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Elections hinder firms' access to credit

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Florian Léon and Laurent Weill

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Helsinki 2022

Florian Léon and Laurent Weill

Elections hinder firms' access to credit*

Abstract

To analyze whether the occurrence of elections affects access to credit for firms, we perform an investigation using firm-level data covering 44 developed and developing countries. The results show that elections impair access to credit. Specifically, firms are more credit-constrained in election years and pre-election years as elections exacerbate political uncertainty. While lower credit demand is a tangible negative effect of elections, their occurrence per se does not seem to affect credit supply. We further establish that the design of political and financial systems affects how elections influence access to credit.

Keywords: elections, access to credit, credit constraints
JEL: G21, D72, O16

Florian Léon, orcid.org/0000-0002-6969-7278. Fondation pour les Etudes et Recherches sur le Développement International, 63 boulevard François Mitterrand, 63000 Clermont-Ferrand, France. E-mail: florian.leon1@ferdi.fr

Laurent Weill, orcid.org/0000-0002-8630-1351. Corresponding author. EM Strasbourg Business School, Université de Strasbourg, 61 avenue de la Forêt Noire, 67000 Strasbourg, France. E-mail: laurent.weill@unistra.fr

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1 Introduction

Access to credit is crucial for companies. Credit-constrained firms are unable to realize worthwhile projects and may find themselves unable to exploit investment opportunities when they arise. The lack of access to credit can hurt firm productivity (Gatti and Love, 2008; Butler and Cornaggia, 2011) or hamper firm growth (Beck and Demirgüc-Kunt, 2006; Fafchamps and Schündeln, 2013). Thus, good access to credit is beneficial for the expansion of the private sector and fosters aggregate productivity that contributes to economic growth.

A wide range of determinants of access to credit has been identified at both the firm level (e.g. gender for Asiedu et al., 2013; ownership for Knyazeva, Knyazeva, and Stiglitz, 2009) and the country level (e.g. foreign bank participation for Clarke, Cull, and Martinez-Peria, 2006, and bank competition for Léon, 2015). The specific attributes of institutional frameworks influence financing obstacles, *directly* through such factors as the legal system's efficiency and quality of governance (Beck et al., 2006), and *indirectly* through relaxation of loan conditions through legal origins and protection of creditor rights (Qian and Strahan, 2007; Bae and Goyal, 2009).

Looking more closely at institutional characteristics that might influence financing obstacles for firms, recent evidence implicates the impact of elections on bank lending decisions (Dinc, 2005; Carvalho, 2014). The literature is divided as to the impact of elections on access to credit.

One view posits that *electoral episodes promote access to credit*, a notion with its roots in the political business cycle literature pioneered by Nordhaus (1975) and extended by Rogoff and Sibert (1988). Accordingly, politicians manipulate economic instruments to enhance their chances of reelection. This view predicts that incumbent governments will use loans as a strategic tool for re-election purposes. They would influence lending behavior of banks so that greater credit would be granted in election times.

Moreover, governments motivate banks to boost their lending during the run-up to an election. Besides their direct influence on state-owned bank lending, governments can influence private bank lending through a wide set of carrots and sticks, e.g. changes in banking regulation, threats of withdrawing banking licenses, and access to public entity loan market (Delatte, Matray, and Pinardon-Touati, 2020).

This view is empirically supported by the findings that state-owned bank lending is used to influence political outcomes. Lending of state-owned banks is correlated with the electoral cycle in the sense that state-owned banks increase lending in election years relative to private banks (Dinc, 2005; Carvalho, 2014; Englmaier and Stowasser, 2017). It is also supported by the finding

that bank failures tend to be delayed during electoral episodes (Brown and Dinc, 2005; Liu and Ngo, 2014) and by the recent work from Muller (2020) showing that macroprudential regulation is influenced by electoral cycles.

A second view assumes that *elections impair access to credit*. Elections exacerbate political uncertainty as e.g. the identity of the winning party, the economic policies to be implemented,[1] and the risk of political violence in election times are at stake. Political uncertainty has been shown to cause firms to delay their investments (Baker, Bloom, and Davis, 2016; Azzimonti, 2018) and increase bank loan pricing (Francis, Hasan, and Zhu, 2014).

Elections can reduce credit demand as fewer firms request loans to finance their future prospects until the uncertainties about the forthcoming economic environment diminish. Further, elections could reduce the credit supply if banks are reluctant to lend in uncertain times. Thus, elections might limit access to credit by either reducing credit demand or credit supply through greater uncertainty. This hypothesis is empirically supported by the finding that firms reduce investment expenditures during election years (Julio and Yook, 2012).

The effect of elections on access to credit is therefore ambiguous from a theoretical perspective. But it is stunning – given the massive body of literature devoted to access to credit – that the influence of elections on access to credit has never been empirically investigated. In this paper, we shed light on the question of whether the occurrence of elections affects access to credit for firms. To scrutinize this question, we perform an empirical investigation on firm-level data from a large cross-country dataset of firms of developed and developing countries. Our main data source is the World Bank Enterprise Survey (WBES). It contains surveys regularly performed in various countries since 2005, including information on credit constraints. We combine this information with data on elections to investigate whether electoral episodes affect access to credit, examining whether election years, pre-election years, and post-election years are associated with changes in access to credit.

One central challenge in our investigation is the identification of credit-constrained firms. We adopt the approach from Popov and Udell (2012) and Léon (2015), defining "credit-constrained firms" as firms that applied for credit and were denied or did not apply for credit because they were discouraged. This approach avoids the drawbacks of alternative approaches such as focusing on firms that perceive access to finance as an obstacle to their operations, which

[1] In a recent work on 23 countries, Baker et al. (2020) show that economic policy uncertainty rises in the months leading up to elections

is subject to perception bias, (Clarke et al., 2006), or the defining "credit-constrained firms" as those that do not use credit, i.e. including firms that have no need to apply for credit (Love and Martinez-Peria, 2015).

A key advantage of our approach is that we can identify whether elections exert an impact separately on borrowers and on lenders. We can thus disentangle the supply-and-demand effects in the relation between elections and access to credit. It allows us to examine how elections influence access to credit and identify the mechanisms through which this effect takes place.

The WBES firm-level data also assure our sample is representative of the experience of small firms and internationally diverse. We do not restrict our analysis to the influence of elections on large listed companies, which likely do not suffer the same lack of access to credit as small companies. We also do not restrict our investigation to developed countries where access to credit is less of a concern than for firms in developing countries.

Furthermore, we can enrich this study of the impact of elections on access to credit by considering the firm-level and country-level characteristics that may affect it. We first concentrate on firm-level characteristics associated with firm opacity. *Firm opacity* is a major determinant of access to credit as opaque firms have greater difficulties to get a bank loan. It therefore matters to know whether elections exert a differentiated impact on the access to credit of firms based on their opacity.

We consider the potential influence of *political and financial system features* on the effects of elections on access to credit. On one hand, the degree of democracy of the political system affects the manipulation and uncertainty channels. Democratic regimes can have fewer opportunities to manipulate bank lending and may face less uncertainty because the risk of political violence after elections is lower. On the other hand, the degree of bank competition and the size of the financial system influence the possibilities for governments to manipulate bank lending. Large, competitive financial systems are associated with lower financing constraints for firms.

Our work has several limitations. First, we lack information about the lenders for each firm. It would have been of interest to know the characteristics of the lending banks in line with the hypotheses. For instance, state-owned banks or banks in poor financial conditions that face losing their licenses may relax access to credit in election times. Even so, this limitation does not prevent us from investigating the impact of elections on access to credit. Second, while our dataset only includes 51 elections from 44 countries, this number of electoral episodes is sufficient for our analysis. Moreover, it is related to the difficulties in getting data on access to credit for a large

cross-country sample of firms. From this perspective, the WBES data are the best dataset to our knowledge for this research question.

We find evidence that elections exert a detrimental influence on access to credit. Firms are most credit-constrained in election and pre-election years, thereby corroborating our intuition about the uncertainty channel. This effect takes place on the *borrower side*, i.e. we observe greater borrower discouragement during electoral periods. By contrast, no impact is found on the lender side, meaning that elections do not overall affect credit supply. Furthermore we show that firm-level and country-level characteristics can affect the effect of elections on access to credit. Although borrower discouragement is observed for all types of firms in election years and pre-election years, credit supply is reduced for opaque firms in both election and post-election years.

Political systems determine the detrimental effects of elections. Borrower discouragement is amplified in more democratic countries. Financial systems also matter. The detrimental impact of elections is stronger when the size of the financial system is larger and the degree of bank competition higher.

We conduct a broad range of robustness tests, tackling potential econometric concerns and checking whether the characteristics of elections drive our results. Our results hold in these tests.

In providing these findings, we contribute to the current literature in three ways. First, we contribute to the literature on elections and banking. While works have shown that electoral episodes can be accompanied with a rise in bank lending (e.g. Dinc, 2005; Carvalho, 2014; Englmaier and Stowasser, 2017), we provide evidence that elections can lead to lower access to credit. We explicate a specific mechanism – *borrower discouragement* – through which the electoral process affects access to credit. We therefore complement the literature identifying the political incentives for banks to increase credit supply by identifying a credit demand mechanism. We do not assert that no manipulation from political authorities takes place to favor bank lending, only that there is evidence that the detrimental impact of the uncertainty channel dominates any manipulation channel for the access to credit.

Second, we add to the burgeoning literature on the impact of democracy on credit, given the key role of elections in the design of democratic regimes. Huang (2010) has shown that democratization promotes financial development. Delis, Hasan, and Ongena (2020) have demonstrated that greater democratic development reduces cost of credit for companies. Unlike studies that analyze the degree of democracy, our research concentrates on a single major characteristic of democracies – the occurrence of elections.

Third, we contribute to the literature on the determinants of access to credit by identifying the influence of electoral episodes. We show that the design of the political regime affects access to credit. We thus provide evidence that the institutional framework can influence the share of credit-constrained firms through political institutions in addition to legal institutions (e.g. Beck et al., 2006).

The remainder of the paper is organized as follows. Section 2 describes the data and variables. Section 3 displays the empirical strategy. Section 4 presents the main results. Section 5 reports the extensions, and Section 6 concludes.

2 Data and variables

2.1 Data

To investigate how elections affect credit access, we combine firm-level data from WBES with information on the date of elections extracted from Election Guide (www.electionguide.org). Sample-selection uses three steps.

Taking the harmonized surveys from WBES (retrieved in March 2020), we exclude surveys for which questions regarding credit experience are unavailable (questions *k16* to *k20*).

Next, information on credit experience refers to the last (fiscal) year, not the year of the interview. Therefore, we need to identify the relevant (last) year for each firm. With some exceptions, the WBES does not provide information on the last fiscal year in survey questions. When the precise date retained for the past year is provided (question *a20y*), we employ this information. For other firms, we assume that the last year is the year before the interview (available in question *a14y*). For instance, if a firm was interviewed in 2016, the last year would be 2015. We provide a simple test to gauge the validity of our assumption, which is largely confirmed.[2]

[2] To gauge the validity of our assumption, we selected all surveys (i) for which we have information on the date of the last year considered in the questionnaire (question *a20y*) and (ii) for which surveys overlap several civil years. We then compared the "real" fiscal year (question *a20y*) with the "theoretical" fiscal year based on our assumption (*a14y-1*). Our assumption is valid for 83% of observations. In detail, we consider the following surveys (number of firms – percentage of cases where our assumption is confirmed): Argentina2017 (991 – 80%); Belarus (600 – 100%); Colombia2017 (993 – 94%); Cyprus2019 (240 – 71%); Egypt2016 (1814 – 70%); Greece2018 (600 – 77%); Kenya2018 (1001 – 74%); Latvia2019 (359 – 85%); Mozambique2018 (601 – 84%); Myanmar2016 (607 – 75%); Peru2016 (1003 – 93%); Turkey2019 (1663 – 97%). For the rest of the document, we will refer to year instead of last fiscal year to simplify the reading. So, when we say firms are observed in an election year, we say that the last fiscal year is an election year.

Finally, we only include countries with firms during an election year and during a non-election year. Let us consider two examples. We exclude Argentina because we *never* observe firms during an election year for this country. Argentina's elections occurred in 2007, 2011, 2015, and 2019. Their firm surveys are for 2009, 2010, 2016, and 2017. While 2010 is a pre-election year and 2016 is a post-election year, we never observe firms during an election year. In contrast, we include Côte d'Ivoire. Elections occurred in 2000, 2010 and 2015 and we have information on firms both during non-election years (2007, 2008 and 2016) and during an election year (2015). In other words, for all countries under investigation, we have firms during an election year and *at least one* non-election year. This restriction is crucial for our identification strategy, which consists on comparing firms operating in the same country but surveyed at different periods of electoral cycle as explained below.

The final sample comprises 24,921 firms from 44 countries (92 surveys). Among all firms, 37% of firms are observed during an election year, 26% during a pre-election year, 23% during a post-election year and 17% neither during an election year nor during a pre- or post-election year. We provide details regarding the number of firms per country and the list of elections considered in the Appendix. This gives a total of 51 elections, because two elections occurred in seven countries (Czech Republic, Ethiopia, Kenya, Russia, Serbia, Turkey, and Zambia).

2.2 Variables

To measure credit access and its components (demand and supply), we follow the methodology developed by Popov and Udell (2012) and extended by Léon (2015). A firm is declared as having access to credit if it obtained at least one loan in the past year. Credit-constrained firms assemble (i) rejected applicants (firms whose application was turned down) and (ii) discouraged borrowers (firms that refused to apply despite a need for external finance). We restrict our sample to firms with a need for external funds as it is impossible to know whether a firm not seeking a loan is constrained or not.[3]

The main dependent variable (*Access*) is a dummy variable equal to one for firms with a credit and zero for discouraged borrowers and rejected firms. In line with Léon (2015), we create two additional variables to identify if the credit constraint is due to demand-side of supply-side factors. The second variable (*Demand*) is a dummy variable equal to one if the firm sought a loan

[3] The complete procedure employed to classify firms in four different categories (firm with no need, firm with a loan, discouraged borrower, and rejected firm) from questions in WBES is described in details by Léon (2015).

(i.e. adding firms with a credit and rejected firms) and zero for discouraged borrowers. Finally, we focus on firms asking for a loan and we create a variable (*Supply*) taken value one if a firm secured a loan, and zero if the firm's loan application was rejected.

Table 1 reports descriptive statistics for the variables. It indicates that only 44% of firms with a need of funds have access to credit, stressing the importance of credit-constrained firms. Credit constraints are largely explained by discouragement of borrowers, which was highlighted in the literature (Chakravarty and Xiang, 2013; Han, Fraser and Storey, 2009). Among constrained firms, almost nine-tenths are discouraged borrowers. When applied, the majority of firms received at least one loan (84% of applicants).

Table 1. Descriptive Statistics

This table provides descriptive statistics for the variables used in the analysis.

Variable	Obs.	Mean	Std. Dev.	Min	Max
Access	24,253	0.44	0.50	0	1
Demand	24,253	0.50	0.50	0	1
Supply	12,149	0.88	0.33	0	1
Employees	24,253	92.95	420.2	0	21,000
Age	24,253	16.03	15.10	0	194
Foreign owned	24,253	0.07	0.26	0	1
State owned	24,253	0.01	0.08	0	1
Partnership	24,253	0.16	0.36	0	1
Sole Proprietorship	24,253	0.29	0.45	0	1
Audited	24,253	0.46	0.50	0	1
Manufacturing	24,253	0.48	0.51	0	1
Services	24,253	0.50	0.50	0	1
Construction	24,253	0.02	0.15	0	1
GDP growth	121	4.06	3.42	-2.81	24.05
Inflation	121	6.47	5.66	-0.94	33.25
Fin Development	121	35.67	26.59	4.28	194.30
GDP per capita (log)	121	8.03	1.30	5.40	10.48
Gov Exp	121	15.46	4.28	6.99	26.24
Democracy	111	5.77	3.45	0	10
Bank competition	117	-0.05	0.16	-0.28	1.09

Our main explanatory variable is the dummy variable *Election* which takes the value of one for an election year, and zero otherwise. We also consider pre-election and post-election years. The pre-

election dummy (*Pre-election*) takes value one for one year before an election, while the post-election dummy (*Post-election*) equals one for one year after an election. "Election" refers to a presidential election for presidential systems and parliamentary election for parliamentary systems. For the few countries with an assembly-elected president, we consider presidential and parliamentary elections.

"Political regime" is obtained from the Database of Political Institutions and completed by us for missing information (notably in the Balkans or in recent years). For pre- and post-election dummies, we consider the elections displayed in Table A2 as well as other elections occurring in the country. In the case of Turkey, for instance, data on firms are available in 2007, 2012, 2013, 2017, and 2018. Elections in Turkey occurred in 2007, 2011, 2015 and 2018. Therefore, the *Election* dummy for election takes a value of one for firms in 2007 and 2018, the *Pre-election* dummy equals one for firms in 2017, and *Post-election* dummy for firms in 2012. The three dummies equal zero for firms in 2013.

3 Methodology

This paper examines the relationship between elections and firms' access to credit. Given the binary nature of our dependent variable, we run probit regressions with the following specification:

$$Pr(Y_{ict} = 1) = \Phi(\alpha_c + \mu_t + \beta_{elec} Election_{ct} + \beta_{pre} Pre - election_{ct} + \beta_{post} Post - election_{ct} + \Upsilon C_{ct} + \Omega F_{ict}), \quad (1)$$

where i, c, and t refer to firm *i*, at year *t*, in country *c*. We consider three alternative dependent variables (Y_{ict}): *Access*, *Demand*, and *Supply*. $Election_{ct}$ equals one if an election occurred in country *c* at year *t*. $Pre\text{-}election_{ct}$ equals one if an election will occur in country *c* at year *t+1* and $Post\text{-}election_{ct}$ a dummy if an election occurred in country *c* at year *t-1*.

If the leader manipulates the election process, we expect that credit access is improved during election years ($\beta_{elec} > 0$), pre-election years ($\beta_{pre} > 0$), or both. However, the effect of post-election years should be negative or null ($\beta_{post} \leq 0$). We expect that banks restrict their loans after softening their standards in previous year(s). The positive effect of election and pre-elections years should reflect in lender decisions and in borrower willingness to apply for loans if they anticipate the positive response from the bank.

If the uncertainty channel dominates, we expect an opposite sign for election years ($\beta_{elec} < 0$), pre-election years ($\beta_{pre} < 0$), or both. The impact of uncertainty can be important for both borrowers and lenders. The impact of post-election years is unclear. On the one hand, the degree of uncertainty could be reduced after elections. A new leader emerges (or the incumbent is reelected) and the political uncertainty sharply declines. On the other hand, elections in many countries do not completely resolve the power transition. Additional rounds may be required especially to build parliamentary coalitions or post-electoral crises could arise due to contested results.

Elections are sometimes considered exogenous as the government cannot adjust the date of the election (fixed calendar) to increase its chances for re-election.[4] However, even when the calendar is fixed, election outcome can be influenced by government decisions. This point often raises a concern regarding the assumption of exogeneity of elections. We are not concerned by this problem of endogeneity here. Indeed, we are quite interested by the possibility that a leader might manipulate the election outcome as a possible explanation for our findings. Our aim is simply to gauge whether the government influences the banking industry in election years.

Exogenous election dates are not enough to tackle all identification issues. The impact of elections can be blurred by difference in environments where firms operate. Our main strategy for limiting this problem involves comparing firms during an election year relative to firms during a non-election year in the same country. To do so, we add country dummies (α_c) that account for all unobserved country heterogeneity. We also add time-dummies (μ_t) to control for common global shocks, such as the global financial crisis. This procedure is still insufficient due to changes in the macroeconomic environment over time and can be influenced by electoral cycles. In particular, leaders may manipulate fiscal tools, expenses, or both, to spur growth during (pre-) election years. Even if manipulation seems exaggerated in the literature (Mandon and Cazals, 2019), we control for this issue. We include two macroeconomic variables that reflect short-run economic situations (*GDP growth*, *Inflation*) and the government fiscal policy with the ratio of government expenditures to GDP (*Gov Exp*). We also add the usual proxies for economic development with the log of income per capita (*GDP per capita*) and financial development with the ratio of domestic credit to the private sector to GDP (*Fin Dev*). Macroeconomic variables are collected from World Development Indicators and Global Financial Development Database.

[4] For some elections, the calendar is not fixed and elections are anticipated or postponed (as indicated in Table A2). While these decisions can be justified for many non-political reasons, they may affect our findings. We discuss this issue in the robustness checks.

Finally, we include a set of firm-level variables (F_{ict}) to control for observable heterogeneity. We add firm size measured by the log of the number of employees (*Employees*) and firm age defined as the log of the age of the firm (*Age*). We include dummy variables equal to one if the firm is owned by foreign investors (*Foreign*), owned by the government (*Government)*, operates in manufacturing (*Manufacturing*), construction (*Construction*) or services (*Services*), and if the firm is audited (*Audited*), is privately held (*Private*) or is a partnership (*Partnership*). These variables are extracted from the WBES, and their definitions appear in the Appendix. Table 1 indicates that firms have on average more than 90 employees and are 16 years old. Of course, this average hides heterogeneity in terms of size. The median firm has 19 employees and less than a fifth (16%) of firms have more than 100 employees. One-half of firms operate in services, 48% in manufacturing and 2% in construction. Firms under investigation are mostly local and privately-owned businesses (92%).

4 Results

4.1 Main estimations

Table 2 displays the results of the main estimations. The dependent variable is the dummy variable for credit access. We consider four specifications according to the inclusion of country-level control variables and to the inclusion of pre- and post-election year dummies to test the sensitivity of the results. In column (1), we include only the dummy *Election* and the firm-level control variables. We add *Pre-election* and *Post-election* in column (2) or country-level control variables in column (3). Finally the specification in column (4) includes *Pre-election*, *Post-election* and country-level control variables. In all estimations, we report marginal effects and standard errors are clustered at the country-year level.[5]

[5] It is usual to cluster standard errors at the treatment unit (Cameron and Miller, 2015). In an unreported analysis, we test whether our findings are sensitive to the correction of standard errors by considering alternative clustering levels (country and year, separately) and alternative procedures to correct standard errors. Statistical significance of results is unaffected and often reinforced.

Table 2. Impact of elections on access to credit

Probit estimations are performed. The dependent variable is *Access*. The table reports marginal effects and associated standard errors in parentheses. *, **, *** denote an estimate significantly different from 0 at the 10%, 5%, or 1% level. Standard errors are adjusted for clustering at the country-year level. Definitions of all variables are reported in the Appendix.

	(1)	(2)	(3)	(4)
Election	-0.025	-0.038*	-0.029**	-0.052***
	(0.017)	(0.021)	(0.012)	(0.014)
Pre-election		-0.097		-0.051***
		(0.072)		(0.019)
Post-election		-0.049		-0.029
		(0.075)		(0.019)
Empl (log)	0.077***	0.077***	0.075***	0.075***
	(0.005)	(0.005)	(0.006)	(0.006)
Age (log)	0.002	0.003	0.001	0.001
	(0.005)	(0.005)	(0.005)	(0.005)
Foreign owned	-0.031**	-0.031**	-0.027*	-0.027*
	(0.015)	(0.015)	(0.015)	(0.015)
State owned	-0.095***	-0.095***	-0.125***	-0.124***
	(0.032)	(0.033)	(0.048)	(0.048)
Partnership	-0.008	-0.007	-0.017	-0.017
	(0.015)	(0.015)	(0.015)	(0.015)
Sole Proprietorship	-0.059***	-0.060***	-0.052***	-0.054***
	(0.013)	(0.013)	(0.012)	(0.012)
Audited	0.102***	0.102***	0.099***	0.099***
	(0.010)	(0.010)	(0.010)	(0.010)
Service	0.019**	0.020***	0.018**	0.019**
	(0.007)	(0.008)	(0.008)	(0.008)
Construction	-0.032	-0.030	-0.019	-0.016
	(0.035)	(0.035)	(0.041)	(0.041)
GDP growth			0.009***	0.010***
			(0.002)	(0.002)
Inflation			-0.008***	-0.009***
			(0.002)	(0.002)
Fin Development			-0.001	-0.002
			(0.001)	(0.001)
GDP per capita (log)			0.372**	0.455**
			(0.164)	(0.153)
Gov Exp			-0.010***	-0.009***
			(0.004)	(0.003)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Obs.	24253	24253	21227	21227
Country#year	142	142	121	121
Pseudo-R2	0.20	0.20	0.22	0.22

Three key findings emerge. First, we find that elections have a negative impact on access to credit. The coefficient of *Election* is negative in all estimations, and significant in all of them with the exception of the first specification. Second, we obtain evidence that access to credit is reduced in pre-election years. The coefficient of *Pre-election* is negative in both estimations, and significant in the specification including all variables. Third, we find no support for any significant difference in access to credit in post-election years. The coefficient of *Post-election* is not significant in either estimation.

To sum up, we conclude that elections affect credit-constrained firms by reducing access to credit during the year of elections and the year preceding elections. Therefore, electoral episodes hamper access to credit in line with the uncertainty channel. By increasing uncertainty, the occurrence of elections deteriorates access to credit for firms. It generates political uncertainty on the identity of the winning party and the risk of political violence surrounding the organization of elections and economic policy uncertainty on the economic policies to be implemented following elections.

In terms of economic significance, the impact of elections on access to credit is far from anecdotal. When considering the specification with all variables in column (4), we observe that access to credit is reduced by 5.2 percentage points in election years and by 5.1 percentage points in pre-election years. This is economically sizeable, given that the average ratio of access to credit is only 44% and that we compare firms operating in the same country at two periods.

We now turn to the analysis of control variables. Larger firms and audited firms have a better access to credit in line with the view that greater transparency reduces credit constraints for firms. State ownership and foreign ownership of firms are associated with lower access to credit. These results can be explained by the lower need for bank credit of state-owned and foreign-owned firms that enjoy alternative sources of funds. An alternative explanation is that banks have better information about domestic-owned firms than foreign-owned firms. At the country level, higher growth and higher per capita income contribute to reduce financing constraints. Conversely, higher inflation and greater government expenses increase the probability that a firm will be credit-constrained.

4.2 How do elections hamper access to credit?

Our main estimations show that elections increase firm credit constraints. We now explore this evidence in greater depth by examining the channels through which elections impair access to credit.

We want to examine whether the transmission channel goes through credit demand channel by discouraging firms from applying for loans, through the supply channel by reducing the number of approved credit applications, or both. The uncertainty channel can take place through lower credit demand and lower credit supply as borrowers and lenders can each react to greater uncertainty by reducing their willingness to get involved in loan contracts. Thus, we investigate whether elections influence the borrower's decision to apply for a loan (credit demand) and the bank's decision to approve or reject the loan (credit supply).

We first test the impact of elections on the decision to apply for a loan for firms by performing regressions to explain the likelihood that a firm will apply for a loan. The results reported in Table 3 show that firms are less likely to apply for a loan during election and pre-election years, while their decision to apply for a loan is unaffected in post-election years. *Election* is negative in all estimations, and significant in all but one specification. Furthermore *Pre-election* is always significantly negative, while *Post-election* is never significant.

Thus, we clearly find support for the view that elections make firms more reluctant to seek a loan during election and pre-election years. Greater uncertainty during these years affects the behavior of borrowers, by reducing their willingness to increase their debt burden and get involved in new investments in uncertain times. This conclusion accords with the results from Julio and Yook (2012) and Azzimonti (2018), who find that greater political uncertainty induced by elections leads firms to delay their investments.

Next, we consider the impact of elections on a bank's decision to accept or reject a loan. We redo our regressions to explain credit approval behavior. Table 4 displays the results. We find no impact of the electoral period on a bank's decision to accept or reject a loan/ *Election*, *Pre-election* and *Post-election* are not significant in all estimations. Thus, elections do not affect credit supply from banks.

Table 3. Elections and loan application decision (credit demand)

Probit estimations are performed. The dependent variable is *Demand*. The table reports marginal effects and associated standard errors in parentheses. *, **, *** denote an estimate significantly different from 0 at the 10%, 5%, or 1% level. Standard errors are adjusted for clustering at the country-year level. Definitions of all variables are reported in the Appendix.

	(1)	(2)	(3)	(4)
Election	-0.24	-0.039*	-0.025**	-0.052***
	(0.019)	(0.023)	(0.012)	(0.015)
Pre-election		-0.040*		-0.065***
		(0.023)		(0.020)
Post-election		-0.011		-0.029
		(0.024)		(0.019)
Empl (log)	0.075***	0.075***	0.073***	0.073***
	(0.005)	(0.005)	(0.005)	(0.005)
Age (log)	-0.004	-0.004	-0.006	-0.006
	(0.005)	(0.005)	(0.006)	(0.006)
Foreign owned	-0.034**	-0.034**	-0.027*	-0.026*
	(0.015)	(0.015)	(0.015)	(0.015)
State owned	-0.078**	-0.078**	-0.152***	-0.152***
	(0.039)	(0.039)	(0.050)	(0.050)
Partnership	-0.020	-0.020	-0.027	-0.027
	(0.017)	(0.017)	(0.016)	(0.016)
Sole Proprietorship	-0.063***	-0.063***	-0.052***	-0.054***
	(0.014)	(0.014)	(0.013)	(0.013)
Audited	0.110***	0.11***	0.107***	0.107***
	(0.010)	(0.010)	(0.009)	(0.009)
Services	0.014*	0.014*	0.015*	0.015*
	(0.008)	(0.008)	(0.008)	(0.008)
Construction	-0.008	-0.008	0.011	0.011
	(0.037)	(0.037)	(0.04)	(0.04)
GDP growth			0.008***	0.009***
			(0.002)	(0.002)
Inflation			-0.009***	-0.010***
			(0.002)	(0.002)
Fin Development			-0.002*	-0.002*
			(0.001)	(0.001)
GDP per capita (log)			0.387**	0.491***
			(0.155)	(0.144)
Gov Exp			-0.014***	-0.013***
			(0.004)	(0.004)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Obs.	24253	24253	21227	21227
Country#year	142	142	121	121
Pseudo-R2	0.20	0.20	0.22	0.22

Table 4. Elections and loan acceptance decision by banks (credit supply)

Probit estimations are performed. The dependent variable is *Supply*. The table reports marginal effects and associated standard errors in parentheses. *, **, *** denote an estimate significantly different from 0 at the 10%, 5%, or 1% level. Standard errors are adjusted for clustering at the country-year level. Definitions of all variables are reported in the Appendix.

	(1)	(2)	(3)	(4)
Election	-0.009	-0.011	-0.011	-0.014
	(0.007)	(0.009)	(0.007)	(0.009)
Pre-election		0.005		0.004
		(0.011)		(0.013)
Post-election		-0.017		-0.016
		(0.012)		(0.011)
Empl (log)	0.031***	0.031***	0.029***	0.029***
	(0.005)	(0.004)	(0.005)	(0.005)
Age (log)	0.011**	0.011**	0.012**	0.012**
	(0.005)	(0.005)	(0.005)	(0.005)
Foreign owned	-0.006	-0.006	-0.011	-0.011
	(0.013)	(0.013)	(0.014)	(0.014)
State owned	-0.060**	-0.059**	-0.001	-0.001
	(0.026)	(0.026)	(0.048)	(0.048)
Partnership	0.018*	0.017*	0.013	0.013
	(0.010)	(0.010)	(0.011)	(0.011)
Sole Proprietorship	-0.011	-0.011	-0.013	-0.013
	(0.009)	(0.009)	(0.010)	(0.010)
Audited	0.019**	0.019**	0.020**	0.020**
	(0.008)	(0.008)	(0.008)	(0.008)
Service	0.017**	0.017**	0.016***	0.016***
	(0.006)	(0.006)	(0.006)	(0.006)
Construction	-0.049**	-0.049**	-0.059***	-0.059***
	(0.020)	(0.020)	(0.016)	(0.016)
GDP growth			0.004***	0.004***
			(0.001)	(0.001)
Inflation			-0.000	-0.000
			(0.002)	(0.002)
Fin Development			0.000	0.000
			(0.001)	(0.001)
GDP per capita (log)			0.062	0.062
			(0.092)	(0.092)
Gov Exp			0.001	0.001
			(0.002)	(0.002)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Obs.	12,129	12,129	10,811	10,811
Country#year	139	139	119	119
Pseudo-R2	0.13	0.13	0.14	0.14

This finding suggests that elections do not at all influence credit approval behavior, but it could also come from the fact that elections exert two opposing effects on credit supply that offset each other. On the one hand, banks can be manipulated to increase their lending during electoral episodes, in line with the manipulation channel, as demonstrated by Dinc (2005) and Carvalho (2014) among others for state-owned banks. On the other hand, banks may be discouraged to lend in election times due to the heightened uncertainty surrounding this period, in line with the uncertainty channel, as shown by Francis et al. (2014).

In a nutshell, the analysis of the mechanisms taking place through credit demand and credit supply provides a better understanding of how elections affect access to credit. Our key finding that elections reduce access to credit takes place only through the influence of elections on borrowers. Elections impair access to credit, in line with the uncertainty channel, through their impact on credit demand. They do not depress credit supply.

4.3 Robustness checks

We check the robustness of our results in several ways. The results of the robustness tests are displayed in Tables 5 and 6. In both tables, Panel A reports results for access to credit access, Panel B for credit demand, and Panel C for credit supply. In all estimations, we consider our baseline model including all firm-level and country-level control variables.

We begin with a series of robustness tests tackling potential econometric concerns in Table 5. Our model is similar to a treatment effect since the variable of interest is a dummy variable. The estimation of treatment effect can therefore be biased if only few observations take value one for the variable of interest as the weight given to these observations is too great in the regression (Sloczynski, 2021). To tackle this concern, we remove those countries where fewer than 20% of firms are in column (1) in an election year.

We next compare firms in the same country in two different years. This comparison can be affected by the fact that the environment for the firms changes over the two years. In the main estimations, we added time-varying country-level variables to take this into account. We test another way to tackle this issue by excluding surveys when the time lapse between surveys is excessive (over four years). These estimations are presented in column (2).

Table 5. Robustness checks 1/2

Probit estimations are performed. The dependent variable is *Access* in Panel A, *Demand* in Panel B, and *Supply* in Panel C. In column (1), we remove countries when less than 20% of firms are in the election year. In column (2), we exclude surveys when the time lapse between two surveys exceeds four years. In column (3), we include country-sector fixed effects. In column (4), we run a probit model with sample selection. The table reports marginal effects and associated standard errors in parentheses. *, **, *** denote an estimate significantly different from 0 at the 10%, 5% or 1% level. Standard errors are adjusted for clustering at the country-year level. Definitions of all variables are reported in the Appendix.

	(1)	(2)	(3)	(4)
	Panel A : credit access			
Election	-0.086***	-0.075***	-0.061***	-0.022**
	(0.014)	(0.016)	(0.014)	(0.010)
Pre-election	-0.048***	-0.062***	-0.062***	0.022
	(0.018)	(0.019)	(0.016)	(0.013)
Post-election	-0.052***	-0.021	-0.038**	-0.022
	(0.016)	(0.019)	(0.016)	(0.014)
Obs.	17,412	19,125	17,752	21,143
	Panel B : credit demand			
Election	-0.091***	-0.073***	-0.067***	-0.021**
	(0.014)	(0.017)	(0.014)	(0.010)
Pre-election	-0.072***	-0.073	-0.068***	-0.037***
	(0.018)	(0.020)	(0.016)	(0.012)
Post-election	-0.044***	-0.021	-0.045***	-0.054***
	(0.016)	(0.019)	(0.016)	(0.013)
Obs.	17,412	19,125	17,752	21,063
	Panel C : credit supply			
Election	-0.017	-0.016*	-0.011	-0.011
	(0.016)	(0.009)	(0.016)	(0.010)
Pre-election	0.011	-0.007	-0.014	-0.026**
	(0.020)	(0.014)	(0.018)	(0.013)
Post-election	-0.029*	-0.013	-0.015	-0.026*
	(0.017)	(0.011)	(0.017)	(0.014)
Obs.	8,852	9,674	9,237	20,981

Table 6. Robustness checks 2/2

Probit estimations are performed. In columns (1) to (3), the dependent variable is *Access* in Panel A, *Demand* in Panel B, and *Supply* in Panel C. In column (1), we exclude anticipated or postponed elections. In column (2), we exclude elections occurring in January or in December. In column (3), we exclude elections won by a very large margin. In column (4), we test alternative dependent variables: a dummy variable equal to one if a firm purchases fixed assets in the previous year (Panel A), has a loan (Panel B) and declares financing to be a minor obstacle to its current operations (Panel C). The table reports marginal effects and associated standard errors in parentheses. *, **, *** denote an estimate significantly different from 0 at the 10%, 5%, or 1% level. Standard errors are adjusted for clustering at the country-year level. Definitions of all variables are reported in the Appendix.

	(1)	(2)	(3)	(4)
	Panel A: credit access			
Election	-0.037***	-0.066***	-0.054***	-0.041***
	(0.016)	(0.023)	(0.015)	(0.013)
Pre-election	-0.029	-0.061***	-0.052***	-0.045***
	(0.023)	(0.024)	(0.018)	(0.017)
Post-election	-0.022	-0.022	-0.030	-0.005
	(0.021)	(0.023)	(0.019)	(0.019)
Wald test				74.3***
Obs.	19,071	15,108	21,227	19,353
	Panel B: credit demand			
Election	-0.038**	-0.052**	-0.052***	-0.038***
	(0.016)	(0.023)	(0.015)	(0.013)
Pre-election	-0.049*	-0.053*	-0.064***	-0.056***
	(0.025)	(0.023)	(0.020)	(0.018)
Post-election	-0.029	-0.015	-0.029	-0.006
	(0.022)	(0.023)	(0.019)	(0.017)
Wald test				51.2***
Obs.	19,071	15,108	21,227	19,353
	Panel C: credit supply			
Election	-0.005	-0.021*	-0.019**	-0.007
	(0.010)	(0.011)	(0.009)	(0.005)
Pre-election	0.015	-0.027*	-0.001	0.007
	(0.015)	(0.014)	(0.013)	(0.007)
Post-election	-0.008	-0.020*	-0.018*	-0.008
	(0.010)	(0.011)	(0.011)	(0.006)
Wald test				9.7***
Obs.	9,837	7,883	10,653	10,040

The main estimations allow considering only country shocks common to all sectors (with the inclusion of country fixed effects) and worldwide shocks (with the inclusion of time fixed effects). Shocks such as a regulatory change or shift in demand, however, can occur at the country-sector level. To take these shocks into account, we include country-sector fixed effects in the estimations. We do not include these country-sector dummies in the main estimations due to the incidental parameter problem. We rerun the baseline model by employing country-sector fixed effects in column (3).

We now consider the problem of sample selection. Our main estimations did not consider the data-generating process, being run on a sub-sample of firms needing credit. To take this problem into account, we run a probit model with sample selection in column (4). We employ the same procedure and the same exclusion variables than those employed by Léon (2015).[6]

We also perform a battery of robustness tests to investigate whether the characteristics of elections drive our results. The results are reported in Table 6. For these tests, we start by excluding anticipated or postponed elections. As explained above, elections are often considered as exogenous as the setting the date of the election is not in the hands of the leadership. However, this assumption does not hold when the calendar of elections is adjustable (not fixed calendar). We thus rerun our baseline model in column (1) by excluding anticipated or postponed elections.[7]

Next, we exclude elections occurring in January or in December. The classification of election year is complex and questionable for elections taking place at the beginning or at the end of the year. For instance, if a firm was surveyed in October 2016 and election occurred in January 2017, it is unclear whether 2016 is a pre-election year for the firm. Estimations excluding elections in January or in December are reported in column (2).

In the third step, we exclude elections won by a wide margin. For example, elections may be staged to placate the international community with no obvious stakes. The name of the winner is then known in advance, despite the election ceremonial. Uncertainty and manipulation channels can therefore be influenced by this situation since uncertainty is then lower and since manipulation may be less appealing for authorities than in regular elections. To take this concern into account, we exclude elections where the margin (in the first round) between the first and the second exceeds

[6] The variables are (i) the perceived constraints due to inadequately educated workforce, (ii) the proportion of goods and services paid before the delivery; (iii) a dummy variable equal to one if the firm submitted an application to obtain a construction-related permit, and zero otherwise. See Léon (2015) for a discussion of the relevance of employed exclusion variables.
[7] See Table A3 for the list of elections.

40 percentage points in column (3). As indicated in Table A3, this low level of electoral competition applies to 18 of the 51 elections in our sample.

We complete our set of robustness tests by testing alternative measures of credit constraints. Here, we ask whether our results are confirmed by alternate measures of access to credit. In Panel A, our dummy equals one if the firm purchased fixed assets in the year before the survey. In Panel B, we create a dummy variable equal to one if a firm has obtained a loan, and zero otherwise. In Panel C, the dummy variable equals one if the firm reported that access to financing was not an obstacle or a minor obstacle to its current operations, and equals zero if a firm stated that access to financing was a major obstacle or a very severe obstacle to its current operations. The estimations with these alternative measures are reported in column (4) of Table 6.

We find confirmation in all robustness tests for our key finding that elections impair access to credit. Access to credit and credit demand are lower in election and pre-election years. Hence, the robustness tests confirm our main results, leading to findings that are consistent with the uncertainty channel.

Although our main estimations find no support for any change in credit constraints for post-election years, we observe limited evidence of a negative impact of post-election year on credit access and credit demand. This finding further corroborates our main conclusion for the uncertainty channel, because uncertainty on economic policies to be implemented in the year following elections can still occur and hamper credit demand (due to unstable coalitions or a post-electoral crisis). We observe a negative and significant impact of the election year on credit supply in some robustness tests. While this finding differs from our observation of no relation between elections and credit supply in the main estimations, the impact remains slight.

5 Extensions

Our main estimations have shown that access to credit is reduced in pre-election and election years by the impact of elections on borrower behavior. These estimations considered the *average* effect of elections on the full sample of firms and countries. Here, we question whether firm-level and country-level characteristics might mitigate or amplify the effect of elections on access to credit. In this section, we consider the influence of firm-level characteristics before investigating how country-level characteristics might affect the impact of elections on access to credit.

5.1 Influence of firm-level characteristics

We examine whether the impact of elections differs across firms by considering three firm characteristics, *firm size*, *firm age*, and whether the firm is *foreign owned*. We report these estimations in Table 7. In all estimations, we consider the baseline model with all firm-level and country-level control variables. Panels A, B, C display respectively the results for access to credit, credit demand, and credit supply.

Size and age are associated with the degree of transparency of firms. We expect large, old firms to be more transparent than small new firms, and thus enjoy easier access to credit. Indeed literature has shown that access to credit is particularly an obstacle for firm growth for opaque firms (e.g. Beck and Demirgüc-Kunt, 2006). It is therefore of interest to examine whether elections have a more detrimental effect on opaque firms than transparent firms. If this is observed, it means that elections amplify the challenge of accessing credit for the most credit-constrained firms.

From a theoretical perspective, elections can affect transparent and opaque firms through the uncertainty channel in different ways. We do not expect any difference between transparent and opaque firms for *credit demand* as all are affected by uncertainty in their loan requests. However, in terms of *credit supply*, opaque firms may suffer more from a credit crunch induced by heightened uncertainty. In periods of higher uncertainty, banks may prefer to lend to the most transparent firms to reduce their potential loan losses.

To test the effect of size and age, we split the sample of firms in two ways: small and large; and young and old. The cutoffs are the median values, 19 for number of employees and 10 years for firm age. We obtain three findings.

First, the negative impact of election and pre-election years on access to credit is observed for transparent and opaque firms. We observe no difference between small and large firms, or between young and old firms.

Second, we observe no difference in influence on credit demand: a negative influence is found for election and pre-election years for both small and large firms, and for young and old firms.

Third, firm opacity affects the impact of elections on credit supply. In both election years and post-election years, we observe a negative and significant impact on credit supply for small and young firms only. In other words, opaque firms suffer from a credit crunch during the year of the election and the following year. This result can be explained by the uncertainty channel in the sense that banks would be more cautious to lend to opaque (i.e. riskier) firms in uncertain times.

By documenting a greater deterioration of credit supply for opaque firms, these estimations show that elections can be particularly hard on the most credit-constrained firms.

Table 7. Influence of firm characteristics

Probit estimations are performed. The dependent variable is *Access* in Panel A, *Demand* in Panel B, and *Supply* in Panel C. Control variables are not displayed. The table reports marginal effects and associated standard errors in parentheses. *, **, *** denote an estimate significantly different from 0 at the 10%, 5%, or 1% level. Standard errors are adjusted for clustering at the country-year level. Definitions of all variables are reported in the Appendix.

	By size		By age		By ownership	
	Small	Large	Young	Old	Local	Foreign
			Panel A: Credit access			
Election	-0.040**	-0.053***	-0.053***	-0.046***	-0.051***	-0.060**
	(0.016)	(0.019)	(0.012)	(0.017)	(0.015)	(0.025)
Pre-election	-0.044**	-0.055**	-0.056**	-0.044**	-0.057***	0.053
	(0.021)	(0.023)	(0.026)	(0.019)	(0.019)	(0.037)
Post-election	-0.015	-0.031	-0.049**	-0.016	-0.026	-0.051
	(0.020)	(0.023)	(0.025)	(0.20)	(0.019)	(0.034)
Obs.	10,752	10,475	8,140	13,087	19748	1,460
			Panel B: Credit demand			
Election	-0.038**	-0.048***	-0.035*	-0.051***	-0.050***	-0.074***
	(0.017)	(0.019)	(0.019)	(0.016)	(0.015)	(0.026)
Pre-election	0.063***	-0.056**	-0.061**	-0.058***	-0.070***	0.017
	(0.022)	(0.024)	(0.027)	(0.021)	(0.020)	(0.038)
Post-election	-0.006	-0.037	-0.034	-0.025	-0.023	-0.087**
	(0.021)	(0.023)	(0.027)	(0.020)	(0.018)	(0.038)
Obs.	10,752	10,475	8,140	13,087	19,748	1,460
			Panel C: Credit supply			
Election	-0.034**	-0.010	-0.069***	-0.001	-0.012	-0.019
	(0.017)	(0.009)	(0.019)	(0.011)	(0.009)	(0.034)
Pre-election	0.010	-0.011	-0.015	0.002	0.001	0.064
	(0.025)	(0.014)	(0.027)	(0.013)	(0.014)	(0.044)
Post-election	-0.042*	0.000	-0.050**	-0.002	-0.024**	0.053
	(0.023)	(0.011)	(0.019)	(0.011)	(0.011)	(0.041)
Obs.	4,081	6,548	3,754	7,082	9,989	643

Foreign ownership can affect the impact of elections on access to credit. Foreign firms can rely on alternative sources of financing more easily than domestic firms. They can consequently be less affected by the conditions of domestic credit markets, and thus by the impact of elections on credit

supply. They can also partly immune to domestic economic conditions as their shareholders and final markets may be located abroad. This would weaken the role of uncertainty in their behavior. We therefore examine the effect of foreign ownership by splitting the sample into domestic and foreign firms. This information is provided in WBES database for each firm.

We observe that election years exert the same negative impact on domestic and foreign firms. During these years, access to credit and credit demand are reduced for both types of firms. However, the detrimental effect of pre-election years is observed solely for domestic firms. No reduction in credit demand is found for foreign firms in the year preceding elections. These findings support the view that uncertainty in election times is more likely to affect domestic firms than foreign firms.

5.2 The impact of political and financial systems

The characteristics of the country can alter the influence of elections on access to credit. In particular, the design of political and financial systems can have an impact by influencing the mechanisms through which uncertainty and manipulation can affect borrowers and lenders.

Since the number of observations per country is small, our empirical approach is an interaction model where the investigated country characteristic is interacted with the dummy *Election* for the election years.[8] By itself, the coefficient associated with the interaction term, and especially its statistical significance, is uninformative (Brambor et al., 2005). Instead of reporting tables, we use a graphical analysis that lets us capture the mitigating impact of the conditional variable (Greene, 2010). We draw a graph displaying the marginal effect of elections according to the characteristics of political and financial systems.

Our first country characteristic, the level of democracy, can influence the impact of elections on access to credit through the uncertainty channel or the manipulation channel. Regarding uncertainty, a greater degree of democracy should reduce the uncertainty surrounding the elections since risk of political violence should be lower. However, a more democratic country may experience greater uncertainty about the election outcome as the election is competitive. In other words, the degree of democracy may increase or reduce credit demand and credit supply in election times through the uncertainty channel. Regarding political manipulation, a greater degree of

[8] We also interact with pre-election dummy and post-election dummy. However, country-level characteristics do not influence the impact of pre- and post-election years (graphs available upon request).

democracy may reduce the ability for authorities to favor bank lending due to the fact there are more checks and balances on authorities in democratic regimes. We should thus observe an amplified impact of elections on credit supply in less democratic countries. The influence of the degree of democracy on the relation between elections and access to credit is therefore ambiguous.

We measure the level of democracy with the Polity IV index from the Polity project, commonly used in works on the economic impact of democracy (e.g. Delis et al., 2020). This index takes into account the presence of institutions through which citizens can participate to the political process. It codes political regimes by considering the competitiveness of political participation, the openness and competitiveness of executive recruitment, the constraints on the chief executive, and the regulation of participation. It ranges from 0 (no democracy) to 10 (full democracy). We display the results in Figure 1.

In Panel A, we show that the negative impact of election on access to credit is stronger in democratic countries. In other words, firms are less likely to get access to a loan during an election year when they operate in a more democratic country. The distinction between the behavior of borrowers (credit demand in Panel B) and lenders (credit supply in Panel C) provides an interesting insight into the general finding on access to credit.

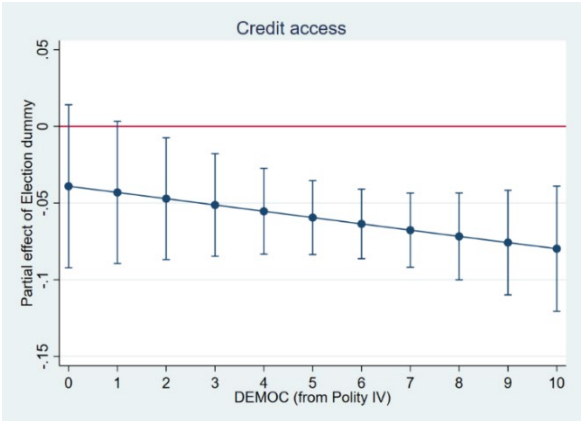
On the one hand, firms are more likely to ask for a loan during an election year when they operate in a more democratic country. This finding can be explained by lower uncertainty in more democratic countries because elections are less conflictual in terms of instability, in line with the uncertainty channel. This mechanism operates to encourage borrowers to seek a loan.

On the other hand, the degree of democracy determines the behavior banks adopt in election years. In highly democratic countries, banks tend to reduce their lending in election years in line with the uncertainty channel, i.e. they may be more concerned about the changes in economic policy in countries with more competitive elections. However, banks operating in autocratic countries tend to increase lending in election years. This can be explained by the manipulation channel. Authorities are more likely to manipulate banks to boost the credit supply in election years in autocratic countries due to the absence of checks and balances on the exercise of the regime's power.

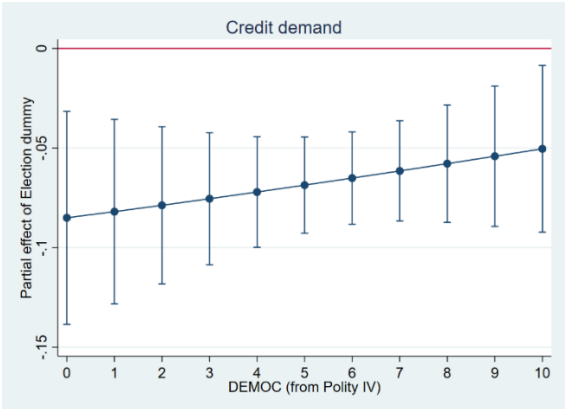
Figure 1. The impact of degree of democracy

Figure displays the marginal effect of election dummy according to the level of democracy (using Polity 4). The level of democracy ranges from 0 (undemocratic) to 10 (democratic). Blue dots represent the marginal effect for each level of democracy. Blue lines show the confidence interval (95%). The dependent variable is *Access* in Panel A, *Demand* in Panel B, and *Supply* in Panel C. Probit estimations are performed.

Panel A: Credit access



Panel B: Credit demand



Panel C: Credit supply

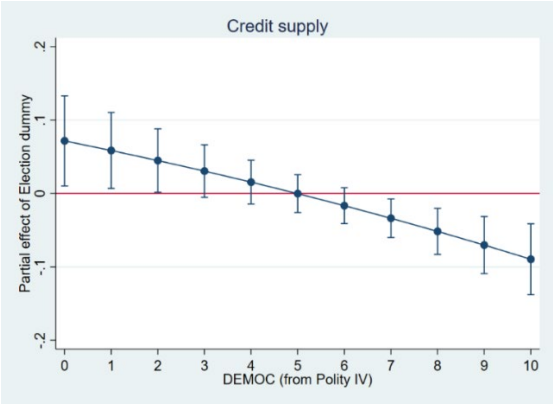
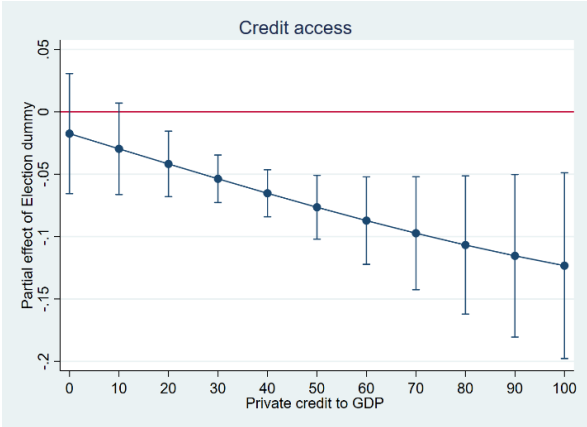


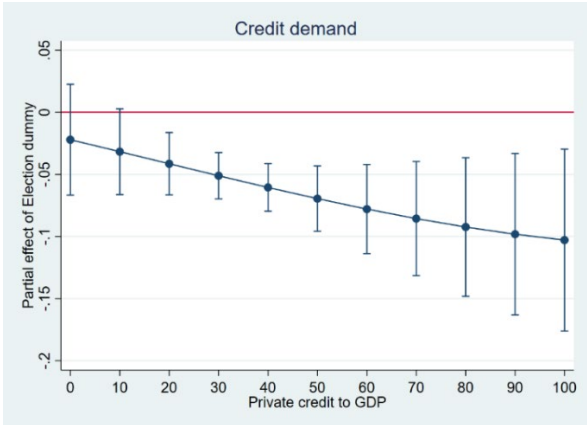
Figure 2. The impact of financial system size

Figure displays the marginal effect of election dummy according to the size of financial systems (private credit to GDP). Blue dots represent the marginal effect for each level of democracy. Blue lines show the confidence interval (95%). The dependent variable is *Access* in Panel A, *Demand* in Panel B, and *Supply* in Panel C. Probit estimations are performed.

Panel A: Credit access



Panel B: Credit demand



Panel C: Credit supply

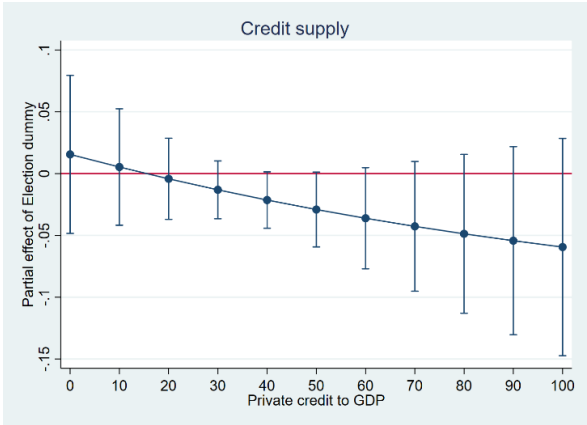
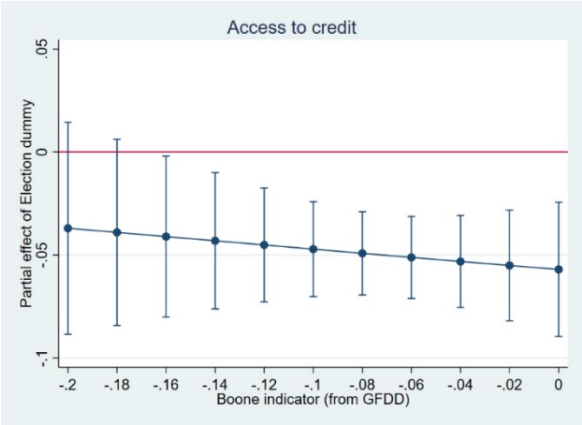


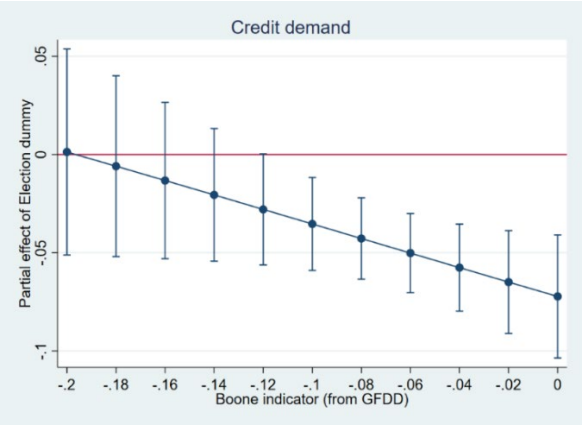
Figure 3. The impact of bank competition

Figure displays the marginal effect of election dummy according to the degree of competition, assessed by the Boone indicator (higher values indicate less competition). Blue dots represent the marginal effect for each level of democracy. Blue lines give the confidence interval (95%). The dependent variable is *Access* in Panel A, *Demand* in Panel B, and *Supply* in Panel C. Probit estimations are performed.

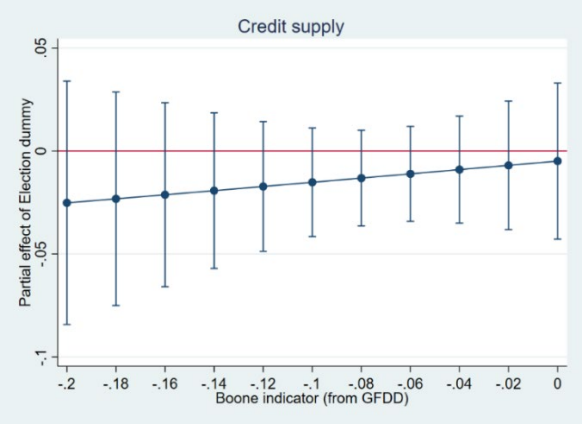
Panel A: Credit access



Panel B: Credit demand



Panel C: Credit supply



The fact that overall access to credit diminishes more during election years in more democratic countries means that the impact on the behavior of banks is stronger than the one on the behavior of firms.[9] In summary, we find evidence that the negative impact of elections on access to credit is stronger in more democratic countries.

We now consider the roles of the size of the financial system and the degree of bank competition.[10] The manipulation channel can be influenced by the characteristics of the financial system and assumes authorities motivate banks to boost their lending in election times. How the financial system is shaped could thus affect the ability of authorities to influence bank behavior.

A *large financial system* is associated with greater possibilities for firms to get access to funding. We thus predict that manipulation should be weaker in countries with larger financial systems. A *more competitive banking system* should restrict the possibilities for manipulation of bank lending. In combination, a large financial system and more competitive banking system should limit the possibilities for the authorities to influence bank behavior. For these reasons, we expect that the impact of elections on credit supply should be lower in countries with large financial systems and competitive banking systems.

We measure the size of the financial system with the ratio of domestic credit to the private sector to GDP (variable *Fin Development*), and the degree of bank competition with the Boone indicator (lower values indicate higher competition). Data for both indicators come from the Global Financial Development Database. Figure 2 displays the impact of the size of the financial system. Figure 3 presents the effect of the degree of bank competition.

We find evidence supporting the hypothesis that banking system characteristics influence the impact of elections on credit access. Banks are less likely to grant a loan during election years in countries with larger financial systems and more competitive banking systems. In such countries, the effect of elections is more detrimental on credit supply than in other countries.

Interestingly, access to credit in election years is lower in countries with larger financial systems. This accords with the expected impact of the size of the financial system on credit supply. However, although bank competition affects negatively credit supply in election years, it affects credit demand positively during election years and has a beneficial influence on access to credit. This finding on credit demand may reflect the fact that higher bank competition reduces

[9] In an unreported analysis, we investigate the degree of electoral competition on the relation between elections and access to credit. We obtain similar findings.
[10] Due to the lack of reliable data for all countries of our sample, we do not investigate the influence of market share of state-owned banks.

uncertainty. Borrowers may consider that economic policies to be implemented by authorities to be bounded by greater competition in the economy, leading to lower economic policy uncertainty in election times.

In summary, the characteristics of the financial system affect the impact of elections on access to credit.

6 Conclusion

This paper examined whether elections affect firms' access to credit. To achieve this objective, we performed a cross-country investigation on a large dataset of firms. Our key finding is that elections exert a detrimental influence on access to credit. We observe that firms are more credit-constrained in election and pre-election years. These results support the *uncertainty channel* according to which the occurrence of elections deteriorates access to credit by enhancing political uncertainty. We demonstrate that this effect takes place on the borrower side, i.e. electoral periods are associated with lower credit demand. In contrast, they do not affect overall credit supply.

Some firm and country characteristics influence the impact of elections on access to credit. We find that the occurrence of elections diminishes credit supply for opaque firms during election and post-election years. The effect of elections on access to credit is influenced by the features of political and financial systems. Elections impair access to credit in more democratic countries and in countries with larger financial systems and more competitive banking systems.

The take-away here is that electoral periods are accompanied with lower access to credit for firms. Our findings accord with the view that greater uncertainty can impair access to credit. Our research provides a major complement to the literature finding evidence that electoral periods are accompanied with a boost in lending influenced by the government. While this literature focuses on credit supply, we show that, when credit demand is taken into account, elections reduce rather than enhance firm access to credit.

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Appendix

Table A1. Definitions and sources of variables

Variable	Definition and source
	Dependent variables
Access	Dummy variable equal to one if a firm needing external funds applied for credit and was denied or refused to apply; zero otherwise. Source: WBES
Demand	Dummy variable equal to one if a firm needing external funds applied for credit; zero if the firm needing funds refused to apply. Source: WBES
Supply	Dummy variable equal to one if a firm applied for loans and received at least one line of credit; zero otherwise. Source: WBES
	Independent Variables
Firm-level variables	
Employees	Number of employees. Source: WBES
Age	Age of the firm. Source: WBES
Foreign owned	Dummy variable equal to one if at least 50 percent of a firm's ownership is held by foreigners; zero otherwise. Source: WBES
State owned	Dummy variable equal to one if at least 50 percent of a firm's ownership is held by the government; zero otherwise. Source: WBES
Partnership	Dummy variable equal to one if a firm is a partnership; zero otherwise. Source: WBES
Sole Proprietorship	Dummy variable equal to one if a firm is a sole proprietorship; zero otherwise. Source: WBES
Audited	Dummy variable equal to one if a firm's financial statements were checked and certified by an external auditor; zero otherwise. Source: WBES
Services	Dummy variable equal to one if the firm industry is services; zero otherwise. Source: WBES
Construction	Dummy variable equal to one if the firm industry is construction; zero otherwise. Source: WBES
Country level Variables	
GDP Growth	Growth rate in GDP. Source: WDI
Inflation	Rate of inflation. Source: WDI
Fin Development	Domestic credit to the private sector as a share of GDP. Source: GFDD
GDP per capita	Gross domestic product per capita. Source: WDI
Gov Exp	General government final consumption expenditures as a share of GDP. Source: WDI
Democracy	Democracy measure which ranges from 0 (no institutional democracy) to 10 (maximum level of institutional democracy). Source: Polity IV project.
Bank competition	Boone indicator is a measure of degree of competition, calculated as the elasticity of profits to marginal costs. Source: GFDD

Table A2. Sample

Country	Observations					Elections	
	Pre-El.	Election	Post-El.	Non El.	Total	# elec years	# elections
Armenia	313	77		20	410	1	2
Azerbaijan	363	9			372	1	2
Bolivia		381		189	570	1	1
Bosnia and Herzegovina		154	268	140	562	1	3
Burundi	19	207		98	324	1	2
Cyprus	44	40			84	1	1
Czech Republic	46	77	70	83	199	2	2
Côte d'Ivoire		175	39	447	661	1	1
Dominican Republic	101	15			116	1	1
Egypt	698	123	1,101	67	1291	1	2
Estonia	100	132	96	18	346	1	3
Ethiopia	553	266	142		961	2	2
Georgia	370	211	21		602	1	3
Ghana	10	248	280	402	940	1	1
Guinea		61			255	1	1
Israel	197	22			219	1	1
Italy	6	296	9		311	1	1
Kenya	315	440	121		876	2	2
Kosovo	90	74	91	98	263	1	2
Laos	192	123	126	153	594	1	3
Latvia	18	103	204		325	1	3
Madagascar	86	155	253	13	507	1	2
Malawi	198	127			325	1	1
Malta		8	43		55	1	1
Mauritania	52	48			302	1	1
Montenegro	23	69		79	232	1	2
Namibia	163	57	108		328	1	1
Nicaragua	72	92			164	1	1
North Macedonia	196	62	277	155	494	1	2
Pakistan	138	150	161		449	1	1
Peru	477	654	81		1212	1	1
Romania	282	300			582	1	2
Russia	1,110	903		1,534	3547	2	3
Rwanda		155		154	309	1	1
Serbia		485		166	651	2	2
Sierra Leone		116		106	222	1	1
Slovakia		56	159		215	1	2
Solomon Islands		33	10		43	1	1
Tajikistan	113	24		332	469	1	1
Tanzania		337		511	848	1	1
Turkey	138	1,351	234	298	2021	2	3
Uzbekistan		222		657	879	1	1
Yemen		119	13		132	1	1

Zambia		313	286	55	654	2	2
Obs.	6483	9070	4193	5775	24921	51	73

Table A3. List of elections

Country	Regime (from DPI)	Date of election				Margins btw
		Year	Month	Day	Calendar	1st and 2nd
Armenia	President	2008	2	19	Fixed	31.32
Azerbaijan	President	2008	10	15	Fixed	84.52
Bolivia	President	2005	12	18	Anticipated	25.15
Bosnia	Parliamentary	2018	10	7	Fixed	0.98
Burundi	President	2005	8	19	Fixed	88.8
Côte d'Ivoire	President	2015	10	25	Fixed	77.37
Cyprus	President	2018	1	28	Fixed	5.27
Czech Rep	Parliamentary	2012	10	19	Fixed	3.15
Czech Rep	Parliamentary	2013	10	25	Anticipated	1.8
Dominican Rep	President	2016	5	15	Fixed	26.76
Egypt	President	2012	6	16	Anticipated	1.12
Estonia	Assembly-Elected President	2007	3	4	Fixed	1.74
Ethiopia	Parliamentary	2010	5	23	Fixed	86.84
Ethiopia	Parliamentary	2015	5	24	Fixed	87.02
Georgia	President	2018	10	28	Fixed	0.9
Ghana	President	2012	12	7	Fixed	2.96
Guinea	President	2015	10	11	Fixed	26.41
Israel	Parliamentary	2013	1	22	Anticipated	9
Italy	Parliamentary	2018	3	4	Fixed	4.32
Kenya	President	2013	3	4	Fixed	6.76
Kenya	President	2017	10	26	Fixed	9.23
Kosovo	Parliamentary	2007	11	17	Fixed	11.7
Laos	Parliamentary	2011	4	30	Fixed	100
Latvia	Parliamentary	2018	10	6	Fixed	1.61
Madagascar	President	2013	10	25	Postponed	5.17
Malawi	President	2014	5	20	Fixed	8.6
Malta	Parliamentary	2017	6	3	Anticipated	11.5
Mauritania	President	2014	6	21	Fixed	73.22
Montenegro	Parliamentary	2012	10	14	Fixed	22.78
Namibia	President	2014	11	28	Fixed	81.76
Nicaragua	President	2016	11	6	Fixed	57.41
North Macedonia	Parliamentary	2011	6	5	Anticipated	6.41
Pakistan	Assembly-Elected President	2013	7	30	Fixed	69.74
Peru	President	2016	4	10	Fixed	18.81
Romania	Parliamentary	2012	12	9	Fixed	42.09
Russia	President	2008	3	2	Fixed	52.56
Russia	President	2018	3	18	Fixed	64.92
Rwanda	President	2010	8	9	Fixed	87.93
Serbia	Parliamentary	2007	1	21	Fixed	5.88
Serbia	Both	2012	5	6	Fixed	1.98
Sierra Leone	President	2007	8	11	Fixed	6
Slovakia	Parliamentary	2012	3	10	Anticipated	35.6
Solomon Islands	Parliamentary	2014	11	19	Fixed	2.94

Tajikistan	President	2013	11	6	Fixed	78.88
Tanzania	President	2005	12	14	Postponed	68.6
Turkey	Parliamentary	2007	7	22	Anticipated	25.81
Turkey	Parliamentary	2018	6	24	Anticipated	19.72
Uzbekistan	President	2007	12	23	Fixed	87.49
Yemen	President	2012	2	21	Anticipated	100
Zambia	President	2006	9	28	Fixed	13.61
Zambia	President	2011	9	20	Fixed	6.56

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