Electoral Cycles in Macroprudential Regulation

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Do politics matter for macroprudential policy? I show that changes to 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 less likely to be tightened before elections during credit booms and economic expansions. Consistent with theories of opportunistic political cycles, this pattern is stronger when election outcomes are uncertain or in countries where political interference is more likely. In contrast to monetary policy, I find limited evidence that central banks are uniquely insulated from political cycles in macroprudential policy. These results suggest that political pressures may limit the ability of regulators to "lean against the wind."

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1 Introduction

In the wake of the Great Financial Crisis of 2008-2009, many countries have made sweeping changes to financial regulation. Because banking crises tend to follow periods of high growth in credit[1], 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; Aikman, Haldane & Nelson, 2015). As a consequence, such macroprudential tools have become part of the standard macroeconomic policy toolkit around the globe (Cerutti, Claessens & Laeven, 2015).

A growing theoretical literature suggests that restricting excessive leverage, especially in the household sector, could be welfare-improving.[2] Recent empirical work has found that macroprudential tools can stabilize growth in credit and house prices (e.g. BIS, 2017).[3] However, existing work is silent on potential political challenges to implementing macroprudential policies. Because these policies restrict voters' access to credit and also have immediate effects on financial institutions, 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 Ireland, for example, 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).[4] I show that such case studies are part of a systematic pattern. Using quar-

[1] See, for example, Kindleberger (1978), Gourinchas & Obstfeld (2012), and Schularick & Taylor (2012).

[2] Much of this literature studies macroprudential policies in the presence of pecuniary externalities and incomplete markets (e.g. Lorenzoni, 2008; Bianchi, 2011; Korinek & Simsek, 2016; Jeanne & Korinek, 2018; Bianchi & Mendoza, 2018). Another strand also incorporates nominal rigidities (e.g. Schmitt-Grohé & Uribe, 2016; Farhi & Werning, 2016).

[3] See, among others, Jiménez, Ongena, Peydró & Saurina (2017), Aiyar, Calomiris & Wieladek (2016), Aiyar, Calomiris & Wieladek (2014), Gambacorta & Murcia (2017), Ayyagari, Beck & Peria (2017), and Epure, Mihai, Minoiu & Peydró (2017), who show that prudential policies affect loan and firm-level outcomes. Altunbas, Binici & Gambacorta (2018) show that macroprudential policy affects bank risk. Macroeconomic evidence includes IMF (2011), Kuttner & Shim (2016), and Akinci & Olmstead-Rumsey (2015).

[4] In the United States, the economic expansion under President Trump has been accompanied by a removal of qualitative grades for American banks' stress tests, 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 (German Council of Economic Experts, 2017).

terly data on 58 countries between 2000 to 2014, I find that targeted macroprudential policy is predictably looser in the run-up to general elections. This finding is robust to controlling for a large number of macroeconomic and financial sector variables, using a wide array of model specifications and estimation techniques, and including a stringent set of $country \times year$ fixed effects, which means comparing the quarters around elections in the *same country* in the *same year*. A battery of tests suggest that upcoming elections, not differences in fundamentals, likely explain these patterns.[5]

Macroprudential policy is particularly looser in the run-up to elections with uncertain outcomes, where the stakes for incumbents are the highest. This cycle is also stronger for regular elections, i.e. those held in the quarters determined by a country's constitution or common practice, which may give incumbents more time to interfere (see Nordhaus, 1975; Ito, 1990; Alesina, Cohen & Roubini, 1992). This also suggests that upcoming elections are unlikely to be the result rather than the cause of changes in regulation.

The election cycle in macroprudential policy reflects a lack of tightening targeted sectoral tools during "good times", rather than an active loosening of regulation. Policy makers are less likely to tighten regulations before elections during economic expansions and financial sector booms when credit growth and bank profitability are high. This may not be coincidental: because contractions in credit markets are politically costly (Funke, Schularick & Trebesch, 2016; Antoniades & Calomiris, 2018; Doerr, Gissler, Peydro & Voth, 2018; Gyongyosi & Verner, 2019), politicians have strong incentives to urge regulators not to choke off economic expansions before elections (e.g. Gersbach & Rochet, 2014). Of course, boom times are precisely when countercyclical macroprudential policy is supposed to help cushion bank balance sheets against a reversal of fortunes. As such, one interpretation is that the electoral cycle weakens efforts to decrease the financial sector's procyclicality. While only suggestive, I also do not find that regulatory decisions are merely postponed until after the election. Taken at face value, this means electoral cycles could lead to permanently looser policy.

I construct cross-sectional tests to study the potential mechanisms underlying looser macroprudential regulation before elections. The evidence is broadly consistent with

[5] The large literature on political business cycles suggests that, if anything, potentially unobserved monetary and fiscal policies should be *looser* before elections (e.g. Shi & Svensson, 2006). This works against finding an electoral cycle in macroprudential regulation because, if other policies are loose, one would expect countercyclical financial regulation to be more, not less stringent.

models of opportunistic political cycles, where incumbent governments try to signal competency with economic fundamentals (e.g. Rogoff & Sibert, 1988; Rogoff, 1990). As a first indication, the electoral cycle is strongest for the sectoral tools that target mortgage and consumer credit – arguably the policies that most directly affect the median voter. I find less evidence for broader instruments. The cycle is also particularly pronounced in countries with weaker political institutions, higher state intervention, more political linkages between regulators and politicians, and a weak opposition. Taken together, these results point in the direction of political interference aimed at voters.

I find no evidence for a role of special interests and regulatory capture in explaining 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 gives rise to a "wait and see" cycle in macroprudential tools. This could be the case if regulators observe higher uncertainty about firm investments in the run-up to elections and thus forego tightening policy. However, I find no evidence that electoral cycles vary with proxies for uncertainty about economic policy.

A key question these patterns raise is whether institutional frameworks matter for electoral pressures in the use of macroprudential tools. In the case of monetary policy, central bank independence is often thought of as a backstop for political interference (Cukierman, 1992; Eijffinger & de Haan, 1996; Crowe & Meade, 2007). A widespread assumption seems to be that independent central banks also anchor *macroprudential* tools (see e.g. Viñals, 2013). To my knowledge, however, we do not have empirical evidence whether this is indeed the case.

Several factors suggest that targeted macroprudential policies may be quite different from monetary policy. First, targeted credit policies have more immediate distributional and thus politically sensitive effects than changes in monetary policy (Kane, 1977; Agur & Sharma, 2013; Fischer, 2014; Tucker, 2016). Second, the effects of macroprudential tools on borrowers can be more easily counteracted by fiscal authorities, e.g. through the use of taxes or subsidies for housing. Indeed, some policy makers argue that coordination with governments is desirable to undo potential unintended harm of these tools (e.g. Fischer, 2014, 2015). This also means independence may be less useful than for monetary policy. Third, "financial stability" as a policy target is vaguely defined and the policy levers are less well understood than for monetary policy. This may also lead to an unwillingness to tighten policy before elections.[6]

In the data, I find limited evidence that central banks are uniquely isolated from the electoral cycle in macroprudential tools. Central bank independence does not seem to be key for mitigating electoral pressures, both where central banks control macroprudential policy decisions and where they do not. In contrast, central bank independence matters consistently for electoral cycles in *monetary policy*, in line with previous evidence (see e.g. Havrilesky, 1988; Cukierman, 1992; Abrams, 2006; Alpanda & Honig, 2009; Dubois, 2016). While countries with low central bank independence show signs of lower policy rates and higher growth in central bank reserves before elections, this pattern disappears for countries with highly independent monetary authorities. Taken together, this suggests that *targeted tools* may create incentives for politicians to interfere with macroprudential policy, even where central bank independence appears to insulate monetary policy. I also find some evidence that electoral cycles are less of a concern in countries where financial stability committees decide on regulation.

To the best of my knowledge, this paper is the first to systematically investigate the politics of macroprudential regulation. I build on three broad strands of the literature. First, my work is grounded in work on opportunistic political cycles (see e.g. 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; MacRae, 1977; Tufte, 1978; Keech, 1995).[7] Brown & Dinc (2005), Dam & Koetter (2012), and Behn, Haselmann, Kick & Vig (2015) show that bank bail-outs are less likely to happen before elections in a variety of settings. 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 (2014) show that increases in government popularity predict financial 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.

[6]Many case studies of political interference in financial regulation are countries with arguably excellent institutions, such as the United States or the United Kingdom (e.g. Calomiris & Haber, 2014).

[7]There is evidence for political cycles in, among others, fiscal transfers (e.g. Akhmedov & Zhuravskaya, 2004), local tax rates (e.g. Foremny & Riedel, 2014; Alesina & Paradisi, 2017), and monetary policy (e.g. Alesina et al., 1992; Block, 2002; Clark & Hallerberg, 2000).

Second, my paper is related to the literature on political interference in (government) bank lending. Previous work by Sapienza (2004), Khwaja & Mian (2005), Dinc (2005), Cole (2009), Carvalho (2014), Halling, Pichler & Stomper (2016), Haselmann, Schoenherr & Vig (2018), Englmaier & Stowasser (2017), and Koetter & Popov (2019) suggests that government ownership of banks is associated with political lending, particularly during election periods. Akey, Dobridge, Heimer & Lewellen (2018) and Akey, Heimer & Lewellen (2018) 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. Kroszner & Strahan, 1999; Rajan & Zingales, 2003; Braun & Raddatz, 2008; Benmelech & Moskowitz, 2010; Calomiris & Haber, 2014). Perhaps most related is work by Gyongyosi & Verner (2019), Doerr et al. (2018), and Funke et al. (2016), who show that disruptions in credit markets can lead voters to shun incumbent governments and vote for extremist parties. Antoniades & Calomiris (2018) provide evidence that voters punish incumbents in US presidential elections if they are cut off from mortgage credit. Mian, Sufi & Trebbi (2010) and Mian, Sufi & Trebbi (2013) study how special interests shape legislative decisions in financial regulation; Chavaz & Rose (2018) and Bertrand, Kramarz, Schoar & Thesmar (2018) show that firms, in turn, may help or reward politicians. Johnson & Mitton (2003) show that imposing capital controls in Malaysia in 1998 primarily benefited firms connected to the prime minister; Igan, Lambert, Wagner & Zhang (2017) study how lobbying affects the sale of failed banks in the US. I add to this body of work by providing evidence that macroprudential regulations – widely hoped to prevent future financial crises – may be subject to political limitations.

2 Data and Econometric Framework

2.1 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 1 and A1 provide summary statistics; tables A2, A3, and A4 outline details on variable construction and sources.

Data on macroprudential policy changes For my baseline results, I use 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 the first quarter of 2000 to the fourth quarter of 2014. The data differentiate between capital buffers (general or sector-specific), limits to interbank exposures, concentration limits, loan-to-value (LTV) ratios, and reserve requirements. Because the data on LTV ratios are barely filled, I add the data on changes in LTV limits from Kuttner & Shim (2016).[8] The data include two aggregate indices: one tracks changes in *any* instrument (ranging from -1 to 1) and the other changes in sectoral capital requirements (ranging from -3 to 3). Sectoral capital requirements are regulations aimed at real estate credit, consumer credit, or other sectors. In practice, many of these reflect changes to risk weights on particular loan types, e.g. on mortgages. For more information on sources and variable construction, I refer the interested reader to Cerutti et al. (2017).

The use of macroprudential tools between 2000 and 2014 is widely dispersed across countries and time. The countries with the most changes in any macroprudential tool are Serbia and India (26 changes), followed by Brazil (24 changes) and Argentina (22 changes). Sector-specific capital buffers are most frequently used in Brazil (9 changes), India (8 changes), followed by Thailand and Israel (4 changes). Overall, I observe the most policy changes in Eastern and Northern Europe (93 and 75 changes, respectively) as well as South America (83 changes). Sectoral capital buffers are also most popular in Europe (20 changes) and South America (13 changes).[9] There is considerable individual variation in the use of macroprudential tools, as suggested by the relatively low correlations among regulatory changes (ranging from -0.05 to 0.21).[10]

Throughout the paper, I focus on sectoral capital buffers as my baseline measure of macroprudential policy stance. These tools are widely used and highly targeted towards real estate and consumer credit, which likely makes them particularly prone to political

[8]Note that the datasets do not follow comparable classifications for other regulatory changes, so I do not merge data on other tools. I discuss this issue in more detail in section 3.7.
[9]I plot the number of changes by year and country in figure A1 in the online appendix.
[10]The correlations between the individual measures can be found in the online appendix Table A5.

interference.[11] I will also consider how other tools vary with the timing of elections.

I also replicate my main results using data on macroprudential policy changes from Kuttner & Shim (2016) and Budnik & Kleibl (2018), who cover fewer countries and/or policy tools. I discuss the construction of these variables in the online appendix. As I discuss in section 3.7, the correlation of changes in macroprudential tools across sources is surprisingly low. I thus chose not to aggregate the data across sources.

Data on general elections I merge the data on changes in macroprudential regulation with election dates in 58 democratic countries. Electoral cycle theories posit that politicians may attempt to influence policy to target special interest groups or the median voter. As such, identifying such cycles requires countries with reasonably credible elections. I thus only keep countries that have a score above 0 on the Polity IV scale in all sample years from 2000 through 2014, as in Canes-Wrone & Ponce de Leon (2018) (also see Persson & Tabellini, 2003; Epstein, Bates, Goldstone, Kristensen & O'Halloran, 2006). This requirement eliminates China, Hong Kong, Kuwait, Saudi Arabia, and Vietnam from the dataset on prudential 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 3.5, I show that the sample selection makes no difference to my results.

To identify elections, I start with the Polity IV database, the Database of Political Institutions (Beck, Clarke, Groff & Keefer, 2001), updated by Cruz, Keefer & Scartascini (2018), the Comparative Political Data Set (Armingeon, Wenger, Wiedemeier, Isler, Knöpfel, Weisstanner & Engler, 2017), and the Global Elections Database (Brancati, 2017). These datasets list the months and years of elections around the world. 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 normally concentrated in the office of the president. In parliamentary systems such as Germany's, prime ministers or premiers 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 A6 in the appendix plots the total number of tightening and loosening

[11] I do not take a stand on whether politicians, bankers or voters understand the exact nature of these tools. What matters in my setting is whether they are seen as potentially restricting credit.

episodes for each macroprudential tool by election quarter. Table A7 details the number of tightening and loosening episodes across countries, the number of elections, and the type of elections used. Table A8 in the online appendix provides a list of the elections.

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 ex-ante hard to predict. 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. Julio & Yook, 2012; Canes-Wrone & Park, 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.[12] Half of the elections in my sample are close and around three quarters are regular.

Data on macroeconomic and financial sector conditions Changes in regulation likely depend on the state of the economy, particularly the financial sector. I control for key quarterly macroeconomic variables using data from the International Monetary Fund's International Financial Statistics, the OECD, and annual financial sector data from the 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 obtain data on central bank independence from Crowe & Meade (2007), Dincer & Eichengreen (2014), and Garriga (2016). Data on macroprudential institutions is from Edge & Liang (2017) and Cerutti et al. (2015). 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 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, extending an existing dataset from Mishra & Reshef (2019). See online appendix Table A2 for more details.

[12] I 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.

2.2 Econometric Framework

The backbone of my empirical analysis are fixed effects dynamic panel regressions of the following type, as standard in the election cycle literature:

$$\Delta R_{it} = \alpha_i + \mu_t + \beta Pre-election_{it} + \mathbf{X}'\gamma + \psi(L)\Delta R_{it-1} + \varepsilon_{it}, \tag{1}$$

where i and t index countries and year-quarters, respectively. α_i and μ_t refer to a full set of fixed effects. I also consider specifications with $country \times year$ fixed effects. In most cases, ΔR will refer to changes in sectoral capital buffers. These tools mostly target mortgage and consumer credit, which makes them particularly prone to political interference.

The main variable of interest is a dummy for the pre-election period $Pre-election_{it}$ that takes the value of 1 in the quarter *before* an election takes place, following the standard approach in previous studies using quarterly data (see e.g. Schultz, 1995; Canes-Wrone & Park, 2012; Julio & Yook, 2012). In section 3.1, I explore how regulation changes from one year before to one year after elections and find that the *average* effects are most pronounced for pre-election quarters.[13] In principle, election timing may also be affected by changes in financial regulation; I address this in section 3.1 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 variables that describe the state of the economy and financial sector. For the baseline set of controls, I use eleven lagged quarterly macro and seven annual financial sector variables.[14] $(L)\Delta R_{it-1}$ is a vector of lags of the dependent variable to account for autocorrelation in regulatory decisions; in the main regressions, I set the lag polynomial (L) to 1.[15] Note that the results appear to be unaffected by "Nickell bias":

[13] This timing is broadly consistent with research on the effectiveness of macroprudential policies, which suggests that these affect the macroeconomy with a lag of about one quarter (e.g. BIS, 2017). However, I show below that regulation is already looser up to a three quarters before elections during credit booms or economic expansions.

[14] These 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 nominal USD exchange rate, bank capitalization, a measure of banking sector concentration, banks' cost-to-income ratio, the NPL ratio, bank return on assets, bank Z-score, and the share of foreign banks. See the online appendix for more details.

[15] Lag selection tests using the Bayesian or Akaike information criteria suggest autocorrelation of between

they also hold using specifications without lagged dependent variable or estimation using panel GMM (see section 3.5). ε_{it} is an error term that is assumed to be well behaved. Standard errors are clustered by country.

I estimate the baseline regressions using ordinary least squares (OLS). In some specifications, I transform the dependent variables into dummies for a tightening or loosening of macroprudential policy. In principle, these regressions could also be estimated using maximum likelihood estimators. However, this is not feasible for many specifications because of the combination of two-way fixed effects, interaction terms, and completely separated variables.[16] In section 3.5, I replicate all regressions with dummy dependent variables using logistic regressions as well as a bias-corrected logit estimator for two-way fixed effects (Fernández-Val & Weidner, 2016).

2.3 Are Pre-Election Quarters Different?

A potential challenge for identifying electoral cycles in regulation is that macroeconomic and financial sector variables may themselves be subject to an electoral cycle. If, for example, financial conditions before elections are relatively gloomy, regulators may see less reason to interfere or loosen existing measures. In the data, we may thus observe a negative correlation of $Pre-election_{it}$ and ΔR_{it} even in the absence of a causal effect of upcoming elections on regulation.

Whether *observable* fundamentals are different in the run-up to elections is an empirical question. I put this to a first test in Table 2, where I run panel regressions of the type in equation 1 but replace the dependent variable with one of the control variables. I allow for two specifications. In the first, $Pre-election_{it}$ refers to the quarter immediately before an election. In the second, I include dummies for each of the four quarters before elections and plot the linear combination of the estimated coefficients.

I find no evidence for large, systematic observable differences in financial and macroeconomic conditions between the election and non-election periods across countries. This

zero and eight quarters. I use one lag as a baseline. As I show in section 3.5, the exact number of lags makes virtually no difference to the results.

[16]Complete separation of a variable arises when a variable perfectly predicts an outcome. This is the case in my setting because, for example, out of the 51 changes in sector-specific capital requirements in the sample, *none* occur in pre-election quarters. Models with completely separated variables cannot be estimated using maximum likelihood because the likelihood of no change in sector-specific capital requirements in pre-election quarters is infinity by definition.

is true both for the levels of these variables and their first differences, which could pick up high-frequency movements rather than deviations from country means.[17] A simple t-test for the equality of means before elections (relative to all other periods) yields almost equivalent results.[18] For the majority of variables, the t-statistics are clearly below one. Even for the more precise estimates, the coefficients switch signs between specifications. 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 suggestive 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. To interpret the estimates from equation 1 as the effect of elections on macroprudential tools, I thus have to assume that unobserved time-varying country factors that are sufficiently orthogonal to the control variables are not the sole cause of differences in macroprudential regulation around elections. I argue that this assumption is likely to hold.

As we will see momentarily, my finding of looser regulation before elections also holds after including a large number of controls. To abstract from the most obvious confounders, I also allow for a specification of equation 1 that includes country $\times$ year fixed effects. This means comparing the quarters around elections in the *same country* in the *same year*. It also eases the identifying assumption one has to make to believe that changes in macroprudential policy are influenced by upcoming elections.

3 Results

3.1 Baseline Results

To begin, figure 1 plots the average change in the sectoral capital buffers and the overall macroprudential regulation indices for pre-election and all other quarters. For sector-specific tools, the magnitudes go in opposite directions: regulation is loosened before elections and tightened in all other periods in the sample. The bar chart on the right suggests that regulation is, on average, tightened in every quarter, but less so in pre-

[17]The latter results can be found in Table A11 in the online appendix.
[18]These results are plotted in Table A12 in the online appendix.

election quarters.

I next challenge these correlations to a regression-based test based on running equation 1. Table 3 shows the main results. I begin by looking at the index of changes in sector-specific capital requirements that particularly target real estate and consumer credit. These tools are widely used, are likely to immediately affect the median voter, and also exhibit the strongest electoral cycle. I then turn to the results for individual tools and provide graphical evidence.

In column 1, I begin by running a fixed effects panel regression without controls. The coefficient of −0.020 is statistically significant at the 5% level. It is also remarkably large: since the mean change of sector-specific capital buffers is 0.011, the effect of pre-election periods is large enough to explain why regulations loosen at these times compared to the rest of the sample quarters. Given that the standard deviation of the dependent variable is 0.182, this implies electoral cycles explain around 11% of the variation in macroprudential policy decisions. Next, I introduce the vector of macroeconomic and financial sector control variables to absorb differences in observable fundamentals in column 2. Using the relatively large number of controls reduces the number of observations; the point estimate, however, increases to −0.029 and is still precisely estimated. This model specification will serve as the baseline model for most of the paper. In column 3, I add $country \times year$ effects, which effectively means comparing the electoral cycle within the *same country* in the *same year*. This yields an even larger coefficient of −0.041 (significant at the 1% level).[19] It implies that more than 20% of the variation in sectoral capital buffers is driven by pre-election periods. Column 4 uses an alternative approach and absorbs $country{\times}election\ year$ dummies, thereby removing any average differences between years with and without elections. The coefficient of −0.039 is highly similar and also significant at the 5% level. As I will show below, the fact that conditioning on these full sets of interacted dummies does little to the point estimate is because most of the average effect on regulation is concentrated in the quarters immediately before an election.

Could it be that I am not sufficiently conditioning on *past* fundamentals, such as the path of monetary and fiscal policy? In column 5, I add four quarters of lags of each variable. The point estimate and its statistical precision remain unaffected, despite including

[19] Note that the observation count of this specification is slightly higher than that in column 2 because the interacted fixed effects do not require countries to have data on the annual financial sector controls.

an additional 51 covariates. As a last check, I further add a *lead* of each of the control variables. The idea is that, while I do not have data on forecasts for each variable, future realizations may serve as a proxy for expected changes in these fundamentals. Again, this leaves the point estimate largely unchanged at −0.029 and still significant at the 5% level. The fact that the coefficient for pre-election periods is almost unchanged across specifications suggests that differences in economic or financial conditions are unlikely to drive the regulatory easing I observe.

Figure 2 graphically plots changes in sectoral capital buffers around elections. More precisely, I rerun the baseline regression equation 1 but replace the pre-election dummy with dummies for the election quarter and four leads and lags of it. I continue to control for country and year-quarter fixed effects, as well as the control variables. The pre-election quarter stands out in terms of magnitude and statistical significance: on average, the cycle seems to be concentrated immediately before elections, consistent with the literature on political business cycles (e.g. Akhmedov & Zhuravskaya, 2004). As we will see later, regulation is already lower up to three quarters before elections during credit booms. There also seems to be a minor re-balancing in the post-election period, but this is far from statistically significant. I will return to a more formal test of post-election reversals in section 3.4.

I next differentiate between tightening and loosening episodes for individual macroprudential instruments in Table 4. The dependent variable is now a dummy equal to 1 for a tightening or loosening, and 0 otherwise. I consider specifications with and without controls and differentiate tools by whether they target particular sectors or not.[20] I classify changes in general capital requirements and reserve requirements for local currency assets as broader tools, which are mostly intended to affect bank health and total credit.

My findings suggest that electoral cycles in prudential regulation are particularly pronounced for targeted sectoral tools. Crucially, targeted tools have also been found to be among the most effective in curbing credit and house price growth (e.g. Akinci & Olmstead-Rumsey, 2015). What particularly stands out is the highly significantly lower likelihood of tightening before elections for tools aimed at housing and consumer lending. Similar to the patterns in the raw data, the implied effect sizes are large: after con-

[20]Table 1 in the appendix plots full descriptive statistics including observation counts. I consider nonlinear estimators in the robustness section below.

trolling for fundamentals in column 2, pre-election periods explain 15% of the standard deviation in capital buffers aimed at residential mortgages. LTV ratio limits are also less likely to be tightened before elections.[21] I find some less robust results for interbank exposures and concentration limits, which are also targeted tools. The evidence is much more limited for local currency reserve requirements or *general* capital requirements, which do not target particular sectors.[22]

How should we interpret these results? The first takeaway is that there is a strong nonlinearity in the political cyclicality of macroprudential regulation. Regulators seem to forego tightening rather than actively loosen policy before elections. Second, and perhaps more importantly, sectoral tools aimed at mortgages and consumer loans appear to react especially strongly to upcoming elections.

3.2 The Role of Close and Regular Elections

Next, I investigate differences in election timing and competitiveness in Table 5. Column 1 reproduces the baseline specification (as in column 2 in Table 3). In column 2, I consider only pre-election quarters of "regular" elections, defined as those held within the time frame specified in a country's constitution or established as regular practice. 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 run-up to regular compared irregular elections. The pre-election dummy has a coefficient of -0.035 for regular compared to -0.014 for irregular elections (the latter is statistically insignificant). The advantage of only coding regular pre-election quarters as 1 is that they are by definition not the outcome of economic fundamentals. Endogeneous 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

[21] Note that, for the LTV ratio changes, the pre-election dummy refers to the second quarter prior to an election, where the effects seem to be largest.

[22] I also do not find average effects for earlier quarters before elections.

timing is predictable (Nordhaus, 1975).

In columns 4 and 5, 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. The point estimate for close elections in column 4 now jumps to -0.053 (statistically significant at the 5% level). The estimate for elections with a more certain outcome is considerably smaller at -0.005 and statistically insignificant. This suggests that the electoral cycle in sectoral capital buffers is concentrated in periods when incumbents face uncertainty about election outcomes – and may thus have more incentive to interfere with regulation.

Figure 5 plots these differences across types of elections visually. More precisely, I plot the estimated coefficients for the pre-election dummy and the associated 95% confidence intervals. This underscores that the political cycle in macroprudential policy is driven by those elections where incumbents have both the largest incentive and ability to interfere.

3.3 Electoral Cycles and Procyclical Regulation

Whether election cycles in macroprudential regulation pose a potential challenge to the design of regulatory frameworks depends on when they occur. By design, these tools are supposed to be countercyclical. They should be tightened during credit expansions that may be accompanied by increased risk taking. This means electoral cycles are potentially more damaging if they occur during booms, and thus partly undermine (or even reverse) 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.”[23]

[23]Horvath & Wagner (2016) put it similarly: “Pressure from the financial industry and politicians will make it difficult for regulators to impose additional capital when excesses start to materialise.” Gersbach

Existing research also gives some guidance on what we should expect. Antoniades & Calomiris (2018), using county-level data for the US, find 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) extend this prediction to macroprudential policy: they also argue that political economy challenges are most likely during the boom, when such tools have the greatest use. The literature on economic policy reforms also usually finds that reforms often follow crises or protracted downturns, not booms (e.g. Rodrik, 1996; Abiad & Mody, 2005).

I test for heterogeneous cycles by interacting the pre-election dummy with measures of real and financial sector booms in Table 6. 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 the interaction term with real GDP growth. The interaction term has a negative coefficient of -0.021 and is highly statistically significant. This implies that macroprudential regulation is particularly loose before elections when the economy is expanding. I next ask if this is also true for *forecasts* of GDP, using data from the World Bank. Mian, Sufi & Verner (2017), for example, show that such forecasts are systematically too optimistic during credit booms. I find almost equivalent point estimates in column 2. I find highly similar results for the interaction with house price growth, bank profitability, and credit growth – indicators closely monitored by financial stability authorities. Again, the interaction terms are negative and statistically significant.

Figure 3 visualizes the overall implied magnitudes of these results. These coefficients are the linear combination of the pre-election dummy and its interaction with the business and financial cycle indicators (which, recall, are standardized to have a mean of zero and a standard deviation of one). These estimates can be interpreted as the election cycle during a "boom" in these variables. For comparison, I also plot the baseline election cycle estimate from table 3 (column 3). This forcefully illustrates that the electoral cycle weakens attempts to decrease the financial sector's procyclicality: macroprudential regulation is considerably looser before elections precisely when regulators are supposed to lean against the wind. Compared to the baseline estimate, sectoral regulation is more

& 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."

than twice as loose before elections during "credit booms", i.e. when the year-on-year growth in private credit/GDP is one standard deviation above the mean.

Figure 4 plots the estimated cycle around elections during credit booms. This reveals that, during a boom, sectoral regulations are systematically looser in quarters before an election, with little noticeable reversal afterward. I find a highly similar pattern for the other indicators.[24] Again, this is consistent with the interpretation that political pressures can prevent regulators from implementing countercyclical macroprudential policy.

3.4 Do Electoral Cycles Have Permanent Effects?

Up to this point, I have mainly focused on the pre-election period where most of the election-related policy changes appear to be concentrated. A crucial question is whether such cyclical effects are permanent. Put differently: do elections merely postpone difficult macroprudential decisions or do they lead to a gradual loosening of regulation over time?

This question is challenging to answer in a cross-country setting, particularly given the relatively short time frame for which data on macroprudential tools are available. I attempt to provide some suggestive evidence in Table 7, where I introduce a dummy variable $Post-Election$ that is equal to 1 for the two quarters *after* an election took place and 0 otherwise.[25] The post-election dummy captures the pooled effect of the year-quarter dummies after elections in figure 2. This allows me to test formally whether regulation reverses immediately after the election by testing for the linear combination of the pre-election and post-election dummies. I continue to focus on sectoral capital buffers.

The evidence appears to be broadly consistent with the idea that election-induced looser macroprudential regulation is not immediately tightened after elections. While the pre-election quarter dummy is estimated with similar magnitude and statistical significance to the main results, the coefficients on the post-election dummy are considerably smaller and imprecisely estimated. What do these coefficients tell us about the net effect of regulatory cycles? In the bottom rows, I compute linear combinations of the pre- and post-election dummies and test the null hypothesis that the estimated coefficients sum to zero. The first observation is that the net effect is always estimated to be negative.

[24] These results are available upon request.

[25] The results are almost equivalent for different definitions of this dummy, e.g. if I instead assign a 1 to the three or four quarters after an election (available upon request).

Once I condition on $country \times year$ fixed effects to mop up unobserved heterogeneity, the combined estimate also becomes statistically significant at the 10% level. While I urge caution in interpreting these results, they at least suggest the possibility that election cycles introduce a permanent bias that leads to looser regulation over time.

3.5 Robustness Tests

A concern with cross-country panel regressions is that they may not be robust to changes in estimation technique; model specification; sample composition; or different sets of control variables. In Table 8, I thus present a wide range of validity exercises to showcase the robustness of the coefficient estimates while differentiating between tightening and loosening episodes.

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 (Pesaran & Smith, 1995) to account for heterogeneous slopes across countries, which suggests there is little heterogeneity in the pooled main estimates.

Since the dependent variables used here are dummies, it is common practice to use non-linear models such as logit regressions. However, I run into the problems of complete separation and incidental parameter bias due to the two-way fixed effects. Since *none* of the tightening episodes of the sectoral capital buffer occur in pre-election quarters, it is not possible to estimate the maximum likelihood in a logit framework, which is infinity by definition. The bias-corrected estimator of Fernández-Val & Weidner (2016), however, allows for the estimation of panel logit regressions with two-way fixed effects. I also re-run the baseline estimation but restrict the sample to two quarters before and after any election. This effectively limits the "control group" to the immediate period around elections and yields a slightly *larger* estimated coefficient for the sectoral capital buffer.

In Panel B, I deal with concerns regarding sample selection. I start by dropping all countries that, at any point in the sample, 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. If anything, this increases both the point estimates and statistical significance. I next drop the individual continents to validate that the findings are not driven by a particular region. I find that they are 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. If anything, I find somewhat larger results here. I also divide the sample into the pre-crisis (up to 2006) and post-crisis (from 2007) period and find similar results, with somewhat larger coefficients on the sectoral capital buffer tightening post-crisis.

Finally, I deal with the issue of cherry-picking control variables in Panel C. I start by either including only bank controls or macro controls, or alternatively controlling for 20 (instead of 7) financial system controls from the World Bank's Global Financial Development Database. I also address the fact that the control variables are likely to be highly collinear. To overcome this issue, I separately take the first principal component of the 10 quarterly macro variables and 20 indicators of financial conditions, and control for these in the fourth row of Panel C; this also makes no difference to the results. I can also condition on country × year, country × half-year or country × quarter dummies, which take out unobserved variation and country-specific seasonality; this, again, makes no difference to the results. Next, I control for the number of macroprudential tools in a given year via Cerutti et al. (2015), interacted with year-quarter dummies, which does not make a difference, either. Finally, conditioning on detailed region × time or World Bank development level × time dummies also leaves the estimates unchanged.

Overall, it seems fair to conclude that a lower likelihood of a regulatory tightening of sectoral capital buffers is a highly robust feature of the data.

3.6 Randomization Test

Another potential concern with attempting to identify electoral cycles is that other country-level shocks may coincide with fewer regulatory changes. But how likely would it be for such random shocks to generate my results?

I approach this question by conducting a randomization test. More specifically, I begin by picking a random quarter between the start of the sample in 2000q1 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 quarter between the first quarter of 2000 and the fourth quarter of 2003. Next, I generate placebo elections every for years until the end of the sample in 2014. If the initial placebo quarter is 2001q3, the other placebo elections would be in 2005q3, 2009q3, and 2013q3.[26]

I create 1000 sets of these placebo election quarters and then rerun the baseline equation 1 for the tightening of sectoral capital buffers, for which I find the strongest electoral cycles. Figure A3 in the online appendix plots the results of this exercise. Less than 1% of the 1000 random election quarters produce a smaller t-statistic than the one I find in the baseline specification. I conclude that unobserved country-level shocks are unlikely to drive my results.

3.7 Replication in Other Datasets

For the baseline tests, I use the data on macroprudential policy changes from Cerutti et al. (2017). The advantage of this dataset is that it uses consistent coding; has a relatively balanced and broad coverage of both countries and instruments; and has been used widely to assess the *effects* of macroprudential policy. Two notable alternatives include data by Kuttner & Shim (2016), who cover housing-related tools across 58 countries, and a recent effort by Budnik & Kleibl (2018), who cover 28 EU countries.[27]

Figure A2 shows that changes in macroprudential policy in these alternative datasets also exhibit an electoral cycle. This suggests that electoral cycles in macroprudential regulation are not limited to the particular country sample or coding of instruments in Cerutti et al. (2017).

4 What Drives Electoral Cycles in Macroprudential Policy?

Macroprudential regulation exhibits a robust and systematic electoral cycle, and this cycle is particularly pronounced during economic expansions and credit booms. In this section, I attempt to shed some light on the potential underlying mechanisms. I divide

[26] I do not code "irregular" placebo elections because their data generating process is harder to pin down. In principle, however, these should only introduce noise.

[27] Table A16 in the online appendix plots the Pearson correlation coefficients between changes in macroprudential tools across these three sources. The correlations are relatively low, and sometimes even negative, which likely reflects the difficulty of coding regulatory actions. For changes to housing-related taxes in Budnik & Kleibl (2018) and Kuttner & Shim (2016), for example, the correlation is only 0.0644. The variables that exhibit reasonably high correlations are changes to reserve requirements, risk weights, LTV ratios, and DTI ratios. But even in these cases, the correlations range between around 0.33 and 0.80. I thus treat these datasets as independent rather than merging them into one.

these potential channels into three broad groups: opportunistic political motives; the financial sector's political connections and regulatory capture; and uncertainty.[28]

4.1 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 likely most affect the median voter. These are tools such as capital requirements on mortgages or consumer loans, as well as caps on LTV ratios. 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 a noticeable lack of tightening during economic expansions and credit booms. 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 but expected to be close, when politicians have the largest incentives and ability to interfere with policy decisions.

Next, I consider a range of tests that, while not conclusive, also point in the direction of opportunistic political motives for cycles in macroprudential policy. These results can be found in Table 9. In column 1, I begin by introducing the interaction of pre-election quarters with the voice and accountability measure from the World Governance Indicators, which captures the quality of political institutions. Here and in what follows I standardize all continuous indicators to have a mean of 0 and a standard deviation of

[28] In unreported results, I find no evidence that party ideology, proportional representation, having a presidential vs. parliamentary system, or other metrics of constitutional design matter for the severity of electoral cycles.

1. While the pre-election dummy continues to be negative, the interaction term is positive (0.012) and statistically significant at the 5% level. It implies that a country with high voice and accountability (one standard deviation above the mean) has no election cycle in macroprudential tools compared to a country with low accountability (one standard deviation below the mean). This implies that the electoral cycle is particularly pronounced in countries with poor political institutions.

I consider more direct measures of potential political interference in columns 2 through 4. In column 2, I use a proxy for state interventionism based on the frequency of state ownership of enterprises from the Fraser Institute. Higher values mean less state intervention. This index is also associated with a considerably more muted election cycle in macroprudential regulation. A one standard deviation increase relative to the mean almost eliminates the cycle. In column 3, I introduce the interaction with a dummy for whether the country's central bank governor has previous work experience at the Ministry of Finance. This measure is based on extending the dataset in Mishra & Reshef (2019) to the countries in my sample until 2017.[29] I interpret it as a proxy for the personal political connections of regulators. The negative (and statistically significant) interaction term suggests that the election cycle in sectoral capital buffers is concentrated in countries where regulators have closer personal linkages with politicians. In column 4, I construct a proxy for interference with bank supervision more specifically from the survey data collected by Barth, Caprio & Levine (2013). They classify countries into having politically independent bank supervisors or not. The estimated coefficient on the interaction term of 0.034 (significant at the 15% level) is almost equivalent to the estimate of the pre-election dummy itself. This suggests that there is no election cycle when supervisors are independent.[30]

Last, I consider if it matters whether executives face a unified opposition. The idea behind this test is that politicians may not be able to "get away" with manipulating policies if they face little fractionalization in the opposition. Indeed, the estimate of the interaction term (0.015) suggests that political cycles in macroprudential policy are more likely when the parliamentary opposition is more fractured.

[29] I would like to thank Ariell Reshef for kindly sharing an updated version of their dataset with me.

[30] Note that this metric of political independence is based on survey data on *microprudential* bank regulators, not macroprudential institutions. I will investigate the role of the latter in section 5.

Figure 6 again visualizes the overall magnitudes by plotting the linear combinations of the pre-election dummy and its interaction with the proxies in Table 9. This is akin to asking what electoral cycles look like in countries with sound political institutions, low political connections of regulators, low state intervention, or considerable opposition power. In those cases, I cannot reject the null hypothesis of no electoral cycle.

In sum, electoral cycles in macroprudential tools are concentrated in regulations aimed at the median voter; stronger during economic expansions and credit booms; and driven by countries with less developed political institutions, closer personal linkages between politicians and regulators, a weak opposition, and higher state intervention in regulatory decisions. This evidence is at least consistent with the idea that opportunistic political interference may play a role for macroprudential policy.

4.2 Election Cycles, Political Connections, and Regulatory Capture

An alternative explanation for electoral regulatory cycles that is unrelated to concerns about the median voter could be regulatory capture (Stigler, 1971). A large literature has established that political connectedness of firms is a pervasive feature of economies around the world (Faccio, 2006). Such connections are used both by politicians to reallocate funds (e.g. Schoenherr, 2017) and by firms to reward or help politicians (e.g. Bertrand et al., 2018; Chavaz & Rose, 2018). Adelino & Dinc (2014), for example, show that nonfinancial firms with weaker fundamentals lobbied more during the financial crisis of 2008 and were subsequently allocated more government funds. Akey (2015) shows that pre-election donations to politicians increase firm values when the preferred candidate wins. More broadly, financial regulation is often seen as the outcome of political bargaining between banks and governments (e.g. Valuing Changes in Political Networks: Evidence from Campaign Contributions to Close Congressional Elections, Raj; Calomiris & Haber, 2014). Political connections may also interact with the electoral cycle: incumbent governments could signal "good will" to financial sector actors by loosening policy while fundraising.

I test the validity of this hypothesis in explaining election cycles in macroprudential regulation by again introducing an interaction term with pre-election quarters, this time based on different proxies for the connectedness and lobbying power of a country's financial sector. Some of these proxies directly attempt to measure political connections.

Others are based on variables that are plausibly correlated with the intensity of financial sector lobbying.

Table 10 reports the results for changes in sectoral capital buffers. In column 1, I begin by introducing the interaction with a measure of banking sector concentration. One would expect that more concentrated sectors with a few powerful institutions wield more lobbying power.[31] Column 2 uses a more direct measure of lobbying. Specifically, I construct a country-level index based on data from the Political Finance Database of the Institute for Democracy and Electoral Assistance. The index counts whether a country has "bans and limits on private income"; "spending regulations"; and "reporting, oversight, and sanctions." In both specifications, the coefficients on the interaction of pre-election quarters with these measures are small and statistically indistinguishable from zero.

Next, I consider whether political connections matter. In column 3, I use the share of politically connected firms (by market capitalization) constructed by Faccio (2006) as an indicator for linkages between private firms and the government. This measure is supposed to capture the prevalence of "revolving doors" but is not specific to the financial sector. Again, the interaction term is close to zero and also has the "wrong" sign, indicating that higher connections mitigate the cycle. In column 4, I use a measure on the political connections of banks from Braun & Raddatz (2010). This "connected banks" variable captures the share of banks with at least one former politician on the board of directors. Again, the interaction term is clearly insignificant.[32] As a last check, in column 5, I again draw on the newly collected extended data building on Mishra & Reshef (2019). In particular, I introduce an interaction with whether a country's central bank governor has previous work experience in the financial sector. The coefficient on the interaction here has the "wrong" sign and again is statistically insignificant.

While the results based on these proxies should be taken with a grain of salt, the regulatory cycle around elections does not appear to reflect interference aimed at financial institutions. The evidence thus continues to point to government interference aimed at the median voter.

[31] The results here are similar when using other measures of competition, namely the Boone indicator, H-statistic, or Lerner index (available upon request).

[32] I find equivalent results using the other, closely related metrics from Braun & Raddatz (2010).

4.3 Electoral Cycles and Uncertainty

A third potential explanation for observing election cycles in macroprudential tools could be uncertainty about future government policy. This uncertainty could decrease firm investment prior to elections, which may in turn affect regulation. Julio & Yook (2012) and Jens (2017), for example, find that firm investment is lower in the run-up to elections, especially for those with uncertain 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 prior to close elections in a panel of OECD economies (also see Canes-Wrone & Park, 2014).[33] 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 only find robust evidence for electoral cycles in tools aimed at *households*, not broader tools that may equally impact firms. While there may be general equilibrium effects, Canes-Wrone & Ponce de Leon (2018) find, if anything, some evidence for *higher* consumption in pre-election periods; in my sample, the data show no differences in consumption around elections.

Third, and most importantly, I find no evidence that the electoral cycle in macroprudential tools varies with the level of or change in economic policy uncertainty prior to elections. In Table 11, I plot the results from introducing an interaction term 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).[34] The interaction terms of interest are *positive*, small, and far from conventional levels of statistical significance for all measures – inconsistent with

[33] A largely separate literature in finance studies the asset pricing implications of uncertainty about government policy; recent work includes Ľuboš Pástor & Veronesi (2012), Pástor & Veronesi (2013), and Kelly, Ľuboš Pástor & Veronesi (2016).

[34] Because 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.

the "waiting out" hypothesis. In the online appendix Table A13, I show similar results for *changes* in these uncertainty measures. I conclude that uncertainty about future economic policies is unlikely to drive the electoral cycle in macroprudential regulation.

A related explanation for an electoral cycle could be legislative inertia, e.g. because regulators or governments pursue a "hands-off" approach when elections are approaching. It is, however, unclear why this should lead to a change in the overall regulatory stance. Legislative inertia would predict a lower likelihood of loosening *and* tightening policy, but I only find evidence for the latter. Also, it is less clear why inertia should be more important during booms or concentrated in tools aimed at the median voter.

5 Institutional Frameworks and Macroprudential Policy

Do 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; Eijffinger & de Haan, 1996; Crowe & Meade, 2007). 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.

There are several reasons to suspect that independent central banks may not be in an ideal position to "tame" regulatory cycles in targeted instruments. First, we may be able to draw some lessons from countries' experiences with credit controls, which are in many ways historical precursors of macroprudential tools (Elliott, Feldberg & Lehnert, 2013; Kelber & Monnet, 2014; Fischer, 2014). Kane (1977), for example, argues 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". According to Kane, such targeted policies are thus more likely to be hijacked for political purposes.[35] Second, in contrast to monetary policy, the effect of many types of macropruden-

[35]Koetter, Roszbach & Spagnolo (2014) find that central bank control over prudential supervision is not

tial tools can be easily undone by fiscal authorities. A tightening of LTV ratios, for example, could easily be counteracted by a decrease in stamp duties on housing transactions. This might reduce the leeway independent central banks and other financial regulators have over macroprudential policy. It also serves as an argument for why governments should potentially have a vote in deciding macroprudential regulation: if politicians have a say in regulatory decisions, they may be more inclined to work with regulators on solutions that enhance financial stability without large costs on the median voter (see e.g. Tucker, 2016, 2018). Third, many of the cautionary tales about political pressures in the design of financial regulation are set in countries with arguably excellent institutions by international comparison, such as the United States, the United Kingdom, Germany, Ireland, or Spain (Dagher, 2017; Calomiris & Haber, 2014; Fernández-Villaverde, Garicano & Santos, 2013; Dubois, 2016).

I conduct an empirical test by introducing interaction terms with different measures of institutional frameworks for macroprudential policy in Table 12. 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 have one. These committees can 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 actually implement policies.[36] I find that financial stability committees matter for election cycles in macroprudential tools, but only if they can implement policies: the estimated coefficient of 0.039 in column 2 (significant at the 5% level) suggests that having a powerful committee fully mitigates the election cycle in sectoral capital buffers.

Next, I turn my attention to who bears the main responsibility for implementing macroprudential policy. In column 3, I find no evidence that electoral cycles are less pronounced in countries where macroprudential tools are mainly decided on by the central bank in column 4 (drawing on data from Cerutti et al. (2015)). This can be seen as a first indication that macroprudential may differ from monetary policy. In columns 4

associated with cross-country differences in non-performing loans during the 2007-2008 crisis.

[36]For 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).

through 7, I then restrict the sample to countries where the central bank has the main decision powers and investigate the role of central bank independence.[37] These results suggest that higher central bank independence or transparency make little difference for the electoral cycle. The point estimates are far from statistically significant and close to zero in most cases; they are *negative* for the transparency measures. Even the largest positive point estimate on the interaction term in column 4 suggests a limited role for central bank governance: independence would have to increase by almost three standard deviations to undo the election cycle.

I provide some additional evidence on the potential role of central bank independence in the online appendix (Table A14). Here, I rerun the regressions in columns 4 through 7 in the full sample (including countries where central banks do not decide on macroprudential tools). I also run them in the sample where central banks have little say over macroprudential regulation.[38] This exercise broadly confirms the results in Table 12. If anything, central bank independence appears to matter more in the sample where they have little decision-making power over macroprudential tools.

Could it be that central bank independence does not matter for the countries in my sample at all? As a type of placebo test, I build on previous studies (e.g. Block, 2002; Alpanda & Honig, 2009) and replace the dependent variable with two simple measures of monetary policy: the policy rate and the growth in central bank reserves. This is equivalent to asking whether, in the same sample, central bank independence is a moderating factor for political cycles in *monetary* policy. Importantly, recall the finding from 2.3 that the monetary policy measures I use here do *not* exhibit an electoral cycle on average.[39]

Table A15 in the online appendix presents the results of this exercise. The interaction term $Pre-election \times CBI$ is statistically significant in all specifications.[40] The coefficient signs suggest that central bank independence decreases electoral pressures for monetary

[37] I also drop the Eurozone countries from these regressions. The reason is that the central bank independence metrics refer to the European Central Bank; 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 per se.

[38] I also consider an additional metric of de jure central bank independence from Garriga (2016).

[39] More accurately, section 2.3 shows that there is no electoral cycle in central bank reserves or the money market interest rate. Because the money market interest rate and central bank policy rate have a Pearson correlation coefficient of 0.83, I do not find an average electoral cycle for the policy rate, either ($t = 0.19$).

[40] I 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 A15 for details.

policy. For the policy rate in column 3, for example, the estimate of −0.307 on the pre-election dummy suggests central banks are more likely to ease monetary policy prior to elections; the interaction term of 0.773 indicates that this effect is mitigated by central bank independence. Similarly, the growth in central bank reserves is higher in quarters before elections, but considerably less so when central banks are more independent.

Taken together, the results presented in this section suggest that while central bank independence is effective in mitigating electoral cycles in *monetary* policy, it may not be so for *macroprudential* policy. I do, however, find some role for powerful financial stability committees. This can be interpreted as some evidence that macroprudential institutions should also have a role for other players, potentially including government officials (Fischer, 2015; Tucker, 2016).

6 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 with to prevent banking crises or at least softening their impact.

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 that changes in macroprudential tools exhibit a systematic electoral cycle. The correlations I document are not driven by strategic election timing and unlikely to be driven by economic or financial fundamentals. Instead, a large number of cross-sectional tests is most consistent with the interpretation that tightening the policies most directly aimed at the median voter is politically costly, particularly when elections are approaching.

I also find the electoral cycle is most pronounced during economic expansions and credit booms, when past crises may seem more distant (Reinhart & Rogoff, 2009; Malmendier & Nagel, 2011). This can be rationalized in models of opportunistic political cycles in which incumbent governments want to avoid deteriorating fundamentals that they can use to signal competency to voters. Of course, the very point of macroprudential tools is to be *countercyclical*. My results thus suggest that political pressures may inhibit regulators' ability to implement these policies as envisioned.

While *monetary* policy is considerably less sensitive to upcoming elections in countries with independent central banks, I find little evidence that central banks are uniquely insulated from political pressures in implementing *macroprudential* policy. One potential explanation for these differences is that targeted credit policies may have much more obvious distributional effects than monetary policy (Kohn, 2014; Fischer, 2015), as argued more than 40 years ago by Kane (1977).

The findings presented here call for more research into the politics of financial stability frameworks. If political interference in the use of prudential regulation is a constraint, this raises the question which institutional setups can mitigate them. I find some evidence that financial stability committees could make a difference. The results also raise the question of how granular macroprudential tools should be, since it is targeted tools that exhibit a clear electoral cycle, and further which role monetary policy should play in the build-up of systemic risks. Haldane (2017) stresses that, in light of political economy questions, "there is a debate to be had ... about the appropriate degree of discretion to confer on regulators". I hope the evidence I present makes a first empirical contribution to this debate.

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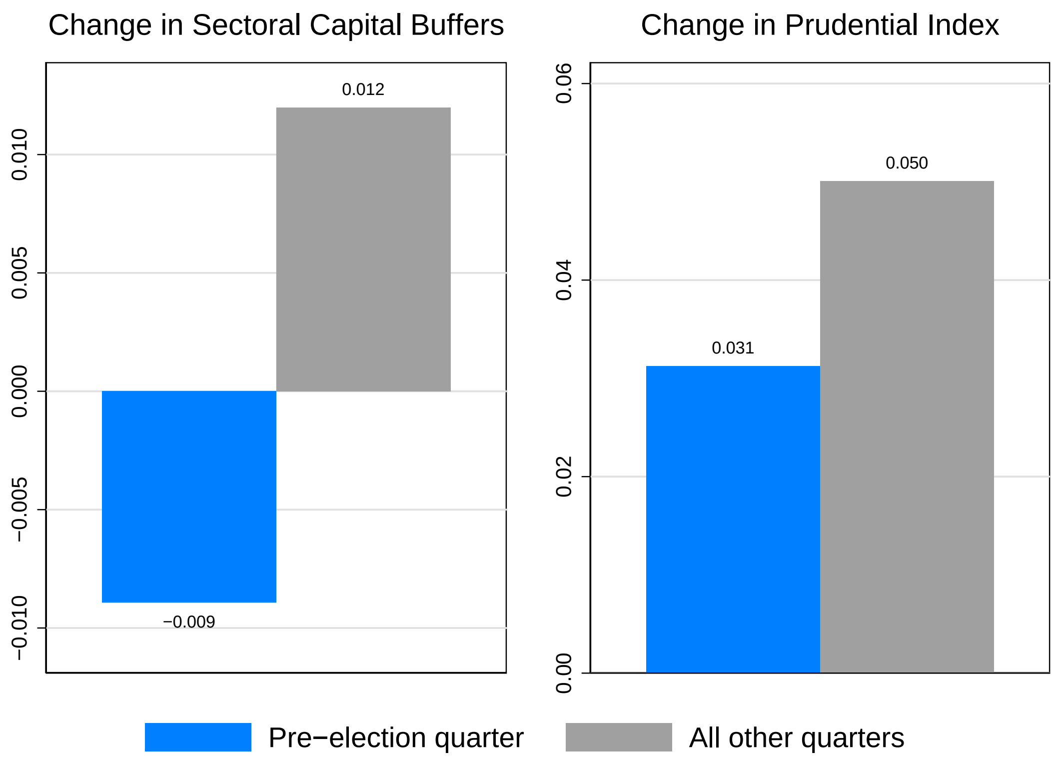
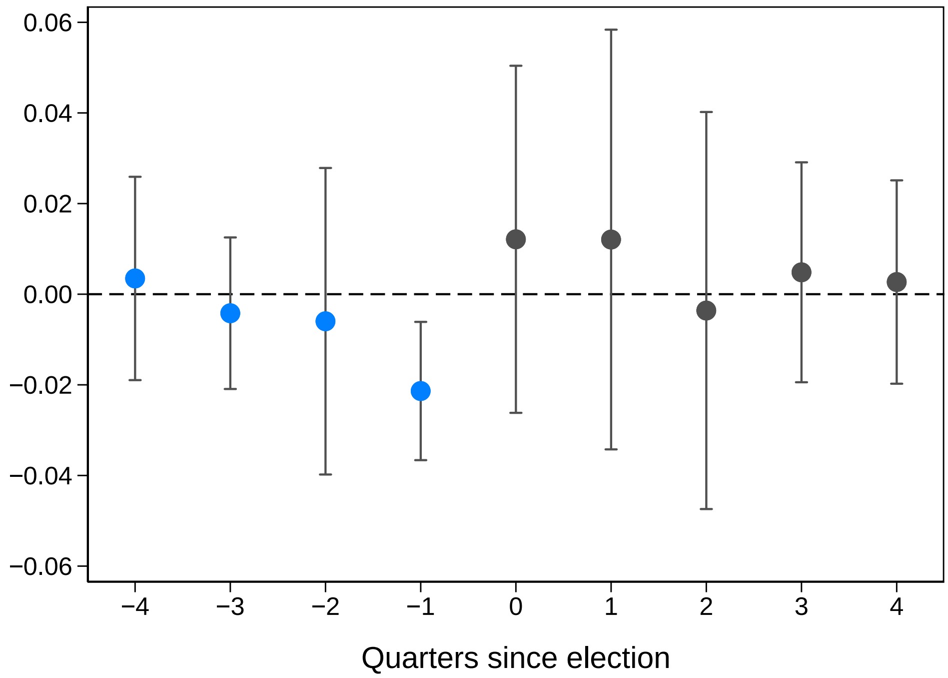
Figures

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



Notes: This figure plots average changes in sector-specific capital buffers and the macroprudential regulation index in pre-election quarters and all other quarters. Positive values indicate regulatory tightening, negative values loosening. The source of the macroprudential policy data is Cerutti et al. (2017).

Figure 2: Elections and Macroprudential Regulation

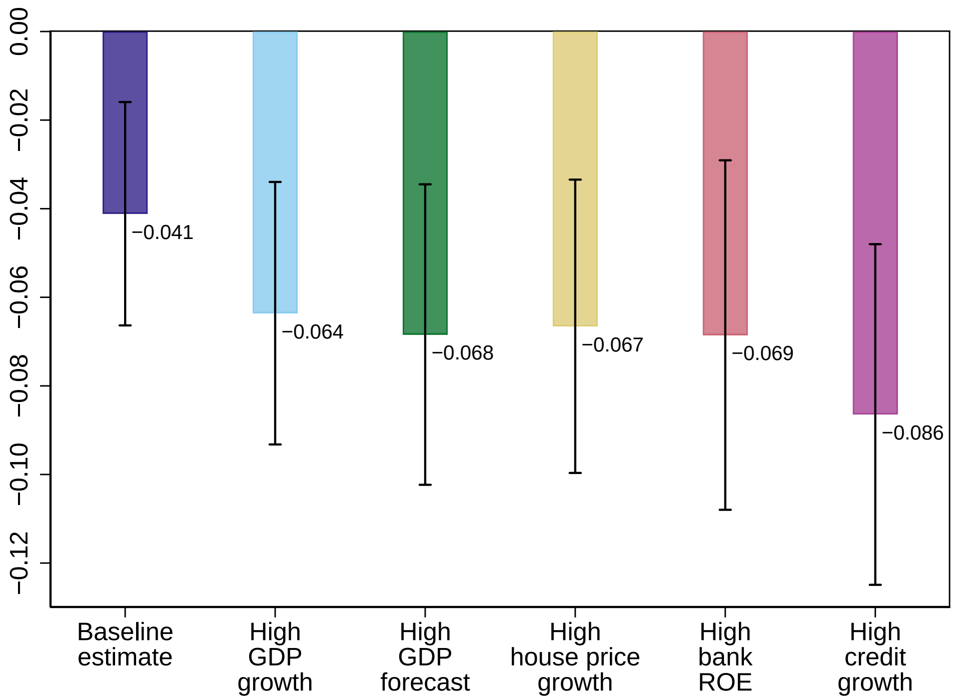


Notes: This figure plots changes in sectoral capital buffers around elections. I plot the estimated OLS coefficients $\hat{\beta}^h$ of the following regression:

$$\Delta R_{it} = \alpha_i + \mu_t + \sum_{h=-4}^{4} \beta^h Election_{it} + \mathbf{X}'\gamma + \psi \Delta R_{it-1} + \varepsilon_{it}$$

The regression also includes one lag of the dependent variable. 95% confidence intervals are based on standard errors clustered by country.

Figure 3: Electoral Cycles and Procyclical Regulation

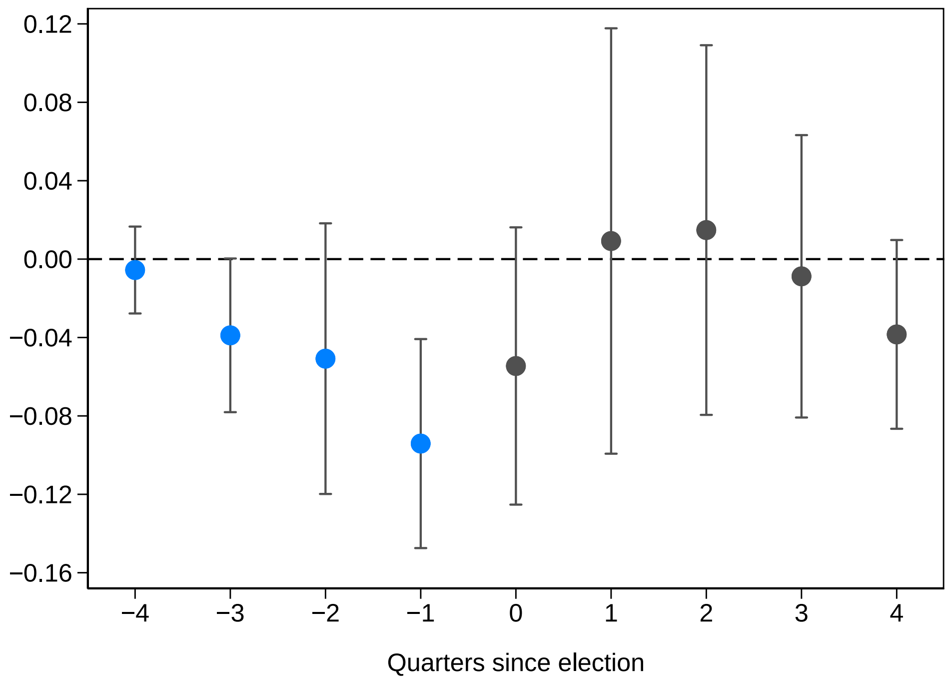


Notes: This figure plots changes in sectoral capital buffers in pre-election quarters during economic expansions and financial sector booms. More specifically, I plot the linear combination of the estimated OLS coefficients $\hat{\beta_1} + \hat{\beta_2}$ of the following regression:

$$\Delta R_{it} = \alpha_i + \mu_t + \beta_1 Election_{it} + \beta_2 Election_{it} \times Interaction_{it} + \theta Interaction_{it} + \mathbf{X}'\gamma + \psi \Delta R_{it-1} + \varepsilon_{it}.$$

$Interaction$ refers to the variable in the bottom row, which is standardized to have a mean of 0 and a standard deviation of 1. The plotted coefficients can thus be interpreted as changes in macroprudential policy prior to elections when the change in the variable is one standard deviation above the mean. For comparison, I also plot the baseline estimate from Table 3 (column 3). The regressions also include one lag of the dependent variable and country × year fixed effects. 95% confidence intervals are based on standard errors clustered by country.

Figure 4: Elections and Macroprudential Regulation during Credit Booms

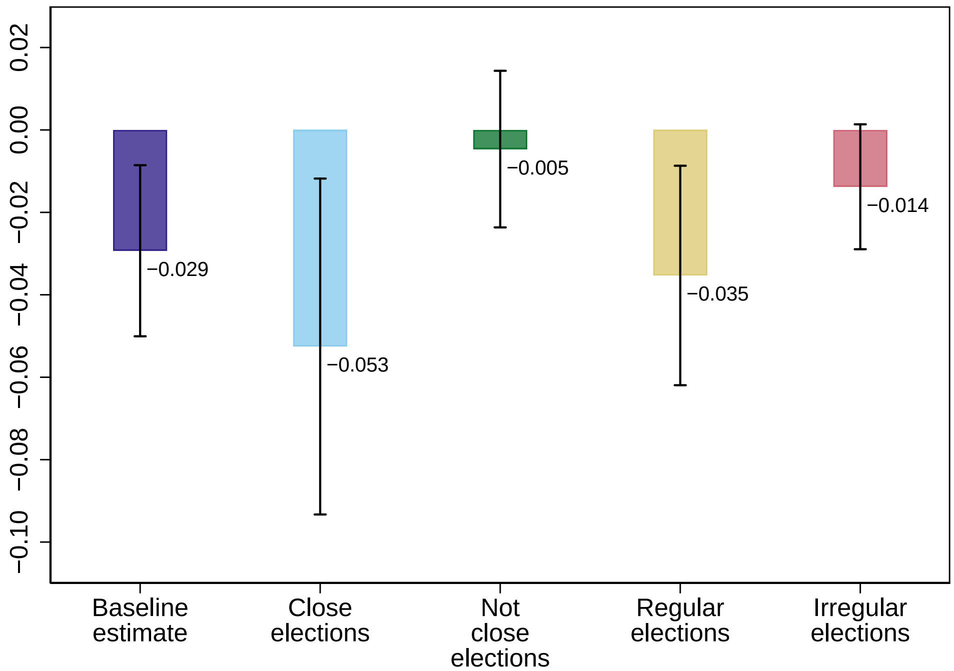


Notes: This figure plots changes in sectoral capital buffers around elections during credit booms. More specifically, I plot the linear combination of the estimated coefficients $\hat{\beta_1^h} + \hat{\beta_2^h}$ of the following regression:

$$\Delta R_{it} = \alpha_i + \mu_t + \sum_{h=-4}^{4} \beta_1^h Election_{it} + \beta_2^h Election_{it} \times \Delta Credit/GDP_{it} + \theta \Delta Credit/GDP_{it} + \mathbf{X}'\gamma + \varepsilon_{it}.$$

$\Delta Credit/GDP$ is standardized to have a mean of 0 and a standard deviation of 1. The plotted coefficients can thus be interpreted as changes in macroprudential policy around elections when the change in private credit/GDP is one standard deviation above the mean. The regression also includes country × year fixed effects. 95% confidence intervals are based on standard errors clustered by country.

Figure 5: Differentiating between Types of Elections

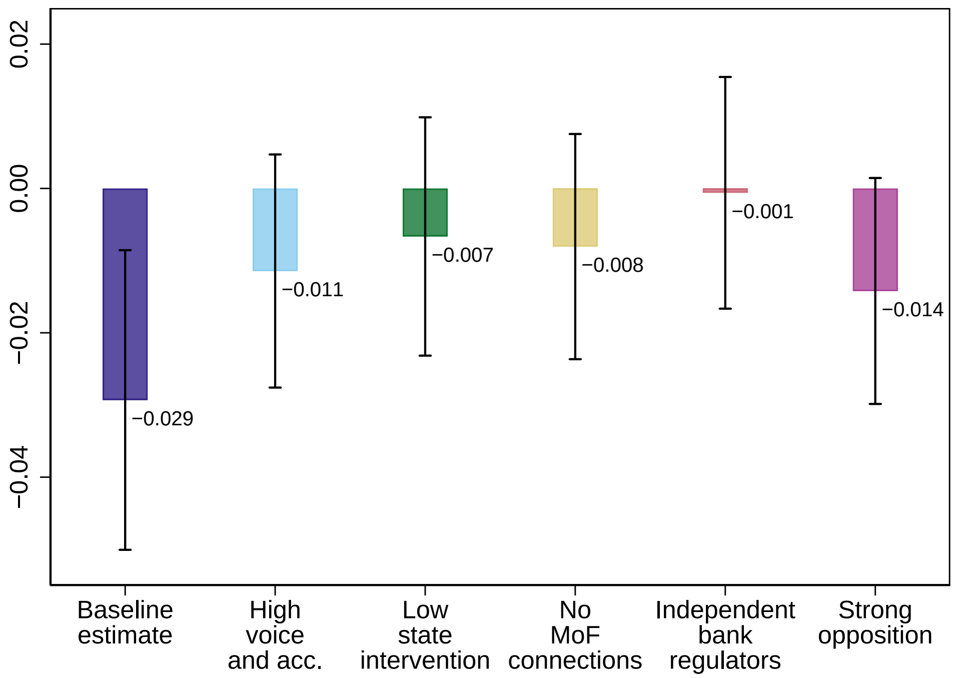


Notes: This figure plots changes in sectoral capital buffers in pre-election quarters depending on whether elections are close or regular. I plot the linear combination of the estimated OLS coefficients $\hat{\beta}$ of the following regression:

$$\Delta R_{it} = \alpha_i + \mu_t + \beta Pre-election_{it} + \mathbf{X}'\gamma + \psi \Delta R_{it-1} + \varepsilon_{it}.$$

For comparison, I also plot the baseline estimate from Table 5 (column 1). The regressions also include one lag of the dependent variable. 95% confidence intervals are based on standard errors clustered by country.

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



Notes: This figure plots changes in sectoral capital buffers in pre-election quarters depending on the level of political institutions, political connections of regulators, state intervention, and strength of the opposition. For the models 1, 2, 3, 5 and 6 (from the left), I plot the linear combination of the estimated OLS coefficients $\hat{\beta_1} + \hat{\beta_2}$ of the following regression:

$$\Delta R_{it} = \alpha_i + \mu_t + \beta_1 Pre-election_{it} + \beta_2 Pre-election_{it} \times Interaction_{it} + \beta_3 Interaction_{it} + \mathbf{X}'\gamma + \psi \Delta R_{it-1} + \varepsilon_{it}.$$

For model 4 I plot $\hat{\beta_1}$, which captures the correlation with upcoming elections for countries where the central bank governor did not previously work at the Ministry of Finance. $Interaction$ refers to the variable in the bottom row, which is standardized to have a mean of 0 and a standard deviation of 1. The plotted coefficients can thus be interpreted as changes in macroprudential policy prior to elections when the variable is one standard deviation above the mean. For comparison, I also plot the baseline estimate from Table 3 (column 2). The regressions also include one lag of the dependent variable. 95% confidence intervals are based on standard errors clustered by country.

Tables

Table 1: Descriptive Statistics

	Observations	Mean	Median	Std. Dev.
Macroprudential policy indices				
Sectoral capital buffers	3248	0.011	0.000	0.186
Prudential regulation index	3248	0.056	0.000	0.390
Targeted macroprudential tools				
Real estate capital buffer	3248	0.006	0.000	0.121
Consumer credit capital buffer	3248	0.002	0.000	0.058
Other sectoral capital buffer	3248	0.004	0.000	0.093
Loan-to-value ratio cap	2986	0.012	0.000	0.158
Concentration limit	1827	0.015	0.000	0.129
Interbank exposure	1065	0.019	0.000	0.143
Reserve requirements (FC)	3248	0.010	0.000	0.258
Broader macroprudential tools				
General capital requirements	3024	0.031	0.000	0.174
Reserve requirements (LC)	3248	-0.008	0.000	0.321
Financial sector variables				
Bank capitalization (%)	3004	8.379	7.900	3.322
Lending concentration	3108	64.839	63.917	20.403
Cost to income ratio (%)	3176	58.677	57.657	15.323
Non-performing loans (%)	2964	5.337	3.200	5.842
Return on assets	3140	1.111	1.166	1.536
Z-score	3180	12.533	11.585	7.547
Foreign bank share (%)	3136	34.321	33.000	24.561
Macroeconomic variables				
Government exp./GDP	2952	0.176	0.185	0.046
Money market rate	3149	5.136	3.820	6.468
Growth in CB reserves	3063	0.143	0.099	0.286
Real credit growth	2931	0.081	0.056	0.160
Real GDP growth	2948	0.032	0.030	0.047
Δ Current account/GDP	2972	0.054	0.000	3.866
Trade/GDP	2988	0.892	0.739	0.535
Investment/GDP	2940	0.227	0.222	0.044
Consumption/GDP	2940	0.579	0.573	0.083
Inflation rate	3135	0.043	0.029	0.053
Log(FX)	3111	2.092	1.293	2.496

Table 2: 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 $C_{it} = \alpha_i + \mu_t + \beta Pre-election_{it} + \varepsilon_{it}$, where C_{it} is one of the control variables in vector $\mathbf{X}'$ (shown in the left column). Each cell is the $\hat{\beta}$ of an individual regression. *1 quarter* refers to specifications where *Pre-election* is a dummy equal to 1 for the immediate pre-election quarter. Under *4 quarters*, *Pre-election* is a vector of dummies for the four quarters prior to an election; I plot the linear combination of the estimated coefficients. The dependent variable is standardized to have a mean of 0 and a standard deviation of 1. The sample is the estimation sample including all controls, as in columns 2 of Table 3. All models include country and year-quarter fixed effects. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

Pre-election horizon:	1 quarter		4 quarters	
	$\hat{\beta}$	t-stat	$\hat{\beta}$	t-stat
Financial sector variables				
Bank capitalization (%)	0.015	(0.698)	0.106	(1.548)
Lending concentration	-0.026	(-1.075)	-0.028	(-0.245)
Cost to income ratio (%)	-0.006	(-0.147)	-0.009	(-0.048)
Non-performing loans (%)	-0.028	(-0.683)	-0.071	(-0.402)
Return on assets	-0.007	(-0.140)	0.096	(0.358)
Z-score	-0.004	(-0.171)	-0.007	(-0.081)
Foreign bank share (%)	-0.006	(-0.736)	-0.035	(-0.963)
Macroeconomic variables				
Government exp./GDP	0.016	(0.596)	0.043	(0.574)
Money market rate	0.011	(0.691)	0.088	(0.642)
Growth in CB reserves	0.130	(1.501)	0.136	(0.439)
Real credit growth	-0.060	(-1.023)	-0.302	(-1.159)
Real GDP growth	0.033	(0.635)	0.006	(0.028)
Δ Current account/GDP	0.043	(0.524)	0.181	(1.155)
Trade/GDP	0.009	(0.749)	0.026	(0.618)
Investment/GDP	0.021	(0.691)	0.241**	(2.083)
Consumption/GDP	-0.004	(-0.148)	0.058	(1.376)
Inflation rate	0.025	(0.747)	0.240	(1.232)
Log(FX)	0.006	(1.406)	-0.010	(-0.406)

Table 3: Baseline Results – Elections and Macroprudential Regulation

Notes: This table shows coefficients from estimating equation 1. The dependent variable is the change in sectoral capital buffers. *Pre-election* is a dummy that is one for the quarter prior to an election. All estimations include one lag of the dependent variable as covariates and other controls as indicated. Note that the specification with $country \times year$ dummies in column 3 does not require countries to have data on the annual financial sector controls, which explains the higher observation count. "Lagged controls" include four quarters of lags of the baseline controls. "Lead controls" include one quarter (or year) of leads. See text for a description of the control variables. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Pre-election	-0.020**	-0.029**	-0.041***	-0.039**	-0.036**	-0.029**
	(0.008)	(0.012)	(0.015)	(0.016)	(0.014)	(0.012)
Observations	3,422	2,409	2,640	2,409	2,251	2,026
R^2	0.04	0.06	0.33	0.07	0.09	0.10
Dep. variable mean	0.011	0.012	0.012	0.012	0.012	0.013
Dep. variable SD	0.182	0.203	0.197	0.203	0.209	0.217
Country FE	Yes	Yes	–	–	Yes	Yes
Country × Year FE			Yes			
Country × Election year FE				Yes		
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls		Yes	Yes	Yes	Yes	Yes
Lagged controls					Yes	Yes
Lead controls						Yes

Table 4: Results by Macroprudential Tool

Notes: This table shows coefficients from estimating equation 1, where the dependent variable is a dummy for tightening or loosening the indicated macroprudential instrument. Each cell represents the results from an individual regression. I only plot the estimated coefficient of the pre-election dummy $\hat{\beta}$. The dummy refers to the quarter immediately prior to an election except for LTV ratios, where it refers to the quarter two quarters before. All estimations include one lag of the dependent variable. In columns 2 and 4, I further include the vector of control variables described in the text. The coefficient on loosened general capital requirements cannot be estimated because these are never loosened in the sample period (see Cerutti et al. (2017)). Standard errors are clustered by country, reported under the estimated coefficient, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Tightening		Loosening	
	Without controls	With controls	Without controls	With controls
	(1)	(2)	(3)	(4)
Macroprudential policy indices				
Sectoral capital buffers	-0.015***	-0.019***	0.004	0.009
	(0.003)	(0.005)	(0.006)	(0.009)
Prudential regulation index	-0.012	-0.015	0.028*	0.030
	(0.019)	(0.025)	(0.016)	(0.020)
Targeted macroprudential tools				
Real estate capital buffer	-0.011***	-0.014***	0.000	0.003
	(0.003)	(0.004)	(0.005)	(0.007)
Consumer credit capital buffer	-0.002**	-0.002*	0.003	0.005
	(0.001)	(0.001)	(0.005)	(0.007)
Other sectoral capital buffer	-0.004**	-0.005*	-0.001	-0.001
	(0.002)	(0.003)	(0.001)	(0.001)
Loan-to-value ratio	-0.002	-0.015***	0.001	0.002
	(0.006)	(0.004)	(0.004)	(0.007)
Concentration limit	-0.007	-0.019**	0.007	0.010
	(0.010)	(0.007)	(0.008)	(0.010)
Interbank exposure	-0.019**	-0.016	-0.000	-0.001
	(0.008)	(0.011)	(0.001)	(0.003)
Reserve requirements (FC)	-0.015*	-0.009	-0.002	-0.005
	(0.009)	(0.006)	(0.006)	(0.008)
Broader macroprudential tools				
General capital requirements	0.003	0.005	—	—
	(0.011)	(0.014)		
Reserve requirements (LC)	-0.004	-0.005	0.014	0.015
	(0.013)	(0.018)	(0.012)	(0.016)

Table 5: Electoral Cycles – Differences across Types of Elections

Notes: This table shows coefficients from estimating equation 1. The dependent variable is the change in sectoral capital buffers. All estimations include one lag of the dependent variable as covariates and other controls as indicated. In column 2, I restrict the pre-election dummy to "regular" elections, defined as those that were not held late or prematurely (while tolerating one quarter of difference); column 3 uses the remaining "irregular" elections. In column 4, I restrict the sample to election periods that are relatively "close", where the outcome is uncertain; column 5 uses the relatively less close elections. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

		Election timing		Election outcome	
	Baseline	Regular	Irregular	Close	Not close
	(1)	(2)	(3)	(4)	(5)
Pre-election	-0.029**	-0.035**	-0.014	-0.053**	-0.005
	(0.012)	(0.016)	(0.009)	(0.024)	(0.011)
Observations	2,409	2,409	2,409	2,007	2,007
Elections	151	107	44	63	63
R^2	0.062	0.062	0.061	0.072	0.070
Dep. variable mean	0.012	0.012	0.012	0.010	0.010
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Table 6: Electoral Cycles and Procyclical Macroprudential Policy

Notes: This table shows coefficients from estimating equation 1 using OLS. The dependent variable is the change in the sectoral capital buffer index. *Interaction* refers to the variable measuring economic expansions or financial sector booms 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 one lag of the dependent variable, country × year fixed effects, time fixed effects, and the macroeconomic control variables as described in the text. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	GDP growth (1)	GDP forecast (2)	House price growth (3)	Bank ROE (4)	ΔCredit/ GDP (5)
Pre-election	-0.043***	-0.042***	-0.037**	-0.040***	-0.046***
	(0.015)	(0.015)	(0.016)	(0.016)	(0.015)
Pre-election × Interaction	-0.021**	-0.027**	-0.029***	-0.029***	-0.041***
	(0.010)	(0.012)	(0.011)	(0.011)	(0.014)
Observations	2,638	2,640	2,147	2,554	2,645
R^2	0.33	0.33	0.36	0.33	0.33
Dep. variable mean	0.012	0.012	0.011	0.012	0.012
Country × Year FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Table 7: Do Regulatory Cycles Have Permanent Effects?

Notes: This table shows coefficients from estimating equation 1 using OLS. The dependent variable is the change in the sector-specific capital buffers index. $Post - Election$ is a dummy variable equal to 1 for the two quarters *after* an election took place, and 0 otherwise. $Pre - election + Post - election$ is the linear combination of the pre-election and post-election dummies. All estimations include one lag of the dependent variable and control variables as described in the text. Column 3 includes $country \times year$ fixed effects that absorb the financial sector controls; the model has more observations because it only requires data on the quarterly macroeconomic variables. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Only FE (1)	Add controls (2)	Add country × year FE (3)
Pre-election	-0.019**	-0.029**	-0.042**
	(0.008)	(0.012)	(0.016)
Post-election (3 quarters)	0.006	0.002	-0.003
	(0.010)	(0.013)	(0.012)
Observations	3,422	2,409	2,640
R^2	0.04	0.06	0.33
Dep. variable mean	0.011	0.012	0.012
Pre-election + Post-election	-0.012	-0.027	-0.045*
	(0.013)	(0.019)	(0.024)
Country FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Controls		Yes	Yes
Country × Year FE			Yes

Table 8: Elections and Macroprudential Regulation – Robustness

Notes: This table shows coefficients from estimating equation 1, where the dependent variable is a dummy for tightening or loosening sectoral capital buffers. Each cell under *Tightening* and *Loosening* represents an individual regression. I only plot the estimated coefficient of the pre-election quarter dummy $\hat{\beta}$ and the associated t-statistic. Unless otherwise indicated, all estimations include one lag of the dependent variable as well as country and year-quarter fixed effects. Note that the coefficients on the tightening dummy cannot be estimated with logit because of perfect separation (they are never loosened prior to elections in the sample). Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Tightening		Loosening	
	$\hat{\beta}$	t-stat	$\hat{\beta}$	t-stat
Panel A: Model specification				
No FE	-0.016***	(-4.81)	0.004	(0.70)
Only time FE	-0.015***	(-4.69)	0.004	(0.73)
AR(0)	-0.015***	(-4.42)	0.004	(0.66)
AR(4)	-0.015***	(-4.33)	0.004	(0.64)
Mean group estimator, no FE	-0.016***	(-4.66)	0.003	(0.63)
Logit, no FE	—		0.601	(0.95)
Logit, only time FE	—		0.695	(0.89)
Logit, both FE	—		1.193	(1.16)
GMM, no FE	-0.021***	(-3.89)	0.004	(0.62)
Only keep election years	-0.025***	(-3.18)	0.002	(0.29)
Panel B: Sample selection				
Only consensus democracies	-0.013***	(-3.81)	0.004	(0.64)
No military leaders	-0.015***	(-4.35)	0.004	(0.66)
Drop Africa	-0.015***	(-4.29)	0.004	(0.65)
Drop Asia	-0.011***	(-3.40)	0.005	(0.65)
Drop Americas	-0.016***	(-4.01)	-0.000	(-0.01)
Drop Europe	-0.018**	(-2.32)	0.007	(0.85)
Drop Oceania	-0.016***	(-4.33)	0.004	(0.64)
Drop countries without changes	-0.036***	(-5.09)	0.018	(0.44)
Drop countries with most changes	-0.010***	(-3.81)	0.002	(0.34)
Pre-crisis only	-0.008***	(-2.68)	0.009	(0.87)
Post-crisis only	-0.020***	(-3.73)	-0.001	(-0.06)
Panel C: Additional controls				
Only bank controls	-0.015***	(-4.08)	0.005	(0.70)
20 bank controls	-0.017***	(-4.20)	0.009	(0.98)
Only macro controls	-0.019***	(-4.19)	0.007	(0.90)
Factor controls	-0.019***	(-4.07)	0.010	(1.01)
Country × Year FE	-0.019***	(-2.92)	0.008	(1.14)
Country × Half-Year FE	-0.036***	(-2.82)	0.010	(1.62)
Country × Quarter FE	-0.017***	(-3.66)	0.006	(0.92)
Regulation × Time FE	-0.015***	(-2.87)	0.004	(0.69)
Region × Time FE	-0.013**	(-2.55)	-0.001	(-0.09)
Development × Time FE	-0.013***	(-3.31)	0.005	(0.84)

Table 9: Political Institutions, Opposition Power, and Electoral Cycles

Notes: This table shows coefficients from estimating equation 1. The dependent variable is the change in sectoral capital buffers. All estimations include one lag of the dependent variable as covariates and other controls as indicated. *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. *CB governor connections at MoF* is a dummy equal to 1 if the central bank's governor worked at the Ministry of Finance prior to his tenure. *Politically independent bank supervision* is a dummy equal to one for countries where the bank supervisors are relatively independent from political influence by the government from Barth et al. (2013). *Unified opposition* measures the concentration of the opposition party in parliament, based on data from Beck et al. (2001) (updated in Cruz et al. (2018)). Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Voice and accountability (1)	State intervention (2)	CB governor connections at MoF (3)	Politically independent bank supervision (4)	Unified opposition (5)
Pre-election	-0.024**	-0.030**	-0.008	-0.035**	-0.030**
	(0.010)	(0.012)	(0.009)	(0.014)	(0.013)
Pre-election × Interaction	0.012**	0.023**	-0.044**	0.034**	0.015*
	(0.006)	(0.010)	(0.021)	(0.016)	(0.008)
Observations	2,292	2,409	2,407	2,269	2,354
R^2	0.06	0.06	0.06	0.06	0.07
Dep. variable mean	0.012	0.012	0.011	0.013	0.011
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Table 10: Election Cycles, Politically Connected Banks, and Lobbying

Notes: This table shows coefficients from estimating equation 1 using OLS. The dependent variable is the change in the sector-specific capital buffers index. *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 one lag of the dependent variable and the baseline control variables as described in the text. *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, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Bank concentration (1)	Campaign fin. lim. (2)	Connected firms (3)	Connected banks (4)	Connected CB governor (5)
Pre-election	-0.030**	-0.029**	-0.035**	-0.029**	-0.035*
	(0.013)	(0.012)	(0.014)	(0.013)	(0.019)
Pre-election × Interaction	-0.007	0.002	0.006	-0.001	0.014
	(0.010)	(0.010)	(0.008)	(0.007)	(0.017)
Observations	2,409	2,409	1,974	2,378	2,407
R^2	0.06	0.06	0.06	0.06	0.06
Dep. variable mean	0.012	0.012	0.015	0.011	0.011
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Table 11: The Electoral Cycle Does Not Vary with Uncertainty

Notes: This table shows coefficients from estimating equation 1. The dependent variable is the change in sectoral capital buffers. $Uncertainty$ 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 one lag of the dependent variable as covariates and the baseline control variables as described in the text. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Log(EPU) (1)	Log(WUI) (2)	Stock price volatility (3)	Log(VIX) (4)
Pre-election	-0.033*	-0.035**	-0.030**	-0.029**
	(0.016)	(0.016)	(0.013)	(0.012)
Pre-election × Uncertainty	0.000	0.015	0.006	0.002
	(0.014)	(0.015)	(0.004)	(0.009)
Observations	1,720	1,811	2,349	2,409
R^2	0.07	0.08	0.06	0.06
Dep. variable mean	0.006	0.015	0.012	0.012
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Table 12: 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 sectoral capital buffers. *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). *C-M* and *D-E* refer to data on central bank independence and transparency from Crowe & Meade (2007) and Dincer & Eichengreen (2014), respectively. The sample in columns 1 through 5 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 one lag of the dependent variable, country and year-quarter fixed effects, and the baseline control variables as described in the text. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

				If central bank decides macroprudential policy...			
			Macropru.	De jure CB independence		CB transparency	
	Financial stability committee		decided	Crowe-	Dincer-	Crowe-	Dincer-
	Advisory	Decides	by CB	Meade	Eichengreen	Meade	Eichengreen
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pre-election	-0.025	-0.036**	-0.023	-0.055***	-0.035*	-0.123	-0.046*
	(0.016)	(0.015)	(0.014)	(0.015)	(0.016)	(0.064)	(0.021)
Pre-election × Interaction	-0.008	0.040**	-0.006	0.020	0.005	-0.016	-0.001
	(0.020)	(0.019)	(0.016)	(0.020)	(0.025)	(0.028)	(0.017)
Observations	2,378	2,378	2,027	527	445	289	552
R^2	0.06	0.06	0.06	0.17	0.26	0.29	0.16
Dep. variable mean	0.011	0.011	0.013	0.034	0.031	0.028	0.031
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Appendix (For Online Publication)

Table A1: Descriptive Statistics of Additional Variables

	Observations	Mean	Median	Std. Dev.
Election timing				
Pre-election	3248	0.065	0.000	0.247
Pre-election (regular)	3248	0.048	0.000	0.213
Pre-election (irregular)	3248	0.018	0.000	0.131
Pre-election (close)	2564	0.032	0.000	0.177
Pre-election (not close)	2564	0.034	0.000	0.181
Post-election (2 quarters)	3248	0.128	0.000	0.334
Economic expansions and credit booms				
GDP forecast	3224	3.287	3.279	1.863
Real house price growth	2408	2.166	1.808	9.191
Banking sector ROE	3140	13.311	14.873	13.105
Credit/GDP growth	2880	0.037	0.029	0.149
Political characteristics				
Voice and accountability	3016	0.745	0.929	0.677
State interventionism	3136	7.573	7.562	1.322
CB governor connections at MoF	3238	0.416	0.000	0.493
Pol. ind. bank regulators	2912	0.269	0.000	0.444
Unified opposition	3168	0.523	0.489	0.236
Political connections and lobbying				
Campaign finance limits	3248	0.131	0.140	0.045
Connected firms	2352	0.083	0.016	0.159
Connected banks	3192	0.632	0.000	1.020
Connected CB governor	3238	0.373	0.000	0.484
Uncertainty				
Log(EPU)	2160	4.737	4.719	0.413
Log(WUI)	2346	-1.677	-1.649	0.678
Stock price volatility	2988	22.299	20.155	12.189
Log(VIX)	3248	2.965	2.943	0.355

Table A2: Variable Description and Sources (1/3)

	Description	Source
Financial Sector		
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.	World Bank GFD
Foreign bank share (%)	Percentage of the total banking assets that are held by foreign banks.	World Bank GFD
Macroeconomic Variables		
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
ΔCurrent 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
Economic expansions and credit booms		
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

Table A3: Variable Description and Sources (2/3)

	Description	Source
Elections		
Pre-election	Dummy variable equal to 1 in quarters prior to a general election.	Various (see text).
Pre-election (regular)	Dummy variable equal to 1 in quarters prior to 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 prior to 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.
Political characteristics		
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	Dummy for countries where bank supervisors are relatively more independent from the government (first survey round).	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)
Connectedness and lobbying		
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)
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

Table A4: Variable Description and Sources (3/3)

	Description	Source
Uncertainty measures		
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)
Macroprudential institutions		
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)
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 A5: Correlation Matrix of Macroprudential Tools

Notes: This table plots pairwise Pearson correlation coefficients of the prudential tools from Cerutti et al. (2017). ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1)	ΔMacroprudential regulation index	1										
(2)	ΔSector-specific capital buffer	0.3110*	1									
(3)	ΔReal estate capital buffer	0.2811*	0.7854*	1								
(4)	ΔConsumer credit capital buffer	0.1286*	0.5338*	0.2144*	1							
(5)	ΔOther capital buffer	0.1743*	0.6402*	0.1311*	0.1672*	1						
(6)	ΔGeneral capital requirements	0.3565*	-0.0101	0.0057	-0.0046	-0.0253	1					
(7)	ΔConcentration limit	0.2515*	-0.0072	-0.0068	-0.0043	-0.0028	0.0054	1				
(8)	ΔInterbank exposure	0.3317*	0.0648	0.0418	0.0059	0.0885*	0.0463	0.1575*	1			
(9)	ΔLoan-to-value ratio	0.5839*	0.0122	0.0169	-0.0021	-0.0052	0.0084	0.0918	-0.0056	1		
(10)	ΔReserve requirements (FC)	0.4296*	0.0104	0.0077	0.0194	-0.0013	-0.0061	-0.0176	0.0143	-0.0059	1	
(11)	ΔReserve requirements (LC)	0.5970*	0.0860*	0.0547*	0.0487*	0.0699*	-0.0564*	-0.0217	0.0083	0.0047	0.3509*	1

Table A6: Tightening and Loosening Episodes, by Pre-Election Quarter

Notes: This table plots the number of tightening and loosening episodes for all prudential tools in the dataset of Cerutti et al. (2017) 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
Macroprudential policy indices						
ΔSector-specific capital buffers	51	0	51	16	2	14
ΔMacroprudential regulation index	329	22	307	166	15	151
Targeted macroprudential tools						
ΔReal estate capital buffer	35	0	35	13	1	12
ΔConsumer credit capital buffer	8	0	8	2	1	1
ΔOther capital buffer	15	0	15	5	0	5
ΔLoan-to-value ratio	64	10	54	26	2	24
ΔConcentration limit	29	1	28	1	1	0
ΔInterbank exposure	24	0	24	1	0	1
ΔReserve requirements (FC)	84	3	81	49	2	47
Broader macroprudential tools						
ΔGeneral capital requirement	88	7	81	0	0	0
ΔReserve requirements (LC)	106	7	99	147	11	136

Table A7: Changes to Regulation and Number of Elections, by Country

	Tightening		Loosening		Type of	Number of
Country	SSCB	Index	SSCB	Index	election	elections
Argentina	0	9	2	13	Presidential	3
Australia	1	5	1	1	Legislative	5
Austria	0	2	0	1	Legislative	4
Belgium	0	1	0	0	Legislative	4
Brazil	5	15	4	8	Presidential	4
Bulgaria	2	11	1	5	Legislative	5
Canada	0	6	0	2	Legislative	5
Chile	0	1	0	2	Presidential	3
Colombia	0	3	0	2	Presidential	4
Croatia	2	11	0	5	Legislative	3
Czech Rep.	0	2	0	2	Legislative	4
Denmark	0	4	0	2	Legislative	4
Estonia	1	5	2	5	Legislative	4
Finland	0	2	0	0	Legislative	3
France	0	6	0	1	Presidential	3
Germany	0	3	0	0	Legislative	4
Greece	0	1	0	1	Legislative	5
Hungary	0	3	0	6	Legislative	4
Iceland	0	3	0	6	Legislative	4
India	6	17	2	9	Legislative	3
Indonesia	0	9	0	1	Presidential	3
Ireland	2	4	0	0	Legislative	3
Israel	4	8	0	0	Legislative	5
Italy	0	2	0	0	Legislative	4
Japan	0	2	0	0	Legislative	5
Latvia	0	9	3	9	Legislative	5
Lebanon	0	4	0	2	Legislative	4

TableA7: Changes to Regulation and Number of Elections, by Country (continued)

	Tightening		Loosening		Type of	Number of
Country	SSCB	Index	SSCB	Index	election	elections
Lithuania	0	3	0	3	Legislative	4
Malaysia	2	7	0	2	Legislative	3
Malta	0	2	0	3	Legislative	3
Mexico	0	3	0	0	Presidential	3
Mongolia	0	4	0	1	Legislative	4
Netherlands	0	6	0	0	Legislative	5
New Zealand	0	2	0	0	Legislative	5
Nigeria	1	6	0	2	Presidential	4
Norway	0	4	2	2	Legislative	4
Peru	1	15	0	6	Presidential	3
Philippines	1	14	0	6	Presidential	2
Poland	3	6	0	2	Legislative	4
Portugal	0	3	0	1	Legislative	4
Romania	0	9	0	8	Legislative	4
Russia	1	13	0	5	Presidential	3
Serbia	2	15	1	11	Legislative	6
Singapore	0	10	0	0	Legislative	3
Slovakia	0	3	0	5	Legislative	4
Slovenia	1	3	0	1	Legislative	5
South Africa	0	2	0	0	Legislative	3
South Korea	1	10	0	4	Presidential	3
Spain	1	3	0	3	Legislative	3
Sweden	2	7	0	0	Legislative	4
Switzerland	3	5	0	0	Legislative	3
Taiwan	0	8	0	4	Presidential	3
Thailand	4	7	0	3	Legislative	6
Turkey	3	16	0	5	Legislative	3

TableA7: Changes to Regulation and Number of Elections, by Country (continued)

	Tightening		Loosening		Type of	Number of
Country	SSCB	Index	SSCB	Index	election	elections
Ukraine	0	4	0	8	Presidential	3
United Kingdom	0	3	0	0	Legislative	3
United States	0	2	0	0	Presidential	4
Uruguay	1	5	0	3	Legislative	3
Total	50	348	18	171		221

Table A8: List of Elections in Main Estimation Sample

Country	Quarter	Country	Quarter	Country	Quarter	Country	Quarter
Argentina	2003q2	France	2007q2	Lithuania	2008q4	Serbia	2007q1
Argentina	2007q4	France	2012q2	Lithuania	2012q4	Serbia	2008q2
Argentina	2011q4	Germany	2002q3	Malaysia	2004q1	Serbia	2012q2
Australia	2001q4	Germany	2005q3	Malaysia	2008q1	Serbia	2014q1
Australia	2004q4	Germany	2009q3	Malaysia	2013q2	Singapore	2001q4
Australia	2007q4	Germany	2013q3	Malta	2003q2	Singapore	2006q2
Australia	2010q3	Greece	2000q2	Malta	2008q1	Singapore	2011q2
Australia	2013q3	Greece	2004q1	Malta	2013q1	Slovak Republic	2002q3
Austria	2002q4	Greece	2007q3	Mexico	2000q3	Slovak Republic	2006q2
Austria	2006q4	Greece	2009q4	Mexico	2006q3	Slovak Republic	2010q2
Austria	2008q3	Greece	2012q2	Mexico	2012q3	Slovak Republic	2012q1
Austria	2013q3	Hungary	2002q2	Mongolia	2001q2	Slovenia	2000q4
Belgium	2003q2	Hungary	2006q2	Mongolia	2005q2	Slovenia	2004q4
Belgium	2007q2	Hungary	2010q2	Mongolia	2009q2	Slovenia	2008q3
Belgium	2010q2	Hungary	2014q2	Mongolia	2013q2	Slovenia	2011q4
Belgium	2014q2	Iceland	2003q2	Netherlands	2002q2	Slovenia	2014q3
Brazil	2002q4	Iceland	2007q2	Netherlands	2003q1	South Africa	2004q2
Brazil	2006q4	Iceland	2009q2	Netherlands	2006q4	South Africa	2009q2
Brazil	2010q4	Iceland	2013q2	Netherlands	2010q2	South Africa	2014q2
Brazil	2014q4	India	2004q1	Netherlands	2012q3	Spain	2000q1
Bulgaria	2001q2	India	2009q2	New Zealand	2002q3	Spain	2004q1
Bulgaria	2005q2	India	2014q2	New Zealand	2005q3	Spain	2008q1
Bulgaria	2009q3	Indonesia	2004q3	New Zealand	2008q4	Spain	2011q4
Bulgaria	2013q2	Indonesia	2009q3	New Zealand	2011q4	Sweden	2002q3
Bulgaria	2014q4	Indonesia	2014q3	New Zealand	2014q3	Sweden	2006q3
Canada	2000q4	Ireland	2002q2	Nigeria	2003q2	Sweden	2010q3
Canada	2004q2	Ireland	2007q2	Nigeria	2007q2	Sweden	2014q3
Canada	2006q1	Ireland	2011q1	Nigeria	2011q2	Switzerland	2003q4
Canada	2008q4	Israel	2003q1	Norway	2001q3	Switzerland	2007q4
Canada	2011q2	Israel	2006q1	Norway	2005q3	Switzerland	2011q4
Chile	2005q4	Israel	2009q1	Norway	2009q3	Taiwan	2000q1
Chile	2009q4	Israel	2013q1	Norway	2013q3	Taiwan	2004q1
Chile	2013q4	Italy	2001q2	Peru	2000q2	Taiwan	2008q1
Colombia	2002q2	Italy	2006q2	Peru	2001q2	Taiwan	2012q1
Colombia	2006q2	Italy	2008q2	Peru	2006q2	Thailand	2001q1
Colombia	2010q2	Italy	2013q1	Peru	2011q2	Thailand	2005q1
Colombia	2014q2	Japan	2000q2	Philippines	2004q2	Thailand	2006q1
Croatia	2000q1	Japan	2003q4	Philippines	2010q2	Thailand	2007q4
Croatia	2003q4	Japan	2005q3	Poland	2001q3	Thailand	2011q3
Croatia	2007q4	Japan	2009q3	Poland	2005q3	Thailand	2014q1
Croatia	2011q4	Japan	2012q4	Poland	2007q4	Turkey	2002q4
Czech Republic	2002q2	Japan	2014q4	Poland	2011q4	Turkey	2007q3
Czech Republic	2006q2	South Korea	2002q4	Portugal	2002q1	Turkey	2011q2
Czech Republic	2010q2	South Korea	2007q4	Portugal	2005q1	Ukraine	2004q4
Czech Republic	2013q4	South Korea	2012q4	Portugal	2009q3	Ukraine	2010q1
Denmark	2001q4	Latvia	2002q4	Portugal	2011q2	Ukraine	2014q4
Denmark	2005q1	Latvia	2006q4	Romania	2000q4	United Kingdom	2001q3
Denmark	2007q4	Latvia	2010q4	Romania	2004q4	United Kingdom	2005q2
Denmark	2011q3	Latvia	2011q3	Romania	2008q4	United Kingdom	2010q2
Estonia	2003q1	Latvia	2014q4	Romania	2012q4	United States	2000q4
Estonia	2007q1	Lebanon	2000q3	Russian Federation	2000q1	United States	2004q4
Estonia	2011q1	Lebanon	2005q2	Russian Federation	2004q1	United States	2008q4
Finland	2003q1	Lebanon	2009q2	Russian Federation	2008q1	United States	2012q4
Finland	2007q1	Lebanon	2010q2	Russian Federation	2012q1	Uruguay	2004q4
Finland	2011q2	Lithuania	2000q4	Serbia	2000q4	Uruguay	2009q4
France	2002q2	Lithuania	2004q4	Serbia	2003q4	Uruguay	2014q4

Table A9: Cross Tabulation of Close and Not Close Elections

Notes: This table shows the proportion of elections that are close. Elections are defined as "close" if the winner's margin of victory is below the sample median.

	Pre-election quarter	Other quarters	Total
Not close	88	2,546	2,634
Close	88	0	161
Total	176	2,546	2,722

Table A10: Cross Tabulation of Regular and Irregular Elections

Notes: This table shows the proportion of elections that are regular. Elections are defined as "regular" if they are held within the time frame specified in a country's constitution or by legislative practice.

	Pre-election quarter	Other quarters	Total
Irregular	60	3,201	3,261
Regular	161	0	161
Total	221	3,201	3,422

Table A11: Testing for Electoral Cycles in Other Variables (First Differences)

Notes: This table tests for electoral cycles in variables other than macroprudential regulation using panel regressions of the type $\Delta C_{it} = \alpha_i + \mu_t + \beta Pre-election_{it} + \varepsilon_{it}$, where ΔC_{it} is the first difference of the control variables in vector $\mathbf{X}'$ (shown in the left column). Each cell is the $\hat{\beta}$ of an individual regression. *1 quarter* refers to specifications where *Pre-election* is a dummy equal to 1 for the immediate pre-election quarter. Under *4 quarters*, *Pre-election* is a vector of dummies for the four quarters prior to an election; I plot the linear combination of the estimated coefficients. The dependent variable is standardized to have a mean of 0 and a standard deviation of 1. The sample is the estimation sample including all controls, as in columns 2 of Table 3. All models include country and year-quarter fixed effects. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

Pre-election horizon:	1 quarter		4 quarters	
	$\hat{\beta}$	t-stat	$\hat{\beta}$	t-stat
Financial sector variables				
Bank capitalization (%)	-0.035	(-0.472)	0.594**	(2.203)
Lending concentration	-0.029	(-0.340)	0.070	(0.244)
Cost to income ratio (%)	-0.011	(-0.123)	-0.296	(-0.809)
Non-performing loans (%)	-0.016	(-0.200)	-0.172	(-0.576)
Return on assets	-0.060	(-0.572)	0.246	(0.548)
Z-score	-0.080	(-1.020)	-0.406	(-1.323)
Foreign bank share (%)	0.015	(0.322)	-0.057	(-0.197)
Macroeconomic variables				
Government exp./GDP	-0.053	(-0.620)	0.049	(0.672)
Money market rate	0.082*	(1.744)	-0.138	(-1.076)
Growth in CB reserves	0.117	(1.052)	0.351	(1.577)
Real credit growth	-0.125	(-0.563)	-0.332	(-1.024)
Real GDP growth	0.155*	(1.900)	0.152	(0.601)
Δ Current account/GDP	0.013	(0.158)	0.086	(1.141)
Trade/GDP	0.146	(1.441)	0.046	(0.457)
Investment/GDP	-0.043	(-0.420)	0.052	(0.425)
Consumption/GDP	-0.006	(-0.052)	-0.051	(-0.537)
Inflation rate	-0.019	(-0.328)	-0.029	(-0.131)
Log(FX)	0.060	(1.386)	0.145	(0.925)

Table A12: Variable Means, by Pre-Election Quarter

	Pre-election quarter	Other quarters	Mean Difference	*t*-test (p-value)
Macroprudential policy indices				
Sectoral capital buffers	-0.014	0.014	0.028	0.009
Prudential regulation index	0.034	0.061	0.027	0.441
Targeted macroprudential tools				
Real estate capital buffer	-0.007	0.009	0.016	0.032
Consumer credit capital buffer	-0.007	0.002	0.009	0.214
Other sectoral capital buffer	0.000	0.003	0.003	0.144
Loan-to-value ratio cap	0.028	0.010	-0.019	0.287
Concentration limit	-0.011	0.018	0.029	0.015
Interbank exposure	0.000	0.018	0.018	0.000
Reserve requirements (FC)	0.000	0.006	0.006	0.751
Broader macroprudential tools				
General capital requirements	0.041	0.030	-0.011	0.514
Reserve requirements (LC)	-0.014	-0.006	0.007	0.769
Financial sector variables				
Bank capitalization (%)	8.196	8.261	0.066	0.825
Lending concentration	64.395	64.716	0.321	0.851
Cost to income ratio (%)	58.924	58.564	-0.360	0.752
Non-performing loans (%)	4.891	4.968	0.077	0.868
Return on assets	1.065	1.149	0.084	0.437
Z-score	12.258	12.230	-0.028	0.959
Foreign bank share (%)	33.762	34.531	0.769	0.706
Macroeconomic variables				
Government exp./GDP	0.180	0.174	-0.006	0.138
Money market rate	4.311	4.370	0.059	0.894
Growth in CB reserves	0.165	0.129	-0.035	0.214
Real credit growth	0.063	0.076	0.013	0.242
Real GDP growth	0.032	0.032	-0.000	0.947
Δ Current account/GDP	0.144	0.034	-0.110	0.504
Trade/GDP	0.874	0.880	0.007	0.874
Investment/GDP	0.227	0.228	0.001	0.787
Consumption/GDP	0.576	0.577	0.001	0.927
Inflation rate	0.037	0.038	0.000	0.931
Log(FX)	1.816	1.863	0.046	0.815

Table A13: The Electoral Cycles Does Not Vary With *Changes* in Uncertainty

Notes: This table shows coefficients from estimating equation 1. The dependent variable is the change in sector-specific capital buffers. $\Delta Uncertainty$ refers to the *change* in the measure of uncertainty listed in the top row: the Economic Policy Index in column 1; World Uncertainty Index 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 themselves (unreported). All estimations include one lag of the dependent variable as covariates and the baseline control variables as described in the text. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	ΔLog(EPU) (1)	ΔLog(WUI) (2)	ΔStock price volatility (3)	ΔLog(VIX) (4)
Pre-election	-0.032**	-0.037**	-0.031**	-0.029**
	(0.015)	(0.015)	(0.013)	(0.012)
Pre-election × Δ Uncertainty	-0.003	-0.002	-0.005	-0.003
	(0.017)	(0.007)	(0.007)	(0.008)
Observations	1,717	1,515	2,319	2,409
R^2	0.07	0.09	0.07	0.06
Dep. variable mean	0.006	0.015	0.012	0.012
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Table A14: Central Bank Independence, 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 sectoral capital buffers. In Panel B, the sample is restricted to countries where 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. *Garriga* is the central bank independence measure from Garriga (2016). The sample excludes the Eurozone countries, where these measures refer to the European Central Bank, which is not in charge of macroprudential policy. The continuous interaction variables are standardized to have a mean of 0 and a standard deviation of 1. All estimations include one lag of the dependent variable, country and year-quarter fixed effects, and the baseline control variables as described in the text. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	De jure CB independence			CB transparency	
	Crowe-Meade (1)	Dincer-Eichengreen (2)	Garriga (2016) (3)	Crowe-Meade (4)	Dincer-Eichengreen (5)
Panel A: Full sample					
Pre-election	-0.039**	-0.026*	-0.036***	-0.050*	-0.038**
	(0.015)	(0.013)	(0.013)	(0.025)	(0.016)
Pre-election × Interaction	0.016	0.012	0.033	0.001	0.012
	(0.013)	(0.017)	(0.022)	(0.014)	(0.010)
Observations	1,827	1,628	1,883	1,166	1,852
Panel B: Countries where central bank has majority decision making power					
Pre-election	-0.055***	-0.035*	-0.047***	-0.123	-0.046*
	(0.015)	(0.016)	(0.014)	(0.064)	(0.021)
Pre-election × Interaction	0.020	0.005	0.008	-0.016	-0.001
	(0.020)	(0.025)	(0.019)	(0.028)	(0.017)
Observations	527	445	579	289	552
Panel C: Countries where central bank does not have majority decision making power					
Pre-election	-0.032	-0.024	-0.029	-0.046	-0.034
	(0.024)	(0.023)	(0.022)	(0.028)	(0.024)
Pre-election × Interaction	0.028	0.034	0.039	0.007	0.033
	(0.028)	(0.033)	(0.036)	(0.029)	(0.020)
Observations	1,043	988	1,043	746	1,043

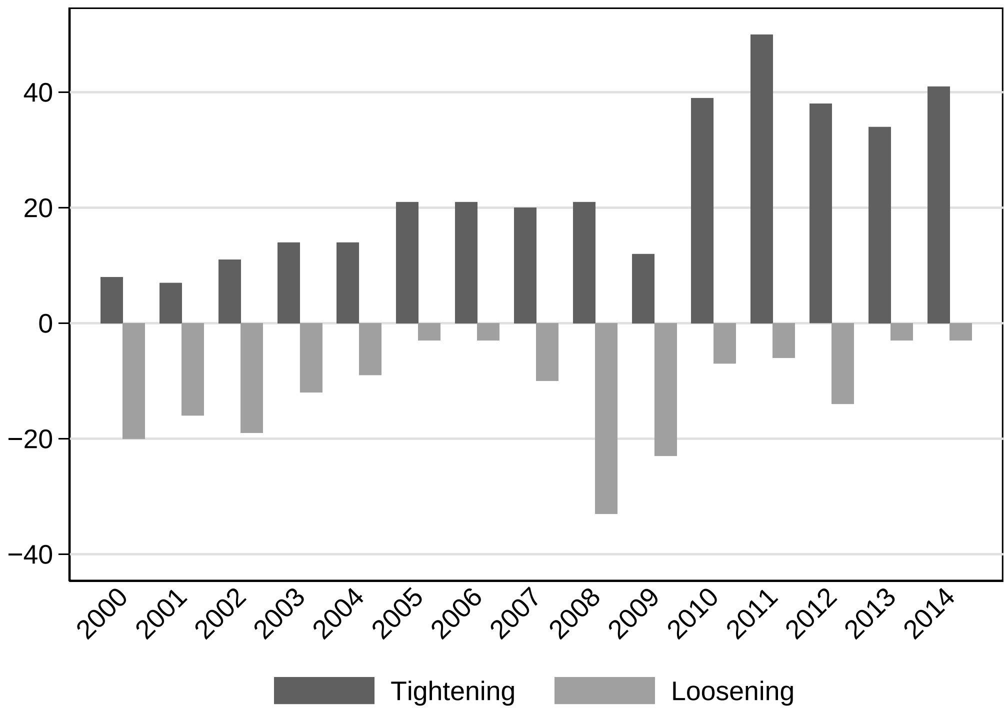
Table A15: Central Bank Independence and Election Cycles in Monetary Policy

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 country fixed effects, year-quarter fixed effects, and the baseline control variables as described in the text except the growth in central bank reserves. For the policy rate I include four lags 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, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

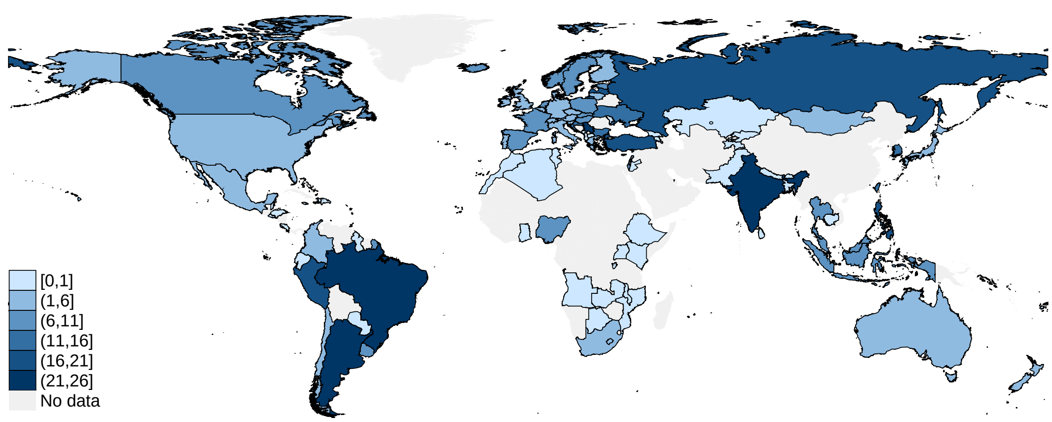
	With Eurozone		Without Eurozone	
	Δ Policy rate	Δ CB reserves	Δ Policy rate	Δ CB reserves
	(1)	(2)	(3)	(4)
Pre-election	-0.269*	0.077**	-0.307*	0.082***
	(0.152)	(0.031)	(0.160)	(0.032)
Pre-election × CBI	0.689*	-0.128**	0.773*	-0.140**
	(0.397)	(0.057)	(0.417)	(0.059)
Observations	1,784	1,947	1,738	1,900
R^2	0.342	0.517	0.339	0.521
Dep. variable mean	-0.115	0.090	-0.117	0.090
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



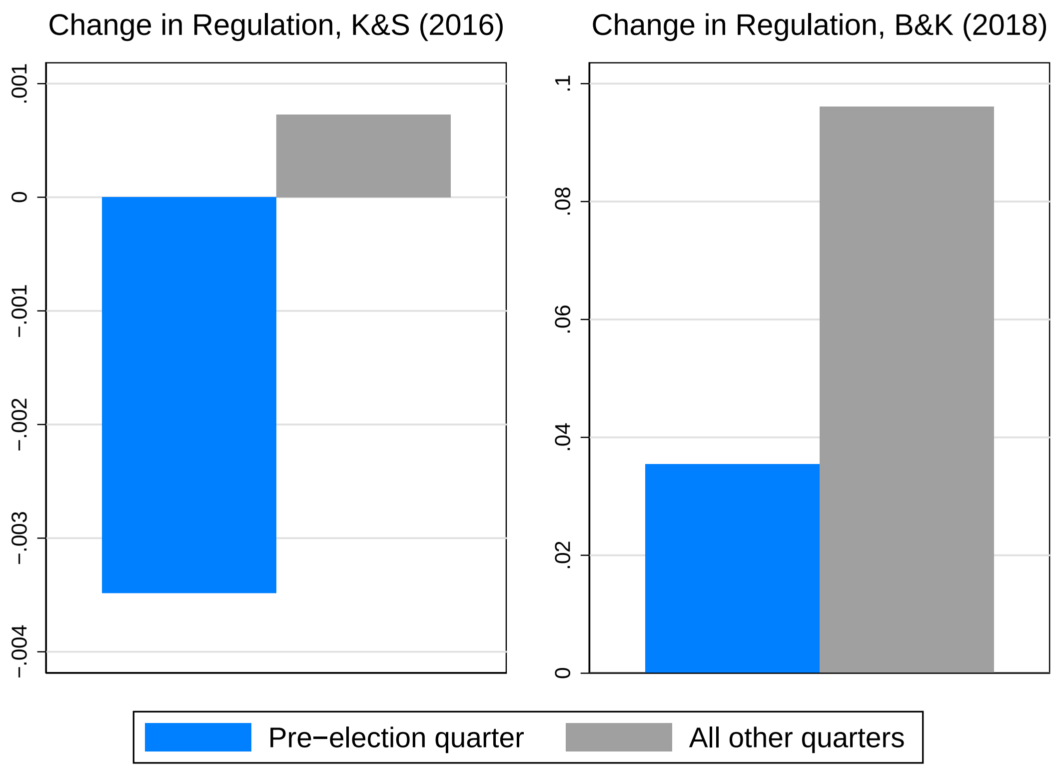
Notes: These figures plots the total number of tightening and loosening episodes in the macroprudential regulation index of Cerutti et al. (2017) for all sample countries between 2000 and 2014.

Table A16: Comparing Datasets on Macroprudential Changes

Notes: This table plots the Pearson correlation coefficients of changes to macroprudential regulation as identified by Cerutti et al. (2017) ("IMF"), Budnik & Kleibl (2018) ("B&K"), and Kuttner & Shim (2016) ("K&S"). The number of overlapping observations is reported in square brackets. I always use the "applied" dates for the B&K dataset (which yields higher correlations). For Δ *Concentration/exposure limits*, I use the IMF variable *concrat*, but results are similar for *ibex* (which refers to interbank exposures). For risk weights, I use the IMF variable *sscb* (also used for the sectoral capital buffers). For reserve requirements, I use the IMF variable *rr_local*, but the results are similar for *rr_foreign* or combining both into one variable. For Δ*Liquidity requirements*, I use the B&K category *Liquidity requirements and limits on currency and maturity mismatch*.

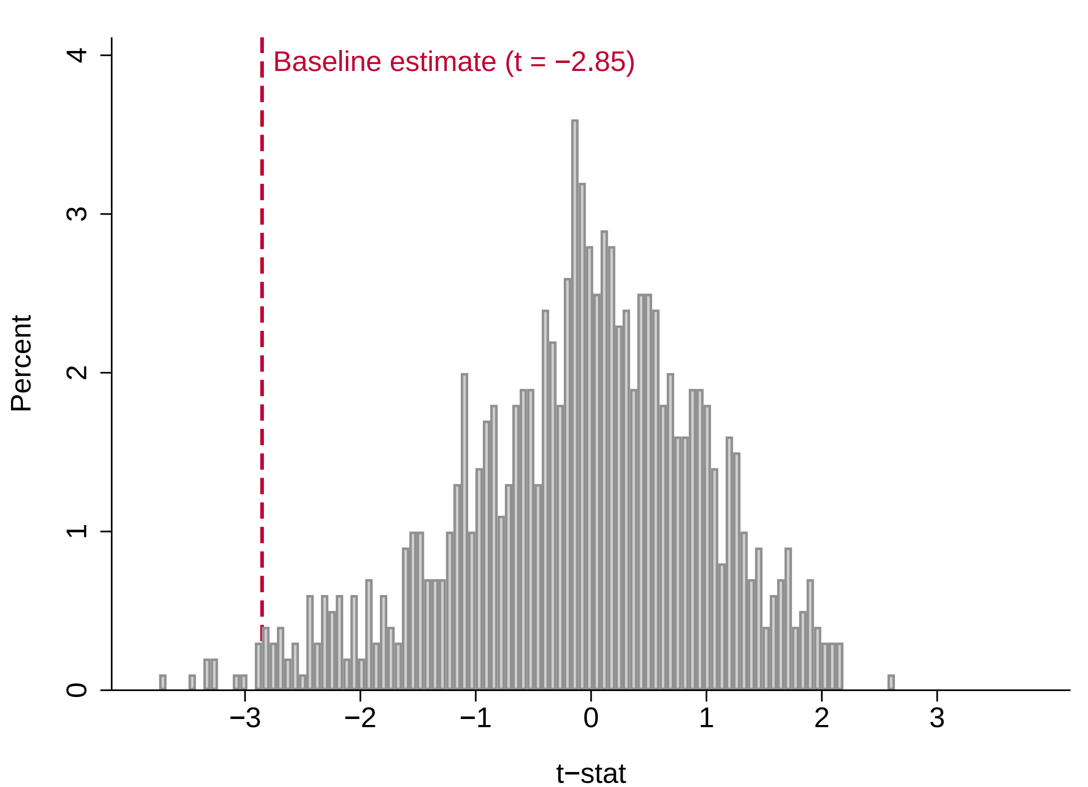
	Corr(IMF,K&S)	Corr(IMF,B&K)	Corr(B&K,K&S)
Δ(Sectoral) capital buffers		0.0483	
N		*[1,560]*	
ΔGeneral capital requirements		0.2382	
N		*[1,560]*	
ΔConcentration/exposure limits	-0.0731	0.1854	0.1416
N	*[1,491]*	*[668]*	*[1,696]*
ΔProvisioning			0.2306
N			*[1,696]*
ΔRisk weights	0.4264	0.3584	0.3311
N	*[1,560]*	*[2,722]*	*[1,696]*
ΔTax/Levy			0.0644
N			*[1,696]*
ΔReserve requirements	0.6872	0.4301	0.3519
N	*[2,722]*	*[1,560]*	*[1,696]*
ΔLTV ratio	0.7953	0.4682	0.3328
N	*[797]*	*[467]*	*[1,696]*
ΔD(S)TI ratio			0.6873
N			*[1,696]*
ΔCredit growth			0.2235
N			*[1,696]*
ΔLiquidity requirements			0.1251
N			*[1,696]*

Figure A2: Election Cycles in Other Datasets



Notes: This figure plots the arithmetic average net changes in macroprudential regulation in pre-election quarters and all other quarters based on the data from Kuttner & Shim (2016) and Budnik & Kleibl (2018). I calculate net changes as the sum of changes in all policy tools in a quarter. Positive values indicate a tightening, negative values a loosening of policy.

Figure A3: Placebo Test with Randomized Election Timing



Notes: This figure plots the t-statistics of the estimated $\hat{\beta}$ coefficients from regressing dummies for the tightening of sector-specific capital buffers on 500 sets of placebo pre-election dummies. These placebo dummies are calculated by first choosing a random quarter between 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 × year fixed effects, the baseline control variables, and one lag of the dependent variable. The red vertical lines indicate the coefficients estimated with the same regression specification and the actual pre-election quarters in the data. Only 1% of the t-statistics of the placebo pre-election quarters yield smaller values than that in the data.