis: How Did We Get Here, and How Do We Get Out?

Chair: Mark Spiegel

Mr. Spiegel: Jerry do you want to take a minute or two to respond to the discussant comments?

Mr. Caprio, Jr.: Just a couple of points. I'd like to thank the discussants very much for their comments. Since the idea of a sentinel over the financial system attracted so much attention, let me just clarify that neither I nor my coauthors think this is going to be easy. Obviously regulators are human, and all humans may talk about accountability but they don't like it for themselves. Banks also don't want regulators who are accountable, because then they'll be a whole lot tougher. So we understand the political economy is against this. Moreover, to set up a truly effective sentinel, as Ashoka was suggesting, it really has to be independent, which the International Monetary Fund (IMF) is not. The IMF wasn't even able to set up a Financial Sector Assessment Program (FSAP) for the United States before the crisis, because the U.S. didn't want one, and even though they should have had one for the U.S., it wasn't done until it was too late. And the people who are overseeing the Fund, who are going to be assessed, are on the IMF's board. So it would be like having an FSAP with Citibank and Morgan Stanley on the oversight board. It would not be very effective. There are a host of conditions that need to be satisfied for it to be effective. You've got to get a diverse group of really highly trained financial experts, a mix including forensic accountants and lawyers. You've got to pay a lot more than anybody in the United States wants to pay public officials. I don't believe even the IMF has that mix of talent. On Takeo's point, there are two countries, Sweden and Ireland, that have formed sort of a sentinel. They call them fiscal councils, and their job is to look for any possible off-balance-sheet liability that could blow the government's budget out of the water. Actually, both countries are obviously doing it because of their experiences with a banking crisis. Now the reason I say it's "sort of" a sentinel is because they're not staffed by a bunch of financial experts, and they really need that expertise to be able to do an effective job of monitoring.

Mr. Spiegel: Okay, we have time for some questions. First, Ric Mishkin.

Mr. Mishkin: I really want to commend you on a terrific paper. I wanted to dig into a few issues you've raised and amplify them. One is the issue of transparency about compensation, which I think is key. One of the things that's most frustrating to me as a policymaker is the focus on irrelevancies. In particular, Glass-Steagall is brought up all the time as a source of the crisis. When you actually look at what happened and where the epicenter was in the crisis, particularly in the U.S. context, it was in two investment banks that were completely unaffected by Glass-Steagall, Bear Stearns and Lehman Brothers. Beyond the disastrous regulation by the Securities and Exchange Commission, which was a business practices regulator and not a safety and soundness regulator, one of the key reasons this happened was that the nature of the compensation scheme changed when these two banks changed from a partnership framework to a corporate structure. To counter, people may say there still were incentives not to tear down this risk because top management lost a lot of money. But the level below that, all the people who got huge bonuses, really created the crisis. What's interesting is that we all thought the hedge funds were going to be the big problem before the crisis, because there are tremendous incentives for them to take on risk. But their transparency offset it. Nobody invests in a hedge fund unless the owners have put almost all their own wealth into it. That's where the transparency issue comes in. I think one of the things you talked about, this transparency about compensation, is a critical element in terms of preventing the next crisis. The other thing I agree with you on is that Basel has headed the wrong direction, and its complexity is a huge part of the problem. One further thing that is very important is that part of this crisis was from reduced transparency. Complicated securities in a sense decreased information in financial markets. And Basel actually decreased information in terms of what the regulators were doing. So this principle of "keep it simple, stupid" is really important, because if you keep it simple then you can actually monitor what the regulators are doing in a much better way. But I think it's going to be hard to get people to move away from the old way of doing things. I actually am not optimistic that we can redo Basel and hit the reset button.

Ashoka, you mentioned the issue of the Swiss National Bank, which has been extremely innovative. But one of the things it shows is that people will work very hard to prevent some of these reforms, in this case the political parties. I think the key issue here is, how do we get the kind of reforms that Jerry is talking about? I'm not sure what the answer is. But this thinking outside of the box is just terrific. And I hope it'll have some impact, but I'm not that optimistic.

Mr. Spiegel: I have a list of people with questions, let's take them all first, and then I'll let the presenters respond. Martin Wolf next.

Mr. Wolf: I'm going to make a very quick series of comments. First of all, I love this discussion, and it relates to a lot that I've been thinking about. I was on the independent commission on banking in the United Kingdom, so I've been through a lot of these things, and I agree with pretty much everything.

So here are the questions. Leverage ratio, I agree, but how much equity? Let's be precise, what numbers are we talking about? It makes a lot of difference. Three percent, 30 percent? Second, how worried are you about the regulatory arbitrage consequences of high equity requirements for formally regulated institutions. That seems like a really big issue to me. The innovative capacity of the system is incredible. Third, it seems to me you cannot underestimate the significance of fundamentally mistaken views about the world. I could have made millions of dollars on bets with very distinguished American economists—I will not list the names—who told me that house prices could not fall in the United States, as a general proposition. Which as a Brit I found rather astonishing, having been through three massive collapses. Fourth, do not underestimate the significance of the belief in a number of countries, including my own, that banking was a profit center for the economy, and regulators' job was to support it. Fifth, I have a lot of sympathy, but I think the idea that we can just have somebody with a $10,000 deposit in JP Morgan police it is fairly nuts. So surely the alternative is to change seniority, which is what we recommended. Just make insured depositors the senior creditors. The creditors who should monitor the bank are the ones who have large claims. And the final point, whose interest does it all serve? The banks' and regulators' and nobody else's.

Mr. Spiegel: Okay, I have Anil Kashyap next.

Mr. Kashyap: Great panel. So I don't know the Calomiris and Herring details, but what was interesting to me is that you didn't really go after pay. So my favorite proposal on contingent capital (CoCo) requirements is to invest the bonus pool in CoCos, and I wonder why you would prefer any form of CoCo to that?

Mr. Spiegel: Okay, Barry Eichengreen, and then Peter Hooper will have the last question.

Mr. Eichengreen: It's easy to be a purist if you're not currently a policymaker. I want to suggest that Jerry is not being pure enough here. You started with a simple leverage ratio for a lot of capital, and then you said that it's not politically possible to get enough capital, so we're going to be a little less pure and a

little more complex, and add CoCos. But it's probably not politically possible to get enough CoCos, so we're going to be a little bit less pure, and a little bit more complex, and add loan-to-value ratios. Isn't this a slippery slope? And isn't it important to identify what the political constraint on having enough capital is, and attack it directly?

Mr. Hooper: Let me add to the slippery slope question. Jerry, what is your capital ratio recommendation? Certainly going from risk-weighted assets to leverage reduces complexity, but at the same time doesn't it incentivize intermediaries to move towards higher risk, higher return assets? Especially if we're considering going from 3 percent to Martin's 30 percent on capital.

Mr. Spiegel: Thank you very much. I'm going to give Jerry a chance to respond to all the questions, and then we'll have a short opportunity for the discussants if they want to respond as well.

Mr. Caprio: On the last point, since that hit several of the questions, yes, there is literature that argues that it's theoretically ambiguous which way a higher capital requirement is going to go. That's one of the reasons why I would not put all the eggs in one basket. It's not just because of political issues. There have been empirical studies that can't identify any impact from varying capital in a relatively narrow range. Now I believe most sensible proposals would call for a much higher range—I'm drawn to the number 20—but we don't have a scientific basis for determining this range and we are worried about affecting people's behavior. It also depends critically on how concentrated vs. how diversified the ownership is. Luc Laeven and Ross Levine have a good paper on that. So I think it is a slippery slope to decide what rules you use, and I hope my paper would help in the discussion of what number of sensible simple rules is manageable. I was not trying to write the definitive paper or draw up the blueprint.

Anil, I like the proposal of having bonuses invested in CoCos. We'll have to talk more about the differences between your proposal and that of Charlie (Calomiris) and Dick (Herring). On Martin's questions about regulatory arbitrage, I am really worried about that. Charles Goodhart has written far more eloquently than I have on this issue, and after Hyun's talk last night, I'm even more worried about it. That's why I find Charles's recommendation about not having on-off buttons as one way to go. But I realize that if the sum total of your interventions pushes people out of the sector to do the same business elsewhere, then you have to follow them in effect, just like we should be regulating money market funds in this country as banks. Obviously, that's really hard to do politically. Martin, I agreed with your points completely.

I would end with a comment that, a year ago in the Wimbledon final, there was a blatantly bad call in favor of Andy Murray, and even before they showed the instant replay the referee immediately reversed the call. And as a commentator, John McEnroe said, with instant replay you might as well call it right the first time. That's the incentive system we want. I have no illusion that having a sentinel will always lead to better regulatory decisions, but I think it could make a difference.

Mr. Spiegel: Just briefly Takeo.

Mr. Hoshi: Very briefly on a couple of points. On simple leverage ratio regulation: No matter how high we set the capital ratio or simple leverage ratio, I think the banks can find a way to get around it and achieve whatever level of risk they want. So I think the necessity for bank supervision won't go away. The second point related to Martin's point, I agree with you. The problem is not so much deposit insurance for small depositors, but rather the protection of the large debt of the bank; it would be a good idea to get rid of the protection of those debts.

Mr. Spiegel: Okay, please join me in thanking our speakers for a very excellent session.

POLICYMAKER PANEL

Current Policy Challenges Faced by Emerging Market Economies and Korea

Woon Gyu Choi

1. Introduction

I would like to begin with a discussion of the policy responses of emerging market economies (EMEs) to monetary policies of advanced economies. I will suggest the likely response of EMEs to tapering of quantitative easing (QE). One of the recent interesting developments in this regard is that they did not respond uniformly to Chairman Bernanke's announcement of possible QE tapering in May and June 2013, which sheds light on the present heterogeneity within the EME group. EME policymakers should consider various factors ranging from economic fundamentals to the long-term challenges that their economies are facing. As a specific example, I will describe current challenges faced by the Korean economy.

2. Monetary Policy Normalization and EME Policy Options

Prospects for Monetary Easing and Portfolio Rebalancing

First I will discuss advanced economies' monetary policy normalizations and policy options for EMEs. Central banks' active provision of liquidity, dubbed unconventional monetary policy, is now widely accepted as a weapon belonging to the central bank arsenal. With the U.S. economy recovering, policymakers are steering the economy toward a new normal—a process accompanied by normalizing the central bank balance sheet and thus reducing liquidity supply. QE tapering in the United States is expected to start in the near future, but the European Union and Japan have not yet witnessed any clear signs of recovery or inflation to presage a change in course. The U.S. Federal Reserve's reduction in liquidity provision will initiate global portfolio rebalancing, forcing EMEs to deleverage or unwind the liquidity that flowed into them during the time of QE.

The driving force behind global portfolio rebalancing would be the U.S. recovery and the concomitant normalization of U.S. interest rates. As expectations of the Fed's QE tapering build, investors in advanced countries become concerned about a projected depreciation of EME currencies. If the Fed decides

to initiate tapering, the markets will turn bearish in the short run, but the decision may also reassure the markets that the economy is on the right track for recovery. Recovery in advanced countries is good news for exporters in EMEs.

Global Liquidity and EME Responses

Along with portfolio rebalancing on a global scale, changes in policy-driven liquidity from advanced economies have a direct impact on EMEs. My understanding on this matter is largely based on the recent study I conducted with my colleagues at the Bank of Korea.[1]

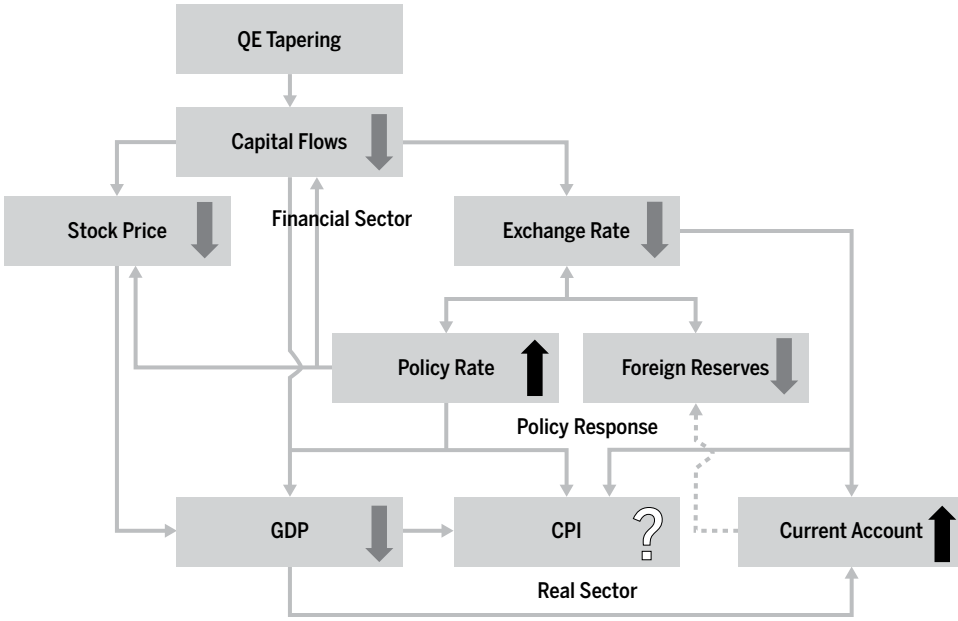
This study derives three global liquidity catalysts from financial data of advanced economies and then analyzes their impacts on EMEs using a panel vector autoregression. The three global liquidity catalysts are exogenous liquidity, endogenous market liquidity, and risk aversion (negative risk appetite). The exogenous liquidity momentum is identified as a policy-driven factor: It increases with the monetary base and decreases upon a policy rate hike.

In the context of this study, QE tapering is regarded as a negative exogenous global liquidity shock to EMEs, having impacts on their financial sectors, which induces policy reactions. The shock and reactions together determine QE's overall impacts on growth, inflation, and the current account in each country.

As summarized in Figure 1, a negative exogenous liquidity shock brings about capital outflows from EMEs, causing the exchange value of the national currency and stock prices to tumble. In response to nominal effective exchange rate depreciation, the authorities increase the policy rate and release foreign reserves to support the currency in a bid to fend off a crisis. Output then suffers from a lack of funds because of the outflow of foreign funds and the scarcity of domestic funds owing to monetary policy tightening. If foreign funds had been directed mainly toward the demand side of the economy, the shock that unwinds foreign funds will exert deflationary pressure. This deflationary pressure is offset by an inflationary pass-through effect from currency depreciation, leaving the ultimate impact on the price level unclear. A silver lining to this economy characterized by sluggish demand and depreciation is a current account surplus, which may moderate concerns over the crisis to some degree.

Tighter policy and release of foreign reserves to avoid leakages of foreign funds are rationalized in terms of the aim of retaining the foreign funds domestically and limiting exchange rate volatility. However, a policy rate hike is controversial since it may further worsen already sluggish growth, leading eventually to enlarged outflows.

FIGURE 1
Effects of QE Tapering and EME Policy Responses



QE Tapering and EME Responses

What I have just described is the average response of EMEs as drawn from data analysis. The recent reaction of global investors to the Fed's announcements regarding the possible QE tapering sheds light on the pattern of differentiation within the EME group (see Figure 2). Between May and October 2013, most Asian EMEs saw their currencies depreciate, and some EMEs suffered a loss of more than 5 percent in their stock market capitalization. The two main exceptions were China and Korea, whose currencies strengthened and whose stock markets turned bullish. Countries with external vulnerabilities—such as the so-called fragile five (India, Indonesia, Brazil, South Africa, and Turkey)—faced sudden capital outflows.

Capital outflows in turn are attributable to persistent deficits on the current account (see Figure 3). The fact that the fragile five also run persistent and large budget deficits suggests that their current account deficits may be engendered by weak fundamentals associated with fiscal deficits and that mounting concerns over external and fiscal sustainability may call for capital outflows.

FIGURE 2

EME Market Responses to Bernanke's Remarks

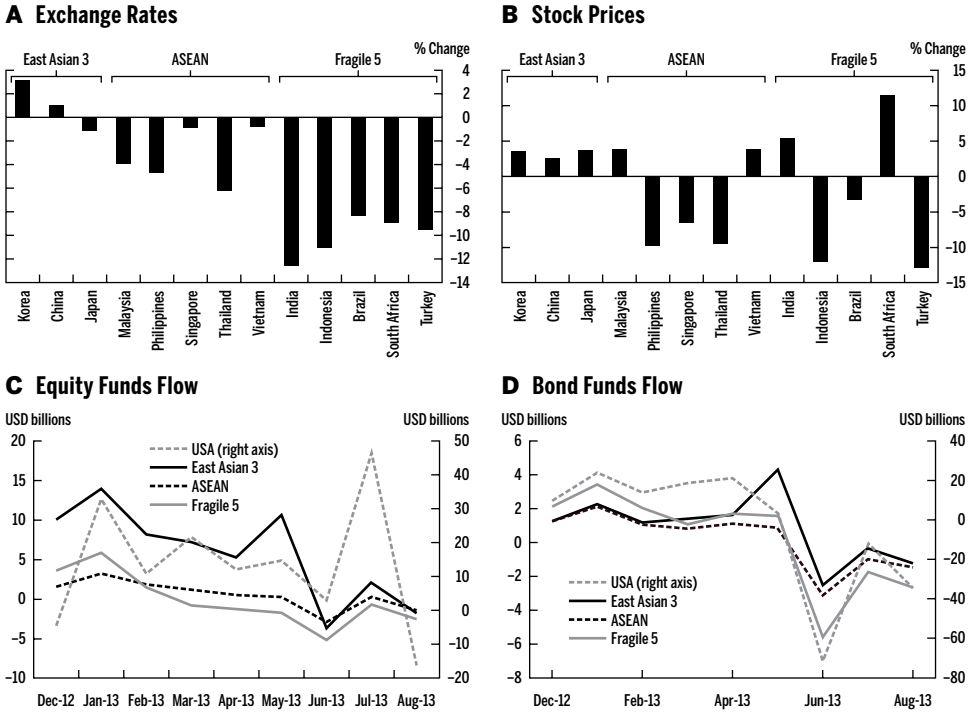
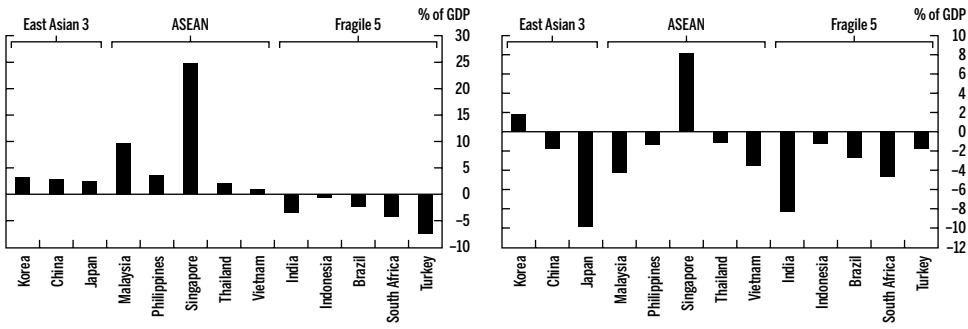


FIGURE 3

EME Fundamentals



Note: Percent of GDP is calculated as three-year average.

EME Policy Choices

In retrospect, the signaling of possible QE tapering served as a test run. The differing outcomes within the EME group left individual countries with two policy options: a defensive policy to fend off a crisis, or a domestic-oriented policy to neutralize or cushion the impact of fund outflows.

A defensive policy would be the conventional choice, as in the panel VAR model I mentioned. Such a policy incorporates a hike in the policy rate and the release of foreign reserves. However, the higher interest rate entails the weakening of the domestic economy, rendering its equity markets less attractive to foreign investors, thereby accelerating fund outflows and currency depreciation. Releasing foreign reserves may also backfire if the level of remaining foreign reserves is perceived as inadequate or if the pace of reserve drawdowns is too fast.

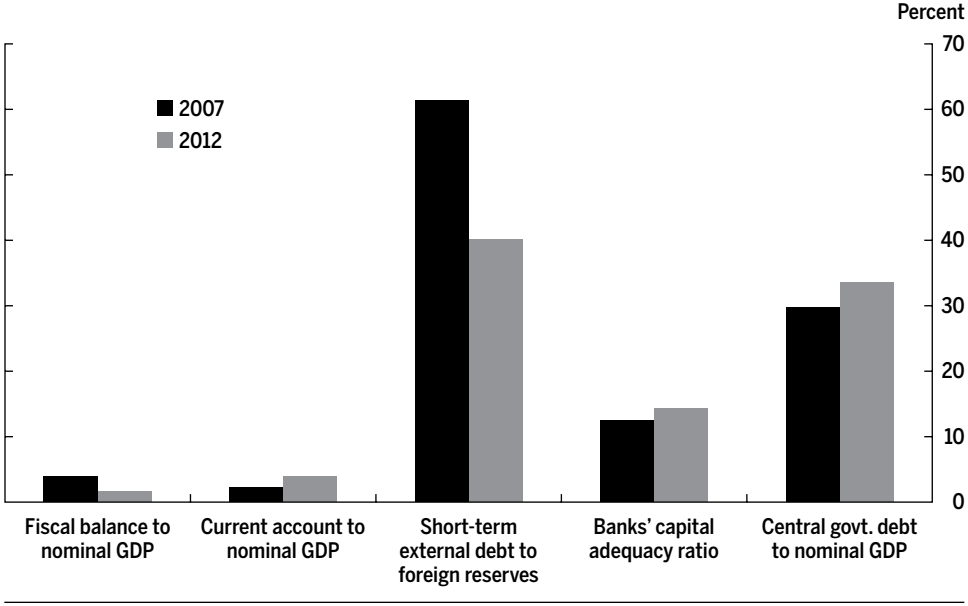
The alternative choice would be a domestic-oriented policy. Countries with solid fundamentals can determine the policy rate to achieve a policy objective in terms of inflation or employment. They may opt to maintain the policy rate while other EMEs increase theirs. If there is little concern about a financial crisis, policymakers may craft their policy for domestic goals even under an external shock that temporarily destabilizes the foreign exchange market. They could also deploy foreign reserves to smooth out excessive volatility in the foreign exchange market. The downside of this policy is that the relatively low interest rates speed up the draining of foreign funds from the bond market, further weakening the domestic currency and raising concerns about financial stability. Where the domestic financial system is still far from being mature, the additional liquidity resulting from the low policy rate may not penetrate those sectors in need of liquidity but be hoarded by financial institutions.

The choice between the two policy options will depend largely on the macroeconomic fundamentals of the particular economy and the nature of the driving shock—a push or pull factor. Having said that, those countries with weak fundamentals—such as twin deficits and high inflation pressure—do not have sufficient room for policy maneuvering. If global factors dominate the nature of the external shock, the efficacy of monetary policy may be limited.

3. Korea's Challenges in Policy Implementation

Now I turn to the case of Korea. Korea has experienced currency appreciation and stock price increases since May 2013. To my mind, these strong developments are attributable to the country's improved fundamentals since the 2008 financial crisis (see Figure 4). In particular, Korea's policy efforts have

FIGURE 4
Indicators of Macro-Financial Soundness in Korea



brought about banks' improvement in capital adequacy and a sharp drop in the ratio of short-term external debt to foreign reserves. The current account has improved—owing to the strong performance of globally competitive Korean firms and weak domestic demand. In the meantime, the budget surplus has shrunk upon the implementation of stimulative fiscal programs.

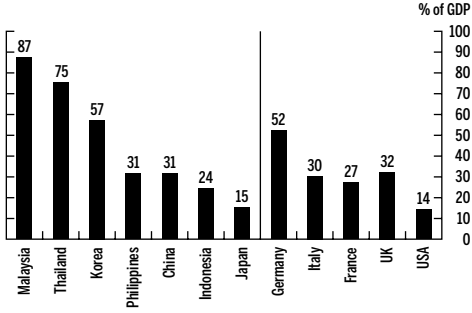
Although short-term fundamentals do not pose an immediate concern, Korea has its own share of challenges. The first challenge is imbalance between domestic demand and export-driven demand. Exports account for more than 50 percent of Korea's GDP (Figure 5A). This imbalance may be partly attributable to the slow pace of the development and integration of regional financial markets, especially the bond markets (Figures 5B and 6).

The second challenge is the combination of disparity in sectoral savings and subdued corporate investment. While the total saving rate has been slowly decreasing, the increase in the corporate saving rate has largely compensated for the decrease in the household saving rate since 2000 (Figure 7). This implies that retained earnings are neither being reinvested nor paid out as dividends to boost household income. While the saving-investment gap in Asia has narrowed

FIGURE 5

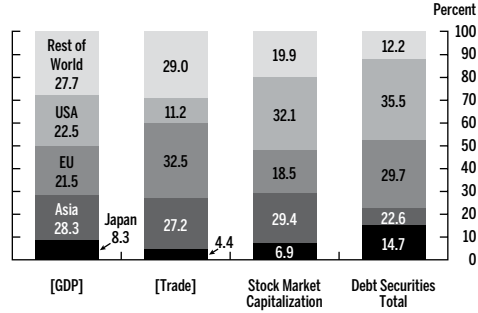
Imbalance between Domestic Demand and Export-Driven Demand

A Export of Goods and Services (2012)



Source: World Bank, WDI, 2013.

B Asia: Share in the World Economy (2012)

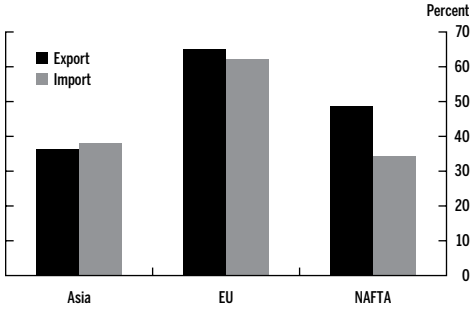


Source: IMF, GFSR, 2013, World Bank, WDI, 2013.

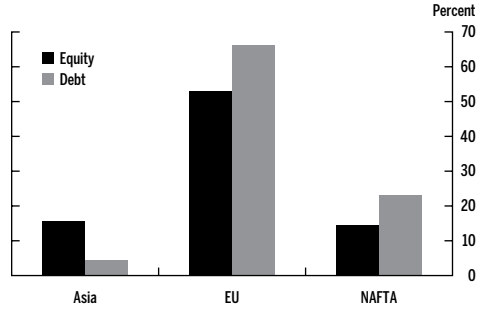
FIGURE 6

Intraregional Trade and Stock/Bond Investment (2012)

A Intraregional Trade



B Stock/Bond Investment

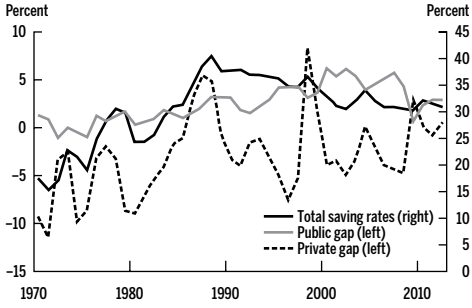


Source: IMF, DOTS and CPIS database.

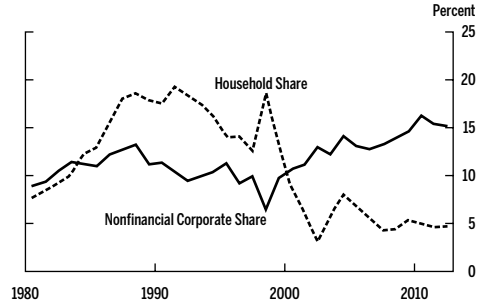
FIGURE 7

Saving Rates in Korea

A Saving-Investment Gap



B Private Saving Rate

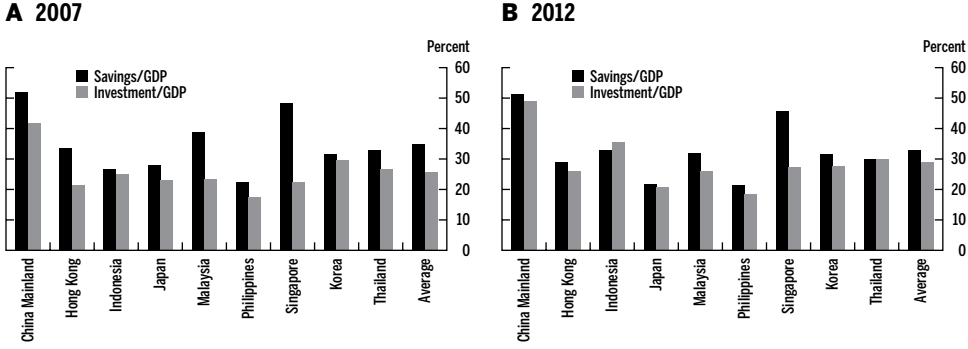


Source: Bank of Korea, ECOS.

since the global financial crisis (Figure 8), reduced savings are coupled with lower investment. In Korea, the recent sluggishness of investment—substantially attributable to heightened policy uncertainty—could weigh on the economy in the long run by constraining its growth potential.

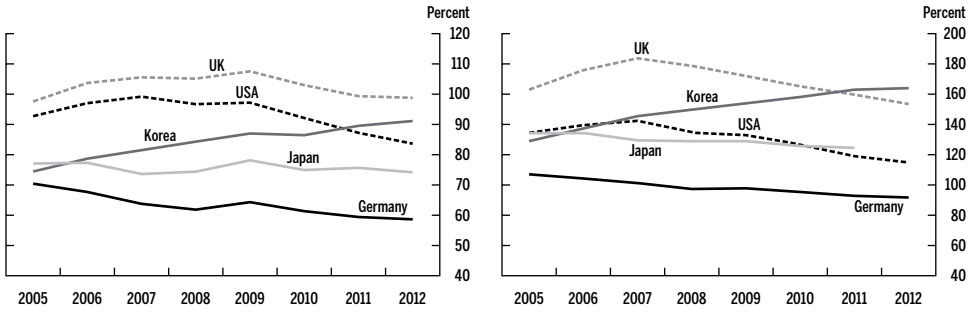
The third challenge is household debt. The ratio of household debt to disposable income in Korea now stands at 160 percent, having steadily increased even after the global financial crisis, in contrast to the situation in the major economies (Figure 9). At this point, household debt does not seem to drag down demand, and the associated risks are under control. Household debt will be manageable unless very large shocks strike. However, prudent caution should be exercised, especially for vulnerable groups (say, multiple-loan borrowers,

FIGURE 8
Savings and Investment in Asia



Source: IMF, WEO.

FIGURE 9
Household Debt



Source: BOK staff calculation.

low-income or old-age groups, and self-employed households) to ward off potential spillovers.

4. Closing Remarks

To sum up, the prospective QE tapering will call for global portfolio rebalancing whose impacts on individual countries in the EME group will be diverse. Individual countries may opt for a defensive policy to ward off a crisis, even at the cost of domestic goals. Alternatively, they may choose a domestic-oriented policy which has its own set of benefits and risks. The challenge is then where to place a fulcrum between global and internal factors in forming the policy positions. While Korea currently benefits from strong and seemingly resilient fundamentals, it faces eventual structural challenges such as the shortfall of domestic demand, weak linkages between savings and investment, and household debt overhang—not to mention the presage of demographic changes.

I close my remarks with some suggestions for policy coordination. Governor Powell suggested the gradual restoration of advanced economies' monetary policy, taking into account international linkages. In this regard, I would like to note as follows. Advanced economies and EMEs are more than ever intertwined, and global policy coordination is critical for the sustainable growth of the global economy. Advanced economies are asked to provide transparent and consistent policy signals to reduce policy uncertainty. EMEs, for their part, have to improve the macroprudential soundness of their financial systems and implement structural reforms to strengthen their fundamentals. Furthermore, once global investors suspect a crisis in one or two vulnerable countries within a peer group, this could provoke panic reactions across comparable EMEs. Against this backdrop, efforts to strengthen the global/regional financial safety net should be a matter of high priority. Korea has recently agreed on bilateral currency swap lines with Indonesia, the United Arab Emirates, and Malaysia. Our moves are likely to help the entire cohort of EMEs better withstand negative external shocks.

Finally, in the event of a global liquidity crunch, central banks would need to carry out appropriate policies of credit easing to ensure the seamless supply of funds to those vulnerable sectors hit by an abrupt credit crunch. EMEs are prone to financial market failures owing to information asymmetries and financial infrastructure shortages. The Korean economy is faced with sectoral liquidity shortage amid ample aggregate liquidity. Funneling aggregate liquidity by the central bank into market liquidity and loans for investment could be called a "modern reincarnation" of credit policy. In light of this, a contemporary

reincarnation of credit policy could help restore growth potential and rebalance liquidity flows.

NOTE

1 The results of the research project, entitled "Global Liquidity Momenta and EMEs' Policy Responses," were presented at the 2013 Bank of Korea annual international conference.

Financial and Fiscal Reforms in Support of China's Rebalancing

David Dollar

China has continued to grow well in recent years, but there are reasons to be concerned about the sustainability of its growth model. Before the global crisis China's growth relied to a considerable extent on net exports, as well as on a high investment rate. When the crisis hit, China responded forcefully with fiscal and monetary stimulus, but the stimulus was aimed largely at investment. So, the already high investment rate rose further, to about half of GDP. At that rate, China doubled its capital stock in the six years, 2005 to 2011. This has led to over-capacity in many sectors and a declining return to capital. There is widespread agreement within policy circles in China that the pattern of growth needs to shift to rely more on consumption and less on investment. China's household consumption is very low by international standards so that there is plenty of scope for increase.

Structural reforms will be an important part of the rebalancing, measures such as opening up the oligopolized service sectors to competition from private investors, including foreign investors. Or, easing up on the restrictions that prevent rural families from moving to cities, where they find significantly higher income and naturally have higher consumption. At the key third plenum meeting, the China Communist Party is likely to endorse in general moving towards a more market-oriented economy. But it is unlikely to roll out a large number of specific measures. Progress in many of the structural reform areas will be gradual.

Where there is more likely to be substantial movement is in the areas of financial and fiscal reforms. The authorities know that they need to slow down the rate of investment, but they want to do it very gradually so that the effect on aggregate growth is not too great. They had some modest success in 2012: Consumption's contribution to growth was actually slightly higher than investment's for the first time in years. However, in the first three quarters of 2013 China was back to the old pattern of investment growing faster than consumption. There are reforms in the financial and fiscal arenas that could accelerate a smooth transition to a more sustainable growth model.

In the financial arena, a key issue is the controlled deposit interest rate. This has been held close to the inflation rate over the past decade so that households get no real return on their financial wealth. It supports relatively low lending rates that encourage investment. Until recently households have had few options outside of the banks for their savings, as the stock and bond markets are underdeveloped. Firms that want to issue stocks or bonds need a number of different approvals, and there is a long line of firms waiting to go to the capital markets. The lack of good financial products encourages households to overinvest in real estate, and that is one of the foundations of the ongoing run-up in housing prices.

Reforming a repressed financial system is a tricky business. The central bank has already allowed banks a little bit of discretion on the deposit interest rate by permitting banks to offer up to 1.1 times the set rate. Banks quickly moved to take advantage of the extra flexibility. A natural next move would be to widen this band considerably. While bank interest rates have remained controlled, an important development in the past few years is the rapid expansion of a shadow banking system that offers trust products at higher interest rates. This has been a good development in terms of giving savers more market-oriented options. However, this is a lightly regulated sector and its development carries lots of risks. In the past few years there has been a very significant run-up in credit to GDP, and the authorities are trying to slow down the credit coming from the nonbank sector. Liberalization of interest rates, streamlining processes for stock and bond issuance, and bringing the shadow banking sector back into the regulated system—these are the natural next steps for financial reform. Well-designed deposit insurance could help contain the risks as China liberalizes the financial system. Financial reforms should support both macroeconomic stability and the transition towards more consumption-driven growth.

On the fiscal side, the key problem is a mismatch between local government expenditure responsibilities and their secure revenue base. Simply put, most taxes go to the center while most spending is at the local level. This mismatch requires ad hoc fiscal transfers each year. It also encourages local governments to pursue unsustainable revenue sources, notably the taking of land (which it is allowed to do with below-market compensation) and the sale of land to industry and developers. This process generates much social conflict, and is also a major source of corruption. An obvious reform under discussion is to introduce a property tax. This would provide a secure basis for local finance and also an incentive to stop hoarding empty apartments. Clearly this would have to be introduced carefully to avoid a sudden disruption of the housing market. Aside

from creating new sources of revenue for local government, there is also talk in Beijing of recentralizing certain expenditure responsibilities.

Another important aspect of local finance reform would be to allow local governments to issue bonds. In theory local governments cannot borrow. In practice they have set up thousands of infrastructure companies that float bonds and borrow from banks in order to finance infrastructure. Estimates of the total local government debt that has been accumulated are around 30 to 40 percent of GDP. One problem with this system is that much of the borrowing is short-term, to finance long gestation infrastructure projects, putting local finance on a risky foundation. Furthermore, the system is not transparent. There are widely differing estimates of the total local debt. Citizens do not have a good picture of all of the expenditures, including investment, by their local government. Moving to a system in which local governments have to disclose more about their overall finances and in which rating companies investigate and rate different local governments should lead to more efficient investment and avoid wasteful projects. A better informed populace is likely to lobby for more spending on education, health, and the environment, and less investment in prestige projects. There is a risk, however, that a move to allow local governments to issue bonds would simply lead to more total financing and hence more wasteful investment. So, it is likely that reform will be introduced gradually.

The extent to which China successfully introduces these and other structural reforms will have a major effect on Asia and the world economy. In the wake of the global crisis China's current account surplus has come down significantly, but it is still large, at about 2 percent of GDP. The overinvestment in China is likely to correct itself one way or another in the next few years—either smoothly with healthy growth and a shift towards more consumption, or in a more disruptive manner with market-oriented investment falling off as overcapacity and low returns become more prevalent. With a smooth transition, China will continue to be an important source of demand for the rest of the region and the world. A rebalancing towards more sustainable growth in China would benefit globally, even though China's growth may slow in the near term. The alternative is that savings remain elevated in China while investment falls off, and in that case there will be a tendency for China's external surplus to rise—at a time when demand is still weak globally. This would be a bad scenario for China and for the region. So, we wish our policymaking colleagues in Beijing well!

Transitioning to More Balanced and Sustainable Growth

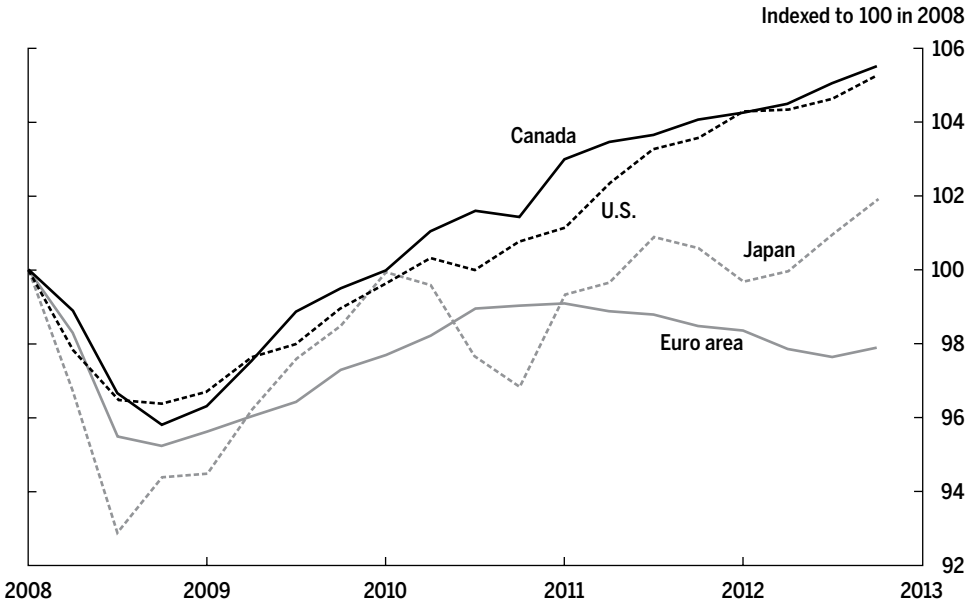
John Murray

Introduction

Canada shares many similarities with emerging market economies (EMEs) in Asia. Indeed, in some respects, we wish we were even more like them—particularly in regard to growth. Like most Asian countries, Canada has a very open economy that is heavily influenced by developments elsewhere, especially those in its southern neighbor. Despite Canada's sound financial system and solid fiscal position, it was seriously affected by the financial crisis and suffered proportionately almost as much as the U.S. economy did over the 2008–09 period, owing to its strong economic and financial links to the United States (Chart 1).

CHART 1

Canada's Economy Was Seriously Affected by the Crisis but Has Recovered



Note: Quarterly data, seasonally adjusted, 2008:Q3=100.

Sources: Statistics Canada, U.S. Bureau of Economic Analysis, Eurostat, Cabinet Office of Japan, and Bank of Canada calculations.

Although economic activity in Canada has now fully recovered and moved well beyond its pre-crisis peak, our economy is in the midst of a difficult rebalancing process and has yet to achieve self-sustaining growth unassisted by exceptionally accommodative monetary policy.

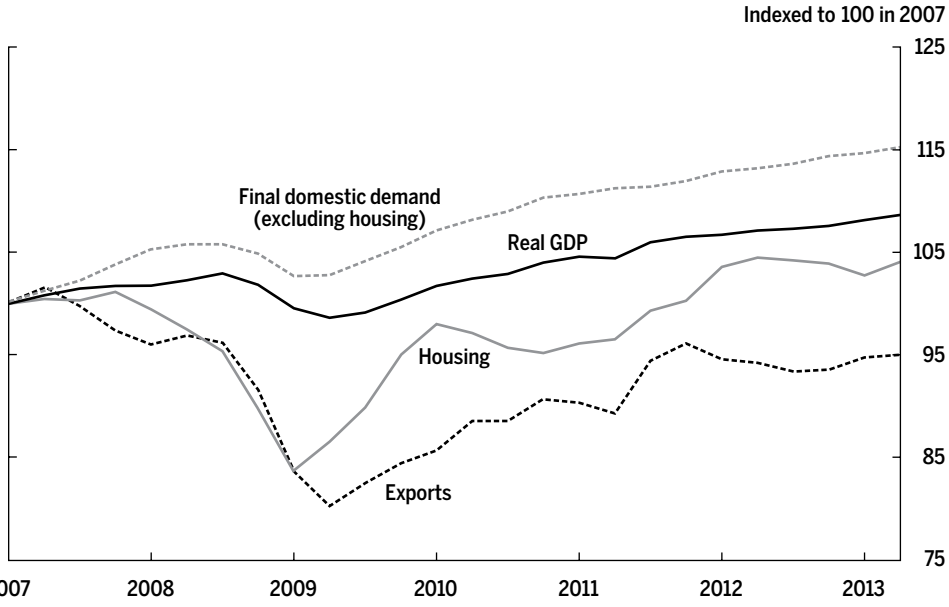
Unlike most Asian economies, Canada hopes to shift away from the excessive domestic demand that it was forced to rely on when its export sector collapsed, and to draw increasing support from external demand (Chart 2).

Unlike many advanced economies *and* EMEs that suffered from serious excesses before the crisis, in Canada's case, this re-equilibration should involve a return to the sort of balanced state that it enjoyed immediately prior to 2007.

There are other important ways in which Canada differs from some of its Asian trading partners. Over most of the post-World War II period, we have operated under a system of freely flexible exchange rates, absent any currency or capital controls. While we are exposed to many of the same external shocks experienced by other open economies, we have always believed that it is better to work with markets rather than against them, allowing the price system to operate. Yet "playing by the rules" has sometimes proven difficult, owing to the contagion created by those who are not. Nevertheless, in the long run, our flexible approach has served us well.

CHART 2

Canada Must Reduce Its Reliance on Domestic Demand



Sources: Statistics Canada and Bank of Canada calculations.

Asia's Phenomenal but Increasingly Unbalanced Growth

Over the past 13 years, the Asia region has experienced phenomenal economic growth, moving from a 7 percent share of global economic activity as recently as 2000 (measured at market prices) to an estimated share of close to 18 percent as of 2013. Measured in terms of purchasing power parity, the latest number would be even more impressive. The process has had some occasional setbacks, of course, and is not without precedent—I am thinking here of the late 19th and early 20th centuries and the emergence of the United Kingdom and the United States. But such growth is nevertheless extraordinary. Emerging Asia has accounted for more than 40 percent of the world's growth over the past 10 years, and hundreds of millions of people have been lifted out of extreme poverty.

Like most episodes of successful development in the postwar period, the Asian miracle has been driven by export-led growth. In many cases this was supported by a fixed exchange rate regime, and an extensive system of currency and capital controls designed to achieve and preserve international competitiveness. Of course, there has been considerable variation across countries with regard to their economic circumstances, institutional arrangements, and development strategies. The simple picture painted above does not apply to all. Nor are Asian countries the only ones in the global economy to enjoy sustained external surpluses. More importantly, for every trade surplus, there must be an equal and offsetting deficit, with many advanced countries eager in the past to play this role.

Such imbalances are not unusual, but the extent to which capital was "flowing uphill" during the pre-crisis period was. This was clearly unsustainable. It is one thing for relatively small countries to play this game, but when they grow too large, they soon run out of space. Foreign reserve accumulation among the EMEs since 2000 has totaled more than US$6 trillion (Chart 3).

The Crisis as a Catalyst for Change

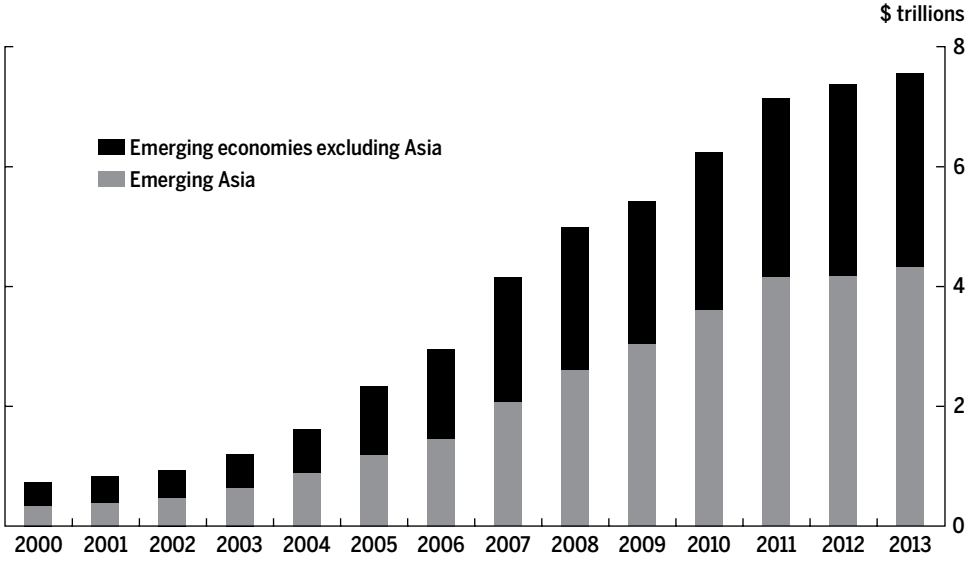
When the crisis hit, export markets for the emerging Asian economies suddenly imploded. Fortunately, many of them had the fiscal and monetary policy space to cushion the blow. However, the crisis merely brought forward a process of global rebalancing that was inevitable. Advanced economies had exhausted their credit lines, and EMEs were running out of foreign customers. Advanced economies were going to have to boost domestic savings to get out of hock, and EMEs were going to have to rely on their own consumers for future growth.

The coordinated and ambitious economic recovery plan that the Group of Twenty (G-20) leaders outlined in the early days of the crisis, the G-20

CHART 3

The Asian Miracle Has Generated Large Surpluses and Large Reserve Accumulations

Emerging Market Foreign Reserves, Annual Data



Source: International Monetary Fund.

Framework, was designed to deliver strong, sustainable, and balanced growth. It had four critical and mutually reinforcing parts: (1) meaningful fiscal consolidation in overly indebted countries; (2) sweeping financial sector reform; (3) wide-ranging structural reforms to boost future growth prospects; and (4) a necessary rebalancing of global demand between deficit and surplus countries, assisted by more flexible, market-determined exchange rates. The first three parts of the plan would inevitably have contractionary effects in the short run, so a domestic-led expansion of demand in surplus countries was a critical component of the G-20 plan if global deflation was to be avoided. Any positive confidence effects that might be associated with promises of fiscal rectitude and substantive structural reform were likely to be small and insufficient, on their own, to correct the widening output gap.

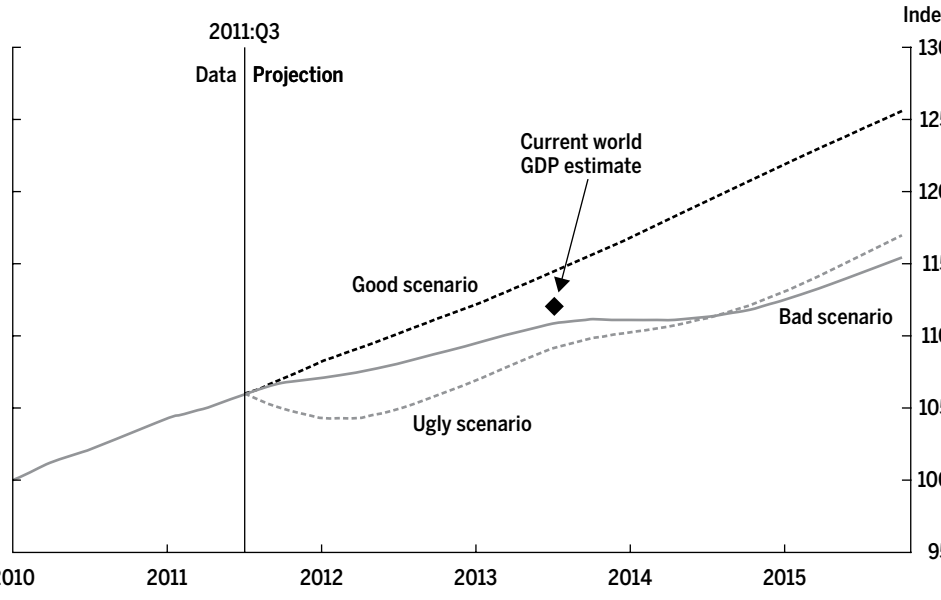
So How Have We Done?

It is safe to say that global economic performance over the past five years has been disappointing. As acknowledged in various G-20 communiqués, growth has been neither strong, nor sustainable nor balanced. Shortly after the crisis and the announcement of the G-20 Framework, economists at the Bank of

Canada decided to use their global model to examine three very different scenarios for how the global economy might unfold. The first was the so-called good scenario, where every player did what it had promised and all four parts of the plan were delivered. It is important to stress, however, that this was not a Goldilocks scenario by any means, just something that, in a rough-and-ready way, would satisfy the requirements of the G-20 Framework. The second scenario was a "bad" one, in which no one initially did what they were supposed to. But it assumed that eventually everyone would come around, after a substantial lag, and do the right thing. Without this assumption the model and, presumably, the global economy would explode. The third scenario was actually worse than the bad one, at least for the first few years of the simulation, and our economists called it the "ugly" scenario. It involved doing only half the job. More specifically, only the first three parts of the G-20 Framework, which were inherently deflationary, were set in motion. The estimated cumulative costs to the global economy from following the bad scenario over 2012–16, as opposed to the good one, were US$16 trillion, or 5.4 percent of global GDP (Chart 4). The estimated cumulative cost for the ugly scenario was even larger, at about US$18 trillion, or 5.8 percent of global GDP.

CHART 4

The "Good," the "Bad," and the "Ugly" Scenarios



Note: All series refer to world GDP.
Sources: GMUSE and BoC-GEM-Fin simulations.

So where is the real world economy now? Our best estimates suggest that we are sitting somewhere between the good and the bad scenarios but, in truth, a little closer to the bad. Performance with regard to the four key elements of the G-20 Framework has been mixed. Significant progress has been made on financial sector reform and fiscal consolidation, with sometimes too much of the latter, but much less has been accomplished on structural reform and global rebalancing. Had it not been for the support provided by exceptional monetary stimulus, the outcome would have been much worse, somewhere between the bad and the ugly scenarios. However, this situation cannot be sustained. Monetary policy provides only a temporary bridge; it cannot act as a substitute for more fundamental reform and economic adjustment.

Hopeful Signs on the Horizon

Happily, there are positive signs on the horizon. The advanced economies seem to be getting their act together. The preconditions for a return to stronger growth are present in the United States. Europe has emerged from a six-quarter recession and is progressing, albeit slowly, with its reforms. Japan has successfully launched the first stage of its "Three Arrows" program. Growth has recently faltered in some EMEs in response to past policy tightening, accumulated supply bottlenecks, and financial market turbulence. However, China appears to have stabilized its economy at a sustainable and solid growth rate of approximately 7.5 percent (conveniently consistent with its target growth rate).

More importantly perhaps, China and several other Asian countries appear to be liberalizing their economies, allowing more flexibility in prices and exchange rates, and otherwise assisting the adjustment process (Chart 5).

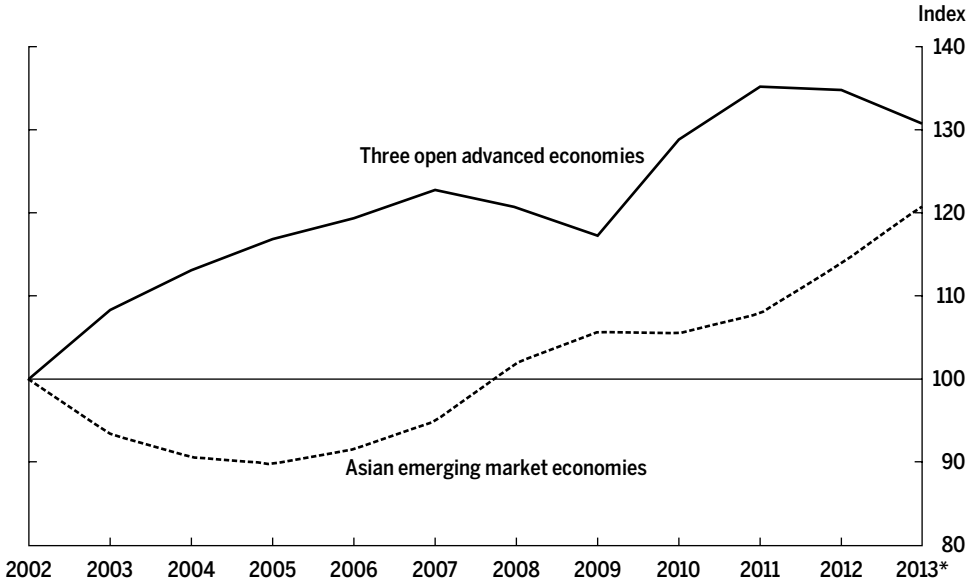
There is a risk, however, that the recent jump in financial market volatility in anticipation of tapering by the United States will tempt some countries to impose additional currency and capital controls and to intervene more aggressively. Indeed, there is newfound sympathy for these tools in the international community, at least when they're applied in a temporary and targeted manner as a form of international macroprudential stabilization. It is important, however, that nothing that is done as a possible short-term palliative be allowed to interrupt the rebalancing and necessary process of normalization that is under way in the global economy. Some may use this more forgiving attitude as cover to continue earlier unhelpful practices, but this would only invite a replay of past unpleasant events.

Exiting from the extraordinary policies that were put in place by several advanced economies to buttress growth is going to be challenging. As many observers have noted, we are traveling in uncharted territory. But at least the

CHART 5

Asian Emerging Market Economies Are Showing More Flexibility in Their Exchange Rates

GDP weighted annual data, 2002=100



*2013 GDP estimates taken from the IMF *World Economic Outlook*, October 2013. The 2013 Bank for International Settlements real effective exchange rates are an average for the first nine months of 2013.

Note: Asian emerging market economies include China, Thailand, and Malaysia. Three open advanced economies include Australia, Canada, and Switzerland.

Sources: Bank for International Settlements, International Monetary Fund *World Economic Outlook*, and Bank of Canada staff calculations.

incentives of the countries that are exiting—and those on the receiving end—should be well aligned. No one should want advanced economies to exit too early or too late, and no one benefits from excessive market turbulence. Some episodes of increased volatility will no doubt be experienced, but advanced economies are committed to being as transparent as possible in order to minimize surprises and smooth the adjustment process.

It is important that countries play by the rules and stand by the commitments that many of them made as part of the G-20 Framework. Displaced pressures from exchange rates that are not allowed to move, from capital flows that are directed elsewhere, and from outsized reserves that are looking for a safe home often squeeze small open economies such as Canada's and, more critically, frustrate the international adjustment process.

GENERAL DISCUSSION
Policymaker Panel

Chair: Mark Spiegel

Mr. Spiegel: We will use the time we have left for fielding some questions from the general audience. So let me make a list, I'll start with Ric.

Mr. Mishkin: David, the paper by Pritchett and Summers earlier on looked statistically at what would happen to China. The statistical analysis suggested that China is likely to have a very sharp slowing sometime in the future. So I'd be interested to hear your view about the probability that China will not slow and will be able to progress from low to middle income status and actually break through to the top tier?

Mr. Dollar: First I want to be clear that I don't believe anyone thinks China can continue to grow at 10 percent, right? So let's just take that off the table. Seven to eight percent over the next five years or so would be an outstanding performance. So as I say, I like Lant Pritchett's paper a lot because it's a good way of thinking about it. But what is missing from it is the policy side. The economies that avoided the kind of sharp slowdown he was talking about are basically South Korea and Taiwan. Depending on how you date it, you could also count Japan, if you think of Japan having some kind of a miracle up to about 1970 and then gradually slowing down before the stagnation. I have a paper on this arguing that China has a lot of important institutional differences from Japan, South Korea, and Taiwan in particular. The distinctive thing is that all three continued catching up to the U.S. total factor productivity (TFP) growth, getting up to 80 percent of U.S. TFP on average. China is now stagnating at about 40 percent, far behind the others. It also has a much higher capital output ratio and seems to have a much lower return to capital compared with the other countries at the same stage of development. So my serious answer to you is, I think China needs a very bold reform program to avoid a sharp slowdown, and I'm not going to put a probability on that. However, a really interesting question is, if China doesn't reform fast enough and there's a sharp slowdown sometime in the next five years, how will they respond? Will they pursue an aggressive agenda, so when you look out 20 years from now it'll be just a nasty blip? Or is there something that really is going to make it difficult? And the last

thing I'll say is, higher income helps to foster democratic political institutions, which in turn contribute to innovation and productivity growth. So among those other economies, Japan had democratic institutions in place at the beginning of its growth path. South Korea and Taiwan transitioned to democracy at about the stage where China is now. It will be really interesting to watch how China's political institutions evolve over the next 10 to 20 years.

Mr. Spiegel: Martin Wolf next.

Mr. Wolf: A comment and a question. The comment is this. You can go a bit further on your point, David. I increasingly believe the choice they're going to face is, what are they more committed to? The preservation of the political system as it is now, or the growth and success of China? I don't know the answer, so I wonder what you think. My question is about the future of foreign currency reserve accumulation. This is addressed particularly to you, Deputy Governors Choi and John Murray. As we all know, and as Carmen Reinhart has discussed, reserve accumulation has been large and unexpected, and it has reshaped the world financial and monetary system in important ways. That's the past. As you show very clearly, there's no break in the trend through the crisis. The question is, is there a good reason to expect those policies to change profoundly over the next 10 years? There are several arguments why they might or might not. The argument that they might is, many countries have enough reserves now. How much does China need, 10 trillion? They've got enough. The other reason why they might think it's enough is, it's very expensive. There are real costs, particularly if you're buying low interest-yielding U.S. Treasuries. On the other hand, they may still want to hold more reserves for other reasons. So the future of reserve accumulation really does affect your rebalancing story in a very big way. Then there's a subquestion, which seems to me absolutely decisive in this regard, which is, is China serious about turning the renminbi into a reserve currency over a reasonably short time horizon, the next decade or so, or not? Because in that case they won't be accumulating reserves, they'll be producing them, and that changes everything.

Mr. Dollar: I can very briefly answer the first part. The way you just framed it about the political institutions and democracy, there's some debate among intellectuals in China along those lines, Martin, but I don't think the people in power are really having this debate. I don't think they're taking it very seriously. If not and you're right about your presumption, then it's likely that you will get a sharp growth slowdown in the medium term. And then, as I said, the interesting question is how things respond. I would like to be optimistic about China,

that in the longer 20-year time horizon there are a lot of positives about Chinese society that could lead to a political transformation, but I wouldn't predict it in the near term.

Mr. Murray: I'm glad you went first David, you know much more about China than I'd ever pretend to. I guess the answer I'd give to you, Martin, does have an optimistic flavor, partly because I don't think there's any choice. As I tried to indicate in my remarks, they've sort of run out of runway. Trying to continue with the export-led growth just isn't going to work. For the advanced economies, the cupboard's bare. There's this need to consolidate and save on the fiscal side. In a way, they've run out of foreign customers, so they're forced by default to turn to their own domestic customers. That may be a delayed realization that rebalancing is the only way to preserve some growth. With regard to the renminbi, what I've seen and heard from the authorities is a fairly serious attempt to move towards broader, deeper financial markets and a more flexible internationalized renminbi. This is part of a package that uses price signals increasingly to help guide rebalancing and reform. So for these reasons I'm fairly hopeful. I shouldn't say optimistic, but there is no other alternative.

Mr. Spiegel: Deputy Governor Choi, would you like to comment?

Mr. Choi: Yes. Korea is one of the most interesting cases of reserve accumulation. Korea now holds $343 billion in its reserve silo. Although we have displayed financial market resilience to global financial turmoil over time, we still are very concerned about the big waves ahead associated with the Federal Reserve's QE tapering and normalization of its monetary policy stance over time. So we are trying to strengthen our financial sector further. In addition, we have lengthened the average maturity of our foreign borrowing. We are moving in the right direction, but still we have memories of the 1997–98 Asian financial crisis. So we are looking forward to strengthening Asian financial integration, and reaching some kind of new arrangement between countries in the region that will lessen the need for reserve accumulation. Our government authorities are aware that accumulating too much reserves will hinder investment for future growth. So the new government is trying to push higher domestic demand, which will be conducive to global rebalancing over time. Thank you.

Mr. Spiegel: Thank you. Peter, next question.

Mr. Hooper: A question for John on Canada. You get much well-deserved praise for having a well-run, well-regulated banking system and for having avoided the excesses during the bubble period in the United States. But now the housing

market does appear to be reaching bubble territory in Canada, judging from movement in price-to-rent ratios and price-to-income. How has this happened, and does this increase Canada's vulnerability to a possible run-up in global bond yields in the event of tapering?

Mr. Murray: Good question. Certainly we've identified household debt and house prices as our primary domestic risk. It has been one result of low interest rates, and this idea of using domestic demand as a bridge until we rebalance so that exports are carrying more of the load. We discussed this in our most recent monetary policy report. We see a soft landing, in which house prices, at least in real terms, actually declined slightly. And we've seen a sharp deceleration in household credit recently. We see things moving in the right direction. I don't think we ever talk about a housing bubble, but we have acknowledged in our financial system review that house prices are stretched in Canada. They no doubt have risen to quite high levels, as has the level of household debt to income. And that's now a consideration in our monetary policy process; we definitely regard this as one for risk management.

Mr. Spiegel: And the last question is from Michael Hutchinson.

Mr. Hutchinson: A question for David. In many respects the remarkable growth of China is associated with how quickly they can invest efficiently. What I mean by that is, if you speak to Americans engaged in foreign direct investment, it's truly remarkable how quickly they set up production facilities, those are approved, and they move forward with infrastructure projects. But the downside is that there are very serious environmental issues, as you pointed out, expropriations, and low real wages up until fairly recently. There are severe restrictions on mobility, that is, workers can come but not families. So in some sense the optimism you're suggesting about moving towards a more democratic system could put a brake on all of those things. Doesn't that suggest that if you have a more participatory process, that the very features of Chinese economic growth would in fact be severely slowed down, at least in the short run? So the scenario mentioned yesterday of a sharp growth slowdown in China seems to me very plausible in a political economy setting. Now to Deputy Governor Choi, we've seen a remarkable transformation in Korea away from a much more centrally controlled, credit controlled system, with the chaebols at the center of the system. Would you have any advice from the Korean experience for Chinese policymakers?

Mr. Spiegel: David, do you want to take the question first?

Mr. Dollar: Yes. I really think the current growth model in China is unsustainable, so I basically agree with the thrust of where you're going.

Democratization and more political participation would undermine a lot of features of the old growth model. I consider that a good thing because I really think the old model is unsustainable. You're right that it's very impressive the way local government can dispossess peasants with far below market compensation, and then they can put a job out to bid and put a metro through my neighborhood in Beijing in an extraordinarily short time during the crisis. But they're building up capital stock first to export, and now they've been so successful that they're the biggest exporter in the world. It's hard to see how that can grow faster than the world market. They invest to build a lot of infrastructure, but now they're at risk of overbuilding, because you've got major airports in small cities that you've never heard of. You've got metros in small cities. And the evidence is that the return to this capital is now dropping very sharply. Also, a lot of it is backed by debt, so to keep growth going I believe the old model will end in some kind of overinvestment crisis or financial crisis. Their best hope is to transform the model and, as you say, more popular participation would help. Because if people knew what their local government was doing, they would want more environmental protection, and more health and education and safety nets, and they would want a lot fewer prestige projects and industrial zones now that the future prospects do not look super.

Mr. Spiegel: And Deputy Governor Choi, you get the last word.

Mr. Choi: I pretty much agree with David's response, but I have some other suggestions for Chinese development strategy. Recently I heard that Chinese investment is mainly focused on boosting aggregate demand without contributing to future growth. Say, suppose, that the depreciation rate of Chinese capital is more than 20 percent, rather than 5 percent or 8 percent per year. Then what is the purpose of investment? If investment is mainly replacing depreciating capital, China must change its development strategy, boost demand that leads to income growth, which in turn leads to demand for investment. Now as regards the liberalization process in the financial sector and the foreign exchange market, I believe China is headed in the right direction in its plans to liberalize domestic interest rates and the foreign exchange market. But I think this process could speed up a little. After that, I believe China should think about overhauling its social infrastructure by improving its regulatory systems and encouraging more social mobility. Korea has moved in that direction very fast, but we still have a lot of regulations and restrictions in various sectors,

especially the service sector. So maybe the next decade's challenge for Korea is reforming the service sector and the labor market. Thank you.

Mr. Spiegel: Thank you. Please join me in thanking the panel for a very interesting discussion.

Closing Remarks

Barry Eichengreen

The Federal Reserve Bank of San Francisco's Asia Policy Conference provides as always a welcome opportunity to reflect on where Asia stands and where it is headed. These questions are even more interesting than usual this year. Where the region stands and where it is headed are, first and foremost, questions about China and Japan, which together account for the vast majority of regional GDP. Both countries are embarked on unprecedented experiments whose outcome is uncertain. In the case of Japan I am referring to the Great Reflation, in the case of China to the Great Rebalancing.

As for how these experiments will turn out, it is appropriate to quote Chou En Lai, who when asked by Richard Nixon, on the latter's trip to China in 1972, to assess the French Revolution, famously remarked, "It's too early to tell." Actually, we have learned recently that Chou was referring not to the 1789 revolution but to the student demonstrations and sit-ins of 1968. So it does not seem that it will be necessary to wait two centuries to evaluate these policy experiments.

My assessment of Japan's Great Reflation is: So far, so good. Governor Kuroda's policies of shock and awe have begun to show up in price-level trends, with inflation having hit a five-year high of 0.8 percent in August.[1] This is not especially impressive by absolute standards; after all, it is also what core inflation in the euro zone ran in the most recent month, and in the euro zone context this is regarded, rightly, as a policy failure. Still, it is an immense improvement for a Japanese economy mired in deflation for 15 long years. Two percent inflation is not yet at hand, but it is in sight, at least for those with 20/20 vision.

In terms of Abenomics' second arrow, I think the government has done a good job at balancing the need for fiscal stimulus in the short run, both to support growth and to lend credibility to the Bank of Japan's reflationary monetary policy, with the need for medium-term fiscal consolidation to prevent the public debt from spiraling out of control. It made the right call by not deferring the 3 percentage point increase in the consumption tax rate scheduled for next spring, with more to follow, while at the same time offsetting any negative impact in the short run with a one-time fiscal stimulus.

As for the third arrow, structural reform, here we must channel Chou En Lai. Mr. Abe continues to talk a good game, but actions speak louder than words. Recently he appears to have deferred to strong political opposition to reducing hiring and firing costs. Joining the negotiations over the Trans-Pacific Partnership may be a way of applying pressure to open up product markets like agriculture and automobiles, but it will do nothing to promote labor market reform, which the experience of other advanced economies, like those in Europe, suggests is key.

China, meanwhile, is engaged in the monumental task of rebalancing its economy from investment to consumption and from exports toward domestic demand. Monumental is the right word, given the extent of the shift that China will have to complete in order to begin to resemble a normal economy. Household consumption is only one-third of national income, where in a normal economy it is more like two-thirds. Investment is nearly 50 percent of national income, where no economy can productively invest more than a third of national income for an extended period. I think it is accurate to say that we have never seen a change in the composition of spending of this magnitude over a short period of time in any country in peacetime.

Raising the question of whether China really is committed to rebalancing dramatically in a short period of time. If rebalancing means significantly slower growth, then the authorities may hesitate. Whenever growth has shown signs of declining below 7½ percent, they've ramped up infrastructure spending and turned on the liquidity tap. (That's different from the liquidity trap.) M2 money supply growth has continued to exceed the official 13 percent target, which is inconsistent with the goal of clamping down on the shadow banking system.

The question thus comes in two parts. Does rebalancing mean slower growth? And, if so, are the authorities prepared to accept it?

The answer to the first question is clear. Rebalancing means slower growth. Much of the increase in consumption will be on services. We know from international experience, and specifically from the experience of East Asian countries like Korea, that it is harder to boost productivity in services than manufacturing. The service sector still accounts for a smaller share of Chinese GDP than manufacturing. As that changes, with rebalancing, growth will slow.

The answer to the second question—are the authorities really prepared to sacrifice some growth in the interest of rebalancing?—is less obvious. Stay tuned for the Communist Party plenum.

The other issue that must be confronted when contemplating where East Asia is headed is the crisis question, as we have heard in the course of the past two days. The year 2013, clearly, is not 1998. Asian countries have more flexible

exchange rates. More debt is in local currencies. They have more international reserves. In most cases they are running current account surpluses rather than deficits. They have stronger monetary and fiscal policies and better regulated financial and corporate sectors, by and large. Note the "by and large." In other words, there are risks.

Most obviously, there is China. Credit broadly defined has increased from 125 percent of GDP to nearly 200 percent of GDP in just five years. If we know one thing about credit booms, it is that they end badly. One has to be more of an expert on Chinese shadow banking than I am to know exactly where the time bombs are. But we can hear them ticking.

Then there are Indonesia and India. Both have been running current account deficits. While they have substantial foreign exchange reserves, those reserves only cover about six months' worth of imports. It is no coincidence that their currencies were hit the hardest last summer when Mr. Bernanke engaged in his tapering talk. They will have difficult adjustments when tapering actually occurs. Whether these are simply difficult adjustments, involving currency depreciation, inflation, and economic slowdown, or something worse will hinge, as it always has historically, on whether currency depreciation and the greater difficulty of tapping foreign finance expose fissures in their banking systems. We are told that their banking systems are prudently managed. We will see.

Allow me now to say a few words on the papers, starting with that by Lant Pritchett and Larry Summers. They remind us that mean reversion in growth rates is a robust regularity. They also remind us that no country grows at double-digit rates forever. That China has done so for more than two decades is historically unprecedented. This means either that mean reversion is overdue or that China's experience is, well, historically unprecedented. Which interpretation is correct? We are about to find out.

Lant and Larry's paper also reminds us that forecasting growth is difficult. Another way of saying this is that there is a significant probabilistic element in the answer to questions like whether there will be a sharp slowdown in Chinese growth. But I do think we know some things about the policies that make for reversion to the mean (in their terms) and sharp growth slowdowns in fast growing economies (my term). Slowdowns are more likely in fast-growing economies as they approach the technological frontier. They are more likely in countries that have been growing fast on the basis of exceptionally high investment (that is, when they have been throwing a lot of capital at the growth problem). They are more likely in countries with undervalued exchange rates (which limit the incentive to move up the technological ladder, out of assembly operations). They are more likely in countries that underinvest in secondary and tertiary

education.[2] Some of these conditions suggest that there is a significant probability of a sharp Chinese slowdown, others not. It is not surprising, then, that commentators disagree.

Reinhart and Tashiro in their paper make an important point, that reserve accumulation and lessened dependency on foreign finance in East Asia since 1998, while prudent, is not without costs. Raising savings relative to investment so as to invest more abroad and accumulate reserves has been associated with a decline in the investment/GDP ratio of about 6 percentage points in the countries they consider compared to the pre-Asian crisis decade. I was not entirely surprised by this finding, for it was heavily emphasized by Raghuram Rajan in his days as chief economist at the International Monetary Fund (IMF), when he spoke of trans-Pacific imbalances as reflecting less a "savings glut," à la Greenspan and Bernanke, than an "investment strike"; it was also highlighted by the Asian Development Bank in various reports.[3]

The important question in this connection is whether reduced capital formation has had costs in terms of growth. Some would argue that a significant share of this earlier investment was unproductive, like investment in toll roads in Spain or country estates in Ireland more recently. It pumped up growth in the short run but set the stage for subsequent problems. Remember all those high rises in Bangkok in the mid-1990s and the expansion of Korean chaebol into unrelated business lines? It is at least conceivable that lower investment since the crisis has been good for stability and free of negative consequences for medium-term growth.

Olivier Jeanne's paper similarly speaks to the management of capital flows and credit booms, asking whether macroprudential policies, including capital controls, can provide an efficient alternative to reserve accumulation. There is an analogy here with the "lean-versus-clean" debate—should central banks lean against credit booms, capital-flow surges, and asset bubbles with macroprudential policies and, in the open economy context, capital controls? Or should they limit themselves to cleaning up after the fact, which in the open economy means accumulating reserves to deal with capital flow reversals? Jeanne takes the answer as uncontroversial, appropriately in my view. Cleaning up after the fact can be very costly, as we have learned the hard way. The case for using macroprudential policies and temporary capital controls has been strengthened by recent experience.

The more controversial question is whether macroprudential policies, including controls, have significant cross-border spillovers, creating a case for international oversight and coordination. The IMF certainly thinks so, scenting an additional role for its staff. Within Asia, this is similarly a ques-

tion that ASEAN+3's Macroeconomic Research Office could usefully take up. Both intuition and evidence, as provided by, among others, Kristen Forbes et al. (2012), suggests that scope for capital flow diversion creates a prima facie case for coordination. It is useful to have Olivier's formal demonstration of the case.

I am similarly happy to have him lay out, in what is the first formal modeling of an oft-heard point, that policy coordination is desirable in response to the currency war problem. In other words, if U.S. efforts to stimulate spending through low interest rates cause problems for countries like China through the capital-inflow channel, while China's efforts to accumulate reserves create problems for the United States by depressing spending on American exports, then mutually accommodating policy adjustments can leave both countries better off. As always, the unanswered question in the policy coordination literature is whether the gains are large.

Turning to Andy Rose's paper on the impact of the crisis on countries with different exchange rate regimes, I learned a lot, as always from Andy's papers. But I for one still find the results surprising. I did not expect to learn that countries that peg and those that float and inflation target had indistinguishable outcomes in the 2007–12 period. This is certainly a striking finding.[4]

Anil Kashyap suggested that the explanation lies in the nature of the shock and the nature of the policy response. Everyone experienced the same deflationary shock. Everyone wanted to cut interest rates to zero in response. The Fed cut rates to zero. So if you pegged to the dollar, you got zero interest rates. And if you targeted inflation instead, you also got zero interest rates, since your central bank cut rates in response to the deflationary shock.[5]

This may be right. But here's another interpretation. Not every one of the 180 countries in the sample experienced a deflationary shock. The financial crisis had multiple dimensions; the global environment after 2007 was quite complex and varied. Some countries experienced deflation and wanted zero interest rates, but others experienced excessive inflation after 2007 and didn't want zero interest rates. Different countries experienced different internal versus external, real versus nominal, monetary versus financial, and price level versus terms of trade shocks, all at the same time. It may be that if you were able to cut up the sample appropriately, distinguishing countries by the type of shock they predominantly experienced, you would find that one exchange rate/monetary regime outperformed another, just not in the same direction in different subsamples.

So should Asian policymakers take the exchange rate cum monetary regime as given and concentrate on the pursuit of sound and stable policies, rather than

worrying about whether they have the appropriate rate/regime in place? I for one am not yet ready to endorse that recommendation.

On the other hand, I am quite happy to endorse the recommendation of Gerard Caprio's paper. Caprio suggests that our current system of financial regulation is excessively opaque, overly complex, and inadequately robust. He documents how each successive crisis, by revealing gaps in regulation, has caused policymakers to pile still more complex regulation on top of an already shaky edifice. The system relies excessively on mechanisms—risk weights and commercial credit ratings—that are too easily rigged and evaded.

It would be better to throw the baby out with the bathwater, as it were, and rely on simple rules and market discipline. The most important rule would be an unweighted capital or leverage ratio. For those worried about the stability of particular markets—property markets or funding markets, for example—this might be supplemented by ceilings on loan-to-value and foreign currency lending and borrowing ratios. Market discipline would be strengthened by requiring financial institutions to issue contingent convertible debt, which would give bondholders strong incentives to monitor banks and, not incidentally, protect taxpayers from losses. Regulators can be better incentivized by strengthening their accountability—in other words, by requiring them to release more of the information on which they base their decisions. More radically, one can imagine tying their compensation to financial stability outcomes, in the same manner that the compensation of the governor of the Reserve Bank of New Zealand is tied to inflation outcomes. Writing that contract would be harder, admittedly, since financial stability is multidimensional. But that's the world in which we live.

These are radical recommendations. They run up against standard objections, like the Morris and Shin (2002) argument that more information can be destabilizing under certain circumstances. But given the serial failures of the current approach, I am inclined to agree with Caprio that it is worth running some risks in order to explore an alternative. It's not as if the current system itself is without risks.

An interesting question is why there has been a reluctance to go in this direction. Implicit in Jerry's paper is a political economy argument. The Western countries that dominate the Basel Committee on Banking Supervision have too much invested in the current approach to entertain radical alternatives. The central bankers on the Basel Committee have an intellectual investment. Their commercial banks have made strategic investments to maximize profitability under the current system. The rating agencies like the system. The hotels and

restaurants of Basel like the system. Asian countries, by comparison, are outsiders. They are the plausible revolutionaries.

I wish it were so, but I'm not convinced. Big banks in Asia, and there are plenty of big banks in Asia, have invested every bit as much as big banks in the West at adapting to Basel III. More and more Asian central bankers and regulators are being invited to partake at the bimonthly buffet at the Bank for International Settlements. They are too invested in the current system, I fear, to be true revolutionaries.

Here's hoping they prove me wrong.

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NOTES

1 This is the most recent data point available at the time of our meeting.

2 I refer to the results of my collaborations with Park and Shin (Eichengreen, Park, and Shin 2011, 2013).

3 See, for example, Rajan (2006). Hence I do not entirely agree that "previous studies have not made a connection between the sustained reserve accumulation and the persistent and significantly lower levels of investment in the region."

4 Note that there were no inflation targeters in our sample, although there was, arguably, a price-level targeter, namely Sweden. Leaving this aside, our comparison was tantamount to Rose's comparison between peggers and countries in the "sloppy center." Thus, the contrast between our respective findings stands.

5 This interpretation can also explain why Rose's results for 2007–12 are so different from what Jeffrey Sachs and I found for the 1930s (Eichengreen and Sachs 1985). (The paper is cited by Rose, but he does not comment on the strong contrast in results.) We found that the exchange rate regime mattered importantly—that countries that did not peg to the dollar or gold did significantly better in that earlier deflationary environment. The reason, of course, is that the Fed did not take adequate action to counter deflation until four years into the crisis.

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Woon Gyu Choi is currently a Deputy Governor and the Director General of the Economic Research Institute at the Bank of Korea. Before joining the BOK in June 2012, he worked at the International Monetary Fund in the Asian Division of the IMF Institute from 2000 to 2012. At the IMF Institute, he taught various courses in macroeconomics, international finance, finance, and related policy issues to government officials worldwide. He led and coordinated various IMF courses such as financial programming and policies, macroeconomic diagnostics, economic policies for financial stability, financial market analysis, macroeconomic management and financial sector issues/fiscal policy, and monetary and exchange rate policy. Prior to joining the Fund, he worked at the Research Department of the BOK (1987–91), and taught all levels of courses including money and banking and advanced macroeconomics as an Assistant Professor at the Hong Kong University of Science & Technology (1995–2000).

Mr. Choi's research interests include monetary policy and financial markets, aggregate and corporate money demand, exchange rate policy and fiscal policy issues, international reserves, financial cycles, macroeconomic policies and unemployment, and global financial market issues. He has publications in leading academic journals including *Journal of Monetary Economics, Journal of International Economics, Journal of Money, Credit, and Banking*, and *Journal of Financial and Quantitative Analysis*. He obtained his PhD in Economics from the University of California, Los Angeles.

J. Bradford DeLong, Professor
University of California, Berkeley

Brad DeLong is Professor of Economics at University of California at Berkeley, Chair of the Political Economy of Industrial Societies major, and a Research Associate of the National Bureau of Economic Research. He was educated at

Harvard University, where he received his PhD in 1987. He joined UC Berkeley as an Associate Professor in 1993 and became a full Professor in 1997.

Mr. DeLong served as the U.S. Deputy Assistant Secretary of the Treasury for Economic Policy from 1993 to 1995. He worked on the Clinton Administration's 1993 budget, on the Uruguay Round of the General Agreement on Tariffs and Trade, on the North American Free Trade Agreement, on macroeconomic policy, and on health-care reform. Before joining the Treasury Department, Mr. DeLong was Danziger Associate Professor in the Department of Economics at Harvard University. He has also been a John M. Olin Fellow at the National Bureau of Economic Research, an Assistant Professor of Economics at Boston University, and a Lecturer in the Department of Economics at Massachusetts Institute of Technology.

David Dollar,
Senior Fellow and Former U.S. Treasury's Economic and Financial Emissary to China
Brookings Institution

David Dollar is a Senior Fellow with the Foreign Policy and Global Economy and Development programs in the John L. Thornton China Center. He is a leading expert on China's economy and U.S.-China economic relations. From 2009 to 2013 he was the U.S. Treasury's Economic and Financial Emissary to China. In that capacity, he facilitated the economic and financial policy dialogue between the United States and China. That included the formal meetings, notably the annual Strategic and Economic Dialogue, as well as constant exchanges between the Treasury Department and Chinese economic policymakers at all levels. Based at the U.S. Embassy in Beijing, Mr. Dollar reported back to Washington on economic and policy developments in China.

Mr. Dollar worked at the World Bank for 20 years, and from 2004 to 2009 was Country Director for China and Mongolia. His other World Bank assignments primarily focused on Asian economies, including South Korea, Vietnam, Cambodia, Thailand, Bangladesh, and India. From 1995 to 2004, Mr. Dollar worked in the World Bank's research department. Prior to his World Bank career, he was an Assistant Professor of Economics at University of California at Los Angeles, spending a semester in Beijing teaching at the Graduate School of the Chinese Academy of Social Sciences. He has a PhD in Economics from New York University and a BA in Chinese history and language from Dartmouth College. He has written extensively about economic reform in China, globalization, and economic growth.

Barry Eichengreen, Professor
University of California, Berkeley

Barry Eichengreen is the George C. Pardee and Helen N. Pardee Professor of Economics and Professor of Political Science at the University of California at Berkeley, where he has taught since 1987. He is a Research Associate of the National Bureau of Economic Research and Research Fellow of the Centre for Economic Policy Research. In 1997–98 he was Senior Policy Advisor at the International Monetary Fund. He is a Fellow of the American Academy of Arts and Sciences (class of 1997).

Mr. Eichengreen is the convener of the Bellagio Group of academics and economic officials and Chair of the Academic Advisory Committee of the Peterson Institute of International Economics. He has held Guggenheim and Fulbright Fellowships and has been a Fellow of the Center for Advanced Study in the Behavioral Sciences in Palo Alto and the Institute for Advanced Study in Berlin. He is a regular monthly columnist for Project Syndicate. His most recent books are *From Miracle to Maturity: The Growth of the Korean Economy* with Dwight H. Perkins and Kwanho Shin (Harvard East Asian Monographs, 2012) and *The World Economy after the Global Financial Crisis*, co-edited with Bokyeong Park (World Scientific Publishing, 2012). Other books include *Labor in the Era of Globalization*, co-edited with Clair Brown and Michael Reich (Cambridge University Press, 2009), and *Institutions for Regionalism: Enhancing Asia's Economic Cooperation and Integration*, coedited with Jong-Wha Lee (2009). Mr. Eichengreen was awarded the Economic History Association's Jonathan R.T. Hughes Prize for Excellence in Teaching in 2002 and the University of California at Berkeley Social Science Division's Distinguished Teaching Award in 2004. He is the recipient of a doctor honoris causa from the American University in Paris, and the 2010 recipient of the Schumpeter Prize from the International Schumpeter Society. He was named one of *Foreign Policy Magazine*'s 100 Leading Global Thinkers in 2011.

Robert C. Feenstra, Professor
University of California, Davis

Robert C. Feenstra holds the C. Bryan Cameron Distinguished Chair in International Economics at the University of California at Davis. He also directs the International Trade and Investment program at the National Bureau of Economic Research, in Cambridge, MA.

Mr. Feenstra has published nearly 100 articles in international trade and 15 books, including the graduate textbook *Advanced International Trade: Theory and Evidence* (Princeton University Press, 2004), and an undergraduate textbook jointly with Alan M. Taylor, *International Economics* (Worth Publishers, 2011). His research has been supported by grants from the National Science Foundation, the Ford Foundation, and the Sloan Foundation. He specializes in U.S. and global trade patterns and has lectured in Canada, Europe, China, and throughout Asia.

Takeo Hoshi, Professor

Stanford University

Takeo Hoshi is Henri and Tomoye Takahashi Senior Fellow at the Walter H. Shorenstein Asia-Pacific Research Center at Stanford University and Professor of Finance (by courtesy) at Stanford Graduate School of Business. Before he joined Stanford University in 2012, he was Pacific Economic Cooperation Professor in International Economic Relations at the Graduate School of International Relations and Pacific Studies at the University of California at San Diego, where he conducted research and taught on the Japanese economy since 1988. Mr. Hoshi also serves on the Board of Directors at Union Bank. He is a Visiting Scholar at the Federal Reserve Bank of San Francisco, Research Associate at the National Bureau of Economic Research and at the Tokyo Center for Economic Research, and Senior Fellow at the Asian Bureau of Finance and Economic Research.

Mr. Hoshi's main research interests include the financial aspects of the Japanese economy, especially corporate finance, banking, and monetary policy. He received the 2011 Reischauer International Education Award of Japan Society of San Diego and Tijuana, 2006 Enjoji Jiro Memorial Prize of Nihon Keizai Shimbun-sha, and 2005 Japan Economic Association-Nakahara Prize. His book, *Corporate Financing and Governance in Japan: The Road to the Future* (MIT Press, 2001) co-authored with Anil Kashyap, received the Nikkei Award for the Best Economics Books in 2002. Other publications include "Japanese Government Debt and Sustainability of Fiscal Policy" (with Takero Doi and Tatsuyoshi Okimoto), *Journal of the Japanese and International Economies*, 2011; "Will the U.S. Bank Recapitalization Succeed? Eight Lessons from Japan" (with Anil Kashyap), *Journal of Financial Economics*, 2010; and "Zombie Lending and Depressed Restructuring in Japan" (with Ricardo Caballero and Anil Kashyap), *American Economic Review*, 2008. Mr. Hoshi received his BA in Social Sciences from the University of Tokyo in 1983, and a PhD in Economics from the Massachusetts Institute of Technology in 1988.

Chang-Tai Hsieh, Professor
University of Chicago

Chang-Tai Hsieh conducts research on growth and development. He has published several papers in top economic journals, including "Misallocation and Manufacturing TFP in China and India" in the *Quarterly Journal of Economics*; "Relative Prices and Relative Prosperity" and "What Explains the Industrial Revolution in East Asia? Evidence from the Factor Markets" in the *American Economic Review*; and "Can Free Entry Be Inefficient? Fixed Commissions and Social Waste in the Real Estate Industry" in the *Journal of Political Economy*.

Mr. Hsieh has been a visiting scholar at the Federal Reserve Banks of San Francisco, New York, and Minneapolis, as well as the World Bank's Development Economics Group and the Economic Planning Agency in Japan. He is a Research Associate for the National Bureau of Economic Research, a Senior Fellow at the Bureau for Research in Economic Analysis of Development, a Co-Director of the China Economics Summer Institute, and a member of the Steering Group of the International Growth Center in London. He is the recipient of an Alfred P. Sloan Foundation Research Fellowship, Smith-Richardson Foundation Research Fellowship, and the Sun Ye-Fang award for research on the Chinese economy.

Olivier Jeanne, Professor
Johns Hopkins University

Olivier Jeanne is Professor of Economics at Johns Hopkins University, which he joined in 2008 after 10 years at the Research Department of the International Monetary Fund. His research spans an array of applied and theoretical topics in international and domestic macroeconomics: capital flows, exchange rate regimes and currency crises, sovereign debt and defaults, international liquidity, and monetary policy. Mr. Jeanne is a Senior Fellow at the Peterson Institute for International Economics in Washington, DC, a Research Associate at the National Bureau of Economic Research in Cambridge, MA, and a Research Fellow at the Center for Economic Policy Research in London.

Mr. Jeanne graduated from Ecole Polytechnique and Ecole Nationale des Ponts et Chaussees in France, and holds an MSc in Economics from the London School of Economics and a PhD in Economics from Ecole des Hautes Etudes en Sciences Sociales in Paris. Before joining Johns Hopkins University, he taught economics at Ecole Polytechnique, University of California at Berkeley and Princeton University.

Anil K Kashyap, Professor
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Anil K Kashyap is the Edward Eagle Brown Professor of Economics and Finance and Richard N. Rosett Faculty Fellow at the University of Chicago Booth School of Business. He is one of the faculty directors of the Chicago Booth Initiative on Global Markets. He has authored and edited five books and over 40 scholarly articles on banking, business cycles, the Japanese economy, and monetary policy.

Mr. Kashyap currently works as a consultant for the Federal Reserve Bank of Chicago, and serves as a member of the Economic Advisory Panel of the Federal Reserve Bank of New York, and as a Research Associate for the National Bureau of Economic Research. He is an advisor to the Swedish Riksbank, is on the Congressional Budget Office's Panel of Economic Advisers, is a member of the Financial Research Advisory Committee to the U.S. Department of the Treasury's Office of Financial Research, and serves on the Board of Directors of the Bank of Italy's Einuadi Institute of Economics and Finance. He is a member of the Squam Lake Group, the Bellagio Group, and the International Monetary Fund's Advisory Group on macroprudential policy. Mr. Kashyap cofounded the U.S. Monetary Policy Forum. He currently teaches advanced MBA classes on analyzing financial crises and understanding central banks. Mr. Kashyap earned an undergraduate degree in Economics and Statistics from the University of California at Davis and a PhD in Economics from the Massachusetts Institute of Technology.

Frederic S. Mishkin, Professor
Columbia University

Frederic S. Mishkin is the Alfred Lerner Professor of Banking and Financial Institutions at the Graduate School of Business, Columbia University. He is also a Research Associate at the National Bureau of Economic Research, and from September 2006 to August 2008 was a Governor on the Board of Governors of the Federal Reserve System. He has been a Senior Fellow at the FDIC Center for Banking Research, and past President of the Eastern Economic Association. Since receiving his PhD from the Massachusetts Institute of Technology in 1976, he has taught at the University of Chicago, Northwestern University, Princeton University, and Columbia. He has also received an honorary professorship from the Peoples (Renmin) University of China. From 1994 to 1997 he was Executive Vice President and Director of Research at the Federal Reserve

Bank of New York and an Associate Economist of the Federal Open Market Committee of the Federal Reserve System.

Mr. Mishkin's research focuses on monetary policy and its impact on financial markets and the aggregate economy. He is the author of *The Economics of Money, Banking and Financial Markets* (Prentice Hall, 1986). In addition he has authored more than 15 other books, and has published over 150 articles. Mr. Mishkin has served on the editorial board of *American Economic Review*, has been associate editor at *Journal of Business and Economic Statistics, Journal of Applied Econometrics*, and *Journal of Economic Perspectives*, and was editor of the Federal Reserve Bank of New York's *Economic Policy Review*. He is currently associate editor at six academic journals. He was a consultant to the Board of Governors, the World Bank, the Inter-American Development Bank, the International Monetary Fund, and numerous central banks. He was on the South Korea Financial Supervisory Service Advisory Board and advisor to the Bank of Korea Institute for Monetary and Economic Research.

Ashoka Mody, Professor
Princeton University

Ashoka Mody is Charles and Marie Robertson Visiting Professor in International Economic Policy at the Woodrow Wilson School, Princeton University. Previously, he was Deputy Director in the International Monetary Fund's Research and European Departments. He was responsible for the IMF's Article IV consultations with Germany, Ireland, Switzerland, and Hungary, and also for the design of Ireland's financial rescue program. Earlier, at the World Bank, his management positions included those in Project Finance and Guarantees and in the Prospects Group, where he coordinated and was principal author of the Global Development Finance Report of 2001. He has advised governments worldwide on developmental and financial projects and policies, while writing extensively for policy and scholarly audiences.

Mr. Mody has been a Member of Staff at AT&T's Bell Laboratories, a Research Associate at the Centre for Development Studies, Trivandrum, and a Visiting Professor at the University of Pennsylvania's Wharton School. He is a nonresident Fellow at the Center for Financial Studies, Frankfurt, and the Center for Global Government, Washington, DC. He received his PhD in Economics from Boston University.

John Murray, Deputy Governor
Bank of Canada

John Murray was appointed Deputy Governor of the Bank of Canada in January 2008. In this capacity, he is one of two Deputy Governors responsible for overseeing the Bank's analysis of domestic and international economic developments in support of monetary policy decisions. As a member of the Bank's Governing Council, he shares responsibility for decisions with respect to monetary policy and financial system stability, and for setting the strategic direction of the Bank.

Born in Toronto, Mr. Murray received a Bachelor of Commerce degree from Queen's University in 1971, as well as an MA and a PhD in Economics from Princeton University in 1974 and 1977, respectively. After completing his PhD, Mr. Murray taught at the University of British Columbia as an Assistant Professor and at the University of North Carolina as a visiting Assistant Professor. From 1985 to 1986, he also lectured at Princeton University. Mr. Murray joined the Bank of Canada in 1980 as a Senior Economist with the Monetary and Financial Analysis Department. In 1981, he was promoted to the position of Research Officer, and in 1982, he became Assistant Chief of the department. He served as Research Adviser in the Monetary and Financial Analysis and International Department from 1984 to 1987. In 1987, he was appointed Deputy Chief of the International Department, and in 1990 he was appointed Chief. Mr. Murray became an Adviser to the Governor in January 2000.

Jonathan D. Ostry, Deputy Director, Research Department
International Monetary Fund

Jonathan D. Ostry is Deputy Director of the Research Department (RES) at the International Monetary Fund. His current responsibilities include leading staff teams on IMF-FSB Early Warning Exercises on global systemic macro-financial risks; vulnerabilities exercises for advanced and emerging market countries; multilateral exchange rate surveillance, including the work of CGER, the Fund's Consultative Group of Exchange Rates, and EBA, the External Balance Assessment; international financial architecture and reform of the IMF's lending toolkit; capital account management (capital controls and prudential tools to manage capital inflows) and financial globalization issues; fiscal sustainability issues; and country insurance.

Mr. Ostry's past positions include leading the division that produces the IMF's flagship multilateral surveillance publication, *World Economic Outlook*, and leading country teams on Australia, Japan, New Zealand, and Singapore. He is the author or editor of a number of books on international macro policy issues and numerous articles in scholarly journals. Mr. Ostry earned undergraduate degrees from the University of Oxford (Balliol College) and Queen's University (Canada), an MSc degree from the London School of Economics in 1984, and a PhD from the University of Chicago in 1988.

Jerome H. Powell, Governor
Board of Governors of the Federal Reserve System

Jerome H. Powell took office on May 25, 2012, to fill an unexpired term ending January 31, 2014. Before his appointment to the Board, Mr. Powell was a visiting scholar at the Bipartisan Policy Center in Washington, DC, where he focused on federal and state fiscal issues. From 1997 through 2005, Mr. Powell was a partner at The Carlyle Group.

Mr. Powell served as an Assistant Secretary and as Undersecretary of the Treasury under President George H.W. Bush, with responsibility for policy on financial institutions, the Treasury debt market, and related areas. Before joining the Administration, he worked as a lawyer and investment banker in New York City. In addition to service on corporate boards, Mr. Powell has served on the boards of charitable and educational institutions, including the Bendheim Center for Finance at Princeton University and The Nature Conservancy of Washington, DC, and Maryland. He received an AB degree in politics from Princeton University in 1975 and earned a law degree from Georgetown University in 1979. While at Georgetown, he was editor-in-chief of the *Georgetown Law Journal*.

Lant Pritchett, Professor
Harvard University

Lant Pritchett is Professor of the Practice of Economic Development at the Harvard Kennedy School at Harvard University and is a Senior Fellow of the Center for Global Development. He graduated from Brigham Young University in 1983 with a BS degree in Economics and in 1988 from MIT with a PhD in Economics. He has authored or coauthored numerous articles in development-related fields, including economic growth, education, population, social capital, health, and migration. His most recent book *The Rebirth of Education: Schooling Ain't Learning* was published in October 2013.

Mr. Pritchett worked at the World Bank in both research and in operations, living and working in Indonesia from 1998 to 2000 and in India from 2004 to 2007. He served as a team member in writing many World Bank reports, including *World Development Report 1994: Infrastructure for Development, Assessing Aid: What Works, What Doesn't, and Why* (1998), and *World Development Report 2004: Making Services Work for the Poor, Economic Growth in the 1990s: Learning from a Decade of Reforms* (2005). In his focus on development, Mr. Pritchett also has served as coeditor of the *Journal of Development Economics*, as a consultant to Google's development efforts, and as Faculty Chair of the Master in Public Administration/International Development program at Harvard Kennedy School.

Carmen M. Reinhart, Professor
Harvard University

Carmen M. Reinhart is the Minos A. Zombanakis Professor of the International Financial System at the Harvard Kennedy School at Harvard University. Previously, she was the Dennis Weatherstone Senior Fellow at the Peterson Institute for International Economics and Professor of Economics and Director of the Center for International Economics at the University of Maryland. Ms. Reinhart held positions as Chief Economist and Vice President at the investment bank Bear Stearns in the 1980s. She spent several years at the International Monetary Fund. Ms. Reinhart is a Research Associate at the National Bureau of Economic Research, and a member of the Congressional Budget Office Panel of Economic Advisers and Council on Foreign Relations. She has served on numerous editorial boards, has testified before Congress, and was listed among Bloomberg Markets' 50 Most Influential in Finance, 2011.

Ms. Reinhart received her PhD from Columbia University. She has written on macroeconomics and international finance and trade, and her papers have been published in leading scholarly journals. Her work has helped to inform the understanding of financial crises for over a decade. Her best-selling book (with Kenneth S. Rogoff) entitled *This Time Is Different: Eight Centuries of Financial Folly* (Princeton Press, 2009) documents the striking similarities of the recurring booms and busts that have characterized financial history; it has been translated into 20 languages and won the 2010 Paul A. Samuelson TIAA-CREF Institute Award, among others.

Andrew K. Rose, Professor
University of California, Berkeley

Andrew K. Rose is the B.T. Rocca Jr. Professor of International Business in the Economic Analysis and Policy Group, Haas School of Business at the University of California at Berkeley; he serves as Associate Dean for Academic Affairs, and Chair of the Faculty. He is also a Research Associate of the National Bureau of Economic Research, and a Research Fellow of the Centre for Economic Policy Research, London. He received his PhD from the Massachusetts Institute of Technology, his MPhil from Nuffield College, University of Oxford, and his BA from Trinity College, University of Toronto.

Mr. Rose has published over 150 papers, including 80 articles in refereed economics journals such as *American Economic Review, Quarterly Journal of Economics, Review of Economic Studies*, and *Journal of Finance*. His research addresses issues in international trade, finance, and macroeconomics, and has received more than 25,000 citations. His teaching is in the areas of international macroeconomics; he has won two teaching awards. Mr. Rose was the managing editor of *Journal of International Economics* from 1995 to 2001, and the founding director of the Clausen Center for International Business and Policy at Haas and the Risk Management Institute at the National University of Singapore. He has organized over 40 academic conferences. Mr. Rose is interested in the theory and practice of economic policy, and most of his work is applied and driven by real world international phenomena. A citizen of three countries, he has worked on six continents and at a number of international economic agencies, including the International Monetary Fund, the World Bank, and the Asian Development Bank.

Hyun Song Shin, Professor
Princeton University

Hyun Song Shin is the Hughes-Rogers Professor of Economics at Princeton University. His research interests cover financial institutions, risk, and financial stability issues, topics on which he has published widely both in academic and policy outlets. He is the author of *Risk and Liquidity* the 2008 Clarendon Lectures in Finance (Oxford University Press, 2010), and coauthored the 2009 Geneva Report on *Fundamental Principles of Financial Regulation*. Before moving to Princeton in 2006, he was based in the United Kingdom, holding academic positions at Oxford and the London School of Economics.

Mr. Shin is a Korean national. In 2010, he was on leave from Princeton, serving as the Senior Adviser on the International Economy to President Lee Myung-bak. During his tenure as Presidential Adviser, Korea designed and began implementation of its macroprudential policies. Mr. Shin will rejoin the policy world in 2014. He was recently appointed as Economic Adviser and Head of Research at the Bank for International Settlements for a term beginning in May 2014. He is a Fellow of the Econometric Society and of the British Academy.

Alan M. Taylor, Professor
University of California, Davis

Alan M. Taylor is a Professor of Economics and Finance at the University of California, Davis. He is also a research associate of the National Bureau of Economic Research in Cambridge, MA, and a Research Fellow of the Center for Economic Policy Research in London. His research interests span international trade, finance, macroeconomics, and economic history. He read mathematics at King's College, Cambridge, and graduated with a PhD in Economics from Harvard University.

Mr. Taylor's publications include numerous articles in a range of economics journals including the *American Economic Review, Econometrica, Quarterly Journal of Economics, Journal of International Economics*, and *Journal of Economic History*; he has edited volumes and the books *Global Capital Markets: Integration, Crisis and Growth* (with Maurice Obstfeld, Cambridge University Press, 2004), and *Straining at the Anchor: The Argentine Currency Board and the Search for Macroeconomic Stability, 1880–1935* (with Gerardo della Paolera, University of Chicago Press, 2001); and essays in the *Financial Times, Foreign Affairs*, Reuters, and Voxeu.org. In 2004 he was awarded a John Simon Guggenheim Memorial Fellowship. In 2009–10 he was named a Houblon-Norman/George Fellow at the Bank of England.

John C. Williams, President and Chief Executive Officer
Federal Reserve Bank of San Francisco

John C. Williams took office as President and Chief Executive Officer of the Federal Reserve Bank of San Francisco on March 1, 2011. In this role, he serves on the Federal Open Market Committee, bringing the perspective of the Federal Reserve's Twelfth District to monetary policy discussions in Washington. Mr. Williams previously served as Executive Vice President and Director of Research for the San Francisco Fed. He began his career in 1994 as an economist at the Board of Governors of the Federal Reserve System.

Mr. Williams's research focuses on topics including monetary policy under uncertainty, innovation, productivity, and business cycles. He has collaborated with economists around the globe to examine economic and policy issues from different perspectives, and has published numerous articles in leading research journals. Mr. Williams currently serves as the managing editor of the *International Journal of Central Banking*. Previously, he served as associate editor of the *American Economic Review*. Additionally, he served as senior economist at the White House Council of Economic Advisers and as a lecturer at Stanford University's Graduate School of Business. He earned a PhD in Economics at Stanford University, a Master of Science with distinction in Economics from the London School of Economics in 1989, and an AB degree with high distinction from the University of California at Berkeley in 1984.

Martin Wolf, Associate Editor and Chief Economics Commentator
Financial Times, London

Martin Wolf is Chief Economics Commentator at the *Financial Times* in London. He was awarded the Commander of the British Empire in 2000 for services to financial journalism. Mr. Wolf is an Honorary Fellow of Nuffield College, Oxford, Honorary Fellow of Corpus Christi College, Oxford University, an Honorary Fellow of the Oxford Institute for Economic Policy (Oxonia), and an Honorary Professor at the University of Nottingham.

Mr. Wolf has been a forum Fellow at the annual meeting of the World Economic Forum in Davos since 1999 and a member of its International Media Council since 2006. He was made a Doctor of Letters, honoris causa, by Nottingham University in July 2006. He was made a Doctor of Science (Economics) of London University, honoris causa, by the London School of Economics in December 2006. He was a member of the U.K. government's Independent Commission on Banking in 2010–11. Mr. Wolf's most recent books are *Why Globalization Works* (Yale University Press, 2004) and *Fixing Global Finance* (Johns Hopkins University Press, 2008).

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