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DISCUSSION PAPER

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

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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 to measure connections between politicians and 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. The economic effects are mediated by better credit ratings while access to subsidies or procurement contracts are documented to be of lower importance.

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 these connections could, in principle, have social value, i.e., by overcoming market failures, they also raise concerns regarding of inefficient resource allocation due to political favoritism and corruption. An extensive literature studies the impacts of connections of firms and politicians and rather supports the latter view. Politically connected firms gain unfair advantage via preferential access to government contracts (Acemoglu, Hassan, and Tahoun 2018, Johnson and Mitton 2003, Khwaja and Mian 2005), loans (Schoenherr 2019, Leuz and Oberholzer-Gee 2006, Khwaja and Mian 2005) or subsidies (Choi, Penciakova, and Saffie 2021, Goldman, Rocholl, and So 2013). Connections to political power can hold certain risks for firms in case of sudden regime changes (Acemoglu, Hassan, and Tahoun 2018) or if connected CEOs exploit firm resources in support of political goals (Bertrand, Kramarz, Schoar, and Thesmar 2018). Furthermore, political connections to business also have the potential to disrupt competitive market structures leading to factor reallocation and distorting the process of creative destruction (Akcigit, Baslandze, and Lotti 2023). While these problems with political connections are negatively related to the strength of a country's institutions, preferential treatment of connected firms is also observed in developed countries with strong institutional structures (Amore and Bennedsen 2013).

In this paper, we study connections between firms and politicians in the federal government in Germany asking the question: how does participation of a current or former member of parliament in an executive leadership team position affect firm dynamics? We argue that this is an interesting setting to study because, on the one hand, we would expect very little room for preferential treatment of firms by governmental politicians. Connections of parliamentary politicians are typically most strongly monitored. Existing evidence of rent-seeking in Europe is based on studies examining connections to local politicians.[1] Further, Germany stands out as a country with highly developed institutions; it ranks 9th out of 180 countries on the Transparency's International 2022 *Corruption Perception Index*. Strict implicit and explicit disclosure policies regulate the extent of interactions between firms and politicians. On the other hand, politicians are directly involved in the oversight and governance of public companies or firms co-owned by the state. In many of these companies, supervisory board seats are reserved for local or higher level politicians by the company statutes. It is thus important to investigate whether the legal framework is strong enough to regulate political impact on firm dynamics.

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

[1] See Akcigit, Baslandze, and Lotti (2023), Cingano and Pinotti (2013), Bertrand, Kramarz, Schoar, and Thesmar (2018), Amore and Bennedsen (2013), Haselmann, Schoenherr, and Vig (2018).

ownership information and the position and identity of members in the executive leadership team, such as CEO/owner, executive and advisory board member. The firm data are provided by *Creditreform* and organized by the ZEW – Leibniz Centre for European Economic Research in Mannheim and contains comprehensive information on firm outcomes such as credit scores, employment, sales, firm entry and exit since around 2000. 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 thus define events at which we can identify causal effects of the political connection on firm outcomes. Our first identification strategy is an event study design which 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.

To confront this identification concern, our second strategy develops a framework for reduced-form identification that relies on election outcomes where 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 seat or loses it.

Our analysis results in four 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 been increasing over the last two decades. Second, we find that the appointment of a politician sends a strong signal to the market as credit ratings of firms who become connected via a current or former politician strongly increase. The results from the event study analysis are confirmed by the RDD, although with lower statistical precision. A firm with a winning candidate experiences an improvement in credit ratings and a firm whose incumbent candidate loses their parliamentary seat sees a decline. The signals from credit ratings appear to manifest in better survival chances of connected firms, who are essentially shielded from exit with the new connection. We see substantial reductions in firm exits in the first years after the appointment of a current and especially of a former politician. In terms of the firm exit outcomes, the findings from the event study are fully confirmed by the RDD analysis. Winning a mandate on the marginal seat is related to better survival chances,

while losing the mandate tends to turn the effect around, in particular for smaller firms.

Third, evidence on the impacts of a political connection on other firm outcomes is less clear cut. A politician's firm appointment is accompanied by gradual increases in firm level employment conditional on survival which results in a significant rise in employment growth over the first years after the appointment starts. However, we do not document a corresponding effect on employment growth for candidates winning an election on the marginal seat. Neither do we find positive effects on productivity growth due to a political connection. Fourth, we also examine the effect of a political connection on the probability of winning large government subsidies or EU-level procurement contracts which might mediate effects on economic outcomes. We do not find overall evidence for increased subsidies and public procurement contracts across all types of political connections. Our results suggest, however, that gaining access to a politician with a current political mandate increases the probability of receiving government subsidies and increases procurement contracts.

Our results contribute new evidence of firm connections to parliamentary politicians to the literature mentioned above. Our paper is closest to Akcigit, Baslandze, and Lotti (2023) who study the effects of connections to local politicians with primarily non-management positions on firm dynamics in Italy. They document important impacts of political connections on firm dynamics. Namely, among connected firms the probability of firm exits decreases and employment growth increases, while productivity dynamics remains unaffected. Akcigit, Baslandze, and Lotti (2023) interpret these results in light of a model of firm investment, where firms invest either in innovation or in political connections which reduce the cost of regulatory frictions. In their model, young firms invest in innovation but once they reach a certain size threshold, firms increasingly invest in political connections to defend their position as market leaders. In contrast to Akcigit, Baslandze, and Lotti (2023), we study Germany, a country with significantly lower levels of corruption, and firm connections to parliamentary politicians in top executive positions. Our paper makes two novel contributions. First, we confirm the Italian results on firm dynamics with a sharper identification strategy. Second, we assemble additional data to investigate mediating factors that help understanding the mechanisms behind the changes in firm dynamics.

Credit ratings are observed by a wide set of market participants and changes in these ratings are important drivers of their decisions (Dichev and Piotroski 2001; Behr and Güttler 2008). We are able to show that a newly established political connection strongly improves a firm's credit ratings. Market signals from sudden changes in credit ratings might thus lead to improved bank lending to connected firms, which is in line with the extensive literature documenting increasing stock market valuations and returns from newly established political connections (Fisman 2001; Faccio 2006; Goldman, Rocholl, and So 2009; Akey 2015; Acemoglu, Hassan, and Tahoun 2018; Brown and Huang 2020; Green and Homroy 2022) as well as the literature documenting favorable bank lending decision regarding connected firms (Sapienza 2004; Khwaja and Mian 2005; Englmaier and Stowasser 2017). In this sense we argue that changes in credit ratings are an important mediator of the impact of political connections on firm dynamics.

In the German context, research has focused on bank lending which has a strong political compo-

nent. Koetter and Popov (2021), Englmaier and Stowasser (2017) study German local savings banks, who adjust their strategies in response to a political change in the local government and strongly increase lending to government projects at the cost of private households and enterprises. Haselmann, Schoenherr, and Vig (2018) explore interactions between members in an elite service club in Germany and find that newly elected mayors who become supervisory board chairmen of state banks increase lending to firms in their club network. Exploiting the tightening of a formal disclosure policy in 2007 in Germany, Niessen and Ruenzi (2010) find that the gap in stock market valuations of connected and unconnected firms declined after implementation of the rule.

We also contribute to the literature that studies preferential access to subsidies and public procurement contracts. Duchin and Sosyura (2012), Amore and Bennedsen (2013) and Cingano and Pinotti (2013) provide suggestive evidence that political connections generate more resources for unprofitable firms. Choi, Penciakova, and Saffie (2021) show for US firms that a political connection increases the chance to win stimulus grants during the Great Recession. Using evidence from US elections, Goldman, Rocholl, and So (2013) show that firms with a politically connected board of directors experience an increase in public procurement contracts.[2] On the contrary, Bertrand, Kramarz, Schoar, and Thesmar (2018) show for France that CEOs with a prior government involvement influence employment decisions without benefiting firms in terms of subsidies or tax exemptions. We do not find that connections to former politicians generate better access to subsidies. However, firms connected to politicians with a current political mandate receive additional government subsidies. We further find that winning a political mandate as compared to staying unconnected increases the probability of winning public procurement contracts in Germany.

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

2 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

[2]See also Agrawal and Knoeber (2001) and Szucs (2024) on favoritism in public procurement contracting.

highest number of votes in each of 299 election districts (regional boundaries as of 2017)[3] enters parliament with a direct political mandate.[4] 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*).

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.[5] 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 C.1 shows characteristics of party lists of the six parties that enter the German parliament over the election cycles from 1998 to 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, 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 3) 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, Folke, Prato, and Rickne 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.[6] 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

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

[4]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.

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

[6]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.

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.[7]

To document the variation in marginal seats across election cycles, Appendix Figure C.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.[8]

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.

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*). 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 member-

[7] 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).

[8] To further support the existence of the random component of the marginal seat, Panel A of Appendix Figure C.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 posibility to predict the marginal seat with election polls.

[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.

ship positions are disclosed by politicians: board of directors, advisory board, advisory council and shareholder.[10] These are also 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 the 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).

3 Data & Measurement

3.1 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 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, Gottschalk, Müller, and Niefert (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 A.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

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

[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 A.1 provides the overall number of observations as well as the number of observations with employment and sales information at the yearly level.

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]

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

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

[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.

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, Schoefer, and Heining 2021).

Civil law partnerships consist of at least two partners who, in contrast to LLCs, are unlimited 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 and 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 simple model-implied productivity measure by calculating $revenue/employment^{\alpha}$, where α takes into account the labor share in the production.[15] For the special case when $\alpha = 1$, the ratio refers to the standard labor productivity measure. In our baseline specifications, we provide results for a value of α of 0.7.[16]

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). 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 and 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

[15] We consider a simple production technology that features decreasing returns to scale with respect to labor: $y_{t,i} = z_{t,i}^{1-\alpha} l_{t,i}^{\alpha}, \quad 0 < \alpha < 1$, where $z_{t,i}$ denotes the level of productivity at firm i at time t, which is heterogeneous across firms, and $l_{t,i}$ is the amount of labor hired. 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.

[16] According to OECD data, the labor share amounts to, on average, 0.68 since 2000, whereas according to PWT data, estimated labor share is around 0.62.

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.[17]

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, Giebler, Heimpold, Titze, and Urban-Thielicke 2018 for a detailed data documentation).[18] 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.[19] We link the projects via record linkage using firm names to obtain information at what point in time a firm receives subsidies.[20]

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 connections 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.[21] Using the TED data, Havlik (2020) documents evidence for political election cycles by showing that public procurement contracts increase prior to national parliamentary elections.

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

[17] Using the MUP data, Bersch, Degryse, Kick, and Stein (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.

[18] 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.

[19] There is a relatively large literature that studies *GRW* subsidies in general at the regional and firm level. These include, among others, Becker, Egger, and Von Ehrlich (2010), Brachert, Dettmann, and Titze (2019), and Etzel, Siegloch, and Wehrhöfer (2021).

[20] 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.

[21] 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.

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).

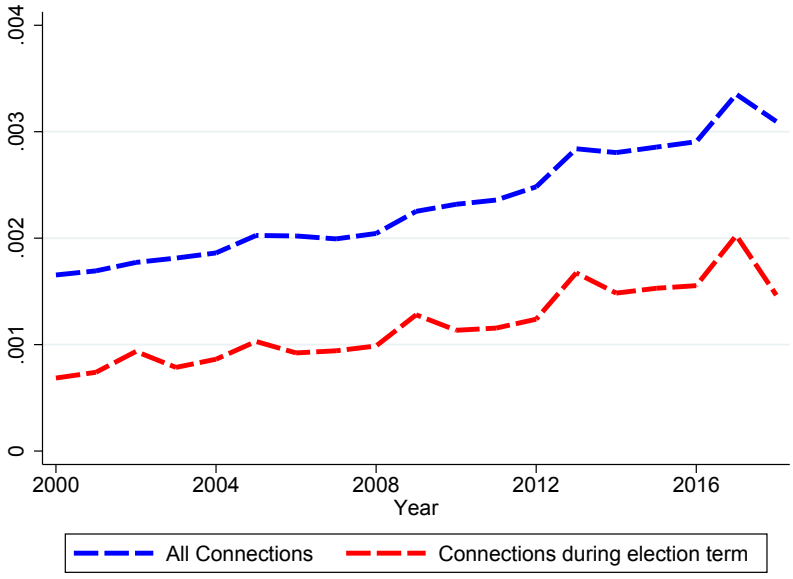
The second data source covers all political candidates between election term 14 and 19 provided to us by the *Bundeswahlleiter* in November 2021. In total, the data contain 24,360 candidate-election year observations with 20,715 unique candidates and 93 unique political parties. Over the six election 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 A.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.

3.2 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.[22]



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

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%, respectively. Second,

[22] Appendix Figure A.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.

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 4.1. 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 revenue than unconnected firms. Overall, connected firms are also more productive as measured by labor productivity.[23]

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, 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 underrepresented 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 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

[23] A further insightful comparison of connected firms is with innovative firms defined as having at least one patent application since 2000. In the MUP environment, we identify about 40,000 patenting firms by matching firm names to PATSTAT. Patenting firms in Germany are large incumbent firms that are also found to be of high labor productivity relative to a random firm in the sample as well as to politically connected firms. Patenting firms are further concentrated in manufacturing, technical services and retail trade.

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.

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 the politician 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.

4 Empirical Strategies

4.1 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 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 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

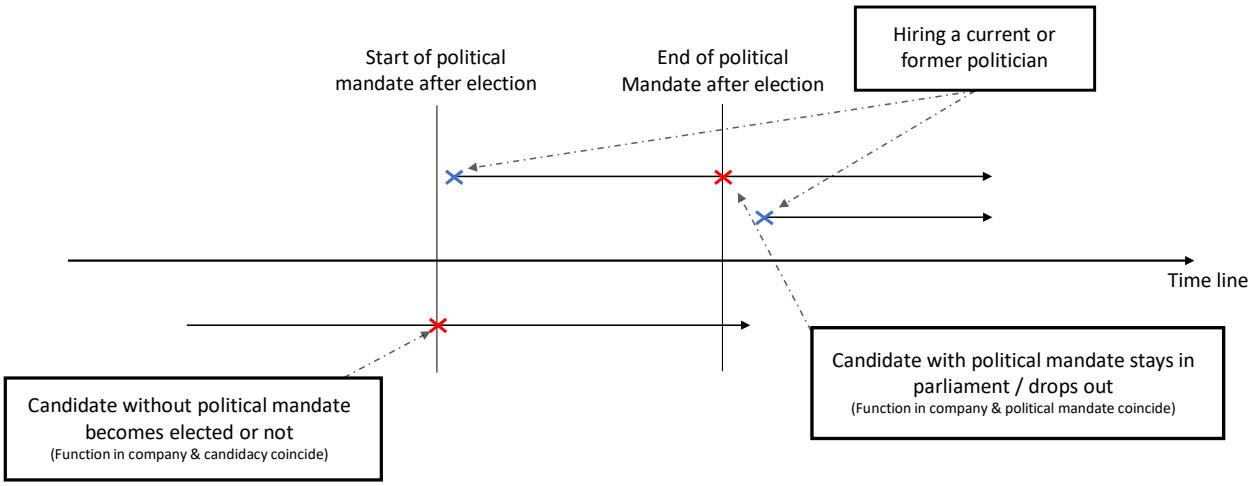


FIGURE 2: TIMING OF EVENTS: CONNECTION TO FIRM AND TO PARLIAMENT

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.

4.2 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, Kim, and Wang (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 and d'Haultfoeuille 2020; Callaway and 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 5.

4.3 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.[24] The main identification assumption is that there is no manipulation around the marginal seat on the party list. We argue in Section 2 that it is hard to predict the marginal seat due to the interaction of 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 6.

5 Impact of Appointing a Politician on Firm-level Dynamics

5.1 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. 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

[24] 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 and Smith 2018).

(for details on the variables and the pre-selection, see Table B.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.[25]

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 B.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.

5.2 Validity Checks

Table B.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.[26] 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.[27]

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 B.1 shows that the employment dynamics of treated and weighted control firms follow a similar trend before the start of the treatment event. Moreover, we see that employment decreases

[25] 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.

[26] 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.

[27] 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 and Strittmatter 2019). These are mainly extremely large stock corporations where we have difficulties to find suitable control firms (see Appendix Table B.3).

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.

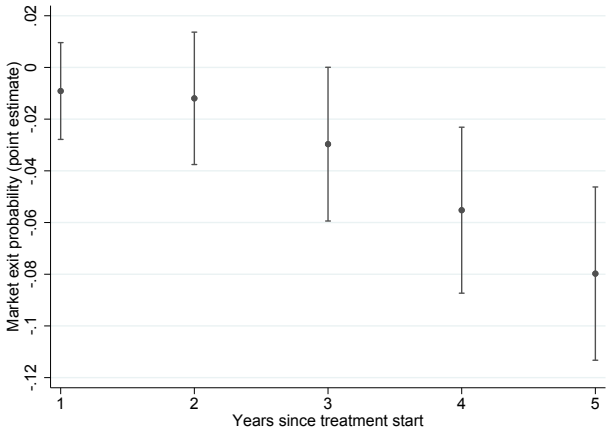
5.3 Estimation Results

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:

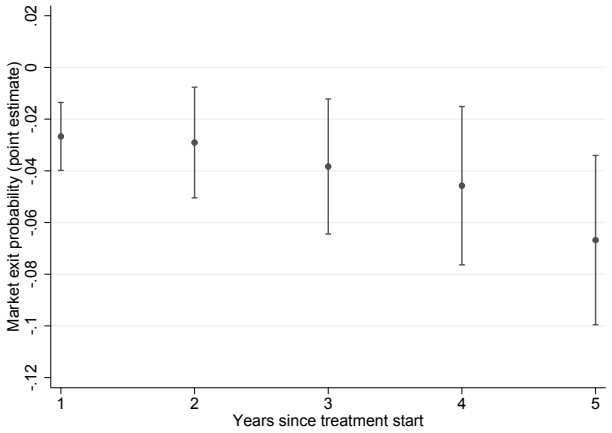
$$y_{i,\tau} = \alpha_\tau + \gamma_\tau I(T_i = 1) + \delta_c + \varepsilon_{i,\tau}, \tag{1}$$

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.



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 B.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

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 6, we present in the following condensed estimates on firm outcomes two years after the appointment event. Table 3 on page 22 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.

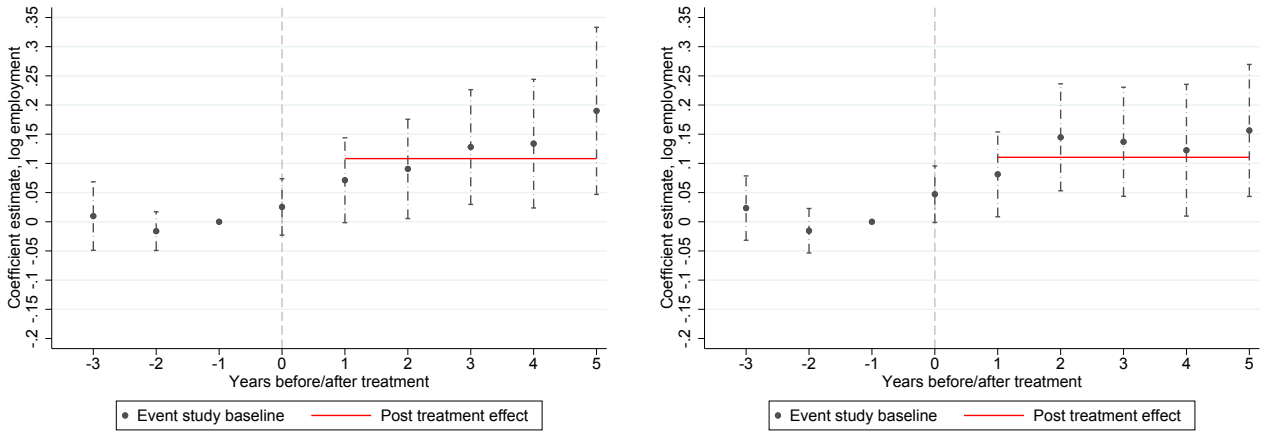
Employment Dynamics. Appendix Figure B.1 shows the development of employment levels in firms appointing a politician and in the weighted sample of control firms. The figure suggests that all groups of firms are on a declining track prior to the start of the new appointment. After the event, this trend 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:

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

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 B.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=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 B.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 B.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 revenue figures to employment. Given recent empirical evidence, labor productivity highly correlates with alternative measures of firm-level total factor productivity (Blackwood, Foster, Grim, Haltiwanger, and Wolf 2021). Specifically, we calculate a simple model-implied productivity measure by calculating $revenue/employment^{\alpha}$, where α is set 0.7 in the baseline. 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 produc-

tivity improvements. Instead, the observed revenue moves in proportion with firm-level employment.

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

	Market exit	Employment		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**	-0.062	-0.034
	(0.013)	(0.043)	(0.034)	(0.065)	(0.037)
Mean in *t=2*	.084	2.694	-.038	13.279	-.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***	0.066	0.067*
	(0.011)	(0.047)	(0.036)	(0.060)	(0.036)
Mean in *t=2*	.084	2.694	-.038	13.279	-.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(revenue/employment$^{\alpha}$) with $\alpha = 0.7$. $\beta_{\tau=2}$ 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 B.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.

5.4 Robustness Checks

In this section, we test the robustness of our findings with respect to different empirical specifications and samples. Specifically, Appendix Tables B.4, B.5 and B.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 and the sensitivity of labor productivity with respect to $\alpha = 1$ is analyzed. Figure B.3 in the Appendix further presents the employment results based on the estimator proposed by De Chaisemartin and d'Haultfoeuille (2020) and weighted by the inverse propensity score weights. Overall, our estimated coefficients are robust to different sample sizes and estimation approaches.

6 The Impact of Winning and Losing Political Connections

6.1 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 parlia-

ment least once, we observe in total 8,690 political candidates (13,002 candidate-year observations).[28] 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 6.2). 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.[29]

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, Todd, and Van der Klaauw 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

[28] 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.

[29] Appendix Figure C.1 shows the number of observations by the marginal seat and Appendix Table C.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, Chyn, and Giuliano (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 6.4.

6.2 Validity Checks

Density of Observations around the Cutoff. Appendix Figure C.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 C.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.

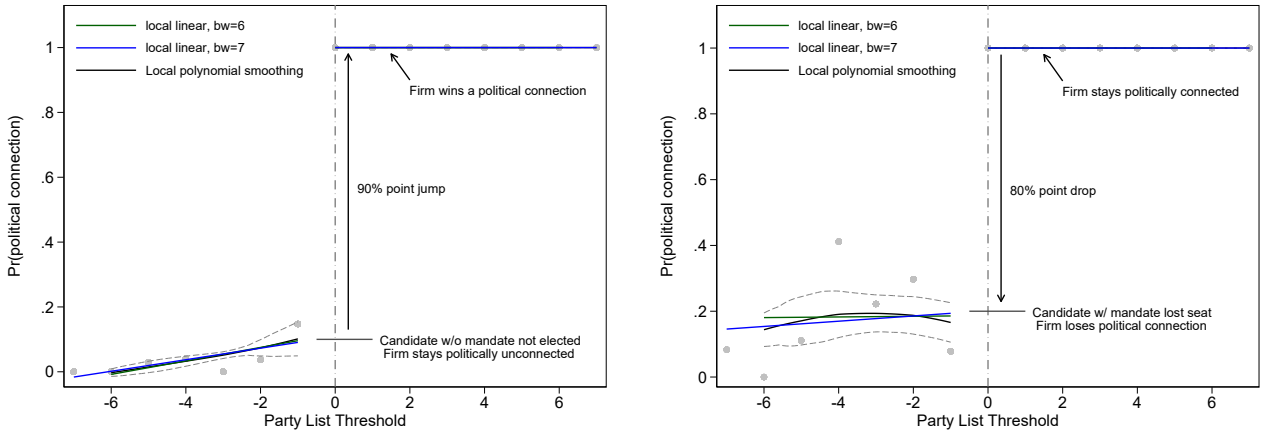
Balancing of Pre-Election Characteristics. In order to support the random nature of the cutoff, predetermined characteristics of firms and political candidates with a position in the firm should balance at the marginal seat cutoff. Table C.3 in the Appendix provides evidence on the balancing of firm- 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 C.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.

6.3 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

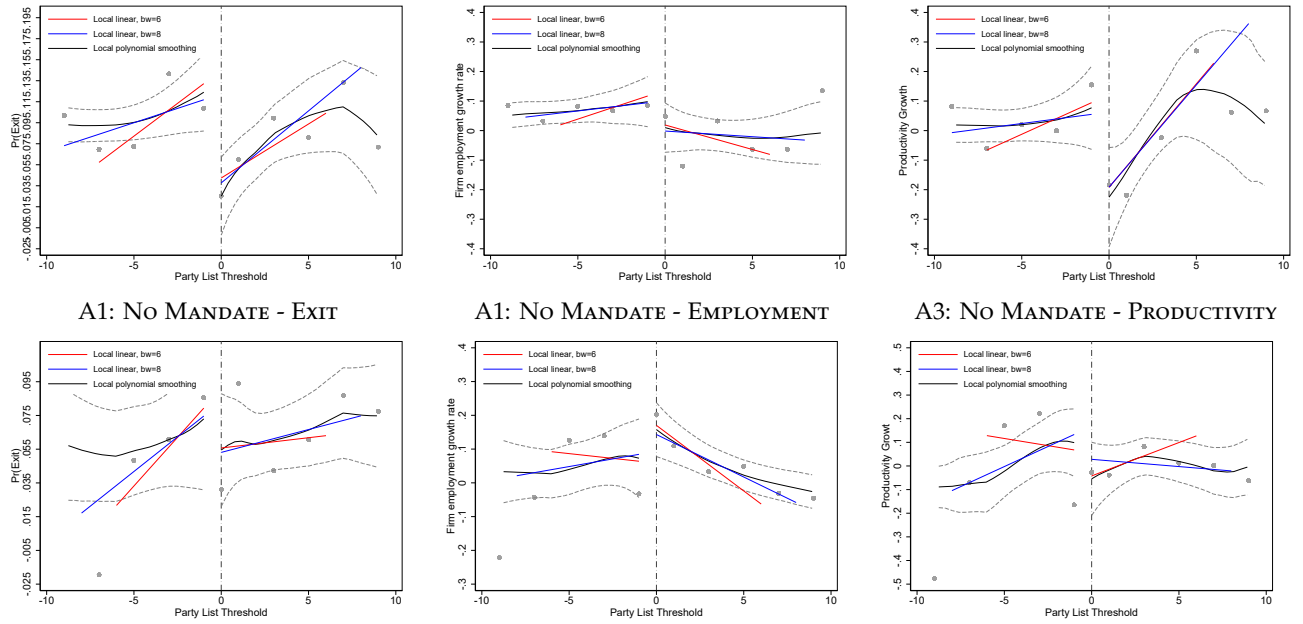
Notes: The figure shows the probability of connecting to the federal parliament by the normalized party list placement of the candidates. Panel A provides the results for candidates without a current political mandate at the time of the election. Panel B provides the results for candidates with a current 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. 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 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 | A1: 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 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 4.3. 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 (and 0 otherwise). For candidates with a current political mandate, we code treatment equal to 1 if the candidate loses his/her 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 $t-1 \to t+2$	Productivity growth $t-1 \to t+2$	Market exit at $t+2$	Employment growth $t-1 \to t+2$	Productivity growth $t-1 \to t+2$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First stage						
Indicator	0.922***	0.871***	0.853***	-0.820***	-0.742***	-0.733***
	(0.0203)	(0.0380)	(0.0429)	(0.0452)	(0.0837)	(0.0899)
Panel B: 2SLS						
Indicator	-0.117***	-0.198	-0.514**	0.047	-0.199	0.323
	(0.0421)	(0.133)	(0.220)	(0.0442)	(0.150)	(0.216)
Mean at threshold	0.0270	0.0690	-0.143	0.0571	0.139	-0.0376
Mean one seat below	0.0946	0.134	0.116	0.0962	-0.0582	-0.104
Initial firm size	62.71	67.34	62.71	149.1	155.4	149.1
Observations	767	324	272	657	290	262
F-Statistic	1312	302.7	206.7	299.2	68.42	54.59
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 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 C.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 C.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.

6.4 Robustness Checks

Covariates and Bandwidth Selection. We provide a set of robustness checks. Table C.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 C.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 C.7 in the Appendix illustrates robustness to the choice of the kernel function and bandwidth selection criteria. For example, Panel A of Table C.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 C.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 productivity growth rates. Likewise, firms that just drop out have no differential effect on firm performance.

Placebo. Table C.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 C.5) in order to overcome potential imprecision problems (Young 2019).

Specifically, we follow Fouka and 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 C.5 in the Appendix confirms the results with a higher precision.

7 Mechanisms

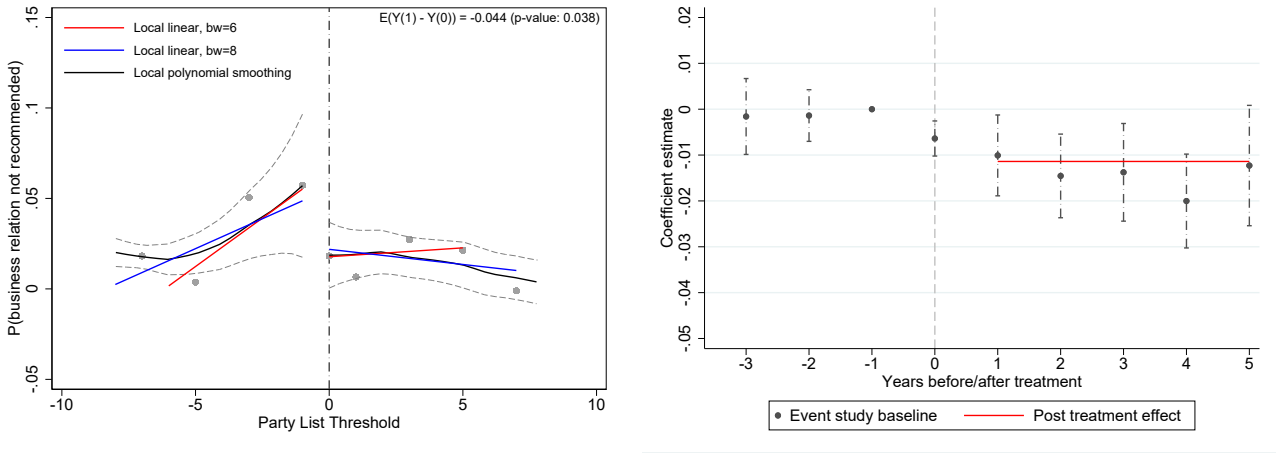
In this section, we provide evidence on potential mechanisms by exploiting and linking further data sources. In particular, we guide our discussion along credit indicators, economic subsidies and public procurement contracts, as well as analyzing the results by sub-samples. Lastly, we study job displacement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

7.1 Credit Ratings

In order to construct an indicator that serves as a proxy for access to credit, we construct 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 D.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 D.1).[30] 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.

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 and 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 and Stowasser (2017) show that German savings banks increase overall lending in the run-up to county elections. Koetter and Popov (2021) document that savings banks increase lending to the state gov-

[30] These results are driven by connections to government parties as opposed to connections to opposition parties (Panel A of Table D.1).



A: Pooled RDD B: Pool 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 B.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

ernment 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, Panizza, and Yanez 2007; Shen and 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.

7.2 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 D.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, Kramarz, Schoar, and Thesmar 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

Table 5: Results - credit rating, subsidies & procurement

	RDD, $t+2$ Election sample		Event Study, $\beta_{\tau=2}$ Appointment sample	
	Without current political mandate (1)	With current political mandate (2)	Appointing a current politician (3)	Appointing a former politician (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 B.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.

a current mandate does generate preferential treatment with respect to subsidies.

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. Although column (1) shows that winning a political mandate as compared to staying out increases the probability of public procurement contracts by 3.8% points[31], overall gaining political connections does not consistently point towards more procurement contracts.

7.3 Heterogeneity Results

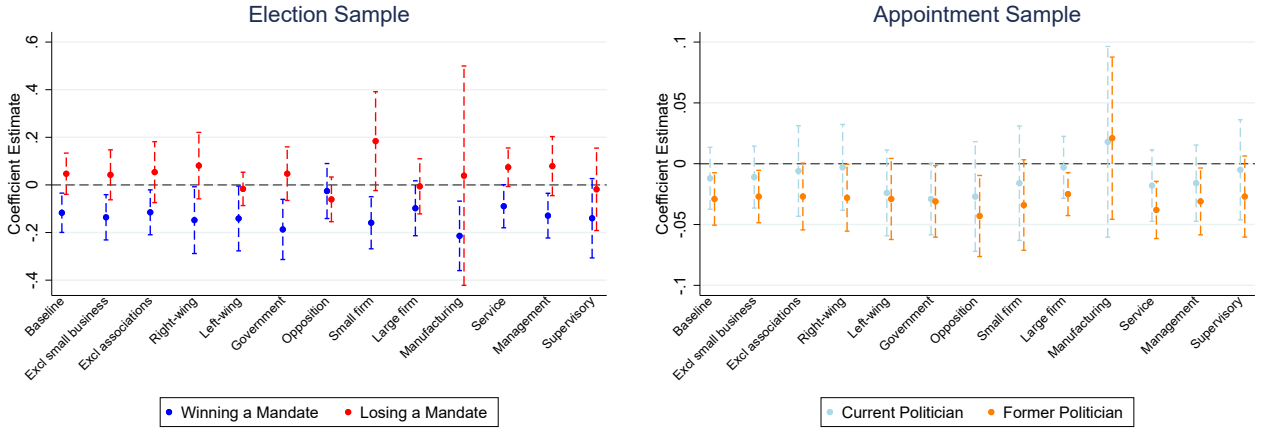
We present heterogeneous effects by sub-samples graphically showing point estimates along with 95% confidence intervals. Figure 8 shows the market exit results. Panel A provides the RDD specification based on the election sample, whereas Panel B show results based on the appointment sample. We refer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

[31] These results are driven by connections to the government party as shown in Panel C of Table D.1.

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. 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 D.4).



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

Notes: The figures present fuzzy RDD estimates from local linear regression discontinuity specifications with bandwidth selection of 6 on market exit panel (A) for sub-samples. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. Robust standard errors are shown in parenthesis. Panels (B) 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 B.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

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 D.4).

7.4 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 D.2 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.

8 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 further provides an unique setting to analyse the contribution of political connections to firm exit, employment, and productivity dynamics jointly with mediating channels.

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 unaffected. These dynamics are facilitated by improvements in credit ratings and better access to subsidies.

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, where these connections carry large risks for the whole German economy. To

fully understand these processes, however, further research will be needed.

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Supplementary Appendix

A Data Addendum

A.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.[32] 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 A.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 A.1: Number of observations over time

	Observations (1)	Obs. with labor (2)	Obs. with sales (3)
2000	667,197	503,864	493,729
2001	699,387	534,661	529,110
2002	718,113	602,682	600,709
2003	730,350	620,879	618,144
2004	743,920	632,795	629,498
2005	752,895	642,907	638,297
2006	760,585	651,210	645,029
2007	765,126	655,984	646,521
2008	766,510	656,158	640,356
2009	767,673	655,363	631,313
2010	767,136	655,965	626,834
2011	765,392	656,704	620,808
2012	756,077	650,010	613,171
2013	742,129	636,602	598,576
2014	725,868	617,861	578,425
2015	704,266	592,364	550,502
2016	676,212	562,404	516,700
2017	639,954	534,839	489,177
2018	576,941	489,543	436,330
2019	210,232	189,147	164,693

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[33] as well as the date of incorporation and closure. The mean values

[32] Bersch, Gottschalk, Müller, and Niefert (2014) provide detailed information on data collection, processing and the definition of variables.

[33] Patent information are provided by the ZEW and represent a merge from PATSTAT to the firm via record linkage.

of log employment, log sales and firm age over time are presented in Table A.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 firm, e.g., owner, CEO, supervisory or executive board member.

Table A.2: Firm level averages over time

	Log employment (1)	Log sales (2)	Firm age (3)
2000	1.45	13.21	21.45
2001	1.43	13.26	21.87
2002	1.37	13.28	22.35
2003	1.34	13.31	22.91
2004	1.32	13.31	23.38
2005	1.31	13.27	23.84
2006	1.30	13.24	24.29
2007	1.31	13.25	24.60
2008	1.31	13.24	24.64
2009	1.32	13.28	23.19
2010	1.33	13.29	25.67
2011	1.35	13.28	26.65
2012	1.38	13.29	27.08
2013	1.40	13.32	27.40
2014	1.43	13.39	27.71
2015	1.46	13.46	27.99
2016	1.52	13.55	28.24
2017	1.58	13.61	28.60
2018	1.66	13.70	28.97
2019	1.85	13.87	28.32

Notes: The table reports means between 2000 and 2019.

A.2 Merging Political Candidates to Firms

The merge between the *Bundestag* candidates and the MUP data requires four major steps. Table A.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 A.3). We use these information to identify individuals in the person data file of the MUP.[34] In total, the MUP person file contains 9.5 million individuals. Panel B of Table A.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

Doherr (2016) provides a detailed discussion about the heuristic approach with an application to patent data and inventor mobility across firms.

[34] 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.

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.

Table A.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

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 A.3).[35] Among the politicians, we observe 4,882 firm IDs, whereas among the candidates who did not enter parliament, 14,864 firm IDs are identified.[36]

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

[35] 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.

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

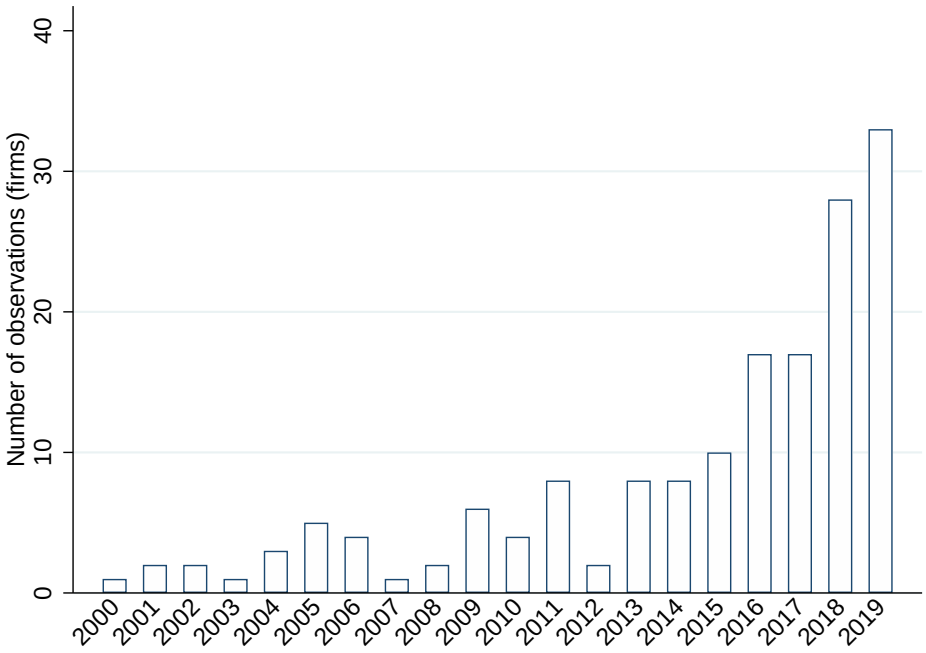
of politicians with outside activities for the election term 1998-2002. These two major selection steps generate a sample of politician-firm connections for 1,420 politicians and 4,374 unique firms. At the politician-firm level, we observe 5,554 connections due to multiple spells.

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 panel file of the MUP, i.e. no observations are available. After dropping these firms, we have 3,842 firms left.[37] 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.

Table A.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.

[37] 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 A.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.



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

Figure A.1: Only one year observed

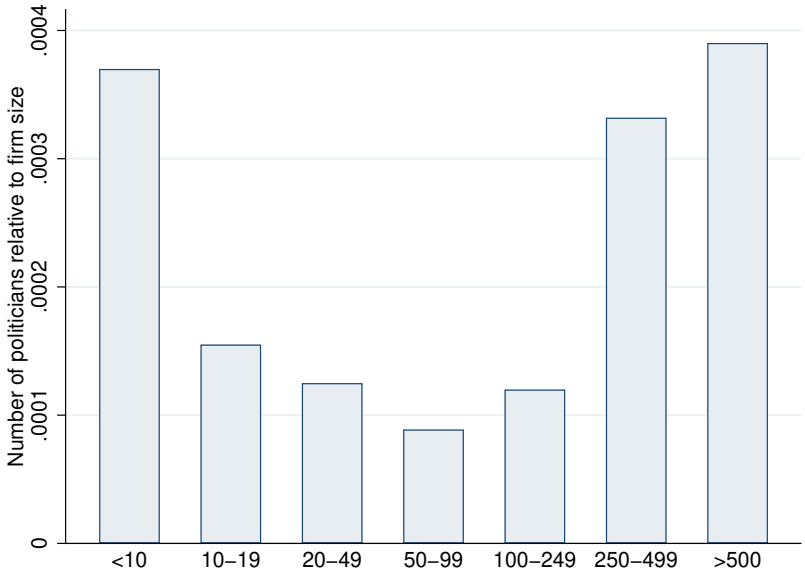
Table A.4: Number of observations by sample

	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.	24,078	16,961	15,804
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 A.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 A.2: Political connected firms over the firm size distribution

B 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 B.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.[38]

Table B.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.

[38] 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.

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 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 B.2 contains a definition of the control variables used in the propensity score estimation.[39]

Table B.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.

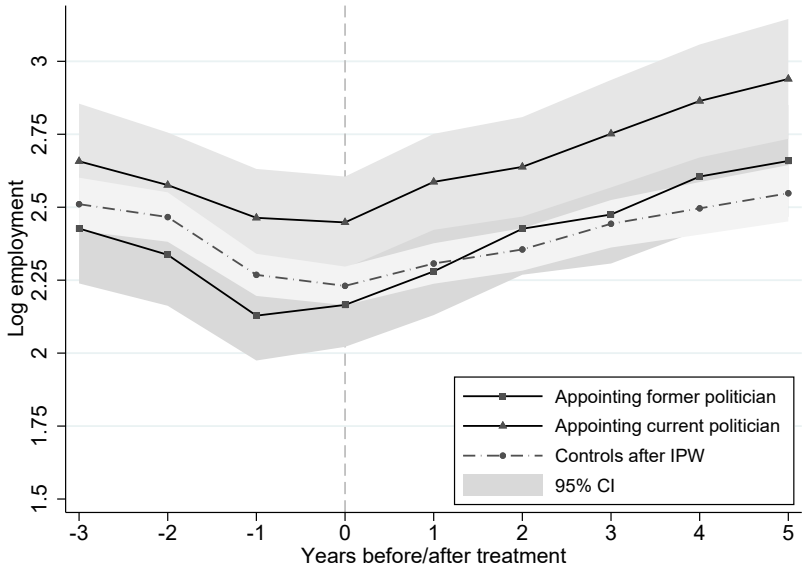
[39] We restrict the set of control variables in case of very low cell occupancy. Low cell occupancy is defined as observing for a dummy variable the value *1* for no treated firms at all, a share of treated firms that is above 95% or below 5% and the number of treated firms equals at least 4 or a share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

Table B.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		

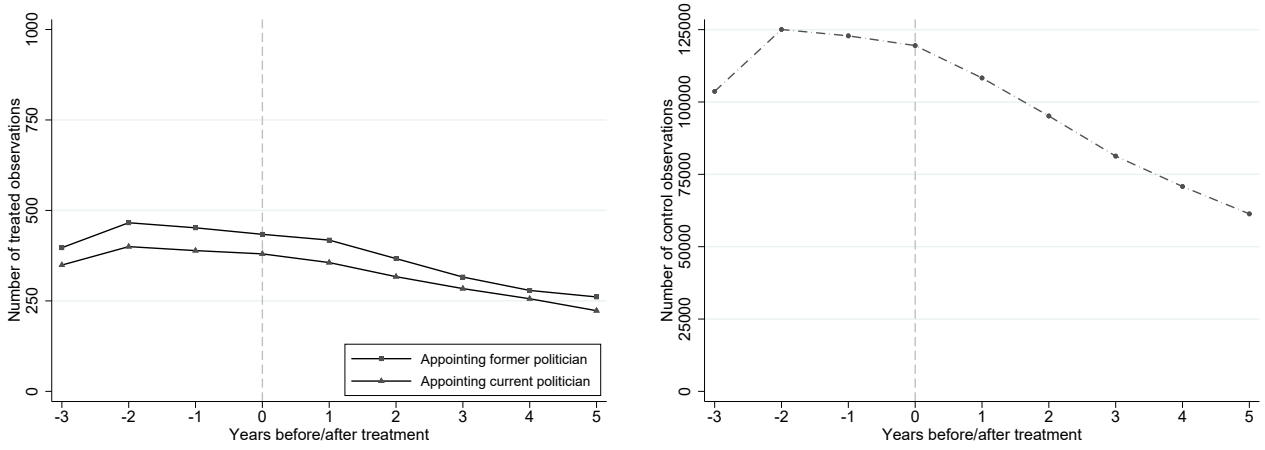
Notes: Std. Diff. = standardized difference. t denotes the year of treatment start. The table shows mean values of pre-treatment (in t-1) firm characteristics for firms that appoint a politician, the un-weighted the re-weighted control group of firms that appoint another person by applying inverse propensity 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 B.2 in the Appendix). Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

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

Table B.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*	.084	.084	.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*	.084	.084	.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 B.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 B.5: The effect of appointing a politician on employment - robustness checks

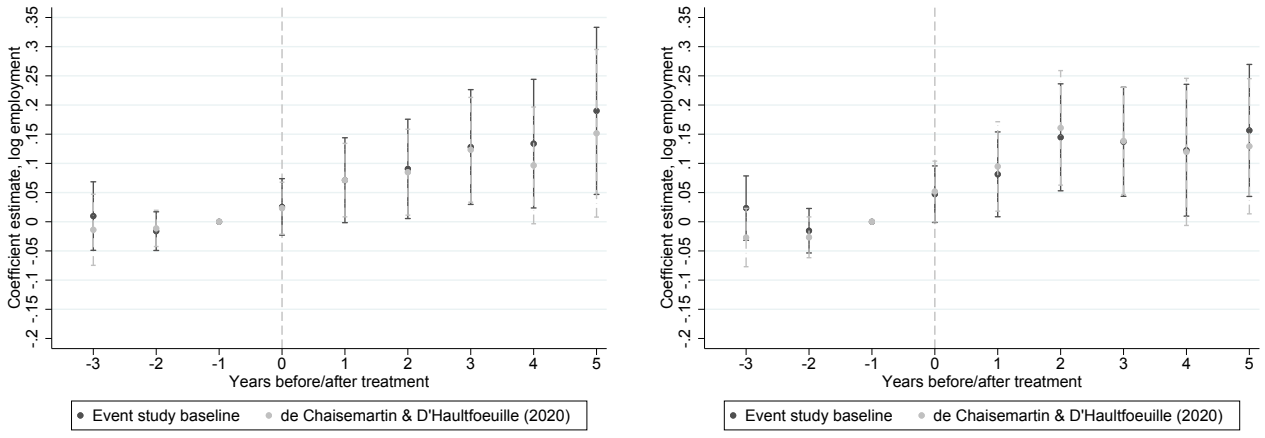
	Baseline $\beta_{\tau=2}$ (1)	DiD (2)	5NN (3)	First treatment (4)	≥ 1 pre-obs of outcome (5)
Panel A: Appointing a current politician					
Connection	0.091**	0.108**	0.074	0.086*	0.048
	(0.043)	(0.044)	(0.045)	(0.049)	(0.039)
Mean in *t=2*	2.694	2.694	2.638	2.694	2.474
Control observations in *t=0*	125,050	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 B.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 B.6: The effect of appointing a politician on productivity - robustness checks

	Baseline $\beta_{\tau=2}$	DiD	$\alpha = 1$	5NN	First treatment	≥ 1 pre-obs of outcome
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Appointing a current politician						
Connection	-0.062	-0.054	-0.090	-0.034	-0.045	-0.020
	(0.065)	(0.060)	(0.065)	(0.066)	(0.061)	(0.059)
Mean in *t=2*	13.279	13.279	12.457	13.236	13.279	13.223
Control observations in *t=0*	125,050	125,050	125,050	4,043	125,050	173,865
Treated observations in *t=0*	400	400	400	400	313	465
Panel B: Appointing a former politician						
Connection	0.066	0.086**	0.021	0.086	0.066	0.047
	(0.060)	(0.042)	(0.063)	(0.062)	(0.065)	(0.055)
Mean in *t=2*	13.279	13.279	12.457	13.236	13.279	13.223
Control observations in *t=0*	125,050	125,050	125,050	4,043	125,050	173,865
Treated observations in *t=0*	466	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(revenue/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(revenue/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 B.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 B.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.

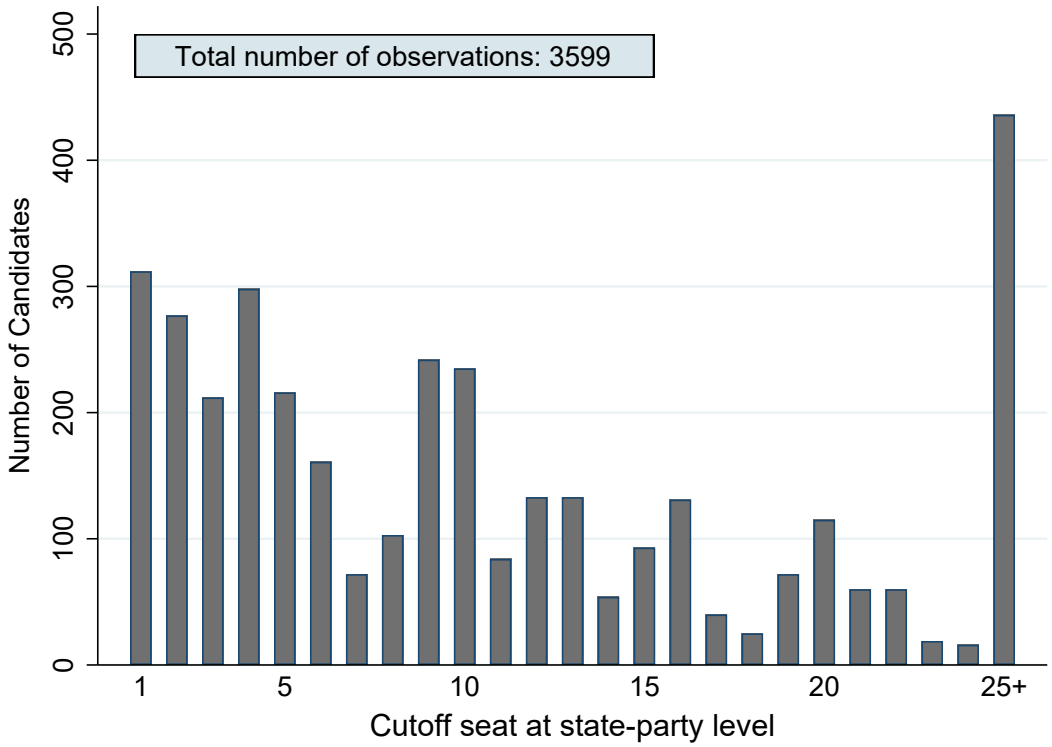
Figure B.3: The effect of appointing a politician on employment dynamics - Estimator by de Chaisemartin and D'Haultfoeuille (2020)

C Additional Empirical Results on Election Discontinuities

Table C.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.



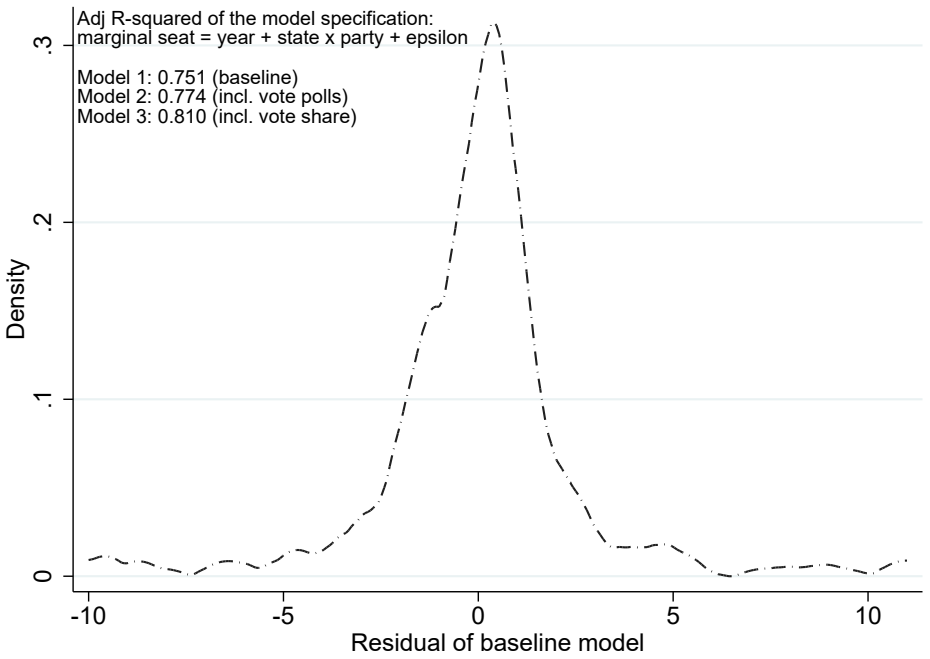
Notes: The figure shows the number of candidates by the identified marginal cutoff seat at the regional (federal state) and political party level. For example, there are about 312 candidates at the state-party level where only the first candidate enters parliament via the party list (*Landesliste*).

Figure C.1: Number of observations per cutoff seat

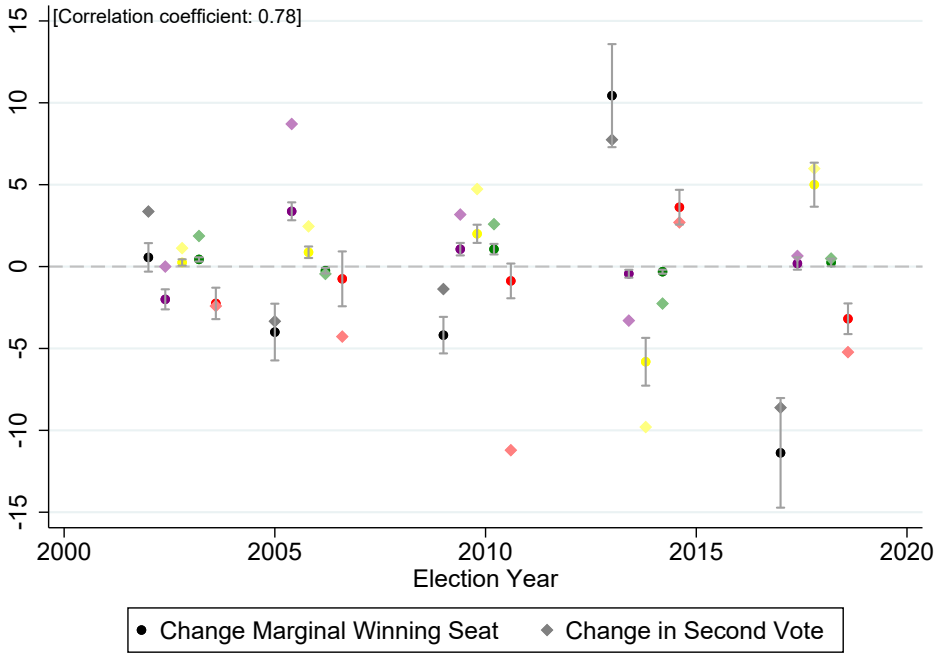
Table C.2: Observations at closest mass points

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.



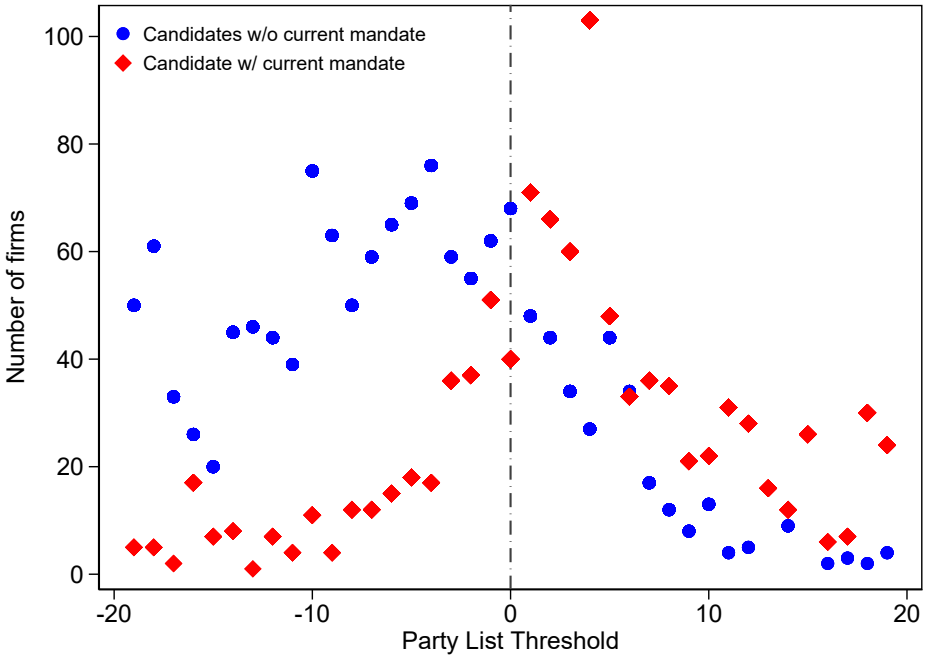
A: Residual Variation



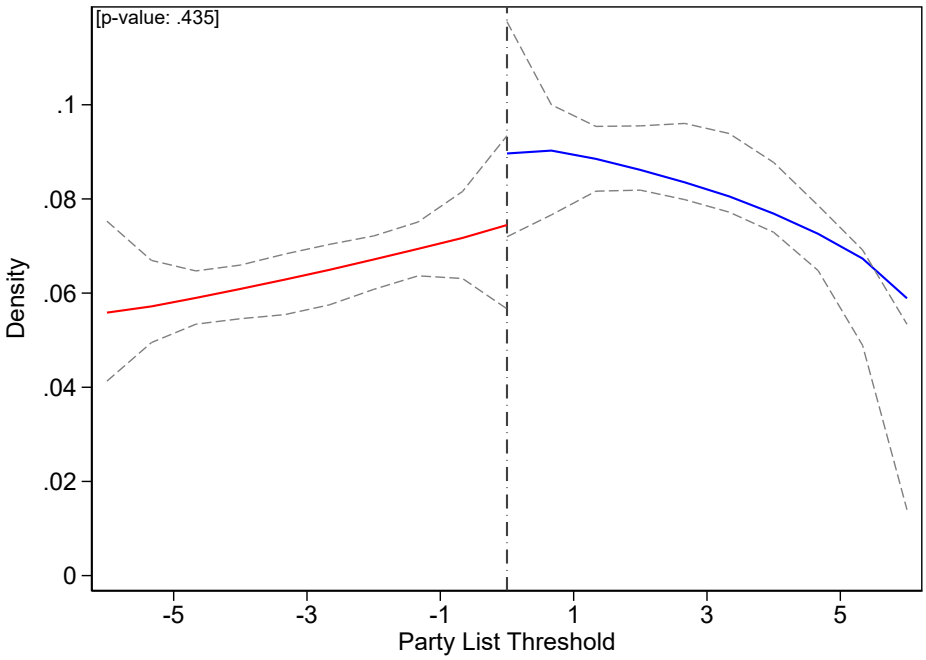
B: Association between Vote and Cutoff

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 C.2: Marginal seat variation



A: Number of Firms



B: MC Test

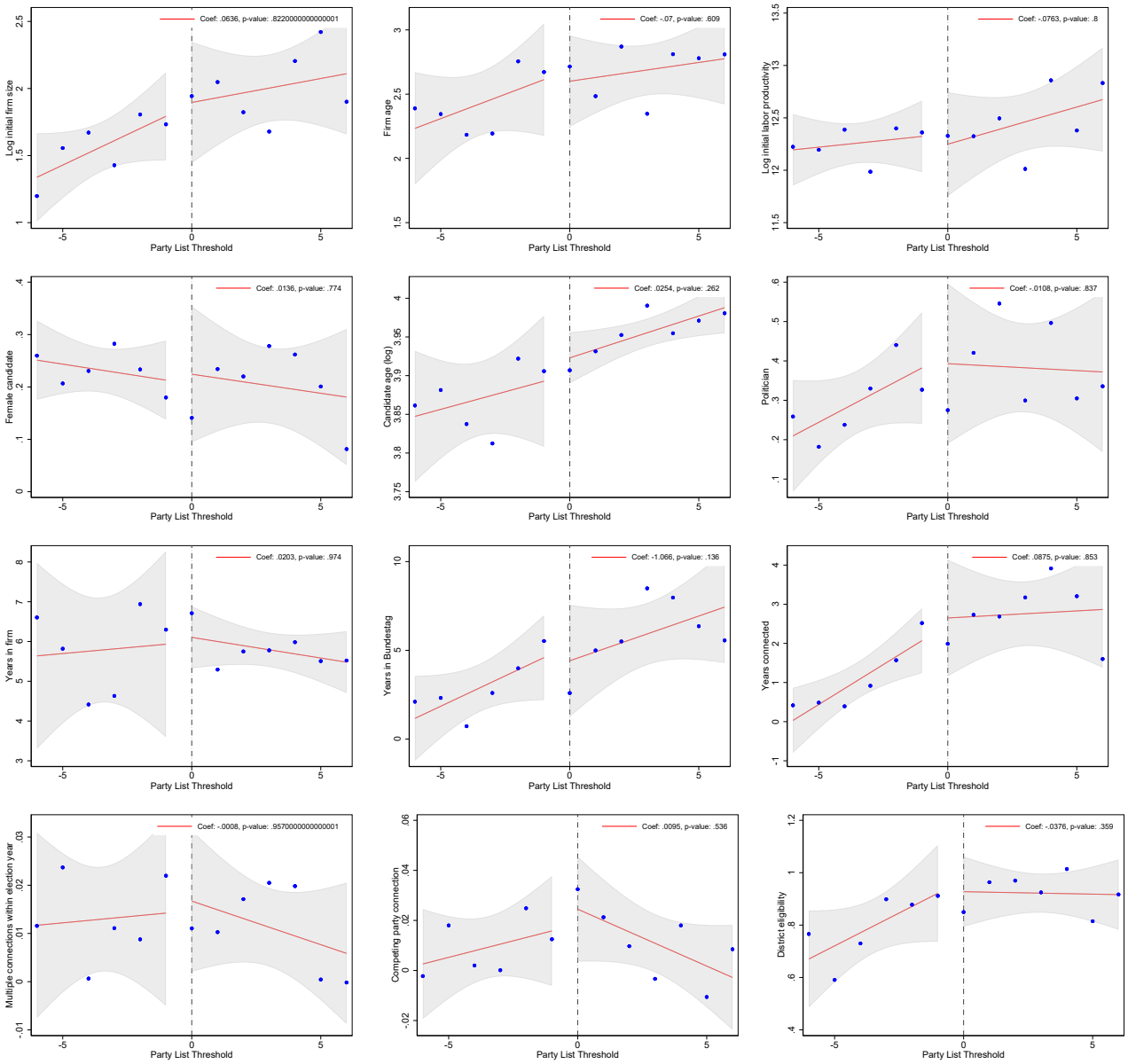
Notes: Panel A shows the number of firms for the normalized party list where 0 represents the marginal seat that just enters parliament. The blue dots represent firms connected to a candidate without a current mandate. The red dots represent firms connected to candidates with a current mandate. Panel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

Figure C.3: Test for manipulation of the cutoff

Table C.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 C.4: Balancing at the cutoff (pooled)

Additional Results on the Effect of Winning and Losing Political Connections

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

	Exit in			Employment growth $t-1 \rightarrow$			Productivity 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	-0.387***	-0.528**	-0.520**
	(0.0350)	(0.0421)	(0.0700)	(0.103)	(0.127)	(0.159)	(0.141)	(0.217)	(0.228)
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	0.0921	0.123	0.240
Mean above	0.0154	0.0385	0.112	0.00954	0.0262	-0.0291	-0.159	-0.232	-0.216
Panel B: Mandate at election ("Staying connected (=0) vs. dropout (=1)")									
Indicator	0.0366	0.0478	-0.0255	-0.0550	-0.272	-0.328*	0.0946	0.242	0.247
	(0.0374)	(0.0506)	(0.0724)	(0.121)	(0.169)	(0.171)	(0.209)	(0.259)	(0.312)
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	-0.173	-0.106	-0.0248
Mean above	0.0286	0.0571	0.111	0.111	0.147	0.164	-0.0572	-0.0333	0.0230

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 C.5: 2SLS results - additional covariates

	Exit in $t+2$		Employment growth $t-1 \rightarrow t+2$		Productivity 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	-0.578**	-0.617**
	(0.0454)	(0.0461)	(0.154)	(0.153)	(0.254)	(0.259)
F-Statistic	946.5	1079	176	193.6	135	146
Observations	731	731	298	298	260	260
Panel B: Mandate at election ("Staying connected (=0) vs. dropout (=1)")						
Indicator	0.0377	0.0383	-0.209	-0.214	0.287	0.293
	(0.0531)	(0.0535)	(0.186)	(0.195)	(0.286)	(0.296)
F-Statistic	297.8	301.4	63.18	59.40	48.92	45.78
Observations	621	621	265	265	237	237
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 C.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)"), productivity growth t+2						
Indicator	-0.758**	-0.430**	-0.287*	-0.125	-0.839***	-0.560**
	(0.350)	(0.197)	(0.164)	(0.122)	(0.280)	(0.234)
Mean outcome	-0.00582	0.0103	0.0221	-0.00771	0.0152	0.0115
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)"), productivity growth t+2						
Indicator	-0.516*	0.310	0.446**	0.154	0.240	0.240
	(0.297)	(0.229)	(0.222)	(0.152)	(0.284)	(0.278)
Mean outcome	0.0758	0.0374	0.0148	0.0608	0.0624	0.0567
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 C.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	Productivity	Exit	Employment	Productivity
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Triangular kernel						
Connection	-0.119***	-0.0816	-0.632***	0.0349	-0.182	0.307
	(0.0438)	(0.0894)	(0.241)	(0.0442)	(0.119)	(0.190)
Bandwidth h	6.824	14.91	6.758	11.10	12.10	11.23
Panel B: Uniform kernel						
Connection	-0.134**	-0.0743	-0.569**	0.0410	-0.218	0.262
	(0.0519)	(0.0891)	(0.230)	(0.0446)	(0.137)	(0.257)
Bandwidth h	4.668	11.10	5.127	8.181	9.141	7.688
Panel C: Two-sided MSE-optimal bandwidth						
Connection	-0.0860***	-0.0741	-0.442***	0.0361	-0.179	0.284
	(0.0319)	(0.0895)	(0.155)	(0.0448)	(0.120)	(0.188)
Bandwidth $-h$	13.55	15.34	12.14	10.73	11.35	10.82
Bandwidth $+h$	8.450	13.34	8.114	10.79	13.98	14.75
Panel D: CE-optimal bandwidth						
Connection	-0.111**	-0.106	-0.760**	0.0437	-0.282**	0.171
	(0.0542)	(0.104)	(0.313)	(0.0514)	(0.135)	(0.213)
Bandwidth h	4.659	10.64	4.845	7.768	8.812	8.215
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 C.8: Local randomization results - marginal seat vs. seat below

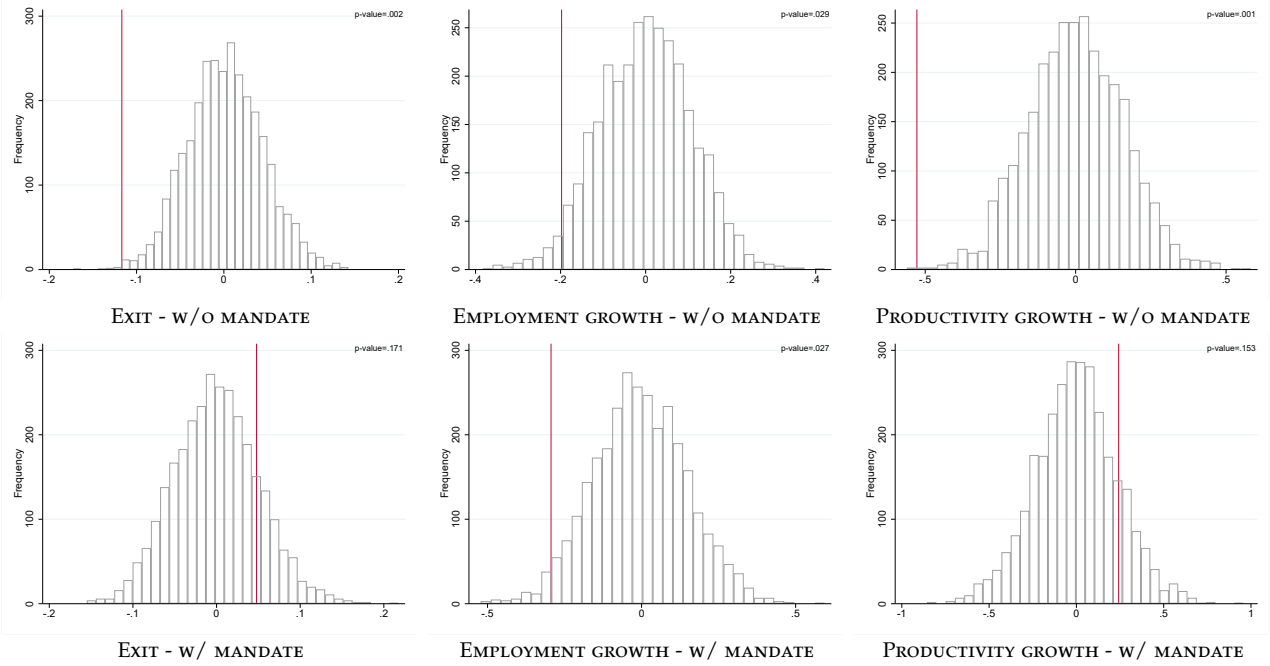
	Candidates without mandate "Winning: marginal seat (=1)"			Candidates with mandate "Dropout: seat below (=1); unlucky (=1)"		
	Market exit at $t+2$	Employment growth $t-1 \to t+2$	Productivity growth $t-1 \to t+2$	Market exit at $t+2$	Employment growth $t-1 \to t+2$	Productivity growth $t-1 \to t+2$
	(1)	(2)	(3)	(4)	(5)	(6)
Indicator	-0.097***	-0.153	-0.306**	0.017	-0.090	0.134
	(0.003)	(0.112)	(0.025)	(0.620)	(0.342)	(0.337)
Mean marginal seat	0.038	-0.012	-0.182	0.057	0.139	-0.037
Mean below	0.135	0.139	0.123	0.074	0.049	0.096
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 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 C.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 productivity growth, $t-5$ to $t-1$						
Indicator	0.0338	0.0692	0.0341	0.0364	0.0485	0.0363
	(0.0618)	(0.0551)	(0.0444)	(0.0669)	(0.0609)	(0.0489)
Observations	289	413	525	254	367	415
F-Statistic	314.6	668.5	1050	119.3	137	150
Mean outcome	0.00529	-0.000979	0.00222	0.0327	0.0328	0.0292
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 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



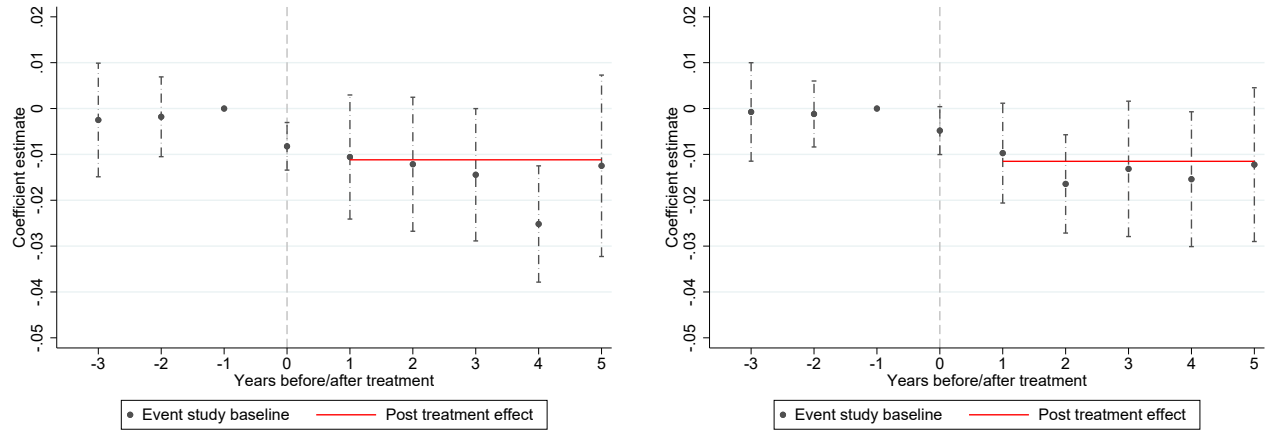
Exit - w/o mandate | Employment growth - w/o mandate | Productivity growth - w/o mandate

Exit - w/ mandate | Employment growth - w/ mandate | Productivity 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 C.5: Permutation results

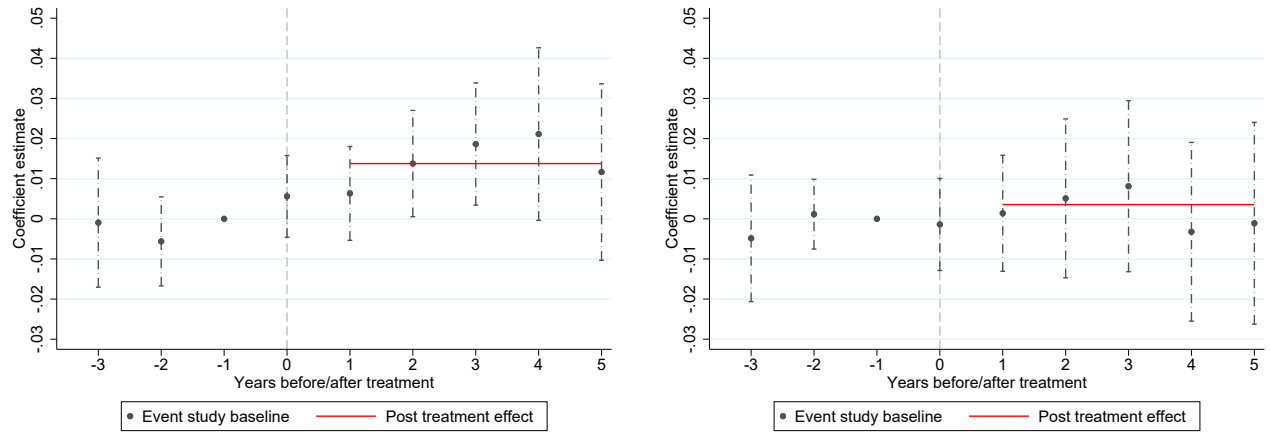
D Additional Empirical Results on the Mechanism



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 B.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 B.2. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

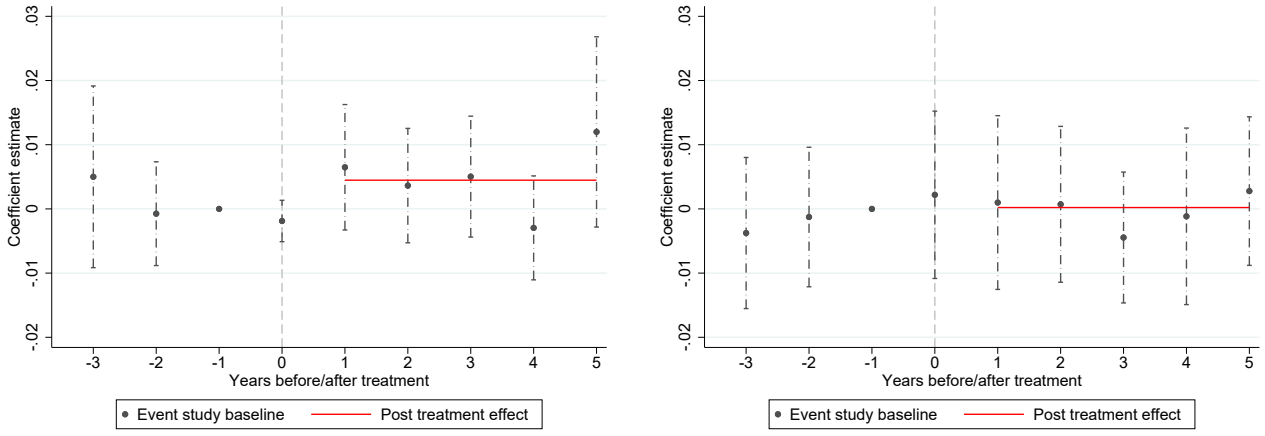
Figure D.1: The effect of appointing a politician on credit rating dynamics



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 B.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 B.2. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

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



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

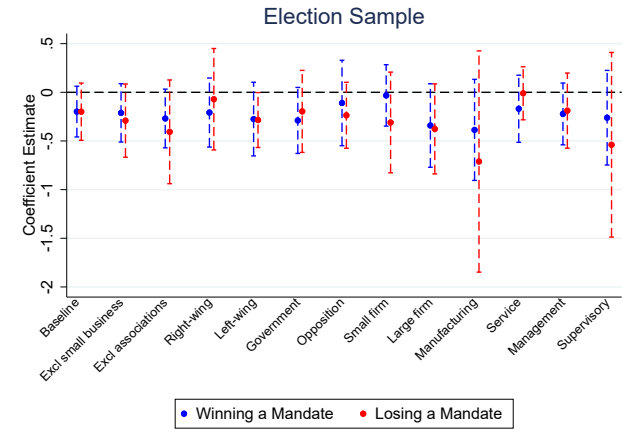
Notes: The figure shows the effect of appointing a current (Panel A) or former (Panel B) politician at time *t=0* on public procurement in *t=τ, τ=-3 to 5*. The black point estimates are based on equation (2). The regression is weighted by the inverse propensity score. Propensity score estimation is 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 B.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 B.2. Standard errors are clustered at the firm level. 95% confidence intervals around point estimates.

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

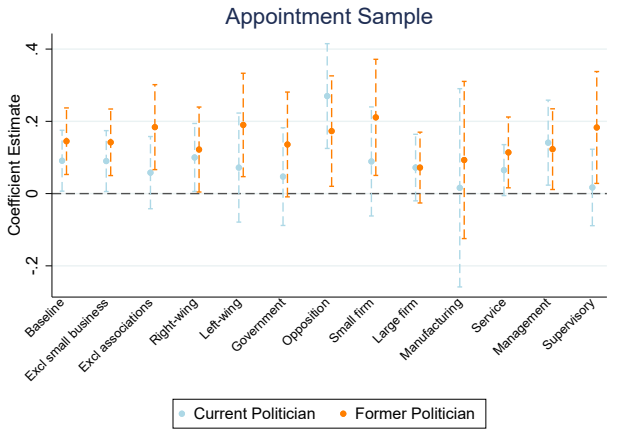
Table D.1: Results - credit rating, subsidies & procurement by government associations

	RDD, $t+2$ Election sample				Event Study, $\beta_{\tau=2}$ Appointing sample			
	Without current political mandate		With current political mandate		Appointing a current politician		Appointing a former politician	
	Govern. (1)	Oppos. (2)	Govern. (3)	Oppos. (4)	Govern. (5)	Oppos. (6)	Govern. (7)	Oppos. (8)
Panel A: P(Business relation not recommended)								
Indicator	-0.0285	-0.0589	0.0529	0.0460	-0.0184**	-0.0068	-0.0221***	-0.0079
	(0.0298)	(0.0696)	(0.0817)	(0.0474)	(0.0081)	(0.0132)	(0.0019)	(0.0109)
Observations	143	166	181	130	942,891	942,257	943,030	942,541
Mean	0.0387	0.044	0.0144	0.017	0.0293	0.0293	0.0293	0.0293
F-Statistic	116.6	210.3	30.75	66.15	-	-	-	-
Panel B: P(Subsidies)								
Indicator	0.015	0.001	0.005	-0.007	0.0134	0.0187*	0.0072	-0.0035
	(0.0183)	(0.0153)	(0.0239)	(0.008)	(0.0101)	(0.0111)	(0.0120)	(0.0226)
Observations	335	477	431	322	969,702	969,065	969,885	969,356
Mean	0.021	0.013	0.020	0.014	0.0303	0.0303	0.0303	0.0303
F-Statistic	732.2	822.0	123.1	973.5	-	-	-	-
Panel C: P(Public Procurement)								
Indicator	0.0527**	0.0182	-0.0396	0.0197	0.0065	-0.0049	-0.0017	0.0008
	(0.0274)	(0.0156)	(0.0443)	(0.0199)	(0.0066)	(0.0053)	(0.0097)	(0.0120)
Observations	306	175	246	188	793,677	793,051	793,835	793,286
Mean	0.009	0.006	0.011	0.008	0.0099	0.0099	0.01	0.01
F-Statistic	187.5	912.1	120.2	373.2	-	-	-	-

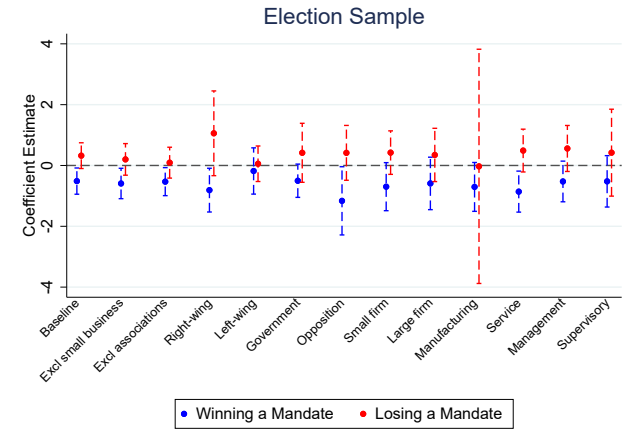
Notes: The table presents fuzzy RDD estimates from local linear regression discontinuity specifications with bandwidth selection of 6 in columns (1) to (4). All specifications control for the outcome variable measured before the election. Election years covered are 1998, 2002, 2005, 2009, 2013 and 2017. Robust standard errors are shown in parenthesis. Columns (5) to (8) 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 B.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.



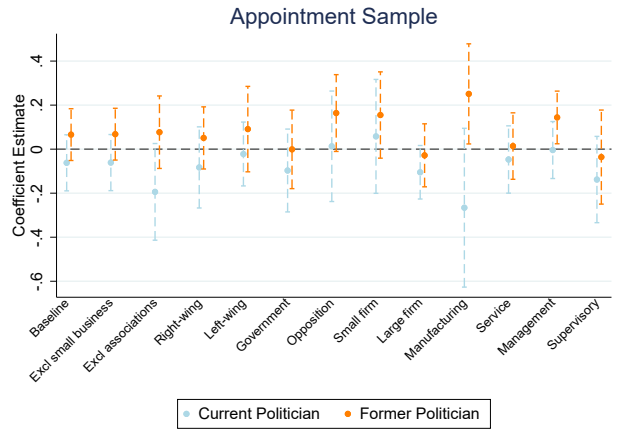
A1: Employment, RDD, $t+2$



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



A2: Productivity, RDD, $t+2$



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

Notes: The figures present fuzzy RDD estimates from local linear regression discontinuity specifications with bandwidth selection of 6 on employment growth and productivity growth panels (A1) and (B1), 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 B.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 D.4: Market Exit Effects by Sub-Samples

Table D.2: 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.

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