

Political Credit Cycles - Myth or Reality?*

Andreas Kern[†]

Puspa Amri[‡]

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Abstract

This paper tests the existence of political credit cycles, the positive co-movement between credit and elections. While support for this relationship has been found in some single-country studies, the link between electoral cycles and credit expansion has seen little exploration at the multi-country level. Using a comprehensive data set covering bank and non-bank credit in 165 countries from 1960 to 2013, we show that both government and private credit significantly increase in election years. This suggests the possibility that politicians use not only fiscal and monetary policy to court voters, but also implement credit market policies such as interest rate subsidies and tax breaks for debt to enhance credit growth. However, we find that a higher degree of financial openness weakens the frequency and magnitude of political credit cycles; yet the conditional effect of financial openness may be offset under the presence of institutional constraints on monetary and fiscal policy. Our findings are robust to different model specifications.

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[†]McCourt School of Public Policy, Georgetown University; Contact: ak679@georgetown.edu.

[‡]Ithaca College and Claremont Institute for Economic Policy Studies; Contact: pamri@ithaca.edu

1 Introduction

Political business cycles is one of the most widely-debated ideas in political economy, yet surprisingly, existing work mostly focuses on macroeconomic outcomes (e.g., output and inflation) or policy instruments (e.g., monetary and fiscal), and less so on credit and financial variables.¹ The inconclusiveness of existing evidence raises the question whether political credit cycles are a fashionable myth that emerged in policy debates around the Global Financial Crisis or represent a (long-standing) political reality?

A wealth of political economy literature suggests the latter. All else equal, voters place a premium on candidates who can deliver good economic outcomes (see e.g., Franzese (2002)). According to the political business cycle literature, incumbents try to expand monetary and/or fiscal policy to court voters. We argue that similar incentives are present with respect to credit policies. When citizens find it easier to borrow, they spend and invest more, boosting growth and employment, and thus increasing a politician's re-election chances. In fact, policies that reduce households' borrowing costs, such as mortgage tax-deductions have become attractive options for economic redistribution (Rajan, 2010; Chinn and Frieden, 2011; Ansell, 2014). For instance, since the 1990s, Fannie Mae and Freddie Mac have made their underwriting more flexible and created a variety of low down-payment products, which substantially increased credit availability (Ahlquist and Ansell, 2014).

Although enhanced credit growth has short-run benefits, political interference in credit markets – if pursued excessively – can be costly in the long-run. Besides nudging consumer- and/or asset-price inflation, credit granted on grounds other than the borrower's expected ability to repay represent a distortion. Several country studies (e.g., India, Pakistan) have shown that poorly-designed credit policies lead to a dramatic rise of indebtedness, culminating in higher loan defaults and even bank failures (e.g., Khwaja and Mian (2005)).

While the focus of previous work has been on analyzing within-country experiences, our paper provides systematic evidence on the effects of elections on credit. Drawing on insights from the political business cycle theory, we develop (and test) an explanation of how credit can be an attractive

¹Notable exceptions include Dinç (2005); Cole (2009); Jackowicz, Kowalewski and Kozłowski (2013).

instrument to win electoral support. Furthermore, given that financial openness limits the degree of autonomy for implementing domestic economic policies (e.g., Clark and Hallerberg (2000)), we test whether such constraints also apply to opportunistic electoral behavior. We hypothesize that the magnitude of political credit cycles depends on the degree of financial openness.²

To test our hypotheses, we assemble a novel data set on credit that covers 165 countries in the time span between 1960 and 2013. Our paper departs from others in two ways. First, we include both private and public credit from not only banks but also non-bank financial institutions to account for different channels through which pre-election credit manipulation may occur. Second, we measure credit as deviations of real credit from a country-specific trend – instead of unfiltered credit data – which allows us to detect unusual credit jumps above and beyond ‘regular’ movements.

A central finding in our study is that government and private borrowing are about 0.1 standard deviations higher in election years and that this effect triples in magnitude (for public borrowing), when governments own the majority of the banks. We also find that the frequency of political credit cycles varies substantially across income groups, regions, time, and different political and institutional settings. For example, we find that the relationship between elections and credit is stronger in presidential compared to parliamentary political systems. We also find support for our second hypothesis: political credit cycles appear to occur less frequently in financially open economies.

We conduct a broad range of robustness tests. Particularly, we show that the effect of elections on credit is not necessarily a result of monetary and fiscal policy expansions. Notably, we find that institutional constraints on monetary and fiscal policy in fact amplify the effect of elections on credit, suggesting that policy makers find other ways to ‘*prime the credit pump*’ before elections, such as perhaps through credit subsidies. Only when fiscal and monetary constraints are combined with a fixed exchange rate regime are political credit cycles in financially open economies mitigated.

Our work contributes to several strands of existing work. First, we add to the political business cycle scholarship by highlighting an additional, separate policy instrument (i.e. credit policies)

²For instance, Dubois (2016) and Kaplan (2013) survey the effects of international capital mobility on domestic economic policies. While most studies find that high capital mobility discourages governments from adopting excessive expansionary policies, this effect varies in magnitude across developed versus developing countries.

to boost the economy before election. Second, while our inclusion of public borrowing dovetails the more conventional political budget cycle literature (e.g., Baber and Sen (1986)), we focus on government borrowing from domestic banks and non-bank financial institutions. Beyond focusing on credit, we contribute to the growing literature that explores how international economic factors constrain politicians' opportunistic electoral behavior (Oatley, 1999; Kaplan, 2013). Finally, our work is also related to the fast growing literature studying the political economy of credit market dynamics (Schularick and Taylor, 2012; Broz, 2013; Ansell, 2014).

2 Synthesis and Hypotheses

Our central claim is that governments seek to '*prime the pumps*' before elections, subject to the financial means available in domestic and international financial markets. We frame our theoretical discussion based on three key pillars. First, we develop a theoretical frame to explain why policymakers have a motive to manipulate credit markets around elections and we discuss conditions that enable these electoral manipulations. Second, we present potential instruments to achieve this goal. Finally, we evaluate how financial openness affects the opportunity to engineer political credit cycles. We synthesize our theoretical expectations in two hypotheses and discuss several scope conditions.

The Motive: The Political Economy of Credit Expansion

The primary motive for any incumbent to deliver a pre-election stimulus is to bolster their electoral prospects. While traditional political business cycle theories emphasize using monetary and/or fiscal policy (Nordhaus, 1975; Tufte, 1978), credit-enhancing policies such as interest rate subsidies, tax incentives for debt, and other policies that ease borrowing constraints for households and firms constitute powerful policy instruments. The benefits of credit in facilitating economic activities of firms and households are well-recognized. When credit is expanding, households and firms obtain easier access to new loans and they also benefit from cheaper refinancing of existing loans (Ansell, 2014). Moreover, credit expansions are usually followed by asset price increases. This leads to an appreciation of borrowers' net worth, which further amplifies their ability to borrow (Bernanke,

Gertler and Gilchrist, 1999). These positive asset valuation effects benefit households with private pensions accounts and also financial firms whose revenues are affected by interest income and credit-funded sales (Pagliari, Phillips and Young, 2015).

A second factor is politically-motivated redistribution. Scholars have shown that broadening access to credit was often used in India and Thailand to target rural voters (Phongpaichit and Baker, 2004; Cole, 2009; Kumar, 2015). Beneficiaries of such policies are not limited to the poor. In exchange for campaign contributions, Brazilian companies with close ties to the government were able to secure loans with favorable terms from Brazil's state-owned banks (Claessens, Feijen and Laeven, 2008).

As noted in the political budget cycles literature, electoral policy manipulation can be problematic as short-run benefits of fiscal expansions (such as lower unemployment) come at the expense of long-term macroeconomic costs (such as higher inflation). The policy choices of stimulating credit is also subject to this inter-temporal trade-off. The benefits of debt-fueled expansions are felt in the short-run, while the costs of the aggressive use of these policies – vulnerability to financial crises due to rising indebtedness and higher inflation – tend to show up later on.³

Why do voters then reward some politicians for this behavior despite the harmful long-run costs? One answer is because voters are myopic and are slow in adjusting inflation expectations (Nordhaus, 1975). If politicians timed the economic boom just before elections such that inflation occurs after the election date, their strategy would pay-off (Drazen, 2000). However, if voters were to form rational inflation expectations instead – a point of contention which has spawned a second generation of more advanced PBC models – such a strategy would backfire as voters would punish incumbents that systematically inflate the economy to win votes. Nonetheless, the more advanced models argue that such manipulations are conceivable even when voters are rational because of imperfect information. Unable to observe the incumbent's true competence in real time, policymakers increase spending on public goods before elections to appear more competent in their voters' eyes (Rogoff and Sibert, 1988; Shi and Svensson, 2006).

³One caveat is that in recent instances of credit booms in advanced economies asset price inflation and the build-up of macro-financial vulnerabilities did not translate into increasing consumer price inflation (e.g., Hume and Sentance (2009)).

The ability of governments to engage in electoral manipulation depends on the extent to which voters can be ‘fooled’ (Nordhaus, Alesina and Schultze, 1989). One might argue that the costs of private credit remain private as households alone shoulder the debt burden. This is not the case if governments provide interest rate subsidies or guarantees private debt, a common practice in many (yet not exclusively to) developing countries.⁴ In our view, this possibility of obtaining private benefits at the (partial) expense of the public is what prompts households to allow such electoral manipulations.⁵

Second, similar to households, firms may not internalize the financial stability implications of their financial decision making. Although firms might postpone some investments during election years due to uncertainty,⁶ this effect might be offset all together if firms have access to financial hedging instruments, or if their investment is backed by government guarantees. Moreover, when political connections are instrumental for easier access to financing, firms benefit from benign lending conditions and will act upon it (Khwaja and Mian, 2005; Claessens, Feijen and Laeven, 2008).

Third, financial intermediaries have similar incentives to play along in the political credit cycle game. Similar to households and firms, scholars have found that banks do not entirely internalize that their lending behavior undermines financial stability, especially when they are facing tight competition for market shares and pursue short term objectives (Rajan, 1994; Acharya, 2011).

Taking these factors together, even when households, firms and banks are able to form rational expectations concerning the intertemporal trade-off underlying excessive credit expansions, they may not internalize the adverse effects of their behavior. As a result, there are substantial electoral gains to be made for politicians to enhance credit before elections.

⁴For example, Carvalho (2014) finds that Brazilian firms eligible for credit subsidies increased employment in politically strategic regions near re-election.

⁵Moreover, households may have such a high discount rate that they attach little weight to the future consequences of high debt burden. In fact, financially less literate voters might even lack awareness of the true amount of their personal debt burden (Zinman, 2015).

⁶For example, Durnev (2010) shows that firm investment more heavily relies on current cash flows rather than demand more credit during elections.

The Instrument(s): Conventional vs. Non-Conventional Credit Market Policies

To engineer political credit cycles, policymakers can draw on a multitude of direct and indirect instruments, including monetary policy. The empirical evidence supporting electoral cycles in monetary policy is, however, much weaker than fiscal policy due to the rise of politically independent central banks (Alt and Chrystal, 1983; Clark et al., 1998). To the extent that central bank independence limits opportunistic behavior, monetary policy induced credit cycles around elections should not be observed once a central bank is politically independent (Clark et al., 1998; Rey, 2015). Yet, there is evidence that suggests otherwise.⁷ For this reason, it is important to explore other policy channels that might explain why credit increases before elections.

Besides direct monetary interventions, governments can encourage or require state-owned banks to increase lending or allocate more credit to targeted recipients. Often this would entail the provision that banks set aside a certain percentage of their loans to agriculture and small-scale industries (Cole, 2009). This idea has considerable empirical support. Dinç (2005) finds cross-country evidence that state-owned banks increase their lending in election years relative to private banks. Ahead of the Brazilian presidential elections in 2014, state-owned Caixa Economica Federal expanded lending by 42% annually from 2010-2013, amid a 3.4% average annual GDP growth during the same period.⁸ In addition, Englmaier and Stowasser (2013) show that German savings banks — which are government controlled on the local level — increase their lending by up to 8% prior to local elections. There are several additional country cases that support this pattern (Sapienza, 2004).

Additionally, in line with the prediction of the political budget cycle theory, governments can borrow more themselves before elections (especially from state-owned banks). This type of fiscal electioneering is politically attractive as not only is it less costly than issuing bonds, these funds can be allocated to targeted constituencies. More importantly, in many instances these credit activities are not accounted for in budgets and can give voters the impression that the economic stimulus came at no fiscal cost (Hauner, 2009). In Brazil, government funding for BNDES — the

⁷For example, officials of the Bank of Thailand could only mutter objections, but were unable to prevent Prime Minister Thaksin's overly generous policies of providing low cost loans in Thailand.

⁸The Economist October 19, 2013.

state development bank that provides credit subsidies for small and large firms – is not considered an expense in the national account.⁹ As the BNDES accounts for 70% of long-term lending in Brazil, these subsidies constitute a major portion of hidden deficits (Colby, 2013).¹⁰

Similarly, regulatory interventions such as modifications of implicit or explicit bailout guarantees impact risk taking behavior and aggregate credit market outcomes (e.g., Burnside, Eichenbaum and Rebelo (2001)). Throughout the 1990s, the Japanese government propped up a large numbers of insolvent companies by supporting the ‘ever-greening’ of non-performing loans. Guaranteeing deposits of banks that continued to increase lending to these virtually defunct corporations, this type of ‘subsidized credit’ formed the basis for emergence of so-called ‘zombie loans.’ Although there is no direct evidence that this policy was done specifically for electoral purposes, Caballero, Hoshi and Kashyap (2008) report that the percentage of Japanese firms receiving subsidized loans grew from 5% in 1989 to 15% in 1993, which was an election year. This pattern can also be observed across several Eastern European countries where governments strategically relaxed regulatory standards to boost credit markets. For example, running for re-election the Hungarian government in 2002 not only lifted remaining capital controls but also significantly relaxed down-payment requirements for mortgages leading to a surge in private credit before the elections (Dobricza, 2004). Since these financial market regulations do not exert an immediate effect on a country’s budget balance, these policy instruments are particularly appealing.

Finally, credit can be stimulated *via* direct fiscal interventions,¹¹ such as credit subsidies for housing and agricultural production (IMF, 2009). While Calomiris and Haber (2014) discuss these policies primarily in the context of the U.S. financial crisis in 2007, they are not exclusive to the U.S. and have widely been implemented (Fernandez-Villaverde, Garicano and Santos, 2013). Hungary is a case in point. Besides removing regulatory barriers for enhanced mortgage finance, the government facing re-election widened its mortgage subsidy scheme. According to this scheme, a household could obtain a mortgage at an effective real interest rate of -4%. Not all too surprisingly,

⁹The Economist Magazine, October 19, 2013.

¹⁰As voters become more informed, it has been argued that politicians favor the use of indirect and opaque instruments which are not immediately observable to voters (Shi and Svensson, 2006).

¹¹For instance, Adams, Einav and Levin (2009) present evidence on the effectiveness of the Tax Reform Act in 2001/2003 in the U.S. driving short-term credit demand.

credit growth reached 54% in the run-up to the election (Vadas, 2009).

All these instruments positively affect credit market dynamics. Whereas direct interventions enhance the supply of credit, tax incentives affect borrowing conditions through reducing effective interest rates. We can expect these effects to be stronger when state-owned banks dominate the domestic financial system as governments have more power to direct credit outcomes, such as through increasing borrowing of government entities and to allocate these funds to targeted recipients.

We synthesize our theoretical predictions in Hypothesis I:

Hypothesis I: Credit to the private and government sector increases during election years compared to non-election years, i.e. political credit cycles are a reality. This effect is more pronounced when a large share of banks is government-owned.

Our predictions implicitly rely on the assumption that sufficient funds are available to engineer political credit cycles. In keeping with our three pillars of analysis, we analyze how international investors shape these opportunities.

The Opportunity: The Role of Financial Openness

Financial openness means greater access to capital, but it also means less freedom to pursue expansionary policies at home. Either by demanding higher lending rates (threatening to exit) or by actually withdrawing their assets, international investors exert their power by punishing governments who pursue unsound policies such as fiscal profligacy or populist monetary policies.¹² The influence of international lenders is likely strongest in times of uncertainty such as elections or financial crises, which ironically are times when steady capital flows would be most needed (Mosley, 2003; Kaplan, 2013; Bernhard, Broz and Clark, 2002). Several authors also argue that capital account openness is the result of a long-standing process to reform nations' domestic financial sector (e.g., Freeman and Quinn (2012)), a process which has led to a weakening of the state's power in lending relationships. As such, governments' ability to direct credit to voters may be curtailed once an economy is open to international capital flows.

¹²For a comprehensive review on how international financial markets exert discipline, see e.g., Mosley (2003), Ahlquist (2006), and Kaplan (2013).

Given these constraints, governments might find it increasingly difficult to pursue expansionary fiscal and monetary policies for electoral purposes.¹³ To the extent that traditional macroeconomic instruments are used to drive credit expansions, we should see fewer occurrences of political credit cycles in financially open economies. However, this effect will critically depend on how much these constraints affect governments' room for maneuvering. Open-economy macro models suggest a mixture of different theoretical predictions depending on the exchange rate regime (Mussa, 1979; Oatley, 1999; Clark and Hallerberg, 2000). While a very high degree of capital mobility limits monetary policy efficacy under fixed exchange rates (Alesina and Rodrik, 1994; Oatley, 1999), the degree of capital mobility limits fiscal policy discretion under flexible exchange rates (Clark and Hallerberg, 2000).

Building on these reinforcing logics, we formulate the following hypothesis.

Hypothesis II: Political credit cycles are mediated by the degree of financial openness. The higher the degree of capital openness, the less likely political credit cycles in private and government credit emerge.

We complement our hypothesis by formulating additional scope conditions. Foreign investors' ability to 'threaten' policy makers may vary across income levels. Sobel (2002) and Mosley (2003) find the threat of exit by mobile asset holders is more credible in developing countries, where sovereign default risk is higher. Compared to developed economies, domestic credit in developing countries is also more sensitive to international capital inflows and should constrain policy makers' ability to impact lending dynamics (Balding, 2011; Kaplan, 2013; Campello, 2013). Accordingly, we expect to see the mediating effects of financial openness to be stronger in developing compared to developed economies. Moreover, while financial openness constrains monetary policy (under fixed rates) or fiscal policy (under floating rates), the literature is silent on how financial openness affects the ability of governments to deploy credit policies such as subsidized credit. To trace possible patterns, we run several robustness tests to flesh out these mechanisms. We discuss the details in section 4.

¹³Other scholars have found that the size of international economic constraints are small (Kitschelt, 1999; Wei and Tytell, 2004). This weakens the hypothesis that openness limits the ability to generate political credit cycles .

3 Empirical Analysis

We structure our empirical analysis in the following way. First, we explain the construction of main dependent and independent variables. Second, we discuss our baseline model. Third, building on the idea that financial openness critically mediates the electoral effect on credit market outcomes, we include an interaction term between elections and financial openness. Fourth, we conduct a series of additional empirical tests to substantiate the mechanisms behind credit market dynamics around elections. In particular, we test whether political credit cycles are driven by factors other than monetary and fiscal policy. Additional robustness checks are included in a supplementary Appendix.

3.1 Data and Empirical Model

3.1.1 Dependent Variable

We focus on aggregate credit as our main dependent variable. We differentiate credit according to the nature of institutions granting credit (banks vs. non-banks) and by the type of borrower (private vs. government). To capture private credit, we use line(s) 22d (lending by banking financial institutions) and combine it (where available) with line 42d (lending by non-bank financial institutions). We repeat the same procedure for cases where the government is borrowing (IFS lines 22a and 42a, respectively). Combining all dimensions of private and public credit, we attain aggregate credit. The data covers the time span between 1960 and 2013 and comes from the IFS of the IMF.¹⁴

To account for discretionary shifts in credit and minimize potential bias arising from cyclical upswings in credit, we make several adjustments. First, taking real credit instead of nominal credit, we account for inflation fluctuations that might bias nominal values of credit (Frankel and Rose, 1996). Additionally, we do not use a credit to GDP measure to ensure that increases in credit are not context driven. It might be the case that GDP suddenly drops in one period whereby lending remains relatively stable at the same time, biasing our results (Elekdag and Wu, 2011).

¹⁴For a detailed description of the construction of all variables and retrieving sources, see, Appendix Table A1.

Furthermore, an isolated analysis of the credit to GDP ratio might ‘overlook’ small, but decisive discretionary shifts.¹⁵

Second, to minimize a potential bias arising from cyclical credit movements, we estimate a ‘credit gap,’ which represents the gap between actual credit values and its long run trend. To be precise, our dependent variable is a measure of how many standard deviations the log of real credit deviates from a country-specific trend. This long-run trend is estimated using a Hodrick-Prescott filter with the smoothing parameter set equal to 100. Concentrating on deviations from a long run trend, we isolate discretionary lending hikes during election years and thus ensure that credit dynamics are not associated with the process of financial deepening. This method is more precise in capturing even small, but systemic shifts in lending patterns.

3.1.2 Independent Variable

Our central independent variable is the timing of elections. We focus on executive elections and use the Political Institutions and Political Events (PIPE) data set compiled by Przeworski et al. (2013) that includes a variable capturing the “*number of elections in which the office of the chief executive was at stake during a year*” (Przeworski, 2013).¹⁶

To account for a potential bias arising from endogenous election timing, we exclude all unscheduled elections (Kaplan, 2013). In many countries incumbent governments can call in early elections during a credit boom, i.e., ‘surf on a credit boom’ (Kayser, 2005). This would positively bias our results. Calling for early elections in our context can be also due to a ‘credit bust’ that leads to popular protest and forces an incumbent government to step down. In addition, unscheduled elections represent greater political uncertainty and governments have less time to change economic policies to affect credit market outcomes. We show that this is the case. When considering unscheduled elections, the coefficient on the election variable is negative across all dimensions of credit.¹⁷

¹⁵Comparing our results to estimations based on the log of real credit and on more traditional credit-to-GDP measures, with and without considering deviations from a country specific trend our results remain similar (Table A6, A7, A8).

¹⁶We also compare our results with elections from the Database on Political Institutions (Beck, Demirguc-Kunt and Levine, 2001). These are similar to the baseline that include scheduled and non-scheduled elections (Table A9, A10).

¹⁷See Table A11.

3.1.3 Additional Control Variables

We avoid including a very large number of control variables as these might inflate the effects of political and institutional variables (Leblang and Satyanath, 2006). Given that investors commonly demand higher interest rates during elections, we exclude real interest rates, as these might conflate the effects of election on credit aggregates. Even including the real interest rate, our results remain qualitatively very similar.¹⁸

To capture the effect of financial and capital account openness, we include the *Kaopen index* developed by Chinn and Ito (2008).¹⁹ Our second hypothesis tests the role of financial account openness in limiting the ability of governments to engineer political credit cycles. To account for this effect, in an extended model, we include an interaction term between *Elections* and the *Kaopen index*.

We also include the log of real GDP per capita and the real GDP growth rate as control variables. The data come from the IMF and the World Development Indicators of the World Bank (IFS, 2013; WDI, 2015). Controlling for GDP growth is essential, as upswings in private credit can be due to cyclical (up)swings and/or might be induced by productivity shocks (Mendoza and Terrones, 2008). Additionally, banks and financial intermediaries might loosen credit during economic upswings; i.e. pro-cyclical lending standards (Dell’Ariccia and Marquez, 2006).

We also include two additional political control variables. It is well-documented that young democracies are prone to experience political budget cycles (e.g., Brender and Drazen (2008)). For this reason, we control for the age of the current political regime. The data come from the Polity IV project (Marshall, Jaggers and Gurr, 2015). Moreover, substantial literature indicates that democracies might have an expansionary bias (Steinberg, Koesel and Thompson, 2015). To account for this effect, we also include a measure of democracy that takes the value of 1 if a country is a democracy and 0 otherwise (Cheibub, Gandhi and Vreeland, 2010). We report the descriptive statistics in Table 1.

¹⁸See Table A17.

¹⁹For robustness, we cross-check our results with the *ckaopen* from Karcher and Steinberg (2013), see Table A18. The results remain identical.

3.1.4 Empirical Model

We follow the literature on political business and budget cycles in formulating our empirical model (e.g., Brender and Drazen (2008); Shi and Svensson (2006)). The model can be written such that:

$$C_{i,t} = \alpha + \beta_1 C_{i,t-1} + \beta_2 * Election_{i,t} + \sum \gamma' X_{i,t} + \mu_i + \theta_t + \epsilon_{i,t} \quad (1)$$

whereby $C_{i,t}$ is our credit variable for country i at time t , $C_{i,t-1}$ is the first lag of our dependent variable and β_i is the coefficient on the independent variables. We include the first lag of the dependent variable to control for potential persistence effects in underlying credit market dynamics. The coefficient on additional control variables $X_{i,t}$ is denoted γ' . The variable $Election_{i,t}$ captures the date of an election in country i at time t , in which the office of the executive was at stake. We also incorporate time fixed effects that we denote θ_t to control for time specific effects and to account for changes in global liquidity conditions (Rey, 2015). Additionally, we include country fixed effects to eliminate unobserved country specific effects to capture time invariant country specific characteristics that might affect the relationship between elections and credit market outcomes. One of the limitations of using a fixed effects regression with the lagged dependent variable introduces a dynamic panel bias, leading to a downward correction of coefficients (Nickell, 1981). Therefore, we follow the recent literature on political budget cycles that proposes the use of Generalized Method of Moments (GMM) estimation techniques (Kaplan, 2013). Although this way of proceeding has primarily been applied in the context of estimating general equilibrium effects in small T (i.e. short time horizons) and large N (i.e. large set of units) panels (Roodman, 2009), we rely on a one step system GMM estimation method for several reasons.

First, our dependent variable(s) is highly persistent or correlated over time. In our case, the correlation coefficient between our central dependent variable(s) and its lagged value range between 0.4 and 0.7. These are also statistically significant at conventional levels, which makes a dynamic panel estimation method a reasonable choice. In fact, a system GMM approach complements first difference instruments by including level equations and thus allows us to control for this effect.

Second, our political and institutional independent variables do not vary substantially over

time so that alternative dynamic panel estimations would lead to inferior outcomes. In selecting the optimal lag length, we restrict the maximum lag for selecting instruments to three lags, as in the unrestricted case, the estimator would allow the inclusion of an unlimited number of instruments. This in turn would excessively weaken tests for instrument endogeneity and thus the robustness of our results (Roodman, 2009).

3.2 Results

We report the empirical test results for Hypothesis I in Table 2, which we divide into three identically structured sections; section 1 (columns 1-3) captures aggregate credit, section 2 (columns 4-6) includes private credit and section 3 (columns 7-9) includes the results for government borrowing. The first column of each section captures aggregate credit, in the second and third columns we report the results for bank and non-bank credit.

Insert Table 2

We detect positive deviations of aggregate, private and public credit from their historic trend around elections. The size of the effect lies between 0.08 and 0.09 standard deviations for private and public credit. To put this finding into context; to be considered a credit boom, a typical threshold lies one standard deviation above the trend. This value typically represents the 90th percentile of the statistical distribution. Our results, however, are clearly located above the median value and show a systemic deviation of credit from its long run trend. These deviations are statistically significant in close to all model specifications.²⁰

As can be seen from columns 2 and 3 of each section in Table 2, a distinction between the effect of elections on bank versus non-bank credit emerges. Political credit cycles in both private and public borrowing seem to be a myth when it comes to non-bank financial institutions. A potential caveat concerning this result is that non-bank financial institutions do not constitute a large share of aggregate credit in many countries. Additionally, Eurozone, Post-Soviet and several other countries do not provide this information, which leads to a substantial drop in observations. Therefore, we are cautious about the robustness of this finding.

²⁰We also test for different model specifications (Tables A3, A4, A5). The results remain identical.

In line with Hypothesis I, when governments own more than 50% of domestic bank assets, the effect of elections on public credit is significantly stronger than at baseline, while the election coefficient is no longer significant for private credit (see Table 3). Compared to the baseline results in Table 2, the election coefficient for public banks increases from 0.09 to 0.31 standard deviations, while non-bank public credit shows an increase from around 0 to 0.38 standard deviations. This may be attributed to developing countries with notable state-involvement in finance. In these economies, non-bank financial institutions often take the form of government credit-insurance companies or state-owned investment management firms that are subject to government discretion. For example, in Malaysia non-bank financial institutions fund a substantial share of the government's development activities (Islam and Osman, 2011). We show that this effect is particularly strong in middle income countries and financially less developed economies, which is due to a lack of bond market development, a more dominant market position of public banks and/or closer government-bank relationships.²¹ As expected, at higher levels of financial market development, political credit cycles in public credit disappear while private credit markets gain importance.²²

Another explanation for the positive and significant coefficient of public, but not private credit when the government owns the majority of the banking system is that we might be picking up a more standard political budget cycle. In this case, our findings underscore the possibility that electoral fiscal cycles are funded through credit from government-controlled banks and directly channeled to targeted constituencies. The recent impeachment of Dilma Rousseff hints to such a pattern in Brazil during the election in 2014. The Brazilian government's lending position of all major state-owned banks in Brazil rose from -0.1 % in 2009 to about -0.9 % to GDP in July of 2014 (Folha de Sao Paulo April 6, 2016). This increase in the deficit is due to delayed payments for social programs for the poor that are transferred *via* these state-owned financial institutions. The Rousseff administration used this strategy systematically to hide the true nature of the federal budget deficit and to increase spending in an election year. Put differently, governments may strategically increase borrowing from state-owned banks to hide the true nature of the deficit during an election year. We leave an in-depth analysis of this mechanism to future research.

²¹See Table A31, A36.

²²See Table A37, A38.

Referring to single country and regional case studies (e.g., Shi and Svensson (2006)), we further disaggregate our results. Besides a regional breakdown, we stratify our results across different time windows. We find that electoral credit cycles in private credit are stronger in Latin America, Asia, and Africa, whereby government-borrowing cycles are more likely to occur in OECD and in transition countries, which is driven by the prominence of government owned banks in these countries.²³ These regional variations might be due to differences in electoral institutions. We show that political credit cycles are more likely to occur in presidential compared to parliamentary systems.²⁴

Finally, we also detect a partisanship effect. It is left wing governments that foster credit growth – in particular public credit – around elections.²⁵ Also, the sign of the coefficient on our democracy variable is not significant in any single specification. Given the vast literature arguing for an expansionary policy bias in democracies (e.g., Broz (2013)), this finding is surprising. We leave an in-depth analysis of these institutional effects to future research. In short, we find robust evidence of a positive and significant relationship between elections and credit. Our findings suggest that political credit cycles are a reality, but at the same time highly context-specific.

3.3 Political Credit Cycles and Financial Openness

To test whether financial account openness limits the ability to engineer political credit cycles, we include an interaction term between *Elections* and the *Kaopen index*. We report our results in Table 4 and 5. All else equal, we expect the marginal effects of election on credit to decline with the degree of financial openness.

Insert Table 4

Financial openness significantly conditions the effect of elections on aggregate and private credit from the banking sector. Although the interaction between *Elections* and the *Kaopen index* is

²³See Table A20, A21, A22, A23.

²⁴This is consistent with results from political budget cycles such as Persson and Tabellini (2005). Compared to parliamentary systems, executives in presidential regimes have a higher incentive for electoral manipulation because in such as system voters tend to attach success or blame to individuals (such as the president) rather than impose collective accountability.

²⁵See Table A15, A14, A16.

negative and significant, this finding does not provide meaningful support for our hypothesis, unless we report marginal effects (Brambor, Clark and Golder, 2006). We calculate and graphically illustrate these in Table 5. As expected, the effect of *Elections* on aggregate and private credit decreases with higher levels of financial openness and is significant only for low values of the *Kaopen index*. This suggests that political credit cycles in private credit are largely absent in financially open economies.

Analyzing public credit, the interaction effects between elections and capital account openness are insignificant. The marginal effect plots in Table 5 suggest that capital account openness does not condition the effects of elections on public borrowing. However, the *Election* coefficient, which captures the effects of elections on public credit when capital account openness equals to zero, is positive and significant when credit is obtained from banks. The results from this conditional model support our first hypothesis. We note that elections are not significantly related to any type of borrowing, private or public, when the source of credit are non-depository corporations.

As expected, the mediating effect of capital account openness is most pronounced for middle- and low-income economies when considering private credit,²⁶ while this mediating effect for high-income economies is muted. A potential reason for this effect to occur is because developing countries face much more scrutiny from international investors due to their sovereign default risk, while developed economies have more ‘room to move’ (Mosley, 2003) . Similarly, analyzing public borrowing across different income groups, it appears that political credit cycles are most pronounced in middle- and low-income economies, whereby the mediating effect of financial openness is muted in low-income and high-income economies. These findings are in line with additional results indicating that the level of financial market development critically shapes the magnitude of the mediating role of capital account openness.²⁷

In sum, our findings indicate that capital account openness dampens political credit cycles across all dimensions of credit. The strength of this effect is critically conditioned on the level of economic development. To isolate competing mechanisms, we conduct several empirical tests.

²⁶See Tables A33, A34, A35.

²⁷See Table A39, A40, A41.

3.4 Mechanisms and Discussion

This section analyzes mechanisms through which financial openness limits the ability to engineer electoral credit cycles. We examine various institutional and policy constraints placed on monetary and fiscal policy. Particularly, we analyze the effects of central bank independence, fiscal policy constraints and exchange rate arrangements in Table 7. All of the models in Table 7 include an interaction term between *Election* and the *Kaopen index*. These tests also serve as robustness tests to examine whether the effect of election on credit still holds when these constraints are present. Adding to the list robustness tests, we analyze co-movements of related macro-financial variables. We report the results in Table 8. Additional robustness checks are reported in the Appendix.

First, without replicating a substantial empirical literature, we show that government ownership, directed lending, credit controls, entry barriers and overall financial repression are negatively associated with the degree of capital account openness (See Table 6).²⁸ As documented, financial liberalization has reduced the prominence of government interference in financial markets.

Second, we restrict our sample to countries with a high degree of central bank independence.²⁹ The coefficient on our *Election* variable is positive and significant for both public and private credit (Table 7, column 3). This suggests that central bank independence is not sufficient to limit the frequency of electoral cycles in credit. In fact, across several specifications the size of the coefficient is larger when compared to the results in our baseline specification (see Table 8, column 1), suggesting that political credit cycles are more pronounced in financially open economies with independent central banks. At the same time, these countries show no evidence of a traditional electoral cycle in monetary policy, as elections seem to have no significant effect on the M2 to GDP (Table 8, column 3). One possible explanation is that investors might feel reassured that a more independent central bank will prevent electorally motivated monetary manipulation (Sattler, 2013). Our findings suggest that policymakers may have used credit market policies to substitute for the loss monetary policy (Acemoglu et al., 2008).

Third, we restrict our sample to countries in which fiscal policy is constrained (Table 7, column

²⁸In order to determine the degree of financial market repression we rely on the index variables proposed in (Abiad, Detragiache and Tressel, 2008).

²⁹We use the Central Bank Independence (CBI) Index from (Bodea and Hicks, 2015) and follow their classification method.

4).³⁰ When financially open economies operate under fiscal policy constraints, elections have a positive and significant effect on government borrowing from domestic banks. This suggests that when faced with regulations that limit discretionary budgetary decisions, governments find ways to get around these by directly borrowing from the domestic banking system. Interestingly, when both fiscal and monetary policy are out of governments' reach (Table 7, column 7), we cannot detect electoral cycles in either fiscal or monetary policy (Table 8, column 7). Instead, signs of political credit cycles in both public and private borrowing become visible (Table 7, column 7). In fact, the size of the coefficient is larger for public borrowing. These findings suggest that higher credit during elections can be attributed to policies other than populist monetary or fiscal policy interventions. If monetary and fiscal policies were the central driving force behind political credit cycles, we would expect the opposite effect.

Fourth, we restrict our samples to countries with fixed exchange rates.³¹ The Mundell-Fleming macroeconomic trilemma suggests that monetary policy is less effective if the exchange rate is fixed. Similarly, monetary policy under fixed rates would be less effective in pushing credit. This constraint is even more enhanced under the presence of independent central banks (Clark, 2002). Our work supports this proposition: countries with fixed exchange rates and central bank independence show no significant relationship between election and monetary variables (Table 8, column 5). On the contrary, we do find some evidence for the existence of traditional political budget cycles, as fiscal deficits worsen, public expenditures slightly increase and revenues fall (Table 8, column 7). This finding is hardly surprising given the well established fact that fixed exchange rates do not place technical constraints on fiscal policy.

Finally, we focus on countries that operate under all three constraints: operating an independent central bank, fiscal policy is constrained and a fixed exchange rate regime is in place. If all three constraints are put in place – a “hard” constraint on policymakers emerges – the question arises whether we can still observe political credit cycles? As expected, none of the coefficients on our

³⁰To measure fiscal policy constraints, we create a dummy variable, which takes the value 1 if the executive cannot change domestic taxes without legislative approval and has to secure legislative approval for the budget. The data come from the Institutions and Elections Project (Regan and Clark, 2015). We then interact the dummy variable with Henisz Political Constraints Index (Polcon3), whereby higher values indicate more constrained fiscal policy making.

³¹In order to isolate whether a country pursues a fixed exchange rate, we follow Kokenyne et al. (2009). For a detailed description of the construction of our variable, see the Appendix.

Election variable is significant, except for public non-bank credit and private non-bank credit. The signs on these coefficients are negative. During elections, hard-constrained economies experience a fall in non-bank borrowing by the public and the private sector. Our interpretation is that election and credit market dynamics reverse once governments face a hard policy constraint (Table 7, column 8). Meanwhile, there is no evidence of an electoral monetary cycle, and fiscal deficits narrow slightly, but government consumption significantly increases (Table 8, column 8).

Interestingly, across all subsamples we can detect a depletion of foreign reserves and a reduction in foreign asset positions (Table 8).³² This loss of reserves may be due to a contraction of foreign monetary base to counter speculative exchange rate pressures and mask domestic monetary expansions (Dreher and Vaubel, 2009). Given that private and public credit increase along with household consumption, our findings suggest that private credit expansions fuel household consumption. We cannot detect a similar effect concerning private investment, which is in line with classic political business cycle theories.

Intuitively, our findings indicate that in financially open economies, governments have less instruments at their disposal, and thus opportunities to engineer political credit cycles are more limited. This effect does not appear to arise due to institutional constraints on macroeconomic policymaking, but can be attributed to two reinforcing mechanisms. First, enhanced international financial exposure leads to a reduction in government influence over the domestic banking system. Second, institutional constraints in combination with an orientation towards an external policy goal – a fixed exchange rate arrangement that *de facto* constrains policy makers – eliminate several options for governments to manipulate credit market dynamics effectively.

4 Conclusion

While political business cycles have been extensively studied, recent contributions test whether similar electoral patterns exist concerning financial market outcomes such as bank credit and stock prices (Dinç, 2005; Cole, 2009; Santiso, 2013; Kaplan, 2013). We expand on these single country

³²We show that the depletion in foreign assets is driven by a drop in foreign portfolio assets, which corresponds to a loss in foreign reserves; see Table A42.

case studies and regional analysis and test whether political credit cycles also exist on a large N-scale. Analyzing 165 countries in the time span between 1960 and 2013, we show that political credit cycles exist across several dimensions of credit (bank vs. non-bank; private and government credit). Although this by itself is not direct evidence of credit manipulation, it suggests the possibility that politicians use not only fiscal and monetary policy to court voters but also credit policies such as subsidies and tax breaks for debt manipulation. That the relationship between election and credit holds even after controlling for monetary and fiscal policy constraints gives us some degree of confidence in our hypothesis. To show direct evidence of credit manipulation, we would need to conduct a test using the government's credit policy as well as financial regulatory instruments, which would be a next step on our research agenda.

Assuming that governments have a clear motive for engineering electoral credit booms, we show that this electoral pattern is mediated by the degree of financial openness. Our empirical findings indicate that these political credit cycles are far smaller and thus harder to detect than political budget cycles. The likely reason is that an analysis of the credit to GDP ratio 'overlooks' small, but decisive discretionary shifts of credit aggregates from their long-term trend paths, so that these marginal shifts remain unnoticed. Therefore, when analyzing political credit cycles through a credit to GDP lens, they appear to be largely absent. We find that the magnitude of political credit cycles seems to critically depend on the level of financial market development. For political credit cycles in private credit to occur a certain level of financial market development is necessary, whereby at lower levels of financial market development governments seem to rob their own banking system. From a policy perspective, these results underscore the critical role of financial market development in shifting political credit cycles from the public into the private sector. The absence of an expansionary economic policy bias in democracies is intriguing and represents an interesting avenue for future research on the role of political institutions shaping credit market outcomes.

The ability of tapping into international financial markets seems to become the critical factor in determining the room for maneuvering of governments when engineering political credit cycles. In fact, the magnitude of political credit cycles is significantly dampened when the economy is more open to international capital inflows and this effect is stronger in developing countries compared to

advanced economies. That is to say, governments have more freedom to pursue arbitrating credit market policies that go unpunished when capital controls are in place; reinforcing the prominence of targeted and directed lending in financially closed economies. Intuitively speaking, governments have less instruments at their disposal in financially open economies. This reduced degree of government influence over credit market outcomes translates into a reduced ability to engineer political credit cycles. In light of recent policy paradigm shift towards a reduction in the free mobility of capital and more government control over financial markets, our results show that these policy shifts might give policy makers more room to engineer political credit cycles.

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Tables and Figures

Table 1: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Aggregate Credit, deviation from Trend	0	0.986	-5.822	5.369	6238
Aggregate Private Credit, deviation from Trend	0	0.986	-5.785	5.384	6324
Aggregate Public Credit, deviation from Trend	0	0.986	-6.774	4.322	6216
Aggregate Bank Credit, deviation from Trend	0	0.987	-5.141	4.653	6641
Private Bank Credit, deviation from Trend	0	0.987	-5.429	3.725	6712
Public Bank Credit, deviation from Trend	0	0.987	-6.774	4.864	6505
Aggregate Non Bank Credit, deviation from Trend	0	0.985	-5.428	4.652	1406
Private Non Bank Credit, deviation from Trend	0	0.986	-4.927	4.652	1429
Public Non Bank Credit, deviation from Trend	0	0.984	-5.41	4.841	1254
Election	0.107	0.309	0	1	7266
Election X Kaopen	0.018	0.522	-1.864	2.439	5271
Kaopen	0.035	1.523	-1.864	2.439	5745
GDP per capita	11.126	2.375	5.436	22.702	5269
GDP growth	0.04	0.167	-5.07	7.058	5819
Regime Durability	22.398	28.673	0	201	6677
Democracy	0.451	0.498	0	1	7030
Real lending rate	6.382	9.791	-48.574	49.994	4004
Total assets, %GDP	99.325	541.966	0	12081.29	5976
Total liabilities, %GDP	134.849	531.505	0.988	11993.643	5979
Net foreign asset position, %GDP	-35.599	144.139	-3297.01	1720.696	5972
Public Deficit, % GDP	-1.607	7.136	-202.697	40.416	1842
Government Expenditure, %GDP	26.027	11.908	0.179	209.854	1699
Government Revenue, %GDP	25.024	10.838	0.241	77.022	1796
Household consumption, % GDP	66.507	16.254	-69.815	190.563	6569
Investment, % GDP	21.814	8.159	-2.424	154.796	6320
Real Exchange Rate	1.872	1.159	0	32.26	7357
Money and quasi money (M2) as % of GDP	49.854	45.581	0.831	669.88	5564
Foreign Reserves, % GDP	12.373	14.997	-0.164	156.536	6046
Fiscal Constraints Index	0.23	0.219	0	0.72	3839
Central Bank Independence Index	0.529	0.229	0.1	0.96	2287
Exchange Rate Regime	0.619	0.486	0	1	6237
Government Ownership	1.267	1.191	0	3	2524

Table 2: Estimation Results - Political Credit Cycles

	Aggregate Credit			Private Credit			Public Credit		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Aggregate	Bank	Non-Bank	Aggregate	Bank	Non-Bank	Aggregate	Bank	Non-Bank
Election	0.09** (0.04)	0.11*** (0.04)	0.01 (0.06)	0.08* (0.04)	0.09** (0.04)	0.00 (0.06)	0.09** (0.04)	0.11** (0.04)	-0.00 (0.07)
Kaopen	0.02** (0.01)	0.01* (0.01)	0.02 (0.01)	0.01* (0.01)	0.01* (0.01)	0.02* (0.01)	-0.00 (0.01)	-0.01 (0.01)	0.02 (0.03)
GDP per capita	0.01*** (0.00)	0.01*** (0.00)	0.00 (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01** (0.00)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)
GDP growth	0.18 (0.18)	0.23 (0.20)	0.51 (0.35)	0.20 (0.18)	0.27 (0.22)	-0.40 (0.53)	0.02 (0.07)	0.01 (0.07)	0.09 (0.14)
Regime Durability	0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Democracy	-0.02 (0.02)	-0.01 (0.02)	0.01 (0.04)	-0.02 (0.02)	-0.01 (0.02)	-0.02 (0.04)	-0.01 (0.02)	-0.00 (0.02)	-0.06 (0.04)
First Lag	0.65*** (0.03)	0.68*** (0.02)	0.57*** (0.06)	0.69*** (0.03)	0.72*** (0.02)	0.57*** (0.06)	0.51*** (0.03)	0.52*** (0.03)	0.45*** (0.05)
Constant	-0.16* (0.08)	0.24** (0.12)	-0.40** (0.20)	-0.01 (0.09)	-0.10 (0.08)	-0.11 (0.12)	-0.11 (0.10)	-0.10 (0.10)	0.36 (0.30)
Observations	3850	4057	1053	3852	4054	1037	3843	3976	886
Sargan χ^2	590.36	543.53	485.52	614.97	571.69	466.03	535.62	531.58	397.28
Pr > χ^2	0	0	0	0	0	0	0	0	0
AR(1)	0	0	0	0	0	0	0	0	0
AR(2)	0	0	1	0	0	0	0	0	0
# Countries	140	140	40	140	140	39	139	138	36

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

GMM instruments are restricted to three lags. Endogenous variables are the first lag of the dependent variable and GDP Growth. AR (1) and AR (2) are the p-values of the test statistics for first and second order serial correlation in first differenced residuals.

Table 3: Estimation Results - Political Credit Cycles, Government Ownership > 50%

	Aggregate Credit			Private Credit			Public Credit		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Aggregate	Bank	Non-Bank	Aggregate	Bank	Non-Bank	Aggregate	Bank	Non-Bank
Election	0.28** (0.11)	0.28** (0.11)	0.51* (0.29)	0.16 (0.10)	0.13 (0.10)	0.20 (0.17)	0.33*** (0.10)	0.31*** (0.09)	0.38** (0.18)
Kaopen	0.04 (0.03)	0.03 (0.03)	0.10 (0.08)	0.03 (0.03)	0.04 (0.03)	0.09* (0.05)	0.01 (0.03)	0.01 (0.03)	-0.02 (0.08)
GDP per capita	0.01 (0.01)	0.01 (0.01)	-0.04 (0.03)	0.01 (0.01)	0.00 (0.01)	-0.01 (0.04)	-0.00 (0.01)	0.00 (0.01)	-0.01 (0.04)
GDP growth	1.05*** (0.20)	1.00*** (0.20)	0.78*** (0.15)	1.22*** (0.22)	1.27*** (0.27)	0.05 (0.52)	-0.06 (0.16)	-0.13 (0.15)	0.04 (0.10)
Regime Durability	0.00 (0.00)	0.00 (0.00)	0.00 (0.01)	0.00* (0.00)	0.00 (0.00)	0.01 (0.00)	-0.00 (0.00)	-0.00 (0.00)	0.02*** (0.01)
Democracy	-0.10* (0.05)	-0.10** (0.05)	0.11 (0.33)	-0.16*** (0.05)	-0.12** (0.06)	-0.16 (0.31)	-0.02 (0.07)	-0.04 (0.07)	-0.12 (0.18)
First Lag	0.57*** (0.05)	0.57*** (0.05)	0.64*** (0.07)	0.63*** (0.03)	0.64*** (0.03)	0.61*** (0.06)	0.42*** (0.04)	0.46*** (0.04)	0.09 (0.09)
Constant	0.18 (0.16)	0.22 (0.15)	-1.21 (0.89)	0.10 (0.14)	0.16 (0.14)	-0.39 (0.55)	0.27 (0.22)	-0.02 (0.19)	-0.30 (0.45)
Observations	775	808	115	772	805	109	774	802	109
Sargan χ^2	354.90	356.40	136.88	338.35	320.94	134.56	365.61	378.70	109.70
Pr > χ^2	0	0	0	0	0	0	0	0	0
AR(1)	0	0	0	0	0	0	0	0	0
AR(2)	0	0	1	0	0	1	1	1	1
# Countries	52	55	12	52	55	11	53	55	11

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

GMM instruments are restricted to three lags. Endogenous variables are the first lag of the dependent variable and GDP Growth. AR (1) and AR (2) are the p-values of the test statistics for first and second order serial correlation in first differenced residuals.

Table 4: Estimation Results - Political Credit Cycles vs. Capital Account Openness

	Aggregate Credit			Private Credit			Public Credit		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Aggregate	Bank	Non-Bank	Aggregate	Bank	Non-Bank	Aggregate	Bank	Non-Bank
Election	0.24*** (0.07)	0.26*** (0.07)	0.09 (0.16)	0.21*** (0.07)	0.22*** (0.07)	0.15 (0.14)	0.19*** (0.07)	0.19*** (0.07)	-0.14 (0.12)
Election X Kaopen	-0.30** (0.12)	-0.29** (0.12)	-0.12 (0.25)	-0.26** (0.11)	-0.26** (0.12)	-0.22 (0.22)	-0.22* (0.13)	-0.17 (0.13)	0.23 (0.22)
Kaopen	0.02*** (0.01)	0.02** (0.01)	0.02 (0.02)	0.02** (0.01)	0.02** (0.01)	0.03* (0.01)	0.00 (0.01)	-0.00 (0.01)	0.02 (0.03)
GDP per capita	0.01*** (0.00)	0.01*** (0.00)	0.00 (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01* (0.00)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)
GDP growth	0.17 (0.18)	0.22 (0.20)	0.49 (0.35)	0.19 (0.18)	0.27 (0.21)	-0.42 (0.53)	0.02 (0.07)	0.01 (0.06)	0.09 (0.14)
Regime Durability	0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Democracy	-0.02 (0.02)	-0.01 (0.02)	0.01 (0.04)	-0.02 (0.02)	-0.01 (0.02)	-0.02 (0.04)	-0.01 (0.02)	-0.00 (0.02)	-0.06 (0.04)
First Lag	0.65*** (0.03)	0.69*** (0.02)	0.57*** (0.06)	0.69*** (0.03)	0.72*** (0.02)	0.56*** (0.06)	0.50*** (0.03)	0.52*** (0.03)	0.45*** (0.05)
Constant	-0.17** (0.09)	0.00 (0.10)	-0.30 (0.19)	-0.00 (0.10)	-0.10 (0.08)	-0.12 (0.12)	0.12 (0.12)	-0.10 (0.10)	-0.30 (0.19)
Observations	3840	4047	1043	3842	4044	1027	3833	3966	876
Sargan χ^2	585.24	540.16	487.56	611.19	568.23	471.42	534.92	527.33	390.16
Pr > χ^2	0	0	0	0	0	0	0	0	0
AR(1)	0	0	0	0	0	0	0	0	0
AR(1)	0	0	1	0	0	0	0	0	0
# Countries	140	140	40	140	140	39	139	138	36

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

GMM instruments are restricted to three lags. Endogenous variables are the first lag of the dependent variable and GDP Growth. AR (1) and AR (2) are the p-values of the test statistics for first and second order serial correlation in first differenced residuals.

Table 5: Estimation Results - Political Credit Cycles vs. Capital Account Openness

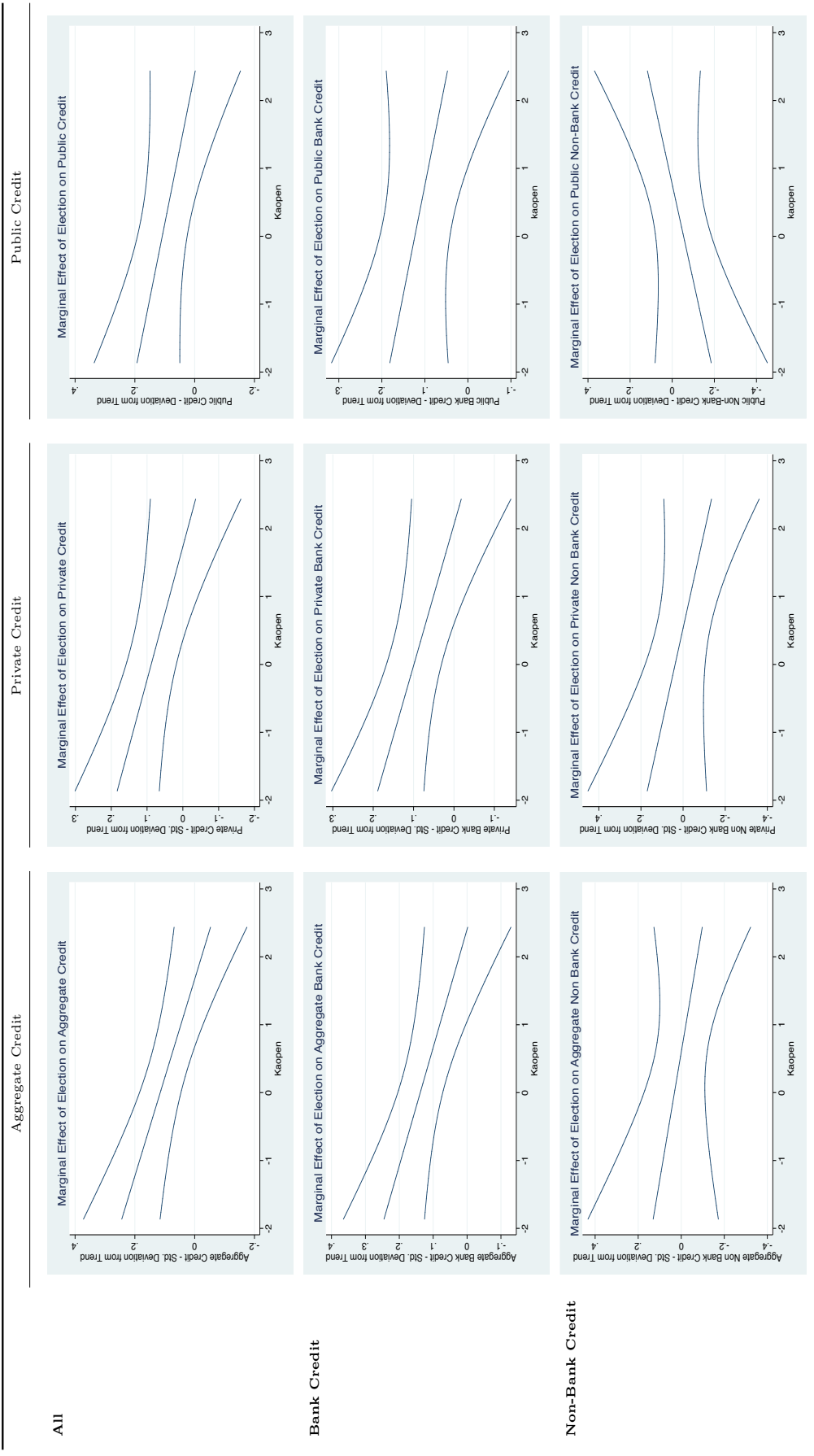


Table 6: Estimation Results - Financial Openness vs. Financial Repression

	(1)	(2)	(3)	(4)	(5)
	Privatization	Directed Credit	Credit Controls	Entry Barriers	Financial Reform
Kaopen	0.12** (0.05)	0.09* (0.05)	0.10* (0.05)	0.08 (0.05)	0.86*** (0.13)
GDP per capita	-0.31* (0.17)	-0.20** (0.08)	-0.20*** (0.07)	-0.12 (0.11)	-0.55** (0.22)
GDP growth	0.53** (0.22)	0.23* (0.12)	0.26* (0.13)	0.07 (0.13)	1.18** (0.54)
Regime Durability	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.01)	0.00 (0.01)	-0.00 (0.01)
Democracy	-0.01 (0.13)	-0.14 (0.13)	-0.09 (0.13)	-0.10 (0.13)	-0.43 (0.45)
Constant	4.39** (1.83)	2.97*** (0.90)	2.96*** (0.87)	2.15* (1.20)	10.84*** (2.53)
Observations	2230	2230	2230	2230	2230
R^2	0.37	0.57	0.60	0.61	0.85
$\hat{\sigma}$	0.56	0.60	0.58	0.55	1.86
Country FE	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓
# Countries	85	85	85	85	85

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The estimations are based on fixed effects regressions including time and year fixed effects. All right hand side variables are lagged by one year.

The positive coefficients on dependent variables indicate a lower level of financial repression.

Table 7: Estimation Results - Political Credit Cycles vs. Constraints

	All	FX	CBI	FC	CBI X FX	FC X FX	CBI X FC	CON
Aggregate Credit								
Aggregate Credit	0.11*** (0.04)	0.08 (0.05)	0.21*** (0.08)	0.12** (0.06)	0.08 (0.11)	0.07 (0.06)	0.26*** (0.09)	-0.08 (0.12)
Bank Credit	0.13*** (0.04)	0.10** (0.05)	0.14* (0.07)	0.15*** (0.05)	-0.00 (0.12)	0.10* (0.06)	0.21** (0.09)	-0.06 (0.13)
Non Bank Credit	0.02 (0.07)	0.00 (0.09)	0.26** (0.12)	-0.05 (0.09)	0.04 (0.27)	-0.05 (0.13)	0.36** (0.14)	-0.05 (0.65)
Private Credit								
Aggregate Private Credit	0.09** (0.04)	0.09** (0.05)	0.18*** (0.07)	0.07 (0.06)	0.09 (0.09)	0.13** (0.06)	0.21** (0.09)	-0.07 (0.13)
Private Bank Credit	0.11*** (0.04)	0.11** (0.05)	0.11* (0.06)	0.11* (0.06)	0.04 (0.10)	0.14*** (0.05)	0.16* (0.09)	-0.07 (0.13)
Private Non Bank Credit	0.03 (0.06)	0.00 (0.10)	0.28** (0.12)	-0.07 (0.10)	-0.19 (0.23)	-0.02 (0.13)	0.32* (0.17)	-1.39*** (0.26)
Public Credit								
Aggregate Public Credit	0.10** (0.04)	0.05 (0.06)	0.22*** (0.08)	0.10 (0.06)	0.05 (0.10)	-0.02 (0.06)	0.26*** (0.10)	0.09 (0.20)
Public Bank Credit	0.12*** (0.04)	0.08 (0.06)	0.20** (0.08)	0.12** (0.06)	0.09 (0.11)	0.03 (0.05)	0.29*** (0.09)	0.19 (0.21)
Public Non Bank Credit	-0.04 (0.06)	-0.16* (0.09)	0.07 (0.13)	-0.02 (0.09)	-0.07 (0.09)	-0.23** (0.09)	0.13 (0.19)	-0.57** (0.24)

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The coefficient is the coefficient on the Election variable. The interaction between Election and Kaopen is included in all models.

All models have been estimated using the GMM estimation technique. The title labels FX, CBI, and ER indicate different subsamples.

FX indicates a fixed exchange rate regime, CBI indicates an independent central bank, and FC represents fiscal constraints. X indicates interactions.

CON represents all cases, in which the central bank is independent, fiscal policy is constrained, and the country pursues a fixed exchange rate.

Table 8: Estimation Results - Mechanism(s)

	All	FX	CBI	FC	CBI X FX	FC X FX	CBI X FC	CON
Monetary Policy	Election	0.93** (0.39)	0.78 (0.55)	1.19* (0.66)	0.91** (0.44)	0.79 (0.57)	0.93 (0.67)	0.93 (0.95)
	Election X Kaopen	-0.03 (0.19)	-0.07 (0.23)	-0.26 (0.31)	-0.11 (0.25)	-0.10 (0.26)	-0.09 (0.34)	-0.12 (0.49)
	M2	0.02* (0.01)	0.02*** (0.01)	-0.00 (0.02)	0.01 (0.01)	0.02** (0.03)	0.00 (0.02)	-0.02 (0.02)
	Election X Kaopen	-0.01*** (0.00)	-0.02*** (0.01)	-0.01 (0.01)	-0.01** (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)
Foreign reserves	Election	-0.06*** (0.02)	-0.05*** (0.02)	-0.10*** (0.03)	-0.08*** (0.03)	-0.07*** (0.03)	-0.11*** (0.03)	-0.11*** (0.03)
	Election X Kaopen	-0.00 (0.01)	-0.01 (0.01)	0.02 (0.02)	0.02 (0.01)	-0.01 (0.01)	0.03 (0.02)	0.03* (0.02)
Fiscal Policy	Election	-0.61 (0.40)	-1.35 (1.00)	-0.99 (0.91)	-0.79 (0.62)	-2.62** (1.28)	-1.20 (1.68)	-7.22*** (2.50)
	Election X Kaopen	0.18 (0.20)	0.48 (0.44)	0.45 (0.42)	0.35 (0.32)	1.93* (1.10)	0.58 (0.79)	3.18*** (1.02)
	Election	0.16 (0.35)	0.86* (0.50)	-0.63 (0.62)	0.33 (0.40)	2.93** (1.34)	-0.72 (0.62)	5.49* (2.91)
	Election X Kaopen	0.12 (0.15)	-0.17 (0.21)	0.56** (0.27)	0.15 (0.18)	-0.98* (0.57)	0.62** (0.28)	-1.99 (1.23)
Fiscal Deficit	Election	-0.58 (0.41)	-1.43* (0.74)	-0.47 (0.41)	0.06 (0.26)	-0.22 (0.81)	0.18 (0.38)	0.67* (0.41)
	Election X Kaopen	0.13 (0.22)	0.68 (0.47)	-0.02 (0.20)	-0.18 (0.19)	-0.11 (0.36)	-0.31 (0.21)	-0.48*** (0.16)
International Capital Flows	Election	-0.02* (0.01)	-0.01 (0.01)	-0.03* (0.02)	-0.03* (0.02)	-0.04 (0.02)	-0.04* (0.02)	-0.09*** (0.03)
	Election X Kaopen	0.00 (0.01)	0.00 (0.01)	0.01 (0.01)	0.01 (0.01)	0.02* (0.01)	0.01 (0.01)	0.04*** (0.01)
	Election	0.01 (0.01)	0.03** (0.02)	0.01 (0.02)	0.01 (0.03)	0.02 (0.03)	0.01 (0.02)	-0.03 (0.04)
	Election X Kaopen	-0.01 (0.01)	-0.00 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	0.01 (0.02)
Net Position	Election	-5.48 (3.99)	-8.69* (4.75)	-3.13 (3.24)	-3.82* (2.21)	-6.86 (4.90)	-6.32* (3.47)	6.68 (6.51)
	Election X Kaopen	1.12 (1.79)	1.36 (2.37)	2.24 (1.81)	1.37 (1.62)	1.96 (2.59)	3.23 (2.17)	-5.00 (3.35)
Consumption	Election	0.01*** (0.00)	0.03*** (0.01)	0.01*** (0.00)	0.01*** (0.00)	0.03*** (0.01)	0.01* (0.00)	0.03*** (0.01)
	Election X Kaopen	-0.00 (0.00)	-0.00 (0.00)	-0.00** (0.00)	-0.00** (0.00)	-0.02*** (0.00)	-0.01** (0.00)	-0.02*** (0.00)
	Election	0.01 (0.01)	-0.01 (0.01)	0.00 (0.01)	0.01 (0.01)	-0.01 (0.02)	0.00 (0.01)	-0.01 (0.02)
	Election X Kaopen	-0.01 (0.01)	-0.01 (0.01)	0.00 (0.00)	-0.00 (0.01)	-0.00 (0.01)	0.00 (0.00)	0.00 (0.01)
Real exchange rate	Election	-0.03 (0.04)	-0.02 (0.06)	0.00 (0.07)	-0.02 (0.05)	-0.00 (0.07)	0.01 (0.08)	-0.12 (0.16)
	Election X Kaopen	0.00 (0.02)	-0.00 (0.03)	-0.01 (0.03)	-0.01 (0.03)	-0.05 (0.06)	-0.05 (0.04)	0.01 (0.08)

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The coefficient is the coefficient on the Election variable. The interaction between Election and Kaopen is included in all models.

All models have been estimated using the GMM estimation technique. The title labels FX, CBI, and ER indicate different subsamples.

FX indicates a fixed exchange rate regime, CBI indicates an independent central bank, and FC represents fiscal constraints. X indicates interactions. CON represents all cases, in which the central bank is independent, fiscal policy is constrained, and the country pursues a fixed exchange rate.