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Bank regulations and surges and stops in credit: panel evidence ◊

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Abstract

Prudential bank regulation complicates monetary policy because it has cyclical effects on the growth of bank credit, which can be a channel through which monetary policy is transmitted to the economy. Employing a panel of 36 advanced and developing countries for the period 1973-2015, we find that changes in regulation have been procyclical with respect to credit surges and counter-cyclical with respect to credit stops. As a result, monetary policy risks being too loose over the credit cycle. The economic effects of prudential regulation on credit have been meaningful given the large-scale bank liberalizations undertaken by many countries. The result is robust to dealing with a wide range of sensitivity tests and endogeneity concerns. In designing regulations, policymakers need to consider transmission mechanisms through which they might affect the real economy and undermine the goals of monetary policy.

Keywords: Prudential regulation; credit surge; credit stop, probit, endogeneity

JEL classification : E42, E51, G28

1. Introduction

Does prudential bank regulation help or hinder monetary policy's macroeconomic stabilization role? This is an important issue for monetary policymakers who have had to face frequent bouts of prudential policy reregulation in recent decades (Yellen, 2017; BIS, 2015). However, studies of how bank regulation impacts monetary policymaking are relatively scarce. In this paper we focus on the link between prudential bank regulations and surges and stops in the growth of bank credit to the private sector, the stabilization of which has been viewed traditionally as a function of monetary policy. Indeed, the traditional view of economists (at least in the USA) has been that prudential regulation is largely unnecessary, serves governments' social agendas and is irrelevant for monetary policy, which can stabilize prices and real activity through central bank open-market operations (e.g., Friedman, 1960; Goodfriend and King, 1988; Benson and Kaufman, 1996).

In relating prudential regulation to monetary policy, we draw on two strands of literature. One strand examines links between financial liberalization and the probability of future banking crisis (e.g., Demirgüç-Kunt and Detragiache, 1998; Rancière et al., 2008a; Angkinand et al., 2018; Saka et al., 2020), and typically posits a causal link from liberalization to crisis at low levels of financial development. A second strand of the literature examines the macroeconomic covariates of credit booms (Gourinchas et al., 2001; Tornell and Westermann, 2002; Ingan et al., 2011; Claessens et al., 2012) without examining specifically the role of prudential regulation. Our contribution is to link changes in an array of prudential regulations to surges and stops in bank credit, to show that liberalization effects credit surges and stops to different degrees, and to make clear that regulatory changes need to be factored into monetary policy settings.

We focus on credit surges and stops for three reasons. First, bank credit can be a channel through which monetary policy is transmitted to the broader economy with a strand of literature in the "bank lending view" finding evidence that changes in lending contribute to the transmission of monetary shocks (e.g., Kashyap and Stein, 1995, 2000; Boivin et al., 2010). Accordingly, to the extent that prudential regulations impact credit growth, they may also affect the transmission of monetary shocks to the economy. Second, understanding the causes of surges and stops in bank credit is important because of their relative frequency and because they have been shown to have real effects, including on output growth (Claessens et al., 2012), productivity and wage growth (Franklin et al., 2020), and long-run economic performance (Rancière et al., 2008b). They are also sometimes associated with banking crises (Schularick and Taylor, 2012), deep recessions followed by slow recoveries (Reinhart and Rogoff, 2011; Gourinchas and Obstfeld, 2012), and stock market bubbles (Allen and Gale, 2000). Third, the focus of monetary policy is still mainly on policy interest rates and that of prudential policy on the soundness of individual banking institutions, notwithstanding the steps taken by many countries after the 2008 financial crisis to strengthen bank capital and introduce macroprudential tools (see, e.g., Cecchetti, 2018; Borio et al., 2020). The impact of the main pillars of prudential regulation more broadly on credit developments remains largely ignored by monetary policymakers.

Studies of the impact of prudential regulation on bank credit developments have been confined mainly to the role of external capital account liberalization (e.g., Tornell and Westermann, 2002; Magud et al., 2014), bank capital regulations (e.g., Repullo and Suarez, 2013; Brei and Gambacorta, 2016), interactions with macroprudential policy (e.g., Bussière et al., 2021;

Vollmer, 2022) and the removal of geographic limits on banks' activities (Lakdawala et al., 2021). In this paper, we focus on the full gamut of prudential regulations and their evolution over time to examine how they have affected credit surges and stops. We expect the liberalization of prudential regulations to support a surge in credit supply generally, for example, by removing administrative controls on interest rates, lowering barriers to entry, and eliminating financial frictions from informational efficiencies that relax credit constraints on firms (e.g., Leaven, 2003), and by promoting business optimism such that credit constrained firms try to expand capacity in anticipation of increased future demand for their outputs (e.g., Tornell and Westermann, 2002). By the same token, we expect credit stops to be less prevalent in more liberalized financial systems given that they should be better placed to support credit supply.[1]

To examine the link between prudential regulation and bank credit we construct a panel that brings together data on surges and stops in bank credit that we identify using a methodology developed by Forbes and Warnock (2012), and data on the different pillars of prudential regulation assembled by Abiad et al. (2010) and Denk and Gomes (2017). The panel comprises data for 36 advanced and developing economies over the period 1973-2015 that we employ to estimate the unconditional probability of a surge or stop in bank credit in response to changes in prudential regulations. Over that period, prudential regulation in most countries was reduced markedly as they liberalized their financial systems.

[1] Of course, more liberalized financial systems might not be better placed to sustain credit during difficult times if governments adopt financially repressive policies by means other than prudential regulation. For example, Becker and Ivashina (2017) and Ongena et al. (2019) show that some governments used moral suasion, government owned banks, and their influence over bank directors to push banks into investing in government bonds during the European sovereign debt crisis, which crowded out bank lending to the corporate sector.

Consistent with our hypothesis, we find that changes in prudential regulations had a statistically significant impact on credit flows with liberalization increasing the likelihood of a credit surge and reducing the likelihood of a credit stop. This is especially the case with respect to liberalizations of controls on credit, interest rates and securities markets. Moreover, liberalization impacts credit surges and stops differently, acting in a procyclical manner with respect to surges and in a counter-cyclical manner with respect to stops. Our baseline result is that the marginal effect of a 1% increase in the liberalization of regulations is to increase the probability of a credit surge by about 0.4% and to reduce the probability of a credit stop by about 0.3%. In economic terms, the effect is relatively large in the context of the major reforms undertaken by many countries in our sample, which reduced the indicator of overall regulation by an average of about 59 percentage points over the sample period.

Our results suggest that changes in prudential regulations have a causal impact on credit growth. This finding has important implications for monetary policy given that bank credit is a channel through which such policy actions are transmitted to the real economy. In particular, our finding that the impact of regulations is procyclical with respect to credit surges (i.e., they are likely to exacerbate surges) and they are counter-cyclical with respect to credit stops (i.e., they are likely to alleviate stops) means that monetary policy risks being too loose over the credit cycle overall. In other words, unless the impact of prudential regulations is considered when setting monetary policy, bank credit may grow faster than desired during a period of policy easing and not as slow as desired during a period of policy tightening. Consistent with the bank lending view of the monetary transition mechanism, a policy error of this type is likely to accentuate the adverse real effects of credit surges and stops. Our key results are robust to a wide

array of checks including to, among others, alternative methodologies for calculating surges and stops in bank credit, an alternative methodology for calculating overall prudential regulation, alternative estimation techniques, and employing instrumental variables to deal with potential endogeneity issues.

Our paper makes several contributions to the literature. First, as noted, a related literature has focused largely on the role of prudential regulation as a determinant of banking crises, or on the macroeconomic covariates of credit booms. In contrast, by linking prudential regulation directly to surges and stops in the growth of bank credit, we contribute to the small literature on the relevance of regulations for monetary policy that to date has focused largely on the impacts of external capital controls, bank capital regulation, macroprudential policy, and geographic limits on banks' activities. We show that prudential regulation has procyclical effects on credit surges and counter-cyclical effects on credit stops that need to be taken account of when setting monetary policy. Second, we contribute to the literature on credit boom identification that has relied mainly on using Hodrick-Prescott (HP) filters to compare a country's credit-to-GDP ratio or real credit per capita to its nonlinear trend (e.g., Mendoza and Terrones, 2008, 2012; Gourinchas, et al., 2001; Tornell and Westermann, 2002). In contrast, we identify episodes of credit surges and credit stops employing a methodology developed by Forbes and Warnock (2012) to explain similar developments in US international capital flows that overcomes a shortcoming of the HP approach. With this methodology, we identify 162 credit surge and 142 credit stop episodes in our panel that we employ to examine how they are impacted by changes in an array of prudential regulations.

Third, we contribute to the literature on the causes of credit booms, which have been shown to include capital inflows (e.g., Lane and McQuade, 2014; Magud et al., 2014), capital account liberalizations (Dell'Ariccia et al., 2016), productivity gains (Gorton and Ordoñez, 2020), financial reforms (Angkinand et al., 2010; Mendoza and Terrones, 2012), and financial deepening (Rousseau and Wachtel, 2017). We add to this literature by showing that changes in a broad range of prudential regulations have been an important contributor to credit surges and stops. Fourth, our paper is related to the recent literature on the effects of macroprudential regulation on the financial sector (e.g., Meuleman and Vander Vennet, 2020; Akinci and Olmstead-Rumsey, 2018; Altunbas et al., 2018; Cerutti et el., 2017). We expand this literature by focusing on the full range of prudential regulations and their evolution to show that they contribute to surges and stops in bank credit. Finally, our paper is related to the literature on the effects of financial liberalization more generally, which has been found to trigger banking and financial market crises (Kaminsky and Reinhart, 1999; Angkinand et al., 2018), lead to risky behavior by banks (Cubillas and González, 2014), and result in boom-bust cycles in economies with credit restrictions and overall imperfections in financial markets (Tornell and Westermann, 2002). We contribute to this literature by showing that the liberalization of prudential regulations increases the likelihood of a credit surge and reduces that of a credit stop, with the latter suggesting that more liberalized banking systems are better able to sustain credit growth.

The rest of the paper is organized as follows. Section 2 presents our model and describes the data. Section 3 discusses the empirical results and their sensitivity to a series of robustness tests, and section 4 concludes.

2. Model and data

We estimate a panel probit regression that links bank regulations and a set of control variables to the likelihood of a sudden surge or stop in bank credit to the private sector for the period 1973-2015. The baseline model is as follows:

$$P(y_{it} = 1|X) = \Phi(\beta_1 Regulation_{i,t} + \beta_2 X_{i,t-8} + \varepsilon_{i,t}) \quad (1)$$

The dependent variable y_{it} is an episode dummy variable equal to one if country i is experiencing a credit surge or stop (zero otherwise) in quarter t, $Regulation_{i,t}$ is an index of prudential regulations and $X_{i,t-8}$ is a vector of factors that have been shown to impact on developments in bank credit and that is lagged by eight quarters to help deal with endogeneity concerns.

The data on bank credit to the private sector are quarterly series from the Bank for International Settlements that we express in 'real" terms using national consumer price indices as deflators. Standard approaches in the literature to identify a credit surge are to estimate the percentage deviation with respect to its trend of either the ratio of nominal credit to nominal GDP (e.g., Gourinchas et al., 2001), or the logarithm of real credit per capita (e.g., Mendoza and Terrones, 2008) where the long-run trend is estimated using an expanding HP filter and to define a credit surge when the deviation from the trend exceeds a given threshold. However, Hamilton (2018) shows that the HP filter introduces spurious dynamic relations in the data—in particular, filtered values at the end of the sample are very different from those in the middle. To avoid this

shortcoming of the HP filter we identify episodes of credit surges and stops employing the methodology developed by Forbes and Warnock (2012) to explain similar patterns in capital flows.

Following the methodology of Forbes and Warnock (2012), we define C_t as the 4-quarter moving sum of bank credit to the private sector and compute annual changes in C_t as:

$C = \sum_{i=0}^{3} Credit_{t-i}$ with t = 1, 2, …, N and

$\Delta C_t = C_t - C_{t-4}$ with t = 5, 6, …, N.

We then compute the rolling means and standard deviations of ΔC_t over the last 5 years and define a credit surge as starting the first month t that ΔC_t increases more than one standard deviation above its rolling mean; the end of the surge episode takes place once ΔC_t falls below one standard deviation above its mean. For an entire period to be considered a surge episode, there must be at least one quarter t when ΔC_t increases at least two standard deviations above its mean. Conversely, a credit stop episode is a period when credit growth falls one standard deviation below its mean, provided it reaches two standard deviations below at some point. The stop ends when bank credit is no longer at least one standard deviation below its mean. Table 1 lists the number of credit surge and stop episodes and the average length of each episode generated by this methodology for each of the countries in our sample. The number of credit surges totaled 162 and averaged 5.5 quarters, with surges occurring most frequently in Belgium, Canada, Spain, Austria and Portugal. The number of credit stops was somewhat less at 142 and

they averaged 5.6 quarters, with stops most frequent in New Zealand, South Africa, and Portugal. The methodology indicates that only Germany, Sweden and Switzerland managed to avoid sudden surges or stops in bank credit over the sample period.

To measure prudential bank regulations, we draw on Abiad et al. (2010) and Denk and Gomes (2017) who construct indices for seven pillars of regulation that sum to a single general indicator. The pillars are credit controls and reserve requirements, interest rate controls, banking sector entry, capital account transactions, bank privatizations, liberalization of securities markets and banking sector supervision and capital regulation. For each pillar, a country receives a score on a graded scale, with zero corresponding to "fully repressed," one to "partially repressed," two to "largely liberalized," and three to "fully liberalized." Denk and Gomes (2017) apply the same methodology to extend the Abiad et al. (2010) dataset through to 2015 for 43 OECD and G20 countries.[2] We examine the effects of the single general indicator of regulation on bank credit surges and stops, as well as the effect of each of the regulatory pillars.

In terms of the path of prudential regulation over our sample period, all countries liberalized, and many did so to a substantial degree. For example, normalizing the values of the general indicator so that the country scores lie between 0 (full regulation) and 1 (full liberalization), the average change over the sample was about 59 percentage points, with many countries (e.g., Australia, Chile, Czech Republic, France, Greece, Italy, Israel, and New Zealand) increasing their scores by much more. Table 2 shows that most liberalizations took place in the 1980s and 1990s and that by 2015 17 countries had scores of 0.95 percent or more and four

[2] Difficulties in obtaining consistent quarterly data on bank credit to the nonbank private sector limit our sample to the mix of 36 advanced and developing countries listed in Table 1. Our sample period ends in 2015 because of the lack of a consistent cross-country series on bank prudential regulations beyond that year.

countries (India, Argentina, China and Turkey) had scores below 0.60. Where countries were reluctant to liberalize, this was mainly with respect to controls on bank credit, reserve requirements, and international capital flows.

We include six control variables in the vector $X_{i,t}$. The coarse grid categorization of the flexibility of the exchange rate regime (1 = least flexible; 5 = most flexible) developed by Reinhart and Rogoff (2004) is included because of the regime's importance in the international transmission of financial shocks (Obstfeld et al., 2018) and because it impacts on the volume of capital flows that affect the supply of loanable funds (Gourinchas and Obstfeld, 2012). We expect more flexible exchange rate regimes to mitigate both credit surges and stops. Real GDP growth and inflation are included because a better economic environment and over-optimism can promote the build-up of a credit boom (Baron and Xiong, 2017; Mendoza and Terrones, 2008, 2012). Thus, we expect credit surges to be associated with high GDP growth and low inflation (good times and optimism) and credit stops to be associated with lower GDP growth and higher inflation (bad times and pessimism). The short-term interest rate is included to capture the effects of monetary policy, where credit surges and stops are likely to be associated with lower and higher interest rates, respectively, at least in the initial stages of each episode (Mendoza and Terrones, 2008). Financial deepening, measured as the ratio to GDP of bank credit to the private sector, is included to capture the level of financial development (Rousseau and Wachtel, 2017), with studies suggesting that credit surges and stops may be associated with greater financial depth (e.g., Tornell and Wetermann, 2002). Finally, we include a dummy variable equal to one (zero otherwise) if a country experiences a systemic banking crisis, for which we rely on the

crisis episodes identified by Nguyen et al. (2022).[3] The literature suggests that many but not all credit surges end in banking crises (Tornell and Wetermann, 2002; Schularick and Taylor, 2012); as such we would expect at least credit stops to be associated positively with banking crisis. Variable definitions and data sources are given in Table 3 and summary statistics for credit episodes, the prudential regulation indicators, and the control variables are shown in Table 4.

3. Empirical results

3.1 Baseline results

Table 5 reports the results from estimating Equation (1) for the single general indicator of regulation. The results in column (1) of panel A show that more liberalized banking systems (reflecting higher bank regulation scores) are associated with an increased probability of a credit surge, with the effect statistically significant at the 1% level. Column (2) adds the control variables. The coefficient on bank regulation remains statistically significant and positive and the size of the coefficient is substantially larger. Columns (3) and (4) report the results for credit stops. In this case, liberalization reduces the probability of a credit stop, with the effect statistically significant at the 1% level.

[3] Nguyen et al. (2022) rely on the Laeven and Valencia (2020) definition of a systemic banking crisis as an event in which there are significant signs of financial distress in the banking system, as reflected by significant bank runs, losses in the banking system, and/or bank liquidations; and there are significant government policy interventions in response to large losses in the banking sector.

As the coefficient estimates in probit cannot be interpreted directly, panel B of the table reports the average marginal effects.[4] These suggest that a 1% increase in the prudential regulation indicator (i.e., a liberalization) increases the probability of a credit surge by about 0.4% and reduces the probability of a credit stop by about 0.3%. While small in absolute terms, these effects are large in the context of the liberalizations that took place over the sample period with countries increasing their liberation scores by, on average, 22 percentage points in the 1980s and again in the 1990s (Table 2).

The control variables indicate that the probability of a credit surge is associated with lower rates of GDP growth, which is contrary to our expectation. A possible explanation is that at times of rapid credit growth much of the credit is channeled into unproductive activities. For example, Loayza and Rancière (2006) and Dell'Ariccia and Marquez (2006) argue that rapid credit expansion might be associated with reduced incentives for banks to screen applicants, which leads to a deterioration in bank portfolios, financial instability, and output losses.[5] Relatedly, Cecchetti and Kharroubi (2019) report that credit growth disproportionately harms output per worker in industries that are R&D intensive, which they interpret as indicative of increasingly indebted entrepreneurs turning to safer, lower return projects, which reduces aggregate productivity growth. On the other hand, credit stops are associated with less deep financial systems, less flexible exchange rate regimes, and banking crises, which is broadly in line with what would be expected from the literature (e.g., Tornell and Westermann, 2002; Magud and Vesperoni, 2015).

[4] Average marginal effects are computed as averages of individual marginal effects. The standard errors of marginal effects are calculated using the delta method (Cameron and Trivedi, 2005).

[5] Similarly, Gopinath et al. (2017) and García-Santana et al. (2020) have found that the credit and asset price boom in Southern Europe that preceded the global financial crisis coincided with a rise in capital misallocation.

3.2 Results for different regulatory pillars

Results for the impact of changes in each regulatory pillar on credit surges and credit stops are reported in Tables 6 and 7, respectively. They show that liberalizations in each pillar are associated with an increased probability of a credit surge and a reduced probability of a credit stop, with the coefficients statistically significant at the 1% level in each case. These results are broadly consistent with the banking literature. For example, credit controls are associated with a reduced credit supply because they force borrowers to rely on more expensive and less convenient substitutes where they exist (Davis et al., 1971); interest rate controls can reduce credit supply by lowering lending spreads and by shifting credit away from the private sector to the public sector (Alper et al., 2020; Koch, 2015); and restrictions on capital flows can reduce the supply of loanable funds (e.g., Gourinchas and Obstfeld, 2012). The signs on the coefficients for entry barriers are consistent with studies reporting that lower barriers increase credit supply (Carlson et al., 2022) and that high entry costs and bank concentration decrease the likelihood of receiving bank finance (Beck et al., 2004; Cetorelli and Strahan, 2006). Thus, lower entry barriers can support credit growth and mitigate credit stops. The results for bank privatization are consistent with studies reporting that privatizations facilitate credit growth (Berkowitz et al., 2014; Bertay et al., 2020). The liberalization of bank supervision refers to factors such as less stringent bank capital regulation and less monitoring of bank activities through audits and sanctions for prudential purposes. Some research suggests that stricter supervision can help boost lending, for example, by overcoming frictions in bank management (e.g., Granja and Leuz, 2017); however, our results support studies suggesting that strong supervision constrains bank

lending (Peek and Rosengren, 1995; Kupiec et al., 2017). Finally, the finding that less regulated securities markets increase the probability of credit surges and mitigate credit stops supports studies reporting that the development of these markets complement rather than replace bank lending (Demirgüç-Kunt and Levine, 1996; Levine and Zervos, 1998).

The largest marginal effects (reported in panel B of both tables) are from the liberalization of controls on interest rates and securities markets in the case of credit surges (a 1% increase in liberalization scores increasing the likelihood of a surge by about 0.6% and 1%, respectively) but are broadly the same across the different regulatory pillars in the case of credit stops (a 1% increase in liberalization scores reducing the probability of a credit stop by about 0.4%). The coefficients on the control variables suggest that credit surges are less probable at lower rates of GDP growth, when exchange rates are more flexible, and at times of banking crisis; and they are more probable at higher rates of inflation and when policy interest rates are increasing (Table 6). Credit stops are less likely when inflation and interest rates are falling, when exchange rates are more flexible and when financial systems are deep; and they are more likely at times of banking crises (Table 7).

3.2 Robustness tests—alternative measures of credit surges and stops

As discussed above, there are other approaches to identifying credit surges and stops in the literature and we examine the robustness of our results to employing two of the more common approaches. The first of these is that of Gourinchas et al. (2001). These authors estimate the percentage deviation from its trend of the ratio of nominal credit to nominal GDP,

where the trend is estimated using an expanding HP filter (λ = 100) and is then compared to a surge threshold. A credit surge takes place when the deviation of the ratio from the trend exceeds a given threshold:

$$\frac{Credit/GDP_{i,t} - Credit/GDP_{i,t}^{Expanding\ HP}}{Credit/GDP_{i,t}^{Expanding\ HP}} \geq \varphi^*$$

The peak of a credit surge takes place at the quarter of the largest deviation and the starting date is one quarter before the peak at which the deviation exceeds a limit threshold. The end of the surge is one quarter after the peak where the deviation is larger than the limit threshold. We follow Gourinchas et al. (2001) and set φ^* at 0.195 and the limit threshold at 0.05 and test the robustness of our results to this methodology for defining credit surge and stop episodes.

A second approach is that of Mendoza and Terrones (2008). In their procedure a credit surge is defined as an episode in which the amount of credit to the private sector exceeds the typical expansion over the business cycle. They estimate the deviation from the long-run trend and its corresponding standard deviation for the logarithm of real credit per capita, with the long-run trend also estimated using the HP filter (setting λ = 100). On their definition, a credit surge takes place when the deviation from the trend exceeds the usual deviation by a factor of at least φ:

$$Credit_{i,t} \geq \varphi\sigma(Credit_{i,t}^{HP})$$

We follow Mendoza and Terrones (2008) and set $\varphi = 1.65$ and test the robustness of our results to this methodology.[6]

Probit results employing these two methodologies for identifying credit surges and stops are reported in Table 8. The results are broadly in line with the baseline estimates with the coefficients on bank regulation statistically significant and positive in the case of credit surges and statistically significant and negative in the case of stops. In addition, the size of the coefficients in each case is not far removed from those reported in our baseline estimates. A notable difference in the alternative estimates, however, is that the coefficients on the control variables perform somewhat better with regard to their statistical significance.

3.3 Robustness tests—additional controls

In this subsection we examine the robustness of our baseline results to additional control variables. Our first test is with respect to the measure of total prudential regulation. The overall indicator developed by Abiad et al. (2010) and Denk and Gomes (2017) is calculated by adding the scores from a graded scale for each of the seven pillars that runs from "fully repressed" to "fully liberalized". As an alternative to this additive approach, we test the robustness of the results to an overall indicator computed by applying Principal Components Analysis to the seven pillars. These results are reported in columns (1) and (2) of Table 9. The baseline conclusion that financial regulation has a positive impact on credit surges and a negative impact on credit stops still holds, with the coefficients both statistically significant at the 1 percent level. The sizes of

[6] In fact, Gourinchas et al. (2001) and Mendoza and Terrones (2008) focus on identifying credit surges only. Mendoza and Terrones (2008) provide a detailed comparison of the two approaches.

the coefficients differ somewhat compared to the baseline (smaller in the case of surges but larger in the case of stops) but the result that prudential regulation is an important determinant of credit surges and stops is upheld strongly.

It might be expected that the effects of prudential regulations would take some time to have an impact on credit flows and real economic activity. Accordingly, as a second robustness test we examine the sensitivity of the baseline results to the inclusion of lagged values of regulation. Mindful of our relatively limited sample period, we add lags in regulation of three years to the baseline specification. These results are reported in columns (3) and (4) of Table 9. The coefficients on lagged regulation are statistically significant and positive in the case of credit surges and statistically significant and negative in the case of stops. This is consistent with our baseline results and confirms that the liberalization of prudential regulations impacts bank credit developments over time.

Our third robustness test is to examine the sensitivity of the results to a country's level of income. There are at least two reasons why this might be the case. First, institutional developments may determine the impact of regulations on economic outcomes. In particular, if institutions are weaker in lower income countries, this might be associated with regulatory frameworks characterized by limited capacity, poor commitment, and lack of accountability (Estache and Wren-Lewis, 2009). This could apply to the prudential regulatory framework for banks and other financial institutions and effect credit growth. Second, there is debate as to whether the monetary policy transmission mechanism is weaker in lower-income countries (e.g., Mishra et al., 2012; Mishra and Montiel, 2013; Brandao-Marques et al., 2020) because of their

less developed credit markets, which might be expected to impact the relationship between prudential regulations and credit growth. To test the robustness of our results to country income levels we add to the baseline specification an interaction term whereby regulation is interacted with a dummy variable that takes the value of 1 for countries in the sample whose GDP per capita is above that of the sample mean and zero otherwise. These results are reported in columns (5) and (6) of Table 9. The interaction term is statistically significant and positive in the case of credit surges and statistically significant and negative in the case of stops. The coefficients on the regulation variables remain statistically significant for credit surges and stops for both groups of countries.[7] Thus, the impact of prudential regulation on credit surges and stops is partly conditional on a country's level of income: it impacts credit surges and stops in both higher- and lower-income countries but has larger effects on the former group.

Finally, we examine the robustness of the baseline results to alternative estimation methodologies. In particular, the nature of our dependent variable means that a large number of observations are necessarily zero (i.e., in the absence of a credit surge or stop). As such, we present results from Poisson regressions (Santos Silva and Tenreyro, 2006) and rare event logistic regressions (King and Zeng, 2001), which are techniques that are suited to model count data that have an excess of zeros. These results are presented in Table 10 and show that the coefficients on bank regulation remain statistically significant and of the expected sign.[8]

[7] We also tested for differences in effects according to a country's income level by dividing our sample according to whether countries average real GDP per capita is above (higher income) or below (lower income) the mean per capita GDP for the full sample. The results showed that the liberalization of prudential regulation is associated with an increased probability of a credit surge and a reduced probability of a credit stop for both higher and lower income country groups.

[8] As some countries in the sample had periods of exceptionally high rates of inflation (Argentina, Brazil, Chile) we also tested the robustness of the baseline model to excluding high inflation outliers. The results remain robust to this specification and are available from the authors on request.

3.4 Endogeneity issues

Our baseline results may be subject to endogeneity concerns for several reasons. First, a country's experience of credit surges and stops might influence the choice of prudential regulation (i.e., reverse causality). Second, factors that influence prudential regulation may be correlated with credit surges and stops. For example, the macroeconomic environment may simultaneously determine both regulation and credit developments (Evans, 1997). Third, there may be omitted variable bias since we are not controlling for all the determinants of credit surges and stops. To address potential endogeneity, we re-estimate the probability of credit surges and stops employing the IV probit methodology in which we instrument for bank regulation.

For our choice of instruments, we draw on previous studies of the determinants of economic and financial reform. Thus, our instruments include: a dummy variable set to 1 when a country has a left wing government (zero otherwise) and a dummy variable set to 1 for a right-wing government (zero otherwise) on the assumption that right-wing governments are more receptive to market-oriented reforms (Alesina and Roubini, 1992); a dummy variable set to 1 to indicate the incumbent executive's first year in office (zero otherwise) on the basis that new incumbents have an incentive to undertake reforms early to realize their benefits before the next election (Kreuger, 1993); a country's openness to international trade, defined as the ratio of foreign trade to GDP, to capture economic structure influences (Abiad and Mody, 2005); the volatility of natural resource rents because as rents become more volatile the economic outlook becomes more uncertain, which makes countries less prone to undertaking economic reform (Boucekkine et al., 2021); and lagged values of the bank regulation indicator.

The series on oil rents is detrended by applying the approach suggested by Hamilton (2018).[9] We calculate the cyclical oil rent volatility in a given year and country as the standard deviation of a centered four-year window of the de-trended oil rents series (see Boucekkine et al., 2021). The political ideology variables are collected from the World Bank Political Institutions dataset. Annual data for each country for the ratio of foreign trade to GDP and for oil rents are from the World Bank's World Development Indicators database, which we interpolate into quarterly series.

The IV probit results are reported in Table 11. The first stage results are reported in panel B. In columns (1) (surges) and (5) (stops) we include all instruments in the estimations and in columns (2) (surges) and (7) (stops) we include only those instruments that were statistically significant in the initial estimates. The coefficient on the volatility of revenues from oil rents suggests that volatility inhibits regulatory reform. The coefficient on the dummy variable indicating the incumbent government's first year in office suggests that new governments are actually more inclined to delay reforms.[10] The coefficient on lagged financial regulation is statistically significant and positive. In terms of test statistics, the Wald test statistic in each case suggests that we can reject the null hypothesis of no endogeneity, and the Amemiya-Lee-Newey test statistic suggests that we cannot reject the hypothesis that the overidentifiying restrictions are valid.[11]

[9] The Hamilton (2018) procedure calculates the cyclical component based on the 2-year-ahead forecast error.
[10] Haggard and Webb (1993) point out that many new governments, especially in Latin America, pursued expansionary policies in their early days and delayed reform.
[11] See Guevara (2018) for a discussion of the Amemiya-Lee-Newey test.

In the remaining columns we show that the three instruments remain statistically significant when included as the sole determinant of prudential regulation. The coefficients on instrumented regulation are reported in panel A of the table. They are in line with the probit results in that they are statistically significant and positive for credit surges and statistically significant and negative for credit stops. In the case of credit stops, however, the coefficient on instrumented regulation (-0.945 in column (7)) is smaller in absolute value than our baseline probit estimate (-1.919 in column (4) of Table 5), which suggests that the probit estimate of the credit–regulation nexus may be biased upwards due to endogeneity and overstate the extent to which regulation affects credit stops.

The IV probit results for overall regulation and its pillars are reported in Tables 12 and 13 for credit surges and stops, respectively. The key points to note are that the coefficients on regulation are highly statistically significant with signs consistent with the probit results, though the probit IV coefficients and the marginal effects are generally smaller. For example, for overall regulation the marginal effect of a 1% increase in regulatory reform increases the probability of a credit surge by about 0.01% (column (1), Table 12) and reduces the probability of a credit stop by about 0.09% (column (1), Table 13). Though smaller than the probit coefficients, the economic effects of regulatory changes remain meaningful in the context of the large-scale liberalizations undertaken. For example, the overall indicator of regulation fell by an average of 21 percentage points in the 1980s and by a further 23 percentage points in the 1990s. The coefficients for the control variables in these estimates are broadly consistent with our earlier findings.

4. Conclusions

In this paper we examined the link between prudential bank regulation and surges and stops in bank credit to the private sector. We employed a panel comprising data for 36 advanced and developing economies over the period 1973-2015 to estimate the unconditional probability of a surge or stop in bank credit in response to changes in regulations. Over the sample period regulation in most countries was reduced markedly as countries liberalized their banking systems. We found that the liberalization of regulations had a statistically significant causal impact on credit flows, increasing the likelihood of a credit surge and reducing the likelihood of a credit stop. Moreover, the change in regulation impacted credit surges and stops differently with the effect of liberalization being to exacerbate credit surges and to alleviate credit stops. In other words, the effects of regulations are procyclical with respect to credit surges and counter-cyclical with respect to credit stops. These results hold for overall regulation and each of the main regulatory pillars and are robust to a wide array of sensitivity checks, including alternative methodologies for calculating surges and stops in bank credit, an alternative methodology for calculating overall regulation, country income levels, and employing instrumental variables to deal with potential endogeneity issues.

Our results have important implications for monetary policy since credit growth is a channel through which monetary policy actions are transmitted to the real economy. They suggest that unless the impact of prudential regulations is considered when setting monetary policy, the policy risks being too loose over the credit cycle overall—i.e., credit will grow faster than desired during a period of policy easing and not as slowly as desired during a period of

policy tightening. This is especially the case as regards changes to prudential regulations affecting controls on credit, interest rates and securities markets. This is a potential monetary policy problem for both advanced and developing economies, though our results suggest that the effects of regulations in this regard are larger for advanced economies. Thus, prudential regulation has a much broader role to play in the efficacy of monetary policy than the one typically ascribed to it of ensuring the soundness of individual institutions—its effects also need to be considered if the objectives of monetary policy include stabilizing the credit cycle.

Data availability

The authors will make the data used in this study available on request.

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Table 1
Bank credit surge and stop episodes by country, 1973-2015

Country	Credit surges		Credit stops	
	Number	Average length (quarters)	Number	Average length (quarters)
Argentina	6	3.6	6	5.6
Austria	9	3.9	7	3.7
Australia	1	1.0	1	0.0
Belgium	10	3.3	6	4.7
Brazil	5	2.8	5	5.0
Canada	10	6.0	5	3.0
Chile	2	2.0	0	0.0
China	1	3.0	1	3.0
Czech Republic	1	2.0	1	2.0
Denmark	1	2.0	1	0.0
Finland	1	1.0	1	1.0
France	5	9.0	5	7.0
Germany	0	0.0	0	0.0
Greece	4	7.0	4	8.0
Hungary	2	11.0	3	10.0
Indonesia	5	11.0	6	4.0
Ireland	7	9.4	4	8.2
India	4	12.7	4	7.7
Israel	4	9.5	4	5.5
Italy	6	6.8	5	4.3
Japan	7	5.8	6	5.0
Korea	7	9.8	2	8.5
Luxembourg	1	1.0	2	3.0
Netherlands	6	7.0	3	5.0
New Zealand	4	11.0	9	3.8
Norway	6	7.8	5	6.0
Poland	2	8.0	4	5.0
Portugal	8	6.0	8	4.1
Russia	4	6.7	3	3.7
South Africa	6	7.0	8	5.6
Spain	10	5.5	7	4.4
Sweden	0	0.0	0	0.0
Switzerland	0	0.0	0	0.0
Turkey	4	7.5	3	3.2
United Kingdom	6	5.5	7	5.1
United States	7	4.5	6	5.6
Total /Average length	**162**	**5.5**	**142**	**5.6**

Bank credit refers to bank credit to the non-financial private sector in 2010 prices. The calculation for credit surges and stops is based on the Forbes and Warnock (2012) methodology for surges and stops in international capital flows. See Table 2 for a more detailed explanation.

Table 2
Percentage point changes in overall prudential regulation indicator

	(1)	(2)	(3)	(4)	(5)
	1973-1980	1981-1990	1991-2000	2001-2015	**Memo item**: Index level in 2015
Argentina	0.34	-0.13	0.37	-0.11	0.62
Austria					1.00
Australia	0.00				0.99
Belgium	-0.04	0.20	0.20	0.07	1.00
Brazil	0.14	0.05	0.21	0.05	0.05
Canada	0.05	0.14	0.14	0.00	1.00
Chile		0.18	0.07	0.12	0.86
China		0.10	0.21	0.10	0.40
Czech Republic		0.19	0.54	0.22	0.95
Germany	0.05	0.10	0.10	0.00	0.90
Denmark	0.05	0.45	0.11	0.05	1.00
Finland	0.09	0.19	0.14	0.19	1.00
France	0.02	0.44	0.27	0.00	1.00
Greece	0.05	0.19	0.42	0.14	0.92
Hungary			0.49	0.13	0.95
Indonesia	0.00	0.42	0.05	0.20	0.77
Ireland	0.02	0.29	0.18	-0.05	0.95
Israel	0.00	0.15	0.37	0.18	1.00
India	0.00	0.10	0.33	0.19	0.64
Italy	0.11	0.26	0.41	0.10	1.00
Japan	0.11	0.17	0.29	-0.10	0.76
Korea	0.13	0.19	0.24	0.14	0.82
Luxembourg					1.00
Mexico	0.14	0.04	0.40	0.00	0.86
Netherlands	0.11	0.10	0.05	0.00	0.95
Norway	0.10	0.29	0.18	0.10	0.95
New Zealand	0.23	0.51	0.05	0.05	1.00
Poland				0.12	0.91
Portugal	-0.05	0.42	0.32	0.05	0.86
Russia				0.07	0.77
South Africa	-0.01	0.29	0.21	0.05	0.83
Spain	0.10	0.19	0.32	0.00	1.00
Sweden	0.10	0.38	0.14	0.00	0.95
Switzerland	0.05	0.00	0.10	0.00	0.95
Turkey	0.14	0.30	0.11	0.04	0.59
United Kingdom	0.18	0.31	0.05	-0.10	0.90
United States	0.14	0.10	0.14	0.00	1.00
Mean	0.09	0.21	0.23	0.06	0.87
Maximum	-0.05	-0.13	0.05	-0.11	0.05
Minimum	0.49	0.51	0.54	0.22	1.00
Standard deviation	0.11	0.15	0.14	0.09	0.20

A country receives an indicator score on a graded scale, with zero corresponding to "fully repressed" and three to "fully liberalized." Values of the indicator are normalized so that country scores lie between 0 (full regulation) and 1 (full liberalization). Figures in columns (1) to (4) are percentage point changes in the level of bank regulation; figures in column (5) are the end of sample period level of the overall indicator.
Source: Abiad et al. (2010) and Denk and Gomes (2017).

Table 3
Variable definitions and data sources

Variable	Definition	Source
Credit surges and stops	A surge is defined as starting the first quarter t that the quarterly year-over-year change in bank credit increases more than one standard deviation above its 5-year rolling mean change and the end of the surge occurs once bank credit falls below one standard deviation of its rolling mean. A credit stop occurs when credit growth falls one standard deviation below its 5-year mean and the stop ends when bank credit is no longer at least one standard deviation below its mean. Real bank credit is defined as bank credit to the private sector in local currency at 2010 prices.	Author calculations based on Forbes and Warnock (2012) methodology for surges and stops in international capital flows; BIS bank credit data.
Prudential bank regulations	Indicator constructed on the basis of the sum of indicators of seven pillars of bank-related regulations where each indicator takes the values between 0 and 3. The seven components relate to credit controls, interest rate controls, entry barriers, banking supervision, privatization, international capital flows, and securities markets.	Denk and Gomes (2017); Abiad et al (2010).
GDP growth	Four-quarter growth rate of real GDP	OECD economic indicators database; Federal Reserve Bank of St Louis FRED database; national statistical agencies.
Inflation	Four-quarter growth rate of consumer prices	Bank for International Settlements database; Federal Reserve Bank of St Louis FRED database.
Financial depth	Bank credit to the private sector in per cent of GDP	Bank for International Settlements database; Federal Reserve Bank of St Louis FRED database.
Interest rate	Central bank policy interest rate or short-term money rate	Bank for International Settlements database; central bank websites; International Monetary Fund, International Financial Statistics database.
Exchange rate regime	The Reinhart and Rogoff (2004) coarse grid categorization of exchange rate regimes, which ranges from 1 (least flexible) to 5 (most flexible).	Reinhart and Rogoff (2004)
Banking crisis	Dummy variable equal to 1 in a quarter of systemic banking crisis and zero otherwise.	Based on banking crises identified by Nguyen et al. (2022)

Table 4.
Summary statistics for credit episodes, prudential regulations and control variables

	Observations	Mean	Standard deviation	Minimum	Maximum
Credit surges	188	4.7	3.01	0.0	10.0
Credit stops	164	4.1	2.56	0.0	9.0
All prudential regulations	1,998	0.6	0.30	0.0	1.0
Credit controls	1,998	0.7	0.36	0.0	1.0
Interest rate controls	1,998	0.8	0.37	0.0	1.0
Entry barriers	1,998	0.7	0.37	0.0	1.0
Banking supervision	1,998	0.3	0.78	0.0	1.0
Privatization	1,998	0.5	0.41	0.0	1.0
Capital flows	1,998	0.6	0.41	0.0	1.0
Securities markets	1,998	0.7	0.38	0.0	1.0
GDP growth	1316	0.9	3.09	-33.8	17.2
Inflation	1316	26.5	315.75	-6.9	20262.8
Financial depth	1316	66.6	35.81	2.7	199.5
Interest rate	1316	6.8	0.39	0.4	17.2
Exchange rate regime	1316	2.4	1.29	1.0	6.0
Banking crisis	1316	0.1	35.81	0.0	1.0

Table 5.
Prudential regulation and the probability of experiencing a surge or stop in bank credit: baseline probit results

	(1)	(2)	(3)	(4)
	Credit surges		Credit stops	
Panel A: Probit coefficients				
Prudential regulations	0.714***	1.697***	-0.489***	-1.919**
	(0.100)	(0.352)	(0.074)	(0.634)
GDP growth		-0.078***		-0.030
		(0.016)		(0.047)
Inflation		0.038*		-0.069***
		(0.012)		(0.012)
Financial depth		0.001		-0.002***
		(0.001)		(0.001)
Interest rate		-0.030		0.001
		(0.030)		(0.009)
Exchange rate regime		-0.119		-0.134**
		(0.051)		(0.060)
Banking crisis		-0.400		0.294**
		(0.333)		(0.163)
Constant	0.132***	1.812***	1.064***	-0.925**
	(0.155)	(0.390)	(0.130)	(0.322)
Observations	1998	1316	1998	1316
Time fixed effects	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES
Pseudo R^2	0.492	0.582	0.482	0.598
Panel B: Marginal effects				
Prudential regulations	0.273***	0.408***	-0.144***	-0.256***
	(0.039)	(0.084)	(0.021)	(0.098)
GDP growth		-0.018***		
		(0.007)		
Inflation		-0.014*		-0.020***
		(0.008)		(0.001)
Exchange rate regime				-0.003**
				(0.002)

Probit estimates in which the dependent variable is a 0-1 variable with 1 indicating a credit surge (columns 1 and 3) or a credit stop (columns 2 and 4). Variables are defined in Table 2. Average marginal effects are computed as averages of individual marginal effects. The standard errors of marginal effects are calculated using the delta method (Cameron and Trivedi, 2005). *** and ** indicate statistical significance at the 1 and 5% levels respectively. Robust standard errors in parenthesis.

Table 6
Prudential regulation pillars and the probability of experiencing a credit surge: probit results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Credit controls	Interest rate controls	Entry barriers	Banking supervision	Privatization	International capital flows	Securities markets
Panel A: Probit coefficients							
Prudential regulations	1.718*** (0.144)	2.251*** (0.163)	0.687*** (0.144)	1.229*** (0.199)	0.937*** (0.140)	1.027*** (0.175)	3.704*** (0.259)
GDP growth	-0.087** (0.025)	-0.090** (0.024)	-0.096*** (0.023)	-0.170*** (0.031)	-0.138*** (0.030)	-0.157*** (0.024)	-0.165*** (0.024)
Inflation	0.027** (0.010)	0.009 (0.014)	0.016 (0.014)	0.045*** (0.012)	0.053*** (0.010)	0.022** (0.011)	-0.012 (0.017)
Financial depth	0.001 (0.001)	-0.002 (0.002)	-0.002 (0.003)	-0.000 (0.002)	0.003 (0.004)	-0.003 (0.003)	-0.010 (0.012)
Interest rate	0.015* (0.009)	0.020* (0.010)	0.014* (0.009)	0.016* (0.009)	0.016* (0.009)	0.001 (0.003)	-0.003 (0.012)
Exchange rate regime	-0.182*** (0.047)	0.132** (0.043)	-0.024 (0.086)	-0.106 (0.065)	-0.285*** (0.060)	-0.151*** (0.041)	-0.046 (0.069)
Banking crisis	-0.564*** (0.161)	-0.012 (0.260)	0.067 (0.262)	-0.724*** (0.250)	-0.914*** (0.250)	-0.627*** (0.158)	-0.289 (0.277)
Constant	0.894*** (0.234)	0.743*** (0.223)	0.276 (0.209)	-0.663*** (0.186)	-0.134 (0.198)	0.123 (0.343)	2.381*** (0.154)
Observations	1316	1316	1316	1316	1316	1316	1316
Time fixed effects	YES	YES	YES	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES	YES	YES	YES
Pseudo R^2	0.525	0.525	0.541	0.562	0.612	0.585	0.598
Panel B: Marginal effects							
Prudential regulations	0.474*** (0.040)	0.634*** (0.049)	0.163*** (0.040)	0.107*** (0.041)	0.267*** (0.039)	0.294*** (0.050)	1.023*** (0.076)
GDP growth	-0.024** (0.007)	-0.025** (0.007)	-0.034*** (0.006)	0.008*** (0.002)	-0.012*** (0.001)	-0.044*** (0.007)	-0.045*** (0.006)
Inflation	0.007* (0.002)			0.010*** (0.003)	0.009*** (0.003)	0.006** (0.003)	
Financial depth							
Interest rate	0.010* (0.005)	0.012* (0.007)	0.004** (0.002)	-0.008* (0.003)	0.005* (0.003)		
Exchange rate regime	-0.052*** (0.017)	0.020** (0.010)			-0.036*** (0.001)	-0.028*** (0.010)	

Probit estimates in which the dependent variable is a 0-1 variable with 1 indicating a credit surge. Variables are defined in Table 2. Average marginal effects are computed as averages of individual marginal effects. The standard errors of marginal effects are calculated using the delta method (Cameron and Trivedi, 2005). ***, ** and * indicate statistical significance at the 1, 5 and 10% levels respectively. Robust standard errors in parenthesis.

Table 7.
Prudential regulation pillars and the probability of experiencing a credit stop: probit results

	(1) Credit controls	(2) Interest rate controls	(3) Entry barriers	(4) Banking supervision	(5) Privatization	(6) International capital flows	(7) Securities markets
Panel A: Probit coefficients							
Prudential regulations	-1.207***	-1.291***	-0.812***	-2.385***	-0.354***	-1.162***	-0.567**
	(0.160)	(0.219)	(0.158)	(0.181)	(0.499)	(0.186)	(0.289)
GDP growth	0.012	-0.033	-0.033	0.008	-0.012	-0.033	-0.003
	(0.012)	(0.038)	(0.034)	(0.018)	(0.040)	(0.042)	(0.089)
Inflation	-0.156***	-0.058***	-0.062***	-0.039**	-0.081***	-0.112***	-0.089***
	(0.011)	(0.010)	(0.013)	(0.013)	(0.013)	(0.010)	(0.019)
Financial depth	-0.009***	-0.018***	-0.013***	0.024***	-0.011***	-0.033***	-0.015***
	(0.003)	(0.000)	(0.002)	(0.002)	(0.002)	(0.009)	(0.002)
Interest rate	-0.030**	-0.007	-0.004	-0.023**	-0.017	-0.032**	-0.007
	(0.014)	(0.012)	(0.011)	(0.010)	(0.014)	(0.012)	(0.012)
Exchange rate regime	-0.134**	-0.169***	-0.144**	-0.354***	-0.098*	-0.114*	-0.123**
	(0.045)	(0.032)	(0.059)	(0.078)	(0.057)	(0.058)	(0.062)
Banking crisis	0.455**	0.234	0.198	-0.094	-0.356**	0.271	0.345**
	(0.189)	(0.192)	(0.178)	(0.182)	(0.181)	(0.191)	(0.145)
Constant	1.782***	-1.999***	-0.962***	1.145***	0.471**	1.612***	-0.823*
	(0.243)	(0.398)	(0.212)	(0.234)	(0.229)	(0.253)	(0.545)
Observations	1316	1316	1316	1316	1316	1316	1316
Time fixed effects	YES	YES	YES	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES	YES	YES	YES
Pseudo R^2	0.525	0.536	0.568	0.658	0.514	0.518	0.582
Panel B: Marginal effects							
Prudential regulations	-0.358***	-0.392***	-0.257***	-0.374***	-0.108***	-0.350***	-0.175**
	(0.046)	(0.063)	(0.047)	(0.036)	(0.045)	(0.056)	(0.089)
Inflation	-0.034***	-0.012***	-0.010***	0.007**	-0.021***	-0.033***	-0.021***
	(0.006)	(0.003)	(0.002)	(0.004)	(0.005)	(0.004)	(0.004)
Financial depth	-0.005***	-0.003***	-0.010***	-0.002**	-0.002***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Interest rate	-0.076**			-0.011**		-0.001**	-0.045***
	(0.010)			(0.003)		(0.000)	(0.003)
Exchange rate regime		-0.022***	-0.018**	-0.049***	-0.045*	-0.009*	-0.019**
		(0.008)	(0.008)	(0.009)	(0.028)	(0.004)	(0.010)

Probit estimates in which the dependent variable is a 0-1 variable with 1 indicating a credit stop. Variables are defined in Table 2. Average marginal effects are computed as averages of individual marginal effects. The standard errors of marginal effects are calculated using the delta method (Cameron and Trivedi, 2005). ***, ** and * indicate statistical significance at the 1, 5 and 10% levels respectively. Robust standard errors in parenthesis.

Table 8
Prudential regulation and the probability of a credit surge or stop—alternative measures of credit surges and stops: probit results

Methodology:	Gourinchas et al. (2001)		Mendoza and Terrones (2008)	
	(1)	(2)	(3)	(4)
	Credit surge	Credit stop	Credit surge	Credit stop
Panel A: Probit coefficients				
Prudential regulations	1.770***	-1.906***	1.607***	-1.703
	(0.361)	(0.164)	(0.352)	(0.339)
GDP growth	-0.021	0.030	-0.078	0.056
	(0.047)	(0.057)	(0.046)	(0.048)
Inflation	-0.054*	-0.066*	-0.017	-0.145***
	(0.022)	(0.037)	(0.021)	(0.026)
Financial depth	-0.331***	0.020***	-0.012***	0.017***
	(0.046)	(0.001)	(0.001)	(0.001)
Interest rate	-0.014	-0.009	-0.037	0.033*
	(0.019)	(0.011)	(0.018)	(0.019)
Exchange rate regime	-0.331***	-0.134**	-0.119**	0.395**
	(0.046)	(0.060)	(0.051)	(0.046)
Banking crisis	0.210	0.294**	-0.400	0.279
	(0.166)	(0.345)	(0.330)	0.158)
Constant	2.661***	16.424***	1.816***	-0.852***
	(0.398)	(1.534)	(0.369)	(0.374)
Observations	1316	1316	1316	1316
Time fixed effects	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES
Pseudo R^2	0.589	0.581	0.521	0.552
Panel B: Marginal effects				
Prudential regulations	0.705***	-0.236***	0.408***	-0.426***
	(0.143)	(0.038)	(0.084)	(0.134)

Probit estimates in which the dependent variable is a 0-1 variable with 1 indicating a credit surge (columns 1 and 3) or a credit stop (columns 2 and 4). Average marginal effects are computed as averages of individual marginal effects. Marginal effects on other controls not reported for reasons of parsimony. The standard errors of marginal effects are calculated using the delta method (Cameron and Trivedi, 2005). ***, ** and * indicate statistical significance at the 1, 5 and 10% levels respectively. Variables are as defined in Table 2. Robust standard errors in parenthesis

Table 9
Prudential regulation and the probability of experiencing a credit surge or credit stop—additional robustness tests: probit results

	PCA of total regulation		Lagged bank regulations		Country income level	
	(1) Credit surges	(2) Credit stops	(3) Credit surges	(4) Credit stops	(5) Credit surges	(6) Credit stops
Panel A: Probit coefficients						
Prudential regulations	0.329***	-2.661***			3.192***	-1.971***
	(0.063)	(0.434)			(0.319)	(0.417)
Lagged bank regulations			1.024***	-1.713***		
			(0.345)	(0.223)		
Bank regulation*high income country dummy variable					0.907***	-2.204***
					(0.452)	(0.425)
High-income country dummy variable					0.720***	1.973***
					(0.312)	(0.363)
GDP growth	-0.084	-0.051	-0.015	0.020	-0.135***	-0.083***
	(0.048)	(0.093)	(0.478)	(0.066)	(0.032)	(0.034)
Inflation	-0.019	0.054	-0.049**	-0.098***	-0.011	-0.061***
	(0.022)	(0.036)	(0.022)	(0.041)	(0.010)	(0.014)
Financial depth	-0.012**	-0.015***	-0.009***	-0.021	0.000	-0.010***
	(0.002)	(0.001)	(0.001)	(0.021)	(0.001)	(0.001)
Interest rate	0.044**	-0.038	0.004	0.036	-0.010	0.005)
	(0.018)	(0.031)	(0.020)	(0.288)	(0.007)	(0.008)
Exchange rate regime	-0.122***	-0.108***	-0.342***	-0.305***	-0.038	-0.064*
	(0.151)	(0.049)	(0.046)	(0.043)	(0.047)	(0.038)
Banking crisis	-0.347	-0.641***	-0.211	0.121	-0.235	0.317**
	(0.232)	(0.221)	(0.165)	(0.261)	(0.156)	(0.133)
Constant	0.730***	-4.102***	1.923***	-4.233***	1.902***	-1.289***
	(0.209)	(0.890)	(0.388)	(2.269)	(0.265)	(0.358)
Observations	1316	1316	926	926	1316	1316
Time fixed effects	YES	YES	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES	YES	YES
Pseudo R^2	0.521	0.621	0.352	0.325	0.513	0.497
Panel B: Marginal effects						
Prudential regulations	0.787**	-0.109***	0.405***	-1.321***	0.871***	-0.598***
	(0.015)	(0.029)	(0.136)	(0.123)	(0.084)	(0.62)

Probit estimates in which the dependent variable is a 0-1 variable with 1 indicating a credit surge (columns 1, 3, 5) or credit stop (columns 2, 4, 6). In columns 1 and 2 total regulation is calculated by applying principal components analysis (PCA) to the individual pillars that constitute total regulation. In columns 3 and 4 bank regulations are lagged by three years. In columns 5 and 6 the bank regulation variable is interacted with a dummy variable that takes the value of 1 if when the GDP of a country is above the mean GDP of countries in the sample and 0 otherwise. Variables are as defined in Table 2. Average marginal effects are computed as averages of individual marginal effects. Marginal effects on other controls not reported for reasons of parsimony. The standard errors of marginal effects are calculated using the delta method (Cameron and Trivedi, 2005). *** and ** indicate statistical significance at the 1 and 5% levels respectively. Robust standard errors in parenthesis

Table 10.
Prudential regulation and the probability of experiencing a surge or stop in bank credit: alternative estimation methodologies

	(1)	(2)	(4)	(5)
	Credit surges		Credit stops	
	Poisson	Relogit	Poisson	Relogit
Prudential regulations	1.261***	2.909***	-1.879***	-2.819***
	(0.100)	(0.512)	(0.453)	(0.058)
GDP growth	-0.076***	-0.132***	0.085	0.070
	(.0253)	(0.076)	(0.033)	(0.098)
Inflation	-0.004*	-0.014**	-0.151***	-0.095***
	(0.002)	(0.006)	(0.036)	(0.034)
Financial depth	0.018	0.021	0.019	0.035
	(0.012)	(0.015)	(0.011)	(0.030)
Interest rate	-0.006	-0.012	0.001	0.018
	(0.230)	(0.010)	(0.020)	(0.047)
Exchange rate regime	-0.210	-0.225	0.206***	0.435***
	(0.123)	(0.181)	(0.0492)	(0.079)
Banking crisis	-0.656	-0.687	0.072	0.661
	(0.434)	(0.459)	(.0597)	(0.511)
Constant	0.132***	3.117***	-1.773***	-2.496***
	(0.155)	(0.613)	(0.123)	(0.867)
Observations	1316	1316	1316	1316
Time fixed effects	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES
Pseudo R^2	0.581	0.611	0.452	0.579
Panel B: Marginal effects				
Prudential regulations	0.175***	1.258***	-0.124***	-0.161***
	(0.043)	(0.025)	(0.011)	(0.076)

The table presents from Poisson and rare event logistic (Relogit) regressions in which the dependent variable is a 0-1 variable if there is a credit surge or a credit stop. Variables are defined in Table 2. Average marginal effects are computed as averages of individual marginal effects. The standard errors of marginal effects are calculated using the delta method (Cameron and Trivedi, 2005). ***, ** and * indicate statistical significance at the 1, 5 and 10% levels respectively. Robust standard errors in parenthesis.

Table 11
Prudential regulation and the probability of experiencing a surge or stop in bank credit: IV probit estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Second stage IV results	Dependent variable: credit surge					Dependent variable: credit stop				
Prudential regulations	1.870***	1.382***	2.012***	1.616***	1.758***	-0.609**	-0.945***	-1.564**	-0.914***	-0.923***
	(0.467)	(0.443)	(0.342)	(0.097)	(0.507)	(0.136)	(0.132)	(0.623)	(0.210)	(0.042)
Time fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Panel B: First stage IV results, dependent variable: $Regulation_{i,t}$										
Oil rent volatility	-0.830***	-0.071***	-0.361***			-0.520***	-0.707***	-0.361***		
	(0.008)	(0.008)	(0.016)			(0.074)	(0.008)	(0.016)		
Left-wing government	0.001					-0.006				
	(0.007)					(0.007)				
Right-wing government	-0.083					-0.084				
	(0.126)					(0.084)				
First year	-0.059***	-0.073***		-0.126***		-0.228***	-0.063***		-0.126***	
	(0.015)	(0.015)		(0.031)		(0.013)	(0.015)		(0.032)	
Trade openness	0.012					0.008				
	(0.128)					(0.066)				
Lagged bank regulation	0.814***	0.826***			0.881***	0.489***	0.828***			0.881***
	(0.018)	(0.012)			(0.010)	(0.027)	(0.012)			(0.010)
Time fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Wald test *p*-value	0.001	0.023	0.000	0.000	0.023	0.012	0.000	0.001	0.012	0.000
Amemiya-Lee-Newey test statistic (p-value)	0.103	0.233				0.245	0.149			
Observations	994	994	994	994	994	994	994	994	994	994

In the second stage IV probit results the dependent variable is a 0-1 variable with 1 indicating a credit surge (columns 1 to 5) or a credit stop (columns 6 to 10). Coefficients on other control variables not reported for reason of parsimony. *** and ** indicate statistical significance at the 1 and 5% levels respectively. Robust standard errors in parenthesis. The Amemiya-Lee-Newey test statistic is the minimum chi-sq statistic for overidentifying restrictions (see Guevara (2018) for a discussion).

Table 12
Prudential regulation pillars and the probability of experiencing a credit surge: IV probit results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All regulations	Credit controls	Interest rate controls	Entry barriers	Banking supervision	Privatization	International capital flows	Securities markets
Panel A: IV coefficients								
Prudential regulations	1.870 ***	0.918***	0.189***	0.501***	0.884***	0.216***	0.411***	0.781***
	(0.467)	(0.025)	(0.026)	(0.014)	(0.056)	(0.023)	(0.044)	(0.080)
GDP growth	-0.095**	-0.090**	-0.089**	-0.101***	-0.057	-0.065	-0.084**	-0.045
	(0.045)	(0.046)	(0.050)	(0.040)	(0.037)	(0.052)	(0.044)	(0.047)
Inflation	-0.043**	-0.046**	-0.064***	-0.256***	-0.089***	0.027	-0.004	-0.113***
	(0.024)	(0.023)	(0.028)	(0.020)	(0.018)	(0.019)	(0.021)	(0.035)
Financial depth	-0.009***	-0.010***	-0.007***	-0.005***	-0.008***	-0.008***	-0.013***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
Interest rate	-0.079***	-0.001	-0.025	-0.150***	-0.210***	-0.109***	-0.081***	-0.021
	(0.019)	(0.017)	(0.015)	(0.015)	(0.014)	(0.018)	(0.019)	(0.015)
Exchange rate regime	-0.079	-0.152***	0.061	-0.247***	-0.338***	-0.078	-0.126**	-0.050
	(0.051)	(0.052)	(0.048)	(0.030)	(0.040)	(0.047)	(0.046)	(0.049)
Banking crisis	-0.345	-0.236	0.004	0.759***	0.287*	0.559**	-0.257	0.001
	(0.231)	(0.223)	(0.201)	(0.107)	(0.159)	(0.200)	(0.217)	(0.178)
Constant	3.965***	2.889***	2.761***	4.710***	2.533***	2.294***	4.618***	7.668***
	(0.456)	(0.303)	(0.268)	(0.225)	(0.149)	(0.272)	(0.499)	(0.754)
Observations	994	994	994	994	994	994	994	994
Time fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Pseudo R^2	0.612	0.645	0.639	0.621	0.625	0.625	0.633	0.648
Panel B: Marginal effects								
Prudential regulations	0.011***	0.028***	0.032***	0.044***	0.038***	0.021***	0.041***	0.078***
	(0.007)	(0.002)	(0.004)	(0.001)	(0.006)	(0.002)	(0.004)	(0.008)

IV probit estimates in which the dependent variable is a 0-1 variable with 1 indicating a credit surge. Variables are defined in Table 2. Average marginal effects are computed as averages of individual marginal effects. The standard errors of marginal effects are calculated using the delta method (Cameron and Trivedi, 2005). Marginal effects on other controls not reported for reasons of parsimony. *** and ** indicate statistical significance at the 1 and 5% levels respectively. Robust standard errors in parenthesis.

Table 13
Prudential regulation pillars and the probability of experiencing a credit stop*:* IV probit results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All regulations	Credit controls	Interest rate controls	Entry barriers	Banking supervision	Privatization	International capital flows	Securities markets
Panel A: IV coefficients								
Prudential regulations	-0.609*** (0.136)	-0.121*** (0.003)	-0.540*** (0.005)	-0.791*** (0.091)	-0.458*** (0.009)	-0.696*** (0.031)	-0.406*** (0.104)	-0.162*** (0.026)
GDP growth	0.014 (0.064)	0.126 (0.168)	0.024 (0.048)	0.058 (0.061)	-0.030 (0.052)	0.036 (0.064)	0.177*** (0.055)	0.163 (0.055)
Inflation	-0.108 (0.058)	-0.186 (0.027)	-0.150*** (0.027)	-0.151*** (0.059)	-0.124*** (0.035)	-0.011*** (0.021)	-0.059 (0.040)	-0.088** (0.044)
Financial depth	-0.018*** (0.004)	-0.015*** (0.003)	-0.005*** (0.001)	-0.002** (0.000)	-0.002 (0.013)	-0.212*** (0.086)	-0.015*** (0.001)	-0.013*** (0.001)
Interest rate	-0.050 (0.074)	-0.128*** (0.024)	-0.153*** (0.019)	-0.207** (0.103)	-0.235*** (0.060)	-0.007 (0.001)	-0.368*** (0.065)	-0.471*** (0.070)
Exchange rate regime	-0.227*** (0.091)	-0.003 (0.033)	0.024 (0.061)	0.159*** (0.046)	0.066 (0.343)	-0.036 (0.048)	-0.233*** (0.056)	0.087 (0.070)
Banking crisis	0.319 (0.254)	-0.004 (0.112)	0.610 (0.145)	0.365** (0.172)	0.454** (0.203)	1.489*** (0.185)	-0.119 (0.214)	-0.339 (0.203)
Constant	-13.109** (4.664)	-9.796*** (0.697)	-1.384*** (0.454)	-17.903*** (1.087)	2.084* (1.143)	-6.109*** (0.389)	-3.691*** (1.020)	-4.044 (2.965)
Observations	994	994	994	994	994	994	994	994
Time fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Pseudo R^2	0.612	0.625	0.654	0.581	0.601	0.623	0.641	0.597
Panel B: Marginal effects								
Prudential regulations	-0.092*** (0.003)	-0.021*** (0.001)	-0.014*** (0.004)	-0.086*** (0.005)	-0.045*** (0.011)	-0.091*** (0.002)	-0.056*** (0.003)	-0.053*** (0.002)

IV probit estimates in which the dependent variable is a 0-1 variable with 1 indicating a credit stop. Average marginal effects are computed as averages of individual marginal effects. The standard errors of marginal effects are calculated using the delta method (Cameron and Trivedi, 2005). Marginal effects on other controls not reported for reasons of parsimony. ***, ** and * indicate statistical significance at the 1, 5 and 10% levels respectively. Variables are as defined in Table 2. Robust standard errors in parenthesis.