

Electoral Cycles in Macroprudential Regulation

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Do politics matter for macroprudential policies? I show that changes in macroprudential regulation exhibit a predictable electoral cycle in the run-up to 221 elections across 58 countries from 2000 through 2014. Policies restricting mortgages and consumer credit are systematically looser before elections, particularly during economic expansions. Consistent with theories of opportunistic political cycles, this pattern is stronger when election outcomes are uncertain, regulators are closely tied to politicians, and institutions are poor. These results suggest that political pressures may limit the ability of regulators to “lean against the wind.”

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In the wake of the Great Financial Crisis of 2008-2009, many countries have made sweeping changes to financial regulation. There has been widespread agreement in policy and academic circles that macroprudential regulation can help to mitigate risks to the financial sector as a whole (IMF, 2017). Such macroprudential tools have become part of the standard macroeconomic policy toolkit around the globe (Cerutti, Claessens & Laeven, 2015). This view of macroprudential policy is also supported by a growing literature suggesting that restricting excessive private sector leverage can be welfare-improving. Recent empirical work has found that macroprudential tools can stabilize growth in credit and house prices (e.g., BIS, 2017). However, existing work is silent on potential political challenges to implementing macroprudential policies. Because these policies restrict voters' access to credit, politicians may have considerable incentives to interfere with their implementation (e.g., Haldane, 2017).

In this paper, I investigate empirically whether politics matter for changes to macroprudential policy across countries. Anecdotal evidence abounds: in Canada, for example, the Finance Minister rejected proposals to tighten mortgage rules twice just months before the 2015 elections (Bloomberg, 2015; Canadian Business, 2015).¹ I show that such case studies are part of a systematic pattern. Using quarterly data on 58 countries between 2000 to 2014, I find that macroprudential policies targeting household credit are predictably looser in the run-up to general elections. Quantitatively, the estimated effect of upcoming elections is broadly comparable to the reaction of macroprudential policy to a one standard deviation increase in credit growth, which suggests that political factors are important for understanding how macroprudential tools are used in practice. This finding is robust to controlling for a large number of macroeconomic and financial sector variables and continues to hold using a wide array of model specifications. A battery of tests suggest that

¹In Ireland, the incumbent government pledged to reduce stamp duties for first-time homebuyers in the run-up to the 2007 general election at the height of the 2000s housing boom (Irish Times, 2007). In the United States, the Trump presidency has been accompanied by an increase in the size thresholds for systematically important banks six months before the 2018 midterm elections, and a widely noted decision not to raise countercyclical capital buffers (Financial Times, 2019). In Germany, parliament blocked the introduction of income-based lending limits for households eight weeks before the 2017 election, despite recommendations by the German Council of Economic Experts (German Council of Economic Experts, 2017).

upcoming elections, not differences in fundamentals, likely explain this pattern.

Macroprudential policy is particularly looser in the run-up to elections with uncertain outcomes, where incumbent governments may have the most incentive to interfere with financial regulation. I also find stronger election cycles for elections held according to a country's regular schedule, which may give incumbents more time to interfere than snap elections (see Nordhaus, 1975; Alesina, Cohen & Roubini, 1992); because the timing of these regular elections is predetermined, these results also rule out reverse causality. The election cycle in macroprudential policy is also stronger during periods of high bank profitability and house price growth, precisely when countercyclical policy is supposed to help cushion bank balance sheets against a reversal of fortunes. These results suggest that political economy pressures may weaken regulators' ability to "lean against the wind". This could lead to poor economic outcomes down the line, including a heightened risk of financial crises.

I construct cross-sectional tests to investigate the potential mechanisms underlying these patterns. The electoral cycle particularly bites for tools aimed at households, such as capital buffers targeting mortgages, caps to loan-to-value (LTV), debt service-to-income (DSTI) ratios, and housing-related taxes. These policies restrict household credit and may thus have the most immediate effect on the median voter. I also find some evidence that the election cycle is more pronounced in countries with weaker political institutions, higher state intervention, more political linkages between regulators and politicians, or a weak opposition. These results suggest that politicians believe restricting access to credit is politically costly.

I find no systematic evidence supporting the idea that special interests and regulatory capture explain the electoral cycle. Voters, not banks, seem to be the reason for looser targeted macroprudential policy around elections. I also find no evidence that uncertainty about future governments' economic policies prompts regulators to forego policy tightening. Inertia in policy decisions is also an unlikely explanation. If regulators were simply averse to changing policy before elections, they should be less likely to tighten *and* loosen macroprudential tools. The data, however, suggest a clear asymmetric pattern. Policies are less likely to be tightened but not less likely to be loosened before elections. This leads to a looser overall policy stance,

suggesting a one-sided bias rather than inertia.

I also explore the role of institutional frameworks. Housing-related taxes, which are mostly under direct government control, are actively loosened before elections. This could suggest that they are particularly susceptible to be influenced by opportunistic politicians, which could contribute to fueling housing booms. In contrast to political cycles in monetary policy, I find limited evidence that central bank independence or transparency are associated with less pronounced election cycles.

Overall, the evidence in this paper is broadly consistent with models of opportunistic political cycles, where incumbent governments use economic fundamentals to signal competency to voters (e.g., Rogoff & Sibert, 1988). Because politically-motivated changes in macroprudential regulation shortly before elections are unlikely to sway voters by affecting macroeconomic outcomes, the results suggest that politicians believe cutting off voters from access to credit itself is costly. Indeed, recent evidence shows that contractions in credit are linked to a shift of votes from incumbents to extremist parties, while expansions bring few political benefits (Funke, Schularick & Trebesch, 2016; Doerr, Gissler, Peydro & Voth, 2018; Gyongyosi & Verner, 2019; Antoniadou & Calomiris, 2020). As a result, politicians may have strong incentives to urge regulators not to choke off economic expansions before elections (e.g., Gersbach & Rochet, 2014). This interpretation is also consistent with the experience of the United States in the run-up to the 2007-2008 financial crisis, which was characterized by a lack of regulatory tightening by the Federal Reserve and other regulators.

To the best of my knowledge, this paper is the first to empirically investigate the politics of macroprudential regulation. I build on three strands of the literature. First, my work is related to work on opportunistic political cycles (see Drazen (2000) and Dubois (2016) for surveys). There is considerable evidence that incumbent governments benefit from favorable economic performance and thus have incentives to manipulate policies before elections (see e.g., Nordhaus, 1975; Tufte, 1978). Such electoral incentives also apply to financial policies: drawing on a plethora of case studies, Dagher (2017) argues that regulatory easing in the run-up to banking crises seems to be the rule rather than the exception. Herrera, Ordoñez & Trebesch (2020) show that higher government popularity predicts finan-

cial crises, over and above financial and macroeconomic variables; my results mesh well with their finding that these pre-crisis increases in popularity coincide with financial deregulation.

Second, my paper is related to the literature on political interference in (government) bank lending. Previous work by suggests that government ownership banks is associated with political lending, particularly during election periods (e.g., Sapienza, 2004; Khwaja & Mian, 2005; Dinc, 2005; Cole, 2009; Carvalho, 2014; Halling, Pichler & Stomper, 2016; Englmaier & Stowasser, 2017; Koetter & Popov, 2019). Delatte, Matray & Pinardon-Touati (2020) study political cycles in private bank lending. Akey, Dobridge, Heimer & Lewellen (2018) and Akey, Heimer & Lewellen (2021) show that politicians actively use their influence over lenders to reallocate credit in their interest. I also study political interference in credit markets but with a focus on regulatory actions.

Third, my paper is related to the broader literature on the political economy of finance (e.g., Braun & Raddatz, 2008; Calomiris & Haber, 2014). Perhaps most related is work by Funke et al. (2016), Doerr et al. (2018), and Gyongyosi & Verner (2019), who show that disruptions in credit markets can lead voters to shun incumbent governments and vote for extremist parties. Antoniadis & Calomiris (2020) provide evidence that voters punish incumbents in US presidential elections if they are cut off from mortgage credit. I add to this body of work by providing evidence that macroprudential regulations—widely hoped to prevent future financial crises—are subject to political limitations.

I. Data and Econometric Framework

A. Data

I combine four different types of data for the empirical analysis: (1) data on changes in macroprudential policy; (2) data on general election dates; (3) data on macroeconomic and financial sector conditions; and (4) data on a wide range of institutional and political characteristics. I briefly discuss each and refer the interested reader to the online appendix. Tables A1 and A2 provide summary statistics; tables A3 outlines details on variable construction and sources.

Data on macroprudential policy changes I construct two main measures of macroprudential policy changes. The first measure is based on the cross-country database on prudential policy instruments compiled by Cerutti, Correa, Fiorentino & Segalla (2017). This dataset comprises quarterly data on changes in the intensity of regulatory tools for 64 countries from 2000 to 2014. To construct the dataset, the authors draw on a large range of primary and secondary sources. They then validated the data with the help of the national central banks and other country representatives. I refer the interested reader to their paper for more information on sources and variable construction.

The first main measure for changes in targeted macroprudential policy is the index for sector-specific capital buffers that Cerutti et al. (2017) provide, which ranges from -3 to 3 . This index essentially measures capital buffers aimed at restricting lending to one particular sector of the economy, e.g., residential mortgages. These tools are widely used by policy makers (66 times in the estimation sample) and have broad coverage across countries and time. They specifically target real estate and consumer credit, which likely makes them particularly prone to political interference.²

The second main measure combines data on macroprudential tools targeting households from Cerutti et al. (2017), Kuttner & Shim (2016), and Budnik & Kleibl (2018). In particular, I construct a “targeted policy index” that captures a total of 311 policy changes. This index is based on changes to maximum loan-to-value (LTV) ratios, debt-service-to-income (DSTI) ratios, housing-related taxes, loan concentration limits, targeted loan provisioning rules, and targeted risk weights. Note that it does *not* include the Cerutti et al. (2017) changes in sector-specific capital buffers and thus provides independent variation. I create the index in two steps. First, for each country and quarter, I sum policy changes from different sources separately for each tool and restrict values to lie between -1 and 1 to avoid double-counting. Second, I create the “net change” in policy by summing across tools and create the final index by restricting values to lie between -1 and 1 . To maximize data availability, I do not require countries to have data from all sources.³

²I do not take a stand on whether politicians, voters or bankers understand the exact nature of these tools. What matters in my setting is whether politicians see them as potentially restricting credit.

³As a result of creating “net changes” in a country and year-quarter, the number of policy changes

Table 1 provides an overview of these two main measures and their underlying components. Panel A lists the two indices described above. Panel B contains the underlying components of the sector-specific capital buffers, and Panel C those of the targeted policy index. Panel D shows broader macroprudential tools not part of any of the indices. The use of macroprudential tools between 2000 and 2014 is widely dispersed across countries and time; see Figure A1 in the online appendix. Different macroprudential tools are not necessarily used at the same time: Table A4 in the online appendix suggests that changes in sector-specific capital buffers are essentially uncorrelated with those in LTV ratios, DSTI limits, housing-related taxes, or concentration limits.

To get a sense of who is in charge of macroprudential policy, I use data on financial stability committees from Edge & Liang (2017). According to their classification, macroprudential policy in around half of the countries in my sample is supervised by a financial stability committee. Examples include the United Kingdom's Financial Policy Committee (FPC) and India's Financial Stability and Development Council (FSDC). These committees are comprised of multiple stakeholders, often including the central bank, other regulators, and the government. Data from Cerutti et al. (2015) suggests that the central bank has no power over macroprudential decisions in around 30% of countries, and full decision-making power in another 30%. The rest of the countries falls somewhere in-between.

Data on general elections I merge the data on changes in macroprudential regulation with election dates in 58 democratic countries. I only keep countries that are classified as democracies by the Polity IV project in all sample years, i.e. countries with a minimum Polity IV score of above 0 in all years (as in, e.g., Canes-Wrone & Ponce de Leon, 2018). This requirement eliminates China, Hong Kong, Kuwait, Saudi Arabia, and Vietnam from the datasets on macroprudential regulation. I also drop Luxembourg, where—given its role as a tiny financial center country—macroprudential regulation may not predominantly target the domestic economy. In section E., I show that the sample selection makes no difference to my results.

of the targeted policy index is somewhat smaller than the sum of the underlying individual policies.

To identify elections, I start with the Polity IV database, the Database of Political Institutions (Beck, Clarke, Groff & Keefer, 2001; Cruz, Keefer & Scartascini, 2018), and the Comparative Political Data Set (Armingeon, Wenger, Wiedemeier, Isler, Knöpfel, Weisstanner & Engler, 2017). I first identify each country’s most relevant elections based on the selection of chief executives. In presidential systems such as the United States, power is usually concentrated in the office of the president. In parliamentary systems such as Germany’s, prime ministers or chancellors are the relevant figures, who are elected in parliamentary elections. Since the classification is unclear in a few cases, I hand-check all elections by drawing on additional information from various internet resources. I also cross-check my classification with Julio & Yook (2012). Table A5 in the appendix plots the total number of tightening and loosening episodes for each macroprudential tool by election quarter. Table A6 shows the total number of policy actions by country, as well as the type and number of elections. Table A7 provides a list of the elections in the estimation sample.

Theory predicts that political cycles should be stronger when incumbent governments are uncertain about electoral outcomes. I thus also differentiate elections by whether their outcome is hard to predict ex-ante. Because I do not have reliable polling data for a sufficient fraction of elections, I follow the existing literature and use actual election results as a proxy (see e.g., Canes-Wrone & Park, 2012; Julio & Yook, 2012; Canes-Wrone & Ponce de Leon, 2018). I define relatively close elections as those in which the winner achieves a margin of victory below the median; for presidential systems, I interpret results that are below the median in the last-round presidential vote share as relatively more competitive. I also single out “regular” elections, which are defined as those that are held within a country’s institutionally determined time frame or common practice.⁴ Table A8 shows that half of the elections in my sample are close and around three quarters are regular.

Data on macroeconomic and financial sector conditions I control for quarterly macroeconomic variables using data from the International Monetary Fund’s International Financial Statistics, the OECD, and annual financial sector data from the

⁴I allow one quarter deviation from the exact quarter of the previous election, similar to Julio & Yook (2012), which is unlikely to reflect severe meddling with election timing.

World Bank’s Global Financial Development Database. For Argentina, I add data from the Instituto Nacional de Estadística y Censos (INDEC). Data on uncertainty come from Baker, Bloom & Davis (2016) and Ahir, Bloom & Furceri (2018).

Data on institutional and political characteristics I construct a variety of political characteristics based on data from the World Governance Indicators, Freedom House, Polity IV, and a few other sources. I obtain data on central bank independence from Crowe & Meade (2007), Dincer & Eichengreen (2014), and Garriga (2016). I also construct a new indicator of whether a central bank’s governor has previous work experience in a country’s Ministry of Finance or private financial sector by extending the data in Mishra & Reshef (2019). Online appendix Table A3 provides more details.

B. *Econometric Framework*

The backbone of my empirical analysis are fixed effects panel regressions of the following type:

$$(1) \quad R_{it} = \alpha_i + \mu_t + \sum_{h=-4}^4 \beta_h Election_{it} + \mathbf{X}'\gamma + \varepsilon_{it},$$

where i and t index countries and year-quarters, respectively. α_i and μ_t refer to a full set of fixed effects. R_{it} refers to changes in macroprudential regulation, where changes in sector-specific capital buffers or the targeted policy index serve as my baseline measures.

The main variables of interest are the dummies for periods between four quarters before and four quarters after elections, $\sum_{h=-4}^4 \beta_h Election_{it}$. For brevity, I only report the estimates for the two quarters before an election in most specifications, similar to related studies using quarterly data (see e.g., Canes-Wrone & Park, 2012; Julio & Yook, 2012). In principle, election timing may also be affected by changes in financial regulation; I address this in section A. by splitting elections into whether they are “regular” and thus pre-determined, i.e. held within the limit implied by a country’s constitution or regular practice.

\mathbf{X}' is a vector of control variables that capture the state of the economy and financial sector. For the baseline set of controls, I use eleven quarterly macro and seven annual financial sector variables.⁵ I estimate the baseline regressions using ordinary least squares (OLS). ε_{it} is an error term that is assumed to be well behaved. Standard errors are clustered by country.

I attempt to hold economic fundamentals constant in my regressions by controlling for a large number of macroeconomic and financial variables. I also allow for a specification of equation 1 that includes country \times election cycle fixed effects, where “election cycles” last from the midpoint between two elections to the next such midpoint. The identifying assumption in this specification is that there are no unobserved time-varying factors unrelated to elections that cause systematically looser macroprudential policy in the *same country* during the same electoral period.

II. Results

A. Baseline Results

Figure 1 plots the average change in the sector-specific capital buffers and the targeted policy index for pre-election and all other quarters. For both measures, the pattern in the data is similar: regulation is loosened before elections and tightened in all other periods in the sample. On average, regulation has been tightened over the sample period.

I next challenge these correlations to a regression-based test by estimating equation 1. The results are in Table 2. I begin by looking at the changes in an index of sector-specific capital requirements that particularly target real estate and consumer credit in Panel A. These tools are widely used and are likely to immediately affect household credit, and thus the median voter. I consider the underlying components of this index as well as other tools individually in the next section.

⁵These include government spending/GDP, the money market interest rate, growth of central bank reserves, real credit growth, real GDP growth, current account/GDP, total trade/GDP, investment/GDP, private consumption/GDP, CPI growth, the USD exchange rate, bank capitalization, a measure of banking sector concentration, banks’ cost-to-income ratio, NPL ratio, return on assets, Z-score (an estimate of the probability of default of a country’s banking system), and the share of foreign banks. See the online appendix for more details.

In column 1, I begin with a panel regression without fixed effects or controls. The coefficient of -0.014 for pre-election quarters is statistically significant at the 5% level. It also suggests a sizable magnitude: given that the standard deviation of the dependent variable is 0.185, this implies elections change macroprudential policy decisions by around 8% of a standard deviation.

Column 2 adds a full set of fixed effects, which slightly changes the coefficient to -0.012 , significant at the 10% level. Next, I introduce the vector of macroeconomic and financial sector control variables to account for potential differences in observable economic fundamentals in column 3. Using the relatively large number of controls reduces the number of observations; the point estimate, however, increases to -0.022 and is still precisely estimated. This model specification will serve as the baseline model for the rest of the paper. In column 4, I add country \times election cycle fixed effects. Election cycles are defined as the period from one mid-point between two elections to the next. This effectively means comparing the electoral cycle in the *same country* during the *same time period* between elections. This yields an even larger coefficient of -0.024 (significant at the 5% level). It implies that sector-specific capital buffers are 12% of a standard deviation looser before elections. The fact that the coefficients for pre-election periods are similar across specifications suggests that differences in economic or financial conditions are unlikely to drive the election cycle.

Panel B provides additional evidence that electoral cycles are particularly pronounced for macroprudential tools that likely affect the median voter. The dependent variable is now the targeted policy index, which combines measures on loan-to-value (LTV) caps, debt-service to income (DSTI) caps, housing-related taxes, concentration limits, sector-specific loan provisioning rules, and sectoral risk weights.

I again find highly statistically coefficients for pre-election periods. The estimate of the baseline specification in column 3 is -0.097 and statistically significant at the 1% level. The implied magnitude is large, around a third of the standard deviation of the dependent variable. This estimate is very similar when controlling for *country* \times *election cycle* fixed effects in column 4.

Figure 2 visualizes these results. I plot all dummies for the quarters from a year before until a year after the election as in equation 1 for specifications with and

without control variables.

The quarter immediately before elections particularly stands out in terms of magnitude and statistical significance. The political cycle in sector-specific capital buffers is largely concentrated in the period immediately before elections. For the targeted policy index, I find a clearly negative coefficient for two to three quarters before, depending on whether controls are included. This short-run timing is consistent with the literature on political business cycles (e.g., Akhmedov & Zhuravskaya, 2004). As we will see later, macroprudential policy is already looser two to three quarters before elections during booms in house prices, credit, bank profitability, or GDP growth.

In line with the case studies discussed in the introduction, this timing is consistent with the idea that politicians believe voters care about access to credit *per se*, even if macroprudential policy is unlikely to have major macroeconomic effects within one to two quarters. This finding is consistent with evidence that disruptions in credit markets hurt the vote shares of incumbents and help elect extremist parties, over and above the effect of economic conditions (Funke et al., 2016; Doerr et al., 2018; Gyongyosi & Verner, 2019; Antoniadis & Calomiris, 2020). For some estimates, there also seems to be a minor re-balancing in the post-election period, but this is not statistically significant. Because of the relatively short sample period and the cross-country setting, it is difficult to say whether macroprudential policy is tightened again after elections or remains permanently looser.

B. Results by Macroprudential Tool

I next differentiate between individual macroprudential instruments in Table 3. For these specifications, the dependent variables are dummy variables equal to 1 for a tightening or loosening of policy, and 0 for no policy change, and I estimate linear probability models.

I differentiate between four groups of macroprudential tools. The first comprises the two main index measures. The second group refers to the components of the sector-specific capital buffer index, such as capital buffers targeting mortgages. The third looks at the components of the targeted policy index. Most of these particularly affect households, such as LTV ratios or concentration limits that usually

restrict housing-related lending. The fourth group are broader tools, such as general capital requirements or reserve requirements, which mostly affect bank health and total bank lending.

By and large, Table 3 shows that electoral cycles in prudential regulation appear to be particularly pronounced for sector-specific capital buffers and other targeted policies that are predominantly aimed at households.⁶ These tools have also been found to be among the most effective in curbing credit and house price growth (e.g., Akinci & Olmstead-Rumsey, 2015).

I find a lower likelihood of tightening before elections for most of the components underlying the sector-specific capital buffer and targeted policy indices. For real estate capital buffers, concentration limits, and risk weights, this is concentrated in the quarter immediately before elections. For LTV ratio caps, DSTI caps, and loan provisioning rules, I find negative coefficients two quarters before elections. 7 out of the 9 components have a coefficient that is negative and statistically significant for at least one of the two quarters before elections.

The implied effect sizes differ somewhat across tools. For DSTI caps, loan provisioning rules, concentration limits, or other sector-specific capital buffers, the magnitudes imply that the probability of a tightening essentially drops to approximately zero before elections. These magnitudes are roughly comparable to the change in policy one would expect in response to rapid growth in credit. Aikman, Bridges, Kashyap & Siegert (2020), for example, find that a one standard deviation in credit growth doubles the probability of tightening for housing-related macroprudential tools. This suggests that political factors may play a similar role for macroprudential policy as objective indicators such as credit growth. In the case of LTV ratio caps, real estate capital buffers, and risk weights, I find negative and highly statistically significant estimates for one of the two quarters but a positive coefficient for the other quarter, which dampens the overall pre-election effect.

A different pattern can be seen for housing-related taxes. These tools, usually controlled by the government, show a statistically significantly higher likelihood of active loosening in the quarter immediately before elections. The magnitude of the coefficient estimate 0.034 in t-1 (statistically significant at the 5% level) is large,

⁶Table A1 in the appendix plots full descriptive statistics including observation counts.

suggesting that a loosening is two times more likely before elections relative to other periods, although the coefficient for t-2 of -0.005 (statistically insignificant) has the opposite sign. For the targeted policy index, I find both a lower likelihood of tightening *and* a higher likelihood of loosening in the quarter immediately before elections, perhaps because tax changes are a major component.

For broader macroprudential tools, I find negative and statistically significant coefficients for interbank exposures and reserve requirements on foreign currency exposures. While reserve requirements aimed at foreign currency exposures are not necessarily targeted, they may still predominantly affect mortgage credit in countries such as Hungary, which experienced a foreign currency-fueled household debt boom during the sample period (Gyongyosi & Verner, 2019). The coefficients for local currency reserve requirements and general capital requirements are statistically insignificant.

C. The Role of Close and Regular Elections

Next, I investigate differences in election timing and competitiveness in Table 4. If the election cycle reflects interference by incumbent governments, it should be particularly pronounced where politicians stand to gain most from manipulating policies, and where they are best positioned to do so. To get at this empirically, I exploit variation in the closeness of election outcomes and in whether election dates are pre-determined.

Column 1 reproduces the baseline specification (as in column 3 in Table 2). In column 2, I consider pre-election quarters of “regular” and “irregular” elections separately. “Regular” elections are defined as those held within the time frame specified in a country’s constitution or established as regular practice, and “irregular” ones are all other ones. To illustrate with two well-known recent examples: the 2017 election in the United Kingdom (announced only two months in advance) would be considered irregular; the 2016 presidential election in the United States (four years after the previous one) would be considered regular. Three-quarters of the elections in my sample are regular.

I find considerably larger effects in the immediate run-up to regular compared to irregular elections. For the sector-specific capital buffer, the estimates in columns

2 of Panel A show that the pre-election dummy has a coefficient of -0.025 (statistically significant at the 5% level) for regular compared to -0.016 for irregular elections (statistically insignificant). A test for the equality of coefficients rejects that they are equal at the 10% level. The advantage of only exploiting variation in regular elections is that they are by definition pre-determined and thus not the outcome of unobserved fundamentals. Endogenous election timing thus does not seem to be a major factor in my setting. The result of a larger electoral cycle for regular elections is also intuitive from a political economy angle: incumbent politicians may be able to influence policy more when they have a longer time horizon to interfere, as is the case when election timing is predictable (Nordhaus, 1975).

The difference between regular and irregular elections is even more pronounced when looking at the targeted policy index in Panel B. Here, the coefficients in column 2 are -0.126 for regular elections (significant at the 1% level) and -0.037 for irregular elections (statistically insignificant). I can reject these coefficients are equal at the 1% level.

In column 3, I split the sample by whether the pre-election dummy precedes election outcomes that are more or less likely to be predictable ex-ante. More precisely, I differentiate between election outcomes that are relatively “close” in terms of the winner’s margin of victory. In Panel A, the point estimate for close elections now jumps to -0.034 (statistically significant at the 10% level). The estimate for elections with a more certain outcome is considerably smaller at -0.003 and statistically insignificant ($p = 0.779$). For the targeted policy index, the difference in coefficients in Panel B is -0.177 for close elections and -0.081 for not-so-close elections. The difference in these coefficients is statistically significant at the 1% level. These results suggest that the electoral cycle in macroprudential regulation is stronger in periods when incumbents face uncertainty about election outcomes—and may thus have more incentive to interfere with regulation.

D. Electoral Cycles and Procyclical Regulation

Could election cycles in macroprudential regulation pose a potential challenge to the design of regulatory frameworks? This depends on when these cycles “bite.” By design, macroprudential tools are supposed to be countercyclical. They should

be tightened during credit expansions that may be accompanied by increased risk-taking. Electoral cycles are thus potentially more problematic if they occur during booms, and thus partly undermine countercyclical policies.

The experience of senior policy makers suggests that political pressures may be particularly strong during booms. Kohn (2014), for example, argues: “Highlighting the cyclical risk and recommending raising capital or liquidity requirements in good times are not going to win any political popularity contests. Banks and other lenders will deny the risk, and will point to the fact that they are already well-capitalized and enjoying good profits. Households and businesses will be resistant to higher costs or nonprice constraints on borrowing as they seek to finance increases in housing, consumer durables and business capital as incomes and sales rise.”⁷

Existing work by Antoniadou & Calomiris (2020), using county-level data for the US, suggests that voters punish incumbent presidential candidates for credit crunches, but do not reward them for mortgage credit booms. The fear of being punished at the voting booth might make politicians particularly likely to put pressure on regulators when the economy is booming. Agur & Sharma (2013) argue that political economy challenges for macroprudential policy are most likely during the boom, when such tools have the greatest use.

I test for heterogeneous cycles by interacting the pre-election dummies with measures of real and financial sector expansions in Table 5. The dependent variables are again the indices of changes in sector-specific capital buffers or targeted policies. I standardize the interaction variables to have a mean of zero and a standard deviation of one to aid comparisons across the different models.

In column 1, I add interaction terms with real GDP growth. In Panel A, the interaction terms with the pre-election dummies have negative coefficients of -0.023 and -0.035 , statistically significant at the 5% and 10% level, respectively. In Panel B, the interaction terms are also negative: the dummy for two quarters before an election is statistically significant at the 1% level, although the dummy for the immediate pre-election quarter is insignificant. These results suggest that macropru-

⁷Gersbach & Rochet (2014) argue that “in the event of excessive credit growth or risk build-ups, governments seem reluctant to use macroprudential tools ... especially if general elections are approaching.”

dential regulation is particularly loose before elections when the economy is expanding. I next ask if this is also true for *forecasts* of GDP in column 2, using data from the World Bank. I find similar point estimates, although with lower levels of statistical significance. Similar results also hold for the interaction with house price growth, bank profitability, and credit growth—indicators closely monitored by financial stability authorities—particularly for the targeted policy index. For the sector-specific capital buffer index, the interactions with house price and credit growth are statistically insignificant. Column 6 reports the result of a regression that interacts the pre-election quarter dummy with a summary measure of procyclicality, which I construct by taking the first principal component of the five variables (GDP growth and forecasts, house price growth, bank ROE, real credit growth). Here, the coefficients on the interaction with election quarters in t-1 and t-2 are of similar magnitude for the sector-specific capital buffer, but only statistically significant for t-2; for the targeted policy index, one of the two coefficients is negative (-0.075) and statistically significant at the 5% level, while the other is positive (0.025) and statistically insignificant.

Figure 3 visualizes the magnitude of these results by focusing on the interaction of elections with the first principal component of the five measures. In particular, I plot linear combinations of dummies for the four quarters before until four quarters after elections and their interaction with the first principal component. To measure election cycles during booms, I add the coefficient for the interaction term to the election period dummies; to measure election cycles during downturns, I subtract the coefficient for the interaction term. This reveals that, during boom times, sector-specific capital buffers are systematically looser from around three quarters before an election, especially relative to downturns. For the targeted policy index, there is a clear loosening pattern in the two quarters before elections during booms. The pattern is less clear-cut during downturns, when macroprudential policy is only looser in the immediate pre-election quarter but tighter two quarters before elections. These findings are consistent with the interpretation that political pressures can prevent regulators from implementing countercyclical macroprudential policy.

E. Robustness Tests

In Table A9 in the online appendix, I present a wide range of validity exercises to showcase the robustness of the baseline findings. I begin by addressing concerns regarding the exact model specification and estimation technique in Panel A. The coefficients are remarkably stable, independent of the included set of fixed effects or lags of the dependent variable. They also hold when using the mean group estimator of Pesaran & Smith (1995). This estimates time series regressions country-by-country with a constant—which de-means the data akin to country fixed effects—and reports average coefficients to account for heterogeneous slopes across countries. The results are similar to the fixed effects specification I use as a baseline. I also re-run the baseline estimation but restrict the sample to periods between six quarters before and six quarters after elections (*only periods close to elections*). This regression estimates all coefficients relative to periods that are 5 or 6 quarters away from an election.⁸ Here, I find very similar estimated coefficients.

In Panel B, I deal with concerns regarding sample selection. I start by dropping all countries that are not defined as an (electoral) democracy by both Polity IV and Freedom House. As an alternative proxy for authoritarianism, I drop countries where the chief executive is a military officer. This makes little difference. I next drop the individual continents to validate that the findings are not driven by a particular region. I find that they are generally not. Similarly, I also drop countries with no or very frequent changes in the dependent variable, where I define frequent changes as countries in the top 5% of the total number of changes. I find similar results here. I also divide the sample into the pre-crisis (up to 2006) and post-crisis (from 2007) period. In both subsamples, the coefficients stay negative and are statistically significant for either the sector-specific capital buffer or targeted policy index.

Finally, I deal with the issue of selecting control variables in Panel C. I start by including additional lags and/or leads of the control variables or include only bank controls or macro controls. I can also condition on country \times election year or country \times quarter dummies, which take out unobserved variation and country-specific

⁸If a country holds an election in 2016q3, for example, the omitted period would be the quarters 2015q1, 2015q2, 2017q4, and 2018q1.

seasonality. This, again, makes no difference to the results. Next, I control for the number of macroprudential instruments countries have in their toolkit in a given year, based on a survey of country authorities conducted by the IMF Cerutti et al. (2015), interacted with year-quarter dummies. This makes no difference, either. Finally, conditioning on detailed region \times time or World Bank development level \times time dummies also leaves the estimates unchanged.

Overall, it seems fair to conclude that macroprudential policies are robustly looser before elections.

F. Are Pre-Election Quarters Different?

A potential challenge for identifying electoral cycles in regulation is that macroeconomic and financial sector variables may themselves exhibit an electoral cycle. If, for example, financial conditions before elections are relatively gloomy, regulators may see less reason to interfere or loosen existing measures.

The way I attempt to address such concerns is by including a large vector of control variables. This made little difference to the point estimates I showed above. As an alternative test, I run panel regressions of the type in equation 1 and replace the dependent variable with one of the control variables or the first principle component of sets of controls.

I report the results from these regressions in Table A10. This exercise suggest that there are relatively limited observable differences in financial and macroeconomic conditions between the election and non-election periods across countries immediately before elections. For the majority of variables, the t -statistics are below one. The estimates for the first principal component of the financial sector or macroeconomic variables are also statistically insignificant at conventional levels. This non-finding is not entirely surprising: the existing literature on political business cycles has found mixed and largely context-dependent evidence for electoral cycles in macroeconomic outcomes (see e.g., Drazen, 2000; Dubois, 2016).

Apart from the evidence above, theories of political business cycles predict that other policies should be expansionary before elections. If anything, this means that regulators should pursue *tighter*, not looser regulation in pre-election quarters.

G. Randomization Test

Another potential concern with identifying electoral cycles is that unobserved country-level shocks may coincide with regulatory changes and elections. How likely would it be for such random shocks to generate my results? I address this question by conducting a randomization test.

I begin by picking a random quarter between the first election date before the start of the sample and the next hypothetical election date based on a country's constitutional term limit or common practice. Consider the United States as an example, where presidential elections take place every four years. I start by picking a random quarter between 1996q4, the last election before the sample starts, and 2000q4. Based on this initial placebo date, I generate placebo elections every four years until the end of the sample in 2014. If the initial placebo quarter is 2001q3, for example, the other placebo elections would be in 2005q3, 2009q3, and 2013q3.⁹

I create 5,000 sets of these placebo election quarters and then rerun the baseline equation 1 for the two main dependent variables. Figure A2 in the online appendix plots the results of this exercise. For sector-specific capital buffers, only 1% of the 5,000 random election quarters produce a more negative t -statistic than the one I find in the baseline specification. For the targeted policy index, it is 0 out of 5000. Put differently, my results are unlikely to be a mere statistical fluke due to the “wrong” standard errors.

III. Mechanisms

Macroprudential regulation exhibits a robust and systematic electoral cycle, and this cycle appears more pronounced during economic expansions. In this section, I attempt to shed some light on the potential underlying mechanisms. I consider three potential channels: opportunistic political motives; political connections or regulatory capture; and economic policy uncertainty or legislative inertia.¹⁰

⁹I do not simulate “irregular” placebo elections because their data generating process is harder to pin down.

¹⁰Online Appendix B discusses the potential role of institutional frameworks, including central bank independence. In unreported results, I find no systematic evidence that party ideology, proportional representation, having a presidential vs. parliamentary system, or other metrics of constitu-

A. *Opportunistic Political Motives*

Theories of opportunistic political cycles suggest that incumbent politicians attempt to influence policies to increase their chances of being re-elected (e.g., Dubois, 2016). While the exact mechanism differs, the common element of these models is that governments signal their “competence” to voters using economic fundamentals. In this section, I discuss whether models of opportunistic politicians may also go some way in explaining electoral cycles in macroprudential policy.

The evidence above already gives us some indication. First, election cycles appear to be concentrated in the tools that most likely affect the median voter. These are tools such as capital buffers aimed at mortgages, caps to LTV or DSTI ratios, and housing-related taxes. This interpretation meshes well with the observation by many policy makers that there “may be a tradeoff between expanding homeownership and reducing rapid mortgage debt growth, by tightening loan-to-value ratios or raising the countercyclical capital buffer” (Edge & Liang, 2017). Second, these cycles are driven by looser policy during economic expansions. This is consistent with the intuition of policy makers that politicians are particularly worried about negative effects on their re-election prospects during “good times” (e.g., Gersbach & Rochet, 2014). Third, political cycles in macroprudential regulation seem to be most pronounced before elections that are regular and expected to have close outcomes, when politicians have both the largest incentives and ability to interfere with policy decisions.

I consider several additional cross-sectional tests that, while not a definitive test, also broadly point in the direction of opportunistic political motives for cycles in macroprudential policy. The results are in Table 6. I consider five different measures to proxy for the quality of political institutions and the power of opposition parties: (i) the voice and accountability measure from the World Governance Indicators, (ii) a proxy for freedom from state interventionism from the Fraser Institute, (iii) a dummy for whether the country’s central bank governor has previous work experience at the Ministry of Finance as a proxy for personal political connections,¹¹ (iv) a proxy for interference with bank supervision based on survey data

tional design matter for the severity of electoral cycles.

¹¹This measure is based on extending the dataset in Mishra & Reshef (2019) to the countries in my

collected by Barth, Caprio & Levine (2013),¹² and (v) data on whether opposition parties in parliament are concentrated, which may better enable them to keep governments “in check”. I standardize all continuous indicators to have a mean of 0 and a standard deviation of 1.

To condense the information contained in these variables, I take their first principal component. Column 6 reports a regression where I interact the resulting index of political institutions and opposition power with pre-election quarters. Panel A shows the estimates where the dependent variable is the index of changes in sector-specific capital buffers, and Panel B looks at the targeted policy index. The coefficients of at least one pre-election quarter remain negative and highly statistically significant in both regressions. In addition, the interaction terms for the sector-specific capital buffer are positive and statistically significant. In terms of magnitude, the coefficient of 0.019 for sector-specific capital buffers suggests that countries with a one standard deviation better institutions have considerably muted election cycles in macroprudential regulation relative to the baseline pre-election coefficient of -0.014 . For the targeted policy index, I am not able to detect statistically significant interaction terms.

Columns 1 through 5 report the results for considering the variables underlying the index one by one. The signs of the coefficients are generally in line with those of the index in column 6 for the sector-specific capital buffer. For the targeted policy index, the coefficient signs are more mixed. One reason for the lack of power in the regressions using the targeted policy index could be that, within a single country, different macroprudential tools are often managed by different organizations, which could cause heterogeneity in the importance of political institutions.¹³ Section B in the online appendix discusses the role of institutional frameworks in more detail.

sample until 2017. I would like to thank Ariell Reshef for kindly sharing an updated version of their dataset with me.

¹²In four survey waves, they classify countries into having politically independent bank supervisors or not, and I combine these classifications into an index. Note that this metric of political independence is based on survey data on *microprudential* bank regulators, not macroprudential institutions. I investigate the role of the latter in section B in the online appendix.

¹³In Switzerland, for example, the Federal Department of Finance, the Swiss National Bank, and the Swiss Financial Market Supervisory Authority all have partial authority over different macroprudential policies.

In sum, electoral cycles in macroprudential tools are strongest for tools aimed at the median voter and appear stronger during economic expansions. There is also some evidence that these election cycles are stronger in countries with less developed political institutions, closer personal linkages between politicians and regulators, a weak opposition, and higher state intervention in regulatory decisions, at least for sector-specific capital buffers. Taken together with the other findings documented above, this evidence is most consistent with the idea that opportunistic political interference may play a role in macroprudential policy decisions.

B. Alternative Explanations

This section discusses three alternative explanations for electoral cycles in macroprudential regulation that do not reflect opportunistic political motives. A first alternative explanation could be regulatory capture. A large literature has established that political connectedness of firms is a pervasive feature of economies around the world (Faccio, 2006). More broadly, financial regulation is often seen as the outcome of political bargaining between banks and governments (e.g., Calomiris & Haber, 2014). Political connections could also interact with the electoral cycle, e.g., if incumbent governments signal “good will” to financial sector actors by keeping policy loose while fundraising for political campaigns.

I test the validity of this hypothesis by again introducing interaction terms with pre-election quarters, this time based on five proxies for the connectedness and lobbying power of a country’s financial sector. The first is a measure of banking sector concentration, where one would expect that more concentrated sectors with fewer institutions wield greater lobbying powers. The second is based on legal restrictions on lobbying activities based on data from the Political Finance Database of the Institute for Democracy and Electoral Assistance.¹⁴ The third is the share of politically connected firms (by market capitalization) constructed by Faccio (2006) as an indicator for linkages between private firms and the government, which should proxy for the prevalence of “revolving doors”. The fourth proxy measures political connections of banks by capturing the share of banks with at least one former politician

¹⁴The index counts whether a country has “bans and limits on private income”; “spending regulations”; and “reporting, oversight, and sanctions.”

on the board of directors, taken from Braun & Raddatz (2010). The fifth measure again draws on the newly collected data building on Mishra & Reshef (2019) by introducing an interaction term with whether a country's central bank governor has previous work experience in the financial sector.

Table A11 in the online appendix reports the results. I find no systematic evidence that regulatory capture explains the election cycle in macroprudential policy. The few interaction terms that are statistically significant have different signs for the two main dependent variables (the sector-specific capital buffer and targeted policy indices). The interaction of pre-election quarters with whether a central bank governor previously worked in the financial sector is statistically significant at the 5% level for the targeted policy index but has the "wrong" sign, suggesting a *less* pronounced election cycle in countries with tighter political connections between the central bank and the financial sector. While the results based on these proxies should be taken with a grain of salt, they suggest that the regulatory cycle around elections does not appear to predominantly reflect interference aimed at financial institutions.

A second potential explanation for election cycles in macroprudential tools could be uncertainty about future government policy. Economic policy uncertainty could decrease firm investment before elections, which may in turn affect regulation. Julio & Yook (2012), for example, find that firm investment is lower in the run-up to elections with unclear outcomes. They show that this most likely reflects uncertainty about future government policies. Canes-Wrone & Park (2012) develop this intuition into a model and also find lower private investment before close elections in a panel of OECD economies. If regulators observe lower investment, they may choose to forgo tighter macroprudential policies even in the absence of political interference.

Three reasons make it unlikely that uncertainty is the driving factor behind the electoral cycle in macroprudential policy. First, I do not find a systematic cycle in the macroeconomic fundamentals in my sample, including the ratio of private investment to GDP. Second, I find particularly robust evidence for electoral cycles in tools aimed at *households*. While there may be general equilibrium effects, the data show no differences in consumption around elections in my sample. Third, and

most importantly, I find no consistent evidence that the electoral cycle in macroprudential tools varies with changes in economic policy uncertainty before elections in Table A12 in the online appendix. I consider interactions with four well-known measures of uncertainty: the standardized Economic Policy Uncertainty (EPU) index from Baker et al. (2016); the World Uncertainty Index (WUI) from Ahir et al. (2018); stock price volatility (see e.g., Bloom, 2009); and the option-implied volatility of the S&P 500 index (VIX).¹⁵ In most cases, the interaction terms of interest are small and far from conventional levels of statistical significance. In column 2, the interaction of changes in the WUI and upcoming elections yields a statistically significant *positive* coefficient for the targeted policy index. These results are inconsistent with the “waiting out” hypothesis. Uncertainty about future economic policies is thus an unlikely mechanism.

A third alternative explanation for an electoral cycle could be legislative inertia. The idea is that regulators or governments may perhaps prefer a “hands-off” approach when elections are approaching. However, if inertia was the driving force, we should see a lower likelihood of loosening *and* tightening of policy before elections. This is strongly rejected by the data. Instead, I find a looser *overall* policy stance before elections, which cannot be explained by inertia. The hands-off hypothesis also cannot easily explain why there is an election cycle only in macroprudential tools that particularly affect households; why this cycle appears stronger during economic expansions; and why it is concentrated in countries where banking regulators are not politically independent.

IV. Conclusion

Macroprudential measures have become an increasingly important part of the macroeconomic policy toolkit. There is widespread agreement in central banks, financial regulators, and academia that such tools should play a role in limiting the build-up of systemic risk to prevent major financial crises or at least softening their impact on the economy.

¹⁵Because the Economic Policy Uncertainty index is only available for a sub-group of countries, I assign the value of the aggregate European EPU to EU countries for which I do not have data. I also assign the values for China to Taiwan. This adjustment does not drive the results shown here.

While a rapidly growing academic literature studies the desirability and effects of such policies, we only have anecdotal evidence on the potential political limitations in implementing them. This paper takes a first step forward by showing empirically that changes in macroprudential tools exhibit a systematic electoral cycle. The patterns I document are not driven by strategic election timing and are unlikely to reflect legislative inertia. Instead, the results are most consistent with the interpretation that policies restricting access to credit for the median voter are politically costly, particularly when elections are approaching.

Electoral cycles are most pronounced when house prices, credit, bank profitability, and GDP are expanding. Of course, the very point of macroprudential tools is to be *countercyclical*. My results thus suggest that political pressures may inhibit regulators' ability to "lean against the wind."

An important open question is what types of institutions and governance mechanisms could limit political pressures on regulators. One key challenge—highlighted more than 40 years ago by Kane (1977)—may be that targeted credit policies have much more obvious distributional effects than monetary policy (e.g., Kohn, 2014). The results here thus also raise the question about the optimal policy mix to address the build-up of systemic risks. Political economy factors could be one type of constraint on macroprudential policy that rationalize the use of prudential monetary policy (Caballero & Simsek, 2019). I hope the evidence I present here brings an additional piece of evidence to this debate.

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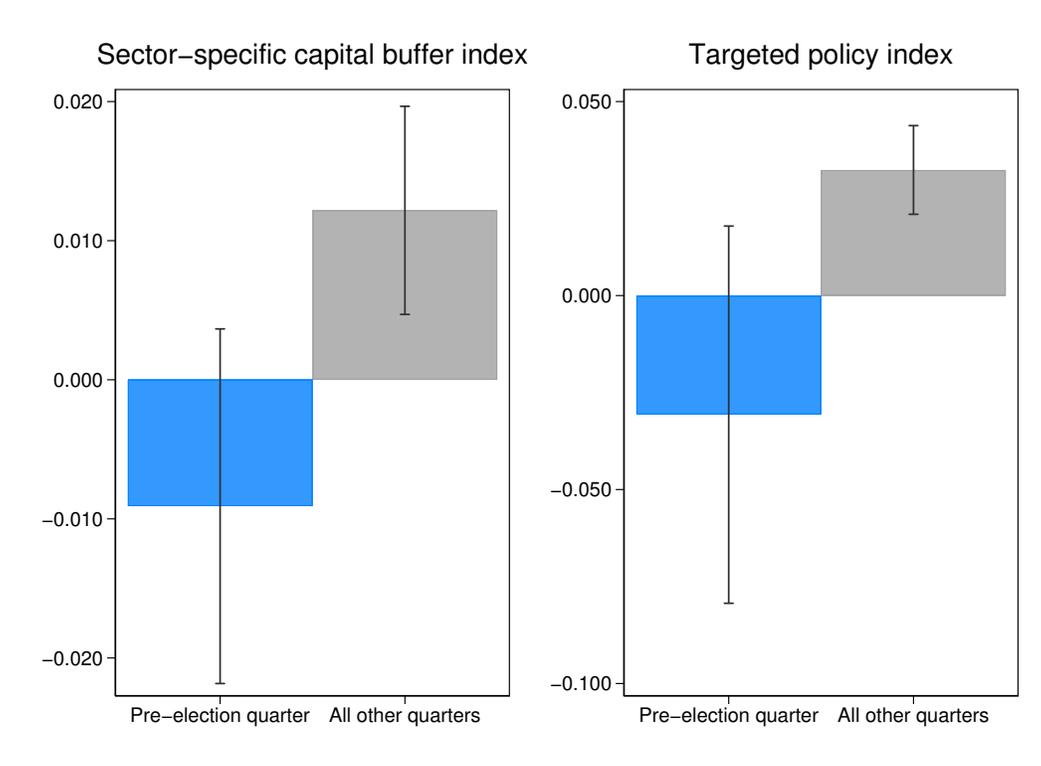
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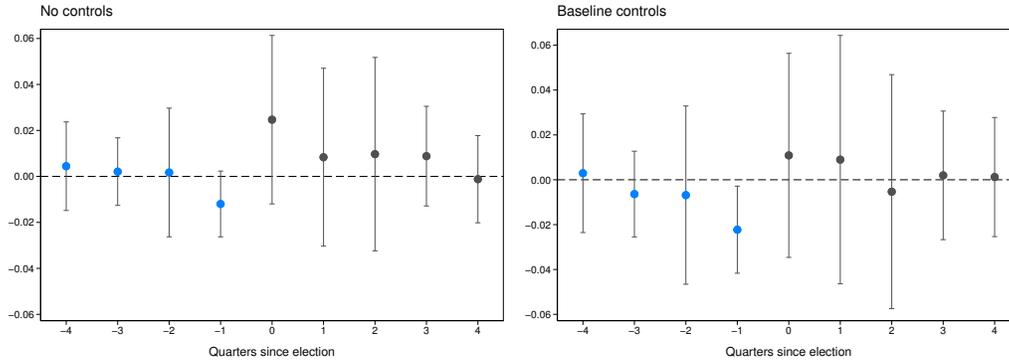
Figure 1: Changes in Macroprudential Policy, by Pre-Election Quarter



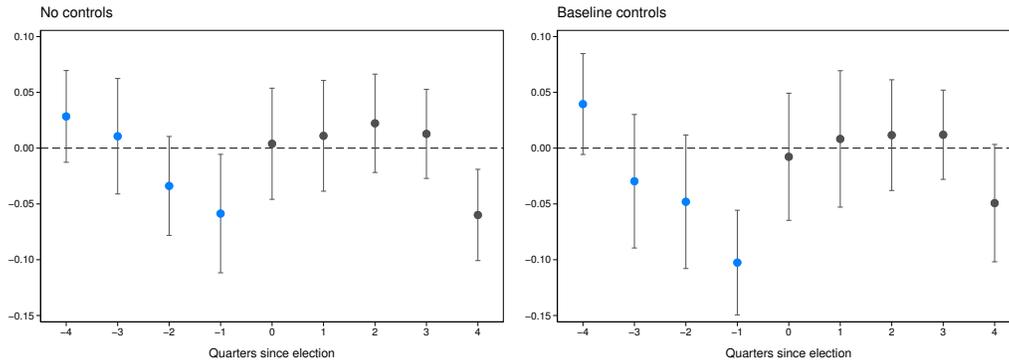
Notes: These figures plot average changes in the sector-specific capital buffer index and the targeted policy index in pre-election quarters and all other quarters. Positive values indicate a tightening of policy, negative values a loosening of policy. The data on sector-specific capital buffers is from Cerutti et al. (2017), the data on the targeted policy index from Cerutti et al. (2017), Kuttner & Shim (2016), and Budnik & Kleibl (2018). See text for details.

Figure 2: Macroprudential Regulation Around Elections

Panel A. Sector-specific capital buffer index



Panel B. Targeted policy index



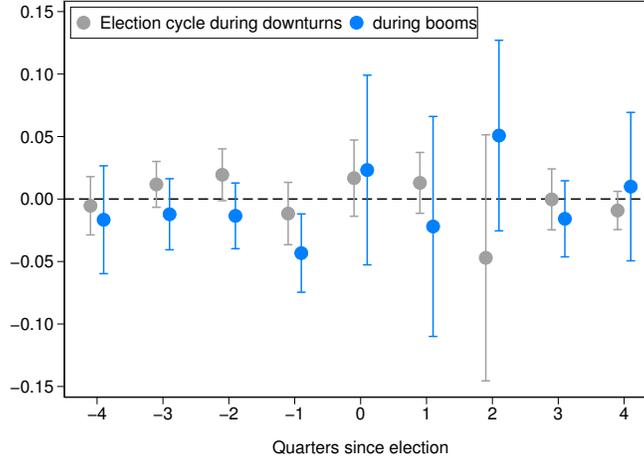
Notes: These figures plot changes in macroprudential regulation around elections. I plot the estimated OLS coefficients $\widehat{\beta}_h$ of the following regression:

$$R_{it} = \alpha_i + \mu_t + \sum_{h=-4}^4 \beta_h Election_{it} + \mathbf{X}'\gamma + \varepsilon_{it}$$

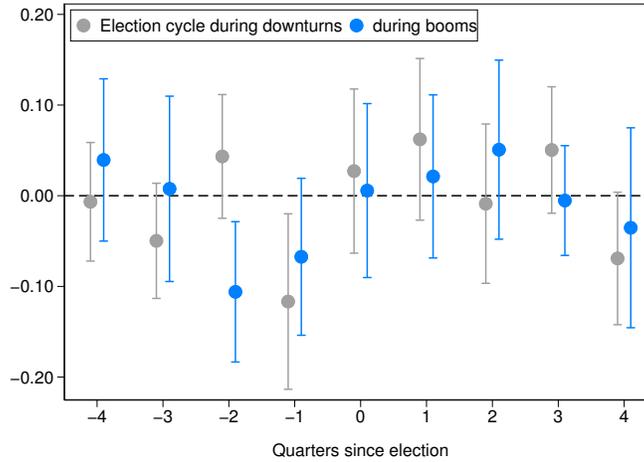
The dependent variable is the sector-specific capital buffer index in Panel A and the targeted policy index in Panel B. The plots labeled “No controls” only include country and year-quarter fixed effects, those labeled “Baseline controls” also include the baseline control variables as in column 3 of Table 2. 95% confidence intervals are based on standard errors clustered by country.

Figure 3: Elections and Macroprudential Policy during Booms and Downturns

Panel A. Sector-specific capital buffer index



Panel B. Targeted policy index



Notes: These figures plot changes in macroprudential policy around elections when the first principal component of the financial and economic variables in Table 5 is one standard deviation above (“boom”) or below (“downturn”) the mean. More specifically, I plot the linear combination of the estimated coefficients $\widehat{\beta}_1^h + \widehat{\beta}_2^h$ (“boom”) and $\widehat{\beta}_1^h - \widehat{\beta}_2^h$ (“downturn”) of the following regression:

$$R_{it} = \alpha_i + \mu_t + \sum_{h=-4}^4 \beta_1^h Election_{it} + \beta_2^h (Election_{it} \times PC_{it}) + \theta PC_{it} + \mathbf{X}'\gamma + \varepsilon_{it}.$$

The dependent variable is the sector-specific capital buffer index in Panel A and the targeted policy index in Panel B. PC is the first principal component of house price growth, credit growth, GDP growth, GDP growth forecasts, and banks’ return on equity, standardized to have a mean of 0 and a standard deviation of 1. \mathbf{X}' contains the baseline controls as in column 3 of Table 2. 95% confidence intervals are based on standard errors clustered by country.

Table 1: Overview of Macroprudential Policy Changes

Notes: This table plots the total number of macroprudential policy changes constructed from the data in Cerutti et al. (2017), Kuttner & Shim (2016), and Budnik & Kleibl (2018) in the estimation sample. Negative values refer to a loosening, positive numbers to a tightening of policy. Panel A lists two indices. Panel B plots the underlying components of the sector-specific capital buffer index. Panel C plots the components of the targeted policy index. Panel D lists broader macroprudential tools (not part of any index). See Tables A1 and A2 in the online appendix for full summary statistics.

	Number of policy changes			Summary statistics		
	Total	Tightening	Loosening	Min.	Mean	Max.
<i>Panel A. Macroprudential policy indices</i>						
Sector-specific capital buffer index	66	49	17	-3	0.011	3
Targeted policy index	311	200	111	-1	0.028	1
<i>Panel B. Components of the sector-specific capital buffer index</i>						
Real estate capital buffer	47	33	14	-1	0.006	1
Consumer credit capital buffer	10	8	2	-1	0.002	1
Other capital buffer	20	15	5	-2	0.003	2
<i>Panel C. Components of the targeted policy index</i>						
Loan-to-value (LTV) ratio	58	40	18	-1	0.008	1
Debt service-to-income (DSTI) limit	32	25	7	-1	0.006	1
Housing-related taxes	106	48	58	-1	-0.003	1
Concentration limit	61	45	16	-1	0.011	1
Loan provisioning limits	60	48	12	-1	0.012	1
Risk weights	47	33	14	-1	0.007	1
<i>Panel D. Broader macroprudential tools</i>						
General capital requirements	64	64	0	0	0.021	1
Interbank exposure	24	23	1	-1	0.021	1
Reserve requirements (LC)	267	111	156	-3	-0.013	5
Reserve requirements (FC)	136	86	50	-3	0.010	5

Table 2: Baseline Results – Elections and Macroprudential Regulation

Notes: This table shows coefficients from estimating equation 1. The dependent variable is the change in the sector-specific capital buffer index in Panel A and the targeted policy index in Panel B. *Pre-election (t-1)* and *Pre-election (t-2)* are dummies equal to one for the quarter before and two quarters an election, respectively. See text for a description of the control variables. Standard errors are clustered by country.

	(1)	(2)	(3)	(4)
<i>Panel A. Sector-specific capital buffer index</i>				
Election quarter (t-1)	-0.014 (0.007)	-0.012 (0.007)	-0.022 (0.010)	-0.024 (0.011)
Election quarter (t-2)	0.001 (0.016)	0.002 (0.014)	-0.007 (0.020)	-0.007 (0.021)
Observations	3,248	3,248	2,279	2,268
Countries	58	58	50	50
R^2	0.002	0.040	0.061	0.117
Dep. variable mean	0.011	0.011	0.012	0.012
Dep. variable SD	0.185	0.185	0.207	0.208
<i>Panel B. Targeted policy index</i>				
Election quarter (t-1)	-0.059 (0.024)	-0.057 (0.025)	-0.097 (0.023)	-0.092 (0.022)
Election quarter (t-2)	-0.040 (0.022)	-0.034 (0.021)	-0.043 (0.028)	-0.037 (0.027)
Observations	3,386	3,386	2,357	2,347
Countries	58	58	50	50
R^2	0.005	0.060	0.078	0.157
Dep. variable mean	0.030	0.030	0.029	0.030
Dep. variable SD	0.314	0.314	0.310	0.310
Country FE		Yes	Yes	—
Country \times Election cycle FE				Yes
Time FE		Yes	Yes	Yes
Baseline controls			Yes	Yes

Table 3: Regressions by Individual Macroprudential Tool

Notes: This table shows coefficients from estimating equation 1. Each row represents the results from two separate regressions: one where the dependent variable is a dummy for a tightening of the indicated macroprudential instrument, and one where the dependent variable is a dummy for a loosening of policy. I only plot the estimated coefficients of the pre-election dummies for one or two quarters before an election ($t-1$ and $t-2$, respectively). *Mean* is the mean of the dependent variable. All estimations include the baseline control variables, as in column 3 of Table 2. Note that the coefficient on loosened general capital requirements cannot be estimated because these are never loosened in the sample period. Standard errors are clustered by country.

Dependent variable:	Tightening			Loosening		
	Pre-election		Mean	Pre-election		Mean
	$t - 1$	$t - 2$		$t - 1$	$t - 2$	
<i>Panel A. Macroprudential policy indices</i>						
Sector-specific capital buffer index	-0.018 (0.005)	0.004 (0.011)	0.018	0.003 (0.007)	0.001 (0.007)	0.007
Targeted policy index	-0.040 (0.014)	-0.027 (0.019)	0.063	0.057 (0.020)	0.016 (0.017)	0.034
<i>Panel B. Components of the sector-specific capital buffer index</i>						
Real estate capital buffer	-0.013 (0.005)	0.008 (0.010)	0.013	0.004 (0.007)	0.003 (0.006)	0.005
Consumer credit capital buffer	-0.001 (0.002)	-0.001 (0.001)	0.003	-0.001 (0.001)	0.000 (0.000)	0.001
Other sector-specific capital buffer	-0.006 (0.003)	-0.005 (0.002)	0.004	-0.001 (0.001)	0.006 (0.008)	0.002
<i>Panel C. Components of the targeted policy index</i>						
Loan-to-value ratio cap	0.006 (0.009)	-0.011 (0.003)	0.013	0.004 (0.008)	0.010 (0.011)	0.006
Debt-service-to-income cap	-0.005 (0.002)	-0.006 (0.003)	0.006	-0.001 (0.001)	0.007 (0.007)	0.001
Housing-related taxes	-0.000 (0.011)	-0.007 (0.008)	0.017	0.034 (0.015)	-0.005 (0.008)	0.018
Concentration limit	-0.030 (0.008)	-0.013 (0.010)	0.018	0.016 (0.011)	0.014 (0.012)	0.006
Loan provisioning rules	-0.014 (0.007)	-0.020 (0.009)	0.015	-0.002 (0.003)	0.011 (0.008)	0.003
Risk weights	-0.007 (0.004)	0.015 (0.012)	0.011	0.011 (0.010)	-0.003 (0.003)	0.006
<i>Panel D. Broader macroprudential tools</i>						
General capital requirements	0.013 (0.014)	0.001 (0.013)	0.022	—	—	—
Interbank exposure	-0.035 (0.015)	-0.021 (0.010)	0.019	-0.004 (0.005)	-0.003 (0.003)	0.001
Reserve requirements (LC)	0.000 (0.017)	0.031 (0.020)	0.035	0.018 (0.020)	0.015 (0.018)	0.041
Reserve requirements (FC)	-0.014 (0.006)	-0.002 (0.012)	0.022	-0.004 (0.010)	-0.001 (0.003)	0.013

Table 4: Electoral Cycles – Differences Across Types of Elections

Notes: This table shows coefficients from estimating equation 1. The dependent variable is the change in the sector-specific capital buffer index in Panel A and the targeted policy index in Panel B. All estimations include the baseline control variables as in column 3 of Table 2. In column 2, I divide the pre-election dummy into “regular” elections, defined as those that were not held late or prematurely (while tolerating one quarter of difference), and the remaining “irregular” elections. In column 3, I restrict the sample to election periods that are relatively “close”, where the outcome is uncertain, and the remaining relatively less close elections. Standard errors are clustered by country.

	Baseline	Election timing	Election outcome
	(1)	(2)	(3)
<i>Panel A. Sector-specific capital buffer index</i>			
Election quarter (t-1)	-0.022 (0.010)		
Election quarter (regular) (t-1)		-0.025 (0.012)	
Election quarter (irregular) (t-1)		-0.016 (0.011)	
Election quarter (close) (t-1)			-0.034 (0.017)
Election quarter (not close) (t-1)			-0.003 (0.012)
Observations	2,279	2,279	1,766
Elections	144	144	108
R^2	0.061	0.063	0.081
Dep. variable mean	0.012	0.012	0.011
p-value: election estimate equal?		0.073	0.162
<i>Panel B. Targeted policy index</i>			
Election quarter (t-1)	-0.097 (0.023)		
Election quarter (regular) (t-1)		-0.126 (0.028)	
Election quarter (irregular) (t-1)		-0.037 (0.034)	
Election quarter (close) (t-1)			-0.177 (0.041)
Election quarter (not close) (t-1)			-0.081 (0.037)
Observations	2,357	2,357	1,754
Elections	148	148	107
R^2	0.078	0.083	0.105
Dep. variable mean	0.029	0.029	0.036
p-value: election estimate equal?		0.000	0.000
Country FE	Yes	Yes	Yes
Time FE	38 Yes	Yes	Yes
Controls	Yes	Yes	Yes

Table 5: Electoral Cycles and Procyclical Macroprudential Policy

Notes: This table shows coefficients from estimating equation 1. The dependent variable is the change in the sector-specific capital buffer index in Panel A and the targeted policy index in Panel B. *Interaction* refers to the variable measuring economic expansions or financial sector booms listed in the top row. The regressions also include the interacted variable (not reported). The interaction variables are standardized to have a mean of 0 and a standard deviation of 1. All estimations include the baseline control variables as in column 3 of Table 2. Standard errors are clustered by country.

Interaction variable:	GDP growth (1)	GDP forecast (2)	House price growth (3)	Bank ROE (4)	Real credit growth (5)	1 st PC (6)
<i>Panel A. Sector-specific capital buffer index</i>						
Election quarter (t-1)	-0.024 (0.009)	-0.024 (0.009)	-0.026 (0.011)	-0.023 (0.010)	-0.022 (0.010)	-0.027 (0.011)
Election quarter (t-2)	-0.010 (0.021)	-0.008 (0.020)	0.001 (0.010)	-0.007 (0.020)	-0.006 (0.020)	0.003 (0.010)
Election quarter (t-1) × Interaction	-0.023 (0.011)	-0.012 (0.007)	-0.007 (0.012)	-0.017 (0.006)	0.005 (0.007)	-0.016 (0.013)
Election quarter (t-2) × Interaction	-0.035 (0.019)	-0.048 (0.044)	-0.010 (0.010)	-0.029 (0.017)	-0.009 (0.018)	-0.016 (0.010)
Observations	2,277	2,274	1,837	2,279	2,222	1,775
R^2	0.06	0.07	0.09	0.06	0.06	0.09
Dep. variable mean	0.012	0.012	0.011	0.012	0.013	0.011
<i>Panel B. Targeted policy index</i>						
Election quarter (t-1)	-0.099 (0.023)	-0.094 (0.022)	-0.100 (0.030)	-0.098 (0.023)	-0.095 (0.024)	-0.092 (0.033)
Election quarter (t-2)	-0.051 (0.028)	-0.047 (0.026)	-0.028 (0.028)	-0.043 (0.028)	-0.049 (0.029)	-0.031 (0.029)
Election quarter (t-1) × Interaction	-0.019 (0.024)	0.039 (0.034)	-0.019 (0.037)	-0.028 (0.030)	-0.003 (0.017)	0.025 (0.044)
Election quarter (t-2) × Interaction	-0.064 (0.019)	-0.065 (0.030)	-0.049 (0.019)	-0.052 (0.024)	-0.119 (0.053)	-0.075 (0.032)
Observations	2,329	2,352	1,858	2,357	2,186	1,739
R^2	0.08	0.09	0.11	0.08	0.09	0.11
Dep. variable mean	0.030	0.029	0.026	0.029	0.031	0.028
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: Political Institutions, Opposition Power, and Electoral Cycles

Notes: This table shows coefficients from estimating equation 1. The dependent variable is the change in the sector-specific capital buffer index in Panel A and the targeted policy index in Panel B. *Voice and accountability* comes from the World Bank’s World Governance Indicators (WGI). *State intervention* is the index on the state ownership of assets from the Fraser Institute. *No CB gov. connections at MoF* is a dummy equal to 1 if the central bank’s governor has not worked at the Ministry of Finance prior to his tenure. *Politically independent bank supervision* is an index for how independent bank supervisors are from the government, based on the survey data in Barth et al. (2013). *Unified opposition* measures the concentration of the opposition party in parliament, based on data from Beck et al. (2001) and Cruz et al. (2018). *1st PC* is the first principal component of the five individual measures. The regressions also include the interacted variable (not reported). Standard errors are clustered by country.

Interaction with:	Voice and accountability (1)	State intervention (2)	No CB gov. connections at MoF (3)	Politically independent bank supervision (4)	Unified opposition (5)	1 st PC (6)
<i>Panel A. Sector-specific capital buffer index</i>						
Election quarter (t-1)	-0.015 (0.007)	-0.023 (0.009)	-0.039 (0.018)	-0.022 (0.009)	-0.022 (0.010)	-0.014 (0.007)
Election quarter (t-2)	-0.007 (0.022)	-0.007 (0.020)	-0.015 (0.043)	-0.008 (0.019)	-0.006 (0.021)	-0.006 (0.023)
Election quarter (t-1) × Interaction	0.015 (0.008)	0.025 (0.010)	0.031 (0.020)	0.013 (0.007)	0.013 (0.007)	0.019 (0.007)
Election quarter (t-2) × Interaction	0.012 (0.015)	0.018 (0.016)	0.014 (0.045)	-0.015 (0.011)	0.017 (0.018)	0.019 (0.021)
Observations	2,135	2,279	2,277	2,279	2,228	2,082
R^2	0.07	0.06	0.06	0.07	0.07	0.07
Dep. variable mean	0.013	0.012	0.012	0.012	0.012	0.012
<i>Panel B. Targeted policy index</i>						
Election quarter (t-1)	-0.088 (0.024)	-0.101 (0.023)	-0.165 (0.041)	-0.096 (0.023)	-0.100 (0.024)	-0.091 (0.026)
Election quarter (t-2)	-0.040 (0.028)	-0.050 (0.030)	-0.014 (0.055)	-0.043 (0.028)	-0.035 (0.029)	-0.031 (0.028)
Election quarter (t-1) × Interaction	-0.029 (0.020)	-0.021 (0.019)	0.111 (0.057)	0.018 (0.025)	-0.013 (0.022)	-0.025 (0.020)
Election quarter (t-2) × Interaction	0.048 (0.030)	0.027 (0.020)	-0.059 (0.065)	-0.020 (0.026)	-0.000 (0.027)	0.034 (0.023)
Observations	2,099	2,243	2,241	2,357	2,312	2,052
R^2	0.09	0.09	0.09	0.08	0.08	0.09
Dep. variable mean	0.035	0.030	0.030	0.029	0.030	0.036
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Appendix (For Online Publication)

A Additional Tables and Figures

Table A1: Descriptive Statistics

	Observations	Mean	Median	Std. Dev.
<i>Macroprudential policy indices</i>				
Sector-specific capital buffer index	3,248	0.011	0.000	0.185
Targeted policy index	3,130	0.028	0.000	0.314
<i>Components of the sector-specific capital buffer index</i>				
Real estate capital buffer	3,248	0.006	0.000	0.120
Consumer credit capital buffer	3,248	0.002	0.000	0.055
Other sectoral capital buffer	3,248	0.003	0.000	0.094
<i>Components of the targeted policy index</i>				
Loan-to-value (LTV) ratio	2,886	0.008	0.000	0.142
Debt service-to-income (DSTI) limit	2,886	0.006	0.000	0.105
Housing-related taxes	2,886	-0.003	0.000	0.192
Concentration limit	2,643	0.011	0.000	0.152
Loan provisioning rules	2,886	0.012	0.000	0.144
Risk weights	2,886	0.007	0.000	0.127
<i>Broader macroprudential tools</i>				
General capital requirements	3,024	0.021	0.000	0.144
Interbank exposure	1,035	0.021	0.000	0.151
Reserve requirements (LC)	3,248	-0.013	0.000	0.331
Reserve requirements (FC)	3,248	0.010	0.000	0.258
<i>Financial sector variables</i>				
Bank capitalization (%)	2,988	8.294	7.800	3.280
Lending concentration	3,104	65.218	64.470	20.571
Cost to income ratio (%)	3,176	59.007	58.060	15.436
Non-performing loans (%)	2,940	5.509	3.243	6.014
Return on assets	3,140	1.131	1.163	1.530
Z-score	3,180	12.474	11.536	7.554
Foreign bank share (%)	3,136	33.703	32.000	24.424
<i>Macroeconomic variables</i>				
Government exp./GDP	2,944	0.175	0.184	0.046
Money market rate	3,149	5.541	4.165	6.719
Growth in CB reserves	3,008	0.147	0.102	0.286
Real credit growth	2,887	0.085	0.062	0.163
Real GDP growth	2,932	0.036	0.033	0.052
Δ Current account/GDP	2,954	0.046	0.000	4.047
Trade/GDP	2,984	0.882	0.734	0.530
Investment/GDP	2,888	0.229	0.224	0.044
Consumption/GDP	2,888	0.578	0.573	0.083
Inflation rate	3,076	0.045	0.030	0.060
Log(FX)	3,124	2.102	1.323	2.491

Table A2: Descriptive Statistics of Additional Variables

	Observations	Mean	Median	Std. Dev.
<i>Election timing</i>				
Election quarter (t-1)	3,248	0.063	0.000	0.243
Election quarter (regular) (t-1)	3,248	0.046	0.000	0.211
Election quarter (irregular) (t-1)	3,248	0.017	0.000	0.128
Election quarter (close) (t-1)	2,634	0.032	0.000	0.176
Election quarter (not close) (t-1)	2,634	0.031	0.000	0.174
GDP forecast	3,162	3.363	3.400	1.907
Real house price growth	2,303	2.235	1.795	9.503
Banking sector ROE	3,140	13.571	14.941	12.969
Real credit growth	2,864	0.090	0.065	0.215
<i>Political characteristics</i>				
Voice and accountability	3,016	0.757	0.942	0.666
State interventionism	3,136	7.583	7.566	1.315
CB governor connections at MoF	3,234	0.409	0.000	0.492
Pol. ind. bank regulators	3,248	1.672	1.000	1.344
Unified opposition	3,168	0.521	0.492	0.233
<i>Political connections and lobbying</i>				
Campaign finance limits	3,248	0.131	0.140	0.045
Connected firms	2,352	0.083	0.016	0.159
Connected banks	3,192	0.632	0.000	1.020
Connected CB governor	3,234	0.378	0.000	0.485
<i>Uncertainty</i>				
Log(EPU)	2,144	4.701	4.684	0.423
Log(WUI)	2,307	-1.690	-1.659	0.683
Stock price volatility	2,956	23.074	20.828	12.331
Log(VIX)	3,248	3.000	3.000	0.347

Table A3: Variable Description and Sources

Description	Source
<i>Macroprudential policy indices</i>	
Sector-specific capital buffer index	Index of changes in sector-specific capital buffers from Cerutti et al. (2017). See description
Targeted policy index	Combined index of changes in loan-to-value ratio caps, debt-service-to-income limits, housing-related taxes, concentration limits, loan provisioning rules, and risk weights, using data from Cerutti et al. (2017), Kuttner & Shim (2016), and Budnik & Kleibl (2018). See description
<i>Components of sector-specific capital buffer index</i>	
Real estate capital buffer	Index of changes in capital buffers targeting real estate lending from Cerutti et al. (2017) See description
Consumer credit capital buffer	Index of changes in capital buffers targeting consumer credit from Cerutti et al. (2017). See description
Other sector-specific capital buffer	Index of changes in capital buffers targeting other sectors from Cerutti et al. (2017). See description
<i>Components of targeted policy index</i>	
Loan-to-value ratio	Index of changes in caps to loan-to-value ratios from Kuttner & Shim (2016) and Budnik & Kleibl (2018). See description
Debt-service-to-income cap	Index of changes in caps to loan-to-value ratios from Kuttner & Shim (2016) and Budnik & Kleibl (2018). See description
Housing-related taxes	Index of changes in housing-related taxes from Kuttner & Shim (2016) and Budnik & Kleibl (2018). See description
Concentration limit	Index of changes in limits on specific large exposures from Cerutti et al. (2017) and Budnik & Kleibl (2018). See description

Table A3: Variable Description and Sources (continued)

	Description	Source
Loan provisioning rules	Index of changes in loan provisioning rules targeting specific sectors from Kuttner & Shim (2016) and Budnik & Kleibl (2018).	See description
Risk weights	Index of changes in risk weights targeting specific sectors from Kuttner & Shim (2016) and Budnik & Kleibl (2018).	See description
<i>Broader macroprudential tools</i>		
General capital requirements	Index of changes in overall capital requirements from Cerutti et al. (2017).	See description
Interbank exposure	Index of changes in limits to interbank exposures from Cerutti et al. (2017).	See description
Reserve requirements (LC)	The index of changes in reserve requirements on local currency exposures from Cerutti et al. (2017).	See description
Reserve requirements (FC)	Index of changes in reserve requirements on foreign currency exposures from Cerutti et al. (2017).	See description
<i>Financial Sector</i>		
Bank capitalization (%)	Ratio of bank capital and reserves to total assets. Capital includes tier 1 capital and total regulatory capital.	World Bank GFD
Lending concentration	The asset market share of a country's three largest banks.	World Bank GFD
Cost to income ratio (%)	Banks' costs divided by their income.	World Bank GFD
Non-performing loans (%)	The ratio of a country's non-performing to total outstanding loans.	World Bank GFD
ROA	The banking system's pre-tax return on assets.	World Bank GFD
Z-score	The Z-score captures the probability of default of a country's banking system by comparing its buffer (capitalization and returns) with the volatility of those returns. It is calculated for each bank as $(ROA + (equity/assets)) / sd(ROA)$ using data from Bankscope and then aggregated to the country level using a weighted average based on each banks' total assets.	World Bank GFD

Table A3: Variable Description and Sources (continued)

	Description	Source
Foreign bank share (%)	Percentage of the total banking assets that are held by foreign banks.	World Bank GFD
<i>Macroeconomic Variables</i>		
Government exp./GDP	Government expenditure scaled over GDP.	IMF, OECD
Money market rate	A typical short-term money market interest rate.	IMF, OECD
Growth in central bank reserves	The year-on-year growth of central bank reserves (or the monetary base, depending on availability), a measure of monetary policy.	IMF, OECD
Real credit growth	The inflation-adjusted year-on-year growth in financial sector claims on the private sector.	IMF
Real GDP growth	Year-on-year growth in gross domestic product, adjusted for inflation.	IMF, OECD
Δ Current account/GDP	The ratio of the current account to GDP.	IMF, OECD
Total trade/GDP	The sum of total exports and imports, scaled over GDP.	IMF, OECD
Investment/GDP	The ratio of gross fixed capital formation to GDP.	IMF, OECD
Consumption/GDP	The ratio of private household consumption to GDP.	IMF, OECD
Inflation rate	The year-on-year growth in a country's consumer price index.	IMF, OECD
Exchange rate (US\$)	A country's exchange rate vis-à-vis the US dollar.	IMF, OECD
Central bank rate	The central bank's official policy rate or the market rate explicitly targeted by the central bank.	IMF, BIS, national central banks
<i>Economic expansions and credit booms</i>		
GDP forecast	The World Bank's GDP forecast for the current year.	World Bank
Real house price growth	The year-on-year real growth in house prices.	BIS, OECD
<i>Elections</i>		
Pre-election	Dummy variable equal to 1 in quarters before a general election.	Various (see text)

Table A3: Variable Description and Sources (continued)

	Description	Source
Pre-election (regular)	Dummy variable equal to 1 in quarters before regular elections, defined as those taking place within a quarter after the anticipated date based on a country's term limit for chief executives or regular practice.	Author's calculation
Pre-election (close)	Dummy variable equal to 1 in quarters before close elections, defined as those where the vote share difference between the election winner and the runner-up is below the median across elections in the sample.	Author's calculation
<i>Political characteristics</i>		
Voice and accountability	Measure of voice and accountability.	World Governance Indicators
State intervention	Index of the degree of state ownership of assets.	Fraser Institute
CB governor connections at MoF	Dummy for whether the central bank governor previously worked at the Ministry of Finance.	Various, Mishra & Reshef (2019)
Politically independent bank supervision	An index for the number of times a country's bank supervisors are classified as independent from the government in the four survey waves of Barth et al. (2013).	Barth et al. (2013)
Unified opposition	The Herfindahl index of opposition parties in parliament. Higher values indicate a more unified opposition.	Cruz et al. (2018)
<i>Connectedness and lobbying</i>		
Connected firms	The share of politically connected firms (by market capitalization).	Faccio (2006)
Connected banks	The share of banks with at least one former politician on its board.	Braun & Raddatz (2008)
Connected CB governor	Dummy for whether the central bank governor previously worked at a private financial institution.	Various, Mishra & Reshef (2019)

Table A3: Variable Description and Sources (continued)

	Description	Source
Campaign finance limits	An index of legal limits on campaign financing. Constructed as sum of bans and limits on private income; regulations of spending; and reporting, oversight and sanctions in a given country.	IDEA Political Finance Database
<i>Uncertainty measures</i>		
Economic Policy Uncertainty	The index of economic policy uncertainty for all countries available at . I re-scale all country-level indices to 1 in 2008q1. For the EU countries that do not have data, I assign the aggregate European index. For Taiwan, I use the Chinese index.	Baker et al. (2016)
World Uncertainty Index	The index of world uncertainty, available on Nicholas Bloom's website.	Ahir et al. (2018).
Stock price volatility	The average of the 360-day volatility of the national stock market index.	World Bank GFD
VIX	Expected stock market volatility implied by S&P 500 index options as calculated by the Chicago Board Options Exchange (CBOE)	St. Louis Fed (FRED)
<i>Macroprudential institutions</i>		
CBI (Dincer & Eichengreen)	Measure of central bank independence covering 2000 to 2010. I extend the series to 2014 using the growth rates of the data in Garriga (2016) (results are unchanged without this adjustment).	Dincer & Eichengreen (2014)
CBI (Crowe & Meade)	A measure of central bank independence in 2003.	Crowe & Meade (2007)
CB transparency (Dincer & Eichengreen)	Measure of central bank transparency covering 2000 to 2010. I extend the series to 2014 by assuming no change between 2010 and 2014 (results are unchanged without this adjustment).	Dincer & Eichengreen (2014)
CB transparency (Crowe & Meade)	Measure of central bank transparency in 2003.	Crowe & Meade (2007)
Financial stability committee (advisory)	Dummy variable equal to 1 if a country has a macroprudential committee consisting of multiple members but no decision making powers, and 0 otherwise.	Edge & Liang (2017)

Table A3: Variable Description and Sources (continued)

	Description	Source
Financial stability committee (power)	Dummy variable equal to 1 if a country has a macroprudential committee that has decision making powers over tools, and 0 otherwise.	Edge & Liang (2017)
Central bank majority powers	Dummy variable equal to 1 if a country's national central bank has more than 50% decision share over macroprudential tools.	Cerutti et al. (2015)

Table A4: Correlation Matrix of Macroprudential Tools

Notes: This table plots pairwise Pearson correlation coefficients of the macroprudential tools constructed from Cerutti et al. (2017), Kuttner & Shim (2016), and Budnik & Kleibl (2018).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
<i>Panel A. Macroprudential indices</i>															
(1) Sector-specific capital buffer index	1.00														
(2) Targeted policy index	0.29	1.00													
<i>Panel B. Components of sector-specific capital buffer index</i>															
(3) Real estate capital buffer	0.86	0.31	1.00												
(4) Consumer credit capital buffer	0.49	0.11	0.35	1.00											
(5) Other capital buffer	0.61	0.10	0.18	0.00	1.00										
<i>Panel C. Components of targeted policy index</i>															
(6) Loan-to-value (LTV) ratio	0.07	0.35	0.11	0.00	-0.00	1.00									
(7) Debt service-to-income (DSTI) limit	-0.00	0.33	0.00	0.00	-0.00	0.57	1.00								
(8) Housing-related taxes	0.00	0.52	0.00	-0.00	0.00	0.00	0.00	1.00							
(9) Concentration limit	-0.00	0.46	0.00	0.00	-0.00	-0.01	-0.01	0.05	1.00						
(10) Loan provisioning limits	0.22	0.38	0.16	0.00	0.22	-0.07	-0.08	0.00	-0.01	1.00					
(11) Risk weights	0.49	0.43	0.62	0.25	-0.00	0.07	0.08	0.00	-0.00	0.05	1.00				
<i>Panel D. Broader macroprudential tools</i>															
(12) General capital requirements	-0.00	0.00	0.00	0.00	-0.00	-0.01	-0.01	0.09	-0.02	0.09	-0.17	1.00			
(13) Interbank exposure	0.05	0.13	0.00	0.00	0.11	-0.01	-0.01	0.00	0.19	0.09	-0.00	0.03	1.00		
(14) Reserve requirements (LC)	0.09	-0.08	0.03	-0.00	0.14	0.00	-0.03	-0.00	-0.06	-0.04	-0.07	-0.03	0.01	1.00	
(15) Reserve requirements (FC)	-0.00	-0.00	0.00	0.00	-0.00	-0.00	0.03	0.00	-0.03	0.02	-0.00	-0.01	0.02	0.46	1.00

Table A5: Tightening and Loosening Episodes, by Pre-Election Quarter

Notes: This table shows the number of tightening and loosening episodes for the prudential tools constructed from the data in Cerutti et al. (2017), Kuttner & Shim (2016), and Budnik & Kleibl (2018) that overlap with the election data criteria described above.

	Tightening episodes			Loosening episodes		
	Total	Pre-election quarters	Other quarters	Total	Pre-election quarters	Other quarters
<i>Macprudential policy indices</i>						
Sector-specific capital buffer index	49	0	49	17	1	16
Targeted policy index	200	9	191	111	14	97
<i>Components of the sector-specific capital buffer index</i>						
Real estate capital buffer	33	0	33	14	1	13
Consumer credit capital buffer	8	0	8	2	0	2
Other capital buffer	15	0	15	5	0	5
<i>Components of the targeted policy index</i>						
Loan-to-value (LTV) ratio	40	5	35	18	2	16
Debt service-to-income (DSTI) limit	25	2	23	7	0	7
Housing-related taxes	48	2	46	58	8	50
Concentration limit	45	1	44	16	2	14
Loan provisioning limits	48	2	46	12	0	12
Risk weights	33	0	33	14	2	12
<i>Broader macroprudential tools</i>						
General capital requirements	64	5	59	0	0	0
Interbank exposure	23	0	23	1	0	1
Reserve requirements (LC)	111	6	105	156	11	145
Reserve requirements (FC)	86	3	83	50	2	48

Table A6: Total Policy Actions and Number of Elections, by Country

Country	Number of policy actions		Type of Election	Number of Elections
	Sector-specific capital buffer index	Targeted policy index		
Argentina	2	4	Presidential	3
Australia	2	1	Legislative	5
Austria	0	0	Legislative	4
Belgium	0	4	Legislative	3
Brazil	8	2	Presidential	3
Bulgaria	3	9	Legislative	4
Canada	0	5	Legislative	5
Chile	0	1	Presidential	3
Colombia	0	0	Presidential	3
Croatia	2	5	Legislative	4
Czech Rep.	0	2	Legislative	4
Denmark	0	3	Legislative	4
Estonia	3	11	Legislative	3
Finland	0	4	Legislative	3
France	0	8	Presidential	3
Germany	0	1	Legislative	4
Greece	0	8	Legislative	5
Hungary	0	11	Legislative	3
Iceland	0	14	Legislative	4
India	8	10	Legislative	2
Indonesia	0	3	Presidential	2
Ireland	2	18	Legislative	3
Israel	4	3	Legislative	4
Italy	0	3	Legislative	4
Japan	0	0	Legislative	5
Latvia	3	5	Legislative	4

Table A6: Total Policy Actions and Number of Elections, by Country (cont.)

Lebanon	0	1	Legislative	4
Lithuania	0	9	Legislative	4
Malaysia	2	10	Legislative	3
Malta	0	5	Legislative	3
Mexico	0	2	Presidential	3
Mongolia	0	1	Legislative	4
Netherlands	0	6	Legislative	5
New Zealand	0	1	Legislative	4
Nigeria	1	0	Presidential	3
Norway	3	4	Legislative	4
Peru	1	4	Presidential	4
Philippines	1	4	Presidential	2
Poland	3	10	Legislative	4
Portugal	0	1	Legislative	4
Romania	0	11	Legislative	4
Russia	1	3	Presidential	4
Serbia	3	2	Legislative	5
Singapore	0	13	Legislative	3
Slovakia	0	6	Legislative	4
Slovenia	1	12	Legislative	4
South Africa	0	0	Legislative	2
South Korea	1	21	Presidential	3
Spain	1	6	Legislative	4
Sweden	1	7	Legislative	3
Switzerland	2	1	Legislative	3
Taiwan	0	3	Presidential	4
Thailand	4	10	Legislative	5
Turkey	3	6	Legislative	3
Ukraine	0	4	Presidential	2
United Kingdom	0	7	Legislative	3

Table A6: Total Policy Actions and Number of Elections, by Country (cont.)

United States	0	4	Presidential	4
Uruguay	1	2	Legislative	2
Total	66	311		207

Table A7: List of Elections in Main Estimation Sample

Country	Quarter	Country	Quarter	Country	Quarter	Country	Quarter
2003q2	Argentina	2003q1	Estonia	2005q2	Lebanon	2009q3	Portugal
2007q4	Argentina	2007q1	Estonia	2009q2	Lebanon	2011q2	Portugal
2011q4	Argentina	2011q1	Estonia	2010q2	Lebanon	2000q4	Romania
2001q4	Australia	2003q1	Finland	2000q4	Lithuania	2004q4	Romania
2004q4	Australia	2007q1	Finland	2004q4	Lithuania	2008q4	Romania
2007q4	Australia	2011q2	Finland	2008q4	Lithuania	2012q4	Romania
2010q3	Australia	2002q2	France	2012q4	Lithuania	2000q1	Russia
2013q3	Australia	2007q2	France	2002q4	Latvia	2004q1	Russia
2002q4	Austria	2012q2	France	2006q4	Latvia	2008q1	Russia
2006q4	Austria	2001q3	United Kingdom	2010q4	Latvia	2012q1	Russia
2008q3	Austria	2005q2	United Kingdom	2011q3	Latvia	2001q4	Singapore
2013q3	Austria	2010q2	United Kingdom	2000q3	Mexico	2006q2	Singapore
2003q2	Belgium	2000q2	Greece	2006q3	Mexico	2011q2	Singapore
2007q2	Belgium	2004q1	Greece	2012q3	Mexico	2000q4	Serbia
2010q2	Belgium	2007q3	Greece	2003q2	Malta	2003q4	Serbia
2001q2	Bulgaria	2009q4	Greece	2008q1	Malta	2007q1	Serbia
2005q2	Bulgaria	2012q2	Greece	2013q1	Malta	2008q2	Serbia
2009q3	Bulgaria	2000q1	Croatia	2001q2	Mongolia	2012q2	Serbia
2013q2	Bulgaria	2003q4	Croatia	2005q2	Mongolia	2002q3	Slovakia
2002q4	Brazil	2007q4	Croatia	2009q2	Mongolia	2006q2	Slovakia
2006q4	Brazil	2011q4	Croatia	2013q2	Mongolia	2010q2	Slovakia
2010q4	Brazil	2002q2	Hungary	2004q1	Malaysia	2012q1	Slovakia
2000q4	Canada	2006q2	Hungary	2008q1	Malaysia	2000q4	Slovenia
2004q2	Canada	2010q2	Hungary	2013q2	Malaysia	2004q4	Slovenia
2006q1	Canada	2004q3	Indonesia	2003q2	Nigeria	2008q3	Slovenia
2008q4	Canada	2009q3	Indonesia	2007q2	Nigeria	2011q4	Slovenia
2011q2	Canada	2004q1	India	2011q2	Nigeria	2002q3	Sweden
2003q4	Switzerland	2009q2	India	2002q2	Netherlands	2006q3	Sweden
2007q4	Switzerland	2002q2	Ireland	2003q1	Netherlands	2010q3	Sweden
2011q4	Switzerland	2007q2	Ireland	2006q4	Netherlands	2001q1	Thailand
2005q4	Chile	2011q1	Ireland	2010q2	Netherlands	2005q1	Thailand
2009q4	Chile	2003q2	Iceland	2012q3	Netherlands	2006q1	Thailand
2013q4	Chile	2007q2	Iceland	2001q3	Norway	2007q4	Thailand
2002q2	Colombia	2009q2	Iceland	2005q3	Norway	2011q3	Thailand
2006q2	Colombia	2013q2	Iceland	2009q3	Norway	2002q4	Turkey
2010q2	Colombia	2003q1	Israel	2013q3	Norway	2007q3	Turkey
2002q2	Czech Republic	2006q1	Israel	2002q3	New Zealand	2011q2	Turkey
2006q2	Czech Republic	2009q1	Israel	2005q3	New Zealand	2000q1	Taiwan
2010q2	Czech Republic	2013q1	Israel	2008q4	New Zealand	2004q1	Taiwan
2013q4	Czech Republic	2001q2	Italy	2011q4	New Zealand	2008q1	Taiwan
2002q3	Germany	2006q2	Italy	2000q2	Peru	2012q1	Taiwan
2005q3	Germany	2008q2	Italy	2001q2	Peru	2004q4	Ukraine
2009q3	Germany	2013q1	Italy	2006q2	Peru	2010q1	Ukraine
2013q3	Germany	2000q2	Japan	2011q2	Peru	2004q4	Uruguay
2001q4	Denmark	2003q4	Japan	2004q2	Philippines	2009q4	Uruguay
2005q1	Denmark	2005q3	Japan	2010q2	Philippines	2000q4	United States
2007q4	Denmark	2009q3	Japan	2001q3	Poland	2004q4	United States
2011q3	Denmark	2012q4	Japan	2005q3	Poland	2008q4	United States
2000q1	Spain	2002q4	South Korea	2007q4	Poland	2012q4	United States
2004q1	Spain	2007q4	South Korea	2011q4	Poland	2004q2	South Africa
2008q1	Spain	2012q4	South Korea	2002q1	Portugal	2009q2	South Africa
2011q4	Spain	2000q3	Lebanon	2005q1	Portugal		

Table A8: Cross Tabulation of Elections by Type

Notes: This table shows the proportion of elections that are defined as regular and close in the main estimation sample. Elections are “regular” if they are held within the time frame specified in a country’s constitution or by legislative practice. Elections are defined as “close” if the winner’s margin of victory is below the sample median.

Election type	Pre-election quarter	Other quarters	Total
<i>Panel A. Full sample</i>			
Irregular	53	3041	3094
Regular	154	0	154
Total	207	3041	3248
<i>Panel B. Subsample with data on vote outcomes</i>			
Not close	82	2431	2513
Close	84	0	84
Total	166	2431	2597

Table A9: Elections and Macroprudential Regulation – Robustness

Notes: This table shows coefficients from estimating equation 1, where the dependent variable is the sector-specific capital buffer index or the targeted policy index. Each cell represents the estimate from a separate regression. I only plot the estimated coefficient of the pre-election quarter dummy (t-1), $\hat{\beta}$, and the associated standard errors (clustered by country). Unless otherwise indicated, all estimations include country and year-quarter fixed effects and the baseline controls as in column 3 of Table 2. *AR(1)* and *AR(4)* refer to specifications with one or four lags of the dependent variable. *Mean group estimator* refers to regressions using the Pesaran & Smith (1995) estimator, which runs time series regressions country-by-country with a constant and reports the average estimated coefficients; note that the inclusion of a constant demeans the data similar to a panel regression with country fixed effects.

	Sector-specific capital buffer index		Targeted policy index	
	$\hat{\beta}$	SE	$\hat{\beta}$	SE
<i>Panel A. Model specification</i>				
Only country FE	-0.022	(0.010)	-0.092	(0.023)
AR(1)	-0.021	(0.009)	-0.099	(0.024)
AR(4)	-0.023	(0.010)	-0.103	(0.024)
Mean group estimator	-0.033	(0.020)	-0.219	(0.088)
Only periods close to elections	-0.028	(0.011)	-0.096	(0.025)
<i>Panel B. Sample selection</i>				
Only consensus democracies	-0.017	(0.009)	-0.103	(0.023)
No military leaders	-0.022	(0.010)	-0.097	(0.023)
Drop Africa	-0.022	(0.010)	-0.097	(0.023)
Drop Asia	-0.016	(0.010)	-0.119	(0.024)
Drop Americas	-0.022	(0.012)	-0.091	(0.027)
Drop Europe	-0.026	(0.014)	-0.051	(0.033)
Drop MENA	-0.021	(0.010)	-0.097	(0.024)
Drop countries without changes	-0.042	(0.024)	-0.105	(0.025)
Drop countries with most changes	-0.018	(0.010)	-0.102	(0.023)
Pre-crisis only	-0.033	(0.020)	-0.046	(0.032)
Post-crisis only	-0.020	(0.013)	-0.132	(0.044)
<i>Panel C. Additional controls</i>				
4 lags of controls	-0.012	(0.007)	-0.057	(0.025)
4 lags and leads of controls	-0.012	(0.007)	-0.057	(0.025)
Only bank controls	-0.014	(0.009)	-0.064	(0.024)
Only macro controls	-0.018	(0.009)	-0.085	(0.029)
Country \times Election Year FE	-0.037	(0.021)	-0.084	(0.032)
Country \times Quarter FE	-0.024	(0.011)	-0.092	(0.022)
Control for other tools	-0.022	(0.011)	-0.095	(0.024)
Regulation \times Time FE	-0.037	(0.013)	-0.098	(0.028)
Region \times Time FE	-0.020	(0.010)	-0.093	(0.025)
Development \times Time FE	-0.028	(0.011)	-0.100	(0.023)

Table A10: Testing for Electoral Cycles in Other Variables

Notes: This table tests for electoral cycles in variables other than macroprudential regulation using panel regressions of the type

$$C_{it} = \alpha_i + \mu_t + \sum_{h=-4}^4 \beta_h Election_{it} + \varepsilon_{it}$$

where C_{it} is one of the control variables in vector \mathbf{X}' of equation 1 (shown in the left column). Each row reports the $\hat{\beta}$ s of separate regressions, where I plot the estimates for the two pre-election quarters. The dependent variables are standardized to have a mean of 0 and a standard deviation of 1. All models include country and year-quarter fixed effects. Standard errors are clustered by country.

	Pre-election (t-1)		Pre-election (t-2)	
	$\hat{\beta}$	SE	$\hat{\beta}$	SE
<i>Panel A. Financial sector variables</i>				
Bank capitalization (%)	0.025	(0.025)	0.007	(0.024)
Lending concentration	-0.049	(0.027)	-0.035	(0.025)
Cost to income ratio (%)	-0.066	(0.031)	-0.077	(0.028)
Non-performing loans (%)	0.008	(0.037)	0.002	(0.038)
Return on assets	0.008	(0.033)	0.035	(0.036)
Z-score	0.003	(0.021)	0.006	(0.018)
Foreign bank share (%)	-0.021	(0.013)	-0.016	(0.012)
<i>Panel B. Macroeconomic variables</i>				
Government exp./GDP	0.001	(0.026)	-0.007	(0.022)
Money market rate	-0.037	(0.036)	-0.039	(0.033)
Growth in CB reserves	0.068	(0.060)	0.033	(0.057)
Real credit growth	-0.078	(0.058)	-0.079	(0.053)
Real GDP growth	0.054	(0.050)	0.045	(0.056)
Δ Current account/GDP	-0.111	(0.122)	0.040	(0.063)
Trade/GDP	0.008	(0.012)	0.008	(0.011)
Investment/GDP	0.049	(0.050)	0.082	(0.046)
Consumption/GDP	-0.028	(0.023)	-0.029	(0.020)
Inflation rate	-0.007	(0.051)	-0.038	(0.046)
Log(FX)	0.004	(0.005)	0.005	(0.005)
<i>Panel C. First principal components</i>				
Financial sector variables	0.009	(0.036)	0.045	(0.039)
Macroeconomic variables	0.057	(0.037)	0.033	(0.038)

Table A11: Election Cycles, Politically Connected Banks, and Lobbying

Notes: This table shows coefficients from estimating equation 1. The dependent variable is the change in the sector-specific capital buffer index in Panel A and the targeted policy index in Panel B. *Interaction* refers to the proxy for political connections or lobbying power of the financial sector listed in the top row. The interaction variables are standardized to have a mean of 0 and a standard deviation of 1. All estimations include the baseline control variables as in column 3 of Table 2. *Campaign fin. lim.* in column 2 is an index of legal restrictions on campaign financing constructed from the IDEA Political Finance Database. *Connected firms* in column 3 is the share of firms with political connections by market capitalization from Faccio (2006). *Connected banks* in column 4 is the share of banks with at least one former politician on the board of directors from Braun & Raddatz (2010). *Connected CB governor* is a dummy for countries where the central bank governor has previous work experience in the financial sector. The regressions also include the interaction measures by themselves in columns 1 and 5 (unreported); they are absorbed by the country fixed effects in the other columns. Standard errors are clustered by country.

Interaction with:	Bank concentration (1)	Campaign fin. lim. (2)	Connected firms (3)	Connected banks (4)	Connected CB governor (5)
<i>Panel A. Sector-specific capital buffer index</i>					
Election quarter (t-1)	-0.023 (0.010)	-0.022 (0.009)	-0.027 (0.011)	-0.022 (0.010)	-0.026 (0.015)
Election quarter (t-2)	-0.007 (0.020)	-0.007 (0.019)	-0.008 (0.023)	-0.006 (0.020)	-0.024 (0.032)
Election quarter (t-1) × Interaction	-0.010 (0.011)	0.016 (0.011)	-0.006 (0.008)	0.004 (0.008)	0.010 (0.018)
Election quarter (t-2) × Interaction	0.018 (0.028)	0.023 (0.010)	0.005 (0.014)	0.007 (0.014)	0.041 (0.036)
Observations	2,279	2,279	1,858	2,252	2,277
R^2	0.06	0.06	0.06	0.07	0.06
Dep. variable mean	0.012	0.012	0.016	0.012	0.012
<i>Panel B. Targeted policy index</i>					
Election quarter (t-1)	-0.097 (0.023)	-0.097 (0.023)	-0.103 (0.026)	-0.098 (0.023)	-0.151 (0.037)
Election quarter (t-2)	-0.043 (0.028)	-0.041 (0.028)	-0.014 (0.029)	-0.044 (0.028)	-0.047 (0.045)
Election quarter (t-1) × Interaction	-0.002 (0.021)	-0.012 (0.016)	-0.013 (0.029)	0.021 (0.017)	0.110 (0.048)
Election quarter (t-2) × Interaction	-0.012 (0.027)	-0.027 (0.024)	0.017 (0.028)	-0.037 (0.021)	-0.001 (0.061)
Observations	2,357	2,357	1,914	2,336	2,241
R^2	0.08	0.08	0.09	0.08	0.09
Dep. variable mean	0.029	0.029	0.030	0.030	0.030
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

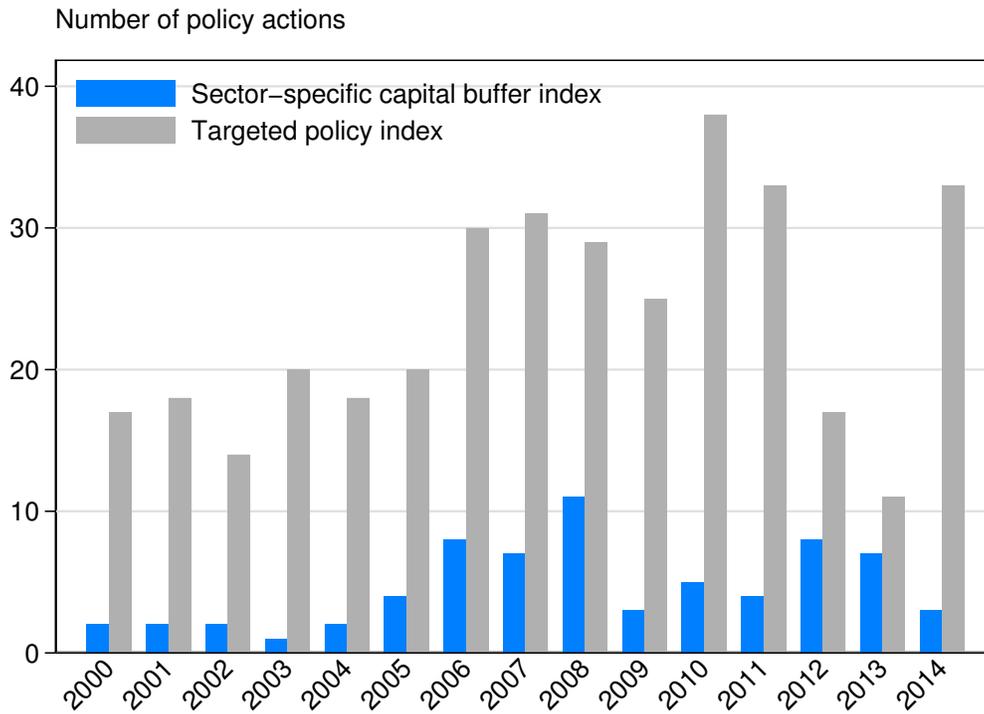
Table A12: The Electoral Cycle and Uncertainty

Notes: This table shows coefficients from estimating equation 1. The dependent variable is the change in the sector-specific capital buffer index in Panel A and the targeted policy index in Panel B. *Uncertainty* refers to the measure of uncertainty listed in the top row: the Economic Policy Index (*EPU*) in column 1; World Uncertainty Index (*WUI*) in column 2; stock market volatility in column 3; and the Chicago Board Options Exchange implied volatility index (*VIX*) in column 4. Note that the VIX only varies by year, not by country. The uncertainty variables are standardized to have a mean of 0 and a standard deviation of 1. The regressions also include the uncertainty measures by themselves (unreported). All estimations include the base-line control variables as in column 3 of Table 2. Standard errors are clustered by country.

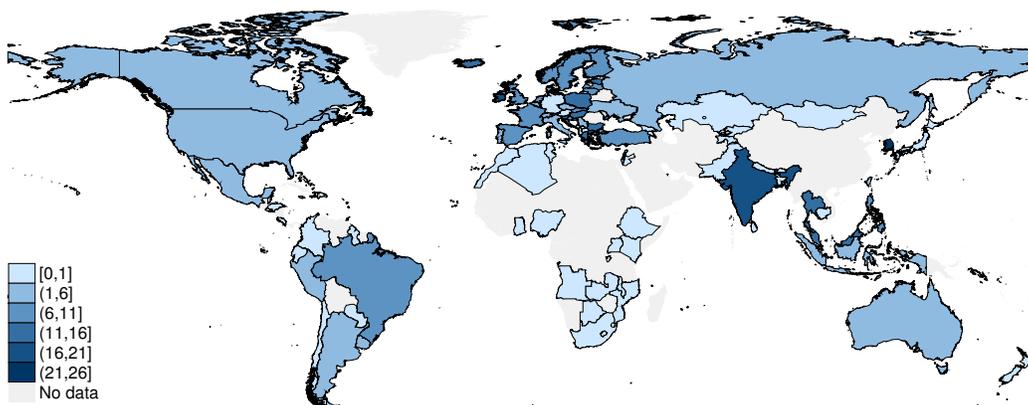
Uncertainty measure:	Log(EPU) (1)	Log(WUI) (2)	Stock price volatility (3)	Log(VIX) (4)
<i>Panel A. Sector-specific capital buffer index</i>				
Election quarter (t-1)	-0.018 (0.010)	-0.018 (0.012)	-0.023 (0.010)	-0.022 (0.010)
Election quarter (t-2)	-0.024 (0.028)	0.018 (0.013)	-0.009 (0.021)	-0.012 (0.023)
Election quarter (t-1) × Uncertainty	0.017 (0.009)	0.021 (0.016)	0.012 (0.006)	-0.004 (0.009)
Election quarter (t-2) × Uncertainty	-0.025 (0.023)	-0.008 (0.014)	-0.006 (0.018)	-0.047 (0.046)
Observations	1,624	1,696	2,215	2,279
R^2	0.07	0.10	0.07	0.07
Dep. variable mean	0.007	0.015	0.013	0.012
<i>Panel B. Targeted policy index</i>				
Election quarter (t-1)	-0.130 (0.028)	-0.116 (0.031)	-0.099 (0.024)	-0.097 (0.023)
Election quarter (t-2)	-0.045 (0.035)	-0.040 (0.033)	-0.045 (0.030)	-0.043 (0.028)
Election quarter (t-1) × Uncertainty	-0.035 (0.049)	0.011 (0.034)	-0.006 (0.021)	-0.039 (0.030)
Election quarter (t-2) × Uncertainty	-0.018 (0.023)	0.012 (0.026)	-0.008 (0.025)	-0.030 (0.033)
Observations	1,680	1,752	2,269	2,357
R^2	0.11	0.09	0.09	0.08
Dep. variable mean	0.028	0.037	0.029	0.029
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Figure A1: Changes in Macroprudential Policy Across Years and Countries

Panel A: Changes Across Years



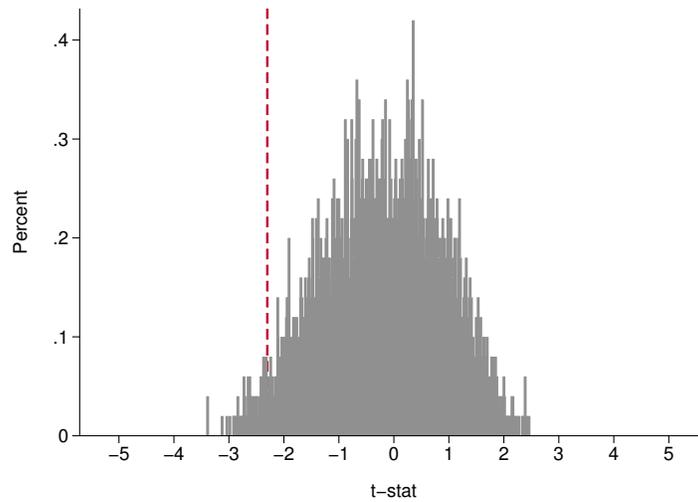
Panel B: Changes Across Countries



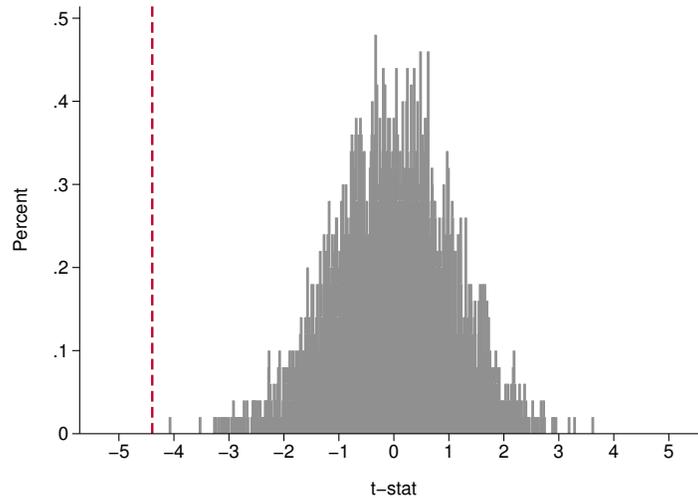
Notes: These figures plot the total number of tightening and loosening episodes for the two main measures of macroprudential policy in the paper for all sample countries between 2000 and 2014: sector-specific capital buffer from Cerutti et al. (2017) and the targeted policy index constructed from data in Cerutti et al. (2017), Kuttner & Shim (2016), and Budnik & Kleibl (2018).

Figure A2: Placebo Test with Randomized Election Timing

Panel A. Sector-specific capital buffer index



Panel B. Targeted policy index



Notes: This figure plots the t -statistics of the estimated $\hat{\beta}$ coefficients from regressing the sector-specific capital buffer index or targeted policy index on 5,000 sets of placebo pre-election dummies. These placebo dummies are calculated by first choosing a random quarter between the first election before the sample start in 2000q1 and the latest quarter one would expect the next election to take place (based on a country's typical practice or term limit), and then assuming that the following placebo elections through 2014q4 were "regular". All regressions include country and time fixed effects and the baseline control variables. The red vertical lines indicate the coefficients estimated with the same regression specification and the actual pre-election quarters in the data. 1% (0%) of the t -statistics of the placebo pre-election quarters for sector-specific capital buffers and the targeted policy index, respectively, yield smaller values than that in the data.

B The Role of Institutional Frameworks

Which institutional frameworks matter for the politics of macroprudential regulation? In the case of monetary policy, a broad consensus holds that central bank independence is an effective means of insulating policy decisions from political interference (see e.g., Cukierman, 1992; Crowe & Meade, 2007; Alpanda & Honig, 2009). A widespread assumption appears to be that central banks are thus also uniquely suited to implement countercyclical *macroprudential* policy. To quote the former director of the IMF’s Monetary and Capital Markets Department, José Viñals, “... in many countries the central bank is unique in being insulated from lobbying and political pressures, which is important to make macroprudential policy work” (Viñals, 2013). The optimal design of governance for macroprudential tools, however, is subject to an ongoing debate.

Section III. already showed that independence from politicians matters. I conduct an empirical test focused on institutional frameworks for macroprudential policy in Table A13 here. I begin by looking at whether a country has a financial stability committee in columns 1 and 2 using data from Edge & Liang (2017); around half of the countries in my sample do. These committees usually consist of representatives of different regulatory agencies (the central bank, securities regulators, prudential regulators), as well as the government. They also differ by whether they have a pure advisory role or can actively implement policies. As an example, Edge & Liang (2017) code the United Kingdom’s Financial Policy Committee (FPC) as having tools beyond sharing and advising, while they assign no such leading role for the German Financial Stability Committee (G-FSC).¹⁶

By introducing interaction terms with the pre-election dummies, I find mixed evidence on the role of financial stability committees. For sector-specific capital buffers, I find a statistically significant negative interaction for advisory committees (−0.063) and a positive interaction with deciding committees (0.030). However, both coefficients are only statistically significant at the 10% level. For the targeted

¹⁶For both types of committees (advisory and equipped with decision-making powers), I create a dummy variable that is equal to 1 for countries that have a committee, and 0 otherwise. The results, however, are not driven by the fact that some countries do not have a committee; they are almost equivalent in the subsample of countries that have one (available upon request).

policy index, I find a negative interaction for deciding committees (0.187), statistically significant at the 5% level.

Next, I turn my attention to who has the main responsibility for implementing macroprudential policy. In column 3, I find that whether the central bank is in charge only matters for the targeted policy index, drawing on data from Cerutti et al. (2015). In particular, I find a coefficient of 0.078 of pre-election quarters with central bank authority over macroprudential tools for the targeted policy index; the results for the sector-specific capital buffer are far from statistically significant.

In columns 4 through 7, I restrict the sample to countries where the central bank has the main decision powers and investigate the role of central bank independence.¹⁷ Central bank independence or transparency seem to make little difference for the electoral cycle. The point estimates are far from statistically significant and close to zero in most cases for the sector-specific capital buffer. For the targeted policy index, I find *negative* coefficients for the interaction with de jure central bank independence and a positive coefficient for the transparency measure from Crowe & Meade (2007).

Taken together, there is limited evidence that central bank governance eliminates election cycles in macroprudential tools. An alternative explanation could be that independent central banks do not tighten macroprudential policy before elections precisely because they do not want to appear partisan. In this view, independent central banks do not tighten before elections to counteract charges of political partisanship, while less independent central banks do not tighten due to opportunistic political motives.

To investigate whether the limited role for central bank independence I find is due to the particular country sample, I test for electoral cycles in monetary policy, building on existing work (e.g., Block, 2002; Alpanda & Honig, 2009). In particular, I replace the dependent variable in regressions of the type in equation 1 with one of two simple measures: the central bank's policy rate or the growth in central

¹⁷I also drop the Eurozone countries from these regressions. The central bank independence metrics refer to the European Central Bank. However, in the Eurozone, the national authorities are mostly in charge of macroprudential regulation. As such, the estimates from regressions including the Eurozone would not be informative about a potential role of central bank governance.

bank reserves.¹⁸ Table A14 shows that the interaction term $Pre - election \times CBI$ is statistically significant in all specifications.¹⁹ The coefficients suggest that central bank independence decreases electoral pressures for monetary policy. For the policy rate in column 3, for example, the estimate of -0.735 on the pre-election dummy (t-1) suggests central banks are more likely to ease monetary policy before elections; the interaction term of 1.933 indicates that this effect is mitigated by central bank independence.

While there are alternative explanations, three factors could explain why independent central banks may be less decisive for political cycles in macroprudential regulation than monetary policy. First, experiences with credit controls, which are in many ways historical precursors of macroprudential tools (Fischer, 2014), suggest that targeted policies are fundamentally different from the “meat-ax” of monetary policy because the “discriminatory effects of aggregate policies (e.g., on housing) are unintentional” (Kane, 1977). According to Kane, such targeted policies are thus more likely to be hijacked for political purposes. In line with this idea, Goodhart (2015) warns that a macroprudential mandate may lead to a “politicization of central bank policy.” Second, the policy targets for macroprudential regulation are much less clearly defined than for monetary policy (e.g., Blanchard, Dell’Ariccia & Mauro, 2013). This may make it more difficult for central banks to make independent decisions, in particular when it comes to tightening during a boom (Chwioroth & Danielsson, 2013). Third, in contrast to monetary policy, the effect of many macroprudential tools could at least in principle be easily undone by fiscal authorities. For example, a tightening of LTV ratios set by an independent central bank could be counteracted by a decrease in housing transaction taxes. This might reduce the leeway independent central banks have over macroprudential policy. It also suggests that active cooperation between the government and regulators may be key to make macroprudential regulation work without imposing large costs on the median voter (see e.g., Fischer, 2014; Tucker, 2018).

¹⁸Section F. shows that there is no electoral cycle in central bank reserves or the money market interest rate.

¹⁹I test for election cycles separately in a sample with and without the Eurozone. When the Eurozone is included, I treat it as a single country. See Table A14 for details.

Table A13: Institutional Frameworks, Elections, and Macroprudential Policy

Notes: This table shows coefficients from estimating regressions of the type in equation 1. The dependent variable is the change in sector-specific capital buffers in Panel A and the targeted policy index in Panel B. *Financial stability committee (advisory)* is a dummy variable for countries where a committee on macroprudential policy decisions exists but the committee does not have decision making powers (Edge & Liang, 2017). *Financial stability committee (decides)* is a dummy for countries where it does. *Macropru. decided by CB* indicates whether the central bank has more than a 50% share in macroprudential policy decisions as classified by Cerutti et al. (2015). *Crowe-Meade* and *Dincer-Eichengreen* refer to data on central bank independence and transparency from Crowe & Meade (2007) and Dincer & Eichengreen (2014), respectively. The sample in columns 4 through 7 excludes the Eurozone countries, where these measures refer to the European Central Bank, which is not in charge of macroprudential policy. It is also limited to countries where the central bank decides on policy. The continuous interaction variables are standardized to have a mean of 0 and a standard deviation of 1. All estimations include country and year-quarter fixed effects as well as the baseline control variables as in column 3 of Table 2. Standard errors are clustered by country.

	If central bank decides macroprudential policy...								
	Financial stability committee			Macropru. decided by CB		De jure CB independence		CB transparency	
	Advisory (1)	Decides (2)	(3)	Crowe- Meade (4)	Dincer- Eichengreen (5)	Crowe- Meade (6)	Dincer- Eichengreen (7)		
<i>Panel A. Sector-specific capital buffer index</i>									
Election quarter (t-1)	-0.027 (0.017)	-0.027 (0.012)	-0.024 (0.014)	-0.058 (0.017)	-0.049 (0.025)	-0.061 (0.040)	-0.037 (0.018)		
Election quarter (t-2)	0.026 (0.016)	-0.005 (0.025)	-0.021 (0.030)	0.012 (0.035)	0.035 (0.040)	0.095 (0.049)	0.042 (0.036)		
Election quarter (t-1) × Interaction	0.009 (0.019)	0.030 (0.017)	-0.002 (0.020)	0.016 (0.024)	0.008 (0.028)	0.010 (0.043)	0.007 (0.018)		
Election quarter (t-2) × Interaction	-0.063 (0.036)	-0.006 (0.027)	0.035 (0.036)	0.019 (0.027)	0.021 (0.021)	0.090 (0.051)	0.046 (0.031)		
Observations	2,252	2,252	2,052	535	452	294	560		
R ²	0.07	0.06	0.06	0.19	0.29	0.33	0.18		
Dep. variable mean	0.012	0.012	0.014	0.034	0.031	0.027	0.030		
<i>Panel B. Targeted policy index</i>									
Election quarter (t-1)	-0.121 (0.040)	-0.082 (0.023)	-0.113 (0.030)	-0.067 (0.045)	-0.063 (0.061)	-0.070 (0.054)	-0.051 (0.034)		
Election quarter (t-2)	-0.028 (0.051)	-0.009 (0.028)	-0.049 (0.041)	-0.050 (0.050)	-0.020 (0.073)	0.073 (0.088)	-0.009 (0.059)		
Election quarter (t-1) × Interaction	0.041 (0.044)	-0.100 (0.063)	0.078 (0.044)	-0.078 (0.033)	-0.098 (0.045)	-0.006 (0.069)	0.047 (0.046)		
Election quarter (t-2) × Interaction	-0.029 (0.058)	-0.190 (0.076)	-0.009 (0.064)	-0.059 (0.074)	-0.051 (0.080)	0.174 (0.071)	0.098 (0.071)		
Observations	2,336	2,336	2,016	517	440	294	548		
R ²	0.08	0.08	0.09	0.27	0.32	0.39	0.25		
Dep. variable mean	0.030	0.030	0.036	0.060	0.070	0.061	0.064		

Table A14: Central Bank Independence and Monetary Policy Cycles

Notes: This table shows coefficients from estimating equation 1. The dependent variable is change in the central bank policy rate or the percentage change in central bank reserves. *CBI* is the measure of central bank independence from Dincer & Eichengreen (2014), extended using the data from Garriga (2016). All estimations include the baseline control variables as in column 3 of Table 2 except the growth in central bank reserves. For the policy rate I include one lag of the dependent variable, for reserves two lags. I treat the Eurozone as a single country in columns 1 and 2, and assign it the timing of German elections and the average of all control variables; the results are almost equivalent if I instead use country-specific elections and controls. Standard errors are clustered by country.

Interaction with:	With Eurozone		Without Eurozone	
	Δ Policy rate (1)	Δ CB reserves (2)	Δ Policy rate (3)	Δ CB reserves (4)
Election quarter (t-1)	-0.890 (0.374)	0.051 (0.030)	-0.735 (0.430)	0.094 (0.037)
Election quarter (t-2)	-0.573 (0.343)	0.005 (0.020)	-0.051 (0.355)	0.003 (0.022)
Election quarter (t-1) \times CBI	1.427 (0.768)	-0.105 (0.058)	1.933 (1.068)	-0.168 (0.067)
Election quarter (t-2) \times CBI	0.227 (0.544)	-0.019 (0.041)	-0.197 (0.833)	0.013 (0.049)
Observations	2,592	3,618	1,699	1,787
R^2	0.151	0.504	0.199	0.529
Dep. variable mean	-0.123	0.096	-0.112	0.088
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes