

Backscratching in Banks: Political Cycles in Bank Manager Appointments.

Online Appendix (not meant for publication)

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Review of existing literature on connections between politicians and firms

The demarcation between the public and private sector is often blurred and firms and politicians interact in variegated ways: the public sector defines the “rules of the game” for the private sector by shaping the regulatory framework and by drawing the boundaries of legal and legitimate business activities. In some cases, the public sector maintains even more direct connections with individual companies through ownership or politicians as firms’ board members. Ample evidence documents the economic consequences of direct forms of political connections for businesses. For example, firms connected to the Indonesian leader Suharto lost market value whenever rumors circulated about the leader’s health (Fisman, 2001), in the United States, firms benefit from abnormal stock returns when they nominate a politically-connected person to their boards (Goldman, Rocholl and So, 2009), and Russian firms increase their profitability when businesspeople win elected office (Szakonyi, 2018). More generally, we know that bankrupt firms are more likely to receive a bailout and the costs of equity are lower for companies with political linkages (Faccio, Masulis and McConnell, 2006; Boubakri et al., 2012). Political connections can be beneficial for firms by reducing economic and political uncertainty as politicians bring detailed knowledge and perhaps curry favors to businesses, e.g. by helping companies to win public procurement contracts or to obtain a bailout when firms run into trouble.

In light of the benefits of politician-firm linkages for businesses, it comes as no surprise that some *businesspeople actively seek political connections*, for instance, by inviting political office-holders to their boards (Goldman, Rocholl and So, 2009; Hillman, 2005), by donating to political campaigns (Mian, Sufi and Trebbi, 2010), or by running themselves for political office (Szakonyi, 2018; Gehlbach, Sonin and Zhuravskaya, 2010).

Yet, other studies find that in some cases *politicians seek access to firms* that might help them to pursue their political agenda (Shleifer and Vishny, 1994; Atkinson and Stiglitz, 1980). Some politically-connected firms show clear signs of politically-motivated business decisions. Publicly-owned banks in developing and developed countries increase lending before elections (Dinc, 2005;

Englmaier and Stowasser, 2017) and Hungarian firms with political officeholders on their boards do business more often with firms that have board members from the same party (Stark and Vedres, 2012). Furthermore, recent evidence demonstrates that politicians, in fact, benefit electorally from close ties to banks (Markgraf and Rosas, 2019). Political connections thus might reduce critical elements of uncertainty for businesses, but can expose them to political rent-seeking.

A growing body of research addresses the implications of political connections for firms and politics, but scholars resort to different definitions of “political connections”. Among the most obvious forms of connections between the public sector and firms are ownership links (*public ownership*). Public ownership is prevalent in various countries and scholars have demonstrated that the operations of publicly-owned banks follow political patterns using cross-country samples (La Porta, Lopez-de Silanes and Shleifer, 2002) and data from developing countries (Dinc, 2005) as well as developed countries, such as Italy or Germany (Sapienza, 2004; Englmaier and Stowasser, 2017). Other studies consider firms to be politically connected when the public sector is an important shareholder of a firm (*political shareholders*) (Faccio, 2006; Faccio, Masulis and McConnell, 2006; Bortolotti and Faccio, 2008; Boubakri et al., 2012). Faccio (2006), for instance, defines a firm as politically connected when the public sector has a vote share of at least ten per cent or when the top-officer is a minister or member of parliament. Finally, political connections can take the form of personal linkages between politicians and firms and some scholars consider businesses as politically tied when at least one board member holds political office (*political board member*) (Hillman, 2005; Stark and Vedres, 2012; Khwaja and Mian, 2005).

Hence, political connections are measured in different ways, mostly relying on criteria that are directly observable from a firm’s corporate governance structure, such as political ownership, shareholdership, or political board members. All these definitions aim to capture the same underlying characteristic, namely a situation when politics interferes in companies’ corporate governing bodies or operations. This paper does not contest those definitions, but adds another facet of private-public connections. It argues that even firms without formal ties to politicians sometimes exhibit political patterns in their business activities. These linkages are much more difficult to grasp systematically and usually rely on anecdotal evidence. Hence, existing measures do not capture the entire picture of firm-politics ties.

Structure and history of Spanish savings banks

After the fall of the Franco regime, *Cajas* were freed from direct political control, the regional principle that had constrained the business areas of savings banks to small geographical units was lifted, and the banks had no longer formal owners (Hallerberg and Markgraf, 2018). Authority over the savings banks shifted from the central government to regional governments, which were one among several stakeholders, including depositors, employees, and founders that decided jointly on the strategic positioning of the Spanish savings banks (*LORCA law*, 1985). Table A1 shows the number of savings banks in each region and the average voting rights of the regional governments in the 17 autonomous regions over time.

Spanish savings banks expanded sweepingly after a period of considerable consolidation (the number of Spanish savings banks fell from initially 90 to 46 banks by 1990) and Spanish savings banks’ business models increasingly resembled those of commercial banks. They became crucial economic actors in Spain’s banking sector: their branches doubled in numbers between 1988 and 2008 while commercial banks reduced the branch network, and their overall credit volume exceeded that of the commercial banking sector (Sagarra, Mar-Molinero and García-Cestona, 2015). Yet, when the Spanish economy tumbled during the global financial crisis in 2009, *Cajas* ran into severe trouble and ultimately all but two savings banks were either privatized or liquidated. Empirical studies have demonstrated that risk-taking and losses were pronounced in *Cajas* with chairpersons that lacked relevant banking expertise and when regional governments had a higher vote share (Cunat and Garicano, 2010; Illueca, Norden and Udell, 2014). *Cajas* were widely assumed to have close ties to Spanish regional governments: newspapers regularly report that *Cajas* helped politicians financing political prestige projects. The various unfinished infrastructure projects,

Table A1: Number of *Cajas* in 2010 by region and average voting power of autonomous regions in savings banks; information about the voting power of the public sector comes from [Fernández, Fonseca and González \(2002\)](#) and was completed for more recent years.

	Number <i>Cajas</i> (2010)	Legal Voting Rights Public Sector (per cent)
Andalusia	6	40.00 (1985); 57.78 (1986–2002); 50.00 (2003–2010)
Aragon	2	40.00 (1985–1990); 42.00 (1991–2010)
Asturias	1	40.00 (1985–1987); 75.00 (1988–2002); 50.00 (2003–2010)
Balearic Islands	2	40.00 (1985–1989); 45.00
Basque Country	3	40.00 (1985–1990); 53.3 (1991–2002); 50.00 (2003–2010)
Canary Islands	2	40.00 (1985–1989); 59.95 (1990–2002); 50.00 (2003–2010)
Cantabria	1	40.00 (1985–1989); 66.00 (1990–2002); 50.00 (2003–2010)
Castile and Leon	6	40.00 (1985–1989); 42.08 (1990–2010)
Castile-La Mancha	2	40.00 (1985–1996); 70.05 (1997–2002); 50.00 (2003–2010)
Catalonia	10	31.90 (1985–2010)
Extremadura	2	40.00 (1985–1993); 41.10 (1994–2010)
Galicia	2	31.90 (1985–2010)
La Rioja	1	40.00 (1985–1987); 64.4 (1988–2002); 50.00 (2003–2010)
Madrid	1	40.00 (1985–1991); 55.00 (1992–2002); 50.00 (2003–2010)
Murcia	1	40.00 (1985–1987); 63.00 (1988–2002); 50.00 (2003–2010)
Navarre	1	40.00 (1985–1986); 58.30 (1987–2002); 50.00 (2003–2010)
Valencia	3	40.00 (1985–1989); 57.70 (1990–2002); 50.00 (2003–2010)

such as the idle airport at Ciudad Real, still shape large areas in Spain and illustrate the past political excesses financed by *Cajas* ([Harter, 2012](#)). Unfit bank chairmen were appointed thanks to their partisan ties. The case of Miguel Blesa is illustrative in this regard: when the Spanish conservative party (Popular Party) won the regional elections in Madrid in 1995 and replaced a socialist government, the career prospects of Miguel Blesa, a close friend and loyal supporter of the conservative party, brightened overnight. Only one year after the election, he left his position as a tax lawyer and became the new chairman of the regional Spanish savings bank, *Caja Madrid*. And while the conservative party consolidated its power as Madrid’s governing party over the following decade, Blesa oversaw the bank’s astonishing expansion and financing of several political prestige projects that eventually brought the bank down in 2010 ([De Barron, 2013](#)).

Nonetheless, systematic evidence about the channels through which politicians wielded control over *Cajas* is missing. In fact, Spain ranks low in existing databases on political connections: according to [La Porta, Lopez-de Silanes and Shleifer \(2002\)](#), only two per cent of bank assets were controlled by the public sector in 1995; political shareholders are rare in Spanish firms and only 1.5 per cent of firms are subject to significant political shareholdership ([Faccio, 2006](#)); and [Bortolotti and Faccio \(2008\)](#) highlight that the public sector successfully transferred control over privatized state-owned enterprises to private actors.

Table A2: Years of Federal and Regional Elections, 1980–2010

	Election Years
Andalusia	1982, 1986, 1990, 1994, 1996, 2000, 2004, 2008
Aragon	1983, 1987, 1991, 1995, 1999, 2003, 2007
Asturias	1983, 1987, 1991, 1995, 1999, 2003, 2007
Balearic Islands	1983, 1987, 1991, 1995, 1999, 2003, 2007
Canary Islands	1983, 1987, 1991, 1995, 1999, 2003, 2007
Cantabria	1983, 1987, 1991, 1995, 1999, 2003, 2007
Castile-La Mancha	1983, 1987, 1991, 1995, 1999, 2003, 2007
Castile and Leon	1983, 1987, 1991, 1995, 1999, 2003, 2007
Catalonia	1980, 1984, 1988, 1992, 1995, 1999, 2003, 2006, 2010
Extremadura	1983, 1987, 1991, 1995, 1999, 2003, 2007
Galicia	1981, 1985, 1989, 1993, 1997, 2001, 2005, 2009
La Rioja	1983, 1987, 1991, 1995, 1999, 2003, 2007
Madrid	1983, 1987, 1991, 1995, 1999, 2003, 2007
Murcia	1983, 1987, 1991, 1995, 1999, 2003, 2007
Navarre	1983, 1987, 1991, 1995, 1999, 2003, 2007
Basque Country	1980, 1984, 1986, 1990, 1994, 1998, 2001, 2005, 2009
Valencia	1983, 1987, 1991, 1995, 1999, 2003, 2007
General Election	1982, 1986, 1989, 1993, 1996, 2000, 2004, 2008

Theoretical contribution: measuring firm-politics ties using timing of corporate appointments

This paper identifies connections between firms and the public sector focusing on the timing of corporate appointments. Controlling appointments of important political and bureaucratic positions can be politically beneficial and can help incumbents winning democratic elections (Camerlo and Pérez-Liñán, 2015; Quiroz Flores and Smith, 2011), ensuring autocratic regime survival (Huber and Martínez-Gallardo, 2008), and sidelining political competitors (Baerg, Gray and Willis, 2018). Yet, appointment patterns in firms can also be informative about politics-business linkages and allow us to identify political connections in cases when those ties are not readily obvious from firms' ownership, shareholder structure, or the composition of their Board of Directors. I argue that political elections constitute decisive moments for political connections. Earle and Gehlbach (2015), for example, show that political turnovers redistribute political connections helping some companies while harming others. I speculate that political elections, and especially political turnovers, affect the corporate governing bodies of politically-connected firms and expect that board members are more likely to be replaced after political elections. Thus, political cycles in firms' board appointments provide strong evidence for close firm-politics ties. Yet, political elections are not only relevant for politically-connected firms when political incumbents lose office. Even if incumbent governments are reelected, elections regularly lead to changes in the political personnel that might affect bank managers' survival. Two mechanisms might cause corporate turnovers in politically-linked enterprises after elections: on the one hand, newly elected governments might seek to appoint a politically-aligned manager to companies' boards in order to exert control over firms' activities; on the other, firms themselves might react to changes in the political environment and strategically appoint a new manager with more valuable political connections in order to reduce uncertainty.

To address the question about the drivers behind corporate turnovers—i.e. whether politicians try to sway appointment decisions or if firms react to changes in the political environment—, I argue that political veto players attenuate the effect of elections on managerial turnover. Veto players and coalition partners can tie policymakers' hands (e.g. Budge, 1985; Keefer and Stasavage, 2002). Thus, if, in fact, politicians drive politically timed appointments of bank managers,

turnover risk of bank managers is expected to be lower under newly formed coalition governments compared to newly incoming single-party governments; yet, if appointments are driven by firms that strategically appoint chairs, the composition of the government should not matter for appointments after political elections.

Additional information on “Data & Empirical Design”

Summary Statistics

Table A3: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Max
Post Election	1,259	0.52	0.50	0	1
Government Change	1,259	0.35	0.48	0	1
Years in Government	1,259	8.28	6.32	1	28
Coalition	1,259	0.32	0.47	0	1
Return on Assets	1,259	0.90	0.48	-2.32	3.67
Party:					
Conservative	1,259	0.31	0.46	0	1
Socialist	1,259	0.35	0.48	0	1
Other	1,259	0.33	0.47	0	1
Public-Sector Vote Share	1,259	0.45	0.11	0.31	0.75
Tenure Chairman	1,259	4.51	3.91	1	20
Tenure CEO	1,259	5.71	4.86	1	24

The dataset includes overall 2,518 observations with 1,259 person-year observations for chairpersons and CEOs, respectively. Table A3 shows that the average survival time of a chairperson is about 4.5 years with some individuals staying in their position for almost the entire period of observation (20 years); on average, CEOs stay in office considerably longer than chairmen (around 5 years and 9 months). Around one out of three elections led to a change in the composition of the regional government, governments stay in power on average slightly longer than eight years (two terms), and 32 per cent of governments consisted of a coalition of at least two parties (19 per cent of governments were single-party minority governments).

Handling of bank mergers

Bank mergers usually include a healthier bank and (at least) one struggling bank, and while the manager of the struggling bank is replaced after the merger, the chairperson of the healthier bank continues to serve on the board of the successor bank. I harness the information about the survival in office of managers after bank mergers in order to distinguish between healthy and struggling banks. Consequently, I code the survival of bank chairpersons as right-censored when they continue to serve on the board of the successor bank; when bank managers are removed from the board after the merger, I code them as having experienced an event. The results are robust to different codings of mergers. I present the regression results when—regardless of the health of the merged bank—(a) all bank managers in a merged bank are coded as right-censored, (b) all are coded as having experienced an event, and (c) when the analysis is restricted to a sample of 32 banks that were never involved in a merger (all in Table A11).

Model Selection and Specification

Proportional Hazards: For the estimation of unbiased non-parametric survival models, the proportional hazard (PH) assumption must be fulfilled, i.e. the effect of any covariate must be proportional over time. A violation of the PH assumption biases estimates and reduces their efficiency. The literature suggests to address those concerns by interacting the offending variable with a measure of time (Box-Steffensmeier and Zorn, 2001; Keele, 2010; Licht, 2011; Jin and Boehmke, 2017). Goodness-of-fit tests should determine the functional form of the duration measure.

The Grambsch-Therneau global test shows that non-proportional hazards are a serious concern in the model and the Harrel’s rho test, which tests the PH assumption for all covariates, indicates that a number of covariates violate the PH assumption (Table A4). I interact the offending variable with a measure of duration and select the parametric form of the time variable based on the best performing model. After correcting for non-proportional hazards, the Grampsch-Therneau and Harrel’s rho test do not indicate violations of the PH assumption. In addition, I inspected the Schoenfeld residuals, as suggested by Box-Steffensmeier and Jones (2004), and none of the residuals of the variables display irregularities that indicate non-proportional hazards.

Table A4: Test of Proportional Hazard Assumption

	Harrel’s rho	chisq	p
Years in Government	0.11	7.64	0.01
Coalition	0.05	1.18	0.28
Post Election	-0.04	0.53	0.47
Public Sector Vote Share	0.03	1.44	0.23
Return on Assets _{t-1}	-0.01	0.05	0.83
Party: Other	-0.02	0.72	0.40
Party: Socialist	-0.06	3.79	0.05
Grambsch-Therneau Global Test		14.40	0.05

Unmodeled Heterogeneity: To account for unobserved unmodeled heterogeneity across banks, I include frailties at the bank level. Compared to stratified models, which resemble fixed-effects models, frailty models allow the estimation of effects for covariates that are constant within a given cluster. This, however, comes at the cost of a less flexible model: frailty models require that we impose a certain distribution on the baseline hazard (McGilchrist and Aisbett, 1991; Therneau, Grambsch and Pankratz, 2003). Goodness-of-fit tests for different distributions of the frailty models show that hazards with a Gaussian distribution perform best.

Tied Events: To deal with the problem of tied events I use the Efron method (Efron, 1977), which yields results that are very similar to other methods that address tied events (Breslow and exact method) and is computationally more efficient.

Censoring: Incomplete information about the survival history of an individual poses a challenge in event history analyses and might yield biased results (“censoring”). To obtain unbiased estimates from survival models, censoring needs to be non-informative about the hazard and randomly distributed. “Left-censoring” describes observations without full information about the survival history because the starting time of an individual is outside the observation period; “right-censoring” refers to incomplete information due to lack of information about the termination time of an individual. Neither type of censoring poses a concern in the empirical analyses of this paper: the starting date of this study marks the beginning of the *Cajas* sector in its modern form. It coincides with fundamental regulatory reforms and changes in the corporate governance structure of the savings banks. Left-censoring is therefore a minor issue as the entire system of bank governance underwent far-reaching reform with new chairmen and CEOs taking over around 1985. The end date, 2010, was chosen for obvious reasons: the Spanish savings banking sector was strongly affected by the consequences of the global financial crisis and eventually almost all *Cajas* were wounded down or privatized in 2011. As explained above, CEOs and chairmen in merged banks were coded as right-censored or experiencing an event depending on the status of their bank

in the respective merger (financially sound or struggling bank).

Additional empirical analyses

The findings are corroborated by a set of additional empirical analyses. These analyses address concerns about alternative mechanisms that may drive the observed patterns, test the robustness of the findings to alternative model and sample specifications, and explore potential effect heterogeneity.

Alternative mechanisms

As discussed in the paper, the observed pattern of corporate turnovers that are associated with political events rests on the assumptions that (a) corporate turnovers do not affect the timing and result of political elections (reversed causality) and (b) that no unmeasured variable jointly causes corporate turnovers as well as the timing and outcome of political elections (omitted variable).

The timing of political elections is arguably exogenous to corporate turnovers and is not affected by other factors omitted in the model as their timing is predetermined by the electoral calendar. Nevertheless, sometimes governments collapse in the midst of their political term and early elections are called. If the reasons for the government failure are associated with corporate turnovers (e.g. scandals in the banking sector or a general economic downturn), this would potentially undermine the validity of the empirical design. I therefore identify irregular regional elections in the sample and re-run the empirical models to test if the findings hold when focusing on elections for which the dates were truly exogenous.¹² The findings presented in Table A5 demonstrate that the results are robust to excluding early and repeated elections.

For the other political variable of interest, *Years in Government*, the exogeneity and no-omitted-variable assumptions require more justification. It is conceivable that a third variable, such as a macroeconomic downturn, simultaneously affects bank performance decreasing bank chairpersons' survival time as well as governments' approval ratings. In a similar vein, a bank scandal might spill over to the political realm undermining support for the government. Furthermore, one could argue that it is not direct political influence over bank appointments that drives the political patterns in corporate appointments and that, instead, changes in the government are associated with far-reaching changes in the bureaucracy including the bank regulatory and supervisory authority (in the case of Spain, the Bank of Spain) to which banks react. To address those concerns, I run three placebo tests. First, I replicate the analysis for the subsample of CEOs. Political connections are particularly common in chair positions and are much less frequent for CEO positions because CEO positions are subject to stricter quality requirements. In the case of Spanish *Cajas*, regional governments could influence appointment decisions of the Board through their representation in banks' General-Assemblies, but not those of bank CEOs (see above). Economic downturns, poor bank performance, and bank scandals should, however, affect both, chairperson and CEO. Thus, if, indeed, politicians sway appointment decisions and corporate turnovers are not driven by economic downturns, bank scandals, or poor bank performance, we should not expect to find a similar pattern for the subsample of CEOs. Table A6 presents results for the same statistical models as in Table 1 using the subsample of bank CEOs. Model 1 and 2 in Table A6 show no relationship between the political variables and bank CEOs' time in office. The performance of a bank is negatively correlated with CEOs' term duration, but imprecisely estimated in most empirical specifications.¹³ Similarly, political dynamics after elections with and without a change in government do not affect the hazard of *Cajas*' CEOs (Table A6, Columns 3 and 4).

¹²Overall, five early elections were called (Catalunya 1995 & 2006; Basque County 1986 & 2001; Andalusia 1996) and one repeated election occurred in Madrid (2003).

¹³While it is surprising that bank performance is only weakly related to CEO turnover in Spanish *Cajas*, the finding

Table A5: Cox PH Model excluding irregular elections

	Raw Model	Full Model 1	Full Model 2
Years in Government	-0.06*** (0.01)	-0.06*** (0.02)	-0.06*** (0.02)
Coalition		-0.21 (0.19)	-0.16 (0.19)
Post Election			0.43*** (0.15)
Public Sector Vote Share		0.25 (0.89)	0.39 (0.84)
Return on Assets _{t-1}		0.06 (0.15)	0.03 (0.15)
Party: Other		-0.22 (0.34)	-0.28 (0.34)
Party: Socialist		0.69** (0.29)	0.88*** (0.28)
AIC	2599.53	1983.26	1985.46
Num. events	212	212	212
Num. obs.	1115	1115	1115

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A6: Cox PH Model, Bank CEOs only

	Full Model 1	Full Model 2	Government Change	
			Yes	No
Years in Government	-0.02 (0.02)	-0.03 (0.02)		
Coalition	-0.00 (0.22)	-0.05 (0.22)	0.45 (0.50)	0.08 (0.44)
Post Election		-0.04 (0.17)	0.15 (0.53)	0.03 (0.23)
Post Election * Coalition			-0.18 (0.63)	-0.07 (0.59)
Public Sector Vote Share	0.84 (1.15)	0.57 (1.15)	2.54 (1.59)	-1.49 (1.38)
Return on Assets _{t-1}	-0.25 (0.18)	-0.22 (0.19)	-0.60* (0.32)	-0.23 (0.22)
Party: Other	0.50 (0.42)	0.52 (0.43)	0.71 (0.56)	-0.61 (0.56)
Party: Socialist	0.57 (0.38)	0.58 (0.40)	-0.69 (0.74)	1.68*** (0.48)
AIC	1541.61	1484.52	439.48	894.22
Num. events	165	165	59	106
Num. obs.	1259	1259	445	814

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A7: Placebo Test using Federal Elections

	Chairpersons	CEOs
Years in Government	0.03 (0.02)	0.03 (0.03)
Post General Election	-0.03 (0.14)	0.11 (0.17)
Public Sector Vote Share	1.32 (0.83)	1.76 (1.12)
Return on Assets _{t-1}	0.11 (0.14)	-0.22 (0.18)
Party: Socialist	0.74** (0.29)	0.59 (0.36)
AIC	2236.76	1544.74
Num. events	234	165
Num. obs.	1259	1259

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Furthermore, I use the dates of the federal elections in Spain (instead of regional election dates) as a placebo treatment to show that, indeed, regional politics mattered for the term duration of bank managers. I explained in the paper why political events at the regional political level – and not the Spanish central government – are expected to matter for Spanish savings banks. Yet, it is conceivable that the observed pattern is not driven by politicians who use their influence to appoint allies to bank boards, but rather by banks that react to changes the regulatory environment; after all, political elections are oftentimes accompanied by changes in the regulatory bodies. In Spain, banks are regulated and supervised at the national level by the Bank of Spain. I exploit the fact that regional elections do not always take place at the same time as federal elections and that regional election dates are not harmonized across regions (see Table A2), and use the election dates of general elections as a placebo (Table A7). Column 1 in Table A7 shows that federal elections do not increase the turnover risk for chairpersons. The hazard of chairpersons is not affected by federal election dates or changes in federal governments. Similar to the relationship between political factors at the regional level and CEO survival in *Cajas*, political events at the federal level do not explain variation in term length of CEOs in Spanish savings banks (Table A7, Column 2). The placebo analysis shows that federal elections are largely unrelated to turnover patterns in *Cajas*' governing bodies which corroborates the claim that regional politics, not events at other political levels, mattered for chairpersons' hazard.

In addition, I collected an additional sample for 16 commercial banks to replicate the models for a sample of banks for which political connections are unlikely. The paper focuses on the arguably politically-influenced Spanish savings banks, but does not ensure that the effect of political events is, indeed, constrained to *Cajas*. It is conceivable that political elections are disruptive events not only for publicly-influenced savings banks, but for the entire banking sector (e.g., through regulatory changes). This would call into question the proposed mechanism that argues that politicians use their power in the banks' General-Assembly to sway corporate appointments. I therefore collected another novel historical dataset on the composition of the governing bodies of the largest commercial banks in Spain for the period from 1985–2010 in order to ensure that the observed pattern is not spurious.¹⁴ The banks are, again, matched with the political variables

is in line with earlier studies that found an only weak connections between *Cajas*' performance and replacements of CEOs (e.g. Crespi, García-Cestona and Salas, 2004).

¹⁴The banks include Banesto, Banco de Santander, BBVA, Banco de Bilbao, Banco Central, Banco Central Hispano,

Table A8: Cox PH Model using 16 commercial banks as a placebo

	Raw Model	Full Model 1	Full Model 2
Years in Government	0.05 (0.04)	0.03 (0.06)	0.02 (0.06)
Post Election			-0.40 (0.47)
Coalition		-0.22 (0.92)	-0.29 (0.89)
Return on Assets _{t-1}		-0.15** (0.07)	-0.15** (0.07)
Party: Other		2.35 (1.70)	2.39 (1.66)
Party: Socialist		3.44* (1.80)	3.51** (1.77)
AIC	208.88	132.47	134.60
Num. events	24	24	24
Num. obs.	176	176	176

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

based on the location of the bank headquarters. Overall, the dataset comprises 16 banks, 176 observations, and 24 events. Table A8 presents the results from running the same models as for the main specification in the paper (Table 1), but excluding the variable for the vote share of the public sector as the public sector had no official voting power in commercial banks (findings are robust to the inclusion of the variable). The coefficients for the political variables of main interest (*Years in Government* and *Post Election*) are imprecisely measured, fail to reject the null hypothesis, and are signed opposite to what theory would predict. Instead and in contrast to the models using samples of *Cajas* chairpersons and CEOs, bank performance (*Return on Assets*) is a strong predictor for chairpersons' turnover in commercial banks: better performing banks are less likely to oust the chairperson (a one per cent increase in RoA is associated with a 14 per cent lower turnover risk for chairpersons in commercial banks). This demonstrates that in commercial banks are subject to market pressures and turnover is related to the performance of the bank (this is in line with a broad literature, .e.g., [Weisbach, 1988](#)). I thank one anonymous reviewer for the suggestion to include this placebo test.

Alternative specifications

I further corroborate the results by testing whether the findings hold in alternative model and sample specifications. Most importantly, I use an ordinary least squares estimation with fixed effects. Moreover, I deploy an alternative specification for the variable *Post Election*, present findings from different codings of bank mergers, exclude merged banks from the sample, and restrict the time period in order to rule out that the patterns are driven by particularly volatile time periods.

Duration models are not the only way to test the research question. Another straightforward way are ordinary least square models with region and year fixed effects where the binary *event*

Banco Santander Central Hispano, Banco de Madrid, BBV, Banco Exterior de Espana, Banco Hispano Americano, Banco Popular Espanol, Banco de Vizcaya, Banco Zaragozano, Banco Comercial Transatlantico, Banco General, Banco Atlantico. To select the largest banks, I used information on banks' total assets in 1985 in order to avoid survivorship bias.

variable is regressed on a number of predictors and year and region fixed effects are included. The year and region fixed effects net out any time-invariant characteristics as well as general time trends that might affect chairpersons' turnover probability. Following the model specification from [Akhmedov and Zhuravskaya \(2004\)](#), I estimate four different models with varying fixed-effects structures that are presented in Table A9. Model 1 does not include any fixed effects and presents a naive OLS model; Model 2 adds fixed effects for the 17 regions; Model 3 adds year fixed effects instead of region fixed effects; and Model 4–5 add region and year fixed effects representing the equation used by [Akhmedov and Zhuravskaya \(2004\)](#). The results are qualitatively similar to the ones from the survival model in Table 1, but the magnitude of the coefficients, and hence the statistical significance, varies across models. Model 3, 4, and 5 show that the effect disappears once year fixed effects are included in the model indicating that time trends are related to the turnover patterns in *Cajas*. A closer look at the years that cause the effect to disappear reveals that a number of “bad years” in the 1980s when many banks were merged (bank-level attrition) are responsible. I therefore look at merged banks in more detail. I exclude merged banks from the sample in Model 6 and 7 and recover the main finding that chairpersons' turnover probability decreases as governments stay longer in power. Furthermore, in Table A10 below I look at the determinants of bank mergers and find that they do not follow political patterns, but are driven by financial fundamentals. I thank one anonymous reviewer for pushing me to run an OLS model with fixed effects which helped me to further explore the scope and limitations of political control over bank appointment in *Cajas*.

Table A9: OLS Model with Region- and Year-Fixed Effects

	Naive Model all banks (1)	Region FE all banks (2)	Time FE all banks (3)	Region & Time FE all banks (4)	Region & Time FE all banks (5)	Region & Time F E excl. merged banks (6)	Region & Time FE excl. merged banks (7)
Years in Government	-0.01*** (0.002)	-0.01** (0.002)	-0.002 (0.002)	-0.002 (0.003)	-0.002 (0.003)	-0.005*** (0.002)	-0.01*** (0.002)
Coalition	-0.02 (0.03)	-0.04 (0.04)	0.03 (0.03)		0.003 (0.03)		-0.01 (0.03)
Post Election	0.05** (0.02)	0.04 (0.03)	0.03 (0.03)		0.03 (0.03)		0.01 (0.03)
Public Sector Vote Share	0.02 (0.12)	-0.18 (0.33)	0.21* (0.12)		-0.04 (0.33)		0.04 (0.28)
Return on Assets _{t-1}	-0.004 (0.02)	-0.005 (0.03)	-0.05** (0.02)		-0.05* (0.02)		-0.05 (0.04)
Party: Other	-0.02 (0.03)	-0.05 (0.04)	-0.03 (0.03)		-0.07 (0.06)		-0.09* (0.05)
Party: Socialist	0.03 (0.03)	0.08 (0.06)	-0.003 (0.03)		0.03 (0.06)		0.02 (0.05)
Region FE	no	yes	no	yes	yes	yes	yes
Year FE	no	no	yes	yes	yes	yes	yes
Observations	1,259	1,259	1,259	1,259	1,259	1,015	1,015

Note:

*p<0.1; **p<0.05; ***p<0.01

The analysis in Table A9 indicates that bank mergers might not be affected by the political variables included in the main specification. I explore this intuition more carefully by regressing mergers of *Cajas* on the covariates in Table A10. The results demonstrate that, indeed, bank mergers are mainly driven by the profitability of banks (as well as negatively related to the public-sector vote share although not statistically significant at conventional levels); political events, in contrast, are not related to merger activities. This provides insight into the *limits* of political control over savings banks: while politicians can appoint political allies to bank boards in good times, they cannot resist market forces when banks run into trouble and need to be liquidated or merged.

Table A10: Explaining mergers in Spanish savings banks

	Bank Merger
Years in Government	-0.004 (0.01)
Coalition	0.001 (0.05)
Post Election	0.01 (0.02)
Public Sector Vote Share	-0.53 (0.45)
Return on Assets _{t-1}	-0.09** (0.03)
Party: Other	0.02 (0.10)
Party: Socialist	0.12 (0.07)
Observations	1,259
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01

Next, I explore whether the findings are robust to alternative codings of bank mergers. To code merger-related turnovers of bank chairpersons, I exploit information about whether bank chairpersons serve as chairs at the successor bank after mergers or not in order to distinguish between acquiring (bank chairperson continues to serve at successor bank) and acquired bank (bank chairperson loses seat). I code chairpersons as being right-censored when they come from an acquiring bank and as having experienced an event when their bank is acquired. In Table A11, I treat all merger-related turnovers as right-censored (column 1) and all merger-related turnovers as an event in chairpersons' careers (column 2). The main findings are largely robust to different codings of bank mergers. Column 3 shows that the findings are robust to restricting the sample to overall 32 banks that were never merged and never acquired another savings bank over the entire period of observation.

Furthermore, one might be concerned that the pattern is driven by particularly volatile periods of bank consolidation and economic crises. In those times, bank managers are sacked at much higher rates. I address this concern by replicating the main specification using a restricted sample

Table A11: Robustness of findings to different codings of bank mergers

	All right-censored	All event	Banks never involved in merger
Years in Government	−0.05*** (0.02)	−0.06*** (0.02)	−0.06*** (0.02)
Post Election	0.31** (0.15)	0.17 (0.14)	0.68*** (0.22)
Coalition	−0.21 (0.19)	−0.19 (0.18)	−0.22 (0.26)
Public Sector Vote Share	0.05 (0.91)	−0.60 (0.90)	1.12 (1.36)
Return on Assets _{<i>t</i>−1}	0.02 (0.15)	0.12 (0.14)	0.25 (0.24)
Party: Other	−0.15 (0.33)	−0.07 (0.31)	−0.81 (0.50)
Party: Socialist	0.56* (0.30)	0.94*** (0.28)	0.33 (0.45)
AIC	1988.57	2365.86	881.16
Num. events	208	248	111
Num. obs.	1259	1259	769

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

for observations between 1991–2007 that excludes observations during the global financial crisis (post–2007) and years when consolidation in the Spanish savings bank sector was severe (1985–1990). Table A12 illustrates that the conclusions about the effect of political elections on bank chairpersons’ turnover remain unchanged.

Finally, I use separate year dummies for the electoral cycle as an alternative way to measure the effect of post-election periods to demonstrate that the effect is not driven by a single year in the electoral cycle and that the hazard decreases smoothly over the cycle. The post-election dummy used in the main analysis eases interpretation of its effect on bank managers’ duration in office, but comes at the cost of making strong assumptions about the years that matter for the termination of bank managers. To ensure that the findings are not sensitive to the coding of the variable, I instead use a dummy for each year in the four-year election cycle. In that way, I test whether the effect of elections on the hazard ratio—as predicted—smoothly shrinks over the election cycle from t to $t + 3$ (Table A13). Table A13 shows that the hazard for bank chairpersons decreases over the four-year election cycle. The risk of being replaced is highest in the election year (baseline hazard) and 28–35 per cent lower in year 3 and 4 of the election cycle (Table A13, Model 1). Hence, the findings presented in the Results section are not sensitive to the specification of the post-election dummy and the conclusions about the relationship between election cycles and bank managers’ term duration remain valid.

Effect heterogeneity

The following empirical analyses explore effect heterogeneity and study potential effect moderators. Specifically, I explore the conditions under which voting power of the public sector in *Cajas*’ General-Assemblies moderates the effect of post-election periods on bank chairpersons’ survival; furthermore, I explore whether the size of a bank matters, and I examine different functional forms of the variable *Years in Government* in order to gain insight into whether the effect of the variable is potentially non-linear. Finally, I present results from models that analyze if the chairpersons’ turnover risk is lower when coalition governments are unseated compared to when single-party governments leave office.

Table A12: Cox PH model with subsample for observations between 1990–2007

	excl. pre-1990	excl. post-2007	1990-2007
Years in Government	−0.04*** (0.02)	−0.05*** (0.02)	−0.05** (0.02)
Post Election	0.37** (0.17)	0.26* (0.14)	0.38** (0.17)
Coalition	−0.20 (0.22)	−0.16 (0.19)	−0.24 (0.23)
Public Sector Vote Share	0.98 (1.10)	−0.09 (0.89)	0.69 (1.11)
Return on Assets _{t−1}	−0.05 (0.22)	−0.05 (0.15)	−0.15 (0.23)
Party: Other	−0.33 (0.40)	0.08 (0.33)	−0.10 (0.42)
Party: Socialist	0.58 (0.36)	0.84*** (0.30)	0.86** (0.38)
AIC	1431.99	2104.05	1307.10
Num. events	165	221	152
Num. obs.	999	1142	882

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A13: Alternative specification of election variable

	Chairpersons
Election _{t+1}	−0.13 (0.18)
Election _{t+2}	−0.44** (0.19)
Election _{t+3}	−0.32 (0.20)
Years in Government	−0.06*** (0.02)
Coalition	−0.21 (0.18)
Public Sector Vote Share	−0.01 (0.92)
Return on Assets _{t−1}	0.07 (0.15)
Party: Other	−0.12 (0.33)
Party: Socialist	0.89*** (0.29)
AIC	2241.11
Num. events	234
Num. obs.	1259

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A14: Cox PH Model exploring effect heterogeneity for public sector vote share

	Full Sample	Government Change	
		Yes	No
Years in Government	-0.06*** (0.02)		
Post Election	-0.51 (0.60)	-1.31 (1.04)	0.03 (0.75)
Public Sector Vote Share	-0.95 (1.15)	-1.19 (1.74)	-1.87 (1.54)
Post Election * Public Sector Vote Share	1.70 (1.25)	3.26 (2.10)	0.86 (1.62)
Coalition	-0.21 (0.18)	-0.03 (0.25)	0.01 (0.30)
Return on Assets _{t-1}	0.04 (0.15)	-0.23 (0.25)	0.08 (0.19)
Party: Other	-0.09 (0.32)	0.74 (0.47)	-0.64 (0.51)
Party: Socialist	0.81*** (0.29)	0.39 (0.58)	1.13*** (0.41)
AIC	2231.21	708.83	1225.57
Num. events	234	94	140
Num. obs.	1259	445	814

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The results in the main analysis have shown that the vote share of the public sector in banks' General-Assembly has no direct effect on the tenure of bank chairpersons. This is not surprising given that – following the argument put forth in this paper – political influence over banks should only be associated with a higher hazard shortly after elections and when new governments enter political office. To explore whether the public sector vote share in a bank's General-Assembly moderates the effect of electoral cycles, I therefore interact the variable *Public Sector Vote Share* with the dummy for post-electoral periods *Post Election*. In model 1, I do not distinguish between terms when new governments enter office and when governments serve consecutive terms. In model 2 and 3, I run separate models for terms after government changes (model 2) and terms when incumbent governments were reelected (model 3). The results appear in Table A14 and show that chairpersons of banks in which the public sector has a higher vote share face a higher risk of being replaced shortly after elections in which new governments were elected (interaction term in Model 2). The effect is however fairly imprecisely measured and thus not statistically significant ($p = 0.12$). After elections without a change in government, the interactive term is much smaller and far away from being statistically significant. This provides suggestive evidence that, in fact, politicians interfere in the appointment procedure of chairpersons rather than banks reacting to political events. Note that the measure of political vote share is the regulatory cap on public-sector vote shares and might deviate from de facto voting power in the respective banks; it is subject to considerable measurement error which might explain the rather large standard error.

Furthermore, one might suspect that larger banks that expanded beyond the borders of their home region and thus have a more diverse portfolio are subject to less political influence from the regional governments compared to banks that operate locally with a small balance sheet. On the other hand, the outlined mechanism of political influence through the General-Assembly does not shield larger banks; to the contrary, their bigger loan volume makes them more attractive for politicians in order to finance their political projects. Be this as it may, it is insightful to explore if geographical expansion moderates the effect. It is hard to come by detailed historical data on

Table A15: Cox PH Model, splitting sample by bank size

	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Years in Government	−0.03 (0.03)	−0.10** (0.04)	−0.05* (0.03)	−0.08** (0.04)
Coalition	−0.01 (0.42)	−0.34 (0.35)	−0.19 (0.33)	−0.09 (0.41)
Post Election	0.68** (0.31)	0.27 (0.29)	0.20 (0.28)	0.25 (0.30)
Public Sector Vote Share	0.56 (1.87)	3.41** (1.63)	−0.23 (1.49)	−2.42 (2.26)
Return on Assets _{t−1}	0.50 (0.31)	0.12 (0.24)	−0.33 (0.31)	−0.67** (0.28)
Party: Other	−0.38 (0.96)	−2.84*** (0.92)	−0.05 (0.60)	0.76 (0.63)
Party: Socialist	0.25 (0.56)	0.10 (0.84)	1.15** (0.56)	0.87 (0.68)
AIC	381.21	403.81	450.61	385.29
Num. events	57	60	62	55
Num. obs.	316	314	304	298

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

the geographical expansion of Spanish *Cajas*. Yet, geographical expansion and diversification of banks' portfolios can be proxied by the size of banks. I thus harness information about banks' total assets in order to test whether the identified patterns vary by bank size. Table A15 presents findings from running models on four samples that divide the entire sample into quartiles based banks' total assets.¹⁵ The results show that for all quartiles, chairpersons' hazard decreases as governments remain longer in office (although the relationship is not statistically significant for the lowest quartile). Also, chairpersons face higher relative hazards shortly after election across banks of all sizes, yet the effect is much larger (and statistically significant) in the lowest quartile.

I, moreover, explore whether a quadratic or a cubic term for the variable *Years in Government* improves the model fit which would indicate a non-linear relationship between the variable and chairpersons' hazard. Table A16 show that including a quadratic (model 2) and a cubic term (model 3) of the variable do not substantively improve model fit: the adjusted R² and AIC are very similar across all three models. The interpretation of the coefficients for the variable is difficult from the regression table alone. I therefore plot the effect for all three models in Figure 3 using the `simPH` package from Gandrud (2015). It shows that including a quadratic or cubic term does not affect the overall conclusions of the main model and shows that a linear interpretation of the variable's effect approximates true relationship sufficiently well while being easier to interpret.

Finally, I entertain the question whether chairpersons face a lower hazard after coalition governments get unseated compared to when single-party governments are voted out of office. This would logically follow from the hypothesis that political veto players can prevent politically motivated appointment decisions to bank boards that I tested in model 4 and 5, Table 1, in the main paper. Following this logic, one could argue that chairpersons should face a lower turnover risk after coalition governments lose political office, because chairpersons under coalition governments are less likely to be political allies of the incumbent government and, hence, the successor government does not have to replace a politically appointed chairperson from bank office. I test this intuition and present the findings in Table A17. The results show that, indeed, chairpersons'

¹⁵I divide the sample into quartiles for each year in the sample in order to account for time trends in bank size.

Table A16: Cox PH Model, different polynomials for variable *Years in Government*

	Linear	Quadratic	Cubic
Years in Government	-0.06*** (0.02)	-0.02 (0.05)	0.02 (0.10)
Years in Government ²		-0.00 (0.00)	-0.01 (0.01)
Years in Government ³			0.00 (0.00)
Coalition	-0.20 (0.18)	-0.17 (0.18)	-0.17 (0.18)
Post Election	0.28** (0.14)	0.31** (0.14)	0.32** (0.15)
Public Sector Vote Share	-0.03 (0.91)	0.03 (0.91)	0.05 (0.91)
Return on Assets _{t-1}	0.04 (0.15)	0.03 (0.15)	0.03 (0.15)
Party: Other	-0.09 (0.32)	-0.11 (0.32)	-0.11 (0.32)
Party: Socialist	0.80*** (0.29)	0.74** (0.30)	0.73** (0.30)
AIC	2230.69	2232.34	2234.15
R ²	0.48	0.48	0.48
Max. R ²	0.90	0.90	0.90
Num. events	234	234	234
Num. obs.	1259	1259	1259

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

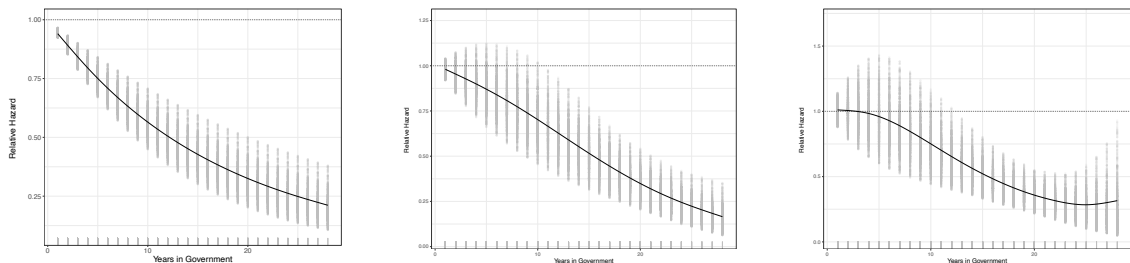


Figure 3: Plots different functional forms of variable *Years in Government* from Models 1-3 in Table A16 based on simulations using Gandrud (2015)'s `simPH` R package.

Table A17: Results from a model that explores the effect of unseated coalition governments

	After Gov't Change	No Gov't Change
Coalition in prev. term	-0.51* (0.26)	0.51* (0.26)
Post Election	0.28 (0.22)	0.45** (0.18)
Public Sector Vote Share	1.29 (1.31)	-1.42 (1.21)
Return on Assets _{t-1}	-0.30 (0.25)	0.11 (0.19)
Party: Other	0.80* (0.46)	-0.55 (0.51)
Party: Socialist	0.27 (0.57)	1.25*** (0.42)
AIC	721.29	1220.01
Num. events	94	140
Num. obs.	445	814

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

hazard ratios are lower when coalition governments are unseated compared to when single governments are voted out of office (model 1). Interestingly, I also find that the hazard ratio is higher for chairpersons when coalition governments serve consecutive terms. This can be interpreted as evidence that single-party governments tend to replace bank managers shortly after being elected into political office whereas coalitions take longer to agree on a new bank chairperson. I thank one anonymous reviewer for suggesting this empirical test that provides further depth to the argument puts forth.

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