Do Politicians Affect Firm Outcomes? Evidence from Connections to the German Federal Parliament*

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Abstract

We study how connections to German federal parliamentarians affect firm dynamics by constructing a novel dataset linking politicians and election candidates to the universe of firms. To identify the causal effect of access to political power, we exploit (i) new appointments to the company leadership team and (ii) discontinuities around the marginal seat of party election lists. Our results reveal that connections lead to reductions in firm exits, gradual increases in employment growth without improvements in productivity. Adding information on credit ratings, subsidies and procurement contracts allows us to distinguish between mechanisms driving the effects over the politician's careers.

Keywords: Politicians, Firm Performance, Identification, Political Connections.

JEL classification: O43, L25, D72

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1 Introduction

The business and political sector have many joint interests and connections between firms and political power are a common phenomenon across the world (Faccio 2006). While political connections could in principle have social value, for instance, by overcoming market frictions, they mostly raise concerns regarding inefficient resource allocation, political favoritism, and corruption. In modern democracies, the recent rise in the number of prominent business politicians whose involvement benefits corporate interests has been particularly viewed with suspicion (Babenko et al. 2023). More generally, it is important to understand how high-profile political connections affect firm outcomes and the economy.

In this paper, we study the effects of members of the federal parliament in top executive or supervisory positions on firm outcomes in Germany. While a large literature examines the effects of political connections on financial outcomes such as firm value or access to government loans, we focus on economic outcomes including employment, labor productivity, and firm survival. We add information on credit ratings, large scale subsidies and EU procurement contract as additional outcomes for a comprehensive analysis of involved mechanisms. So far, economic firm outcomes have been mostly studied in the context of firm connections to local governments where powerful politicians have the means to flexibly allocate funding or assign government contracts to connected firms and thus improve their competitive market position. In contrast, we focus on connections to members of the federal government, who are more strongly monitored by transparency regulations and have less direct access to local finances.[1] We ask the question whether top level politicians in a country with strong institutions and rigid transparency regulations can influence firm outcomes, and, if so, how?

To answer our research question, we compile a novel data base combining information from multiple administrative sources. We start with collecting detailed information on all members of the German *Bundestag*, the federal parliament, since 1949 and of all candidates on party lists for federal elections since 1998. We merge these individuals to the universe of German firms exploiting firm-level ownership information and their position and identity members in the executive leadership team, such as CEO/owner, executive and advisory board member. The firm data, provided by *Creditreform* and organized by the ZEW – Leibniz Centre for European Economic Research in Mannheim, contains comprehensive information on firm outcomes such as credit scores, employment, sales, firm entry and exit dates. At the firm level, we link data on economic subsidies provided by the Halle Institute for Economic Research (IWH) and European-wide public procurement data from Tenders Electronic Daily (TED) provided by the European Commission.

The data allow us to exploit the timing of political mandates and firm-level positions and implement two research designs that rely on events at which we can identify causal effects of a change in political connection on firm outcomes. Our first identification strategy is an event study design which

[1] In Germany, disclosure requirements only exist for national and, more recently, for state-level politicians. Local politicians at the county- and municipal-level are not obliged to disclose their activities outside of parliament.

analyses new appointments of current or former parliamentary members. Specifically, we compare firms appointing a politician with similar firms appointing a non-politician in their executive leadership team using a difference-in-differences strategy with a matched control group. This strategy identifies the causal effect of the political appointment unless firms choose the timing of political appointments over non-politician appointments conditional on expected outcomes.

Our second strategy develops a framework that relies on election outcomes in which the winner of the election is arguably random. In particular, we focus on ranked lists of candidates that are submitted in advance of each election by each party and in each federal state. State-level party vote shares determine the number of candidates from each list who enter parliament. This system creates a marginal seat on each list with a winning candidate who just enters parliament and the candidate on the next seat who just loses. We exploit this discontinuity along the ranking on the party list, pooling 256 local discontinuities in a regression discontinuity design (RDD). This exceptional situation allows us to compare companies who have a political candidate in one of their executive positions and become connected to parliament once this candidate wins the marginal seat with companies in the same situation whose candidate misses the marginal seat. We also consider an analogous situation with an incumbent candidate who is up for re-election and either keeps the mandate or loses it.

Our identification strategies define a series of events spread over the politician's career. Given that the identification assumptions hold, we can thus learn from the estimation results how the effects of a political connection vary over the political life cycle. We first observe the start of the political career when a political candidate with a position in the firm wins an election and enters parliament. At a later career stage, we observe active members of parliament, who are appointed to an executive or supervisory position. At the end of the political career, we observe the exit from a government mandate when an incumbent politician with a position in the firm loses the election. Finally, we observe a former politician who has already resigned from their government mandate entering a firm in an executive or supervisory position.

Together with the rich set of outcome variables observed in the data, the identification of different events over the political career allows us to distinguish between potential mechanisms by which a connection to a national politician can affect firm outcomes. One potential mechanism by which top executives affect firm outcomes is *management quality*. Our analysis can address the question whether parliamentary politicians are better managers than non-politicians. Empirical evidence from Sweden shows that politicians are positively selected in terms of leadership skills (Dal Bó et al. 2017). In addition, politicians collect experience of political decision processes and form contacts that may be valuable for firms. These arguments suggest that the entry of an active or former politician into executive ranks might improve firm outcomes, whereby the latter may be more experienced and has more time to devote to the firm. In contrast, an election win should not affect management quality in the firm because the candidate already had the executive position before the election. We, expect management quality to be reflected in all our outcome variables, except credit ratings.

The connection to a prominent politician can serve as a *positive market signal*, enhancing the firm's reputation as a reliable business partner. The market signal mechanism is likely to be directly reflected

in improved credit ratings. Additionally, such signals may have short-term effects on firm survival, potentially reversing the fortunes of struggling businesses. We expect that this effect is particularly pronounced when candidates win elections or when active politicians are appointed to high-ranking positions, as these events tend to improve market perceptions.

The third mechanism we consider is *direct intervention* by politicians in decision-making processes to favor connected firms. The politician's direct involvement in decisions should primarily impact the likelihood that a firm wins subsidies and procurement contracts, and it may amplify the impact on employment and productivity growth. Specifically, active politicians with political power and experience should be able to directly intervene in favor of their connected firms. Thus, we expect to find positive impacts from appointments of active politicians and candidates winning an election (and the negative impacts for incumbents losing an election).

In our main specifications, we estimate short-term impacts in a period of two years after the new appointment or the election year. The empirical analysis results in two main findings. First, our data reveal that firm connections to parliamentary politicians are not uncommon in Germany and the share of firms connected to politicians has almost doubled over the last two decades. Second, we document significant effects of political connections on several firm outcomes, even though the German environment is strongly regulated. In particular, we find that gaining access to parliament improves credit ratings and leads to reductions in firm exit rates. We find increases in employment growth for firms appointing active and, especially, former politicians, but we do not find corresponding increases if a candidate wins an election. We also find positive effects on the probability of winning subsidies and procurement contracts for appointments of politicians and for candidate election wins, not all of which are statistically significant. If an incumbent loses an election, we generally find the opposite sign from election wins, but the coefficients are smaller and lack statistical precision.

Our evidence is consistent with all three of the mechanisms we introduced above in determining the outcomes. In line with the *signal mechanism*, we find positive impacts of appointments of active or former politicians on credit ratings. Similarly, we find positive impacts on credit ratings after an election win and a negative impact after election losses. Short-run reductions in firm exits after appointments of politicians and election wins are also compatible with the signal mechanism. Evidence in favor of positive impacts of the *management quality* of politicians in top executive positions is provided by the increase in employment growth and to a lesser extent productivity growth after a political appointment. Importantly, we do not see corresponding positive effects if a candidate wins an election, which is expected because in this case the new connection to politics is not related to a positive change in management quality. Finally, positive impacts on gaining access to subsidies and procurements can be affected by both the *management quality* and the *direct involvement* mechanisms in case of new appointments of active or former politicians. But, as management quality remains constant in the event of election wins, the positive impact on subsidies and procurement contracts in the RDD analysis provides evidence that direct involvement in favor of connected firms plays a role.

We draw three conclusions from the analysis of mechanisms. First, parliamentary politicians improve the quality of management in connected firms. Second, new political connections formed

from appointments of parliamentary politicians or election wins of candidates who are already in the firm send positive signals about the firm to the market. Third, there is evidence that connected politicians affect firm outcomes by direct intervention in decision processes favoring these firms.

The paper is organized as follows. Section 2 discusses the related literature and highlights our contributions. Section 3 describes the institutional details of Germany's electoral system and transparency regulations for activities outside of parliament. In Section 4, we describe the data and the corporate governance structure and provide descriptive statistics for different types of connections. The empirical strategies are introduced in Section 5. Section 6 documents the effect of appointing a politician in an event study design on firm-level outcomes. Section 7 develops reduced-form identification exploiting election results and submitted party lists and shows the results on winning and losing political power. Section 8 provides evidence on the mechanisms. Section 9 concludes.

2 Related Literature

An active literature investigates the connection between the political and business sector and analyzes the impacts of these connections on the economy. To review this literature and clarify our contribution, we have assembled a list of 25 recent studies examining the effects of political connections on the outcomes of connected firms in Table A.1 in the Appendix. We categorize these studies by the type of connection, distinguishing whether the firm is directly connected to an active politician who holds a position in the firm and a position in government (in Panel A), whether the firm is directly connected to a former politician, who holds a position in the firm but no longer has a position in government (in Panel B), or whether the connection is indirect and firm representatives and politicians interact outside the firm (in Panel C). This is the case, for example, when politicians and firm representatives belong to the same family or the same social network, or when companies donate to political campaigns.

We further distinguish between studies investigating connections to the national government (in Panels A.1, B.1, C.1) and connections to the local governments at the state or district (in Panels A.2 and C.2). Overall, there is a relatively even split of studies across these categories. But we found only two studies explicitly investigating firm connections to former politicians both of which study connections to the national government (Goldman et al. 2013, 2009). In this scheme, our paper contributes evidence on firm connections to active and former politicians in the national government (Panels A1 and B1).

Column (5) in Table A.1 lists the main outcome variables considered in the respective study and column (6) shows the direction of the estimated effects. The table clearly shows that most studies investigate financial firm outcomes, analyzing the effects of a political connection on stock returns, stock prices, or returns on assets. Starting with the seminal study by Fisman (2001) these studies unambiguously document positive effects from a connection to political power on firm value across a wide range of countries. Another prominent set of outcomes concerns firm access to financing via stimulus grants (Choi et al. 2024, Duchin & Sosyura 2012) or government bailout (Faccio et al. 2006),

loans and procurement contracts (Khwaja & Mian 2005, Schoenherr 2019, Brown & Huang 2020, Goldman et al. 2009), and foreign investment (Leuz & Oberholzer-Gee 2006). Again, the findings across studies strongly support the hypothesis that political connections help firms in getting access financing at the local or national level.

A few studies focus on savings banks in Germany, a special type of firms, which are strongly politically connected by legal rules mandating that a senior local politicians has to serve as the supervisory board chairman. Comparing savings banks to other local banks without political connections, Englmaier & Stowasser (2017) find that local politicians push for savings bank lending prior to elections to improve local economic outcomes and their own reelection chances. Using a similar strategy, Koetter & Popov (2021) show that saving bank lending strongly increases after local elections that result in changes in political power. Haselmann et al. (2018) exploit indirect connections via a German service club and find that club members who are elected as mayors and become heads of savings banks increase lending to the other club members. The extra lending has lower returns, however, and does not result in increased investment of loan-receiving firms.

Compared to the large number of studies analyzing outcomes related to firm value or financing, relatively few studies focus on the effects of political connections on economically relevant outcomes such as sales, investment, employment, productivity, or firm survival. Importantly, these outcomes are so far only studied for direct political connections to local governments or for indirect connections to politics. We could not find previous work that studies economic outcomes in the context of connections to active or former politicians in the national government, as we do in our analysis.

The direction of the effects on economic outcomes found in the literature are more ambiguous than those for financial outcomes. Several studies report positive effects of political connections on sales (Schoenherr 2019, Akey & Lewellen 2017, Amore & Bennedsen 2013) which is often interpreted as a consequence of the preferential access to government funding. Two studies in Italy find zero effects of connections to active politicians in the local government on labor productivity (Cingano & Pinotti 2013, Akcigit et al. 2023). Bertrand et al. (2018) focus on firms with connected CEOs who served as cabinet members in France and analyze abnormal employment and sales responses in election years. Similarly to Englmaier & Stowasser (2017), they find that connected firms increase hiring in election years with the aim of generating support for local election results of their party friends. But these extra hires do not reflect other firm outcomes as sales remain stable and firms do not get improved access to loans or tax reductions.

The literature provides even fewer results on the effects of political connections on employment dynamics or firm exit and the existing estimates exclusively refer to connections to local governments. Akcigit et al. (2023) and Bertrand et al. (2018) find positive employment effects but Choi et al. (2024) report negative effects for firms donating to local government candidates. Besides our paper, Akcigit et al. (2023) are the only ones studying firm exit and find that political connections tend to increase firm survival. We advance on the identification of the causal effects by exploiting political candidates in our RDD specification, a setting that is not possible in the Italian case.

This overview of the literature makes it clear where our paper contributes. First, we study firms

directly connected to active or former politicians at the level of the national government in Germany based on large scale data covering a period of 20 years and the universe of German firms, parliamentary politicians, and election candidates. Second, our results are based on rigorous identification designs exploiting appointments to executive and supervisory firm positions and quasi-random election results. Third, we study a wide range of outcomes that have been less investigated in the literature and for which we still lack clear evidence.

3 Institutional Details

One empirical identification strategy exploits discontinuities caused by elections at the federal state level. For this reason, we first describe in this section the electoral system in Germany. We further provide information on disclosure requirements for politicians who serve on board or manage firms.

Federal Elections in Germany. According to the electoral system for the German *Bundestag* each voter has two votes. Based on the first vote – also called the direct vote – the candidate with the highest number of votes in each of 299 election districts (regional boundaries as of 2017)[2] enters parliament with a direct political mandate.[3] With the second vote, citizens vote for a political party. The second vote is therefore decisive for party representation in parliament and decides on the number of seats each party gets. Parties below a 5% vote share threshold do not enter parliament unless at least three party candidates win a direct political mandate in their respective election districts (*Grundmandatsklausel*). After parliamentary mandates have been assigned, parties start negotiations. Unless one party holds an absolute majority of mandates, two or more parties form the government.

Our empirical strategy exploits results based on the second vote. Prior to each election, parties submit a ranked list of candidates in each of the 16 federal states (*Landesliste*). These party lists are submitted for approval to the respective local election authorities at least 69 days before the election and cannot be changed later. At the state level, party vote shares determine each party's number of parliamentary mandates.[4] The first mandates in each party and state are assigned to winners of direct votes (if they have a party affiliation) and the remaining mandates are assigned to candidates on the respective party lists starting with the highest ranked candidates. If a party in a certain state wins more election districts via the first vote than assigned mandates based on the second vote, no candidate formally enters from the party list.

This system creates a marginal seat on each party list where the candidate on the marginal seat enters parliament and the candidate on the next seat does not enter. Our identification strategy exploits the discontinuity around marginal seats on party lists. Appendix Table D.1 shows characteristics of party lists of the six parties that enter the German parliament over the election cycles from 1998 to

[2]The number of election districts was 328 in the election year 1998.

[3]Candidates on the district ballots are not required to be affiliated with a political party. But typically, the biggest party wins the direct political mandates. In the election year 2017, the Christian Democrats (CDU with its sister party in Bavaria CSU) won 73.5% and the Social Democrats (SPD) won 18.4% of all election districts.

[4]The number of individual mandates per state is determined by population size.

2017. The six parties are the Christian Democrats (CDU/CSU), the Social Democrats (SPD), the Liberals (FDP), the Greens (Die Grünen), the Left (Die Linke) and the right-wing party AFD. The average number of submitted candidates per list is around 21 and varies across political parties. As shown in the third row of the table, the average marginal seat varies around 6 by party and time. For example, the marginal seat for the CDU/CSU was about 14 in the elections of 2013 and 2017, while for the SPD the marginal seat was about 9. For the smaller parties, the marginal seat varies between 3 and 6. The share of listed candidates with a firm connection (for a definition of connections see Section 4) has been increasing over election cycles; in the last two election cycles more than half of the candidates on the average list were connected to a firm. But there is also strong variation across parties.

The placement of candidates on the respective party lists has a strategic component (Buisseret et al. 2022). Typically, prominent party members are placed at the top of the party list to maximize chances of entering parliament. But it is also the case that some candidates with high chances of winning the election district on the direct vote are not placed on the party list.[5] Due to the preference that is given to the first vote when assigning mandates, it is difficult to accurately predict the marginal seat on the party lists. An additional complication with implications for marginal seats on all party lists arises due to the so-called *Überhangmandate*. If a party wins more direct mandates than entitled mandates based on the vote share from the second vote, all candidates winning their election districts still enter parliament via the direct mandate. The resulting imbalance in party representation is compensated by increasing the total number of seats in parliament. Additional mandates are equalized via so-called *Ausgleichsmandate* and assigned to the other parties. These additional seats are in turn filled with candidates from the respective party lists.[6]

To document the variation in marginal seats across election cycles, Appendix Figure D.2 Panel B shows the correlation between the change in vote shares and the change in the marginal seats across elections and state party lists which is with 0.78 positive but not perfect.[7]

Disclosure Requirements. Members of the German federal parliament are obliged to follow the rules of conduct at the federal level that were first formulated in the Act of Parliament (*Abgeordnetengesetz*) and in the Rules of Procedure (*Geschäftsordnung*) of the German *Bundestag* which were first passed in 1972. These rules stipulate that the main focus of the activities of a member of the federal parliament is the execution of the political mandate. But activities of a professional or other nature are generally allowed alongside the mandate.

[5]Over the six election cycles between 1998 and 2017, we observe 1,728 candidates who are not placed on the party list but enter parliament via the direct mandate and 8,690 political candidates on party lists.

[6]Over time, this system led to a substantial increase in the total size of the German *Bundestag*. Between the election years 1998 to 2009, there were 58 *Überhangmandate* (32 among the CDU/CSU and 26 among the SPD).

[7]To further support the existence of the random component of the marginal seat, Panel A of Appendix Figure D.2 shows that there exists high residual variation. Specifically, we run the following regression: $\textit{marginal seat}_{tsp} = \theta_t + \omega_{sp} + \epsilon_{tsp}$, where the marginal seat is the cutoff seat number at the election year – state – party level and θ_t, ω_{sp} represent election year and federal state $\times$ party fixed effects, respectively. After taking out the fixed effects, the standard deviation is estimated to be 2.36, which shows the importance of factors that determine the cutoff seat beyond year, state and political party effects. Including election polls two months before the election in the specification increases R^2 slightly from 0.75 to 0.77. Including the actual vote share instead of the polls increases the R^2 to 0.81. This shows the limited possibility to predict the marginal seat with election polls.

With regard to these activities, members of the federal parliament have comprehensive duties of disclosure. They are obliged to notify the president of the *Bundestag* of their most recent professional activity, remunerated activities in addition to their mandate and functions in companies, corporations and institutions under public law. Functions in clubs, associations and foundations are also subject to notification, as are shareholdings in corporations or partnerships and agreements on future activities or pecuniary advantages. In 2007, the constitutionality of these rules was confirmed by the Federal Constitutional Court (*Bundesverfassungsgericht*).[8] Since then, each parliamentarian publishes online and in the *Amtliches Handbuch des Deutschen Bundestags* (part II) the name of the firm or cooperation, the position, and the income (in three income brackets) that is generated through this outside activity. A subsequent reform in 2021 obliges members of the federal parliament to publish the exact amount of income from their outside activities if they exceed 1,000 euros per month or 3,000 euros per year.[9]

According to information from the online government portal, the following types of membership positions are disclosed by politicians: board of directors, advisory board, advisory council and shareholder.[10] These are the positions that can be matched to the firm data for our empirical analysis.

In Germany, involvement of politicians in firms is systematically more likely to be observed in sectors with a high level of state involvement or regulation, such as the energy sector, transportation/infrastructure or the banking sector. This practice facilitates the representation of interests and objectives of public authorities. For instance, politicians represent their parties in municipal bodies like the board of directors of public banks (e.g., *Sparkassen*, *Volksbanken*, *Landesbanken*) or the supervisory board of water and energy providers (e.g., *Stadtwerke*, E.ON or RWE). Politicians are also on the supervisory boards of formerly state-owned companies such as *Deutsche Bahn* and *Deutsche Telekom*, which were privatized in the 1990s. A further prominent and historically rooted example is the state of Lower Saxony which, as a co-owner, has two supervisory mandates at *Volkswagen* one of which is reserved for the Minister President of Lower Saxony. According to information from the online government portal, politicians also hold connections to welfare organizations (e.g., *Caritas*, *Deutsches Rotes Kreuz*, *Arbeiterwohlfahrt*). As a consequence of these types of political representation political connections are particularly frequent among associations (see Table 2 on page 15).

4 Data & Measurement

4.1 Data

Firm-Level Data. The basis of our firm-level data is the *Mannheim Enterprise Panel* (MUP), a panel dataset generated and hosted by the ZEW – Leibniz Centre for European Economic Research. The

[8] Disclosure requirements at the German state level have been introduced over the recent years, starting in Bavaria in 2013, followed by Lower-Saxony, Hesse, Thuringia, Saxony and Brandenburg in 2014. There are no disclosure requirements at the district or municipal level.

[9] In 2015, the Federal Cabinet passed a draft law according to which current or former ministers and parliamentary state secretaries must report their intentions to move from politics to business. The decision will then be made by the government on the basis of a recommendation from an advisory committee. If the committee sees a conflict of interest, it can impose a waiting period of twelve months, or up to 18 months in exceptional cases.

[10] For more information, see https://www.bundestag.de/abgeordnete/nebentaetigkeit/nebentaetigkeit-213826.

data are provided by *Creditreform e.V.*, the largest credit rating agency in Germany. Besides the official Business Register of the Federal Statistical Office, the MUP is the most comprehensive micro database of companies in Germany with full coverage of all firms starting around 2000. Bersch et al. (2014) provide detailed information on data collection, processing and the definition of variables. For our analysis, we use wave 56 with the latest available year being 2019. More detailed information on the MUP, the number of observations and further descriptive statistics can be found in Appendix B.1.

The MUP contains a large number of firm characteristics. Most importantly, we observe firm size, total sales, the industry affiliation at the five-digit industry code according to NACE rev. 2, local municipality code, the legal form, as well as the date of incorporation and closure.[11] In addition to information related to firm performance, we are able to exploit detailed information on the ownership structure and individual members of the executive leadership team. These are the individuals we can then link to political candidates and parliamentary members.

We focus on individuals who are involved in all decisions of fundamental importance to the corporation. In particular, the following types of positions are of interest: (i) owner, (ii) CEO, (iii) member of the executive board, (iv) member of the supervisory board, and (v) partners.[12] The type of executive position and therefore the corporate governance structure is determined by the legal form of the company. Table 1 shows the available executive positions by the main legal form of corporations where Limited Liability Companies (LLCs), stock corporations, small businesses (combining single-owned companies, liberal professions and commercial companies), civil law partnerships, and associations amount to about 97% of all firms in the data.[13]

Small businesses represent with about 52% the majority of all firms in Germany and they are governed by owners, mostly a single owner. The corporate governance structure of all other legal forms typically consists of a managing and monitoring body. *LLCs* represent about one third of all firms and 62% in terms of employment. They are governed by a CEO who manages the day-to-day business and by partners, who have limited control over the day-to-day business but are involved in fundamental decisions of the company and typically appoint a CEO.[14] The corporate governance structure of *stock corporations* consists of an executive board that is the managing body and a supervisory board that gets elected by the shareholders of the company. The supervisory board controls the executive board and is involved in decisions of fundamental importance to the corporation (Jäger et al. 2021).

Civil law partnerships consist of at least two partners who, in contrast to LLCs, are unlimited

[11] The raw data contain missing values for employment and sales information. We impute some of the missing values in both cases if we observe a gap of up to two years and assign values based on linear interpolation. We further impute missing values with the last observed variable entry for up to two years if the firm did not exit during these two years. Appendix Table B.1 provides the overall number of observations as well as the number of observations with employment and sales information at the yearly level.

[12] The data also contain official functions of administrator/trustee in case the company went bankrupt and capital provider. In terms of the latter, the data contain the main stock holder for stock corporations and the limited partner for limited partnerships (*Kommandist*).

[13] The remaining legal forms are limited partnerships, general partnerships, and registered cooperatives.

[14] Partners in the data differ by the legal form of the company. In the case of LLCs, which are the majority of cases, the German coding refers to *Gesellschafter*. In the case of limited partnerships, the general partner (*Komplementär*) is responsible for the day-to-day business.

Table 1: Corporate Governance by Legal Form

Executive Position	Involvement	LLC	Stock Corporation	Small Business	Civil Law Partnership	Association
(1)	(2)	(3)	(4)	(5)	(6)	(7)
Owner	Managing/ day-to-day			✓		
CEO	Managing/ day-to-day	✓			(✓)	(✓)
Executive Board	Managing/ day-to-day		✓			✓
Supervisory Board	Supervising/ monitoring		✓			
Partner	Supervising/ monitoring	✓			✓	
Share of firms		33.3	0.42	52.1	8.20	2.50
Share of employment		61.7	5.87	23.9	2.93	2.63
Average firm size		9.22	67.09	2.24	3.79	9.16
Share politically connected		0.38	9.22	0.03	0.13	3.86

reliable for all joint obligations. In the special case when civil law partnerships are combined with joint ventures, there is also a formal CEO, whereas under the more standard civil law partnership, both partners manage the firm. The final legal form are *associations* which are organized, represented and managed by an executive board. The board is elected by the members of the association. In contrast to stock corporations, the executive board is controlled by the General Assembly of Members. In some cases, we also observe a CEO in associations. This is typically the case in large associations with many members. The lower part of the table further documents the share of political connected firms by the legal type. Among stock corporations, more than 9% are politically connected, which is the highest share across all legal types. As discussed in the previous section, the share of connected firms is also relatively high among associations with about 3.9%. The connection share among the remaining firm types is less than 1%.

Outcome Measures. To measure the effects of political connections on firm outcomes, we consider several outcome variables which we observe at an annual frequency in the MUP data. Based on the year of closure, we construct an indicator of market exit. We further define firm-level employment and employment growth rates. When considering employment growth, we follow Davis & Haltiwanger (1999) and calculate the growth rate between two points in time as: $(L_{t+k} - L_t)/0.5(L_{t+k} + L_t)$. Next, we derive firm-level productivity from a standard labor productivity measure by calculating $sales/employment$.[15]

Because the MUP data originate from a credit rating agency, we also have detailed information on the firm's annual credit rating score, ranging from 100 (highest creditworthiness) to 600 (default).

[15]Note that the MUP data can be linked to BvD's Orbis data that allow for more sophisticated productivity measures. After linking the data, for only about 4% of connected firms value added is observed, altering a full analysis.

This rating is based on information about the payment behavior, the credit opinion, company development, industry and order situation. As described by *Creditreform*, financial reporting, regional risk, managerial experience and performance indicators such as sales and capital enter the calculation of the score as well. The score has been analyzed in a number of papers, including, for instance, Cremers & Schliessler (2015) and Höwer (2016). The final score provides information on the creditworthiness of the firm. A significant part of about a quarter of the overall index consists of a normative judgement on the question whether a business relation is approved or the relationship with a business partner is not recommended, i.e., one should use collateral when providing credit. Thus, the variable is informative for business partners and financial institutions in terms of credit lines. Although indirect, we exploit this information to provide evidence on potential access to more or cheaper credit.[16]

Data on Public Procurement Contracts & Subsidies. The annual MUP panel data can be linked to two additional data sources. We first explore the *IWH Subsidy Database* that contains information on subsidized projects including a firm name (see Brachert et al. 2018 for a detailed data documentation).[17] The projects available in the database come from various programs and they typically either aim to support innovative activities or – through capital investment subsidies – maintaining/increasing employment.[18] We link the projects via record linkage using firm names to obtain information at what point in time a firm receives subsidies.[19]

Second, we explore project-level public procurement data available to us between 2006 to 2016 from Tenders Electronic Daily (TED) to analyze public procurement contracts by the political connection status of the firm. The data are provided by the European Commission and contains contracts whose value exceeds a certain threshold described in the EU Public Procurement Directives 2014/23/EC and 2014/24/EC (see European Commission 2020 for technical details). The data provide information on the winning bidder including the name and the address. We use the information and link the contracts via record linkage to the firms in our main dataset.[20] Using the TED data, Havlik (2020) documents evidence for political election cycles by showing that public procurement contracts increase prior to national parliamentary elections.

[16] Using the MUP data, Bersch et al. (2020) analyze firm-bank relationships during times of crisis. They first show that bank distress is orthogonal to initial firms' credit risk. They further provide evidence that the expected probability of default of a firm increases if the bank is under distress, indicating a negative association between the credit rating score and lending possibilities.

[17] The two largest programs are (i) the *Förderkatalog*, which represents 59.4% of all covered projects that are organized and supervised by federal ministries. The database does not contain all subsidized projects. The corresponding special units (*Fachreferate*) within each ministry decide on publishing the projects, and the largest place-based subsidy program in Germany representing 23.3% of all covered projects (*GRW - Gemeinschaftsaufgabe "Verbesserung der regionalen Wirtschaftsstruktur"*). The data further cover EU projects (13.7%) coming from the funding periods 6 and 7. The remaining 3.5% of the projects are smaller programs related to subsidizing innovative projects.

[18] There is a relatively large literature that studies *GRW* subsidies in general at the regional and firm level. These include, among others, Becker et al. (2010), Brachert et al. (2019), and Etzel et al. (2021).

[19] In the IWH Subsidy Database, we observe over 697,539 projects with names and regional information for the record linkage. The success rate of the record linkage is 77.7%, which represents 48,694 unique firm IDs in the MUP.

[20] In the TED data, we observe over 400,000 entries and conduct a record linkage based on the firm names and the address. The success rate of the record linkage is 89.6%, which corresponds to 58,507 unique firm IDs in the MUP.

Data on Political Candidates. Our candidate-level data originate from two sources. First, we make use of publicly available data on the online government portal to obtain all members of the German *Bundestag* between the first election term in 1949 and the election term that started in 2018. We downloaded the list of politicians in January 2021 which covers politicians up to the 19th election term (election year of 2017). In total, there are 4,084 unique politicians who served in parliament over this period and 1,790 individuals who served between the election terms 14 to 19, which corresponds to the election year 1998 and 2017 and covers the main period with firm-level information. The dataset further includes baseline information such as the name and surname of the politician, gender, marriage and parental status, birth date, death date and nationality. It further provides information on the party affiliation and the mandate (party list (*Landesliste*) or direct mandate), as well as whether the politician is part of a ministry (information available since election term 14).

The second data source covers all political candidates between election term 14 and 19 provided to us by the *Bundeswahlleiter* in November 2021. In total, the data contain 24,360 candidate-election year observations with 20,715 unique candidates and 93 unique political parties. Over the six election terms between 1998 and 2017, we observe six major parties that are at least once represented in parliament. Among these parties, we observe 8,690 unique candidates and 13,002 candidate-election year observations. Besides information on party affiliation, the data also contain the first and the last name, gender, the year of birth, the election district and the placement on the respective party list, as well as occupational information at the time of the election.

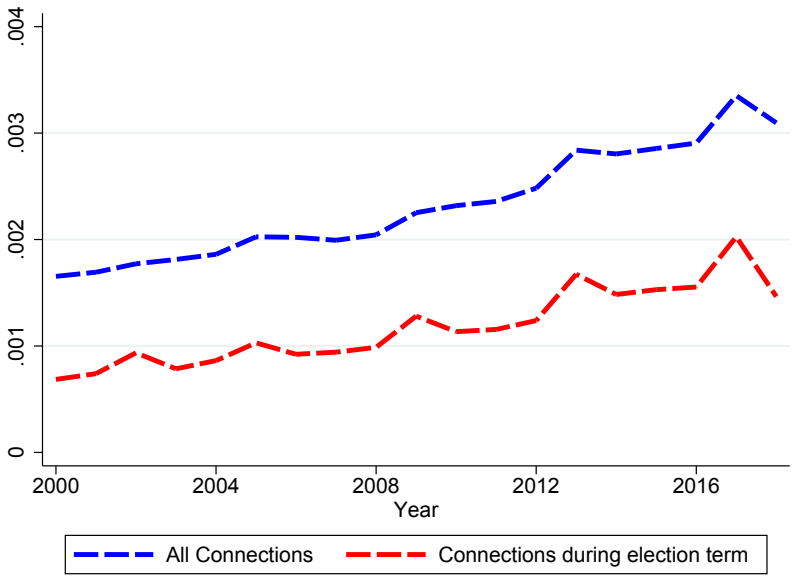
Merging Political Candidates to Firms. We identify political connections of firms by merging politicians and political candidates to the data on executive positions in the MUP based on the full name and the date of birth. Note that this merge provides information on the exact timing of the start and end years of an individual's executive position in a firm over the period 2000 to 2019. In addition, the election data provide the start and end dates of individual mandates. This means that we can identify politicians who hold a current mandate during their position in the firm and former politicians who have already left parliament when they start a firm position. At the firm level, the procedure results in 3,842 firms that are connected to a current or former politician and 14,078 firms with a connection to a person who was at some point a political candidate. Appendix B.2 provides a detailed description of the merge. For the construction of a comparison group, we make use of a 50% random sample of unconnected firms.

Our proposed strategy to match individual politicians and firms has several advantages. First, politicians with a current mandate are subject to public disclosure online since 2007. Our linked firm-politician dataset provides information over a longer time horizon covering the years since 2000. More importantly, we are able to observe connections to firms after the political mandate has ended. Second, we have exact information on the timing, as we can observe the start and end years of political mandates and of the executive position at a firm. Third, we are able to merge successful and unsuccessful political candidates to the firm dataset. A cross-validation with hand-collected data from online disclosures for the election term 2017 shows that 53 out of 58 politicians who are connected to

firms during this election term according to our linked MUP dataset can also be found in the online government portal. Among the remaining five politicians, one had an official job position in the previous election term. Overall, this indicates that a very high share of the merged politicians in our data indeed provide public disclosure statements which guarantees a high degree of representatives of our data.

4.2 Descriptive Statistics

How do political connections of firms evolve over time? Figure 1 shows the share of connected firms, measured as the number of firms observed with a connection at time t relative to all firms weighted by firm size, over time. We distinguish between all connections (blue line) and connections during the current election term of the politician (red line). The figure shows a sizable increase in the connection share between 2000 and 2018. This is not only true for all possible connections, which might increase mechanically because the number of politicians is increasing over time. Also when conditioning on politicians who hold a mandate in the current election term, the share of connected firms doubles over the course of the last two decades. The spikes in the red line correspond to the start of each election term when the share of connected firms is highest.[21]



Notes: The figure shows the share of connected firms over the time period between 2000 and 2018. The blue line shows the share of all connections, including connections to former politicians, relative to the firm population, whereas the red line conditions on connections to current politicians.

FIGURE 1: POLITICAL CONNECTED FIRMS OVER TIME

[21] Appendix Figure B.2 shows the connection intensity over the firm size distribution, indicating a u-shaped association. The connection intensity among small firms with fewer than 10 employees is as high as the connection intensity among firms in the highest two firm size categories. The reason for this shape is that we typically observe one politician at the firm level, but this is different for the largest firms. Large firms can also be connected to multiple politicians at each point in time.

How different are connected firms from unconnected firms? We compare the main characteristics of a 50% random sample of all firms (around 1.28 million firms) in Germany in column (1) of Table 2 with our two main analysis samples of firms with political connections. These are, first, firms who appoint a politician to one of their executive positions where we distinguish between appointments of current and former politicians, shown in columns (2) and (3), respectively. Firms appointing a current or former politicians are relatively evenly split with 44% versus 56%. Second, we consider firms who either have a political candidate in one of their executive positions who runs for a new office in parliament or an incumbent politician who runs for re-election, shown in columns (4) and (5). Here the sample of firms with a candidate who is about to newly enter parliament is larger than the sample of firms with incumbent politicians with 65% versus 35%. We will call the samples of connected firms the *appointment sample* and the *election sample*. The construction of the samples of connected firms is explained in Section 5.1. In Table 2, we measure firm characteristics of unconnected firms as an average over the years 2000 - 2019, firms appointing politicians in the year before the appointment, and firms with political candidates in the election year.

Table 2 Panel A presents baseline firm characteristics. It shows that firms appointing either a current or former politician are larger in size (in terms of the number of employees and firm sales) than the average firm in Germany. This is also the case for firms with a political candidate who is up for re-election. Firms that place a candidate without a political mandate are, however, smaller in terms of the number of employees but larger in terms of sales than unconnected firms. Overall, connected firms are also more productive as measured by labor productivity.

The yearly exit rate of average German firms amounts to 6.1%, but it is substantially lower among connected firms. Consistent with this observation, connected firms also have better credit ratings (here lower numbers in the rating index indicate a higher creditworthiness) and a lower share of connected firms have a rating in the highest default category as shown in Panel B. This is particularly true for connected firms with a candidate who is running for re-election. Only around 0.4% of these firms have a credit rating in the highest default category, implying a rather low market exit risk. Part of the credit rating is the judgement by *Creditreform* whether or not a business relation is approved or needs collateral. The share of firms where a business relation is not recommended correlates strongly with the highest default probability category.

Panel C of Table 2 shows that the overall share of firms receiving a subsidy or a procurement contract is low, as less than 1% ever receive those over the full observation period. This is consistent with the fact that we only observe large contracts in the data. However, the share of firms with subsidies or procurement contracts is substantially higher among connected firms. Among connected firms theses shares are between 2.4% and 6.2% in the year before the appointment or the election year, respectively.

Panel D indicates also major differences with respect to the sector affiliation. Throughout the different samples, connected firms are under-represented in manufacturing, construction, retail trade, and in the hotel/accommodation sector and over-represented in services, banking & insurance, and energy and water (although at low absolute levels). While the majority of connected firms are in the

Table 2: Descriptive Statistics

	Unconnected	Appointing a		Candidates	
	firms	current politician	former politician	without mandate	with mandate
	(1)	(2)	(3)	(4)	(5)
A: Baseline firm-level characteristics					
Log employees	1.259	2.554	2.251	1.112	1.754
Log sales	13.069	15.053	14.911	13.932	14.791
Log(sales/employment)	11.818	12.488	12.651	12.638	12.290
Average yearly exit rates	0.061	0.015	0.020	0.028	0.020
Firm age	21.800	36.753	27.272	19.598	32.533
B: Credit rating					
Credit rating index	289.6	245.7	249.9	273.7	246.7
Credit rating (default risk)	0.089	0.012	0.009	0.037	0.004
Relation not recommended	0.084	0.012	0.008	0.030	0.005
C: Subsidies/procurement					
Any economic subsidies	0.009	0.057	0.062	0.028	0.036
Any procurement contracts	0.006	0.034	0.024	0.035	0.042
D: Sector classification					
Manufacturing	0.083	0.030	0.040	0.052	0.036
Energy	0.003	0.011	0.016	0.017	0.007
Water	0.005	0.009	0.015	0.015	0.012
Construction	0.137	0.008	0.011	0.044	0.015
Retail trade	0.224	0.026	0.039	0.100	0.046
Accommodation	0.074	0.012	0.016	0.021	0.015
ICT	0.029	0.030	0.014	0.041	0.030
Banking, insurance	0.027	0.074	0.063	0.050	0.042
Technical service	0.115	0.154	0.198	0.241	0.209
Business service	0.058	0.029	0.046	0.053	0.029
Other service	0.061	0.391	0.259	0.145	0.356
E: Legal form					
Small Business	0.521	0.023	0.025	0.189	0.067
LLC	0.333	0.245	0.378	0.466	0.339
Stock Corporation	0.004	0.168	0.183	0.044	0.086
Civil Law Partnership	0.082	0.021	0.020	0.050	0.041
Association	0.025	0.495	0.346	0.178	0.419
Other	0.019	0.035	0.033	0.073	0.048
F: Function in firm					
Managing	-	0.657	0.582	0.653	0.589
Supervision	-	0.343	0.418	0.347	0.411
G: Party affiliation					
CDU/CSU	-	0.497	0.479	0.239	0.399
FDP	-	0.127	0.131	0.390	0.188
SPD	-	0.291	0.305	0.156	0.304
Greens	-	0.044	0.050	0.095	0.038
Left	-	0.038	0.031	0.069	0.072
AfD	-	0.002	0.004	0.050	0.000
Government	-	0.594	0.580	0.546	0.749
Close to election district	-	0.281	0.211	0.310	0.395
N	1.3m	662	852	2,564	1,355

Notes: The table shows means for unconnected firms (averaged over the years between 2000 and 2019) and different samples of connected firms. Unconnected firms consist of a 50% random sample of all firms in the MUP. Columns (2) and (3) provide the means for firms appointing a current or former politician measured in the year before the appointment event. Columns (4) and (5) provide the means for firms that place a candidate without a political mandate in the year of the election and for firms with a politician who is up for re-election.

service sector, the share of unconnected firms in this sector is only 23%. Panel E provides information on the legal form. In all samples of connected firms, the share of firms labeled as small business is lower than in the overall population of firms, whereas stock corporations and especially associations

are strongly over-represented among connected firms.

Regarding the function of politicians within the connected firm and their party affiliation, Panel F shows that roughly two-thirds of politicians or political candidates have a managing function in the firm. Panel G shows that, except for candidates without a mandate, the share of firms with connections to the CDU/CSU amounts to 40 to 50%. This is not surprising as this is also the party with the highest number of parliamentary members over the sample period. Relative to the party size in parliament, the share of firm connections to the Liberals (FDP) is relatively large. This is particularly the case for firms with candidates running for a political mandate, where 39% of the firms have a connection to the FDP. Moreover, firms are disproportionately often connected to politicians in government parties, which is particularly the case for firms with connections to candidates who hold a political mandate and are up for re-election (75%). The last variable in the panel shows the proximity between the firms' location and the politician's election district (dummy variable equal to 1 if distance is below 50km). Firms with a candidate who runs for election are more likely to have a local connection as compared to firms that appoint a current or former politician.

5 Empirical Strategies

5.1 Sample Definitions

Our empirical strategy exploits the interplay in the timing of firm appointment and the start and end dates of political mandates. Figure 2 illustrates how these events evolve along a time-line of a politician's career, where the vertical lines mark the start and end dates of a political mandate and the horizontal lines show different scenarios of jobs in executive positions. The crosses in the figure indicate the events we exploit.

We focus first on the blue crosses marking the start of politician appointments in a firm. Here we distinguish between the case of an appointment of a current politician which starts during their political mandate in the left blue cross and the case of an appointment of a former politician which starts after the politician has left parliament in the right blue cross. We use an event study design to compare firms appointing either type of politician with similar firms appointing a new member in their executive leadership team who is not a politician.

Second, we focus on events around election dates which start or terminate a political mandate and are indicated by the red crosses. In the case of the cross at the bottom left of the figure, a person who holds an executive position with a firm runs for election and has the chance to win which means the firm will become connected at this point. Here, we compare firms with a candidate who wins the election with firms with a candidate who loses. For identification we can exploit the discontinuity created by marginal seats on the party list. This means we zoom in on the party list and compare a firm with a candidate who marginally wins the election by getting the mandate on the marginal seat and a firm with a candidate who marginally loses because he/she is ranked below the marginal seat on the party list.

The red cross at the end of the political mandate corresponds to the case where a firm executive

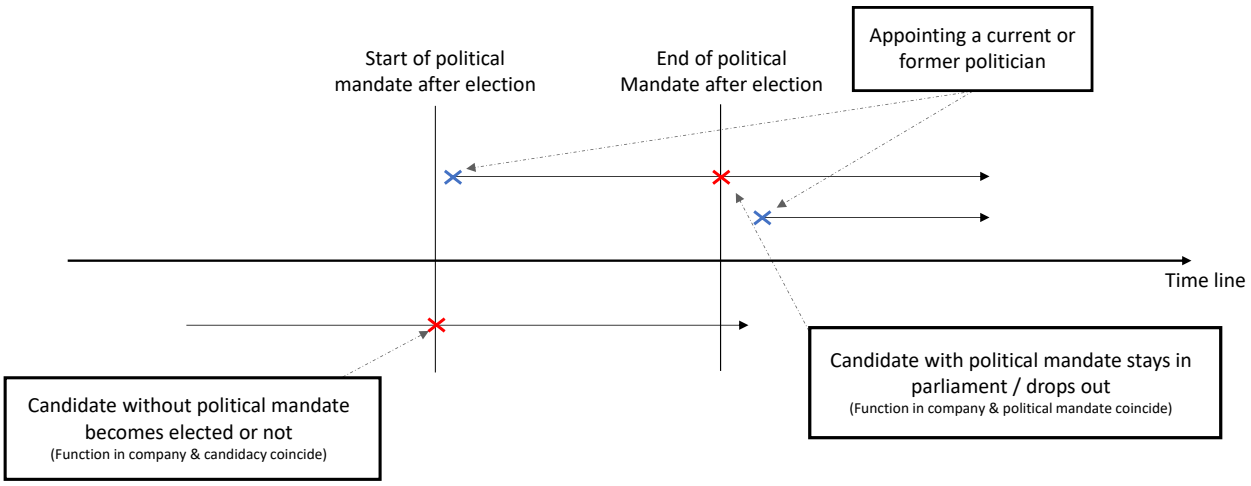


FIGURE 2: TIMING OF EVENTS: CONNECTION TO FIRM AND TO PARLIAMENT

with a political mandate is up for re-election. This person can either be re-elected or drops out of parliament which means that the firm loses the political connection. Analogous to the above case we exploit the marginal seat and compare a firm with a candidate who just wins re-election at the marginal seat and a firm with a candidate who just loses because he/she ranks below the marginal seat on the party list.

5.2 Event Study Design

To identify the effect of appointing a politician (blue crosses in Figure 2), we make use of a combined matching and dynamic difference-in-differences (DiD) design which exploits variation in political connections within firms over time. Specifically, we follow Imai et al. (2021) and match *treated* firms before the start of the political connection to *control* firms without a connection based on observable firm characteristics such as age, employment structure, legal form, industry affiliation and region. Most importantly, we advance the identification of the nexus between firms and politicians by exploiting the detailed information in the MUP data on the composition of the firm's leadership team. As the data allow us to observe the composition of the board at each point in time such that we can select control firms with a similar leadership structure. In particular, we condition on control firms that appoint a new member to the leadership team in the same year and in the same position as the treated firm and that have the same number of overall entries and exits from the leadership team in this year.

Comparing firm outcomes of both groups of firms before and after the start of a political connection in a DiD setting, allows us to identify the effect of the event "becoming a politically connected firm" on different outcome variables over time. Identification relies on two assumptions. First, we assume that absent the political connection outcomes would have followed similar trajectories in treated and control firms. To justify this assumption, parallel trends in firm-level outcomes prior to the start of the connection are crucial and we will incorporate recent advances in the DiD literature to show

robustness of our findings (De Chaisemartin & d'Haultfoeuille 2020; Callaway & Sant'Anna 2021). Second, we assume that firms do not anticipate the start of the political connections for example, by strategically appointing a politician to the leadership team when the firm benefits most from this connection. This assumption is hard to test empirically, which is why we rely on the second identification strategy based on the discontinuity around marginal seats on election lists. The firm-level responses caused by the appointment events are discussed in Section 6.

5.3 Regression Discontinuity Design

To exploit discontinuities in election results around the marginal seat on the party list, we apply a regression discontinuity design (RDD). The intuition follows the literature on close elections (Lee 2008) which is based on the assumption that the winner and the runner up in a close election are as good as randomly assigned. In our case, we compare the firm with a connection to the candidate on the marginal seat who wins a mandate with the firm that has a candidate on the next seat who does not enter parliament. The discrete running variable in our design is thus defined as the rank of the candidate on the party list (where rank 1 corresponds to the top ranked candidate) minus the marginal seat.[22] The main identification assumption is that there is no manipulation around the marginal seat on the party list. We argued in Section 3 that it is hard to predict the marginal seat due to the interaction of the first and second vote in German elections. In addition, we will show that predictions based on election polls are very imprecise. Further, we will discuss standard density tests and balancing checks of pre-election firm and politician characteristics. Detailed results of the RDD analysis are presented in Section 7.

6 Impact of Appointing a Politician on Firm-Level Dynamics

6.1 Matching

Selection of Treated Firms. In the following, we concentrate on treated firms defined as firms appointing a current or former politician to an executive position in the years 2001-2019 for which we additionally observe at least one pre-treatment year in order to be able to compare outcomes before and after treatment. By applying these sample restrictions, we end up with 1,514 treated firms which form the appointment sample (columns (2) and (3) of Table 2). In our empirical analysis, we allow for multiple treatment events per firm. In most cases, however, the appointment event represents a single event at the firm level: in 88% of the cases a firm appoints only one (current or former) politician during the whole observation period.

Selection of Control Firms. The construction of a sample of comparable control firms involves selection steps. First, we keep only firms for which at least two yearly observations are available.

[22] An alternative to our RDD approach with a continuous running variable is the approach proposed by Folke (2014) which measures the distance to the seat threshold as the minimum total vote change across all parties that would be required for a party to experience a seat change. See also Fiva & Smith (2018).

Next, we perform a successive pre-selection of firms based on observable characteristics such as industry, region, size, and firm board composition to obtain a manageable size of the control group (for details on the variables and the pre-selection, see Table C.1 and the description thereof in the Appendix). Most importantly, we require the control firms to employ individuals in one of the top executive positions and that an entry or exit in the respective job position takes place as observed for treated firms in the treatment start year t. All in all, this results in a sample of 274,610 potential control firms. To further ensure comparability of treatment and control group, we perform propensity score estimations.[23]

Propensity Score Estimators. We directly match on the year of appointment and then estimate the propensity of having a political connection separately for each year based on probit regressions controlling for a set of pre-treatment observables: firm age in groups, sales in $t-1$ and $t-2$ (including missing category), employment in $t-1$ and $t-2$ (including missing category), the general existence of specific job positions (CEO/owner, executive board, supervisory board and other) as well as the number of entries and exits in the respective position in t, firm type such as LLC, stock corporation and association, 1-digit industry and state fixed effects. Appendix Table C.2 contains a definition of the control variables used in the propensity score estimation. In a second step, we use the inverse propensity score weighting (IPW) to weight control observations by the inverse of the fitted values of propensity scores.

6.2 Validity Checks

Table C.3 in the Appendix compares the means of firm characteristics for firms that appoint a politician and control firms that appoint non-politician in the same year based on IPW. The last column reports the standardized differences Δ_X between treated and re-weighted control firms as a scale-free measure of balancing.[24] Since there is no universally agreed criterion for how small the standardized difference must be to provide balance, we lean on the rule of thumb of $\Delta_X < |0.1|$ as suggested by Austin (2011). The standardized differences point to no significant differences between treatment and control group after IPW.[25]

With the matching approach, we aim to find a comparable control group for which the parallel trends assumption is likely to hold. This assumption is, however, only partly testable. Appendix Figure C.1 shows that the employment dynamics of treated and weighted control firms follow a

[23] We apply further sample restrictions on the treatment and control group to adequately analyze the effects of appointing a politician in a dynamic setting. In particular, we only keep firms for which the connection starts before 2018 and the respective control firms in order to observe at least one post-treatment period. Moreover, we condition on observing employment information in at least two periods before treatment.

[24] The standardized difference is defined as $\Delta_X = \left(\bar{X}_1 - \bar{X}_0\right) / \left((S_1^2 + S_0^2)/2\right)^{0.5}$, where $\bar{X}_w$ is the sample mean of treated ($w = 1$) or control ($w = 0$) firms and S_w^2 are the respective sample variances (Austin 2011). The advantage of Δ_X over the usual t-statistic is that it does not mechanically increase with the sample size and therefore avoids exaggerating small imbalances that would still appear significant in a t-test.

[25] In order to guarantee sufficient overlap between treatment and control group, we drop 24 treated firms with propensity score values above the maximum value of the control firms (Lechner & Strittmatter 2019). These are mainly extremely large stock corporations where we have difficulties to find suitable control firms (see Appendix Table C.3).

similar trend before the start of the treatment event. Moreover, we see that employment decreases continuously in treated firms in the pre-treatment period and this trend is only reversed in the post-treatment period. Hence, we do not find an indication for anticipation in the treatment group.

6.3 Estimation Results

Market Exit. We start our analysis by studying the impact of appointing a politician on market exit and estimate the following equation separately for each year τ after the start of the political connection:

$$y_{i,\tau} = \alpha_\tau + \gamma_\tau I(T_i = 1) + \delta_c + \varepsilon_{i,\tau}, \tag{1}$$

where $y_{i,\tau}$ reflects the probability of exiting the market within τ years after treatment start, with τ ranging from 1 to 5. δ_c indicates calendar year fixed effects and $\varepsilon_{i,\tau}$ the idiosyncratic error term. Equation (1) is weighted by the inverse propensity score.

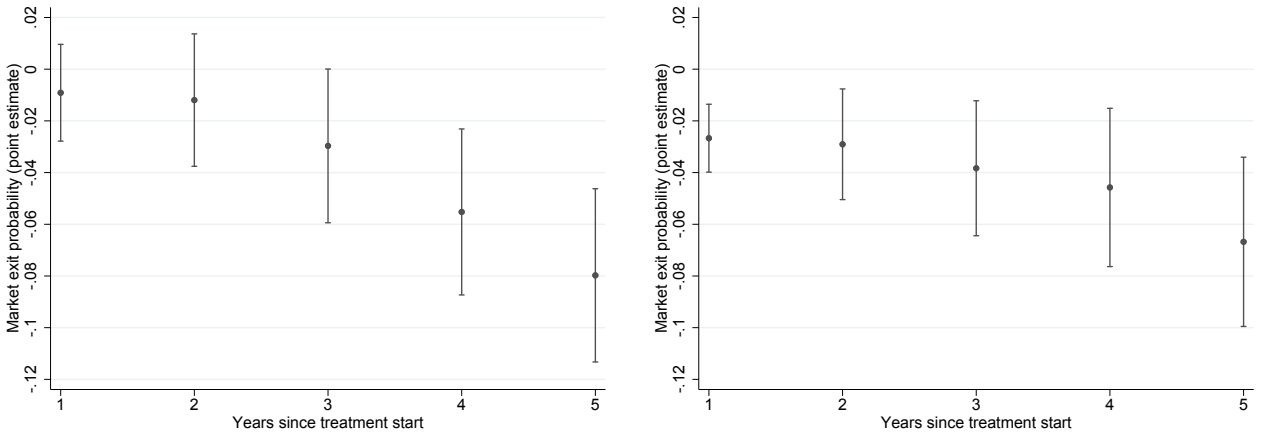
Figure 3 shows the effects of appointing a current or former politician on the probability of exiting the market within one to five years after treatment start. One year after treatment start, firms that appoint a current politician have the same exit probability as unconnected firms. Those firms that appoint a former politician have a significantly lower exit probability: the probability to leave the market within the next year is reduced by 2.7% points compared to the control group. Over time, however, both types of connections come along with a reduced market exit rate. Five years after treatment start, the effect on the probability to have left the market amounts to 8.0% points for firms that appoint a person with a current political mandate and 6.7% points for firms that appoint a person whose political mandate has already ended.

In order to compare our estimated effects of appointing a current or former politician in terms of magnitude with the effects of gaining or losing political connections in a RDD framework in Section 7, we present in the following condensed estimates on firm outcomes two years after the appointment event. Table 3 on page 23 shows in column (1) the estimated coefficient of γ_2 of equation (1). Compared to the sample mean of control firms, the estimated coefficient is large in magnitude: the probability to exit the market within two years decreases by about 35% for firms that appoint a former politician.

Employment Dynamics. Appendix Figure C.1 shows the development of employment levels in firms appointing a politician and in the weighted sample of control firms. The figure suggests that all groups of firms are on a declining track prior to the start of the new appointment. After the event, this trend is reversed in both treated and control firms. However, the number of employees increases more strongly in connected firms over the following years.

To quantify the effect on employment, we estimate the following event study model:

$$y_{i,t} = \alpha_i + \sum_{\tau \neq -1} \beta_\tau I(t = \tau) + \sum_{\tau \neq -1} \gamma_\tau I(t = \tau) I(T_i = 1) + x_{i,t} + \delta_c + \varepsilon_{i,t}, \tag{2}$$



A: Appointing a current politician

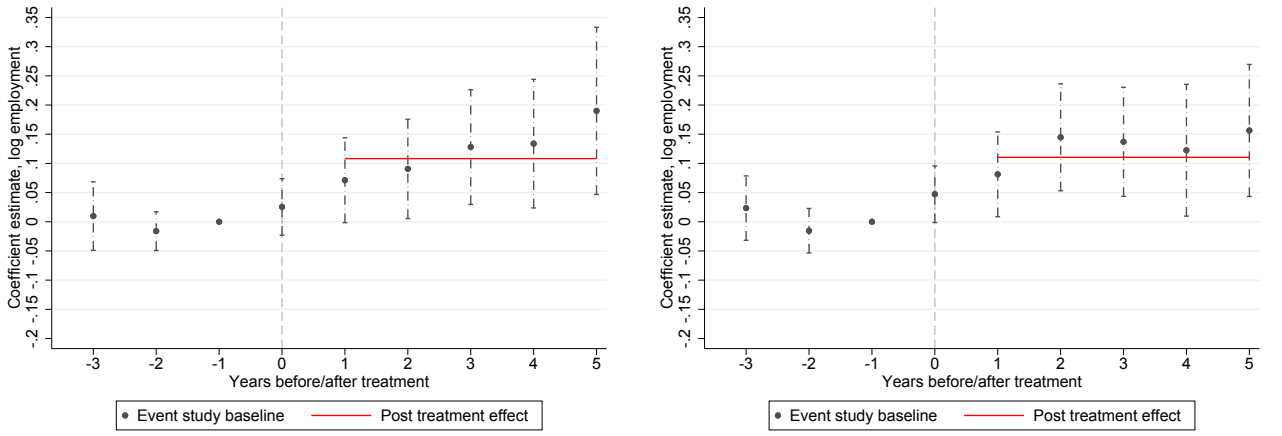
B: Appointing a former politician

Notes: The figure shows the effect of appointing a current (Panel A) or former (Panel B) politician at time *t=0* on market exit one to five years after the appointment event. Market exit is equal to *1* if the firm is exiting the market within *t=τ, τ=1 to 5*, years after the appointment. Estimates are based on equation (1). All regressions are weighted by the inverse propensity score. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint a person in *t=0*. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

Figure 3: The effect of appointing a politician on market exit

where $y_{i,t}$ is the log employment level of firm i at time t, α_i represents firm fixed effects, T_i the treatment dummy which takes the value 1 if the firm is connected to a politician and 0 otherwise, $x_{i,t}$ captures time-varying firm characteristics (in our specifications we include dummies for firm age), δ_c calendar year fixed effects and $\varepsilon_{i,t}$ the idiosyncratic error term. τ runs from -3 to 5 covering the time period of nine years, the point in time 0 indicates the start of the treatment. For a fixed point of time τ, β_τ is the average value of outcomes for the control group relative to the reference period (conditional on fixed effects) and γ_τ the average difference between the treatment and the control group at that point in time.

Figure 4 shows the effect of appointing a current (Panel A) or former (Panel B) politician on log employment based on equation (2), again weighted by the inverse propensity score. First to note is that we do not observe any pre-treatment trends between the treatment and the weighted control group in both panels. One year after appointing a politician, however, the employment levels start to diverge. For both types of connections, appointing a current or a former politician, we document a positive and significant effect on employment growth of around 0.08 log points. Two years after treatment start, the effects become larger for firms that appoint a former politician: employment size increases significantly by 0.15 log points conditional on staying in the market as compared to 0.09 log points for firms that appoint a current politician. From year two onward, the estimated coefficient becomes larger for firms that appoint a person who currently holds a political mandate: in year five after treatment start it amounts to 0.19 log points. For firms that appoint a person whose political mandate has already ended, the estimated coefficient remains on the level of about 0.15 log points. Overall, this results in an average change of 0.11 log points between the pre- and post-period for both appointment events (see column (2) in Appendix Table C.5).



A: Appointing a current politician B: Appointing a former politician

Notes: The figure shows the effect of appointing a current (Panel A) or former (Panel B) politician at time *t=0* on log employment in $t=\tau$, τ*=-3 to 5*. The black point estimates are based on equation (2). The regression is weighted by the inverse propensity score. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint a person in *t=0*. The red bar gives the effect on the average change in log employment between the pre-period = -3 to 0 and the post-period 1 to 5. The number of observations with employment information are shown in Figure C.2. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

Figure 4: The effect of appointing a politician on employment dynamics

Table 3 shows in column (2) the coefficient estimate of $\beta_{\tau=2}$ based on equation (2) as well as the estimated effect on the growth rate between the year before the appointment and two years after in column (3) separately for firms appointing a current (Panel A) or a former (Panel B) politician. In both specifications, we document a positive and significant impact of appointing a politician on employment. In terms of size, the coefficient in Panel A corresponds to 3.4% of the mean of log employment of control firms in $t = 2$ and to 5.4% in Panel B, respectively. In terms of the employment growth rate from $t = -1$ to $t = 2$, results show 8.6% points higher growth in Panel A and 11.1% points higher growth rates of connected firms in Panel B.

Productivity Dynamics. Finally, we provide evidence on productivity dynamics relating firm-level sales figures to employment. Specifically, we look at labor productivity defined as the logarithm of *sales/employment*. Given the documented positive impact on employment growth and reduced market exit, productivity dynamics can be informative about the underlying growth process in the firms. For example, a decrease in firm-level productivity would be indicative about the selection process of the firms and potentially point to dynamic inefficiency induced by the connection.

Columns (4) and (5) of Table 3 show the estimated coefficients for the appointment of a current or former politician on the firm-level productivity measure after two years. Estimates for the effect of appointing a politician on productivity are not significantly different from zero, which suggests that the induced survival advantage and employment growth process is not driven by significant productivity improvements. Instead, the observed sales moves in proportion with firm-level employment.

Together, our results suggest that the entry of an active and former politician into executive ranks

Table 3: The effect of appointing a politician on market exit, employment and productivity

	Market exit	Employment		Labor productivity	
	at $t+2$	Event Study $\beta_{\tau=2}$	Growth $t-1 \rightarrow t+2$	Event Study $\beta_{\tau=2}$	Growth $t-1 \rightarrow t+2$
	(1)	(2)	(3)	(4)	(5)
Panel A: Appointing a current politician					
Connection	-0.012	0.091**	0.086**	-0.090	-0.034
	(0.013)	(0.043)	(0.034)	(0.065)	(0.037)
Mean in *t=2*	0.084	2.694	-0.038	12.457	-0.052
Control observations in *t=0*	125,050	125,050	125,050	125,050	125,050
Treated observations in *t=0*	400	400	400	400	400
Panel B: Appointing a former politician					
Connection	-0.029***	0.145***	0.111***	0.021	0.067*
	(0.011)	(0.047)	(0.036)	(0.063)	(0.036)
Mean in *t=2*	0.084	2.694	-0.038	12.457	-0.052
Control observations in *t=0*	125,050	125,050	125,050	125,050	125,050
Treated observations in *t=0*	466	466	466	466	466

Notes: The table shows the effect of appointing a current (panel A) or former politician (panel B) at time *t=0* on market exit, employment and labor productivity two years after the appointment event. Labor productivity is defined as log(sales/employment). $\beta_{\tau=2}$ is based on equation (2). The growth rate is calculated between the year before the appointment and two years after. Market exit is equal to *1* if the firm is exiting the market within two years after the appointment. All regressions are weighted by the inverse propensity score. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint a person in *t=0*. The mean refers to the outcome level of the control group measured two years after the appointment. Standard errors are clustered at the firm level. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

might improve firm outcomes measured by lower market exit rates, higher employment with constant labor productivity, which implies an increase in sales. Appointing former politicians, who are more experienced and potentially devote more time to the firm, show stronger firm-level improvements with a marginal significant increase in labor productivity.

6.4 Robustness Checks

In this section, we test the robustness of our findings with respect to different empirical specifications and samples. Specifically, Appendix Tables C.4, C.5 and C.6 show the market exit, employment and productivity results based on 5 nearest-neighbor matching and concentrating on the first treatment only. In addition, the employment and productivity effects are also provided for a sample with employment information in at least one pre-treatment year. Figure C.3 in the Appendix further presents the employment results based on the estimator proposed by De Chaisemartin & d'Haultfoeuille (2020) and weighted by the inverse propensity score weights. Overall, our estimated coefficients are robust to different sample sizes and estimation approaches.

7 The Impact of Winning and Losing Political Connections

7.1 Empirical Specification

Party List Discontinuities. Our election data cover information on all political candidates from the six national elections between 1998 and 2017. Among the six parties that were represented in

parliament at least once, we observe in total 8,690 political candidates (13,002 candidate-year observations).[26] To exploit discontinuities in the probability of entering parliament around the marginal seats on party lists, we pool all available lists and center them at the marginal seats. In total, we have party lists from 16 states, six elections and six political parties which results in 576 potential discontinuities. The effective number of discontinuities is lower, however, because not all parties have candidates in each state and each election and from some party lists no candidate enters parliament. Furthermore, not all party lists include a candidate with an executive position in a firm. We further drop all lists from which only the first or second candidate enters parliament. This is because we want to have a balanced sample of observations around the marginal seat. In addition, the top seat on the list is often very strategically chosen and dropping those reduces concerns about manipulation (see Section 7.2). Our final sample thus exploits 254 discontinuities and consists of 2,233 firm, candidate, election year observations from 1,694 unique firms and 947 individual candidates.[27]

Fuzzy RDD. Due to the first vote in German national elections where candidates can also win a direct seat, the firm's connection status in not fully determined by the marginal seat on the party list. Therefore, we adopt a fuzzy RDD approach for analyzing the effect of a connection to parliament on firm outcomes. In particular, we specify local linear models for the first stage, estimating the jump in the firm's probability of being connected at the marginal seat cutoff and the reduced form estimating the change in the firm outcomes at the cutoff (Hahn et al. 2001). Specifically, our first stage model takes the following form:

$$connect_{i(m)} = \pi \mathbb{1}(placement_{i(p)} \geq marginal_{i(p)}) + g(\cdot) + \lambda_m + \epsilon_i, \tag{3}$$

where $connect_{i(m)}$ is a dummy variable equal to 1 if firm i's candidate with party affiliation p wins a political mandate for the next election term in election year m. The cutoff value is determined by the marginal seat at the election year and party level. The indicator function therefore is equal to 1 for all firms connected to candidates placed at the marginal seat or better. The function $g(\cdot)$ is a continuous linear function of the normalized relative placement of the candidate that allows a slope change above the cutoff:

$$\begin{aligned} g(threshold_{i(p)}) = g_0 + g_1(marginal_{i(p)} - placement_{i(p)}) + \\ g_2(marginal_{i(p)} - placement_{i(p)}) \times \mathbb{1}[(placement_{i(p)} \geq marginal_{i(p)})] \end{aligned} \tag{4}$$

Similarly, the reduced-form specification models the jump in the outcome variable as:

$$y_{i(m+k)} = \delta \mathbb{1}(placement_{i(p)} \geq marginal_{i(p)}) + h(\cdot) + \lambda_m + \kappa_i, \tag{5}$$

where $y_{it(m+k)}$ refers to firm performance of firm i, k years after election m and $(placement_{i(p)} \geq marginal_{i(p)})$ is equal to 1 if the firm is connected to a successful candidate. The function $h(\cdot)$ is a

[26] Throughout the covered time period, the government was formed by a coalition of two parties. These coalitions were formed as follows. 1998: Social Democrats and the Greens; 2002: Social Democrats and the Greens; 2005: Christian Democrats and Social Democrats; 2009: Christian Democrats and Liberals; 2013: Christian Democrats and Social Democrats; 2017: Christian Democrats and Social Democrats.

[27] Appendix Figure D.1 shows the number of observations by the marginal seat and Appendix Table D.2 shows the number of observations for mass points close to the cutoff.

piece-wise linear function analogous to equation (4). All specifications include election year fixed effects, λ_m, and we report results for different sets of control variables, e.g., party and state fixed effects or initial firm-level observables. The causal effect of a connection to the federal parliament among firms with candidates close to the marginal seat is given by $\hat{\beta} = \frac{\hat{\delta}}{\hat{\pi}}$. We estimate model parameters using a standard two-stage least square estimator with the indicator $\mathbb{1}(placement_{i(p)} \geq marginal_{i(p)})$ as the instrument for $connect_{i(m)}$. For baseline results, we provide robust standard errors.

The choice of bandwidth is crucial for the approximation of the conditional expectation function. As our running variable is discrete, we follow in the baseline specification in Card et al. (2023) and report results for a selection of bandwidths. Our baseline results are estimated with a symmetric bandwidth of 6 seats around the marginal seat. For discussion of alternative bandwidth choices see Section 7.4.

7.2 Validity Checks

Density of Observations around the Cutoff. Appendix Figure D.3 Panel A shows the number of (candidate, firm, election year) observations around the marginal seat cutoff, separately for the sample of new candidates and incumbents shown by blue dots and red diamonds, respectively. In this graph, observations to the right of the cutoff are those entering parliament. We can see that for both types of candidates the probability to be placed on a top seat in the list is low but it is increasing the closer one moves towards the marginal seat. To the left of the cutoff the mass remains high for new candidates, who are more likely to be placed on hopeless places on the list. Incumbents running for re-election, are more concentrated around the marginal seat. But importantly, we do not see a spike to the left or a discontinuous drop to the right of the cutoff for either of the two samples. This observation is also confirmed by the McCrary density test which is shown in Panel B of Appendix Figure D.3. These tests support the validity of our research design.

Predicting the Marginal Seat. To further support the random nature of the cutoff, we predict the marginal seat based on polls from *Politbarometer* three months before the election. The reason why we focus on polls three months before is because parties submit their respective party lists around three months before the election, after which the list can typically not be adjusted. Due to the fact that the polls refer to the national-wide party, which does not allow us to differentiate at the state level, we estimate an implied marginal seat. To do so, we calculate the total number of votes based on the polls taking into account the actual number of eligible voters in the election. Specifically, we estimate:

$$\#votes^{poll}_{p(m)} = polls_{p(m)} \times \#actual\ voters_m,$$

where $polls_{p(m)}$ is the predicted share from *Politbarometer* and $\#actual\ voters_m$ is the number of eligible voters in election m. This provides an implied number of votes for each party p in election m based on the polls following the assumption that the number of eligible voters would have been stable. We allocate this overall number to the state level based on the actual share each state received in the

election, which gives an implied number of votes at the party-election-state level. For example, if the CDU in Baden-Wuerttemberg received 10% of all actual votes for the nation-wide CDU during the election in 2017, we allocate 10% of $\#votes^{poll}_{CDU(2017)}$ to Baden-Wuerttemberg. This takes into account that the shares are unequally distributed across Germany. The implied marginal seat is then calculated as the implied number of votes in each state for each party divided by the number of actual votes per seat, which represents a prediction of the marginal seat based on the polls.

Based on this calculation, we find that overall just 39.5% of the actual marginal seats are correctly predicted by the polls. The prediction is typically more precise for party lists from which only few candidates enter parliament. Once we restrict the analysis to election lists for which actual marginal seat is three or higher – as it is the case in our estimation sample – the polls only predict 30.6% of the cases correctly.

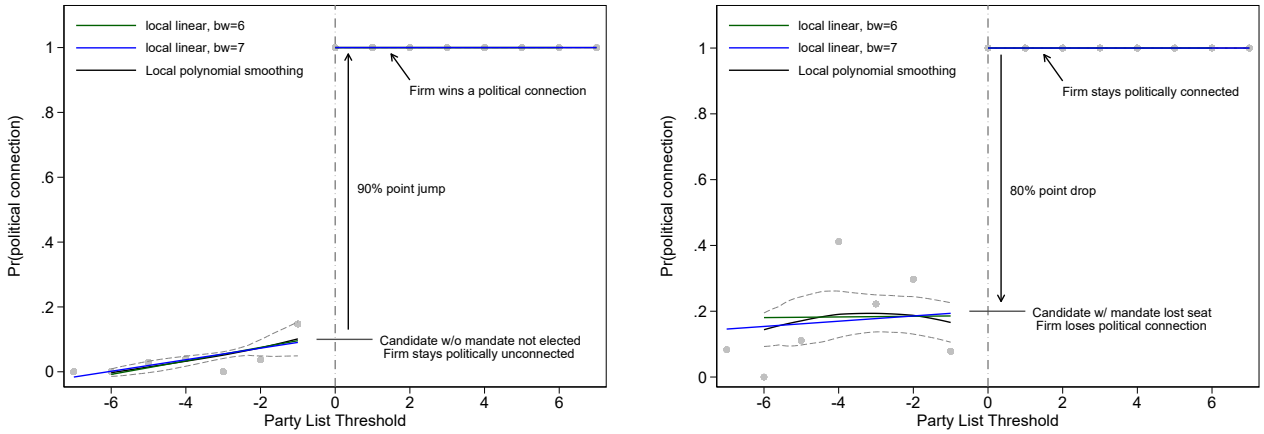
Balancing of Pre-Election Characteristics. In order to support the random nature of the cutoff, predetermined characteristics of firms and political candidates with a position in the firm should balance at the marginal seat cutoff. Table D.3 in the Appendix provides evidence on the balancing of firm- and politician-level characteristics around the cutoff for the two different samples. The first two columns show average differences of observable variables between firms that have a candidate who successfully enters/stays in parliament via the party list. Specifically, each line represents a separate regression of the initial observable characteristic on an indicator equal to 1 if the candidate will enter/stay in parliament in the next election term and 0 otherwise. For the sample of firms that place a candidate without a current political mandate, the table shows that becoming connected in the next term is associated with a larger initial firm size, firm age and legal type (stock corporation). At the candidate-characteristic level, being successful during the election also correlates with age. Thus, firms that will be connected in the next term are different along many observables. For firms connected to a candidate with a current mandate, observable characteristics are rather balanced in the year of the election.

Columns (3) and (4) show differences between the observable characteristics at the normalized cutoff implemented using the reduced form specification in equation (5). The results show that firm-level variables related to the quality of the firm such as size, years since foundation and labor productivity are precisely estimated with point estimates close to zero. Also, the characteristics of the candidates at the cutoff do not differ between candidates at the cutoff and just below. Figure D.4 in the Appendix provides the graphical counterpart of the regression specification in column (3) pooling both samples. The results provide strong evidence for quasi-random assignment of firms to political candidates at the cutoff and support the continuity assumption of potential outcomes.

7.3 Results on Market Exit, Employment Growth & Productivity Growth

First Stage Results. Figure 5 visualizes the jump in the probability of connecting to parliament during the next election term at the cutoff. Due to the normalization at the marginal seat, all firms at *0* and to the right of it will have a political connection during the following parliamentary term. Thus,

the probability of connecting to a current member of the German federal parliament is *1* without any variation. To the left of the threshold, we observe firms with candidates whose placement on the party list is below the marginal seat. For these, we see a large and significant drop in the probability of being in parliament during the following term by 80 and 90 percentage points, depending on whether candidates run for re-election or first-time election, respectively. The probability does not drop to zero, however, because some candidates enter parliament via a direct political mandate by winning their election district. Panel A shows the results for candidates without a current political mandate,



A: Candidate without political mandate B: Candidate with political mandate

Notes: The figure shows the probability of connecting to the federal parliament by the normalized party list placement of the candidates. Panel A provides the results for candidates without a current political mandate at the time of the election. Panel B provides the results for candidates with a current political mandate at the time of the election. Firms connected to candidates at the threshold seat and above will all be connected to parliament in the following term. Firms connected to candidates below the threshold seat have a lower probability of being connected. The green and blue lines represents local linear fits on both sides of the cutoff with 6 and 7 seat bandwidth, respectively. The black line represents a local polynomial smoothing with degree 1.

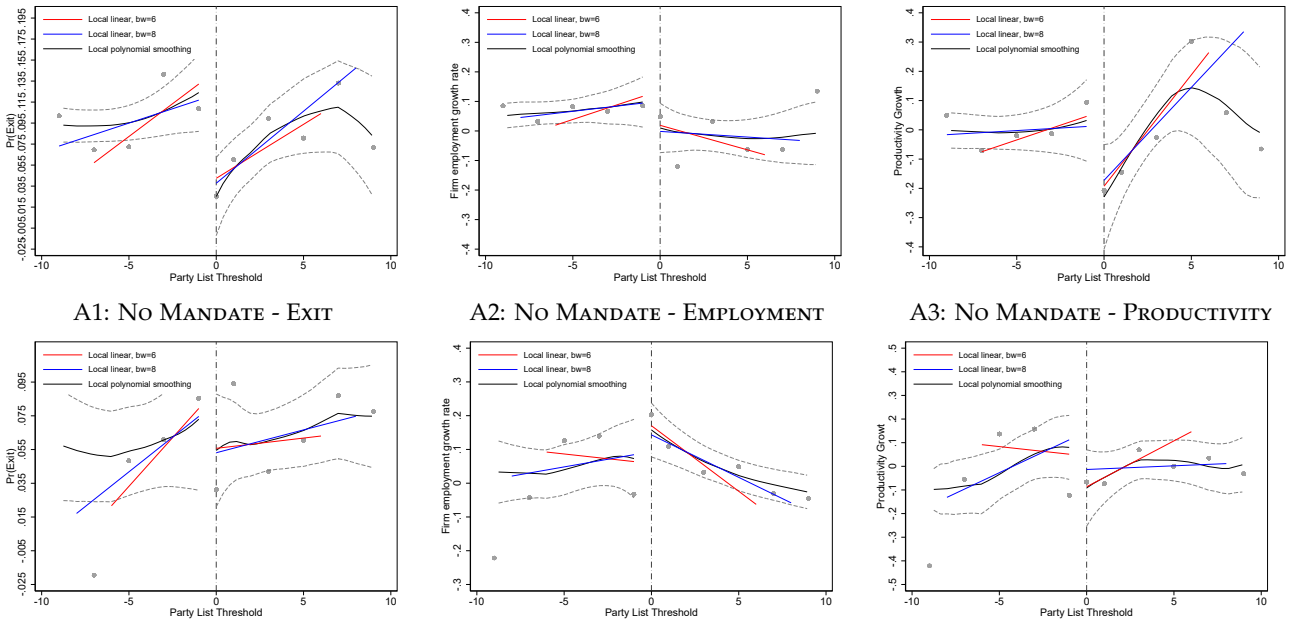
Figure 5: Probability of parliament connection

whereas Panel B shows the results for candidates who run for re-election. First-time candidates who just end up one seat below the marginal seat have a probability of entering parliament of around 10%. Thus, the probability of becoming politically connected jumps by 90% points when moving just one seat above. In turn, politicians with a current mandate have a probability of around 20% if their party list seat is just one seat below the marginal seat. Thus, the probability of staying politically connected drops by 80% points when moving just one seat below the threshold.

Graphical Evidence. We start by providing graphical evidence between the normalized party list placement and firm outcomes after the election in Figure 6. The left panels show market exit two years after the election.[28] The middle panels show employment growth between the year before and two years after the election. Finally, the right panels provide the results on firm productivity growth. To the right of the party list cutoff (vertical dashed line) are firms with a political candidate who is entering parliament with a placement on the party list at the marginal seat or above. To the left of the

[28]Typically four years after each election, a new election takes place. Therefore, focusing on two years after the election provides the best trade-off in terms of sample selectivity and measuring responses at the firm level.

cutoff are firms with a political candidate just below the marginal seat. The dots represent conditional means of the outcome variable. We distinguish between candidates who run for parliament without a current political mandate (Panels A1 to A3) and candidates with a current mandate who want to get re-elected (Panels B1 to B3). The solid black lines display local polynomial regressions with degree 1 using a triangular kernel weighting with the 90% confidence intervals. The red and blue lines represent local linear regressions on each side of the cutoff with different bandwidths.



A1: No Mandate - Exit A2: No Mandate - Employment A3: No Mandate - Productivity

B1: Current Mandate - Exit B2: Current Mandate - Employment B3: Current Mandate - Productivity

Notes: The figure shows market exit, employment growth and labor productivity growth two years after the election against the normalized party list threshold. Panels A1 to A3 show the results for firms connected to candidates without a current political mandate who run for parliament. Panels B1 to B3 shows the results for firms connected to candidates with a current mandate who want to get re-elected. Observations to the right of the party list cutoff (vertical dashed line) represent firms connected to candidates with a placement on the party list at the marginal seat or above. Likewise, negative values depict firms that are connected to candidates who did not won a seat via the party list. The solid black line represents local linear regressions on each side of the cutoff. The dashed lines represent 90% confidence intervals. The dots represent conditional means of the outcomes. The blue and red lines present linear regressions with different bandwidths.

Figure 6: Employment, exit and productivity dynamics after election

Among firms with candidates who win a political mandate, we observe a strong effect in Panel A1 that depicts a large negative jump at the cutoff in market exit rates. Firms at the cutoff or just above have an exit rate of around 2%, whereas firms just below have an exit rate of, on average, 10%. Likewise, Panel A3 shows that the growth in productivity is significantly lower for those firms at the cutoff. Descriptive results for firms that lose a political connection (Panels B1 to B3) are more imprecise and do not hint to discrete jumps at the cutoff.

Fuzzy RDD Results. Next, we turn to illustrate the estimates of the fuzzy RDD described in Section 5.3. Table 4 reports the benchmark results on market exit, employment growth and productivity growth after two years for connections to candidates without a current mandate and candidates who

run for re-election, i.e., candidates with a current political mandate. For better interpretation, we code the treatment indicator equal to 1 if a candidate without a current mandate wins a mandate (0 otherwise). For candidates with a current political mandate, we code treatment equal to 1 if the candidate loses the current mandate. Treatment therefore represents a change of the baseline state.

Each specification in Table 4 is presented including election year fixed effects. Panel A provides first stage estimates. For firms connected to a candidate without a current mandate, the probability of entering the national parliament jumps at the threshold c by about 90% points. In contrast, firms with a political connection in the election year experience a drop in the probability of staying connected by about 80% points. The F-Statistic refers to the Kleibergen-Paap F-Statistic and takes in most specification values above 100. Thus, weak identification issues do not apply in our setting.

Table 4: Fuzzy RDD results - employment, market exit and productivity

	Winning a mandate "Staying unconnected (=0) vs. winning (=1)"			Losing a mandate "Staying connected (=0) vs. dropout (=1)"		
	Market exit at $t+2$	Employment growth $t-1 \to t+2$	Lab. prod. growth $t-1 \to t+2$	Market exit at $t+2$	Employment growth $t-1 \to t+2$	Lab. prod. growth $t-1 \to t+2$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First stage						
Indicator	0.917***	0.849***	0.821***	-0.797***	-0.676***	-0.652***
	(0.0245)	(0.0472)	(0.0552)	(0.0463)	(0.0817)	(0.0881)
Panel B: 2SLS						
Indicator	-0.117***	-0.198	-0.425*	0.0478	-0.294	0.311
	(0.0421)	(0.133)	(0.224)	(0.0506)	(0.187)	(0.256)
Mean at threshold	0.0270	0.0690	-0.161	0.0571	0.139	-0.0762
Mean one seat below	0.0946	0.134	0.0350	0.0962	-0.0582	-0.0768
Initial firm size	64.95	69.74	69.74	154	160.3	154
Observations	733	314	277	624	278	252
F-Statistic	1399	322.5	220.9	296.3	68.47	54.81
Bandwidth h	6	6	6	6	6	6

Notes: The table presents fuzzy RDD estimates from local linear regression discontinuity specifications. Outcome variables are market exit at $t+2$, employment growth from $t-1 \to t+2$ and labor productivity growth from $t-1 \to t+2$ relative to the election years. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. The outcome means in the middle part of the table refer to averages for firms that enter parliament in the next term at the threshold seat and for firms that are out of parliament just one seat below the threshold. Robust standard errors are shown in parentheses. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

In terms of market exit, Panel B reports a highly significant reduction in the probability of exiting the market at the cutoff by about 12% points (column (1)) for firms that win connections as compared to staying unconnected. Comparing firms that stay connected versus parliament dropouts shows no statistical significant effect on market exit (column (4)). One reason for the lower absolute point estimate among the latter group is that connected firms at the time of the election are with around 150 employees more than twice as large as firms that "run" for a mandate. Employment growth results shown in columns (2) and (5) are statistically not different from zero. These results correspond to two years after the election. Appendix Table D.4 Panel A presents the results one and three years post election and shows that the results on market exit are persistent over the next three years. The results on employment growth in Appendix Table D.4 Panel B are, however, more nuanced. While employment dynamics do not differ immediately one year after the election, losing political

connections seems to cause a downsizing process in the medium run.

The results, in particular on market exit, are not informative on selection or the lack of selection of firms. Results in column (3) indicate a decline in productivity growth among firms that win a seat in parliament as compared to firms that stay unconnected. This result is consistent with the fact that the pool of survivors is of lower productivity due to the fact that the political connection causes firms to survive (lower market exit). It indicates a lack of selection among firms that gain political connections because productivity dynamics would have suggested to absorb less resources had these firms not been connected to political power. This result is consistent with Akcigit et al. (2023) who argue that investments in political connections may reduce the cost of regulatory frictions and political connections are used to defend their position in the market.

Together with the dynamic difference-in-differences results, we argue that a new appointment to the executive team increases the quality of the management and subsequently improves firm outcomes. In the RDD specification, we do not observe positive employment responses. The point estimates even indicate lower employment growth. This is in line with our expectations, as management quality should not improve if the candidate is already part of the firm's executive team. If anything, the new mandate might induce the politician to devote less time to the firm, which might result in lower management quality. We show in Section 8.4 that candidates who run for re-election and lose are more likely to leave the firm. The results for employment (although not significant) are thus consistent with a negative change in management quality.

7.4 Robustness Checks

Covariates and Bandwidth Selection. We provide a set of robustness checks. Table D.5 in the Appendix provides robustness checks with respect to included covariates. Including party and state fixed effects, firm age and firm size at the time of the election, and the function of the politician in the firm does not alter the empirical findings. Appendix Table D.6 shows results with different bandwidth selections. We provide specifications with relative bandwidths of 50%, i.e., depending on the absolute number of the marginal seat, we allow the bandwidth to move 50% of that number to the left and to the right. Independent of the bandwidth choice, our results proof to be robust.

Optimal Bandwidth Selection. In a next step, we perform robustness based on optimal bandwidth selection criteria. Table D.7 in the Appendix illustrates robustness to the choice of the kernel function and bandwidth selection criteria. For example, Panel A of Table D.7 using MSE-optimal bandwidth selection and a triangular kernel shows in column (1) a similar point estimate of -0.12 as the baseline specification in column (1) of Table 4 Panel B, suggesting lower market exit for winning a connection as compared to staying unconnected.

Local Randomization. To provide local randomization evidence, we first manually select the estimation window. Specifically, we estimate $\mathbb{E}[Y_i(1)|threshold_+] - \mathbb{E}[Y_i(0)|threshold_-]$, where $threshold_+$ represents the marginal seat (and one seat above due to low sample size) and $threshold_-$ is the seat

up to two seats below the marginal seat. Appendix Table D.8 shows the results for the group of firms that win access. The local randomization results presented in Panel A are rather close to our preferred baseline specification in Table 4. Firms becoming political connected compared to firms that just stay out by two party list seats have a lower probability to exit and have lower labor productivity growth rates. Likewise, firms that just drop out have no differential effect on firm performance.

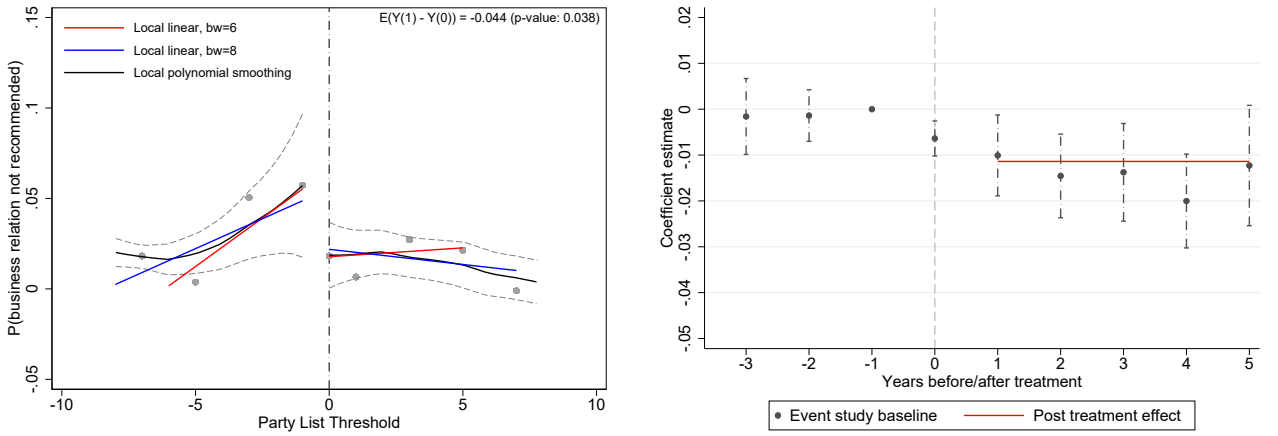
Placebo. Table D.9 in the Appendix further conducts placebo results by analyzing employment and productivity dynamics before the election. Point estimates are small and insignificant providing evidence that these firms perform similarly before the election. Alternatively, we run randomization inference (Appendix Figure D.5) in order to overcome potential imprecision problems (Young 2019). Specifically, we follow Fouka & Voth (2016) and perform 2,999 random permutations of the dependent variable and the baseline model for each permutation. This approach reshuffles the dependent variable and randomly assigns an outcome to each firm. To calculate p-values, we combine these with the non-permuted estimates. Figure D.5 in the Appendix confirms the results with a higher precision.

8 Mechanisms

In this section, we provide evidence on potential mechanisms by exploiting and linking further data sources. In particular, we guide our discussion along credit indicators, economic subsidies and public procurement contracts, as well as analyzing the results by sub-samples. Lastly, we study job displacement events of candidates after the election as potential explanations for the documented firm dynamics.

8.1 Credit Ratings

In order to construct an indicator that serves as a proxy for access to credit, we rely on a dummy variable equal to 1 (and 0 otherwise) if engaging a business relation is not recommended and collateral is needed. Figure 7 shows the results for the election and appointment samples. We pool the groups mainly for sample size reasons and provide results by sub-groups in Appendix Figure E.1 and Panel A of Table 5. Firms that appoint a politician (Panel B of Figure 7) experience a sudden decrease of around -1.5% points in the probability that a business relation is not recommended. This effect is observed for both, current and former politicians (Appendix Figure E.1).Panel A of Figure 7 shows the pooled RDD results (pooling candidates with and without a current mandate). Firms with candidates just one seat below the marginal seat have a 4.4% points lower credit rating two years post election. Although with lower precision, Panel A of Table 5 provides RDD estimates separately for both groups. Point estimates become insignificant but point towards similar directions, i.e., a drop when winning a mandate relative to staying out and an increase when losing a mandate relative to staying in. These results suggest that connected firms experience easier business conditions with respect to collateral requirements.



A: Pooled RDD B: Pooled Event Study

Notes: Panel A shows the credit rating two years after the election against the normalized party list threshold pooling candidates with and without a current mandate who run for parliament. Observations to the right of the cutoff (vertical dashed line) represent firms connected to candidates with a placement on the party list at the marginal seat or above. Likewise, negative values depict firms that are connected to candidates who did not won a seat via the party list. The solid black line represents local linear regressions on each side of the cutoff. The dashed lines represent 90% confidence intervals. The dots represent conditional means of the outcomes. The blue and red lines present linear regressions with different bandwidths. Panel B present the effect of appointing a current or former politician at time *t=0*. The regression is weighted by the inverse propensity score. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint a person/non-politician in *t=0*. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

Figure 7: Political connections and credit rating

The related literature documents that politicians use state-owned banks as vehicles to achieve their goals. Sapienza (2004) demonstrates that state-owned banks in Italy provide reduced interest rates compared to private banks. This discrepancy is particularly noticeable when the political party linked to a specific company holds more influence in the region where the company obtains its loans. Similarly, Khwaja & Mian (2005) reveal that businesses with political affiliations in Pakistan encounter less difficulty in securing credit from government-affiliated banks. The German institutional setting of bank lending has a strong political component, in particular, at the local level. Englmaier & Stowasser (2017) show that German savings banks increase overall lending in the run-up to county elections. Koetter & Popov (2021) document that savings banks increase lending to the state government if the party in power at the state level changes. Evidence for developing countries shows that such political components in bank lending are associated with lower bank profitability (Micco et al. 2007; Shen & Lin 2012). We document a market reaction of connecting to political power as shown by improved credit ratings that can improve the financing situation and thereby help firms to stay in the market. This result is consistent with a market signaling channel. Political connections (or the loss of connections) enhance (reduce) firm's reputation as a reliable business partner and this reputation becomes visible in improved credit ratings. This signaling mechanism likely generates improved actual credit conditions and may have immediate short-run effects on firm survival.

Table 5: Results - credit rating, subsidies & procurement

	RDD, $t+2$ Election sample		Event Study, $\beta_{\tau=2}$ Appointment sample	
	Without current political mandate	With current political mandate	Appointing a current politician	Appointing a former politician
	(1)	(2)	(3)	(4)
Panel A: P(Business relation not recommended)				
Indicator	-0.0554	0.0678	-0.0121*	-0.0164***
	(0.0471)	(0.0596)	(0.0074)	(0.0055)
Observations	277	261	944,494	944,972
Mean	0.0265	0.0134	0.0293	0.0293
F-Statistic	301.5	60.52	-	-
Panel B: P(Subsidies)				
Indicator	0.0113	-0.0073	0.0138**	0.0051
	(0.0122)	(0.0164)	(0.0068)	(0.0101)
Observations	733	624	971,356	971,894
Mean	0.0165	0.0181	0.0303	0.0303
F-Statistic	1398	296.2	-	-
Panel C: P(Public Procurement)				
Indicator	0.0383**	-0.0187	0.0036	0.0007
	(0.0193)	(0.0310)	(0.0045)	(0.0062)
Observations	399	311	795,004	795,435
Mean	0.0078	0.0096	0.0099	0.0099
F-Statistic	372.7	158.7	-	-

Notes: The table presents fuzzy RDD estimates from local linear regression discontinuity specifications with bandwidth selection of 6 in columns (1) and (2). All specifications control for the outcome variable measured before the election. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. Robust standard errors are shown in parenthesis. Columns (3) and (4) present the effect of appointing a current or former politician at time *t=0*. $\beta_{\tau=2}$ is based on equation (2). All regressions are weighted by the inverse propensity score. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint a person/non-politician in *t=0*. The mean refers to the outcome level of the control group measured two years after the appointment. Standard errors are clustered at the firm level. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

8.2 Subsidies & Public Procurement Contracts

Panels B and C of Table 5 show the results of receiving subsidies and public procurement contracts in the election and appointment sample. To do so, we construct indicators if the firm receives subsidies or public procurement contracts. Appointing a politician with a current political mandate (column (3)) increases the probability of economic subsidies by 1.4% points. This effect amplifies over the post-election period as shown by Panel A of Appendix Figure E.2. The point estimate among appointments of former politicians is insignificant and close to zero. These results are in line with evidence from France, where firms with connected CEOs but without a current political mandate do not received more subsidies (Bertrand et al. 2018). Winning a political connection (column (1) in Panel A) shows similar but insignificant point estimates, whereas losing a current political mandate shows a negative and close to zero coefficient. These results suggest that becoming connected to a politician with a current mandate likely creates higher levels of subsidies. Relative to the baseline subsidy share of 1.6 to 3%, gaining a political access seems to be an economically significant impact. Our results suggest that direct access to parliament through a politician with a current mandate does generate

preferential treatment with respect to subsidies and is consistent with an intervention mechanism.

Panel B shows the results with respect to receiving public procurement contracts. Appointing a current or former politician does not affect the probability to receive procurement contracts. Column (1) shows that winning a political mandate as compared to staying out increases the probability of public procurement contracts by 3.8% points. This result again points to an intervention mechanism that helps the firm staying in the market. Together with lower labor productivity dynamics, our results suggest that part of the newly founded connections generate distortive market outcomes by intervening in the market in favor for the firms.

8.3 Heterogeneity Results

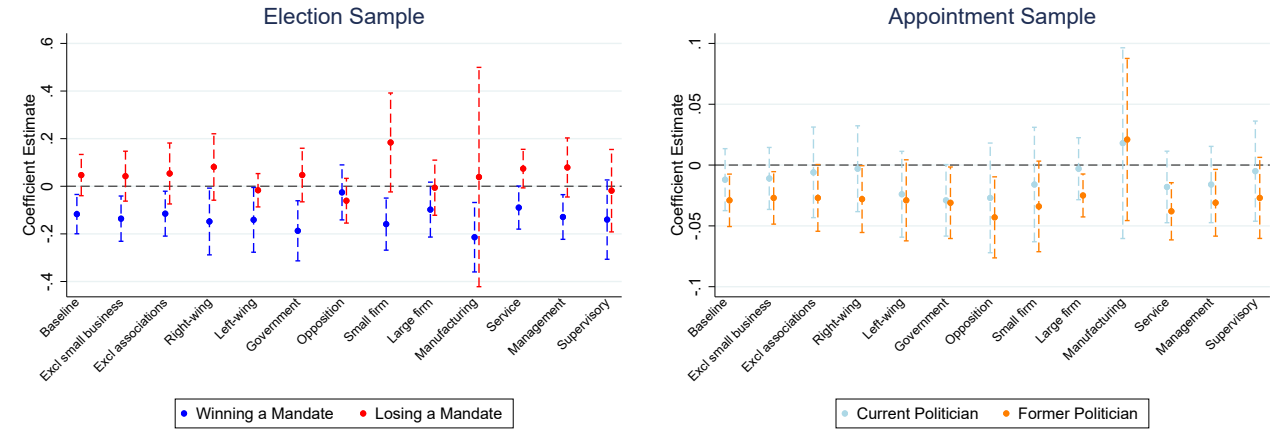
We present heterogeneous effects by sub-samples graphically showing point estimates along with 95% confidence intervals. Figure 8 shows the market exit results. Panel A provides the RDD specification based on the election sample, whereas Panel B show results based on the appointment sample. We refer to Figure E.4 in the Appendix for employment and productivity results by sub-samples.

We first test whether specific legal firm types such as small businesses and associations defined in Table 1 are driving the documented effects by excluding these legal forms from the sample. Point estimates remain very stable suggesting that these firm types are not primarily driving the effects.

Second, we provide evidence distinguishing between party affiliation (right/left-wing) and government-/opposition-party connections.[29] The figures first show that the documented exit and employment effects are not particularly driven by right- or left-wing connections but are present for both affiliations. What matters is the connection to the government versus opposition party when the firm appoints a politicians with a current mandate. The drop in the probability to exit the market when winning a mandate is driven by connections to the government party, whereas the point estimate for opposition affiliation is close to zero and insignificant (Panel A of Figure 8). Employment responses after appointing a former politicians are observed for both, affiliations to the government and opposition party (Panel B1 of Appendix Figure E.4).

The following sample splits provide heterogeneous estimates by initial firm size (below/above median), broader sector affiliation (manufacturing/service) and politicians' management or supervisory function in the firm. Overall, RDD results on market exit and productivity are observed throughout the sample splits with slightly higher point estimates for smaller firms and firms in the manufacturing sector. Losing a political mandate as compared to staying connected generates higher firm exit for smaller firms and firms in the service sector (both marginally significant). Appointing a current politician in a managing role as opposed to supervisory drives the employment response (Panel B1 of Appendix Figure E.4).

[29] Unless one party has the absolute majority of mandates in parliament, a coalition of parties form the government. Connections of parliamentarians to firms might influence the formation of a governmental coalition. For this reason, the distinction between government-/opposition-party connections potentially suffers from endogeneity issues.



A: RDD, $t+2$ B: Event Study, $\beta_{\tau=2}$

Notes: The figure presents in panel (A) fuzzy RDD estimates from local linear regression discontinuity specifications with bandwidth selection of 6 on market exit for sub-samples. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. Robust standard errors are shown in parenthesis. Panel (B) presents the effect of appointing a current or former politician at time *t=0*. $\beta_{\tau=2}$ is based on equation (2). All regressions are weighted by the inverse propensity score. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint in *t=0*. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

Figure 8: Market Exit Effects by Sub-Samples

8.4 Job Displacement & New Appointments

Not becoming (re-)elected might have consequences for the political candidates which causes the firm to stay unconnected or to become unconnected to parliament during the next election term. These candidates might lose their jobs in the firm as well or might even be replaced by a new politician with a current mandate. We test this by relating start and end dates of the positions within the firms. Table E.1 in the Appendix shows the results on the probability of job displacement in the year after the election in columns (1) and (2). We do not find any significant effects in the sample of candidates who want to gain a political mandate (Panel A). For politicians who want to become re-elected, Panel B provides point estimates that are significant for connections to the government in the next term. When losing the seat in parliament, politicians whose party affiliation aligns with the government are also more likely to lose their appointment in the firm. Although our evidence on new appointments is statistically rather imprecise (columns (3) and (4)), these firms might take action to replace the politician and remain connected. This observation might provide a rationale why, in our sample, losing a political mandate has no robust effect on firm outcomes.

9 Conclusions

Efforts to regulate the interplay between political and business power rank high on the political agenda, especially in countries with highly developed institutions. It is, however, not clear whether the regulatory efforts are fully successful, as the literature documents examples of preferential treatment and unfair advantage among companies who are closely connected to political agents. We study this question in Germany which is known for low levels of corruption and a strict legal framework

of disclosure policies, while close interaction between politicians and firms is highly prevalent at the same time.

We construct a novel database from multiple administrative sources to document firm-level connections to parliamentary politicians at the highest level of the German government and document that involvement of parliamentarians in corporate governance of German companies has almost doubled over the last two decades. The database and empirical designs further provide an unique setting to analyse the contribution of political connections on firm outcomes and distinguish between potential mechanisms.

We exploit the timing of political mandates and firm-level positions for two identification designs which corroborate the causality of the estimated effects. Our findings highlight that, even in the highly regulated setting, politicians in leadership positions have an impact on firm dynamics. Political connections shield connected firms from closure in the subsequent years, enhance employment dynamics, while leaving productivity growth mainly unaffected. Our results are consistent with the fact that new appointments have a positive impact on the firm's management quality. The new connections of firms also send positive market signals, which are reflected in improved credit ratings. However, we also document evidence of market interventions in favor of the connected firms.

Our results contribute first pieces of empirical evidence to an active public debate that was ignited by lobbying activities of prominent German politicians in an environment of rapidly changing international relations. Lobbying activities to secure preferential treatment of single and often large firms can lead to lower productivity improvements when connected firms shield away from competition. In this way, the whole business environment might be affected through decisions of unconnected firms to innovate, exit or enter the market, potentially disrupting the process of creative destruction. Thus, these connections may carry large risks for the whole economy.

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Supplementary Appendix

A Literature Overview

Table A.1: Studies on Political Connections and Firms Dynamics

Study	Country	Connection	Method	Main Outcomes	Effects
A.1. Connections via Active Politicians to National Government					
Babenko et al. (2023)	U.S.	Corporate executives elected to federal office	Event study	Stock return	↑
Green & Homroy (2022)	UK	Members of Parliament as directors or consultants	DiD	Stock return	↑
Acemoglu et al. (2018)	Egypt	Firm's major shareholders or board members belong to incumbent regime	Event study	Stock return (during protests)	↓
Niessen & Ruenzi (2010)	Germany	Current national politicians with paid jobs	DiD	Stock return	↑
Faccio (2006)	35 countries	Large shareholder or top officer is member of parliament or minister	Event study	Stock price	↑
Faccio et al. (2006)	35 countries	Large shareholder or top officer is member of parliament or minister	Event study	Bailout / Leverage	↑ / ↑
A.2. Connections via Active Politicians to Local Government					
Akcigit et al. (2023)	Italy	Current local politicians employed in firm	RDD Cox / FE	Employment / LP Exit / TFP / VA	↑ / → ↓ / ↓ / ↑
Koetter & Popov (2021)	Germany	Savings banks headed by local politicans	DiD	Lending / Default post election	↑ / ↑
Englmaier & Stowasser (2017)	Germany	Savings banks headed by local politicans	DiD	Lending / Default pre election	↑ / ↑
Cingano & Pinotti (2013)	Italy	Current local politicians employed in firm	Firm FE	Sales / Productivity	↑ / →
Khwaja & Mian (2005)	Pakistan	Directors running for office at national or provincial level	Firm FE	Loans / Default rate	↑ / ↑
B.1. Connections via Former Politicians to National Government					
Goldman et al. (2013)	U.S.	Former national political position as board member	DiD	Procurement	↑
Goldman et al. (2009)	U.S.	Former national political position as board member	OLS	Stock return	↑

C.1. Indirect Connections to National Government					
Brown & Huang (2020)	U.S.	White House visits of Executives	OLS PS Matching	Stock return Procurement	↑ ↑
Schoenherr (2019)	Korea	Members of president's network appointed as CEO in SOEs	Firm FE	Sales/ Investment/ Loans/ Procurement	↑ / ↑ / ↑ / ↑
Akey & Lewellen (2017)	U.S.	Donation to congressional candidates	triple Diff	ROA/ Sales/ Investment/ Volatility/ Leverage	↑ / ↑ / ↑ / ↓ / ↓
Akey (2015)	U.S.	Firm donation to congressional candidates	RDD	Equity return	↑
Leuz & Oberholzer-Gee (2006)	Indonesia	Firms with connections to former president	OLS/IV	Foreign securities	↓
Johnson & Mitton (2003)	Malaysia	Officers with close relationships to key government officials	OLS	Stock price	↑
Fisman (2001)	Indonesia	Connection Index	OLS	Stock price	↑
C.2. Indirect Connections to Local Government					
Choi et al. (2024)	U.S.	Firm donations to candidates	Election yr FE	Grant / Employment	↑ / ↓
Bertrand et al. (2018)	France	CEO with government connection supports local politician	Election yr FE	Employment / Sales pre election Taxes / Subsidies	↑ / → → / →
Haselmann et al. (2018)	Germany	CEO in same service club as local politicians	Event Study DiD	Loans/ Investment	↑ / ↓
Amore & Bennedsen (2013)	Denmark	Firms with family ties to local current politicians	DiD	OROA/ Sales/ Cash/ Leverage	↑ / ↑ / ↑ / →
Duchin & Sosyura (2012)	U.S.	Directors with current or former political positions	OLS	Government funds	↑

Note: Abbreviations: LP: Labor Productivity, TFP: Total Factor Productivity, VA: Value Added, ROA: Returns on Assets, OROA: Operating Return on Asset.

B Data Addendum

B.1 Firm-Level Data

The basis of our firm-level data is the *Mannheim Enterprise Panel (MUP)*, a panel dataset generated and hosted by the ZEW – Leibniz Centre for European Economic Research. The data stem from *Creditreform e.V.*, the largest credit rating agency in Germany. Besides the official Business Register of the Federal Statistical Office, the *MUP* is the most comprehensive micro database of companies in Germany with full coverage of all firms starting in 2000.[30] Due to sample size restrictions, we make use of a 50% random sample of all available firm identifiers and construct a yearly firm panel dataset between 2000 to 2019. The latest available year is 2019. However, Table B.1 shows that coverage significantly declines for the last year in the sample, for the other years the number of observations ranges between 580 and 770 thousand.

Table B.1: Number of observations over time

	Observations	Obs. with labor	Obs. with sales
	(1)	(2)	(3)
2000	667,793	504,362	494,169
2001	700,587	535,613	530,044
2002	719,318	603,557	601,588
2003	731,622	621,824	619,111
2004	745,198	633,774	630,501
2005	754,134	643,828	639,258
2006	761,797	652,075	645,924
2007	766,215	656,756	647,294
2008	767,501	656,838	641,003
2009	768,663	656,025	631,941
2010	767,694	656,552	627,392
2011	765,838	657,242	621,324
2012	756,435	650,505	613,655
2013	742,473	637,092	599,045
2014	726,169	618,332	578,863
2015	704,442	592,822	550,936
2016	676,366	562,880	517,154
2017	640,017	535,294	489,598
2018	579,116	492,219	436,540
2019	209,228	188,742	153,059

Notes: The table reports the number of observations between 2000 and 2019. Column (1) reports the total number of observed firms per year. Columns (2) and (3) report numbers conditional on non-missing labor and sales observations in each year, respectively.

The MUP contains a large number of firm characteristics. Most importantly, we observe firm size, total sales, the industry affiliation at the five-digit industry code according to NACE rev. 2, the legal form, the number of patents[31] as well as the date of incorporation and closure. The mean values of log employment, log sales and firm age over time are presented in Table B.2. In addition to the performance-related information, we are able to exploit detailed regional information on the firm (municipality code), the shareholder structure and personal information on the involved individuals. In particular, we observe, at the individual level, information on the function of the individuals in the

[30] Bersch et al. (2014) provide detailed information on data collection, processing and the definition of variables.

[31] Patent information are provided by the ZEW and represent a merge from PATSTAT to the firm via record linkage. Doherr (2016) provides a detailed discussion about the heuristic approach with an application to patent data and inventor mobility across firms.

firm, e.g., owner, CEO, supervisory or executive board member.

Table B.2: Firm-level averages over time

	Log employment (1)	Log sales (2)	Firm age (3)
2000	1.45	13.00	21.45
2001	1.43	13.06	21.86
2002	1.37	13.08	22.34
2003	1.34	13.12	22.89
2004	1.32	13.12	23.36
2005	1.31	13.11	23.81
2006	1.31	13.12	24.27
2007	1.31	13.14	24.58
2008	1.32	13.15	24.62
2009	1.33	13.17	23.18
2010	1.34	13.18	25.65
2011	1.36	13.19	26.63
2012	1.39	13.20	27.06
2013	1.42	13.23	27.38
2014	1.45	13.28	27.68
2015	1.49	13.33	27.96
2016	1.55	13.39	28.20
2017	1.64	13.46	28.57
2018	1.71	13.57	28.98
2019	1.85	13.67	28.24

Notes: The table reports means between 2000 and 2019.

B.2 Merging Political Candidates to Firms

The merge between the *Bundestag* candidates and the MUP data requires four major steps. Table B.3 shows an overview of the merging procedure. We have baseline information on the full name (first and second name) as well as the birth date of the candidates (precise birth date is available for all candidates with at least one political mandate, whereas the data only provide information on the year of birth for candidates without any mandate) and firm representatives (e.g. CEO, advisory board members, partner), respectively (see Panel A of Table B.3). We use these information to identify individuals in the person data file of the MUP.[32] In total, the MUP person file contains 9.5 million individuals. Panel B of Table B.3 shows that the record linkage of both datasets generates a match for 1,489 politicians and 7,232 political candidates. Among those who become a member of parliament at some point, only 5 individuals are found twice in the MUP environment with the same name and date of birth, whereas for 999 candidates we find multiple matches. The reason is that the birth date information of candidates only contains the year of birth, which results in matches that cannot be distinguished from each other.

With the identified unique linked individuals, we move on to the ownership part of the data infrastructure which identifies for each individual one or several links to firm IDs, including information on the timing and the type of the job (Panel C of Table B.3).[33] Among the politicians, we observe 4,882 firm IDs, whereas among the candidates who did not enter parliament, 14,864 firm IDs

[32] After name cleaning in both datasets, we use Stata's `reclink2` command with first name, last name and birth information to combine the datasets with a minimum linkage score of 0.97.

[33] Precise start and end dates in the data are often missing. We are able to make use of each wave (bi-annual) to approximate the start and end year of the connection.

Table B.3: Merging political candidates to firms

	Politicians (1)	Candidates (2)
Panel A: Baseline information		
Election years	1949-2017	1998-2017
Name information	Fist & last name	Fist & last name
Birth date information	Date of birth	Year of birth
Number of individuals	4,084	16,631
Panel B: Record linkage to person file		
Linked to MUP person file	1,489	7,232
Multiple matches	5	999
Panel C: Ownership file		
Identified firm IDs	4,882	14,864
Panel D: Firm panel file		
Panel firm IDs	4,666	14,078

are identified.[34]

The firm IDs can then be linked to the firm-level panel of the data infrastructure. This part contains, among others, information on employment, sales and the credit rating score. Among both individual groups – politicians and candidates – around 95% of the identified firm IDs are observed in the panel file of the MUP. At this stage, the linked IDs of firms to political candidates are unrelated to start and end dates of the political mandate of the individual and the position in the firm.

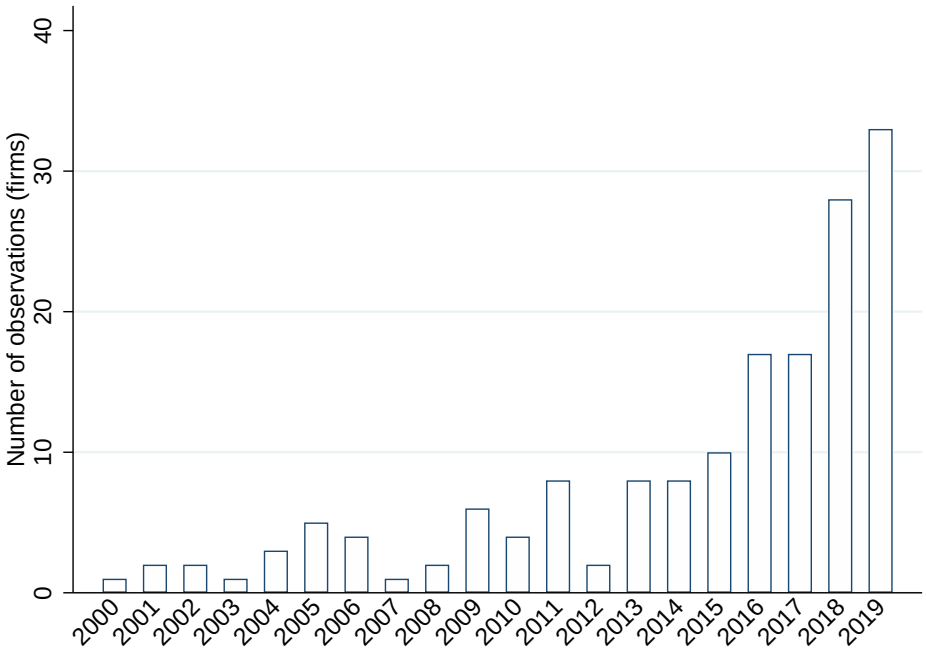
We then perform several further steps of selection. We start with the 1,484 politicians identified in the person file of the MUP and drop firms, if (*i*) the firm is exiting the market before the political mandate starts. This reduces the sample of politicians to 1,446. We then (*ii*) drop observations because the firm is exiting before 1998, which reduces the sample by 156 firms and 26 politicians. Although the 50% random sample of unconnected firms starts in 2000, we apply the year 1998 for politically connected firms because of the election in 1998. This allows us, for example, to calculate the share of politicians with outside activities for the election term 1998-2002. These two major selection steps generate a sample of politician-firm connections for 1,420 politicians and 4,374 unique firms. At the politician-firm level, we observe 5,554 connections due to multiple spells.

In order to construct a panel of connected firms, we move on to the firm panel file of the data infrastructure. For 95 firms, we have no information in the panel part of the data which reduces the firm sample to 4,279 firms. For the remaining firms, we can define treatment status by taking into account the start and end dates of the jobs at the firm and the political mandate. Conceptionally, connections are either simultaneous, meaning that the job at the firm and the political mandate overlap (at least partly), or activities in parliament and in the firm are strictly distinct. Figure 2 of the paper provides a graphical representation. We mainly distinguish between three groups to identify the start of the political connection. The start of a political connection can occur by appointing a current or former politician. A firm can further become connected if it places a member of the firm as a political candidate. There exists a fourth group of firms, where the firm member drops out of the firm and then enters parliament.

Following this treatment definition, for 436 firms the connection period is fully censored in the

[34]There are 235 firm IDs where – at some point – a politician and a candidate is connected to.

panel file of the MUP, i.e. no observations are available. After dropping these firms, we have 3,842 firms left.[35] An additional number of 67 firms drop out either in 1998 or in 1999, leaving us with 3,755 unique connected firms between 2000 and 2019.



Notes: Figure shows the number of firms with only one observation in the panel dataset (N=170). *Source:* Firm panel sample.
Figure B.1: Only one year observed

Table B.4 provides an overview on the number of observations over the sample period between 2000 and 2019 and the number of unique firms. Overall, our sample contains 1.28 million firms with almost 14 million firm $\times$ year observations (column (1)). Columns (2) and (3) provide the same information conditional on having at least one non-missing employment entry (column (2)) or non-missing employment and sales entry (column (3)). Conditional on non-missing information, the sample size reduces only slightly by about 2.3% when considering the number of unique firms, whereas the reduction is 20.5% in terms of firm $\times$ year observations.

[35] We also drop one firm that is the only firm in the industry classification "Organizations". 170 firms are only observed once in the yearly panel dataset. Figure B.1 shows the distribution over the observation window. The majority of the single observations happen at the end of the observation window; with 28 firms only in 2018 and 33 firms only in 2019. In total, 759 treatment start observations are left-censored, i.e. the year of the start of the treatment is not observed in the data.

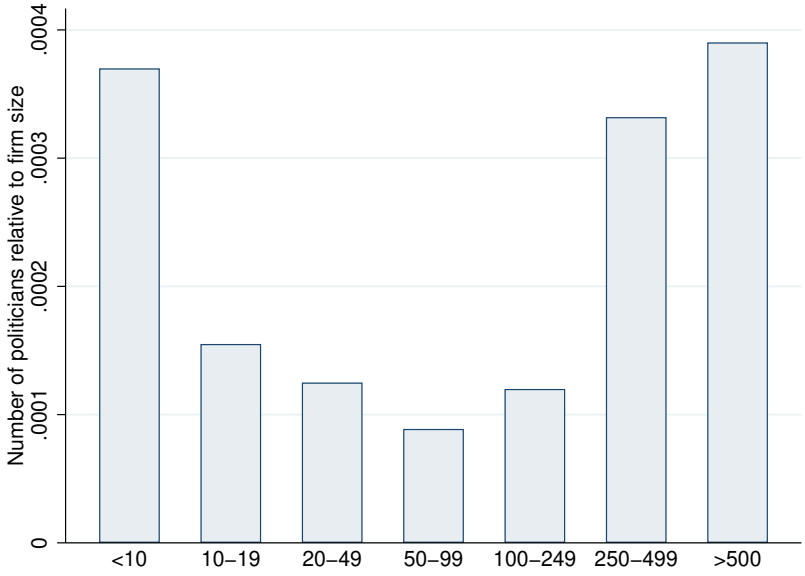
Table B.4: Number of observations by sample

	All firms	Firms with employment	Firms with employment & sales
	(1)	(2)	(3)
Panel A: Full sample			
First year	2000	2000	2000
Last year	2019	2019	2019
Firm × year observations	13,950,606	11,089,519	10,440,870
Unique firms	1,284,033	1,255,931	1,226,806
Panel B: Political connected firms			
Connected firm × year obs.	24,093	16,969	15,810
Unique connected firms	3,755	3,077	2,957

Notes: The table reports the number of observations between the start and the end of the sample period which corresponds to the time span 2000-2019. The firm × year observations refer to total number of available observations. Unique firms are the number of firms in the dataset. Panel A shows the observations for the full representative sample. Panel B shows the observations for the sample of firms with a political connection. Among the politically connected firms, 67 drop out before 2000. For this reason, the number of unique firms become 3,755 instead of 3,842.

Table B.5: Definition of treatment events

Type	Description	Comparison
Appointing a current politician	Firm appoints a person who has currently a political mandate.	Firms appointing current politician vs. firms appointing non-politician
Appointing a former politician	Firm appoints a person who had a political mandate.	Firms appointing former politician vs. firms appointing non-politician
Candidate becomes elected	Political candidates with a position within the firm without a current mandate runs for *Bundestag*.	Firms with successful vs. unsuccessful candidates in election
Politician gets re-elected	Political candidates with a position within the firm and with a current mandate runs for re-election.	Firms with successful vs. unsuccessful candidates in election



Notes: The figure shows the connection intensity over the firm size distribution. We estimate the connection intensity at the firm level by calculating the number of connected politicians relative to the number of employees for each year and, in a second step, average this intensity measure at the firm level.

Figure B.2: Political connected firms over the firm size distribution

C Additional Empirical Results of Appointing a Politician

Matching Control Firms to Connected Firms

Selection of Treated and Control Firms. Treated firms are defined as firms that appoint a former or current politician. We additionally require to observe at least one pre-treatment year per firm. This results in 1,524 observations. As we perform exact matching on the year of appointment, we also include firms that place a candidate that becomes elected as treated firms for the matching to increase treated observations and to be able to run separate propensity score estimations per year. This group consists of 535 firms that employ a person in a leading position before the election.

In order to construct a sample of comparable control firms, we implement several selection steps. First, we keep only firms for which at least two observations are available. Next, we perform a successive pre-selection of firms based on observable characteristics such as industry, region, size, and firm board composition (see Table C.1). Specifically, our data situation allows us to a priori exclude firms that do not possess certain characteristics. First, we exclude 5-digit industries as well as labor market regions that are not represented by treated firms in the pre-treatment year. In the next step, we only keep firms that belong to the same broad employment size category in the pre-treatment year and experience similar dynamics in the firm board composition in the following year as treated firms. To be precise, we require the control firms to employ CEOs, owners, executive and supervisory board members, partners and main shareholders as well as that an entry or exit in the respective job position takes place as observed for treated firms in the treatment start year t. All in all, this results in a sample of 274,610 potential control firms.[36]

Table C.1: Definition of variables for pre-selection

Pre-treatment variables measured in year $t-1$	
Employment groups	Number of employees of the firm in groups: *1* "$\leq$9 employees"; *2* "9-49 employees"; *3* "50-249 employees"; *4* ">249 employees"; *5* "Number of employees is missing"
Job composition in t	Incidence, entry and exit of job positions: *1* "Owner"; *2* "CEO"; *3* "General partner"; *4* "Executive board"; *5* "Supervisory board"; *6* "Partner"; *7* "Stille partner"; *8* "Main shareholder"
Sector type	5-digit industry of the firm
Labor market regions	254 labor market regions based on firms municipality identifier following the definition of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR)

Notes: t denotes the year of treatment start.

Propensity Score Estimators. We estimate the propensity of having a political connection separately for each year based on probit regressions controlling for a set of pre-treatment observables: firm age in groups, sales in $t-1$ and $t-2$ (including missing category), employment in $t-1$ and $t-2$ (including missing category), the general existence of specific job positions (CEO/owner, executive

[36] As some treatment starts included in the matching do not come along with a new appointment (see above), we randomly chose the same proportion of control firms as observed treated firms without an entry and include also them in the matching.

board, supervisory board and other) as well as the entry and exit in the respective position in t, firm type such as LLC, stock corporation and association, 1-digit industry and state fixed effects. Table C.2 contains a definition of the control variables used in the propensity score estimation.[37]

Table C.2: Definition of variables for propensity score estimation

Pre-treatment variables measured in year $t-1$	
Age groups	Age of the firm in groups: *1* "$\leq$1 years"; *2* "2-5 years"; *3* "6-15 years"; *4* "16-30 years"; *5* "31-75 years"; *6* ">75 years"; *7* "Age missing"
Log employment in $t-1$ and $t-2$	Log number of employees of the firm in $t-1$ and $t-2$
Log sales in $t-1$ and $t-2$	Log sales of the firm in $t-1$ and $t-2$
Job composition in t	Incidence, entry and exit of job positions: *1* "CEO/Owner"; *2* "Executive board"; *3* "Supervisory board"; *4* "Other";
Firm type	Firm type: *1* "Limited liability company (*Gesellschaft mit beschränkter Haftung*, GmbH)"; *2* "Stock corporation (*Aktiengesellschaft*, AG)"; *3* "Association"; *4* "Firm type is other business or missing"
Sector type	19 indicators for 1-digit industry of the firm
State	16 states based on firms municipality identifier

Notes: t denotes the year of treatment start.

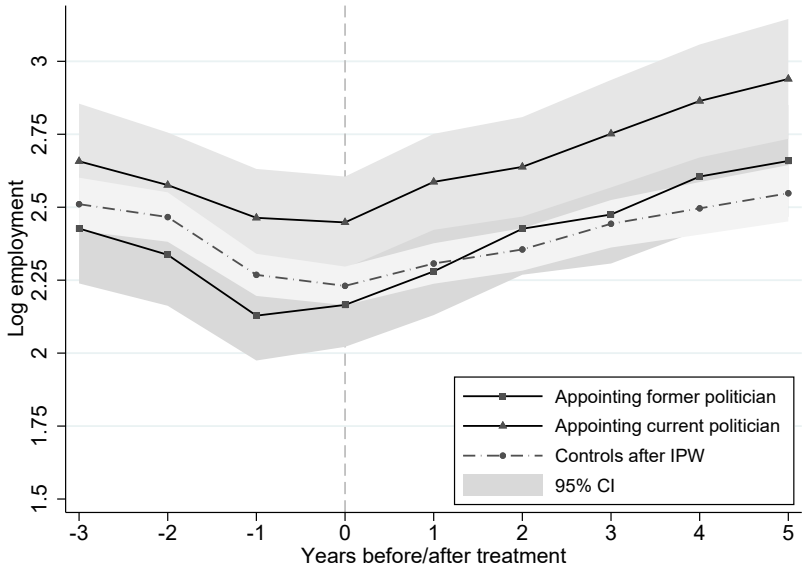
[37] We restrict the set of control variables in case of very low cell occupancy. Low cell occupancy is defined as observing for a dummy variable the value *1* for no treated firms at all, a share of treated firms that is above 95% or below 5% and the number of treated firms equals at least 4 or a share of treated that is above 90% or below 10% and the number of treated firms is less than 4.

Table C.3: Balancing of treatment and re-weighted control group with a new appointment in t

	Treated		Controls			
	On support	Off support	Un-weighted	Re-weighted	*P*-value	Std. Diff.
Firm age	31.120	45.792	24.489	29.628	0.253	0.042
Log employment	2.322	5.657	1.286	2.317	0.954	0.003
Employment missing	0.232	0.083	0.147	0.229	0.767	0.008
Log employment in t-2	2.482	6.086	1.408	2.507	0.814	-0.013
Employment in t-2 missing	0.360	0.167	0.380	0.357	0.827	0.006
Log sales	14.882	19.554	13.444	14.891	0.936	-0.004
Sales missing	0.257	0.083	0.168	0.258	0.944	-0.002
Log sales in t-2	15.008	19.398	13.711	15.041	0.795	-0.015
Sales in t-2 missing	0.381	0.167	0.398	0.378	0.830	0.006
CEO/owner in t	0.415	0.042	0.772	0.403	0.447	0.023
CEO/owner entry in t	0.256	0.000	0.633	0.249	0.559	0.016
CEO/owner exit in t	0.113	0.000	0.225	0.127	0.121	-0.042
Executive board member in t	0.607	1.000	0.192	0.589	0.245	0.036
Executive board member entry in t	0.507	0.708	0.179	0.468	0.012	0.078
Executive board member exit in t	0.284	0.625	0.086	0.263	0.133	0.048
Supervisory board member in t	0.177	0.958	0.017	0.187	0.463	-0.027
Supervisory board member entry in t	0.165	0.958	0.015	0.174	0.518	-0.023
Supervisory board member exit in t	0.090	0.583	0.007	0.099	0.397	-0.033
Other job position in t	0.315	0.250	0.397	0.321	0.685	-0.012
Other job position entry in t	0.253	0.167	0.273	0.250	0.784	0.008
Other job position exit in t	0.089	0.042	0.094	0.097	0.315	-0.027
Legal type: llc	0.325	0.000	0.533	0.349	0.092	-0.050
Legal type: stock company	0.164	0.958	0.011	0.172	0.563	-0.022
Legal type: association	0.417	0.042	0.165	0.390	0.082	0.057
Legal type: other	0.094	0.000	0.290	0.090	0.619	0.014
Sector: Agriculture, mining	0.004	0.000	0.006	0.002	0.196	0.046
Sector: Manufacturing	0.036	0.042	0.056	0.035	0.956	0.002
Sector: Energy, water	0.027	0.000	0.011	0.020	0.214	0.047
Sector: Construction	0.009	0.000	0.076	0.022	0.000	-0.101
Sector: Retail trade	0.034	0.000	0.093	0.035	0.793	-0.007
Sector: Transportation, storage	0.038	0.250	0.023	0.031	0.191	0.038
Sector: Hotelling	0.015	0.000	0.037	0.008	0.064	0.064
Sector: ICT	0.021	0.042	0.044	0.032	0.009	-0.069
Sector: Banking, insurance	0.066	0.208	0.043	0.081	0.134	-0.059
Sector: Real estate	0.070	0.042	0.078	0.072	0.824	-0.006
Sector: Technical service	0.177	0.333	0.205	0.189	0.276	-0.033
Sector: Business service	0.039	0.000	0.045	0.029	0.094	0.053
Sector: Other service	0.321	0.042	0.128	0.294	0.081	0.059
Sector: Public admin, education	0.024	0.000	0.021	0.018	0.141	0.043
Sector: Social, health	0.077	0.042	0.084	0.076	0.917	0.003
Sector: Other	0.043	0.000	0.051	0.056	0.028	-0.061
Observations	1,490	24	213,541	213,541		

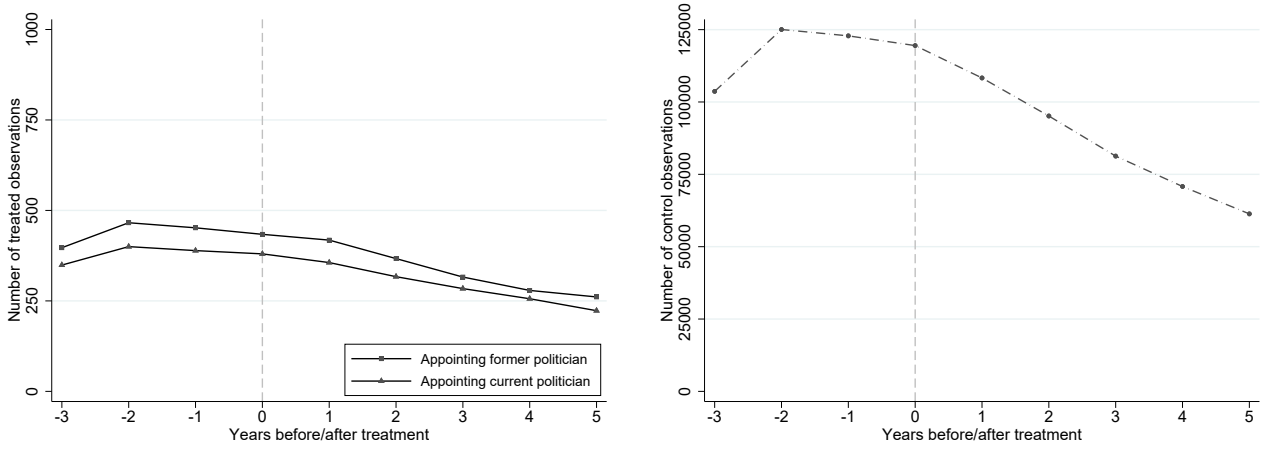
Notes: Std. Diff. = standardized difference. t denotes the year of treatment start. The table shows mean values of pre-treatment (in t-1) firm characteristics for firms that appoint a politician, the un-weighted the re-weighted control group of firms that appoint another person by applying inverse propensity score weighting. The p-values and standardized differences refer to the differences between the treatment and re-weighted control group.

Additional Analyses on the Effect of Appointing a Politician



Notes: The figure shows the log employment levels in $t=\tau$, τ=*-3 to 5* for firms that appoint a politician at time *t=0* and control firms between 2001-2017 conditional on survival and year fixed effects. The control group is restricted to firms that appoint a person in *t=0* and weighted by the inverse propensity score. The propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

Figure C.1: Development of log employment for treatment and control group



A: Treated observations B: Control observations

Notes: The figure shows the number of treated (Panel A) and control (Panel B) observations underlying the effect of appointing a current or former politician at time *t=0* on firm-level log employment in $t=\tau$, τ=*-3 to 5* between 2001-2017 conditional on survival (estimates are presented in Figure 4).

Figure C.2: The effect of appointing a politician on employment dynamics - Number of observations

Table C.4: The effect of appointing a politician on market exit - robustness checks

	Baseline $t+2$ (1)	5NN (2)	First treatment (3)
Panel A: Appointing a current politician			
Connection	-0.012	-0.011	-0.001
	(0.013)	(0.014)	(0.015)
Mean in *t=2*	0.084	0.084	0.084
Control observations in *t=0*	125,050	4,043	125,050
Treated observations in *t=0*	400	400	332
Panel B: Appointing a former politician			
Connection	-0.029***	-0.029**	-0.026**
	(0.011)	(0.012)	(0.012)
Mean in *t=2*	0.084	0.084	0.084
Control observations in *t=0*	125,050	4,043	125,050
Treated observations in *t=0*	466	466	428

Notes: 5NN: 5 nearest neighbor matching. The table shows the effect of appointing a current (Panel A) or former (Panel B) politician at time *t=0* on market exit two years after the appointment event. Market exit is equal to *1* if the firm is exiting the market within two years after the appointment. Estimates are based on equation (1). Regressions in columns (1) and (3) are weighted by the inverse propensity score. Estimates in column (2) are based on 5 nearest neighbor matching. Column (3) includes only the first treatment. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint a person in *t=0*. The mean refers to the outcome level of the control group measured two years after the appointment. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

Table C.5: The effect of appointing a politician on employment - robustness checks

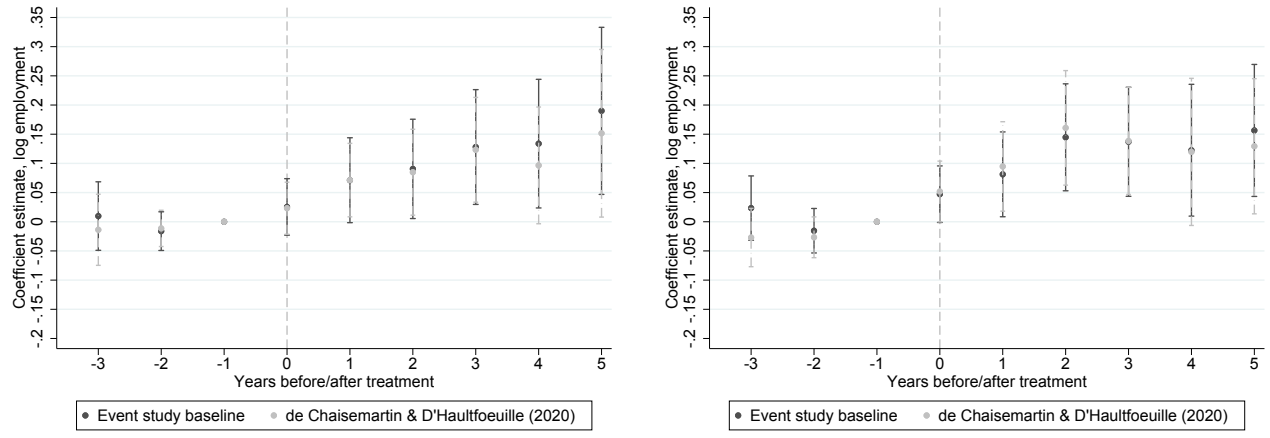
	Baseline $\beta_{\tau=2}$ (1)	DiD (2)	5NN (3)	First treatment (4)	≥ 1 pre-obs of outcome (5)
Panel A: Appointing a current politician					
Connection	0.091**	0.108**	0.074	0.086*	0.048
	(0.043)	(0.044)	(0.045)	(0.049)	(0.039)
Mean in *t=2*	2.694	2.694	2.638	2.694	2.474
Control observations in *t=0*	125,050	125,050	4,043	125,050	179,032
Treated observations in *t=0*	400	400	400	313	493
Panel B: Appointing a former politician					
Connection	0.145***	0.110***	0.132***	0.146***	0.149***
	(0.047)	(0.038)	(0.048)	(0.051)	(0.043)
Mean in *t=2*	2.694	2.694	2.638	2.694	2.474
Control observations in *t=0*	125,050	125,050	4,043	125,050	179,032
Treated observations in *t=0*	466	466	466	419	601

Notes: 5NN: 5 nearest neighbor matching. The table shows the effect of appointing a current (Panel A) or former (Panel B) politician at time *t=0* on employment two years after the appointment event. $\beta_{\tau=2}$ is based on equation (2). Column (2) shows the effect on the average change in log employment between the pre-period = -3 to 0 and the post-period 1 to 5. Regressions in columns (1), (2) and (4) - (5) are weighted by the inverse propensity score. Estimates in column (3) are based on 5 nearest neighbor matching. Column (4) includes only the first treatment. The sample in column (5) is restricted to observing the outcome variable at least once in the pre-period = -3 to -1. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint a person in *t=0*. The mean refers to the outcome level of the control group measured two years after the appointment. Standard errors are clustered at the firm level. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

Table C.6: The effect of appointing a politician on productivity - robustness checks

	Baseline $\beta_{\tau=2}$	DiD	5NN	First treatment	≥ 1 pre-obs of outcome
	(1)	(2)	(3)	(4)	(5)
Panel A: Appointing a current politician					
Connection	-0.090	-0.087	-0.058	-0.071	-0.036
	(0.065)	(0.062)	(0.067)	(0.062)	(0.060)
Mean in *t=2*	12.457	12.457	12.432	12.457	12.466
Control observations in *t=0*	125,050	125,050	4,043	125,050	173,865
Treated observations in *t=0*	400	400	400	313	465
Panel B: Appointing a former politician					
Connection	0.021	0.054	0.045	0.021	0.002
	(0.063)	(0.043)	(0.065)	(0.068)	(0.057)
Mean in *t=2*	12.457	12.457	12.432	12.457	12.466
Control observations in *t=0*	125,050	125,050	4,043	125,050	173,865
Treated observations in *t=0*	466	466	466	419	584

Notes: 5NN: 5 nearest neighbor matching. The table shows the effect of appointing a current (Panel A) or former (Panel B) politician at time *t=0* on labor productivity two years after the appointment event. Labor productivity is defined as log(sales/employment$^{\alpha}$) with $\alpha = 0.7$. $\beta_{\tau=2}$ is based on equation (2). Column (2) shows the effect on the average change in log labor productivity between the pre-period = -3 to 0 and the post-period 1 to 5. In column (3) labor productivity is defined as log(sales/employment$^{\alpha}$) with $\alpha = 1$. Regressions in columns (1) - (3) and (5) - (6) are weighted by the inverse propensity score. Estimates in column (4) are based on 5 nearest neighbor matching. Column (5) includes only the first treatment. The sample in column (6) is restricted to observing the outcome variable at least once in the pre-period = -3 to -1. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint a person in *t=0*. The mean refers to the outcome level of the control group measured two years after the appointment. Standard errors are clustered at the firm level. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.



A: Appointing a current politician B: Appointing a former politician

Notes: The figure shows the effect of appointing a current (Panel A) or former (Panel B) politician at time *t=0* on log employment in *t=*τ, τ*=-3 to 5*. Estimates are based on equation (2). The regression is weighted by the inverse propensity score. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint a person in *t=0*. The light grey results are based on the estimator proposed by de Chaisemartin and D'Haultfoeuille (2020). 100 bootstrap replications are used in the computation of the standard errors. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

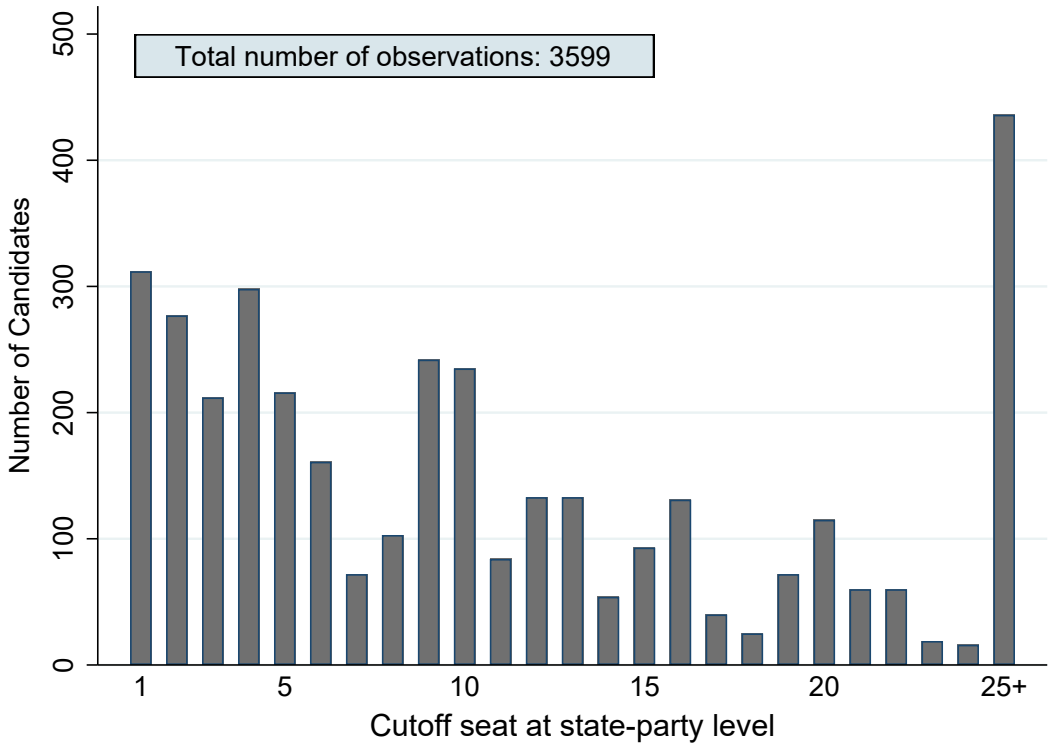
Figure C.3: The effect of appointing a politician on employment dynamics - Estimator by de Chaisemartin and D'Haultfoeuille (2020)

D Additional Empirical Results on Election Discontinuities

Table D.1: Party lists and the marginal seat

	1998/2002		2005/2009		2013/2017	
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
	(1)	(2)	(3)	(4)	(5)	(6)
All parties						
Number of seats	22.669	18.817	22.381	18.048	20.960	17.667
Share no entry through party list	0.300	0.460	0.144	0.352	0.210	0.409
Marginal winning seat	7.536	9.059	5.526	5.535	6.691	7.387
Share elected	0.254	0.159	0.254	0.124	0.305	0.145
Number of connected seats	8.782	6.260	11.010	7.282	12.159	8.389
CDU/CSU						
Number of seats	31.813	20.932	30.750	22.562	30.250	22.519
Marginal winning seat	11.100	10.186	7.000	8.031	14.500	14.100
Share candidate last election	0.392	0.116	0.447	0.111	0.464	0.111
Share elected	0.329	0.132	0.242	0.125	0.391	0.204
Number of connected seats	11.632	7.460	17.543	9.012	20.915	9.081
SPD						
Number of seats	33.094	22.044	28.656	21.376	27.813	21.297
Marginal winning seat	19.200	11.681	9.304	6.885	9.464	7.371
Share candidate last election	0.461	0.143	0.502	0.164	0.383	0.150
Share elected	0.407	0.235	0.284	0.112	0.321	0.129
Number of connected seats	9.688	5.616	11.629	4.844	11.312	4.902
FDP						
Number of seats	21.938	16.779	22.344	17.665	20.625	16.560
Marginal winning seat	3.214	3.035	4.968	4.923	5.333	5.394
Share candidate last election	0.208	0.145	0.265	0.139	0.255	0.161
Share elected	0.130	0.050	0.210	0.099	0.232	0.084
Number of connected seats	7.719	3.665	10.225	4.788	11.691	6.663
Greens						
Number of seats	15.156	13.735	13.625	8.511	17.344	13.985
Marginal winning seat	3.483	3.169	3.774	3.603	4.000	3.919
Share candidate last election	0.204	0.152	0.340	0.148	0.255	0.116
Share elected	0.224	0.102	0.259	0.124	0.210	0.069
Number of connected seats	2.375	1.338	4.316	2.016	6.377	3.235
Left						
Number of seats	11.344	5.672	16.531	9.632	11.906	6.140
Marginal winning seat	3.200	2.394	4.032	2.373	4.094	2.607
Share candidate last election	0.240	0.157	0.221	0.180	0.382	0.208
Share elected	0.239	0.149	0.277	0.146	0.343	0.148
Number of connected seats	3.000	1.832	4.355	2.052	2.969	1.274
AfD						
Number of seats					14.688	8.822
Marginal winning seat					5.875	4.544
Share elected					0.372	0.140
Number of connected seats					5.836	3.072

Notes: The table shows means and standard deviations at the election year – party – federal state level.



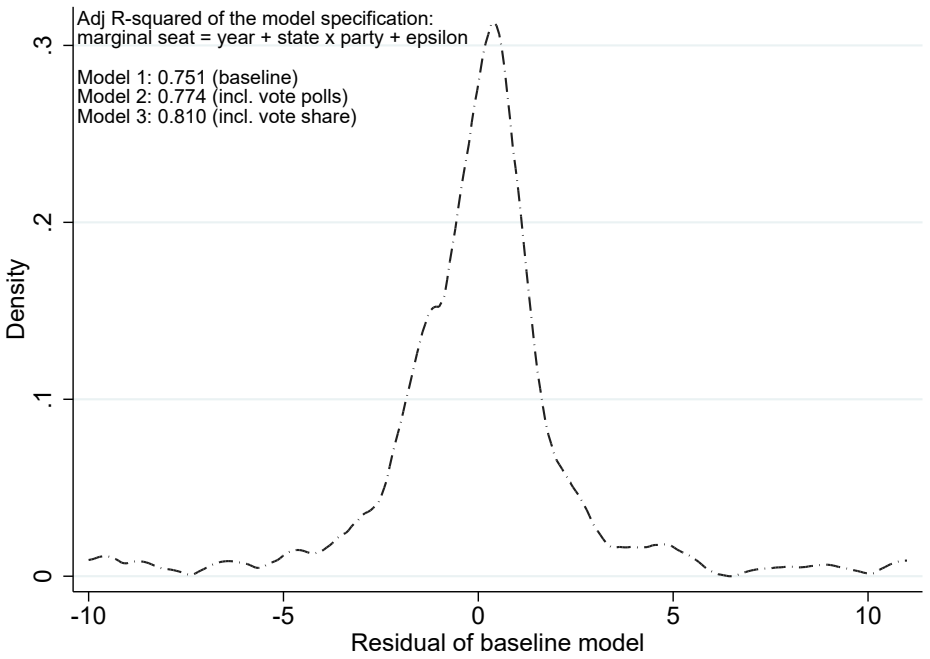
Notes: The figure shows the number of candidates by the identified marginal cutoff seat at the regional (federal state) and political party level. For example, there are about 312 candidates at the state-party level where only the first candidate enters parliament via the party list (*Landesliste*).

Figure D.1: Number of observations per cutoff seat

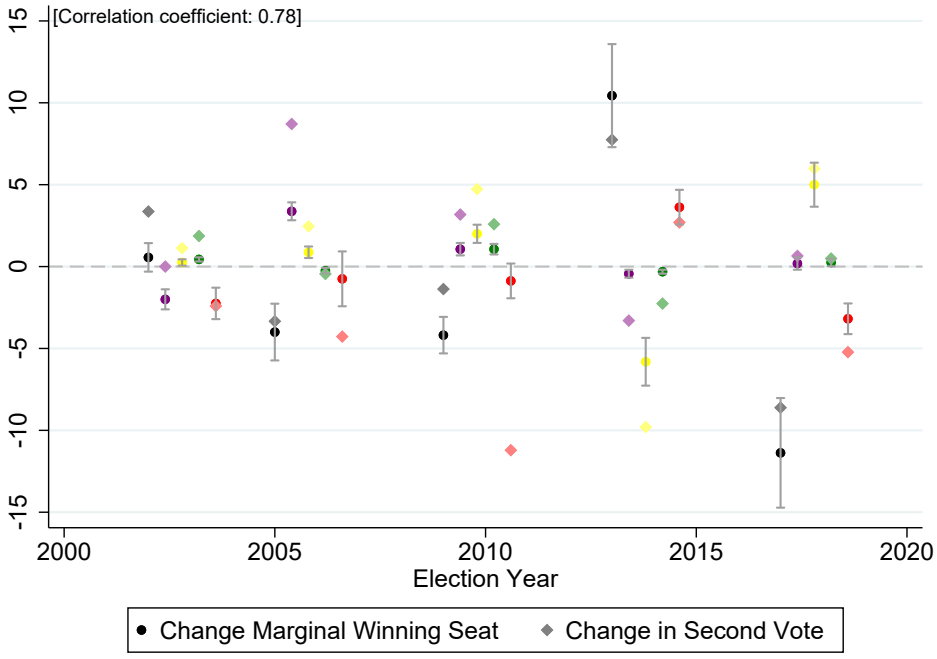
Table D.2: Observations at closest mass points

Threshold	Treatment status	Number of observations		
		All	Unique firms	Unique politicians
(1)	(2)	(3)	(4)	(5)
⋮				
-6	Control	80	80	46
-5	Control	87	87	50
-4	Control	93	93	51
-3	Control	95	94	60
-2	Control	92	90	49
-1	Control	113	113	61
0	Treated	108	102	49
1	Treated	119	110	55
2	Treated	110	100	55
3	Treated	94	91	49
4	Treated	130	125	58
5	Treated	92	90	38
6	Treated	67	64	30
⋮				

Notes: The total number of observations across all mass points is 2,233, with 1,694 unique firms, 947 unique politicians and 98 unique mass points.



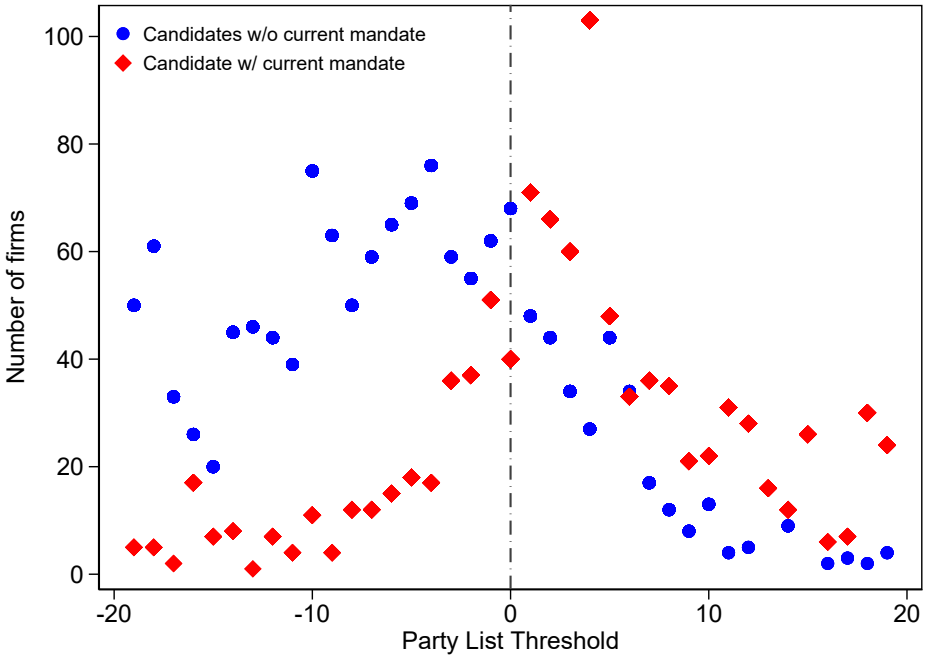
A: Residual Variation



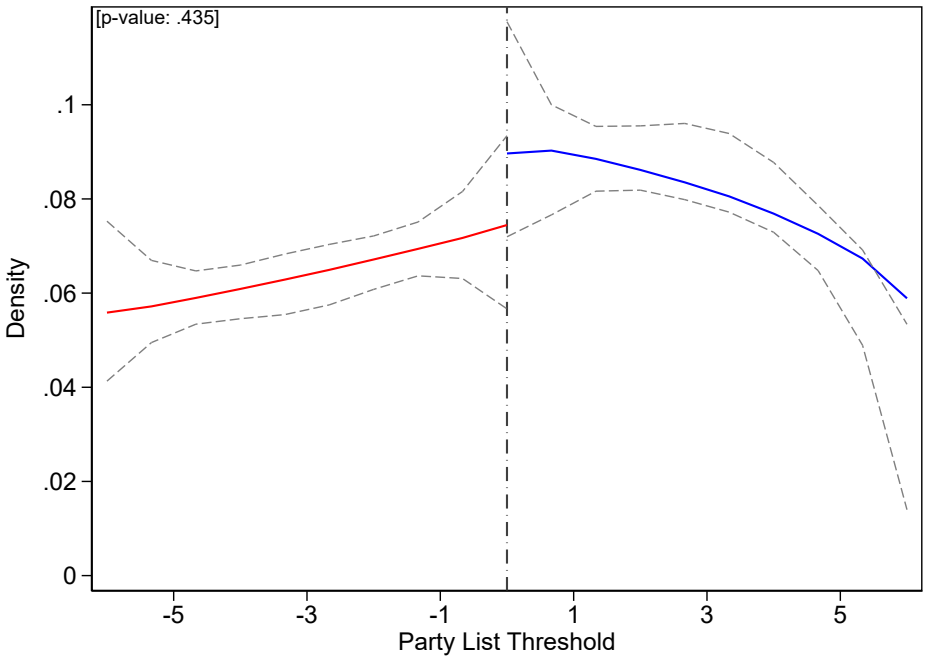
B: Association between Vote and Cutoff

Notes: Panel A shows the distribution of residuals from the regression equation: $marginal\ seat_{tsp} = \theta_t + \omega_{sp} + \epsilon_{tsp}$. The regression is organized at the election year – party – federal state level and is constructed based on all 13,002 political candidate–election year observations. The marginal seat is identified if at least one candidate enters parliament from the submitted party list. Panel B shows the change in the identified marginal seat (circle) from the previous election to the current election at the party – state level (along with the 95% confidence intervals) and the change in the second vote for the respective party (diamond). Each color represents a political party: CDU/CSU - black; SPD - red; Greens - green; FDP - yellow; Left - purple. AfD is not shown in the Panel B.

Figure D.2: Marginal seat variation



A: Number of Firms



B: MC Test

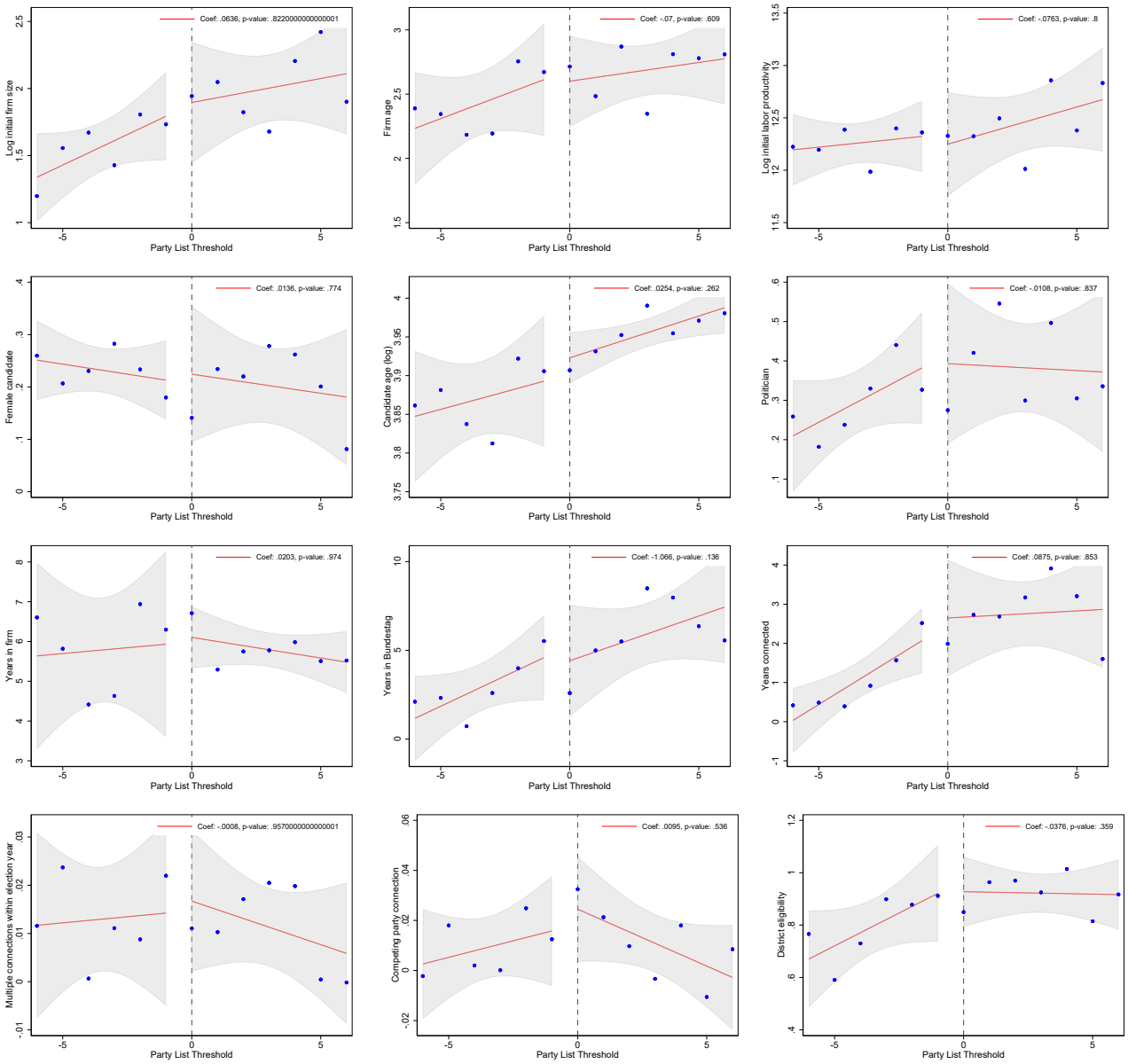
Notes: Panel A shows the number of firms for the normalized party list where 0 represents the marginal seat that just enters parliament. The blue dots represent firms connected to a candidate without a current mandate. The red dots represent firms connected to candidates with a current mandate. Panel B shows the McCrary test for manipulation pooling both groups.

Figure D.3: Test for manipulation of the cutoff

Table D.3: Balancing test around the cutoff - firm characteristics

	Indep. variable				Dep. variables	
	Connection indicator		RDD cutoff			
	Coefficient (1)	*P*-value (2)	Coefficient (3)	*P*-value (4)	Mean (5)	SD (6)
	Candidates *without* a current mandate					
Firm size, age, productivity						
Log initial firm size	0.602	0.001	0.032	0.928	1.619	1.608
Initial firm size > 100	0.097	0.001	0.007	0.906	0.056	0.230
Firm age	7.810	0.125	8.542	0.183	21.530	37.944
Log initial labor productivity	-0.190	0.334	0.184	0.629	12.239	1.627
Legal form						
Small business	-0.103	0.003	-0.065	0.345	0.184	0.388
LLC	-0.048	0.295	-0.084	0.372	0.455	0.498
Stock corporation	0.055	0.017	0.002	0.970	0.051	0.219
Civil public partner	-0.025	0.185	0.001	0.980	0.052	0.223
Association	0.082	0.042	0.064	0.417	0.198	0.399
Major industries						
Manufacturing	-0.016	0.403	-0.060	0.146	0.054	0.226
Finance & insurance	-0.024	0.272	0.045	0.295	0.052	0.223
Technical service	0.030	0.469	-0.068	0.418	0.252	0.434
Other service	0.092	0.012	0.107	0.140	0.152	0.360
Candidate characteristics						
Female candidate	-0.029	0.453	0.019	0.803	0.161	0.368
Age	4.373	0.000	0.829	0.620	48.488	9.820
Years in firm	0.241	0.630	-1.105	0.246	6.631	23.210
Match characteristics						
Multiple connections	0.008	0.582	0.020	0.539	0.012	0.109
Competing party connection	0.005	0.317	0.007	0.322	0.005	0.068
	Candidates *with* a current mandate					
Firm size, age, productivity						
Log initial firm size	0.253	0.356	0.142	0.797	2.411	2.052
Initial firm size > 100	0.034	0.479	-0.036	0.711	0.160	0.366
Firm age	2.806	0.393	-2.492	0.713	33.376	31.232
Log initial labor productivity	0.216	0.406	0.685	0.177	12.346	1.694
Legal form						
Small business	-0.059	0.064	0.021	0.641	0.067	0.251
LLC	0.008	0.882	-0.206	0.064	0.339	0.474
Stock corporation	-0.004	0.884	0.048	0.424	0.079	0.270
Civil public partner	0.005	0.744	0.055	0.179	0.037	0.190
Association	0.004	0.939	0.036	0.748	0.427	0.495
Major industries						
Manufacturing	0.053	0.020	0.029	0.526	0.039	0.194
Finance & insurance	0.046	0.057	0.053	0.158	0.044	0.205
Technical service	-0.039	0.404	0.008	0.925	0.199	0.399
Other service	0.029	0.585	0.112	0.288	0.362	0.481
Candidate characteristics						
Female candidate	0.027	0.593	0.064	0.520	0.260	0.439
Age	1.612	0.104	-0.099	0.958	54.169	8.158
Years in firm	0.354	0.567	-1.704	0.125	5.766	5.355
Match characteristics						
Multiple connections	0.002	0.940	-0.050	0.363	0.091	0.287
Competing party connection	-0.019	0.571	-0.047	0.372	0.068	0.252

Notes: Columns (1) to (4) show regression results using a symmetric bandwidth selection of 4 seats below and above the cutoff seat. Connection indicator is 1 if the firm is connected to parliament and zero otherwise. RDD cutoff refers to the reduced-form estimate at the marginal seat. Columns (5) and (6) show the mean and standard deviation of the respective outcome variable. *p*-values are based on robust standard errors. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.



Notes: The figure shows firm, candidate and match-specific variables across the party list threshold. All variables represent normalized values conditional on party, state, and election year fixed effects. Positive values denote firms connected to candidates who won a seat in the German federal parliament. Likewise, negative values depict firms that are connected to candidates who did not won a seat via the party list. The solid red line represents local linear regressions on each side of the cutoff. The grey area represent 95% confidence intervals. The blue dots represent conditional means. Baseline selected bandwidth of 6 below and above the cutoff.

Figure D.4: Balancing at the cutoff (pooled)

Additional Results on the Effect of Winning and Losing Political Connections

Table D.4: 2SLS results - post election years

	Exit in			Employment growth $t-1 \rightarrow$			Lab. prod. growth $t-1 \rightarrow$		
	$t+1$	$t+2$	$t+3$	$t+1$	$t+2$	$t+3$	$t+1$	$t+2$	$t+3$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: No mandate at election ("Staying unconnected (=0) vs. winning (=1)")									
Indicator	-0.106***	-0.117***	-0.119*	-0.0961	-0.160	-0.227	-0.335**	-0.425**	-0.389*
	(0.0350)	(0.0421)	(0.0700)	(0.103)	(0.127)	(0.159)	(0.144)	(0.224)	(0.235)
F-Statistic	1399	1399	501.9	410	314.9	280.7	407.4	220.9	246.3
Observations	733	733	504	357	300	240	351	277	240
Mean below	0.0723	0.0964	0.172	0.0909	0.110	0.164	0.0347	0.0605	0.149
Mean above	0.0154	0.0385	0.112	0.00954	0.0262	-0.0291	-0.151	-0.219	-0.179
Panel B: Mandate at election ("Staying connected (=0) vs. dropout (=1)")									
Indicator	0.0366	0.0478	-0.0255	-0.0550	-0.272	-0.328*	0.0929	0.311	0.343
	(0.0374)	(0.0506)	(0.0724)	(0.121)	(0.169)	(0.171)	(0.211)	(0.256)	(0.308)
F-Statistic	296.3	296.3	222	70.92	55.40	61.97	62.60	54.81	48.78
Observations	624	624	483	320	268	227	301	252	213
Mean below	0.0357	0.0893	0.111	0.0183	-0.0364	-0.0277	-0.182	-0.0890	-0.0103
Mean above	0.0286	0.0571	0.111	0.111	0.147	0.164	-0.0874	-0.0743	-0.0253

Notes: The table presents fuzzy RDD estimates from local linear regression discontinuity specifications. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. Panel A provides the results for the candidates without a current political mandate, whereas Panel B provides the results for candidates with a current political mandate. All specifications are based on a bandwidth selection of 6 below and above the cutoff and include election year FE. Robust standard errors are shown in parentheses. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

Table D.5: 2SLS results - additional covariates

	Exit in $t+2$		Employment growth $t-1 \rightarrow t+2$		Lab. prod. growth $t-1 \rightarrow t+2$	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: No mandate at election ("Staying unconnected (=0) vs. winning (=1)")						
Indicator	-0.127***	-0.133***	-0.152	-0.134	-0.608**	-0.653**
	(0.0454)	(0.0461)	(0.154)	(0.153)	(0.259)	(0.263)
F-Statistic	946.5	1079	176	193.6	135.3	145.6
Observations	731	731	298	298	261	261
Panel B: Mandate at election ("Staying connected (=0) vs. dropout (=1)")						
Indicator	0.0377	0.0383	-0.209	-0.214	0.252	0.232
	(0.0531)	(0.0535)	(0.186)	(0.195)	(0.291)	(0.296)
F-Statistic	297.8	301.4	63.18	59.40	49.02	46.48
Observations	621	621	265	265	240	240
Election Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Initial Employment	No	Yes	No	Yes	No	Yes
Initial Firm Age	No	Yes	No	Yes	No	Yes
Job Type	No	Yes	No	Yes	No	Yes

Notes: The table presents fuzzy RDD estimates from local linear regression discontinuity specifications. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. Panel A provides the results for the candidates without a current political mandate, whereas Panel B provides the results for candidates with a current political mandate. All specifications are based on a bandwidth selection of 6 below and above the cutoff. Robust standard errors are shown in parentheses. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

Table D.6: Fuzzy RDD results - variation in the bandwidth choice

	Absolute bandwidth			Relative bandwidth		
	bw: 4	bw: 8	bw: 10	bw: 50%	bw: 50% & bw: 6	bw: 50% & bw: 8
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A1: No mandate at election ("Staying unconnected (=0) vs. winning (=1)"), exit t+2						
Indicator	-0.152**	-0.110***	-0.0705*	-0.0617**	-0.134***	-0.128***
	(0.0601)	(0.0392)	(0.0360)	(0.0283)	(0.0506)	(0.0441)
Mean outcome	0.0945	0.0871	0.0907	0.0972	0.0942	0.0938
Observations	506	917	1079	1007	571	648
Panel A2: No mandate at election ("Staying unconnected (=0) vs. winning (=1)"), employment growth t+2						
Indicator	-0.131	-0.176	-0.134	-0.171**	-0.204	-0.159
	(0.211)	(0.134)	(0.117)	(0.0789)	(0.177)	(0.149)
Mean outcome	0.0945	0.0871	0.0907	0.0972	0.0942	0.0938
Observations	506	917	1079	1007	571	648
Panel A3: No mandate at election ("Staying unconnected (=0) vs. winning (=1)"), Lab. prod. growth t+2						
Indicator	-0.626*	-0.326	-0.210	-0.0465	-0.703**	-0.441*
	(0.359)	(0.205)	(0.171)	(0.125)	(0.287)	(0.240)
Mean outcome	-0.0332	-0.00726	0.000246	-0.0261	0.00117	-0.00545
Observations	199	342	406	402	212	239
Panel B1: Mandate at election ("Staying connected (=0) vs. dropout (=1)"), exit t+2						
Indicator	0.0329	0.0495	0.0132	-0.0153	0.0569	0.0316
	(0.0677)	(0.0479)	(0.0461)	(0.0317)	(0.0544)	(0.0534)
Mean outcome	0.0500	0.0558	0.0584	0.0533	0.0540	0.0577
Observations	436	714	785	880	534	586
Panel B2: Mandate at election ("Staying connected (=0) vs. dropout (=1)"), employment growth t+2						
Indicator	-0.196	-0.133	-0.00553	-0.0928	-0.124	-0.133
	(0.189)	(0.157)	(0.161)	(0.0875)	(0.185)	(0.184)
Mean outcome	0.0882	0.0685	0.0480	0.0402	0.0886	0.0775
Observations	182	314	350	410	234	254
Panel B3: Mandate at election ("Staying connected (=0) vs. dropout (=1)"), Lab. prod. growth t+2						
Indicator	-0.503*	0.329	0.405*	0.169	0.237	0.235
	(0.299)	(0.222)	(0.216)	(0.151)	(0.274)	(0.269)
Mean outcome	0.0413	0.0122	-0.00134	0.0481	0.0336	0.0321
Observations	167	277	309	379	219	233
Election Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents fuzzy RDD estimates from local linear regression discontinuity specifications with different bandwidths. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. Robust standard errors are shown in parentheses. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

Table D.7: Fuzzy RDD results - continuity-based optimal bandwidth selection

	Winning a mandate "Staying unconnected (=0) vs. winning (=1)"			Losing a mandate "Staying connected (=0) vs. dropout (=1)"		
	Exit	Employment	LP	Exit	Employment	LP
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Triangular kernel						
Connection	-0.119***	-0.0816	-0.526**	0.0349	-0.182	0.345*
	(0.0438)	(0.0894)	(0.247)	(0.0442)	(0.119)	(0.190)
Bandwidth h	6.824	14.91	6.908	11.10	12.10	11.49
Panel B: Uniform kernel						
Connection	-0.134**	-0.0743	-0.471**	0.0410	-0.218	0.384*
	(0.0519)	(0.0891)	(0.237)	(0.0446)	(0.137)	(0.201)
Bandwidth h	4.668	11.10	5.034	8.181	9.141	8.348
Panel C: Two-sided MSE-optimal bandwidth						
Connection	-0.0860***	-0.0741	-0.368**	0.0361	-0.179	0.327*
	(0.0319)	(0.0895)	(0.159)	(0.0448)	(0.120)	(0.186)
Bandwidth $-h$	13.55	15.34	12.80	10.73	11.35	11.31
Bandwidth $+h$	8.450	13.34	7.824	10.79	13.98	14.70
Panel D: CE-optimal bandwidth						
Connection	-0.111**	-0.106	-0.650**	0.0437	-0.282**	0.253
	(0.0542)	(0.104)	(0.322)	(0.0514)	(0.135)	(0.213)
Bandwidth h	4.659	10.64	4.952	7.768	8.812	8.405
Election Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents fuzzy RDD estimates from local linear regression discontinuity specifications with different optimal bandwidth selection criteria. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. Robust bias-corrected p-values are shown in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Table D.8: Local randomization results - marginal seat vs. seat below

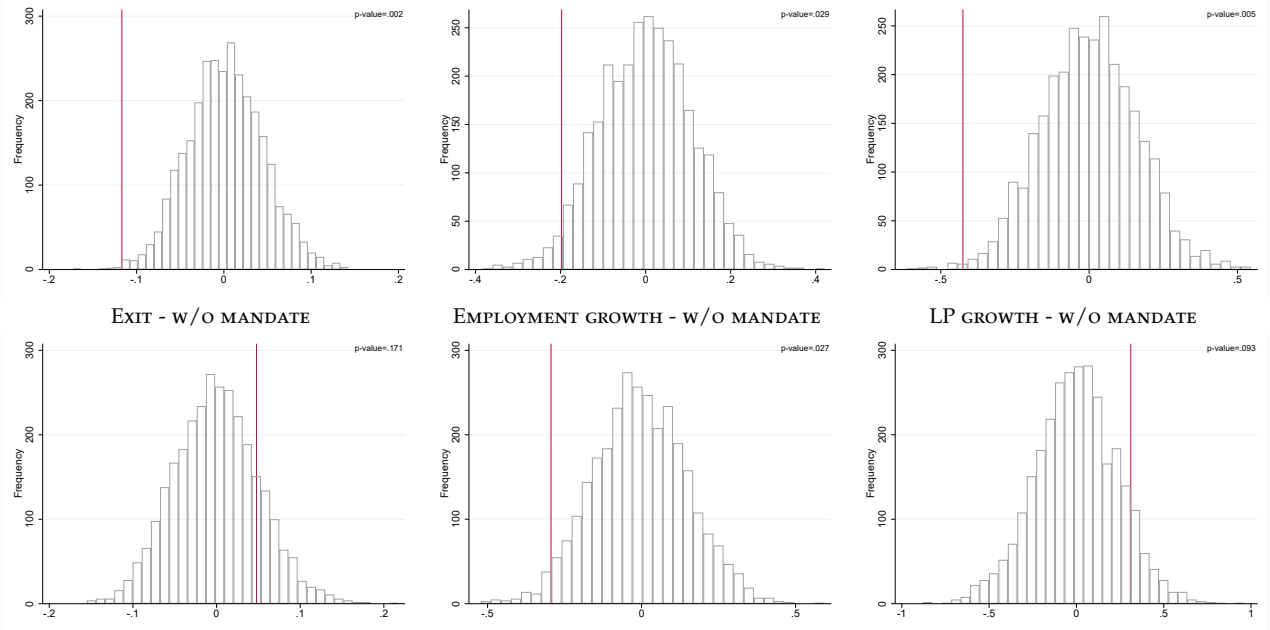
	Candidates without mandate "Winning: marginal seat (=1)"			Candidates with mandate "Dropout: seat below (=1); unlucky (=1)"		
	Market exit at $t+2$	Employment growth $t-1 \to t+2$	Lab prod growth $t-1 \to t+2$	Market exit at $t+2$	Employment growth $t-1 \to t+2$	Lab. prod. growth $t-1 \to t+2$
	(1)	(2)	(3)	(4)	(5)	(6)
Indicator	-0.097***	-0.153	-0.225	0.017	-0.090	0.148
	(0.003)	(0.112)	(0.113)	(0.620)	(0.342)	(0.292)
Mean marginal seat	0.038	-0.012	-0.165	0.057	0.139	-0.076
Mean below	0.135	0.139	0.060	0.074	0.049	0.069
Observations	278	120	99	235	101	93

Notes: The table presents local randomization results comparing the outcome variables for firms with candidates at the marginal seat and one seat below. Outcome variables are market exit at $t+2$, employment growth from $t-1 \to t+2$, and labor productivity growth from $t-1 \to t+2$ relative to the election years. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. Finite p-values shown in Panel A. Robust standard errors are shown in Panel B. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

Table D.9: Placebo results - average yearly growth before election $t-5$ to $t-1$

	Candidates without mandate "Staying unconnected (=0) vs. winning (=1)"			Candidates with mandate "Staying connected (=0) vs. dropout (=1)"		
	$bw=4$	$bw=6$	$bw=8$	$bw=4$	$bw=6$	$bw=8$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average yearly employment growth, $t-5$ to $t-1$						
Indicator	-0.0171	-0.0567	-0.0395	-0.0576	-0.0558	-0.0226
	(0.0490)	(0.0348)	(0.0294)	(0.0662)	(0.0605)	(0.0492)
Observations	308	438	554	267	392	449
F-Statistic	337.8	719.5	1150	137.3	154.9	163.8
Mean outcome	0.0304	0.0281	0.0282	0.00737	0.0150	0.0160
Panel B: Average yearly lab. prod. growth, $t-5$ to $t-1$						
Indicator	0.0349	0.0823	0.0429	0.0543	0.0644	0.0486
	(0.0626)	(0.0558)	(0.0456)	(0.0726)	(0.0659)	(0.0530)
Observations	289	413	525	254	367	415
F-Statistic	314.6	668.5	1050	119.3	137	150
Mean outcome	-0.0031	-0.0089	-0.00593	0.0274	0.0233	0.0203
Election Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents fuzzy RDD estimates from local linear regression discontinuity specifications with different bandwidths. Outcome variables are employment growth (Panel A) and labor productivity growth (Panel B) before the election. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. Robust standard errors are shown in parentheses. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01



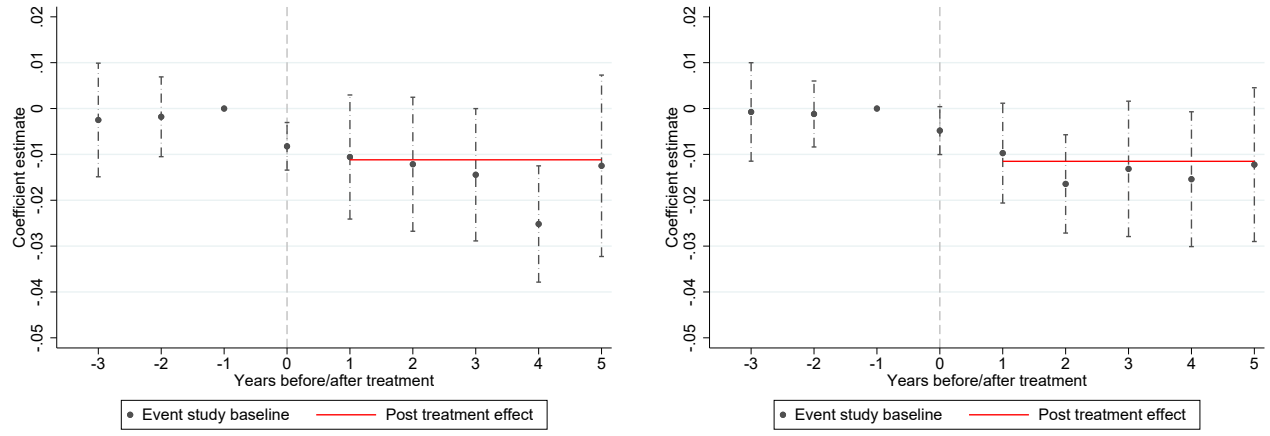
Exit - w/o mandate | Employment growth - w/o mandate | LP growth - w/o mandate

Exit - w/ mandate | Employment growth - w/ mandate | LP growth - w/ mandate

Notes: The figure shows the distribution of the treatment effect coefficients for 2,999 permutations of the dependent variables. The red lines show the baseline 2SLS coefficients.

Figure D.5: Permutation results

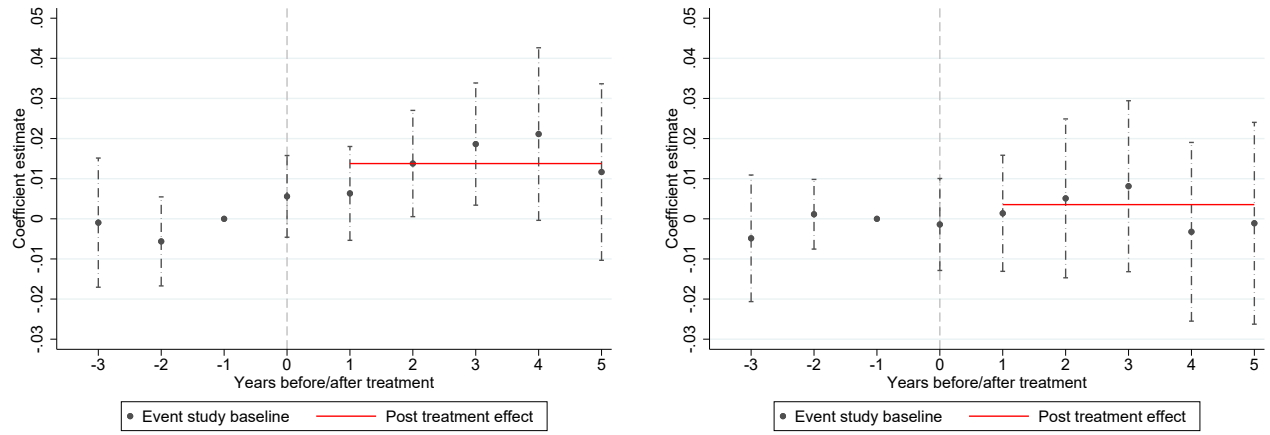
E Additional Empirical Results on the Mechanism



A: Appointing a current politician B: Appointing a former politician

Notes: The figure shows the effect of appointing a current (Panel A) or former (Panel B) politician at time *t=0* on credit rating in *t=τ*, *τ=-3 to 5*. The black point estimates are based on equation (2). The regression is weighted by the inverse propensity score. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint a person in *t=0*. The red bar gives the effect on the average change in credit rating between the pre-period = −3 to 0 and the post-period 1 to 5. The number of observations with employment information are shown in Figure C.2. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

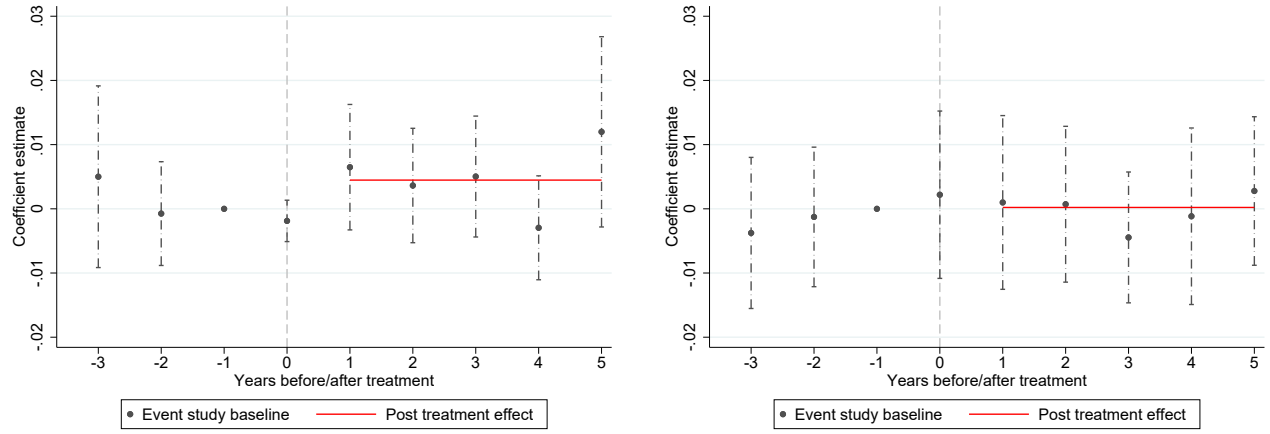
Figure E.1: The effect of appointing a politician on credit rating dynamics



A: Appointing a current politician B: Appointing a former politician

Notes: The figure shows the effect of appointing a current (Panel A) or former (Panel B) politician at time *t=0* on subsidies in *t=τ*, *τ=-3 to 5*. The black point estimates are based on equation (2). The regression is weighted by the inverse propensity score. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint a person in *t=0*. The red bar gives the effect on the average change in subsidies between the pre-period = −3 to 0 and the post-period 1 to 5. The number of observations with employment information are shown in Figure C.2. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

Figure E.2: The effect of appointing a politician on subsidy dynamics



A: Appointing a current politician B: Appointing a former politician

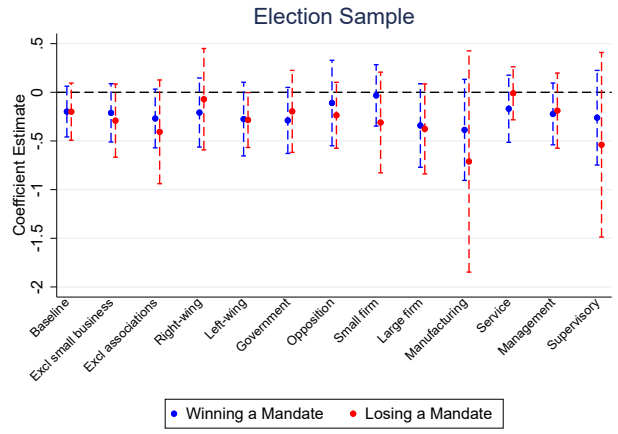
Notes: The figure shows the effect of appointing a current (Panel A) or former (Panel B) politician at time *t=0* on public procurement in $t=\tau$, τ=*-3 to 5*. The black point estimates are based on equation (2). The regression is weighted by the inverse propensity score. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint a person in *t=0*. The red bar gives the effect on the average change in public procurement between the pre-period = -3 to 0 and the post-period 1 to 5. The number of observations with employment information are shown in Figure C.2. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

Figure E.3: The effect of appointing a politician on public procurement dynamics

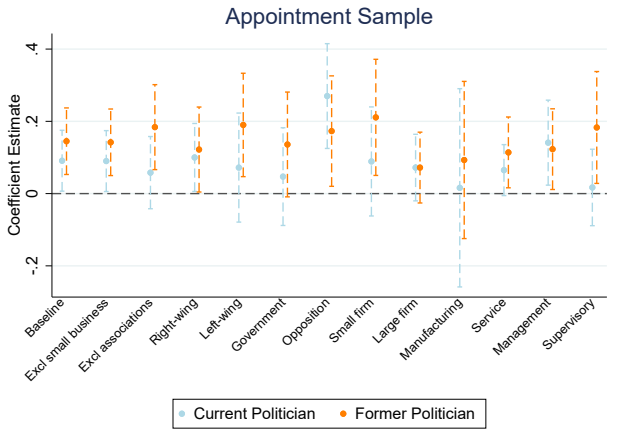
Table E.1: Fuzzy RDD results - job displacement/new appointment

	Job end		New appointment	
	Government	Opposition	Government	Opposition
	(1)	(2)	(3)	(4)
Panel A: Candidates without mandate ("Staying unconnected (=0) vs. winning (=1)")				
Indicator	0.0414	0.0773	0.0149	-0.0127
	(0.0488)	(0.0680)	(0.0150)	(0.0118)
Observations	251	494	251	494
F-Statistic	330.7	937.8	330.7	937.8
Mean outcome	0.123	0.113	0.00283	0.00355
Panel B: Candidates with mandate ("Staying connected (=0) vs. dropout (=1)")				
Indicator	0.809**	0.0482	0.169	0.0420
	(0.355)	(0.0673)	(0.142)	(0.0296)
Observations	287	298	287	298
F-Statistic	16.09	839.9	16.09	839.9
Mean outcome	0.177	0.187	0.0349	0.187
Election Year FE	Yes	Yes	Yes	Yes

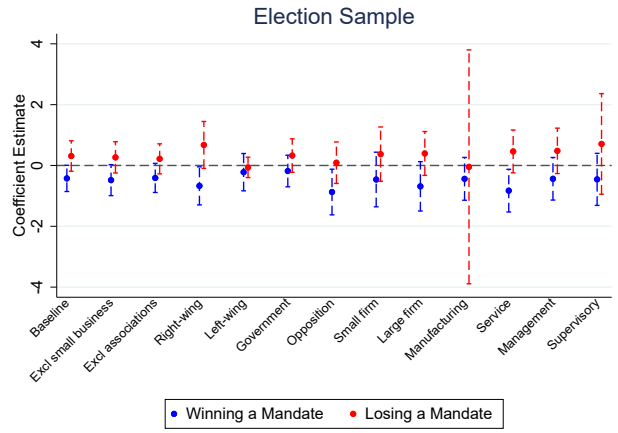
Notes: The table presents fuzzy RDD estimates from local linear regression discontinuity specifications. All specifications are based on a bandwidth selection of 6 below and above the cutoff. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. Outcome variables are job displacement after the election (columns 1 and 2) and appointment of a politician with a current mandate post election (columns 3 and 4). Robust standard errors are shown in parentheses. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.



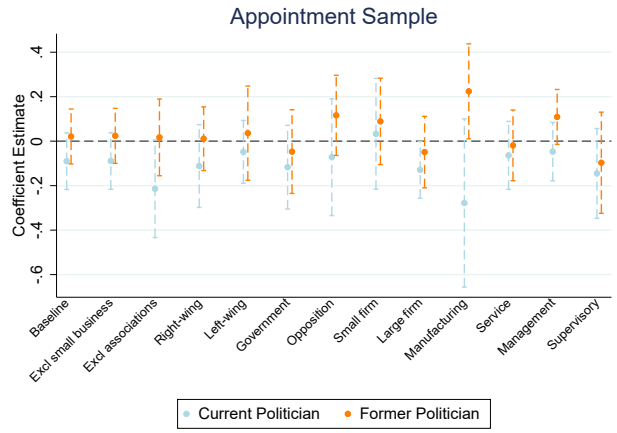
A1: Employment, RDD, $t+2$



B1: Employment, Event Study, $\beta_{\tau=2}$



A2: Productivity, RDD, $t+2$



B2: Productivity, Event Study, $\beta_{\tau=2}$

Notes: The figure presents fuzzy RDD estimates from local linear regression discontinuity specifications with bandwidth selection of 6 on employment growth and labor productivity growth in panels (A1) and (A2), respectively. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. Robust standard errors are shown in parenthesis. Panels (B1) and (B2) present the effect of appointing a current or former politician at time *t=0*. $\beta_{\tau=2}$ is based on equation (2). All regressions are weighted by the inverse propensity score. Propensity score estimation is performed by year and includes firm age in groups, sales in *t=-1* and *t=-2*, employment in *t=-1* and *t=-2*, composition of job positions in *t=0*, firm type, 1-digit industry and state fixed effects (see Table C.2 in the Appendix). The control group is restricted to firms that appoint in *t=0*. The mean refers to the outcome level of the control group measured two years after the appointment. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

Figure E.4: Employment and Productivity Effects by Sub-Samples