In the Face of Spillovers: Prudential Policies in Emerging Economies*

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Abstract

We examine whether emerging market prudential policies help to reduce the macro-financial spillover effects of US monetary policy. We find that emerging markets with tighter prudential policies face significantly smaller, and less negative, spillovers to total credit from US monetary policy tightening shocks. Reserve requirements and, to a lesser extent, loan-to-value ratio limits appear to be particularly effective prudential measures at mitigating the spillover effects of US monetary policy. Consistent with the bank-lending channel, our findings indicate that domestic prudential policies can dampen emerging markets' exposure to US monetary policy and the associated global financial cycle, even when accounting for capital controls. These findings suggest they may be a useful tool in the face of international macroeconomic policy trade-offs.

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Key Words: International spillovers; Local projections; Policy Interactions; Monetary policy; Prudential policy.

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1 Introduction

Prudential policies—both micro and macro in nature—have been widely used to address financial stability concerns since the 2007-09 global financial crisis. Yet their effects are still debated. On the one hand, they are seen to contain risks and contribute to macroeconomic stability (Galati and Moessner, 2018); on the other, some have suggested they could harm macroeconomic activity (Sánchez and Röhn, 2016). We contribute to this debate from a novel angle, assessing the extent to which domestic prudential policies interact with the spillovers from foreign shocks.

This paper focuses specifically on prudential policies in emerging markets (EMs). EMs are often disproportionately hit by spillovers from shocks emanating from advanced economies (Bernanke, 2017) and their 'spillbacks' are a growing concern for developed countries (Carney, 2019). The sensitivity of EMs to foreign shocks is, in part, related to the well-documented 'global financial cycle' (Passari and Rey, 2015), characterised by a high degree of cross-border co-movement in capital flows, asset prices and credit growth in the world economy. The influence of US monetary policy on the global cycle (Miranda-Agrippino and Rey, 2019), alongside the dominant role of the US dollar in global trade and financial markets (Boz, Casas, Díez, Gopinath, Gourinchas, and Plagborg-Møller, 2019; Maggiori, Neiman, and Schreger, 2018), ensures that US monetary policy is a timeless concern amongst policymakers in EMs.

In the face of the global financial cycle, Rey (2015) argues that policymakers face a dilemma: domestic policymakers can pursue independent monetary policy if they have recourse to capital controls or prudential policies. In this paper, we assess whether domestic prudential policies are an effective tool for helping to offset the spillover effects of US monetary policy and, in turn, dampen the cyclical macro-financial fluctuations associated with it. In doing so, our analysis provides novel empirical evidence on the dynamic interactions between monetary and prudential policy in a global context, that complements existing analyses of the direct and indirect effects of prudential policies on macro-financial stability.

Specifically, we ask three questions. First, to what extent do EM prudential policies offset the spillover effects of US monetary policy? Second, which specific prudential policies are most effective at doing so? Third, to what extent do other factors in an EM influence the size of the interaction between domestic prudential policy and US monetary policy? Our questions are at the heart of contemporary policy debates, contributing to a broader assessment of the optimal policy mix for EMs facing external pressures. For instance, while Blanchard (2017) suggests that EMs are equipped with policy instruments to deal with foreign shocks, Rajan (2015) states that "macroprudential policies have little traction" against cross-border capital flows. Our paper contributes to this debate from a novel, but specific, angle, focusing exclusively on the extent to which prudential policies help to shield EMs against foreign monetary policy shocks, and accounts for other factors—such as capital controls—that could also reduce spillovers.

While beyond the scope of this paper, a complete assessment of the appropriate EM policy mix should account for all the costs and benefits these policies could have—for instance, their direct and indirect effects on domestic real activity and financial stability.

Using a panel dataset summarising prudential policy actions in EMs, we show that the macro-financial spillovers from US monetary policy shocks differ depending on the prudential policies enacted by EMs. In particular, we find that an EM with tighter prudential policies faces significantly smaller reductions in total credit and bank credit—common indicators of financial (in)stability—following a US monetary policy tightening shock. A $+1$pp exogenous tightening of US monetary policy leads to around a 5.7% fall in total credit and a 3.8% fall in bank credit in EMs on average, after around 15 months, for countries with no prudential policy actions in place. However, an EM with an additional (a one standard deviation) prudential policy tightening action faces a substantially smaller spillover, seeing reductions in total and bank credit of around 4.2% (2%) and 2.9% (1.6%), respectively. These results indicate that an additional (a one standard deviation) prudential policy tightening can reduce the monetary policy spillover by around a quarter (over a half)—an economically significant amount—implying that national prudential policies help to offset some of the spillovers of US monetary policy to domestic lending. This is consistent with the bank-lending channel logic developed in Kashyap and Stein (2000), albeit in an international context. By partially insulating an EM's financial sector against global financial market moves and limiting the cyclicality of leverage, tighter EM prudential policies leave financial intermediaries better placed to absorb potential losses in the face of a surprise US monetary policy tightening, resulting in a smaller reduction in lending to the domestic non-financial sector than would otherwise have been the case without prudential policy tools in place.

Our empirical study builds on a long-standing literature quantifying the spillover effects of US monetary policy to EMs. We extend a local projection-based empirical setup for monetary policy spillovers—used in, for example, Banerjee, Devereux, and Lombardo (2016) and Iacoviello and Navarro (2019)—to study the dynamic interactions of US monetary policy with prudential policy in EMs. To attain unbiased estimates of coefficients of interest, we identify exogenous US monetary policy shocks via external instruments (Gürkaynak, Sack, and Swanson, 2005; Gertler and Karadi, 2015). To measure prudential policies, we use data spanning 29 EMs from 2000:Q1 to 2017:Q4 summarising prudential policy actions from Cerutti, Correa, Fiorentino, and Segalla (2017b). The dataset covers changes in several widely used prudential tools, with both micro- and macro-prudential objectives, specifically: capital buffers, loan-to-value (LTV) ratio limits, reserve requirements, interbank exposure limits, and concentration limits. Although the dataset captures prudential policy actions within a given quarter, we cumulate actions—over a two-year period in our baseline specification, alongside alternatives in robustness analysis—to proxy the prudential policy actions relevant for monetary policy spillovers, accounting for transmission and activation lags, as well as the persistence of policies—an approach used in Bussière, Cao, de Haan, Hills, Lloyd, Meunier, Pedrono, Reinhardt, Sinha, Sowerbutts, and Styrin (2021a) and the references within. Importantly, our estimates for the interaction between US monetary policy spillovers and EM prudential policies are robust to the inclusion of potential competing explanations and economic mechanisms for the heterogeneous transmission of US monetary policy to EMs—including their degree of capital controls, exchange rate regime, housing market structure and measures of underlying country vulnerabilities. Our results are also robust to the use of, *inter alia*, alternative definitions of the prudential policy measure and a different monetary policy shock.

To assess the specific channels at play, we use the different prudential policy instruments in the Cerutti et al. (2017b) dataset to assess the differential impact of various prudential policies. In particular, we make a distinction between prudential instruments that 'dampen the cycle' and those that 'increase resilience'—a categorisation laid out in Borio (2010) and Claessens et al. (2013). We find that policies that dampen the cycle are those with significant interactions: reserve requirements and, to a lesser extent, LTV ratio limits are particularly effective at partially mitigating the spillover effects of US monetary policy shocks, thereby dampening a country's exposure to cyclical fluctuations driven by US monetary policy. LTV ratio limits most strongly help to offset the response of EM house prices to US monetary policy shocks, reflecting their primary application to real-estate transactions. Reserve requirements have a broad-based effect on credit quantities. This finding is consistent with the bank-lending channel: prudential policy tightening in an EM can mitigate the effects of US monetary policy tightening on EM lending by making otherwise less resilient banks more resilient to external shocks—for example, by ensuring they hold sufficient reserves in the face of funding-cost shocks.

We further explore the channels at play by studying the factors that influence the size of the interaction. Our analyses of these factors indicates that regional differences matter. Latin American economies—which are both close to the US geographically and are highly dollarised—have prudential policies that most strongly offset the spillovers from US monetary policy, although regional differences are not statistically significant. We also find that prudential policies—especially LTV ratio limits—are particularly effective at dampening cyclical fluctuations in EMs with higher home ownership shares, while differences in the interaction appear more limited across fixed and floating exchange rate regimes and countries with differential amounts of US dollar debt.

The remainder of the paper is structured as follows. Following a short literature review, Section 2 describes the empirical specification and data. Section 3 briefly presents evidence on US monetary policy spillovers to EMs, placing our main results in context, before discussing the interactions between US monetary policy and aggregate EM prudential policies. Section 4 discusses cross-border interactions for specific prudential policies. Section 5 assesses the economic determinants of the interaction, assessing the drivers of heterogeneity in policy interactions across countries. Section 6 concludes.

Related Literature Our work relates to three main strands of literature. First, and most substantively, we contribute to work studying the interaction between monetary and prudential policies. To date, much of this literature has focused on within-country effects, with theoretical (e.g Angelini, Neri, and Panetta, 2014; Chen and Columba, 2016) and empirical (e.g. Bruno, Shim, and Shin, 2017) contributions. But studies into cross-border policy interactions, such as this paper, are relatively scarce. Takáts and Temesvary (2021) is the most closely related paper to ours. Like us, they analyse policy interactions in a global context. However, their analysis is on the implications of monetary and macroprudential policy interactions for cross-border lending, focusing on major currency issuers. In contrast, we focus on the implications of cross-border policy interactions for domestic outcomes—closely linked to domestic welfare, for example, total credit and domestic house prices—and for EMs specifically—who, as explained earlier, are often disproportionately hit by spillovers from abroad (Bernanke, 2017). Additionally, we also explore the differential role of different prudential policies—rather than a single aggregate prudential measure. Nevertheless, our conclusions are complementary: we both find that, consistent with the bank-lending channel, prudential tightening can mitigate the effect of core-country monetary policy on lending.

In related work, Takáts and Temesvary (2019) also find that macroprudential measures applied in EMs prior to the 2013 'Taper Tantrum'—linked to US monetary policy—reduced volatility in cross-border bank lending flows. We extend these findings by studying the interactions between US monetary policy and EM prudential policies in the last two decades, rather than focusing on interactions arising from a specific monetary policy event such as the Taper Tantrum. Moreover, we show how these interactions influence aggregate credit in EMs, rather than funds with a specific cross-border origin. Thus, our results have generalisable implications for financial stability in EMs.

Our work also complements a recent International Bank Research Network initiative (see Bussière et al., 2021a, and the references within) which assesses cross-border policy interactions. Similar to these papers, we focus on prudential policy changes in advance of monetary policy shocks in our baseline setup, employing the same cumulation strategy for our prudential proxy. Relative to this body of recent work, our paper contributes an EM-specific angle, demonstrating that prudential policies can contribute to financial stability in EMs in the face of foreign shocks. In addition, we examine how the cross-border interaction between prudential and monetary policies depends on country characteristics, building on the work of Beirne and Friedrich (2017), who examine the effectiveness of macroprudential policies as a function of banking sector characteristics.

Second, our work contributes the extensive literature studying the spillover effects of advanced economy shocks to EMs, much of which has focused on the effects of US monetary policy from both empirical (e.g. Banerjee et al., 2016; Rey, 2016; Bräuning and Ivashina, 2018) and theoretical (e.g. Akinci and Queralto, 2018) standpoints. A common theme in this literature is that the spillovers of US monetary policy shocks can have dynamic effects, typically taking some quarters to affect economic activity in EMs. While a number of papers have assessed factors contributing to cross-country differences in US monetary spillovers to EMs (e.g. Iacoviello and Navarro, 2019), we specifically focus on the role of prudential policies. And, in line with evidence that such spillovers have lagged effects, we show how a local projections empirical methodology—used to estimate monetary policy spillovers in Banerjee et al. (2016) and Iacoviello and Navarro (2019)—can be adapted to study the dynamic interaction of prudential policies with these spillovers.

Third, our paper is related to a literature studying the direct effects of prudential policies. Recent work has sought to assess how specific prudential policy tools can affect domestic credit quantities (Alam, Alter, Eiseman, Gelos, Kang, Narita, Nier, and Wang, 2019), while a sizable literature has amassed studying the international spillover effects of prudential policies (e.g. Aiyar, Calomiris, Hooley, Korniyenko, and Wieladek, 2014; Berrospide, Correa, Goldberg, and Niepmann, 2017; Buch and Goldberg, 2017; Hills, Reinhardt, Sowerbutts, and Wieladek, 2017) and their potential unintended consequences (e.g. Ahnert, Forbes, Friedrich, and Reinhardt, 2018). We contribute to this literature by emphasising the interaction effects that specific prudential policy tools can have, over and above their direct effects. In particular, we show that specific prudential policy tools, like LTV ratio limits and reserve requirements, can have a particular role in dampening cyclical fluctuations associated with US monetary policy, itself a key driver of the global financial cycle (Miranda-Agrippino and Rey, 2019).

2 Empirical Specification

We use the local projection methodology (Jordà, 2005) to estimate the dynamic interaction of US monetary policy shocks with EM prudential policies. For our purposes, there are two major advantages to adopting this setup relative to vector autoregressive methods. First, as we demonstrate, a standard local projections spillover framework can be parsimoniously extended to account for prudential policy interactions. Second, compared to alternative empirical specifications, the local projection setup is more robust to misspecification, a pertinent concern when studying the effect of various heterogeneous prudential policy instruments. Moreover, compared to dynamic panel regression setups (e.g. Bussière, Hills, Lloyd, Meunier, Pedrono, Reinhardt, and Sowerbutts, 2021b) the local projection methodology is better suited to capturing the dynamic interaction effects by directly regressing forward lags of the variable of interest on contemporaneous (time-t) policy actions.

Monetary Policy Spillovers To provide context to our analysis of policy interactions, we first document the spillovers of US monetary policy to EMs. We model the impact of a US monetary policy shock in quarter t, $MP_t^{\$}$, on the variable of interest $y_{i,t+h}$ in country i at quarter $t+h$

using the following local projection specification:

$$y_{i,t+h} - y_{i,t-1} = \alpha^h + \beta^h_{mp} MP^{\$}_t + \boldsymbol{\gamma}^{h\prime}\mathbf{X}_{i,t-1} + \boldsymbol{\theta}^{h\prime}\mathbf{G}_{t-1} + f^h_i + \varepsilon_{i,t+h} \tag{1}$$

for $h = 0, 1, ..., H$. $\mathbf{X}_{i,t-1}$ is a $K \times 1$ vector of control variables known prior to the US monetary policy shock, with associated coefficients $\boldsymbol{\gamma}^h$. Country fixed effects f^h_i capture potentially confounding factors that are specific to countries, but fixed over time. Because the US monetary policy shock $MP^{\$}_t$ is the same for all countries, we cannot include time fixed effects in equation (1) as these would absorb all variation in the explanatory variable of interest. As a consequence, we account for variables summarising the global cycle in $\mathbf{G}_{t-1}$, a $J \times 1$ vector with associated coefficients $\boldsymbol{\theta}^h$. β^h_{mp} then measures the average effect of a period-t US monetary policy shock on $y_{i,t+h}$ at $t+h$.

For the majority of the paper, we focus on total credit and bank credit as our key dependent variables, given their close link with overall financial and banking-sector stability. In our data, total credit is defined as lending to the domestic private non-financial sector in a given EM, measured as total claims from domestic banks, other domestic financial corporations, non-financial corporations and non-residents. Bank credit is defined as lending to the domestic private non-financial sector by domestic banks. A large body of work has highlighted a role for credit growth as a leading indicator of financial and banking crises (e.g. Schularick and Taylor, 2012), motivating our focus on these variables. In addition, we use data on house prices, as a number of prudential policies—such as LTV ratio limits—have been applied with a focus on housing sector risks.[1]

In our benchmark regression, we include two lags of output growth, inflation and the dependent variable (quarterly changes) in the set of country-varying controls $\mathbf{X}_{i,t-1}$ to capture the prevailing macroeconomic state ahead of a US monetary policy innovation. The global controls $\mathbf{G}_{t-1}$ include two lags of US output growth, VIX and past US monetary policy shocks, reflecting global economic and financial conditions. Our macro-financial dataset spans 29 EMs, reflecting country coverage in the prudential policy actions dataset.

Interactions of Spillovers with Receiving-Country Prudential Policy The monetary policy spillover regression marks the point of departure for our empirical specification. To analyse how prudential policies in EMs interact with spillovers from US monetary policy shocks we adapt equation (1) to account for country-i prudential policy $Pru_{i,t}$ using the following setup:

$$\begin{aligned} y_{i,t+h} - y_{i,t-1} =& \alpha^h + \delta^h \left(MP^{\$}_t \times Pru_{i,t-1} \right) + \beta^h_{pru} Pru_{i,t-1} + \boldsymbol{\gamma}^{h\prime}\mathbf{X}_{i,t-1} \\ &+ \boldsymbol{\theta}^{h\prime}\boldsymbol{\Gamma}_{i,t-1} + \boldsymbol{\vartheta}^{h\prime}\left(MP^{\$}_t \times \boldsymbol{\Gamma}_{i,t-1} \right) + f^h_i + f^h_t + \varepsilon_{i,t+h} \end{aligned} \tag{2}$$

[1] In Appendix B.2, we also report results using non-bank credit (i.e. credit issued by other domestic financial corporations, non-financial corporations and non-residents) as our dependent variable.

where $Pru_{i,t-1}$ represents an indicator of prudential actions, taking positive values for a (net) tightening and negative for a (net) loosening. We use a lagged indicator of prudential policy to prevent the possibility that our estimates capture a (potentially endogenous) response of EM prudential policy to a US monetary policy shock, or simultaneity of economic conditions and domestic prudential policy. Thus, the interaction coefficients $\{\delta^h\}_{h=0}^H$ capture the interaction between a time-t US monetary policy shock and EM prudential policy, set in advance of a US monetary policy innovation, at time $t-1$ across horizons h.

In comparison to equation (1), we include time fixed effects f_t^h to account for potentially confounding factors that are the same for all countries in a given time period—for example, the state of the global financial cycle. Because the time fixed effects f_t^h account for all observed *and* unobserved global factors that vary over time, we exclude $\mathbf{G}_{t-1}$ from equation (2).[2]

Our controls $\mathbf{X}_{i,t-1}$, which vary by time *and* country, are the same as in equation (1). In robustness analyses, we extend the controls to include other factors that could plausibly interact with US monetary policy spillovers, aside from prudential policies, in order to rule out competing hypotheses. To account for them, we define $\mathbf{\Gamma}_{i,t-1}$ as a (set of), possibly time-varying, country characteristic(s) that could potentially influence the size of US monetary policy spillovers to EMs—such as a country's capital controls. We then include the interaction of this variable with the contemporaneous US monetary policy shock, $MP_t^{\$} \times \mathbf{\Gamma}_{i,t-1}$, in addition to its lagged level $\mathbf{\Gamma}_{i,t-1}$ in our set of time- and country-varying controls $\mathbf{X}_{i,t-1}$.[3]

The time fixed effects f_t^h also absorb all variation in $MP_t^{\$}$, explaining why this is not in equation (2). Nevertheless, the *sign* of coefficient estimates from equation (1) can help to interpret results from equation (2).[4] The coefficient of interest in the latter is δ^h. If, for a given dependent variable $y_{i,t+h}$, monetary policy spillovers are negative $\hat{\beta}_{mp}^h < 0$, then a positive interaction coefficient $\hat{\delta}^h > 0$ implies that tighter prudential policy helps to *offset* some of the negative spillover effects of a US monetary policy tightening. In contrast, if the interaction coefficient is negative $\hat{\delta}^h < 0$, tighter prudential policy *does not mitigate* the negative spillover effects of tighter US monetary policy.[5] The sequence $\{\hat{\delta}^h\}_{h=0}^H$ can thus be interpreted as the average interactions associated with a time-t US monetary policy impulse.

[2]Because the time fixed effects f_t^h capture all observed and unobserved time-varying factors, equation (2) is our preferred specification for statistical inference. However, to illustrate the *economic significance* of our findings and compare monetary policy spillovers with prudential policy interactions—i.e. $\hat{\beta}_{mp}^h$ and $\hat{\delta}^h$—we also estimate a hybrid specification of equations (1) and (2) that includes the monetary policy term $MP_t^{\$}$, its interaction with lagged prudential policy $MP_t^{\$} \times Pru_{i,t-1}$ and lagged observed global factors $\mathbf{G}_{t-1}$, but excludes time fixed effects f_t^h to avoid absorbing variation in $MP_t^{\$}$.

[3]In some cases, we classify the variable using an indicator variable. To do this, we define a (potentially) time-varying indicator variable $\mathbb{1}_{g,i,t-1} \equiv \mathbb{1}\left[\mathbf{\Gamma}_{i,t-1} \in g\right]$ where $\mathbb{1}$ is an indicator function equal to 1 if the country characteristic falls into a particular 'bin' of the distribution, a country's group $g = 1, ..., G$.

[4]A direct quantitative comparison of coefficients from equations (1) and (2) is not possible, because the former specification excludes time fixed effects f_t^h while the latter includes them.

[5]The reverse is true if the US monetary policy spillover is positive $\hat{\beta}_{mp}^h > 0$. Then, a negative interaction coefficient $\hat{\delta}^h < 0$ reflects an offsetting policy interaction, and a positive coefficient $\hat{\delta}^h > 0$ a non-offsetting one.

Assessing the Determinants of Interactions Regression (2) captures the average interaction between US monetary policy and EM prudential policy, but suppresses potential cross-country heterogeneity in the interaction by imposing $\delta_i^h = \delta^h$ for all i. To account for this in an economically meaningful way, we estimate an extended local projection regression. To do so, we define $\mathbf{Z}_{i,t-1}$ as a (set of) country characteristic(s) that could potentially influence the size of the interaction, such as the exchange rate regime or home ownership share in a country. We define a (potentially) time-varying indicator variable $\mathbb{1}_{g,i,t-1} \equiv \mathbb{1}\left[\mathbf{Z}_{i,t-1} \in g\right]$ where $\mathbb{1}$ is an indicator function taking a value of 1 if the country characteristic falls into a particular 'bin' of the distribution, which we denote as a country's group $g = 1, ..., G$. The general form for this extended specification is:

$$
\begin{aligned}
y_{i,t+h} - y_{i,t-1} = &\sum_{g=1}^{G} \alpha_g^h \cdot \mathbb{1}_{g,i,t-1} + \sum_{g=1}^{G} \delta_g^h \left(MP_t^{\$} \times Pru_{i,t-1} \times \mathbb{1}_{g,i,t-1} \right) \\
&+ \sum_{g=1}^{G} \tilde{\beta}_{mp,g}^h \left(MP_t^{\$} \times \mathbb{1}_{g,t-1} \right) + \sum_{g=1}^{G} \beta_{pru,g}^h \left(Pru_{i,t-1} \times \mathbb{1}_{g,i,t-1} \right) \\
&+ \gamma^{h\prime} \mathbf{X}_{i,t-1} + f_i^h + f_t^h + \varepsilon_{i,t+h}
\end{aligned} \tag{3}
$$

By accounting for country characteristics in this way, we estimate separate interaction coefficients δ_g^h for each group $g = 1, ..., G$, non-parametrically capturing potential cross-country heterogeneity in policy interactions.[6]

Importantly, equation (3) includes a term for the interaction of the monetary policy shock with the indicator variable $\left(MP_t^{\$} \times \mathbb{1}_{g,i,t-1} \right)$, with associated coefficient $\tilde{\beta}_{mp,g}^h$, reflecting the fact that the product of the monetary policy innovation and the indicator variable can vary along both country and time-dimensions, so is not fully absorbed by time fixed effects.

In all regressions, we use Driscoll and Kraay (1998) standard errors to account for potential cross-sectional and temporal dependence in inference, and impulse responses are reported out to a two-year horizon—i.e. $H = 8$.

2.1 Prudential Policy Data

We use the prudential policy actions dataset of Cerutti et al. (2017b), constructed for the cross-country International Banking Research Network project on cross-border spillovers of prudential policy (Buch and Goldberg, 2017).[7] The dataset spans 64 countries, including the same 29 EMs in our panel of macro-financial data.[8] The prudential policy data is quarterly, from

[6]Because $\mathbb{1}_{g,i,t-1}$ is time and country-varying, the product of the indicator with monetary and prudential policy measures also varies across time and countries, ensuring the time fixed effect f_t^h remains well-defined in this specification.

[7]In Section 3.4, we show that our headline results are robust to the use of an alternative macroprudential policy dataset, the IMF Integrated Macroprudential Policy (iMaPP) database (Alam et al., 2019).

[8]The 29 EMs in our dataset span 11 Asian economies, 8 in Europe, 7 in Latin America and 3 in Africa.

2000:Q1 to 2017:Q4,[9] and covers five types of prudential policy instruments with both micro- and macro-prudential objectives: capital requirements; interbank exposure limits; concentration limits; LTV ratio limits; and reserve requirements. The dataset further breaks down some of these categories, differentiating general capital requirements—which predominantly reflect convergence to Basel Accords—from sectoral capital buffers—such as risk weights on specific bank exposures—as well as the currency breakdown of reserve requirements. The availability of prudential policy data defines the beginning of our sample period. With forward lags in our local projection setup, we use macro-financial data up to 2018:Q2 for our dependent variables.

The Cerutti et al. (2017b) dataset has been constructed from a range of sources, and observations in the database were reviewed by staff from national central banks.[10] The raw dataset measures *changes* in prudential policy instruments within a quarter, assigning a value of $+1$ to a given prudential policy if it was tightened in a specific quarter, a value of -1 if it was loosened, and 0 if no change occurred. For some policy instruments, information about the *intensity* of the policy change is retained. For example, for sectoral capital requirements and reserve requirements, indices ranging from -3 to 5 reflect the intensity of policy changes.

To suit our study, we manipulate the raw dataset in the following ways. First, in our baseline regressions, we sum prudential policy actions over a number of quarters. This reflects the possibility that changes in prudential polices in a single quarter may not solely influence the spillovers from US monetary policy shocks once accounting for transmission lags, persistence and the level of prudential policies that could remain in place for some time. In our baseline formulation we sum actions over two years such that the prudential policy measure at time $t-1$, $Pru_{i,t-1}$, includes information on all prudential policy changes from $t-8$ to $t-1$, inclusive. The choice of a two-year summation period in our baseline specification balances a compromise.[11] On the one hand, we need a long enough summation period to capture sufficient variation in prudential policy measures over time, as well as proxy aspects of cross-country differences in their level. On the other hand, we need to ensure that the summation period is not too long such that it suppresses variation in the prudential measure because of policy reversals over time. Nevertheless, we discuss alternative assumptions around this summation in Section 3.4. In particular, we consider alternative cumulation periods: a shorter one-year summation period, which Alam et al. (2019) use in their study into the direct effects of macroprudential policy; and a longer summation period starting from the beginning of the sample, which Takáts and Temesvary (2019) use in their study. We also report results for an extended variant of regression (2) using lags of uncumulated prudential policy actions to address concerns that our summed measures may generated serial correlation in regressors.

[9] The Cerutti et al. (2017b) dataset has recently been extended by its authors, from a 2014:Q4 end date to 2017:Q4.

[10] The database builds on existing information in Cerutti, Claessens, and Laeven (2017a) which covers a smaller set of macroprudential policy instruments in 125 countries, as well as secondary sources compiled by Lim et al. (2011), Akinci and Olmstead-Rumsey (2015), and Reinhardt and Sowerbutts (2015).

[11] Bussière et al. (2021a), and the references within, all use a two-year summation period for prudential policy actions in their analyses of policy interactions using bank-level data.

Second, we construct measures of *aggregate* prudential policy, by summing cumulated measures of different instruments. Our baseline measure of aggregate prudential policy actions includes all prudential policy instruments in the Cerutti et al. (2017b) dataset, with the exception of general capital requirements. Like Takáts and Temesvary (2019) and Takáts and Temesvary (2021), we primarily exclude general capital requirements because they reflect micro-prudential policy adjustment, such that the remaining instruments in the proxy more closely match macroprudential measures.[12] In addition, Takáts and Temesvary (2019) and Takáts and Temesvary (2021) note that these general capital requirements largely reflect the adoption of the Basel III regime, an internationally harmonised and broadly anticipated move resulting in limited cross-country variation useful for the estimation of interactions using equation (2) in the specific series. However, our aggregated measure still includes sectoral capital buffers—for instance those levied on real estate or consumer credit—as these buffers are predominantly macroprudential tools.

Our aggregate prudential policy measure includes reserve requirements, including those levied on both domestic and foreign currency-denominated deposits. Although reserve requirements have been used as instruments to conduct monetary policy in some jurisdictions, Cordella, Federico, Vegh, and Vuletin (2014) note that these policies have predominantly recently been used as countercyclical macroprudential tools in EMs. Cerutti et al. (2017b) account for this in the construction of their dataset, ensuring that the reserve requirements they capture are used to satisfy prudential objectives within a country, warranting their inclusion in our aggregate prudential policy proxy.

In Section 4, we also present analyses using cumulated measures of specific prudential policies, to isolate their differential impacts across dependent variables, and assess the relative importance of different economic mechanisms with reference to the existing literature. This marks a major departure from Takáts and Temesvary (2021), who focus on aggregate prudential policy actions only.

Table 1 presents summary statistics for our two-year cumulated prudential policy proxies, constructed by pooling observations across the 29 EMs in our dataset and over the full sample.[13] Over the sample, all policy proxies—aggregate and specific—were, on average and on net, tightened. Nevertheless, all measures take a range of positive and negative values, with the aggregate prudential policy measure varying from -9 to 11 with a standard deviation of around 2.5. In our sample of 29 EMs, reserve requirements and, to a lesser extent, LTV ratio limits are the most actively used measures, with the widest range and standard deviation.

[12]We include these minimum general capital requirements in our robustness checks in Section 3.4, and show that this exclusion does not materially driver our results for the aggregate prudential policy measure.

[13]See Appendix A.1 for additional details and summary statistics on the prudential policy dataset.

Table 1: Summary statistics for prudential policy proxies constructed by cumulating actions over a two-year period

Prudential Policy Measure	# Obs.	$\overline{Pru_{i,t}}$	$\sigma\left(Pru_{i,t}\right)$	$\min\left(Pru_{i,t}\right)$	$\max\left(Pru_{i,t}\right)$
Aggregate Proxy	1885	0.255	2.517	−9	11
Specific Prudential Instruments					
Reserve Requirements	1885	0.048	2.179	−9	11
LTV Ratio Limits	639	0.078	0.834	−3	5
Sectoral Capital Buffers	1885	0.100	0.675	−3	4
Concentration Ratio Limits	1272	0.078	0.371	−1	2
Interbank Exposure Limits	611	0.083	0.283	−1	1

Note: Statistics constructed by pooling observations across the 29 EMs and over full sample period.

2.2 Monetary Policy Shocks

A key concern for our analysis is that our measure of US monetary policy $MP_t^{\$}$ is exogenous in order to attain unbiased estimates of the parameters of interest. This prevents us from using raw measures of US interest (or shadow) rates to capture changes in US monetary policy. Of particular concern in our setting are potentially omitted factors, such as a global financial moves, that could simultaneously affect the US monetary policy stance as well as macro-financial outcomes in EMs, especially if they have heterogeneous effects across EMs.

Drawing on an extensive literature, we identify monetary policy shocks with the widely used external instruments VAR approach of Mertens and Ravn (2013) and Stock and Watson (2018), applied to US monetary policy by Gertler and Karadi (2015). Relative to Gertler and Karadi (2015), we make one change to our VAR specification: estimating it with data up to the end of 2018 (instead of 2012).[14] Like Gertler and Karadi (2015), our VAR consists of four monthly frequency US variables: industrial production, the consumer price index, the 1-year zero-coupon government bond yield, and the excess bond premium (Gilchrist and Zakrajsek, 2012). We estimate the model with 12 lags of monthly variables, using monthly data from 1979 to 2018. We construct quarterly monetary policy shocks from the monthly VAR, by cumulating monthly shocks within the quarter. To identify a monetary policy shock, we use high-frequency monetary policy surprise measures from Gürkaynak et al. (2005)—changes in monetary policy expectations in a short time window (30 minutes) around Federal Open Market Committee (FOMC) announcements—as instruments for the reduced-form monetary policy innovation. The key identifying assumption is that no other potentially confounding events, which could simultaneously drive private sector behavior and the monetary policy decision, can occur within the short time window around the FOMC announcements. Despite the sample extension, our instrument—changes in the three-month-ahead federal funds futures rate in 30-minute windows around FOMC announcements—continues to pass tests for instrument validity, with a first-stage F-statistic in excess of 10.

[14] Additional detail on our monetary policy shocks is provided in Appendix A.2.

As Gertler and Karadi (2015) note, the baseline policy indicator in the VAR—the one-year US government bond yield—remained responsive to economic news even when the short-term policy rate reached the zero-lower bound (Swanson and Williams, 2014), indicating some degree of central bank leverage over this instrument. Nevertheless, to assuage concerns that the lower bound is not adequately captured, we report additional robustness exercises in Section 3.4 using a monetary policy shock derived from shadow interest rates by Iacoviello and Navarro (2019).[15]

3 Monetary Policy Spillovers and Prudential Policy Interactions

In this section, we describe our main empirical results. We first describe the spillovers of US monetary policy to EMs. We then assess the extent to which these spillovers interact with EM prudential policy, where we aggregate different prudential policy types. We discuss the economic significance of these results, as well as robustness exercises.

3.1 Monetary Policy Spillovers

To contextualise our estimates of prudential policy interactions, we estimate the spillover effects of US monetary policy to EMs by estimating equation (1). Panels A and B of Figure 1 present estimates of the spillover coefficient for (log) total credit and bank credit in EMs. Columns (1) and (4) of Table 2 contain the corresponding point estimates and standard errors at each horizon for the respective variables. The coefficient estimates can be interpreted as average impulse responses to a $+1$pp US monetary policy tightening shock.

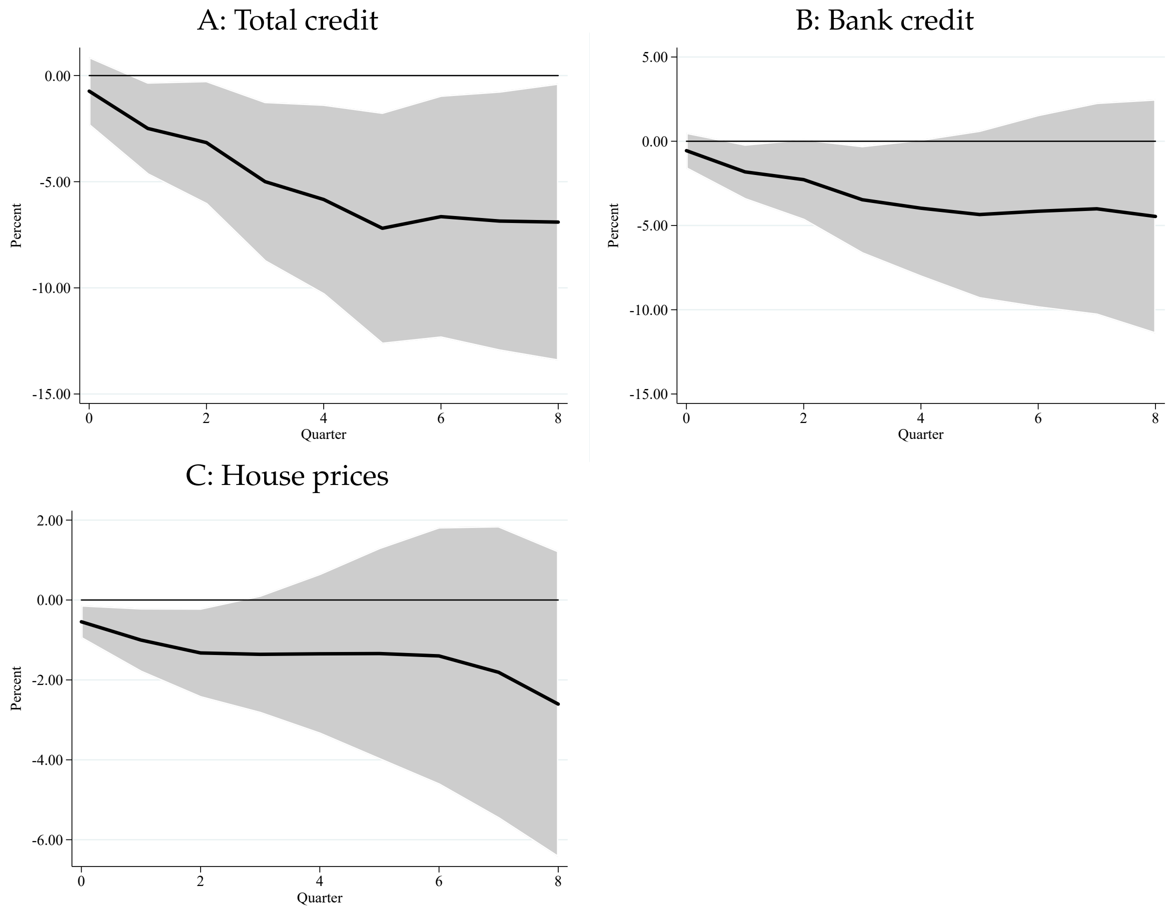
In line with a large body of literature and consistent with the bank-lending channel (Kashyap and Stein, 2000) in an international context, our results show that a US monetary policy tightening is associated with a financial tightening abroad. Total credit and bank credit fall significantly in EMs within two years of a US monetary policy shock. As well as being statistically significant, the spillover is economically significant: following a $+1$pp US monetary policy tightening shock, total credit in EMs falls by, on average, 7% in the two years after the shock. The corresponding peak for bank credit is around 4.4%.

We present corresponding spillover estimates for EM non-bank credit in Appendix B.2. Although the point estimates for these average spillover coefficients (Column 1, Table 8) are negative, they are insignificantly different from zero at all horizons.[16] This suggests that US monetary policy imparts more significant spillovers via bank credit than non-bank credit. In

[15] Iacoviello and Navarro (2019) derive this shock using US shadow interest rates from Wu and Xia (2016). Unlike our shock, this is not derived using high-frequency identification methods. Instead the shocks are defined as the residual of a regression of the shadow rate on contemporaneous and lagged values of inflation, log US GDP, corporate spreads, log foreign GDP, the lagged federal funds rate and a quadratic time trend.

[16] This is also true in the hybrid regression (Column 2, Table 8, when spillovers and interactions are jointly estimated.

Figure 1: US monetary policy spillovers to total credit, bank credit and house prices in emerging markets



Notes: $\{\beta^h_{mp}\}^8_{h=0}$ estimates with (log) total credit (Panel A), bank credit (B) and house prices (C) for 29 emerging markets as dependent variable (regression (1)). The classification of emerging economies is from the IMF's World Economic Outlook. The gray shaded area denotes 90% confidence intervals around point estimates, constructed from Driscoll and Kraay (1998) standard errors.

view of the insignificant spillover estimate for non-bank credit, we do not place emphasis on the interaction coefficient in this paper. In cases where the interaction coefficient is statistically significant, it is difficult to argue that it is economically significant when the baseline spillover is itself statistically indistinguishable from zero.

Panel C of Figure 1 plots the spillover coefficient estimates for house prices. The point estimate for spillovers to EMs is negative at all horizons. A US monetary policy tightening is associated with a reduction in EM house prices. Although the point estimates for house prices are statistically insignificant at longer horizons, the wide confidence bands, in part, reflect heterogeneity across emerging markets. The heterogeneity in spillovers to emerging markets is well known. Iacoviello and Navarro (2019), for example, assess how the effects of higher US interest rates on EMs differ depending on an economy's exchange rate, trade openness and vulnerability. In this paper, we assess a policy-relevant dimension of US monetary policy spillover variation in EMs: prudential policy.

3.2 Aggregate Prudential Policy Interactions

We estimate equation (2) to assess how EM prudential policies interact with US monetary policy spillovers. In this section, we focus on our *aggregate* prudential policy measure, which includes all prudential policies in the Cerutti et al. (2017b) dataset with the exception of general capital requirements, thus capturing macroprudential policy actions. We consider *specific* prudential policies in the next section.

Panels A and B of Figure 2 present estimates of the sequence of interaction coefficients $\{\hat{\delta}^h\}_{h=0}^{8}$ when EM (log) total credit and bank credit are dependent variables, respectively.[17] The corresponding point estimates and standard errors are presented in columns (3) and (6) of Table 2.[18] The results show that the interaction coefficient is significantly positive around the 12 to 21-month ($h = 4$ to 7) horizon when total credit is the dependent variable. The point estimates of the coefficient for lending by domestic banks are positive at all horizons, significantly so at $h = 4$. The coefficients have the following interpretation: controlling for time fixed effects, an additional prudential policy tightening in an EM in advance of a $+1$pp US monetary policy tightening can, on average, reduce the total credit hit from the US monetary policy tightening by around 1.4pp over a 15-month horizon ($h = 5$).[19]

These results alone have important policy implications for EMs. In the face of spillovers from US monetary policy and the associated global financial cycle, our results indicate that EMs can rely on prudential policies to significantly reduce the extent to which US monetary policy drives cyclical fluctuations in credit conditions. Following an unexpected US monetary policy tightening, EMs with tighter prudential policy face smaller falls in total domestic lending. This is consistent with the bank-lending channel: the EM financial sector is better placed to absorb the adverse spillovers tighter US monetary policy when domestic prudential policy is tighter—for instance due to banks holding more reserves or capital against sector-specific exposures that can guard against losses emanating from the US shock.[20]

These findings have some parallels with those in Takáts and Temesvary (2021). However,

[17] We report two sets of interaction estimates in Figure 2: (a) estimates using our baseline macroprudential policy measure from the Cerutti et al. (2017b) dataset; and (b) estimates using a two-year cumulated measure of aggregate macroprudential policy actions from the IMF Macroprudential Policy database (Alam et al., 2019), which we explain in greater detail in Section 3.4. Doing so emphasises the robustness of our headline result.

[18] We report the interaction coefficient estimates for house prices in Appendix B.1. Using equation (2), the interaction coefficient is insignificant at all but the first-quarter horizon for house prices. However, as the Appendix demonstrates, when estimating the hybrid regression we do not find the interaction coefficient for aggregate prudential policy to be significant at any horizon. We return to house prices later in the paper, when considering prudential policies with a specific housing market focus.

[19] The corresponding interaction coefficient estimates for non-bank credit (Column 3, Table 8, Appendix B.2) are also significantly positive, and point estimates are more positive than for bank credit. In isolation, this might suggest that EM prudential policy actions can have positive effects on the whole domestic financial sector, over and above the banking sector alone. While we believe this result merits further research in future, we reiterate the insignificant spillover coefficient and further note that we do not find robustly significant interaction coefficients for non-bank credit. For example, using the alternative shadow-rate monetary policy shock series, interaction coefficient estimates are insignificantly different from zero at all horizons.

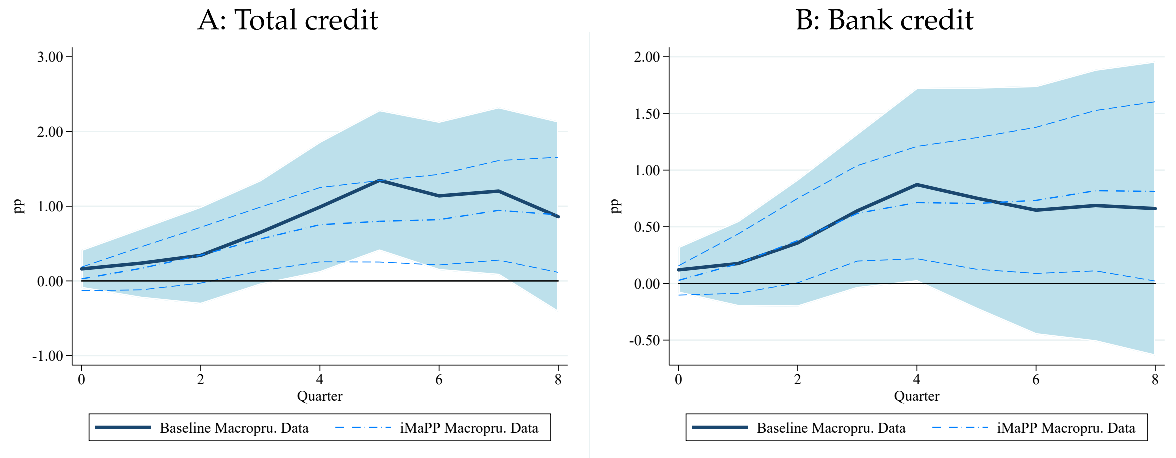
[20] In Appendix B.1, we report estimates for the regressions where we normalise total and bank with respect to GDP. Using the hybrid regression, we find the interaction coefficients to be significantly positive at some horizons.

Table 2: Estimated coefficients from regressions (1) and (2) for total credit and bank credit using aggregate prudential policy measure, which excludes capital requirements, in recipient emerging markets

	Total Credit			Bank Credit		
	(1) Mon. Pol. Spillover	(2) Hybrid	(3) Interaction	(4) Mon. Pol. Spillover	(5) Hybrid	(6) Interaction
$MP_t^{\$}$						
$h = 0$	-0.73	-0.40		-0.56	-0.67*	
	(0.82)	(0.35)		(0.54)	(0.38)	
$h = 1$	-2.49**	-2.33***		-1.82**	-2.31***	
	(1.11)	(0.79)		(0.83)	(0.64)	
$h = 2$	-3.16**	-2.70***		-2.28*	-2.69***	
	(1.48)	(1.02)		(1.22)	(0.80)	
$h = 3$	-5.00**	-4.36***		-3.47**	-3.74***	
	(1.92)	(1.41)		(1.63)	(0.99)	
$h = 4$	-5.84**	-4.91***		-3.97*	-3.84***	
	(2.28)	(1.65)		(2.07)	(1.24)	
$h = 5$	-7.19**	-5.71***		-4.35*	-3.53***	
	(2.78)	(1.94)		(2.54)	(1.33)	
$h = 6$	-6.65**	-4.34**		-4.15	-2.55*	
	(2.91)	(1.93)		(2.91)	(1.52)	
$h = 7$	-6.85**	-4.01*		-4.01	-1.76	
	(3.11)	(2.12)		(3.21)	(1.58)	
$h = 8$	-6.90**	-3.27		-4.46	-1.73	
	(3.33)	(2.26)		(3.54)	(1.79)	
$MP_t^{\$} \times Pru_{i,t-1}$						
$h = 0$		0.31**	0.16		0.24**	0.12
		(0.14)	(0.16)		(0.09)	(0.12)
$h = 1$		0.38	0.24		0.27	0.18
		(0.26)	(0.28)		(0.18)	(0.23)
$h = 2$		0.53	0.34		0.44	0.36
		(0.37)	(0.40)		(0.28)	(0.34)
$h = 3$		0.82*	0.65		0.67*	0.64
		(0.48)	(0.43)		(0.41)	(0.41)
$h = 4$		1.17**	0.99*		0.90**	0.87*
		(0.49)	(0.53)		(0.39)	(0.52)
$h = 5$		1.52***	1.35**		0.81**	0.75
		(0.57)	(0.57)		(0.41)	(0.59)
$h = 6$		1.33**	1.14*		0.67	0.65
		(0.61)	(0.60)		(0.47)	(0.67)
$h = 7$		1.26*	1.20*		0.60	0.69
		(0.71)	(0.68)		(0.59)	(0.73)
$h = 8$		0.96	0.86		0.61	0.66
		(0.79)	(0.78)		(0.68)	(0.79)
Country FE	YES	YES	YES	YES	YES	YES
Time FE	NO	NO	YES	NO	NO	YES

Notes: $\hat{\beta}^h$ and $\hat{\delta}^h$, for $h = 0, 1, \ldots 8$ coefficient estimates from regression (1) in columns (1) and (4), regression (2) in columns (3) and (6), and a hybrid of the two in columns (2) and (5) for (log) total credit and bank credit, respectively. *, ** and *** denote statistically significant coefficient estimates at 10%, 5% and 1% significance levels, respectively, using Driscoll and Kraay (1998) standard errors (reported in parentheses).

Figure 2: Interaction of US monetary policy spillovers with aggregate prudential policy measures in recipient emerging markets for total credit and bank credit



Notes: $\{\delta^h\}_{h=0}^{8}$ estimates with (log) total credit (Panel A) and bank credit (B) for 29 emerging markets as dependent variable (regression (2)) using an aggregate prudential policy measure defined as: (a) the two-year cumulated sum of all prudential policy actions, excluding aggregate capital requirements, in the Cerutti et al. (2017b) dataset (dark blue solid line); and (b) the two-year cumulated sum of all macroprudential policy actions in the Alam et al. (2019) dataset (light blue dot-dash line). The light blue shaded area denotes the 90% confidence interval around point estimates for (a), constructed from Driscoll and Kraay (1998) standard errors. The dotted lines denote the corresponding confidence interval for (b).

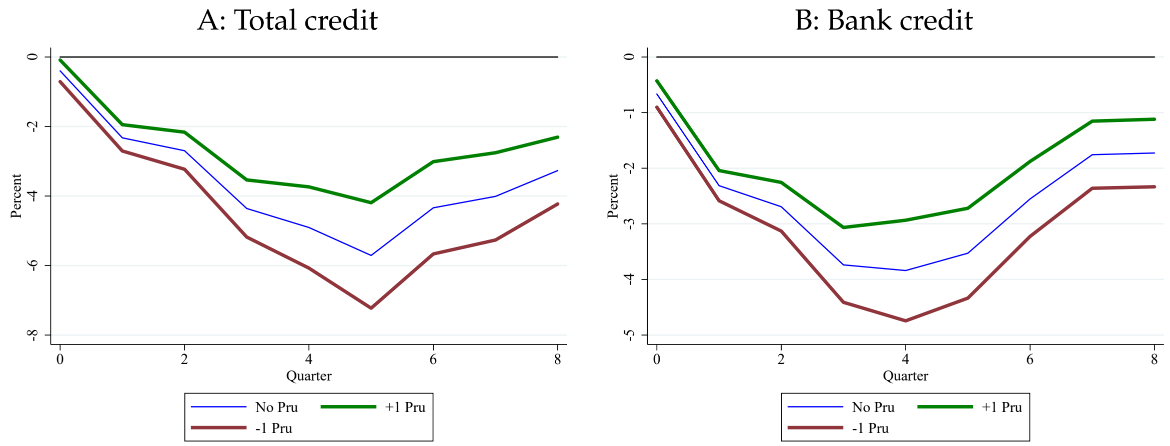
our results emphasise that prudential policy can dampen the cycle for overall domestic lending conditions in EMs, rather than conditions in cross-border lending specifically—the focus of Takáts and Temesvary (2021).

3.3 Economic Significance

Direct comparison of coefficient estimates from equations (1) and (2) is not possible as the latter includes time fixed effects f_t^h, while the former does not. To illustrate the economic significance of our headline results, we estimate a hybrid version of equation (2) that includes the monetary policy variable $MP_t^{\$}$ as an additional explanatory variable and, in order to do so, omits time fixed effects f_t^h. To replace the fixed effects, we add observed time-varying global control variables $\mathbf{G}_{t-1}$ to the regression specification. This enables concurrent estimation of the direct average spillover effect of US monetary policy to EMs β_{mp}^h and the interaction coefficient with domestic prudential policy δ^h. We use this hybrid specification to compare the two coefficients and gauge the economic significance of our findings. The coefficient estimates and standard errors from this hybrid regression are presented in columns (2) and (5) of Table 2 for total and bank credit, respectively.

Using these regressions, Figure 3 illustrates how the estimated spillover from a +1pp US monetary policy tightening varies depending on the lagged aggregate prudential policy actions carried out in an EM. Here the prudential policy measure sums all two-year cumulated actions in the Cerutti et al. (2017b) dataset. The blue line plots the estimated spillover to an EM

Figure 3: US monetary policy spillovers to total credit and bank credit for different levels of aggregate prudential policy in recipient emerging markets



Notes: $\{\beta^h_{mp} + \delta^h\}^8_{h=0}$ estimates with (log) total credit (left-hand side) and (log) bank credit (right-hand side) for 29 emerging markets as dependent variable in hybrid version of regression (2) that excludes time fixed effects f^h_t, but includes US monetary policy measure $MP^{\$}_t$ and lagged global controls $\mathbf{G}_{t-1}$. The aggregate prudential policy measure is defined as the two-year cumulated sum of all prudential policy actions, excluding aggregate capital requirements, in the Cerutti et al. (2017b) dataset. The blue line denotes estimated spillover from a 1pp US monetary policy tightening shock to an EM with a 0 value for prudential policy. The green line denotes the comparable spillover estimate for an EM with a +1 prudential policy action—i.e. on net, one additional policy tightening. The red line denotes the opposite spillover, for an EM with a -1 prudential policy action.

with zero net prudential policy actions, $Pru_{i,t-1} = 0$, indicating that a $+1$pp exogenous tightening of US monetary policy leads to around a 5.7% fall in total credit and 3.8% fall in bank credit in such EMs after around 12 to 15 months. An EM with an additional prudential policy tightening action, $Pru_{i,t-1} = 1$, is estimated to face a substantially smaller spillover. The peak spillover of a US monetary policy tightening shock in this EM to total credit is around 4.2% and to bank credit around 2.9%, indicating that an additional prudential policy tightening can offset the monetary policy spillover by around a quarter.

In the context of a the summary statistics presented in table 1, this figure can be scaled by the standard deviation of the aggregate prudential policy proxy (2.5). In this case, the peak spillover of a US monetary policy tightening shock to an illustrative EM with a prudential policy setting one standard deviation above the mean would be around 2% and 1.6% to total and bank credit, respectively. A one standard deviation tightening is then associated with a spillover reduction of over 50%. And an even larger prudential tightening could neutralise the effects of US monetary policy on EM domestic lending. Because the prudential policy tightening is likely to support less resilient banks—for example, by ensuring they hold sufficient reserves to withstand economic shocks—this finding is consistent with the logic of the bank-lending channel in international context, which suggests that the mechanism can operate more strongly for less resilient intermediaries.

3.4 Robustness

In this sub-section, we discuss the robustness of our headline findings for total credit using the aggregated prudential policy measure.[21] Our robustness analyses is split into two. First, we discuss alternative definitions of variables and lags within equation (2). Second, we account for additional, and potentially competing, factors that could interact with US monetary policy spillovers to EMs that are distinct from prudential policy.

3.4.1 Alternative Variable Definitions and Lags

Table 3 summarises the robustness exercises for (log) total credit, with column (1) reporting our baseline interaction coefficient estimates from regression (2).

Shadow Rate Monetary Policy Shock As discussed in Section 2.2, we examine the robustness of our results to an alternative monetary policy shock measure, defined using shadow interest rates (Wu and Xia, 2016) and estimated by Iacoviello and Navarro (2019). Column (2) reports interaction coefficient estimates from this exercise, which remain statistically significant—at the 1% level from $h = 5$ to $h = 8$—albeit a little smaller than our baseline estimates.

Alternative to Cumulating Prudential Policy Actions As discussed in Section 3.2, we construct our baseline prudential policy measure by cumulating actions over a two-year period. One may have concerns that cumulating prudential policy actions generates serial correlation in our regressor. To assuage this worry, we report an alternative specification as a robustness exercise in column (3) using lags of raw, uncumulated prudential policy actions. Specifically, we adapt regression (2) to account for this, by estimating:

$$
\begin{aligned}
y_{i,t+h} - y_{i,t-1} =& \alpha^h + \sum_{k=1}^{K} \delta_k^h \left(MP_t^{\$} \times Pru_{i,t-k} \right) + \sum_{k=1}^{K} \beta_{pru,k}^h Pru_{i,t-k} \\
&+ \gamma^{h'} \mathbf{X}_{i,t-1} + f_i^h + f_t^h + \varepsilon_{i,t_h}
\end{aligned} \tag{4}
$$

where $Pru_{i,t-k}$ is the raw aggregate prudential policy measure at time $t-k$. As in equation (2), we continue to ensure that prudential policies are lagged relative to the US monetary policy shock to omit the potential endogenous response of the former with respect to the latter.

Column (3) reports the sum of interaction coefficient estimates at each horizon h, $\sum_{k=1}^{K} \hat{\delta}_k^h$, where $K = 8$ quarters to mirror the two-year cumulation in our baseline regression.[22] As in the baseline regression, these alternative interaction coefficients are positive at all horizons.

[21] Appendix B.3 reports the corresponding robustness exercises for bank credit. Our results for bank credit are somewhat less robust to alternative specifications than our findings for total credit.

[22] Note that coefficient estimates in column (3) need to be divided by K for quantitative comparison with others in Table 3.

Table 3: Robustness of interaction coefficient estimates $\hat{\delta}^h$ from regression (2) for total credit using aggregate prudential policy measures, which exclude aggregate capital requirements in recipient emerging markets

	(1) Baseline	(2) MP Shadow Rate Shock	(3) Prudential Policy Measure: No Cumulation, Eq. (4)	(4) Prudential Policy Measure: 1-Year Cumulation	(5) Prudential Policy Measure: Full Sample Cumulation	(6) Prudential Policy Measure: Incl. Gen. Capital Req.	(7) Prudential Policy Measure: IMF iMaPP Database	(8) Controls: Eight Lags
$MP_t^{\$} \times Pru_{i,t-1}$								
$h = 0$	0.16	0.01	2.16	0.61**	-0.09	0.15	0.03	0.09
	(0.16)	(0.06)	(1.32)	(0.25)	(0.12)	(0.17)	(0.10)	(0.12)
$h = 1$	0.24	0.20	1.81	0.49	0.02	0.18	0.17	0.10
	(0.28)	(0.12)	(3.12)	(0.44)	(0.24)	(0.29)	(0.17)	(0.21)
$h = 2$	0.34	0.29*	3.24	0.66	-0.05	0.26	0.34	0.22
	(0.40)	(0.17)	(4.55)	(0.59)	(0.38)	(0.39)	(0.23)	(0.32)
$h = 3$	0.65	0.32*	6.67	0.90	0.29	0.57	0.56**	0.49
	(0.43)	(0.19)	(5.18)	(0.61)	(0.52)	(0.43)	(0.26)	(0.37)
$h = 4$	0.99*	0.40*	9.57	1.51**	0.48	0.87	0.75**	0.81*
	(0.53)	(0.23)	(6.43)	(0.70)	(0.67)	(0.55)	(0.30)	(0.46)
$h = 5$	1.35**	0.60***	13.71*	1.49**	0.83	1.22**	0.80**	1.36***
	(0.57)	(0.19)	(7.39)	(0.70)	(0.74)	(0.59)	(0.33)	(0.49)
$h = 6$	1.14*	0.75***	11.38	1.14	0.81	0.96	0.82**	1.24**
	(0.60)	(0.18)	(8.16)	(0.85)	(0.90)	(0.61)	(0.37)	(0.48)
$h = 7$	1.20*	0.72***	13.54	1.16	0.96	1.05	0.95**	1.39***
	(0.68)	(0.23)	(9.36)	(0.98)	(1.03)	(0.70)	(0.41)	(0.50)
$h = 8$	0.86	0.87***	9.09	0.73	0.98	0.69	0.89*	1.14*
	(0.78)	(0.24)	(9.93)	(1.05)	(1.16)	(0.79)	(0.47)	(0.61)
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES

Notes: $\hat{\delta}^h$, for $h = 1, ..., 8$, coefficient estimates from various specifications of regression (2), with exception of column (7) which reports summed coefficient estimates $\sum_{k=1}^{8} \hat{\delta}_k^h$ from regression (4). *, ** and *** denote statistically significant coefficient estimates at 10%, 5% and 1% significance levels, respectively, using Driscoll and Kraay (1998) standard errors (reported in parentheses).

In addition, the peak coefficient—arising at the 5-quarter-ahead horizon—is significant at the 10% level. The significance of this aggregate raw prudential measure is somewhat lower than the baseline in column (1), potentially reflecting that fact that we estimate more coefficients when using the raw prudential policy measures in equation (4) than in the baseline regression (2). Nevertheless, we later emphasise in Section 4 that our results for specific prudential policy instruments—reserve requirements specifically—are robust to this alternative specification, suggesting that the potentially heterogeneous effects of different prudential policies in our aggregate proxy could be washing out in our baseline regression. The fact that findings for specific prudential policy instruments are robust to this alternative specification is reassuring.

Cumulation of Prudential Policy Measure Columns (4) and (5) report alternative exercises, where we cumulate prudential policy actions over different summation periods — relative to the baseline two-year cumulation in column (1). Column (4) presents estimates using a one-year cumulation period, the same summation horizon used in Alam et al. (2019) to study the direct effects of macroprudential policy. Our estimated interaction coefficients remain positive (at all horizons) and significant (at some horizons) when using the this measure.

Column (5) presents interaction coefficients when the prudential policy series is cumulated from the start of the sample in 2000, as in Takáts and Temesvary (2019). The point estimates are positive from the third quarter-ahead, but are insignificantly different from zero at all horizons. One explanation for this insignificant finding could be that, by reflecting all policy actions since the start of the same, the full-sample cumulated measure does not adequately capture cyclical variation in prudential policy necessary to identify policy interactions in regression (2). Moreover, we note that the full-sample cumulated measure is, at best, a proxy of prudential policy 'stance' that is likely to contain measurement error, owing to the fact the Cerutti et al. (2017b) dataset does not include complete information on the intensity of prudential policy changes. Full-sample cumulation can, in effect, compound any such mismeasurement, with policy tightenings and loosenings able to cancel out—despite the intensity of policy changes potentially not offsetting one another. To the extent this measurement error for stance is evenly distributed (akin to classical measurement error), then we would expect coefficients in column (5) to be biased towards zero.[23] Thus, as in Bussière et al. (2021a) and the references within, we place greater weight on results acquired using shorter summation periods.

Including General Capital Requirements Column (6) documents coefficient estimates when general capital requirements are included in the two-year cumulated aggregate prudential policy measure. The point estimates are positive at all horizons, significantly so at 5-quarters-ahead. The coefficient at this horizon indicates that an additional tightening of prudential policy in an EM in advance of a $+1$pp US monetary policy tightening can, on average, reduce the hit to total credit by around 1.2pp. As discussed in Section 2.1, we primarily exclude general capital requirements from our baseline measure, as they reflect *micro*prudential adjustment—the same approach adopted in Takáts and Temesvary (2021). In addition, they largely capture the adoption of the Basel III regime, an internationally harmonised and broadly anticipated move, resulting in limited cross-country variation in the measure. In view of this, it is unsurprising that the coefficient estimates in column (6) are somewhat smaller, and less significant, than those in column (1).

Prudential Policy Database We use the Cerutti et al. (2017b) prudential policy dataset in our baseline regression. Column (7) shows that our results are robust to the use of an alternative macroprudential database: the IMF Integrated Macroprudential Policy (iMaPP) database

[23]Nevertheless, it is noteworthy that the corresponding interaction coefficient estimates with the full-sample cumulated prudential policy measure and the shadow-rate monetary policy shock are significantly positive at the 10% level at $h = 5$ (coefficient estimate 0.65) and $h = 6$ (0.75).

(Alam et al., 2019). Using this dataset, we construct an aggregate macroprudential policy measure by summing across of 17 macroprudential policy instruments incorporated in the database and then cumulate actions over a two-year period, as in our benchmark regression.[24] The interaction coefficient estimates are significantly positive from 3 to 8-quarters-ahead, as the light blue lines in Figure 2 also demonstrate.

Lagged Control Variables As explained in Section 2, we use two lags of output growth, inflation and quarterly changes of the dependent variable in our set of control variables $\mathbf{X}_{i,t-1}$. Column (8) reports an additional robustness exercise where we include eight lags of the country-specific control variables to mirror the number of periods over which prudential policy actions are cumulated in our baseline regression. Our point estimates for interaction coefficients are similar in magnitude to the baseline and remain statistically significant.

3.4.2 Competing Hypotheses

As an additional test of the robustness of our headline findings, we extend equation (2) to account for other factors $\mathbf{\Gamma}_{i,t-1}$ that could possibly interact with the US monetary policy spillovers to EMs that are distinct from prudential policy. Table 4 documents the results of this robustness analyses, presenting estimates of the interaction coefficient δ^h for spillovers to total credit when additional interactions $MP_t^{\$} \times \mathbf{\Gamma}_{i,t-1}$ (and $\mathbf{\Gamma}_{i,t-1}$ alone) are included in the set of controls $\mathbf{X}_{i,t-1}$. For reference, column (1) reports the $\hat{\delta}^h$ estimates from the baseline specification.

In columns (2) and (3), we extend the regression specification to include interactions between the US monetary policy shock and countries' lagged capital flow restrictiveness—i.e. $MP_t^{\$} \times KC_{i,t-1}$ and $KC_{i,t-1}$ are included in the set of time and country-varying controls $\mathbf{X}_{i,t-1}$, where $KC_{i,t-1}$ is a measure of capital controls in country i at time $t-1$. This is an important robustness test because, like prudential policies, capital controls may help to mitigate the spillovers from foreign shocks by limiting cross-border flows, and distinct from the bank-lending channel. By accounting for this interaction independently, we assuage worries that our results could simply reflect the effects of, potentially correlated, capital flow restrictions, and attempt to isolate the effects of prudential policies on bank lending specifically. To measure capital flows in our set of EMs, we use the index of Fernández, Klein, Rebucci, Schindler, and Uribe (2016).[25] In column (2), we use a measure of overall capital controls, spanning restrictions on inflows and outflows of a range of asset categories. In column (3), our measure of capital controls is focused on inflow restrictions, again spanning a range of asset categories.

Importantly, although the inclusion of an additional capital control interaction reduces the absolute size of our $\hat{\delta}^h$ estimates at all horizons relative to the baseline in column (1), the in-

[24] The iMaPP data is reported on a monthly basis, so we convert it to quarterly frequency by summing actions within a quarter.

[25] This index is measured at an annual frequency. We translate to quarterly frequency by assuming the index value for the calendar year is maintained in each quarter.

Table 4: Interaction coefficient estimates $\hat{\delta}^h$ from regression (2) for total credit using aggregate prudential policy measures, which exclude aggregate capital requirements, in recipient emerging markets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Additional Interaction Variables					
	Baseline	Capital Control	Capital Inflow Control	Credit-to-GDP Growth	FX Regime	Home Own. Share	Country FE
$MP_t^\$ \times Pru_{i,t-1}$							
$h=0$	0.16	0.09	0.10	0.11	0.10	0.17	0.12
	(0.16)	(0.17)	(0.16)	(0.16)	(0.14)	(0.15)	(0.10)
$h=1$	0.24	0.07	0.11	0.12	0.25	0.26	0.31
	(0.28)	(0.27)	(0.27)	(0.29)	(0.25)	(0.28)	(0.23)
$h=2$	0.34	0.10	0.15	0.18	0.38	0.37	0.40
	(0.40)	(0.37)	(0.37)	(0.40)	(0.35)	(0.39)	(0.31)
$h=3$	0.65	0.42	0.45	0.45	0.79**	0.67	0.60
	(0.43)	(0.39)	(0.39)	(0.38)	(0.39)	(0.41)	(0.36)
$h=4$	0.99*	0.74	0.76	0.79*	1.09**	0.98*	0.81*
	(0.53)	(0.51)	(0.50)	(0.47)	(0.50)	(0.52)	(0.44)
$h=5$	1.35**	1.02**	1.06**	1.12**	1.52***	1.36**	1.47***
	(0.57)	(0.49)	(0.52)	(0.54)	(0.52)	(0.56)	(0.52)
$h=6$	1.14*	0.86*	0.90	0.90	1.27**	1.14*	1.32**
	(0.60)	(0.51)	(0.55)	(0.58)	(0.54)	(0.59)	(0.61)
$h=7$	1.20*	0.90	0.95	0.92	1.37**	1.22*	1.68**
	(0.68)	(0.57)	(0.62)	(0.65)	(0.63)	(0.67)	(0.73)
$h=8$	0.86	0.57	0.59	0.56	0.91	0.84	0.89
	(0.78)	(0.73)	(0.75)	(0.73)	(0.73)	(0.77)	(0.85)
Country FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
Country FE $\times MP_t^\$$	NO	NO	NO	NO	NO	NO	YES

Notes: $\hat{\delta}^h$, for $h = 1, ..., 8$, coefficient estimates from various specifications of (2) designed to account for other potential interactors with monetary policy spillovers. *, ** and *** denote statistically significant coefficient estimates at 10%, 5% and 1% significance levels, respectively, using Driscoll and Kraay (1998) standard errors (reported in parentheses).

teraction coefficient estimates remain statistically significant at (at least) the 5-quarter horizon. The coefficient estimates in columns (2) and (3) indicate that, once controlling for capital flow restrictiveness, an additional tightening of prudential policy in an EM in advance of a +1pp US monetary policy tightening can, on average, reduce the hit to total credit from the shock by around 1pp after one year.[26]

In columns (4)-(6), we add alternative interaction time and country-varying variables. In column (4), we additionally allow US monetary policy shocks to interact with the level of lagged credit-to-GDP growth in country i. Column (5) includes an interaction between US monetary policy and the *de facto* exchange rate regime of the emerging market (Ilzetzki, Reinhart, and Rogoff, 2019). By accounting for this, the peak prudential policy interaction coef-

[26]We present the capital flow interaction coefficients—i.e. the coefficients on $MP_t^\$ \times KC_{i,t-1}$—in Appendix B.4.

ficient increases in size and significance, indicating that the exchange rate regime, indicating that the exchange rate regime is an important dimension of heterogeneity in monetary policy spillovers. Column (6) accounts for the role of home ownership in the interaction, defined as an indicator variable equal to 1 if a country's home ownership share exceeds the cross-country median of 70%, and 0 otherwise.[27] In all cases, the alternative interaction term reflects a dimension of a country's vulnerability to foreign shocks, distinct from prudential policy and the continued positive and statistically significant coefficient, on $MP_t^{\$} \times Pru_{i,t-1}$, at the four and five-quarter horizons at least, validates our main result.

In column (7), we further extend the regression specification to include interactions between the US monetary policy shock and the country fixed effects—i.e. $MP_t^{\$} \times f_i^h$ is included in the set of time and country-varying controls $\mathbf{X}_{i,t-1}$. This extension accounts for the possibility that country-specific, non-time-varying, factors could also interact with spillovers from US monetary policy aside from prudential policies. These factors are likely to capture persistent country-specific vulnerabilities, including structural imbalances, debt levels and institutional features. The fact the interaction coefficient estimate remains positive and statistically significant across these various robustness interactions, supports our main conclusion.

By illustrating that our findings are robust to the inclusion of potentially competing channels, these results further suggest that the interaction between domestic prudential policy and foreign monetary policy works through a bank-lending channel—rather than capital flows or broader macroeconomic conditions that might be correlated with prudential policy.

4 Specific Prudential Policy Instruments

In this section, we explore the interaction of US monetary policy with *specific* prudential policies in EMs to further isolate the economic channels at play. Within the Cerutti et al. (2017b) dataset, we are able to investigate interactions with five categories of prudential policies: (i) LTV ratio limits, (ii) reserve requirements, (iii) (sectoral) capital buffers, (iv) interbank exposure limits, and (v) concentration ratio limits. This classification is particularly interesting in light of the distinction between prudential policy instruments that 'dampen the cycle'—(i) and (ii), in particular—and those that 'increase resilience'—(iii)-(v)—laid out in Borio (2010) and Claessens et al. (2013).

Within our framework we find that, consistent with the view that US monetary policy is a driver of global cyclical fluctuations, LTV ratio limits and reserve requirements significantly interact with US monetary policy spillovers, while we do not find evidence of a significant interaction for sectoral capital buffers, interbank exposure limits and concentration ratio caps—the latter two of which were used to a limited extent by EMs in our sample. Our results provide

[27]Home ownership share data is from HOFINET and measures average home ownership rates in each country over the 2005-2014 period.

a novel test of the Borio (2010) and Claessens et al. (2013) classification, externally validating the separation of instruments that predominantly dampen the cycle, versus those that increase resilience.

Our findings also mirror those in Alam et al. (2019) to some degree. Focusing on the direct effects of macroprudential policy on household credit, they find that capital-based supply-side macroprudential policy tools—like capital buffers—have less potent effects in containing credit growth. In the context of our study, focused on cross-border policy interactions and spillovers, we similarly find that capital-related supply-side tools have limited effects on credit quantities, consistent with the idea that their main objective is to 'increase resilience' of the overall financial system rather than 'dampen the cycle'.

4.1 Loan-to-Value Ratio Limits

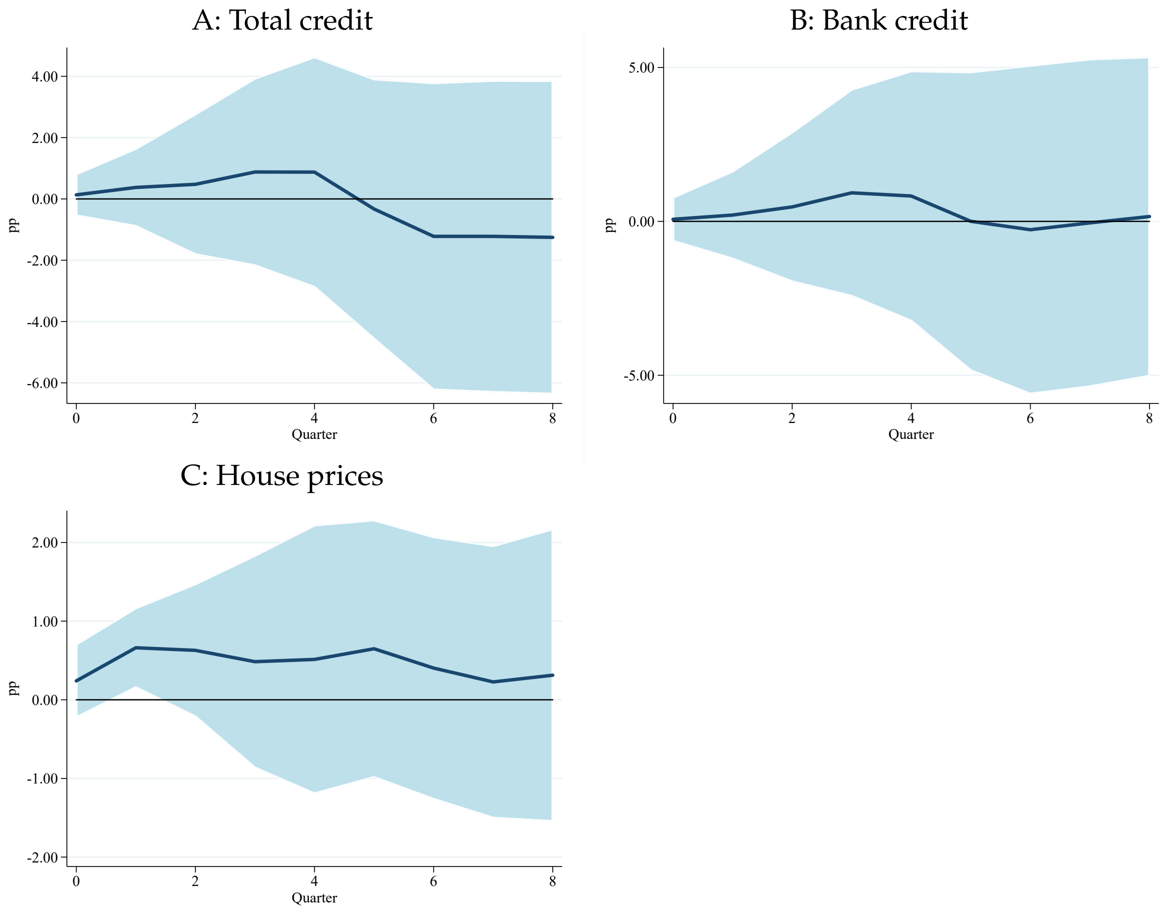
LTV ratio limits restrict the maximum amount an individual or firm can borrow against their collateral. These restrictions are most commonly applied to real estate transactions and the Cerutti et al. (2017b) dataset focuses on this aspect. As a consequence, we assess the interaction of LTV ratio limits with US monetary policy spillovers with the hypothesis that the policy should significantly reduce cyclical fluctuations in house prices and, possibly, curb excessive lending.

Using the two-year cumulated LTV ratio limit indices as the prudential policy measure in regression (2), Figure 4 presents the interaction coefficients for total credit, bank credit and house prices, respectively.[28] The results for total credit and bank credit—in panels A and B, respectively—indicate that domestic LTV ratio limits in EMs have no significant interaction with US monetary policy spillovers to credit EM credit *quantities*. This result has parallels with Alam et al. (2019), who find that loan-targeted demand-side tools—like LTV ratio limits—have more muted effects on household credit than supply-side tools. Similarly, studying the direct effects of LTV ratio limits, Akinci and Olmstead-Rumsey (2015) and Richter, Schularick, and Shim (2018) find that changes in LTV limits do not have significant effects on overall credit.

However, the results for house prices in panel C indicate that these demand-side tools may have *price* effects. Here, the estimated interaction coefficient is positive, and statistically significant in the near-term—according with our hypothesis. Tighter LTV ratio limits are associated with smaller cyclical fluctuations in house prices in EMs following innovations to US monetary policy. These results are perhaps not surprising: in a market, like the housing market, with an inelastic short-run supply curve, the majority of adjustment in response to cyclical fluctuations must occur in prices and not quantities.

[28] See Appendix B.5 for additional robustness analyses.

Figure 4: Interaction of US monetary policy spillovers with loan-to-value ratio limits in recipient emerging markets for house prices and total credit



Notes: $\{\delta^h_{mp}\}^8_{h=0}$ estimates with (log) total credit (Panel A), bank credit (B) and house prices (C) for 29 emerging markets as dependent variable (regression (2)). The light blue shaded area denotes the 90% confidence interval around point estimates, constructed from Driscoll and Kraay (1998) standard errors. The prudential policy measure is defined as the two-year cumulated sum of loan-to-value ratio limits in the Cerutti et al. (2017b) dataset.

4.2 Reserve Requirements

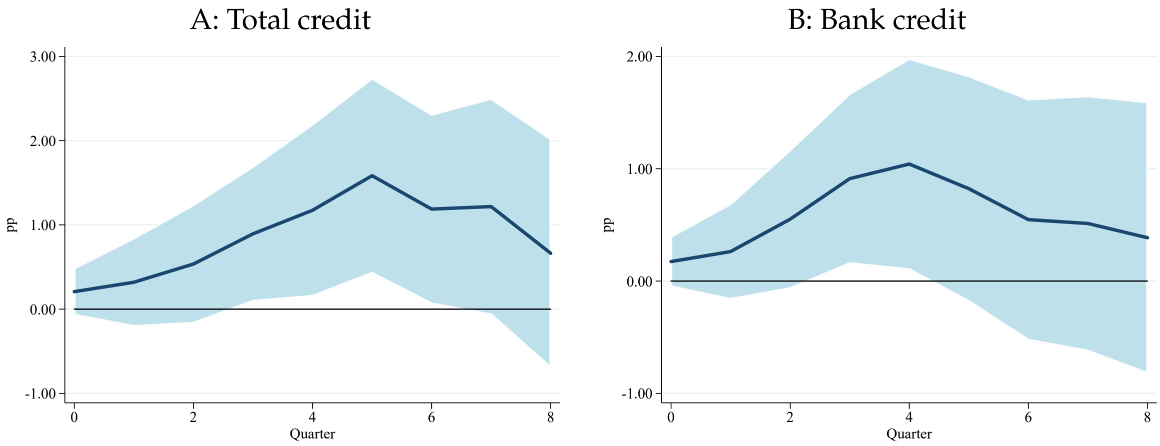
Within the Cerutti et al. (2017b) dataset, reserve requirements encompass all changes imposed on deposit accounts denominated in both domestic and foreign currency with prudential policy objectives. Given their broad application, we estimate equation (2) with the two-year cumulated reserve requirement indices, hypothesising that these policies should significantly interact with US monetary policy spillovers for both total credit and bank credit.

Figure 5 presents the interaction coefficient estimates across horizons for total and bank credit. In line with our hypothesis, the estimates are significantly positive at a range of horizons, indicating that reserve requirements can help to offset the spillover effects of US monetary policy shocks in EMs.[29] In response to a US monetary policy tightening shock that reduces

[29]Within our dataset, we do not find significant differences between domestic and foreign currency denominated reserve requirements, although more granular analyses of currency denomination in future research may prove fruitful.

credit in EMs, tighter reserve requirements can help lenders withstand such spillovers and reduce lending to the non-financial sector to a lesser extent than otherwise would be the case.[30]

Figure 5: Interaction of US monetary policy spillovers with reserve requirements in recipient emerging markets for total credit and bank credit



Notes: $\{\delta_{mp}^{h}\}_{h=0}^{8}$ estimates with (log) total credit (Panel A) and bank credit (B) for 29 emerging markets as dependent variable (regression (2)). The light blue shaded area denotes the 90% confidence interval around point estimates, constructed from Driscoll and Kraay (1998) standard errors. The prudential policy measure is defined as the two-year cumulated sum of all reserve requirements, levied on domestic and foreign currency-denominated deposits, in the Cerutti et al. (2017b) dataset.

Taken together, the results in this section suggest that two subsets of prudential policies instruments—specifically LTV ratio limits and reserve requirements—are particularly effective at offsetting the spillover effects of US monetary policies and dampening a country's exposure to the associated global credit cycle. This is an important mechanism through which Borio (2010) and Claessens et al. (2013) identify these prudential policies to be effectively used by national authorities to counter-cyclically dampen an expected credit boom or credit crunch.

5 Cross-Country Heterogeneity in Policy Interactions

In this section, we ask what conditions make this interaction effect stronger? To do so, we estimate equation (3) using different country characteristics $\mathbf{Z}_{i,t-1}$ as additional interactors. This relates to work by Beirne and Friedrich (2017), who examine the effectiveness of macroprudential policies as a function of banking sector characteristics.

5.1 Geography

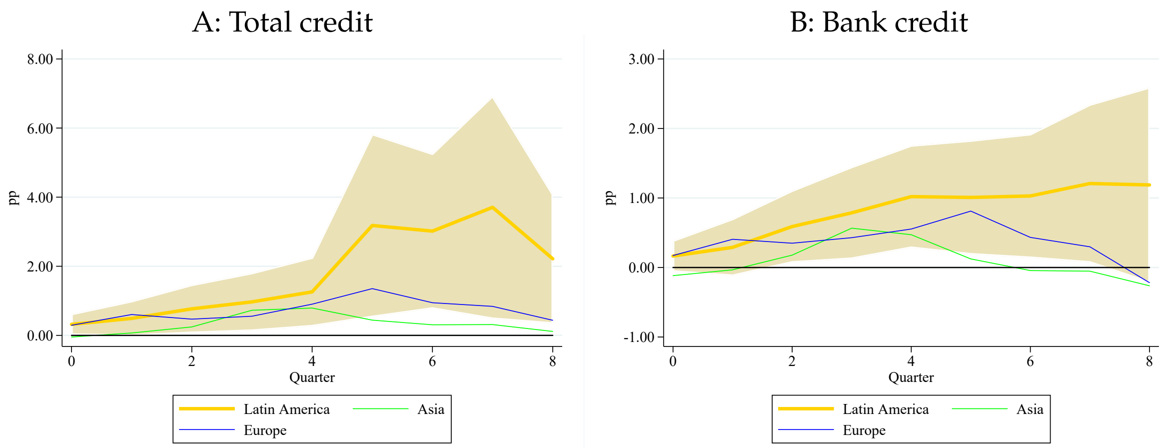
We first investigate geographical differences in the strength of the policy interaction (Figure 6). To do this, we estimate separate interaction coefficients for the 7 Latin American economies,

[30] In Appendix B.5, Table 12, we demonstrate that these findings for reserve requirements and credit quantities are robust to the use of raw prudential policy changes, as per equation (4). In Column 2, we find interaction coefficients to be significantly positive at the 10% level or more for $h = 3$ to $h = 5$. And, at $h = 5$, the coefficient is significant at the 5% level, as in the baseline specification (Table 11, Column 2).

8 European economies and 11 Asian economies that comprise our set of EMs.[31] There are reasons to expect differences across the groups. First, the geographical proximity of Latin American economies to the US is likely to strengthen economic ties between the two regions, both through trade and financial links. Second, data from Gopinath (2015) shows that the degree of dollar currency invoicing of international trade differs significantly across the three regions. Based on a smaller subset of EMs than in our macro-financial panel, the data from Gopinath (2015) illustrates that the average degree of dollarisation in Latin American EMs is around 97%, while in Asia and Europe the shares are around 80% and 41%, respectively.

We thus assess regional differences in the interaction with the hypothesis that geographically close and more dollarised EMs are likely to gain the most from offsetting prudential policy actions in the face of US monetary policy spillovers. Our results appear to indicate this to some degree, quantitatively at least. Estimates of the interaction coefficient are most strongly positive for Latin America, although differences are not statistically significant.

Figure 6: Interaction of US monetary policy spillovers with aggregate prudential policies, excluding aggregate capital requirements, in recipient emerging markets in Latin America, Asia and Europe



Notes: $\{\delta^h_{mp}\}^8_{h=0}$ estimates with (log) total credit (Panel A) and bank credit (B) for 29 emerging markets as dependent variable (regression (3)), grouped by region. The light yellow shaded area denotes the 90% confidence interval around point estimates for countries in Latin America, constructed from Driscoll and Kraay (1998) standard errors. The aggregate prudential policy measure is defined as the two-year cumulated sum of all prudential measures, excluding aggregate capital requirements, in the Cerutti et al. (2017b) dataset.

5.2 Home Ownership

The degree of home ownership within an economy is another potentially important determinant of financial risks. Higher home ownership rates tend to be associated greater housing market risks, as more households borrow and lever-up to buy housing.

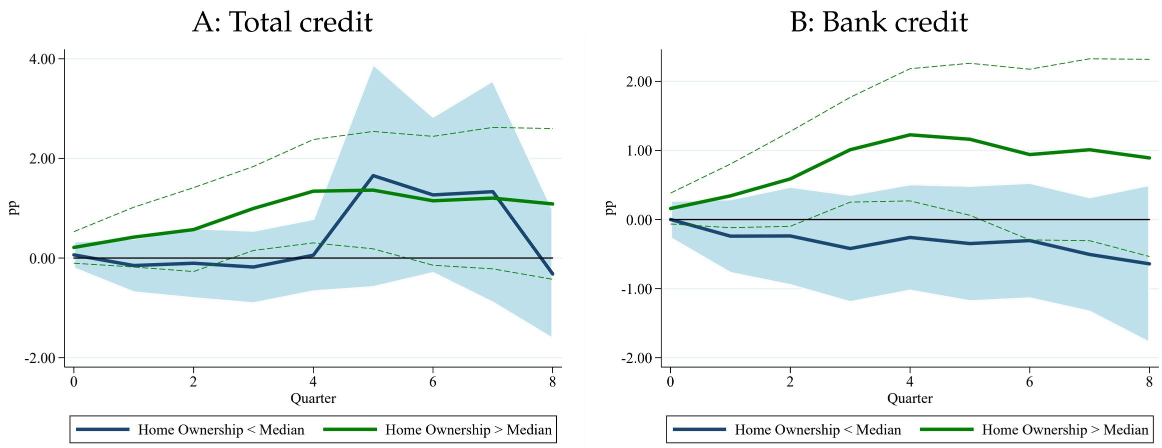
In Figure 7, we plot estimates of the interaction coefficient for aggregate prudential policy from equation (3) for total and bank credit, where countries are differentiated by home owner-

[31] We omit the remaining 3 African economies from this comparison.

ship share. To implement this triple interaction, we use the same home ownership share data as in Table 4, from HOFINET. As this data is only available for the 2005-2014 period, we construct a time-invariant triple interaction $\mathbb{1}_g$ by averaging country realisations over the sample, then calculating the cross-country median of the averaged data.

Our results indicate that prudential polices are indeed more effective at dampening cyclical fluctuations in EMs' credit quantities with higher home ownership shares (Figure 7). The left-hand plot shows that, in countries with above median home ownership shares, the interaction between our baseline measure of aggregate prudential policy and US monetary policy for spillovers to total credit and bank credit is significantly positive. In contrast, the interaction is insignificantly different for countries with home ownership shares that lie below the cross-country median.

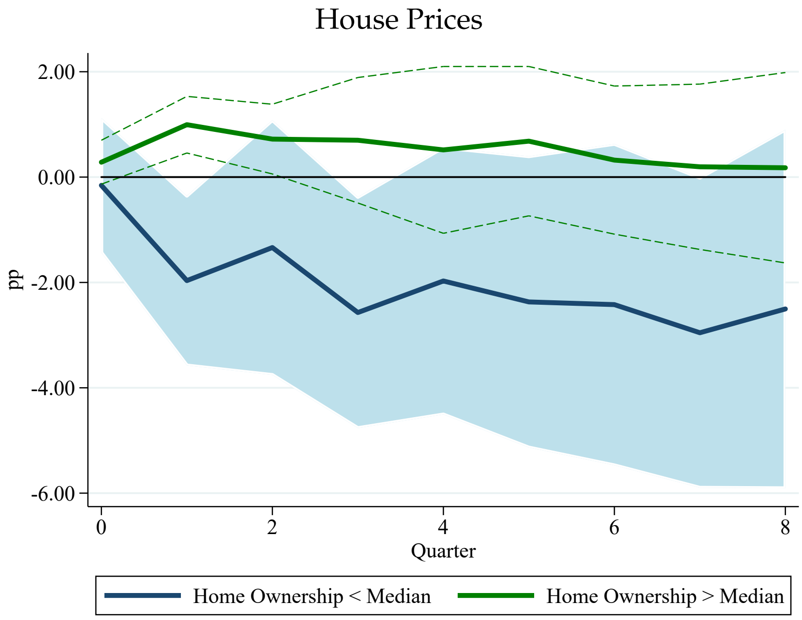
Figure 7: Interaction of US monetary policy spillovers with aggregate prudential policies, excluding general capital requirements, in recipient emerging markets with home ownership shares above and below the median



Notes: $\{\delta^h_{mp}\}^8_{h=0}$ estimates with (log) total credit (Panel A) bank credit (B) for 29 emerging markets as dependent variable (regression (3)), grouped by share of home ownership. The light blue shaded area and green-dashed lines denote the 90% confidence interval around point estimates for countries with below and above median home ownership shares, respectively, constructed from Driscoll and Kraay (1998) standard errors. The aggregate prudential policy measure is defined as the two-year cumulated sum of all prudential measures, excluding general capital requirements, in the Cerutti et al. (2017b) dataset.

Figure 8 hones in specifically on the role of LTV ratio limits, which we found to be significantly important for spillovers to house prices in the previous section. Consistent with the view that housing sectors with high home ownership shares pose greater risks to a country's financial stability, our results indicate that LTV ratio limits work, as expected, are indeed more effective at dampening housing market fluctuations in these economies. We find a significantly positive interaction coefficient for above-median home ownership countries when considering spillovers to house prices and the role of LTV ratio limits. In contrast, the interaction for below-median home ownership countries is broadly insignificant. The difference between the two is significantly positive at the $h = 1$ horizon.

Figure 8: Interaction of US monetary policy spillovers with LTV ratio limits in recipient emerging markets with home ownership shares above and below the median



Notes: $\{\delta^h_{mp}\}^8_{h=0}$ estimates with (log) house prices for 29 emerging markets as dependent variable (regression (3)), grouped by share of home ownership. The light blue shaded area and green-dashed lines denote the 90% confidence interval around point estimates for countries with below and above median home ownership shares, respectively, constructed from Driscoll and Kraay (1998) standard errors. The prudential policy measure is defined as the two-year cumulated sum of LTV ratio limits in the Cerutti et al. (2017b) dataset.

5.3 Exchange Rate Regimes and Dollar-Denominated Debt

Exchange rate regimes are at the centre of the traditional international macroeconomic policy trilemma. A country with free capital mobility can only pursue an independent monetary policy if their exchange rate is allowed to float. Ever since the work of Mundell (1963) and Fleming (1962), it has been understood that the spillover effects of foreign shocks will depend on the prevailing exchange rate regime. Within the Mundell-Fleming paradigm, monetary policy spillovers are likely to be larger than for countries with floating exchange rates, as relative prices are unable to adjust to insulate against the effects of foreign shocks. But, in the context of our EM-focused study, a competing channel is at play: foreign currency-denominated debt. In a country with a high share of foreign currency-denominated debt, a fixed exchange rate is likely to insulate against foreign shocks to some extent, by preventing valuation effects. The role of dollar currency debt has been shown to be important when studying the spillovers of US monetary policy to EMs. So to assess how the interaction changes with respect to exchange rate regimes, we estimate two variants of equation (3).

First, we use the *de facto* exchange rate regime classification of Ilzetzki et al. (2019) as our indicator of country characteristics $\mathbf{Z}_{i,t-1}$, differentiating between countries anchored to the US dollar and those not.[32] We favour a *de facto* classification precisely because we wish to account for the role of exchange rate adjustment in determining the interaction, rather than possible institutional characteristics associated with *de jure* measures.

Our results indicate that prudential policies appear to equally dampen cyclical fluctuations from US monetary policy shocks under floating and fixed exchange rate regimes (Figure 9). Although point estimates are significantly positive for fixed regime countries and insignificant for floating, estimates for the interaction effect are approximately the same for fixed and floating regimes, with confidence bands overlapping. This suggests that the competing effects of exchange rate regimes for spillovers tend to roughly balance in the context of cross-border prudential policy interactions.

Second, we use country-level data on the share of debt liabilities denominated in US dollars from Lane and Shambaugh (2010), and updated by Bénétrix, Lane, and Shambaugh (2015), as our indicator of country characteristics $\mathbf{Z}_i$. As foreign currency debt data is only available at an annual frequency, we construct a time-invariant triple interaction $\mathbb{1}_g$ by averaging the share of dollar debt by country over the sample, and then calculating the cross-country median of the average data for the EMs we consider.

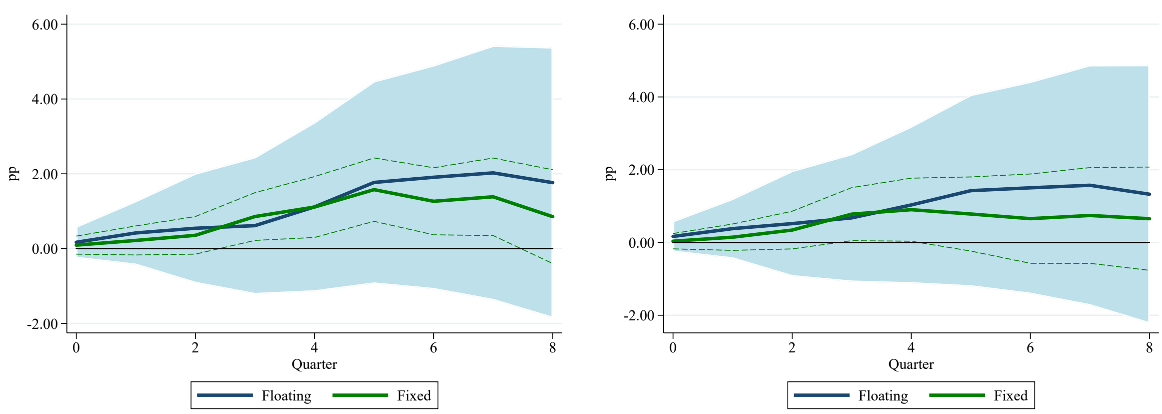
Again, while our results (Figure 10) indicate that the interaction coefficient for above-median US dollar-debt countries is significantly positive and insignificant for below-median countries, the confidence bands for both overlap. So, the interaction effects are approximately the same for both groups and prudential policies appear to equally dampen cyclical fluctuations from US monetary policy shocks, regardless of the share of dollar-denominated debt. However, we note that further work, with more granular data, could uncover additional insights. For instance, in the context of cross-border lending data, Takáts and Temesvary (2021) demonstrate a role for US dollar-denominated debt in global policy interactions.

6 Conclusion

This paper has presented novel evidence into the role of prudential policy in reducing the dynamic macro-financial spillover effects of US monetary policy shocks to EMs. By developing a local projections framework to assess the dynamic interactions between policies, we find that prudential policies can partially offset the negative spillover effects of US monetary policy, and dampen a country's exposure to the associated global credit cycle in a statistically and economically significant manner. Importantly, our findings are robust to accounting for other factors—such as capital controls—that could also reduce spillovers to EMs. In particular, we identify LTV ratio limits and reserve requirements to be effective tools for achieving this. Re-

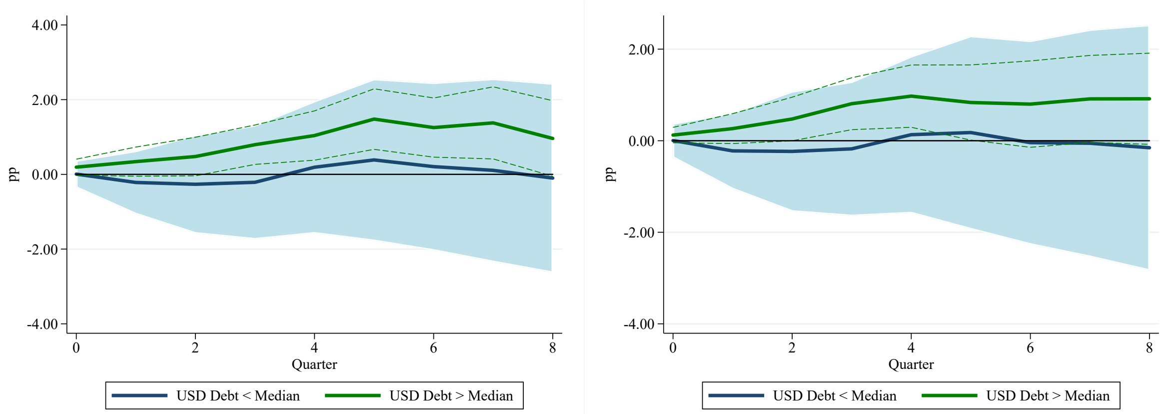
[32]We construct a time-varying triple interaction by using the monthly classification of exchange rate regimes from Ilzetzki et al. (2019). To convert the data to quarterly frequency, we classify an exchange rate regime as 'fixed' or 'floating' based on the realisation for the majority of the quarter, i.e. for at least 2 out of 3 months.

Figure 9: Interaction of US monetary policy spillovers with aggregate prudential policy in recipient emerging markets under fixed and floating exchange rates for total credit and bank credit



Notes: $\{\delta^h_{mp}\}_{h=0}^{8}$ estimates with (log) total credit (Panel A) and bank credit (B) for 29 emerging markets as dependent variable (regression (3)). The light blue shaded area and green-dashed lines denote the 90% confidence interval around point estimates for countries with floating and fixed exchange rate regimes, respectively, constructed from Driscoll and Kraay (1998) standard errors. Aggregate prudential policy measure is defined as the two-year cumulated sum of all prudential measures, excluding general capital requirements, in the Cerutti et al. (2017b) dataset. Exchange rate regimes are classified using the *de facto* measure of Ilzetzki et al. (2019).

Figure 10: Interaction of US monetary policy spillovers with aggregate prudential policy in recipient emerging markets under fixed and floating exchange rates for total credit and bank credit



Notes: $\{\delta^h_{mp}\}_{h=0}^{8}$ estimates with (log) total credit (Panel A) and bank credit (B) for 29 emerging markets as dependent variable (regression (3)). The light blue shaded area and green-dashed lines denote the 90% confidence interval around point estimates for countries with above-median and below-median share of US dollar-denominated debt, respectively, constructed from Driscoll and Kraay (1998) standard errors. Aggregate prudential policy measure is defined as the two-year cumulated sum of all prudential measures, excluding general capital requirements, in the Cerutti et al. (2017b) dataset. Dollar debt denomination data from Bénétrix et al. (2015).

serve requirements significantly reduce the spillover effects of US monetary policy to credit supply, while LTV ratio limits significantly mitigate spillovers to house prices.

While our empirical specification allows us to estimate cross-border monetary and prudential policy interactions, some limitations in our analysis remain—common to the majority of extant literature on prudential policy. First, our prudential policy dataset measures policy actions and captures the intensity of policy changes only to a limited extent. Second, our empirical framework cannot be easily reversed to study how the level of interest rates influences the spillovers of prudential policy 'shocks', due to challenges identifying exogenous innovations to prudential policies. Future research will, no doubt, benefit from improvements in prudential policy data coverage and granularity.

Nevertheless, our findings have important implications, suggesting that prudential policies can be effective at reducing the spillover effects of US monetary policy, helping policymakers to maintain monetary policy autonomy in the face of spillovers and the global financial cycle.

Appendix

A Data Sources

Our macro-financial dataset spans 29 EMs. Dependent variables are from the following sources: Total credit and Bank credit are from the Bank for International Settlements and International Financial Statistics; House prices are from the Bank for International Settlements and Oxford Economics. Additional control variables are from the following sources: real GDP data is from the International Monetary Fund, OECD and National Statistics Institutes); Consumer Price Index data is from the International Monetary Fund.

A.1 Prudential Policy Data

The Cerutti et al. (2017b) dataset captures the following types of prudential policies:

1. Capital requirements: (a) Aggregate capital requirements, reflecting the implementation of Basel capital agreements; (b) Sector-specific capital buffers, levied on: (i) Real estate credit; (ii)Consumer credit; (iii) Credit to other sectors.
2. Concentration ratio limits
3. Interbank exposure limits
4. Loan-to-value ratio limits
5. Reserve requirements on: (a) local currency-denominated accounts; and (b) foreign currency-denominated accounts.

A.1.1 Exploratory Prudential Policy Data

Figure 11 illustrates the cross-sectional variation in prudential policy for the 29 EMs in our dataset, plotting a global heat map of cumulated aggregate prudential policy actions from 2011:Q1 to 2012:Q4. The plot shows a wide degree of cross-country variation in the activeness with which prudential policy is used across countries. For this period, the tightest aggregate prudential policies occurred in Peru and Nigeria, with the loosest in India.

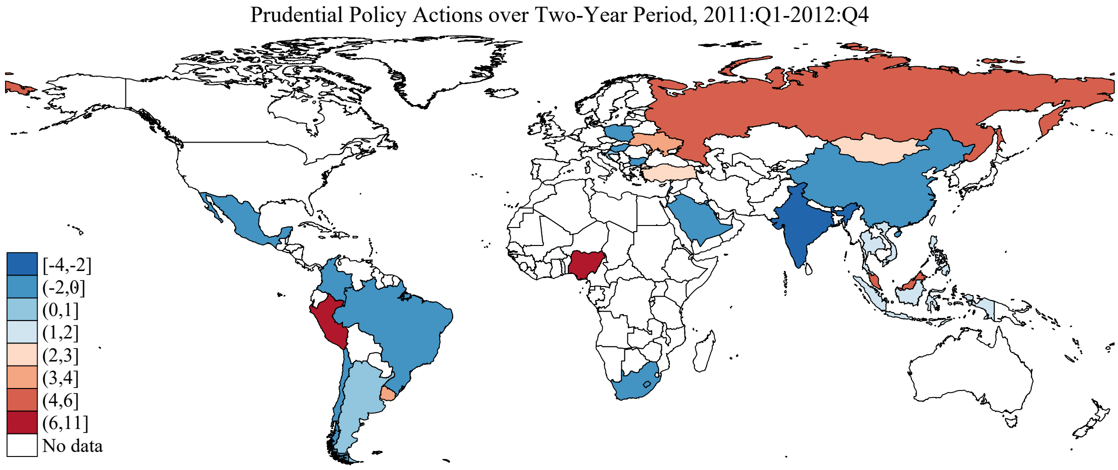
Figures 12 illustrates the time series variation in our baseline measure of aggregate prudential policy for Emerging Asian economies in our sample.[33]

A.1.2 Summary Statistics

Table 5 presents summary statistics for our aggregate prudential policy proxy cumulated over 2 years by country. Table 6 presents summary statistics for the same variable by year. Column (1) illustrates that, on average, most EMs tightened their prudential policies in aggregate (excluding general capital requirements) before the 2007-2008 financial crisis, loosening them immediately after. Columns (2)-(6) present summary statistics for 2-year cumulated measures of specific prudential policy instruments.

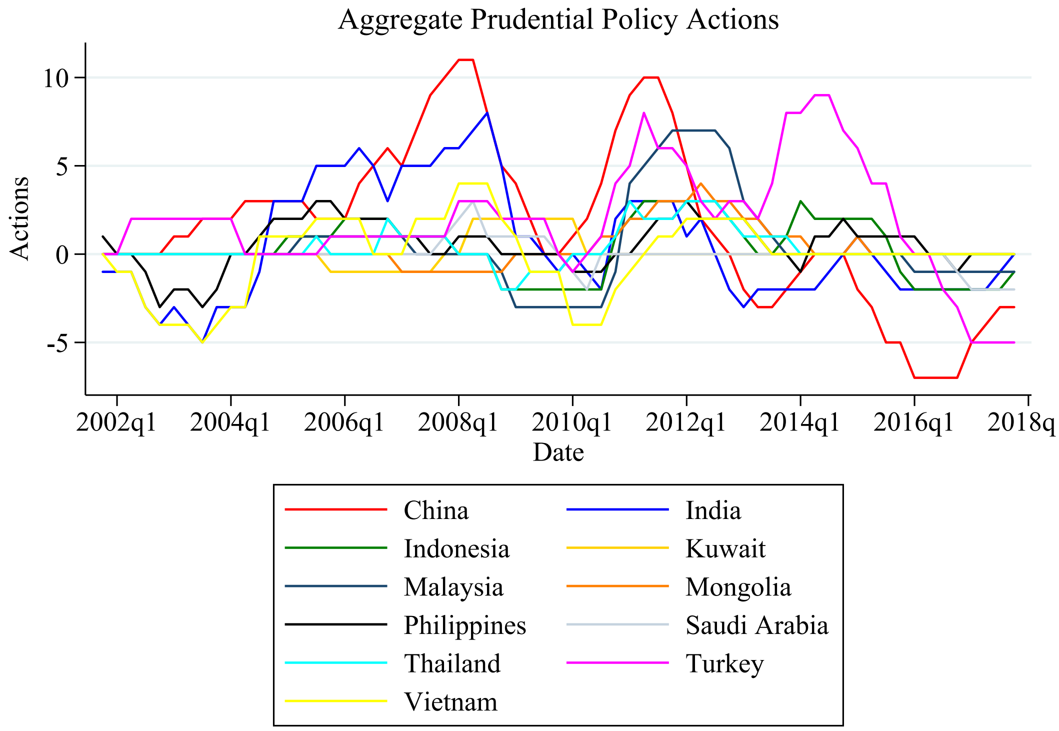
[33]Equivalent plots for Emerging European, Latin American and African economies can be found in the Appendix to a working paper version of this paper, accessible here: `www.ecb.europa.eu/pub/pdf/scpwps/ecb.wp2339~2287f8fae8.en.pdf?d18fee99ab2a611b0cf6a719039778f8`

Figure 11: Cumulated aggregate prudential policy actions by EM country, 2011:Q1-2012:Q4



Notes: Sum of the all prudential policy actions, excluding general capital requirements, in the Cerutti et al. (2017b) prudential policy actions dataset between 2011:Q1 and 2012:Q4 in each of the 29 EMs in our dataset.

Figure 12: Time series variation in two-year cumulated aggregate prudential policy actions in Emerging Asian economies, 2001:Q4-2017:Q4



Notes: Time series of two-year cumulated aggregate prudential policy actions, constructed using all prudential policies, except general capital requirements, in the Cerutti et al. (2017b) prudential policy actions dataset between 2001:Q4 and 2017:Q4 in Emerging Asian economies.

Table 5: Summary statistics for prudential policy proxies constructed by cumulating actions over a two-year period by country

Prudential Policy Measure	# Obs.	$\overline{Pru_{i,t}}$	$\sigma\left(Pru_{i,t}\right)$	$\min\left(Pru_{i,t}\right)$	$\max\left(Pru_{i,t}\right)$
Argentina	65	-0.046	2.308	-7	5
Brazil	65	1.000	2.817	-4	8
Bulgaria	65	0.585	2.297	-5	7
Chile	65	-0.123	0.484	-2	1
China	65	1.631	4.629	-7	11
Colombia	65	0.000	0.810	-2	2
Croatia	65	-0.185	2.221	-5	4
Hungary	65	-1.046	0.975	-3	2
India	65	0.569	3.211	-5	8
Indonesia	65	0.354	1.643	-2	3
Kuwait	65	0.123	0.781	-1	2
Lebanon	65	0.092	0.897	-2	2
Malaysia	65	0.492	2.463	-3	7
Mexico	65	0.246	0.662	0	2
Mongolia	65	0.385	1.128	-1	4
Nigeria	65	1.723	3.560	-3	11
Peru	65	1.415	4.687	-9	11
Philippines	65	0.538	1.347	-3	3
Poland	65	-0.292	1.826	-4	3
Romania	65	-1.923	2.740	-7	2
Russia	65	0.585	3.832	-7	7
Saudi Arabia	65	-0.015	0.800	-2	3
Serbia	65	-1.215	3.059	-8	6
South Africa	65	0.000	0.000	0	0
Thailand	65	0.385	1.026	-2	3
Turkey	65	1.908	3.121	-5	9
Ukraine	65	-0.538	2.586	-7	5
Uruguay	65	0.800	1.897	-3	4
Vietnam	65	-0.062	2.098	-5	4

Notes: Summary statistics are constructed for each country by pooling observations over the full sample period.

This aggregate loosening post-crisis was concentrated in capital requirements, LTV ratio limits and reserve requirements.

A.2 Monetary Policy Shocks

To describe the econometric framework for identifying US monetary policy shocks, we draw heavily on Gertler and Karadi (2015) (Section II). Let $\mathbf{Y}_t$ be a $K \times 1$ vector of economic and financial variables and ε_t be a vector of structural white noise shocks. A general structural form VAR is given by

$$\mathbf{A}\mathbf{Y}_t = \sum_{j=1}^{p} \mathbf{C}_j \mathbf{Y}_{t-j} + \varepsilon_t \tag{5}$$

where $\mathbf{A}$ and $\mathbf{C}_j$ $\forall j \geq 1$ are conformable coefficient matrices.

Table 6: Summary statistics for prudential policy proxies constructed by cumulating actions over a two-year period by year

Prudential Policy Measure	# Obs.	$\overline{Pru_{i,t}}$	$\sigma\left(Pru_{i,t}\right)$	$\min\left(Pru_{i,t}\right)$	$\max\left(Pru_{i,t}\right)$
2002	116	-0.336	1.704	-5	7
2003	116	-0.405	2.261	-7	8
2004	116	-0.034	1.724	-7	3
2005	116	0.586	1.818	-5	5
2006	116	0.966	2.030	-6	7
2007	116	1.043	2.019	-2	10
2008	116	1.138	2.509	-6	11
2009	116	-0.759	1.998	-7	4
2010	116	-1.164	2.442	-8	7
2011	116	1.638	3.425	-7	10
2012	116	2.078	3.005	-3	11
2013	116	0.871	2.983	-4	11
2014	116	0.310	2.576	-6	9
2015	116	-0.345	2.506	-9	6
2016	116	-0.707	2.068	-7	5
2017	116	-0.638	1.944	-6	6

Notes: Summary statistics are constructed for each year by pooling observations over quarters within the year and across the 29 EMs.

The reduced-form representation is attained by pre-multiplying each side of (5) by $\mathbf{A}^{-1}$:

$$\mathbf{Y}_t = \sum_{j=1}^{p} \mathbf{B}_j Y_{t-j} + \mathbf{u}_t \tag{6}$$

where $\mathbf{u}_t$ is a vector of reduced-form shocks, with the following relationship to structural disturbances

$$\mathbf{u}_t = \mathbf{S}\boldsymbol{\varepsilon}_t \tag{7}$$

with $\mathbf{B}_j = \mathbf{A}^{-1}\mathbf{C}_j$ and $\mathbf{S} = \mathbf{A}^{-1}$. $\boldsymbol{\Sigma} = \mathbb{E}\left[\mathbf{u}_t\mathbf{u}_t'\right] = \mathbb{E}\left[\mathbf{S}\mathbf{S}'\right]$ is the variance-covariance matrix of the reduced-form errors.

Define $Y_t^p \in \mathbf{Y}_t$ as the *policy indicator*, with corresponding exogenous structural shock $\varepsilon_t^p \in \boldsymbol{\varepsilon}_t$. Like Gertler and Karadi (2015), let the policy indicator in the VAR differ from the policy instrument to permit interest rate variation due to forward guidance. The policy indicator is defined as a government bond interest rate of somewhat longer maturity than the policy instrument—e.g. federal funds rate in US. The government bond rate captures innovations to both the current policy rate and expectations about the path of future policy rates.

To identify the monetary policy shock ε_t^P, let $\mathbf{Z}_t$ be a vector of instrumental variables and $\boldsymbol{\varepsilon}_t^q$ a vector of structural shocks excluding the policy shock. The identification assumptions are: $\mathbb{E}\left[\mathbf{Z}_t\varepsilon_t^{p\prime}\right] = \phi$ and $\mathbb{E}\left[\mathbf{Z}_t\boldsymbol{\varepsilon}_t^{q\prime}\right] = \mathbf{0}$. To be valid, the instruments must be correlated with the policy shock, but orthogonal to all other structural shocks.

Let s represent the column in matrix $\mathbf{S}$ corresponding to the impact of the structural monetary policy shock ε_t^p on each element of the reduced-form residuals $\mathbf{u}_t$ (see (7)). Estimates of the elements in the vector s can be obtained in the following two steps:

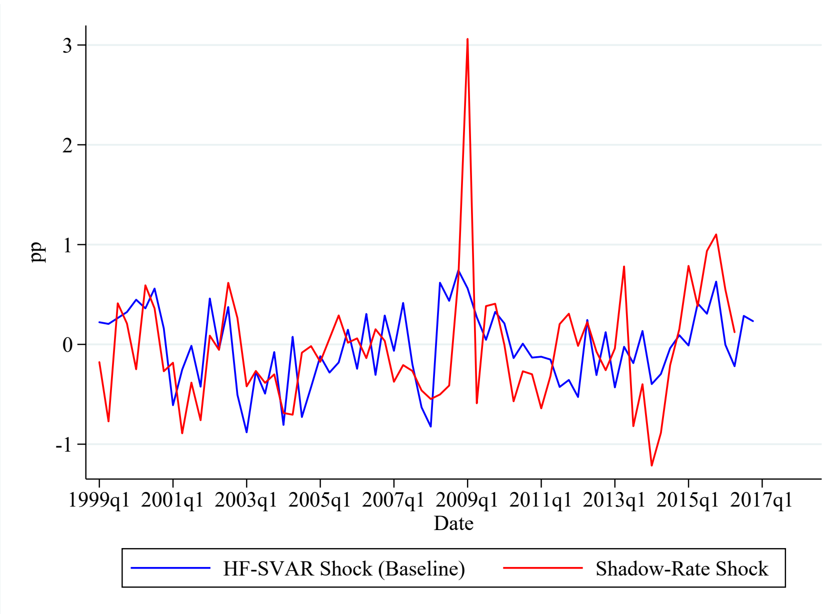
1. Estimate the reduced-form VAR (6) by OLS to obtain a vector of reduced-form residuals $\hat{\mathbf{u}}_t$. De-

fine $\hat{u}_t^p$ as the reduced-form residual from the equation for the policy indicator, and let $\hat{\mathbf{u}}_t^q$ be the vector of reduced-form residuals from the other variable equations, for $q \neq p$.

2. Define $\mathrm{s}^q \in \mathrm{s}$ as the response of $\mathbf{u}_t^q$ to a unit increase in the structural policy shock ε_t^p. Obtain an estimate of the ratio s^q/s^p from a two-stage least squares regression of $\hat{\mathbf{u}}_t^q$ on $\hat{u}_t^p$, using the instrument set $\mathbf{Z}_t$. Estimates of s^p and s^q can be derived up to a sign convention using the estimates reduced-form variance-covariance matrix of residuals $\hat{\mathbf{\Sigma}}$.[34]

Data and Results We estimate the structural shocks using a 1979:07-2018:10 sample for the US. Because the regression framework includes 12 lags, monthly frequency estimates of policy shocks begin in 1980:07. The VARs include four variables that match the baseline specification in Gertler and Karadi (2015): the 1-year government bond interest rate, industrial production, the consumer price index (CPI), and a measure of corporate credit spreads.[35] Industrial production and CPI are included in log levels, while interest rates and credit spreads are included in levels. The VARs for all regions include 12 lags of monthly variables—i.e. $p = 12$. The instrument for monetary policy $\mathbf{Z}_t$ is selected based on its relevance, measured by first-stage F-statistics, which is 22.9 for this sample. The estimated shock from this SVAR, identified with high-frequency methods, is plotted in Figure 13, where it is compared to the shadow-rate shock (Iacoviello and Navarro, 2019) we use in robustness analysis.

Figure 13: Estimated monetary policy shock series from high-frequency SVAR (HF-SVAR) compared to shadow-rate shock from Iacoviello and Navarro (2019)



Notes: Time series of estimated monetary policy shocks. HF-SVAR shock is the baseline shock used in this paper. Shadow-rate shock estimated by Iacoviello and Navarro (2019) using US shadow interest rates (Wu and Xia, 2016). Over the sample period the two shock series have a correlation of 0.42.

[34] See Gertler and Karadi (2015, pp. 51-52).

[35] US industrial production and CPI data are from FRED. The 1-year government bond interest rate is from Gürkaynak et al. (2007). Credit spreads are measured using the excess bond premium from Gilchrist and Zakrajsek (2012). Monetary policy surprises are from Gürkaynak et al. (2005), constructed using intraday variation in the three month-ahead federal funds futures rate in 30-minute windows around FOMC announcements.

B Additional Empirical Results

Here, we report additional coefficient estimates to support the analyses in the paper's main body.

B.1 Additional Dependent Variables and Aggregate Prudential Policy Interactions

Table 7 reports monetary policy spillover and interaction coefficient estimates for house prices, total credit-to-GDP and bank credit-to-GDP using the baseline aggregate prudential policy measure.

Table 7: Estimated coefficients from regressions (1) and (2) for house prices, total credit-to-GDP and bank credit-to-GDP using aggregate prudential policy measure, which excludes capital requirements, in recipient in emerging markets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		House Prices			Total Credit-to-GDP			Bank Credit-to-GDP	
	Mon. Pol. Spill.	Hybrid	Int.	Mon. Pol. Spill.	Hybrid	Int.	Mon. Pol. Spill.	Hybrid	Int.
$MP_t^{\$}$									
$h = 0$	-0.55**	-0.46*		-0.0043	-0.0203***		-0.0027	-0.0123***	
	(0.21)	(0.24)		(0.0041)	(0.0056)		(0.0021)	(0.0030)	
$h = 1$	-1.00**	-0.52		-0.0045	-0.0010		-0.0031	-0.0021	
	(0.41)	(0.44)		(0.0061)	(0.0031)		(0.0032)	(0.0021)	
$h = 2$	-1.32**	-0.52		-0.0082	-0.0069*		-0.0054	-0.0055***	
	(0.57)	(0.63)		(0.0064)	(0.0035)		(0.0035)	(0.0021)	
$h = 3$	-1.36*	-0.12		-0.0093	-0.0095*		-0.0054	-0.0063**	
	(0.75)	(0.83)		(0.0062)	(0.0052)		(0.0033)	(0.0025)	
$h = 4$	-1.35	-0.05		-0.0249**	-0.0404***		-0.0132***	-0.0228***	
	(1.02)	(1.25)		(0.0097)	(0.0122)		(0.0047)	(0.0057)	
$h = 5$	-1.34	-0.12		-0.0271**	-0.0226**		-0.0134**	-0.0122**	
	(1.35)	(1.67)		(0.0133)	(0.0111)		(0.0064)	(0.0048)	
$h = 6$	-1.40	0.04		-0.0285**	-0.0243**		-0.0148**	-0.0135**	
	(1.64)	(2.02)		(0.0118)	(0.0117)		(0.0059)	(0.0054)	
$h = 7$	-1.81	-0.23		-0.0171*	-0.0148		-0.0086	-0.0080	
	(1.87)	(2.32)		(0.0102)	(0.0102)		(0.0053)	(0.0051)	
$h = 8$	-2.61	-1.06		-0.0242**	-0.0317***		-0.0130**	-0.0188***	
	(1.95)	(2.65)		(0.0108)	(0.0122)		(0.0059)	(0.0067)	
$MP_t^{\$} \times Pru_{i,t-1}$									
$h = 0$		-0.05	-0.09		0.0004	0.0010		0.0001	0.0003
		(0.09)	(0.10)		(0.0010)	(0.0014)		(0.0006)	(0.0008)
$h = 1$		-0.30	-0.28**		0.0005	-0.0007		0.0005	-0.0002
		(0.21)	(0.12)		(0.0008)	(0.0012)		(0.0004)	(0.0006)
$h = 2$		-0.28	-0.25		0.0006	-0.0007		0.0009	0.0002
		(0.23)	(0.17)		(0.0012)	(0.0013)		(0.0009)	(0.0007)
$h = 3$		-0.31	-0.27		0.0021*	0.0006		0.0019	0.0012
		(0.38)	(0.24)		(0.0013)	(0.0011)		(0.0012)	(0.0010)
$h = 4$		-0.18	-0.25		0.0022	0.0025		0.0012	0.0012
		(0.44)	(0.30)		(0.0015)	(0.0016)		(0.0010)	(0.0010)
$h = 5$		0.05	-0.15		0.0038**	0.0024		0.0016**	0.0007
		(0.35)	(0.36)		(0.0019)	(0.0022)		(0.0008)	(0.0009)
$h = 6$		0.06	-0.16		0.0022	0.0012		0.0009	0.0003
		(0.36)	(0.39)		(0.0019)	(0.0020)		(0.0010)	(0.0011)
$h = 7$		0.11	-0.19		0.0029	0.0018		0.0016	0.0011
		(0.31)	(0.40)		(0.0022)	(0.0019)		(0.0012)	(0.0012)
$h = 8$		0.25	-0.05		-0.0003	0.0005		0.0000	0.0004
		(0.25)	(0.48)		(0.0031)	(0.0024)		(0.0016)	(0.0014)
Country FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	NO	NO	YES	NO	NO	YES	NO	NO	YES

Notes: $\hat{\beta}^h$ anf $\hat{\delta}^h$, for $h = 0, 1, \ldots, 8$ coefficient estimates from regression (1) in columns (1), (4) and (7), regression (2) in (3), (6) and (9), and a hybrid of the two in columns (2), (5) and (8). *, ** and *** denote statistically significant coefficient estimates at 10%, 5% and 1% significance levels, respectively, using Driscoll and Kraay (1998) standard errors (reported in parentheses).

B.2 Spillovers and Interactions for Non-Bank Credit

In this Appendix, we briefly summarise our results for non-bank credit. In our data, non-bank credit is defined as total claims from domestic financial corporations, non-financial corporations and non-residents. Our headline findings suggest that US monetary policy tightening shocks exert negative, but statistically insignificant, changes in EM non-bank credit (Column 1, Table 8). While our baseline interaction specification features positive and significant interaction coefficients (Column 3, Table 8), we do not find this result is robust. For instance, using a shadow-rate monetary policy shock, the interaction coefficients are insignificantly different from zero across all horizons (Column 4, Table 8).

Table 8: Estimated coefficients from regressions (1) and (2) for non-bank credit using aggregate prudential policy measure, which excludes capital requirements, in recipient in emerging markets

	(1) Mon. Pol. Spillover	(2) Hybrid	(3) Interaction: Baseline Mon. Pol. Shock	(4) Interaction: Shadow Rate Shock
$MP_t^{\$}$				
$h=0$	0.24	-0.04		
	(1.35)	(1.55)		
$h=1$	-0.75	-2.40		
	(2.08)	(2.56)		
$h=2$	-1.59	-3.95		
	(2.23)	(3.43)		
$h=3$	-3.51	-5.39		
	(2.74)	(3.76)		
$h=4$	-3.26	-5.23		
	(3.65)	(3.93)		
$h=5$	-4.94	-6.92		
	(3.45)	(4.37)		
$h=6$	-2.79	-3.39		
	(3.04)	(4.42)		
$h=7$	-3.80	-3.51		
	(2.70)	(4.30)		
$h=8$	-2.77	-1.06		
	(2.90)	(4.58)		
$MP_t^{\$} \times Pru_{i,t-1}$				
$h=0$		0.35	0.27	-0.16
		(0.25)	(0.29)	(0.23)
$h=1$		1.14	0.90	0.05
		(0.84)	(0.72)	(0.19)
$h=2$		0.89	0.69	-0.07
		(0.59)	(0.70)	(0.32)
$h=3$		1.41**	0.97	-0.05
		(0.72)	(0.64)	(0.31)
$h=4$		3.04**	2.60***	0.54
		(1.47)	(0.85)	(0.37)
$h=5$		3.27**	3.15***	0.27
		(1.56)	(0.86)	(0.37)
$h=6$		3.78*	3.66***	0.51
		(2.14)	(1.15)	(0.37)
$h=7$		2.94	3.28***	0.49
		(1.84)	(1.08)	(0.32)
$h=8$		3.08	3.25**	0.46
		(1.97)	(1.27)	(0.39)
Country FE	YES	YES	YES	YES
Time FE	NO	NO	YES	YES

Notes: $\hat{\beta}^h$ anf $\hat{\delta}^h$, for $h = 0, 1, \dots, 8$ coefficient estimates from regression (1) in column (1), regression (2) in (3) and (4), and a hybrid of the two in column (2). *, ** and *** denote statistically significant coefficient estimates at 10%, 5% and 1% significance levels, respectively, using Driscoll and Kraay (1998) standard errors (reported in parentheses).

B.3 Robustness: Aggregate Prudential Policy Interactions for Bank Credit

Table 9 reports robustness analyses to complement Section 3.4 for bank credit using measures of aggregate prudential policy.

Table 9: Robustness of interaction coefficient estimates $\hat{\delta}^h$ from regression (2) for bank credit using aggregate prudential policy measures, which exclude aggregate capital requirements, in recipient emerging markets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		MP		Prudential Policy Measure				Controls
	Baseline	Shadow Rate Shock	No Cumulation, Eq. (4)	1-Year Cumulation	Full Sample Cumulation	Incl. Gen. Capital Req.	IMF iMaPP Database	Eight Lags
$MP_t^{\$} \times Pru_{i,t-1}$								
$h=0$	0.12	0.05	1.11	0.22*	-0.08	0.10	0.03	0.14
	(0.12)	(0.06)	(1.01)	(0.13)	(0.13)	(0.13)	(0.08)	(0.12)
$h=1$	0.18	0.22	0.84	0.01	0.00	0.11	0.17	0.26
	(0.23)	(0.14)	(2.18)	(0.28)	(0.26)	(0.23)	(0.16)	(0.24)
$h=2$	0.36	0.34*	2.73	0.19	-0.05	0.25	0.38*	0.49
	(0.34)	(0.19)	(3.56)	(0.36)	(0.42)	(0.33)	(0.23)	(0.36)
$h=3$	0.64	0.40*	5.78	0.43	0.10	0.53	0.62**	0.76*
	(0.41)	(0.21)	(4.55)	(0.40)	(0.59)	(0.41)	(0.26)	(0.45)
$h=4$	0.87*	0.43	7.37	0.96*	0.21	0.72	0.71**	1.02*
	(0.52)	(0.26)	(5.73)	(0.52)	(0.78)	(0.52)	(0.30)	(0.55)
$h=5$	0.75	0.41	5.90	0.74	0.23	0.54	0.71**	1.04*
	(0.59)	(0.28)	(6.44)	(0.60)	(0.94)	(0.58)	(0.35)	(0.61)
$h=6$	0.65	0.46	4.40	0.50	0.16	0.40	0.73*	1.09
	(0.67)	(0.32)	(7.28)	(0.73)	(1.11)	(0.65)	(0.39)	(0.66)
$h=7$	0.69	0.62*	4.82	0.49	0.14	0.44	0.82*	1.16*
	(0.73)	(0.33)	(8.16)	(0.79)	(1.30)	(0.71)	(0.43)	(0.68)
$h=8$	0.66	0.71**	4.40	0.31	0.07	0.42	0.81*	1.23
	(0.79)	(0.35)	(8.99)	(0.85)	(1.44)	(0.76)	(0.48)	(0.76)
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES

Notes: $\hat{\delta}^h$, for $h = 1, ..., 8$, coefficient estimates from various specifications of regression (2), with exception of column (7) which reports summed coefficient estimates $\sum_{k=1}^{8} \hat{\delta}_k^h$ from regression (4). *, ** and *** denote statistically significant coefficient estimates at 10%, 5% and 1% significance levels, respectively, using Driscoll and Kraay (1998) standard errors (reported in parentheses).

B.4 Competing Hypotheses: Coefficients on Capital Controls

Although not the main focus of our paper, the coefficients on the $MP_t^{\$} \times KC_{i,t-1}$ provide some indication about the extent to which capital controls can insulate against the spillover effects of US monetary policy. A positive interaction coefficient for capital controls can be interpreted in a similar vein to the coefficient estimates on the prudentail policy interaction in the main body of the paper. Table 10 reports the capital control interaction coefficients for total credit that come from the same regressions as the prudential policy interaction coefficients presented in columns (2) and (3) of Table 4.

Table 10: Interaction coefficient estimates for capital controls from regression (2)) for total credit, when assessing the interaction of emerging market aggregate prudential policy with US monetary policy

	(1)	(2)
	Total Credit	
	Capital Control	Capital Inflow Control
$MP_t^{\$} \times KC_{i,t-1}$		
$h = 0$	0.74	0.65
	(1.21)	(1.14)
$h = 1$	3.60*	3.03
	(1.96)	(1.87)
$h = 2$	5.19*	4.37
	(3.02)	(2.82)
$h = 3$	5.65	5.79
	(3.77)	(3.69)
$h = 4$	5.96	6.87
	(4.75)	(4.54)
$h = 5$	9.89	10.07
	(6.97)	(6.51)
$h = 6$	8.13	8.74
	(6.88)	(6.67)
$h = 7$	10.17	10.69
	(8.60)	(8.38)
$h = 8$	9.27	11.42
	(8.47)	(8.50)
Country FE	YES	YES
Time FE	YES	YES

Notes: $MP_t^{\$} \times KC_{i,t-1}$ coefficient estimates from an extended variant of regression (2), which includes $MP_t^{\$} \times KC_{i,t-1}$ and $KC_{i,t-1}$ in the set of control variables o account for other potential interactors, in addition to prudential policy, with monetary policy spillovers. *, ** and *** denote statistically significant coefficient estimates at 10%, 5% and 1% significance levels, respectively, using Driscoll and Kraay (1998) standard errors (reported in parentheses).

B.5 Specific Prudential Policy Instruments

Table 11 presents interaction coefficient estimates pertaining to the regressions discussed in Section 4.

Table 11: Interaction coefficient estimates $\hat{\delta}^h$ from regression (2) for total credit, bank credit and house prices using loan-to-value ratio limits ('LTV') and reserve requirements ('RR') prudential policy measures in recipient emerging markets

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Credit		Bank Credit		House Prices	
	LTV	RR	LTV	RR	LTV	RR
$MP_t^{\$} \times Pru_{i,t-1}$						
$h=0$	0.13	0.21	0.07	0.17	0.24	-0.11
	(0.41)	(0.17)	(0.43)	(0.14)	(0.28)	(0.14)
$h=1$	0.37	0.32	0.21	0.26	0.66**	-0.41***
	(0.77)	(0.32)	(0.86)	(0.26)	(0.31)	(0.12)
$h=2$	0.48	0.54	0.47	0.55	0.63	-0.38**
	(1.39)	(0.43)	(1.48)	(0.37)	(0.51)	(0.15)
$h=3$	0.88	0.89*	0.93	0.91*	0.48	-0.43*
	(1.86)	(0.49)	(2.04)	(0.46)	(0.82)	(0.23)
$h=4$	0.88	1.17*	0.82	1.04*	0.51	-0.48
	(2.28)	(0.62)	(2.47)	(0.57)	(1.04)	(0.33)
$h=5$	-0.33	1.58**	0.00	0.82	0.65	-0.41
	(2.58)	(0.70)	(2.95)	(0.61)	(0.99)	(0.39)
$h=6$	-1.22	1.19*	-0.27	0.55	0.40	-0.47
	(3.04)	(0.68)	(3.24)	(0.65)	(1.01)	(0.46)
$h=7$	-1.22	1.22	-0.05	0.51	0.23	-0.53
	(3.09)	(0.78)	(3.23)	(0.69)	(1.05)	(0.48)
$h=8$	-1.25	0.66	0.16	0.39	0.31	-0.33
	(3.11)	(0.83)	(3.15)	(0.73)	(1.13)	(0.52)
Country FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES

In Table 12, we report robustness analyses for the regressions presented in the previous Table 11. Specifically, we estimate interactions for specific prudential policy measures using our alternative regression specification with raw, uncumulated prudential policy actions presented in equation (4). As Section 4 of the paper, we find that reserve requirements have significant interactions with credit quantities, especially total credit. However, the interaction results for LTV ratio limits are not particularly robust. Therefore, our results indicate that reserve requirements play a particularly important role in dampening the cycle in EMs.

Table 12: Interaction coefficient estimates $\hat{\delta}^h$ from regression (4) for total credit, bank credit and house prices using loan-to-value ratio limits ('LTV') and reserve requirements ('RR') prudential policy measures in recipient emerging markets

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Credit		Bank Credit		House Prices	
	LTV	RR	LTV	RR	LTV	RR
$MP_t^{\$} \times Pru_{i,t-1}$						
$h = 0$	5.11	1.83	4.41	1.62*	4.89	-0.78
	(5.52)	(1.12)	(8.00)	(0.81)	(3.16)	(1.27)
$h = 1$	2.76	1.96	-1.36	1.61	6.21	-2.50
	(7.43)	(2.07)	(10.71)	(1.86)	(6.27)	(1.45)
$h = 2$	-1.52	3.56	-2.83	3.39	9.18	-2.75
	(10.85)	(3.35)	(13.54)	(3.14)	(7.85)	(1.71)
$h = 3$	0.03	6.36*	0.36	6.73*	6.80	-3.60
	(12.29)	(3.78)	(15.44)	(3.71)	(9.89)	(2.50)
$h = 4$	5.45	7.80*	4.61	7.32	5.89	-3.65
	(14.29)	(4.65)	(16.79)	(4.52)	(10.76)	(3.04)
$h = 5$	-5.17	10.55**	-8.61	5.97	5.72	-5.51
	(15.67)	(5.11)	(19.04)	(4.96)	(13.07)	(3.93)
$h = 6$	-14.58	7.24	-12.22	3.51	0.76	-5.29
	(18.83)	(5.19)	(20.72)	(5.51)	(14.23)	(4.40)
$h = 7$	-16.41	9.50	-12.11	3.79	-5.27	-5.78
	(18.12)	(6.32)	(20.50)	(5.68)	(14.90)	(4.50)
$h = 8$	-15.10	3.53	-8.29	2.42	-4.84	-3.71
	(18.49)	(6.64)	(20.53)	(6.14)	(15.95)	(4.88)
Country FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES

Table 13 complements the analysis in section 4.1 in the main body of the paper, focusing specifically on the effects of loan-to-value ratio limits. Column (1) documents the $\hat{\delta}^h$ estimates presented in left-hand side of figure 4 for house prices. Columns (2)-(7) present the robustness of these estimates to the inclusion of additional interaction terms, designed to capture potentially competing hypotheses.

Table 13: Interaction coefficient estimates $\hat{\delta}^h$ from regression (2) for house prices using loan-to-value ratio limit prudential policy measures in recipient emerging markets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Additional Interaction Variables					
	Baseline	Capital Control	Capital Inflow Control	Credit-to-GDP Growth	FX Regime	Home Own. Share	Country FE
$MP_t^{\$} \times Pru_{i,t-1}$							
$h = 0$	0.24	0.41	0.35	0.16	0.24	0.23	0.46
	(0.28)	(0.35)	(0.32)	(0.35)	(0.28)	(0.25)	(0.34)
$h = 1$	0.66**	1.00**	0.84**	0.45	0.62**	0.62**	0.75
	(0.31)	(0.44)	(0.36)	(0.38)	(0.31)	(0.28)	(0.54)
$h = 2$	0.63	0.82	0.74	0.39	0.60	0.47	0.29
	(0.51)	(0.81)	(0.68)	(0.61)	(0.49)	(0.45)	(0.70)
$h = 3$	0.48	0.69	0.55	0.21	0.44	0.30	-0.22
	(0.82)	(1.29)	(1.09)	(0.90)	(0.75)	(0.75)	(0.96)
Country FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
Country FE $\times MP_t^{\$}$	NO	NO	NO	NO	NO	NO	YES

Notes: $\hat{\delta}^h$, for $h = 1, ..., 8$, coefficient estimates from various specifications of regression (2) designed to account for other potential interactors with monetary policy spillovers. *, ** and *** denote statistically significant coefficient estimates at 10%, 5% and 1% significance levels, respectively, using Driscoll and Kraay (1998) standard errors (reported in parentheses).

Tables 14 and 15 complement the analysis in section 4.2 in the main body of the paper, focusing specifically on the effects of reserve requirements. Column (1) of each table documents the $\hat{\delta}^h$ estimates presented in figure 5 for total credit and bank credit, respectively. Columns (2)-(7) present the robustness of these estimates to the inclusion of additional interaction terms, designed to capture potentially competing hypotheses.

Table 14: Interaction coefficient estimates $\hat{\delta}^h$ from regression (2) for total credit using reserve requirement prudential policy measures in recipient emerging markets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Additional Interaction Variables					
	Baseline	Capital Control	Capital Inflow Control	Credit-to-GDP Growth	FX Regime	Home Own. Share	Country FE
$MP_t^{\$} \times Pru_{i,t-1}$							
$h=0$	0.21	0.18	0.19	0.16	0.15	0.21	0.20
	(0.17)	(0.18)	(0.17)	(0.17)	(0.15)	(0.16)	(0.13)
$h=1$	0.32	0.23	0.27	0.21	0.33	0.34	0.37
	(0.32)	(0.32)	(0.31)	(0.31)	(0.28)	(0.31)	(0.26)
$h=2$	0.54	0.43	0.48	0.38	0.58	0.57	0.61*
	(0.43)	(0.42)	(0.41)	(0.41)	(0.38)	(0.42)	(0.36)
$h=3$	0.89*	0.85*	0.88*	0.70*	1.04**	0.91*	0.89**
	(0.49)	(0.48)	(0.47)	(0.41)	(0.45)	(0.47)	(0.44)
$h=4$	1.17*	1.14*	1.15*	0.97*	1.27**	1.16*	1.04*
	(0.62)	(0.62)	(0.60)	(0.53)	(0.57)	(0.61)	(0.53)
$h=5$	1.58**	1.54**	1.58**	1.35**	1.75***	1.59**	1.85***
	(0.70)	(0.60)	(0.62)	(0.66)	(0.65)	(0.69)	(0.62)
$h=6$	1.19*	1.22**	1.27**	0.96	1.31**	1.18*	1.58**
	(0.68)	(0.57)	(0.61)	(0.64)	(0.60)	(0.66)	(0.71)
$h=7$	1.22	1.27**	1.32*	0.96	1.37*	1.22	1.99**
	(0.78)	(0.63)	(0.68)	(0.75)	(0.73)	(0.77)	(0.87)
$h=8$	0.66	0.73	0.76	0.40	0.69	0.63	0.96
	(0.83)	(0.78)	(0.80)	(0.76)	(0.78)	(0.82)	(1.02)
Country FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
Country FE $\times MP_t^{\$}$	NO	NO	NO	NO	NO	NO	YES

Notes: $\hat{\delta}^h$, for $h = 1, ..., 8$, coefficient estimates from various specifications of regression (2) designed to account for other potential interactors with monetary policy spillovers. *, ** and *** denote statistically significant coefficient estimates at 10%, 5% and 1% significance levels, respectively, using Driscoll and Kraay (1998) standard errors (reported in parentheses).

Table 15: Interaction coefficient estimates $\hat{\delta}^h$ from regression (2) for bank credit using reserve requirement prudential policy measures in recipient emerging markets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Additional Interaction Variables					
	Baseline	Capital Control	Capital Inflow Control	Credit-to-GDP Growth	FX Regime	Home Own. Share	Country FE
$MP_t^{\$} \times Pru_{i,t-1}$							
$h = 0$	0.17	0.15	0.15	0.14	0.11	0.17	0.12
	(0.14)	(0.15)	(0.14)	(0.13)	(0.12)	(0.13)	(0.11)
$h = 1$	0.26	0.22	0.23	0.19	0.28	0.28	0.26
	(0.26)	(0.27)	(0.25)	(0.24)	(0.23)	(0.25)	(0.23)
$h = 2$	0.55	0.51	0.53	0.44	0.58*	0.56	0.56
	(0.37)	(0.39)	(0.37)	(0.34)	(0.33)	(0.36)	(0.34)
$h = 3$	0.91*	0.97**	0.97**	0.80*	1.04**	0.92**	0.86**
	(0.46)	(0.48)	(0.46)	(0.41)	(0.41)	(0.44)	(0.39)
$h = 4$	1.04*	1.11*	1.09*	0.93*	1.08**	1.01*	0.84*
	(0.57)	(0.60)	(0.57)	(0.53)	(0.50)	(0.56)	(0.48)
$h = 5$	0.82	0.92	0.93	0.72	0.90*	0.80	0.71
	(0.61)	(0.64)	(0.63)	(0.57)	(0.53)	(0.60)	(0.58)
$h = 6$	0.55	0.68	0.69	0.48	0.61	0.52	0.44
	(0.65)	(0.69)	(0.68)	(0.61)	(0.57)	(0.64)	(0.71)
$h = 7$	0.51	0.66	0.67	0.47	0.60	0.49	0.43
	(0.69)	(0.74)	(0.71)	(0.66)	(0.61)	(0.68)	(0.83)
$h = 8$	0.39	0.55	0.54	0.36	0.40	0.34	0.18
	(0.73)	(0.77)	(0.74)	(0.70)	(0.65)	(0.73)	(0.97)
Country FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
Country FE $\times MP_t^{\$}$	NO	NO	NO	NO	NO	NO	YES

Notes: $\hat{\delta}^h$, for $h = 1, ..., 8$, coefficient estimates from various specifications of regression (2) designed to account for other potential interactors with monetary policy spillovers. *, ** and *** denote statistically significant coefficient estimates at 10%, 5% and 1% significance levels, respectively, using Driscoll and Kraay (1998) standard errors (reported in parentheses).

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