The Cost of Favoritism in Public Procurement*

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Abstract

Are political connections in public procurement harmful or efficiency-gaining for the public sector, and what are the costs of favoritism toward politically connected firms? Exploiting detailed data on firm representatives' political affiliations in the Czech Republic, we find that favoritism toward politically connected firms increases the price of procurement contracts by 6% of the estimated costs, while no gains in terms of quality are generated. Interestingly, these adverse effects of political connections are not present for procurement contracts that get additional oversight from a higher level of the government because they were co-funded by the European Union. Based on our estimates, total procurement expenditures increased by 0.36% due to favoritism.

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1 Introduction

In 2005, a medium-sized Czech company called Viamont won railway construction procurement contracts worth 25 million USD. One year later, the owner of the company, Aleš Řebíček, was elected to the Chamber of Deputies and became the new Minister of Transportation. He sold the company but remained an external consultant. In the following two years, the firm won public procurement contracts worth 75 million USD annually, tripling its revenues from public procurement contracts in Czechia.[1] Several studies confirm this anecdotal evidence of politically connected firms winning disproportionate amounts of procurement contracts (Baltrunaite, 2020; Goldman et al., 2013; Do et al., 2015; Dicko, 2016; Schoenherr, 2019; Titl and Geys, 2019; Brogaard et al., 2020; Brugués et al., 2020).

We contribute to this literature by evaluating the performance of public procurement contracts delivered by politically connected firms and quantifying the cost of this type of favoritism in a procurement market that accounts on average for 12% to 20% of GDP in OECD countries. The question whether favoritism is harmful or efficiency-enhancing for the public sector was posed three decades ago but remains largely open (Shleifer and Vishny, 1993).

On the one hand, preferential treatment of some firms may lead to misallocation of contracts to less competitive yet connected firms. This favoritism would be associated with inefficiencies (Mauro, 1995; Shleifer and Vishny, 1993). Moreover, as Shleifer and Vishny (1993) argue, red tape for non-connected firms might increase dramatically because of corruption, especially in countries with weak institutions. On the other hand, personal connections might promote easier and cheaper cooperation between a firm and a government agency (Mauro, 1995). Similarly, if more productive firms are more likely to establish a relationship with politicians, favoritism might bolster efficiency (no matter the higher chances of winning,

[1] This is a well-known story documented by an investigative journalist from the major Czech media. See, for example `https://www.idnes.cz/zpravy/domaci/firma-kterou-vlastnil-ministr-rebicek-ziskala-zakazky-za-3-5-miliardy.A080516_220226_domaci_zra`.

higher volumes and/or higher prices).

Our main contribution lies in answering the aforementioned question. We show that contracts allocated to politically connected firms are associated with adverse contract-level outcomes: they are overpriced and not of higher quality. We calculate the approximate costs of this favoritism. On top of that, we provide suggestive evidence that additional oversight from a higher level of government mitigates the effects of political connections on price.[2]

The previous literature largely focused on the benefits for personally connected firms (Goldman et al., 2013; Do et al., 2015; Schoenherr, 2019) and the channels behind political favoritism (including discretion and renegotiation, see Brogaard et al., 2020; Szucs, 2020).[3] Schoenherr (2019) provides evidence that procurement contracts delivered by connected firms are more likely to need a repair. With our approach, we can actually quantify the cost of the follow-up repairs and evaluate the total additional costs for the public sector. The most recent evidence from Ecuador also suggests that politically connected firms receive higher volumes of contracts and charge higher prices than unconnected firms, and that the chosen firms are less efficient (Brugués et al., 2020). In this study, we examine the quality of delivered procurement projects rather than the productivity of procurement suppliers. Our evidence on the role of oversight also provides additional guidance on how to mitigate the adverse effects of the favoritism because we document that oversight can be effective in this matter.

We construct a unique database of links between politicians and firms. In the Czech Republic, each firm must publish details about both its ownership structure and its management (including the names of board and supervisory board members). Using data about political candidates in 11 recent elections, we identify firms that have direct (personal) links to political parties. We subsequently link these connections with electoral outcomes and

[2]In our case, agencies set up by the Czech ministries provide additional oversight over regional and municipal procurement projects that are co-funded by the European Union.

[3]We do not observe this in our data. This is most likely given by the specificity of the Czech institutional setting. See Section 4.5 and Section E in the Appendix for details. We suggest that tailoring of public procurement contracts is an important channel.

data on procurement projects, where we leverage the availability of detailed contract-level data. For example, we have access to reliable engineering estimates of the costs for most procurement contracts. Thus, we can compare the final price to the engineering estimate of the costs when a contract is allocated to a firm with a political connection, relative to a contract delivered by the same firm with no connection.

Our results show that favoritism does not lead to better outcomes, as contracts are persistently overpriced by approximately 6%, while the quality is not better. We show the effect of political connections while controlling for each firm-procurer pair. A firm's connection disappears (emerges) because politicians to whom the company is connected are voted out of (elected to) the office. There are 11 elections that took place in the studied period. These elections can induce a change in the connection status. Hence, our empirical approach is based on two-way fixed effects (TWFE) estimation with staggered treatment timing. Recent literature shows that this approach could lead to invalid inference in the presence of heterogeneous treatment (Baker et al., 2022; Borusyak K. and Spiess, 2022; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; de Chaisemartin and D'Haultfoeuille, 2022; Gardner, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). We therefore augment our baseline estimation strategy with new estimators developed by (de Chaisemartin and D'Haultfœuille, 2020; de Chaisemartin and D'Haultfoeuille, 2022) and (Gardner, 2021). The baseline and these more robust estimators lead to very similar results.

Although the price increases are worrying by themselves, there are still scenarios in which price increases might be compensated for by better outcomes in another dimension. We tackle this issue in two steps. First, we look at commercially available goods with presumably little quality differentiation, such as raw materials or bricks. We isolate goods that are homogeneous and can be purchased in supermarkets or specialized stores. Even in this sub-sample, we find that the contracts are significantly overpriced when delivered by politically connected firms.

Second, we also examine directly whether politically connected firms deliver services of

higher quality. Measures of quality are not generally available, which makes this a complicated issue. Therefore, we develop a new method for calculating a measure of quality for construction projects that has been proposed in Baranek (2020). With contract-level data such as short descriptions, dates of award, and procurers' and buyers' identities, we use text analysis to link construction projects with their subsequent repairs.[4] We then calculate a measure of the total lifetime cost of a particular construction project. The findings show a small and statistically insignificant adverse effect on the quality of projects delivered by connected companies.

Our approach also allows us to calculate the total cost of favoritism for the public sector. We conclude that it amounts to approximately 93 million USD annually. This cost can be interpreted as a transfer from taxpayers to connected firms. It is unlikely, also, that the direct monetary effect is the only cost induced by favoritism. Such behavior by public institutions may also lead to lower trust in the institutions, the government, and the rule of law, and consequently, it can undermine the functioning of democracy in the country. Although we cannot take into account these effects of favoritism, we acknowledge their existence. At the same time, there might also be savings we cannot account for, such as cheaper ex-post monitoring. If the contracts supplied by politically connected firms were significantly cheaper to monitor ex-post, the savings could offset the additional cost induced by the favoritism. We do not have data to examine this empirically.

We analyze the mechanisms by which procurers allocate contracts to the favored bidders. Coviello and Mariniello (2014); Coviello et al. (2017); Palguta and Pertold (2017); Titl and Geys (2019) find that procurers use discretion and restrict competition to favour specific firms. Perhaps surprisingly, we do not see that the main channel of contract allocation relates to choosing restricted mechanisms, but we observe overall lower competition in contracts allocated to connected firms. An alternative channel is politicians' tailoring of the project specifications to the advantage of one firm. By tailoring the technical specifications, procurers

[4] A manual verification procedure shows that the algorithm is correct in 93% of cases.

both (i) restrict competition and (ii) make the favored bidder more competitive. To provide evidence of tailoring of procurement contracts, we show that politically connected firms receive contracts with longer and more complicated descriptions.

Procurers appear to use sophisticated methods to mask corruption, which makes it difficult to suggest policies that would help alleviate this problem. One possibility is to have an independent agency overlook the allocation of contracts. The contracts that are co-funded by the European Union are additionally overseen by national agencies and the European Commission. In our data, we can distinguish between contracts that are fully covered by national resources and those that are co-funded. Our results show that projects that are allocated to politically connected firms are not overpriced when they are overseen by an agency set up at a higher level of government. This provides suggestive evidence that additional oversight might be beneficial as long as it is not done at the same level of government.[5]

We structure the paper in the following way. Section 2 describes the institutional features of the Czech procurement market. Section 3 describes the data. In Section 4, we present the empirical analysis of the effects of political connections on prices, quality of public works, and other procurement market outcomes. Section 5 presents the cost calculation. Section 6 concludes and lays out the policy implications of our findings.

2 Institutional Setting

After the Velvet Revolution in 1989 and the split of Czechoslovakia in 1993, the Czech Republic transitioned from a communist, centrally planned economy to a modern market-based economy. Even though the Czech economy enjoyed a relatively fast average real GDP growth of 2.5% from 1997 to 2017, it still has not caught up with neighboring developed countries such as Austria or Germany that were similarly rich in the pre-communist era. In 2010, the National Economic Council of the Czech government identified corruption and

[5]This finding seems to be in contrast to the findings of Calvo et al. (2019); Giuffrida and Rovigatti (2018), who study the effects of oversight in US federal procurement. They find that active procurement officers' oversight is associated with negative outcomes due to excessive red tape.

weak institutions as the crucial factor hindering economic growth.[6] In terms of perceived institutional quality, the Czech Republic could be compared to high-income countries with high corruption levels such as South Korea or Italy.[7]

Public procurement is a market where these institutional issues are manifested. Public officers are in charge of running and designing public procurement contracts. Nevertheless, politicians can influence the allocation of procurement contracts. The European Commission (2016) reports that there have been multiple instances of politicians using *"undue influence over the specification of contracts, subjective and unclear selection criteria, and bid rigging"*. Politicians can also appoint public officials responsible for public procurement. The politicization of public officials was criticized by the Council of the EU and (inter)national anti-corruption organizations.[8]

The degree to which politicians and public officers can influence procurement contracts mostly differs across two dimensions: procurement procedures and evaluation criteria. There are two categories of allocation procedures: open and restricted procedures. In an open procedure, any bidder fulfilling the qualification criteria can bid. [9] In restricted procedures, procuring authorities can, to some extent, arbitrarily restrict the set of potential bidders. Lowest-price auctions and so-called most economically advantageous tender (MEAT) are the types of evaluation criteria used in Czechia. In the lowest-price auctions, there is only one criterion – the price. In MEAT tenders, other criteria are very often combined with price to choose the winning bid.

[6]For details, see the report `https://www.vlada.cz/assets/media-centrum/aktualne/vyrocni-zprava_NERV.pdf`.

[7]According to the Corruption Perception Index, which ranks countries by their perceived levels of corruption in the public sector, the Czech Republic ranked 38^{th}, South Korea 45^{th} and Italy 52^{nd} in 2018 (Transparency International, 2019)

[8]See, for instance, the recommendation of the Council of the European Union from June 19, 2013, stating that there is a "need to adequately separate political appointees from non-political staff, guarantee the independence of state officials and create a well-functioning career system to reduce high staff turnover" (p. 7). The recommendation is available at `http://register.consilium.europa.eu/doc/srv?l=EN&f=ST%2010626%202013%20REV%201`.

[9]The restricted procedures include several different mechanisms (general restricted procedures, negotiated procedures with call, negotiated procedure without call, competitive dialogue, and direct award).

3 Data

3.1 Public Procurement

The key part of our analysis is data regarding public procurement. A nationwide regulation governs the procedure of awarding public procurement contracts. The regulation describes the processes for allocation. Nevertheless, these regulatory procedures are actually implemented by the procurers, who thus have substantial leeway in choosing allocation procedures and qualification and evaluation criteria. One important requirement is that procurers have to publish details about contracts in an online system. The data from this system are the basis of our paper.[10]

Our data include contracts procured by the central, regional, and municipal governments and government-owned businesses in the period from 2006 to 2018. The overall quality of the raw reported data is weak in some cases, with incorrectly coded data and some missing values. A private company corrected and extensively cleaned the data, ensuring that the final dataset is of good quality. The company used automated software to correct systematic errors, crosschecked data with separate databases, and accessed public information to fill in missing data.

The final dataset contains 34,007 contracts that accrue to 491 billion CZK (22 billion USD) in total value. It contains project industry classifications (CPV codes), final prices, allocation mechanisms, numbers of competitors, and identities of contractors and procurers. Basic summary statistics on these variables are provided in Table 1. Similar information is available for datasets from various countries. We will use one more important variable available in this data, which is the engineering estimate of costs. This is a qualified estimate of the costs of delivering the project for an average firm on the market. The cost estimates

[10]Contracts above a specific threshold (circa 87,000 USD for public service contracts, 261,000 USD for public works) are legally required to be published, but many smaller contracts are published as well.

Table 1: Descriptive Statistics - Dataset of Public Procurement Contracts

	mean	sd	p25	p75
Price	14,800	123,000	1,593	11,000
Estimated Costs	18,300	160,000	2,099	13,700
Relative Price	.844	.263	.692	.998
Open	.526	.499	0	1
Scoring	.2640633	.4408396	0	1
No. of Bidders	4.500	3.985	2	6
N		34,007		

Notes: These are descriptive statistics of contract-level procurement data. The price and estimated costs are in thousands of CZK. Note that for the number of bidders, there are few missing observations and the total number of observations is 33,251.

are done by specialized employees of the procuring authorities.[11] [12]

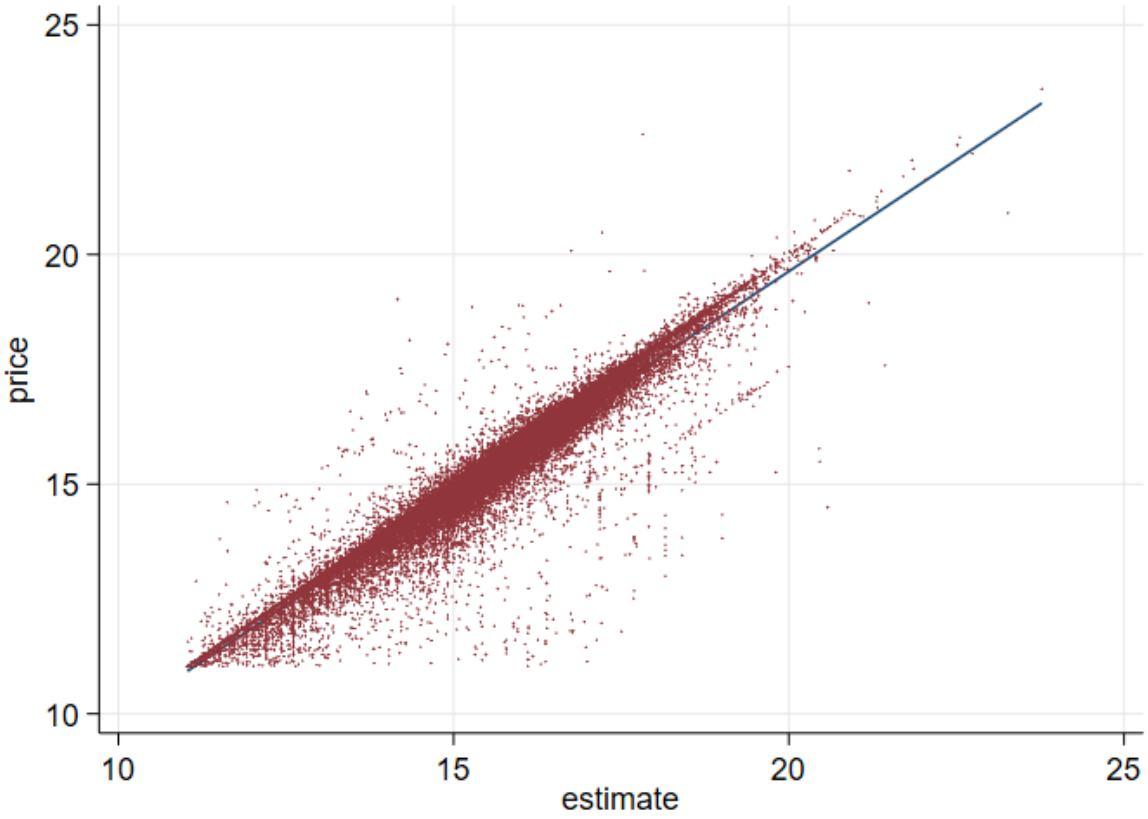
Engineering cost estimates seem to be a very good predictor of the final price. The regression of the logarithm of the price on the logarithm of the estimate explains 91% of the total variation in prices.[13] Figure 1 shows the correlation between estimates and prices.

[11]To account for potential systematic differences in the estimates, we include procuring authority fixed effects in our empirical specifications.

[12]They are done ex-ante before public procurement announcements are published and are among others used to decide whether a procurement contract follows below- or above-the-threshold procedure. Given that the below-the-threshold procedure is administratively less demanding (and also more prone to manipulation), procuring officers might have the motivation to manipulate the estimates to fit below the threshold (Palguta and Pertold, 2017). Later, we show that politically connected firms are not more likely to receive below-the-threshold contracts and that there is no bunching below the threshold specific to public procurement contracts supplied by politically connected firms (see Appendix for details). This potential systemic manipulation does not pose a threat to our identification.

[13]The separate regressions of the (log) of the price on the (log) of estimated cost) for procurement contracts delivered by connected and non-connected firms reveal that the estimated costs are similarly good predictors of the final prices in both cases.

Figure 1: Correlation of estimates and realized costs



Notes: The log of the engineering estimate of costs is on the horizontal axis, and the log of the final price is on the vertical axis. Each dot represents one public procurement contract. The solid red line is a regression line.

In terms of specific industries, the majority of projects are construction (51%) and IT services (10%). A more detailed overview is provided in Table 2. Note: In the table, we show the least detailed classification. In the analysis itself, we use 3 digits of the CPV (Common [public] Procurement Vocabulary) codes, which allows us to distinguish between the 983 types/industries of projects.

Table 2: Types of Projects

	Count	Percentage	Cumulative percentage
Transport	1,647	4.843	4.843
Energy	841	2.473	7.316
IT and telecommunication	3,584	10.539	17.855
Others	1,213	3.567	21.422
Office equipment	797	2.344	23.766
Forestry and agriculture	761	2.238	26.004
Medical equipment	579	1.703	31.735
Clothes, shoes and other similar equipment	1,623	4.773	36.507
Legal and other advisory	1,650	4.803	36.697
Natural resources	79	.232	36.740
Construction	16,866	49.596	86.335
Industrial machinery	886	2.605	88.941
Technical services	1,562	4.593	93.534
Healthcare, social care and educational services	2,199	6.466	100
N		34,007	

Notes: An overview of the most common types of contracts. These types are derived from 1-digit CPV codes and represent broadly defined industries in which particular contracts can be assigned.

3.2 Electoral Data and Parties in Power

For our analysis, we need to know what political party is in charge of each municipality, region, and ministry of the central government at each point in time. For the central government, we identify the political party in power at each ministry. For regions and the 50 biggest municipalities, we identify the coalition of governing parties, and for smaller municipalities,

we approximate the party in power by using the party that won the elections.

In total, 11 elections took place during the period studied (see Table A1). These include 3 municipal elections (2006, 2010, and 2014), 3 regional elections (2008, 2012 and 2016), and 4 parliamentary elections (2006, 2010, 2013, and 2017), and resulted in about 370 relevant changes to the party in power.[14] This provides the variation that will be used when studying the political connections of firms.

3.3 Political Connections

To obtain a measure of political connectedness for each firm, we create a novel dataset linking political candidates and companies. We link all members and supporters of a political party to identities of business owners and board members. For this purpose, we combine several mainly publicly available data sources.

Identities of politicians We need to create a list of politicians associated with each political party. The elected politicians are only a very small fraction of the people who could be associated with a party. All candidates from a given political party make up a much larger population. We collect data on all persons who ever appeared on an electoral ballot. We scrape data from a publicly available server `www.volby.cz` that collects information about all candidates for each political party across the entire history of the Czech Republic. We focus on recent history and download data from 2004 to 2017. In total, we work with approximately 720,000 records of candidates.[15] Arguably, political connections may also be established through family members or friends/business partners. Because we do not observe these connections, we are likely to underestimate the total impact of all types of connections.

Identities of executives and owners Every company in the country must be listed in the official business registry. The information available there consists of the names, ages,

[14]By relevant, we mean changes to the party in power for procurers that procure at least two contracts from the same supplier. For details, see our empirical specification.

[15]These are not necessarily unique people, as politicians often run for office repeatedly. Most of identified politicians are candidates in municipal elections (about 660,000).

and addresses of the owner(s) and members of the board.[16] In total, our dataset contains about 3,200,000 records of all Czech companies. A challenge arises here, as not only people but also companies can own companies, and complicated ownership structures are often used to disguise the real owner. We develop an algorithm that links politically connected companies to final owners even if there are numerous other firms in between. This is an important contribution. A naive approach, ignoring that companies are often owned by other companies, would identify Andrej Babis (the former Czech Prime Minister) as an owner of 1 firm, whereas this more sophisticated procedure finds about 250 firms.[17] With the small exceptions of foreign-owned firms and some stock companies that have a fractionalized ownership structure[18], we ultimately uncover the real owner for each company delivering procurement contracts.

Defining connections We match these two datasets – political candidates with the dataset of members of boards and supervisory boards of all Czech firms. We consider firms with a board member, a supervisory board member, or an owner affiliated with a political party as politically connected to that specific party. Specifically, a firm is labeled as connected to a particular party if either of the following conditions hold: *(i)* the firm is or was owned by a person who is or was on a slate for a particular political party or *(ii)* the firm has a member of the board or supervisory board who is or was on a candidacy slate of a particular party. In total, we identified 3,578 political connections between firm contractors and Czech institutions and their subsidiaries, which constitutes about 1.3% of all contractors.

Note that this approach differs from the usual definitions of political connections. A firm does not need to be connected to an elected politician; it suffices that the firm is connected to an unelected politician who belongs to a winning party. The underlying idea is that even if unelected, these politicians can still use their political intra-party networks to influence the allocation of public procurement. This definition is based on the structure of political parties

[16]Or equivalently supervisory boards of stock-owned companies.

[17]Andrej Babis is the 2^{nd}-3^{rd} richest person in the country. So the high number of owned firms indeed reflects the reality.

[18]Meaning that there is no shareholder with a share higher than 20%.

within the Czech Republic. First, the Czech Republic is a relatively small country with a population only slightly larger than New Jersey's. Second, it has a fragmented political system consisting of many political parties. The result of these two elements is that each political party is relatively small, and members of the party are likely to know each other regardless of their region of origin.[19] Moreover, all elected politicians are elected in and for a specific region. This complicates distinguishing between regional or national politicians (since even politicians within the national government are elected for a particular region).

Only 1.3% percent of all contractors are connected to a political party. Nevertheless, these firms receive 2% of all contracts, constituting 6% of the total volume of all procurement contracts. Several papers established how political connections help firms to win procurement contracts (Goldman et al., 2013; Schoenherr, 2019). Simple summary statistics of our data suggest a similar pattern: the average value of procurement contracts per procurer per year is 509,000 USD for connected firms and 96,000 USD for non-connected firms. In other words, firms connected to a given procurer tend to receive a disproportionate amount of contracts per year. Over the span of our data, these amount to a total of 35 billion CZK (1.6 billion USD) awarded to firms with political connections.

[19]For instance, the political party 'ANO,' which currently has the largest voter support, still has fewer than 3,000 members.

4 Empirical Analysis

4.1 Baseline Model

We use the following basic specification to explore the effect of political connections on the price of the contract, which we label as TWFE specification:

$$rel\_price_i = \beta * connection_{p,f,t} + \delta_{pf} + \sigma_t + X_{p,f,t}\omega + \epsilon_i$$

where $rel\_price_i = \frac{price_i}{estimate_i}$ with $price_i$ being the final price the contract i was contracted for and $estimate_i$ the engineering estimate of the price. $connection_{p,f,t}$ is a dummy variable that is equal to 1 if the contract is awarded to a firm f that has a political connection to the party in power at procurement agency p in year t procuring the contract i. δ_{pf} represents procurer-firm fixed effects and σ_t represents time (year) fixed effects. $X_{p,f,t}$ is a set of additional controls. In particular, this vector includes industry fixed effects (defined as the 3-digit level of CPV codes, containing 983 different industries). Further, we include month fixed effects to control for seasonality. And we control also for major changes in the government by including a dummy equal to 1 if there is a change in the governing party in the last elections and 0 otherwise. The same specification is used for outcomes other than the price, such as the intensity of competition and the quality of contracts.

The procurer-firm fixed effects δ_{pf} are included to isolate any time-invariant relationship between a certain pair of a firm and a procurer. Including only procurer FEs would help to control for heterogeneity of procurers stemming from, for instance, geographical locations of procurers or some specific requirements, which lead to systematically higher or lower prices (such as a special security requirement of a ministry of defense or secret service). Separately controlling for firm FEs would then capture any firm (time-invariant) characteristics such as high productivity. Combining these FEs into relationship firm-procurer FEs means that we control for patterns of matching of specific firms to specific procurers.[20]

[20]In the Appendix, we provide the full table that shows how the coefficients change with including

The coefficient of interest is β that measures the effect of a change in the connection status. The connection status $connection_{p,f,t}$ can change over time, specifically, when a party in power changes.[21] For example, imagine firm f that has a personal connection to a political party. If this political party is in power at procurer p and then it loses elections, the connection status of the firm f with regard to procurer p changes from 1 to 0. This means that our estimate β measures the difference in the relative price for a contract procured by the same procuring authority and supplied by the same firm in the situation when the connection status is equal to 1 versus when it is equal to 0. A potential issue with this two-way fixed effects approach is that the estimate comes from comparisons based on multiple treatment status changes at different points of time – staggered treatment adoption.

4.1.1 Staggered Treatment Adoption

Recent literature has studied the applicability of staggered treatment adoption designs (Baker et al., 2022; Borusyak K. and Spiess, 2022; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; de Chaisemartin and D'Haultfoeuille, 2022; Gardner, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). This literature shows that in the presence of heterogeneous treatment across group-time cells, such models can lead to an inadmissible weighting of individual measured average treatment effects, in which some observations receive a negative weight. In that case, the measured point estimate could be negative while all average treatment effects were positive.

We follow the most up-to-date literature on difference-in-differences design with staggered treatment adoption (Baker et al., 2022; Borusyak K. and Spiess, 2022; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; de Chaisemartin and D'Haultfoeuille, 2022; Gardner, 2021; Sun and Abraham, 2021) to verify the validity of

gradually more fixed effects.

[21] This variable can be seen as traditional $post\_treatment \cdot treatment\_group$, where $treatment\_group$ is being connected to a particular political party and $post\_treatment$ is a dummy equal to 1 when this political party is in power at a particular procurer that procured the given contract. However, in our case, the treatment status can be also reversed (when the political party loses power).

our findings. Using the methodology of de Chaisemartin and D'Haultfœuille (2020), we find that only 4 of the 62 average treatment effects received a negative weight. The sum of the positive weights is equal to 1.0075783, while the sum of the negative weights is equal to -.00757823. These results mean that we have few negative weights in our estimation (6.45%) and they are very small (about 0.75% of the sum of all weights, which is always 1). Our estimation is thus unlikely to be invalidated by the presence of negative weights.

To rule out any concerns, we use two alternative estimators introduced by Gardner (2021) and de Chaisemartin and D'Haultfœuille (2020); de Chaisemartin and D'Haultfoeuille (2022). These are most suitable in our case for two reasons: *(i)* our data are not balanced panel, and *(ii)* our treatment can be switched on and off. Most other estimators, such as those introduced by Borusyak K. and Spiess (2022); Callaway and Sant'Anna (2021); Goodman-Bacon (2021), do not accommodate this.[22]

The estimator introduced by Gardner (2021) can be seen as a two-stage difference-in-differences. First, outcomes are regressed on group and time fixed effects and any covariates on the sample of untreated/not-yet-treated observations. Second, the residuals from the first regression are regressed on the treatment variable. This procedure removes the issues arising from the staggered treatment adoption.

The estimator developed by de Chaisemartin and D'Haultfœuille (2020); de Chaisemartin and D'Haultfoeuille (2022) (henceforth DID_M) works on the principle of choosing a sub-sample of "valid" comparison to estimate the treatment. The estimated effect is a weighted average of the effect on *joiners* and the effect on *leavers*. The effect on joiners then compares the mean outcome between two periods for the units that are not treated in $t-1$ and become treated in t versus the units that remain untreated in both periods. The effect on leavers compares the units that lose treatment status from period $t-1$ to period t and those that remain treated in both periods. The DID_M estimator relies on parallel trends between switchers and groups that have stable treatment. de Chaisemartin and D'Haultfoeuille (2022)

[22]For an overview, see Baker et al. (2022).

developed a placebo test to check this assumption. Our implementation of it is shown in Figure A1. Although the standard errors are large, there appears to be no pre-trend, and the effects follow the expected pattern. There is no effect or a negative effect of being connected before the connection becomes active ($t = 0$), after which the effect becomes positive and (marginally) significant. A clear disadvantage of this estimator is that it can substantially reduce the sample. The associated loss of statistical power leads to larger standard errors.

4.2 Effects of Favoritism on Prices of Contracts

Our main focus in this section, is on the prices of contracts allocated to politically connected firms. Studies by, for instance, Goldman et al. (2013) and Schoenherr (2019) show that connected firms are awarded contracts of higher volume compared to other firms. Knowing what the final price should be according to administrative price estimates will help us shed light on whether the disproportionate allocation of contracts to politically connected firms can be efficient or whether it should rather be labeled as corrupt behavior.

In Table 3, we document that contracts allocated to politically connected firms are priced higher. This result is consistent across different estimators. The standard two-way fixed effects specification shows an increase of 6% of the baseline estimated costs. The two other specifications (de Chaisemartin and D'Haultfœuille, 2020; Gardner, 2021) show slightly larger but statistically indistinguishable point estimates.[23][24] The TWFE model and the other two estimators include a procurer-firm fixed effect that directly utilizes the variation of connections between firms and procurers due to election results. This allows us to control for any firm- and procurer-specific heterogeneity as well as any match-specific heterogeneity, such as a firm's comparative advantage due to accumulated relational capital. To accommodate potential correlation between contracts procured by the same procuring authority, we cluster standard errors at the procurer level (Cameron and Miller, 2015).

[23] In Table OA1 in the Appendix, we present simpler alternative specifications without all the fixed effects.

[24] An additional finding shows that the probability of the final price being above the estimated cost is significantly higher for connected firms. See Table A2 in the Appendix for both parametric and non-parametric tests.

Our approach with procurer/firm pair fixed effects allows us to rule out the possibility that the results are driven by either *(i)* inefficient procurers to which there are many politically connected firms, or *(ii)* politically connected firms that focus predominantly on overpriced contracts.

Table 3: Effects of political connections on contract price

	(1)	(2)	(3)
	Rel. Price	Rel. Price	Rel. Price
TWFE	0.0602**		
	(0.0253)		
Gardner		0.0814**	
		(0.0318)	
DID_M			.0634*
			(.0372)
Industry FE	Yes	Yes	Yes
Firm/procurer FE	Yes	Yes	Yes
N	34,007	33,849	2,170

Robust standard errors clustered at procurer level in parentheses

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

Notes: The outcome variable is the final price over the estimated cost. We also control for industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections throughout the three estimators.

4.2.1 Empirical Assumptions

We use changes in the political connections of firms to estimate the effect of political connections on prices. These changes are induced by changes in the party in power around elections. Our estimation depends on the assumption that firms that experience positive

shocks in the treatment are otherwise similar to the firms that do not. The fact that the parallel trends assumption appears to be satisfied reassures us that our results are not driven by pre-trends.

To verify that our findings do not arise from some systematic difference between firms that experience a positive treatment shock and those that do not, we run a balance test comparing firms that experience a positive shock (that is getting connected to the party in power) with those that do not experience this positive shock. The findings are shown in Table 4. We find that politically connected companies that experience the switch tend to be slightly older. Yet, this is captured by our firm fixed effects. Otherwise, we do not find any difference.

Table 4: Firms' characteristics – firms with affiliation to a political party that experience positive shocks vs. firms with such affiliation that do not

	Mean No Positive Shock	Mean With Positive Shock	Diff.	P-value.
Age	19.273	20.2848	-1.010	.062*
Assets	6,060	3,270	2,790	.453
Equity	1,150	1,090	59.5	.935
EBIT	172	153	19	.903
Registered Capital	281	226	55	.605
No. of Employees	297.5839	258.303	39.281	.648
Return on Assets	.057	.064	-.007	.703
Observations	1,719			

Notes: Age is stated in years, the number of employees in full-time equivalent, and the rest of the variables in millions of CZK. *** p<0.01, ** p<0.05, * p<0.1

Another issue with our approach could be endogeneity of political connections. For instance, imagine that more competitive/ambitious firms intentionally seek political connec-

tions and also contracts with higher profit margins. This appears unlikely in our application for several reasons. First, we pull together procurement contracts from three different levels of government. Elections on these levels take place virtually every year in the studied period, and different political parties are in power on different levels. This means that firms would have to quickly switch or acquire new connections to be able to obtain advantages from different procurers. This does not correspond to the reality that most firms are connected to one party only. Second, it is virtually impossible to predict which party will be in power in a particular ministry, as the party in power in each ministry is decided only after parliamentary elections during complicated coalition negotiations in a multiparty proportional system. Third, the variation in political connections that we exploit is largely coming from losing political connections rather than gaining them. In this case, competitive/ambitious firms intentionally seeking political connections would not bias our results. Last, to fully rule out the possibility that our results are biased, we construct a measure of close elections and examine empirically whether the estimates of the effects of political connections differ between close elections and those with more unequal results. In a close election, it is almost impossible to seek political connections in advance, as it is not possible to predict which party will be in power. We do not find any evidence that the estimates differ (see Section G.3 in the Appendix). This suggests that the endogeneity concern is not a threat to our identification.

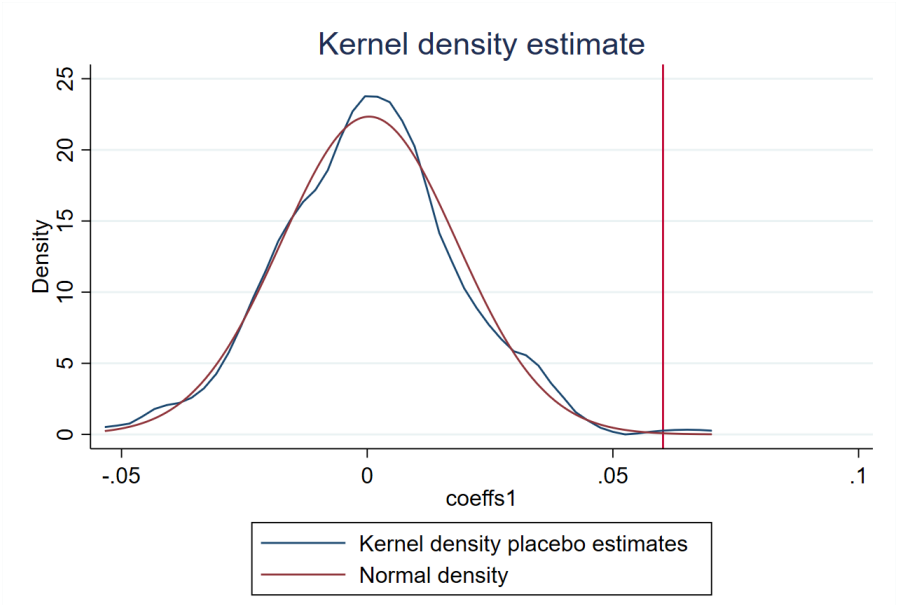
The accuracy of our estimates could be influenced by the precision of our measure of political connections. We rely on matching firms' board members and owners with politicians based on names, ages, and city of residence. From manual inspection of 100 random matches, we find that there are close to zero mismatches – the algorithm does not yield firms labeled connected that are actually not connected. However, it is still possible that we do not find all connected firms. In this case, we would likely underestimate the actual effect of political connections. Therefore, our estimates of total costs of political connections can be seen as a lower bound. Finally, our findings could be biased if final prices were substantially

renegotiated. It could lead to either upward or downward bias. First, renegotiation is legally restricted in the Czech Republic (at most, 10% to 30% of the baseline price, with the cap varying by contract types and dates). Second, we test whether connections lead to more renegotiation, and we do not find any evidence for this hypothesis. The findings are presented in Section E of the Appendix.

4.3 Placebo Randomizations of Connections and Electoral Shocks

To ensure that our results are not driven by some quirk in our specifications, we randomize the allocation of connections and electoral shocks while keeping the share of connections the same as in our real dataset and run our preferred specification. Then we plot the distribution of obtained coefficients. The plot is in Figure 2. The actual estimate is represented by the vertical line. The first and the 99^{th} percentile contain values lower than 0.043 and higher than 0.040, respectively. This shows that our estimate on the real data would be very unlikely to occur if there were no real treatment effect.

Figure 2: Placebo randomizations of connections and electoral shocks



Notes: The plot shows the distribution of point estimates of the treatment effects from 200 bootstrap samples with randomly allocated treatment. The distribution is obtained using the Epanechnikov kernel.

4.3.1 Homogeneous Goods

In the analysis above, we have considered the sample of all (heterogeneous) procurement contracts. Next, we focus on a more homogeneous market[25] – public procurement of commercially available goods. This is an interesting setting because we are able to study how much goods are overpriced in a market where quality is largely the same for all suppliers. Moreover, for the market of homogeneous goods, our administrative estimates should be almost equal to the market prices. A simple predictive regression shows that 97% of price variation can be explained by the cost estimates. We can thus not only control for quality differences, but also for potential bias in cost estimates in this analysis. Figure OA1 shows that the price estimates are particularly precise for this sub-market of goods.

[25]It would be ideal to study a completely homogeneous market with the exact same product. Given that our data only include expensive goods above circa 87,000 USD (2,000,000 CZK), we have a limited number of observations for each particular homogeneous good.

We include all goods that should be equivalent to the products on the commercial market (namely those where there is no or very little need for specific adjustments of the goods for a given procurer. Such contracts include for example cars, agricultural machines, and raw materials.[26] We repeat the analysis in Table 3, but with the sample limited to homogeneous goods.

The main motivation for studying the homogeneous market is that tailoring competition for a specific supplier might be a rational, albeit illegal, behavior of a contracting authority. The lowest price auctions are often criticized for not necessarily delivering the best-quality project. For a very complicated project, a connection could serve as a guarantee of quality. We would still see similar quantitative effects as in the previous section, but the behavior as such would actually be welfare improving. In case of homogeneous goods, this argument does not seem valid, as the goods are comparable to the products sold directly on the commercial market. Thus, this may serve as an additional check that the differences in price are socially undesirable.

[26]We define homogeneous goods as those belonging to one of the following categories: clothing, fuels, materials, food, electricity, chemical products, office equipment, communication devices, medical equipment, transport equipment, musical instruments, furniture, water, industrial machinery, agricultural products, and machinery.

Table 5: Effects of political connections on contract price – homogeneous goods

	(1)	(2)
	Rel. Price	Rel. Price
TWFE	0.274**	
	(0.0857)	
Gardner		.1789***
		(0.0340)
Industry FE	Yes	Yes
Firm/procurer FE	Yes	Yes
N	7,223	7,223

Robust standard errors clustered at procurer level in parentheses

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

Notes: The outcome variable is the share of the final price divided by the estimate. We also control for the industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections throughout all specifications.

Table 5 presents our findings. We observe significant and larger effects of political connections on prices.[27] The estimates suggest that if a connected firm wins the contract, the price tends to be 27% higher.[28] The findings suggest that it is easy for procuring authorities to tailor these contracts, which leads to even higher prices charged by the favored firms, and/or that it might be costlier for suppliers to adjust homogeneous goods rather than a complicated construction project.

[27]Note that the DID_M is impossible to estimate, due to insufficient degrees of freedom. We provide alternative specifications in Table OA4.

[28]The sample of homogeneous goods is much smaller than the original dataset. This leads to the estimate being less robust toward different specifications. However, the effect is economically and statistically significant in all but one specification.

4.4 Favoritism and Quality of Projects

Next, we present evidence that a connection between a procurer and a supplier does not lead to higher project quality. To tackle this issue, we use a novel measure of quality proposed in Baranek (2020). We focus on the construction projects that constitute about 51% of the market. The effect of political connections on prices in this sub-market is virtually the same (please see Figure A2 in Appendix for comparison). In such a market, a natural way to assess the quality of a project is the cost of repairs that follow the project. Any required repairs need to be allocated via new procurement contracts, which will appear in our data. Unfortunately, repairs are legally considered new independent contracts, and it is impossible to link them to original projects via a unique identifier. However, we have a rich text description for each project at our disposal.

A human reader is able to distinguish whether a project is likely to be a repair of an original project. Given the size of our data, it is impossible to manually link projects to each other. Looking only at the biggest construction procurer in the country – the Road and Motorway Directorate – this would mean manually analyzing almost 10 million project combinations. We thus decide to automate the process.

We decrease the dimension of the problem by imposing several restrictions. Both the original project and the follow-up have to be procured by the same buyer, and the tender for the original project has to precede the follow-up. No procurer can hold tenders for projects that are not in their competency and the latter assumption is trivial. Further, we restrict the set of possible follow-ups only to projects that contain a word suggesting the project is actually a follow-up. These include keywords such as *repair, maintenance, revision, follow-up, reconstruction,...* . Given the complexity of the Czech language, the final library of keywords consists of around 50 words.

We turn to text analysis tools to calculate the probability that each project tagged by a keyword is a follow-up for a particular previous project. We proxy this probability by the similarity of the text descriptions of the original project and the possible follow-up.

To calculate the similarity of the project, we employ the tri-gram text-matching algorithm. Intuitively, such an algorithm translates a high-dimensional object, such as a word or a sentence, into a vector that contains all triplets of subsequent characters in a given text. Then the similarity of the text is calculated as the share of overlapping triplets.

We adjust the probability that a project is a follow-up by also considering how similar the project is to all other projects. Specifically, the probability $p_{i,j}$ that j is a follow-up of i is calculated as a function of the similarity of i and j, denoted as $s_{i,j}$.

$$p_{i,j} = \frac{s_{i,j}}{\sum_{i' \in I} s_{i',j}}$$

where I is the set of all projects. To verify that our matching algorithm works well, we manually check all public procurement contracts of a randomly chosen middle-size procuring authority – the city of Most. According to this manual verification, the accuracy of our algorithm is about 93%. For more information about the this algorithm and verification details, see Baranek (2020)[29]. Finally, we calculate the expected sum of repairs of a project i by:

$$R_i = \sum_j p_{i,j} P_j$$

P_j denotes the price of the repair j. For the subsequent empirical analysis, we normalize this amount by the engineering estimate of costs of the original project. The variable *Followups* should thus be interpreted as the increase in repairs with respect to the engineering estimate of costs of the original project. The following table summarizes the repairs of each construction project.

[29]Baranek (2020) uses a more complex version of this algorithm.

Table 6: Descriptive Statistics - Repairs of Construction Contracts

	mean	sd	p25	p75
Followups	.542	3.876	0	.270
N		13,667		

Notes: These are the descriptive statistics of the construction repairs. The variable described is equal to the total number of repairs divided by the size of the original project.

The findings in Table 7 indicate that favoritism toward connected firms does not significantly influence quality. There are no significant quality gains from political connections, but rather, insignificant losses. We provide detailed results with various specifications in Table OA3.

Table 7: Effects of political connections on quality

	(1)	(2)	(3)
	Followups	Followups	Followups
TWFE	0.0847		
	(0.2379)		
Gardner (2021)		0.5656	
		(0.5171)	
DID_M			1.787654
			(2.085698)
Industry FE	Yes	Yes	Yes
Firm/procurer FE	Yes	Yes	Yes
N	16,866	16,665	1,576

Robust standard errors clustered at procurer level in parentheses

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

Notes: The outcome variable is the value of repairs over the engineering estimate. We also control for logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections throughout all specifications.

It is important to note that all regressions control for exact project type and size, so we do not compare projects of varied complexity. It might be that a form of favoritism is allocating follow-up contracts to a project to continue helping the favored firm. This is, however, not happening in our data as the vast majority (86%) of contracts that are likely a follow-up of the original projects are delivered by a new company (meaning that the original vendor and the vendor of the follow-up are different companies).

4.5 Mechanisms of Favoritism

Politicians have several available tools to steer contract allocation toward their preferred firms. 53% of contracts are allocated via an open auction and the rest via more discretionary mechanisms in which the procurer can, to some extent, arbitrarily restrict the set of potential competitors. For each mechanism, the procurer can also choose to use either the lowest price allocation or the *"most economically advantageous allocation"*, which is equivalent to a scoring auction. Here, the winning vendor is the vendor with the lowest weighted average of price and a quality score. Finally, the procurer also specifies exact technical requirements for each project. There is vast anecdotal evidence that politicians tailor projects so that just the preferred firm is able to deliver this project for a competitive price. We investigate all these possible channels in Table 8.[30]

[30]In the Appendix, we present a detailed overview of various specifications for testing robustness of these findings as well as (whenever possible) the results of using the alternative estimators by Gardner (2021) and de Chaisemartin and D'Haultfœuille (2020).

Table 8: Mechanisms

	(1)	(2)	(3)	(4)	(5)
	Connection	Connection	Connection	Connection	Nr. of Bidders
Open	-0.0000386				
	(0.0019)				
Scoring		-0.00364*			
		(0.0022)			
Length			0.0000699***		
			(0.0000)		
Complexity				0.0000115**	
				(0.0000)	
Connection					-0.224
					(0.1747)
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm/procurer FE	Yes	Yes	Yes	Yes	Yes
N	23256	23256	23256	23256	21355

Standard errors in parentheses

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

Notes: In Columns (1)-(4), the outcome variable is a dummy variable equal to 1 if the contract was delivered by a connected firm. In Column (5), it is the number of bidders. We control for the logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections throughout all specifications.

In the following analyses, we adjust the empirical specifications (except for the analysis of the number of bidders). The dummy variable for political connections becomes the depen-

dent variable, and the channels such as the usage of an open auction or a scoring auction, the length of the description, and the complexity of the description are independent variables. This is because all these variables are determined ex-ante. They are decided before the actual auction takes place, and thus, the specification with political connections as an independent variable would suffer from reverse causality. For the sake of transparency, we include regressions with our standard specification in Section A.5.2 of the Appendix.

First, we analyze the probability that an open contract is allocated to a politically connected firm (see Column (1) of Table 8). Perhaps surprisingly, we do not find any evidence that open contracts are more likely to be allocated to politically connected firms. Talking to experts in the industry revealed that procurers simply do not do this because it is an obvious sign of corruption and they try to engage in more sophisticated methods.

Second, we find evidence of a lower probability of scoring auctions being allocated to politically connected firms(see Column (2) of Table 8). The procurer could choose the less objective non-price criterion to bias the final score in favor of the politically connected companies. However, a close investigation of the Czech procurement legislation explains why this is not done in practice. Scoring auctions are heavily monitored because any firm losing a scoring auction has the right to object to the non-price score in court. While the complaint is being investigated, the project is interrupted. This regulation is often criticized and leads to the marginalization of non-price criteria. Therefore, often even if a scoring auction is used, it is de facto a lowest-price auction, with the non-price criterion contributing only several percent of the weight. This decreases the appeal of scoring auctions, both for the purpose of acquiring a high-quality project but also for an opportunity for corruption.

A potential alternative explanation of the mechanism behind the favoritism could be tailoring of procurement contracts to connected firms. Even though tailoring of project characteristics seems to be a well-known practice in this market, only a few cases of tampering were brought to the courts. This is likely because it is inherently difficult to measure contract tampering in the data. Just a single word in several pages of description of the project can

cause the contract to be fitted to a specific firm. We focus on two potential measures that could shed light on whether procurers use such mechanisms.

First, we look at project descriptions directly and provide a simple, albeit imperfect, measure of contract tampering. According to our industry sources, project tampering is most often done by adding an unnecessary technical requirement. This requirement must be mentioned in project descriptions and leads to longer text descriptions – which we have access to – compared to equivalent projects within the same product category. We calculate the length of this description and analyze whether a longer description increases the likelihood of procurement contracts being awarded to politically connected firms. The results of this analysis are presented in Column (3) of Table 8. The average length of this short description is 74 characters, with the maximum being 385 and the standard deviation 40. Our findings suggest that one standard deviation increase in the length of description is associated with a 0.2796% increase in the probability of the supplier being connected. The unconditional probability of being connected is 2%. It may also be interesting to look at the regressions with the dependent variable being the length of the description. We present these regressions in Table OA13 in the Appendix. The results suggest a similar story – winning a procurement contract as a politically connected firm is associated with longer description.

An alternative explanation for this result (in contrast to project tailoring) could be that politically connected firms win larger projects that tend to have longer description. That is why we introduce the new variable *complexity*, which is normalized by the size of the project [31]. It is calculated as a fraction $\frac{length\ of\ description}{value\ of\ project\ in\ mil.}$. The results are in Column (4) of Table 8. Even after accounting for the value of the projects, the results remain the same, and the length of the description of procurement contracts is associated with a larger probability of a contract being allocated to a politically connected firm.[32]

[31] A similar proxy for product specificity was used by Baltrunaite (2020).

[32] A possible concern with the results regarding *complexity* could be that politically connected firms, which are probably larger firms, are better at dealing with complex contracts – that also likely have a longer description. However, at least in the specification in Column (4), we control for the firm-year fixed effects. This means that our result cannot be driven by larger complexity of contracts that are delivered by politically connected firms.

One of the signs of successful tailoring would be limited competition, because some non-favored firms might be directly prohibited from participation due to restrictive qualification criteria, or they might decide not to participate if they perceive that they couldn't reasonably compete with the favored firm. Thus, we investigate overall competition for contracts delivered by politically favored firms. In Column (5) of Table 8, we see that there is a decline in the number of competing bidders of approximately .3 to .5 bidders. The effect is marginally insignificant, while other, less demanding specifications show a significant effect (see Table **??** in the Appendix). This is relatively sizable, as the average number of bidders is 4.5 when the contract is allocated to a politically connected firm.[33]

An implication of the story of successful project tailoring is that the length of the descriptions (and also the measure of complexity) should be negatively correlated with the number of bidders. This is confirmed by our data. We show that a longer description as well as larger complexity are associated with a lower number of bidders (see Table OA10 in the Appendix).

4.6 Favoritism and Stricter Oversight

Many contracts in the Czech Republic are subsidized from the structural funds of the European Union (40% of public procurement contracts in our sample). For procurers, this has two main consequences. They bear only a fraction of the cost of the project, and these contracts are overseen by the local agency for redistribution of European funds, as well as the EU itself in case of an audit. Throughout the studied period, the EU funds were allocated at either a ministerial (90%) or a regional level (10%). We later exploit this to examine whether there is a difference in mitigating effects for the oversight done by a higher level of government over a lower level of government (henceforth, inter-government level oversight) and the oversight over a ministry done by an agency set up by the same ministry (hence-

[33]Restricting competition partially but not fully explains the price increase from the previous section (see Table A7 in the Appendix, where we show that the effects on price persist but are attenuated after controlling for the number of bidders).

forth, intra-government level). Furthermore, in the event of misconduct during procurement contract allocation and execution, the Czech Republic might have to return the subsidy to the EU. This creates incentives for the procurer to behave in a transparent way, as well as for the national overseeing agency to inspect subsidized contracts more thoroughly. We thus hypothesize that the effect of political connections on prices would be lower for procurement contracts that are co-funded by the EU.

In Table 9, we present results of a model similar to the one employed in Table 3, to which we add an interaction of *connection* with a dummy *subsidized* (equal to 1 when the contract is co-funded by the EU and 0 otherwise). We see that this interaction highlights a mitigating effect on overpricing. The sum of the coefficients for political connection and higher oversight is jointly statistically indistinguishable from zero.The t-statistic is 0.87, and the corresponding p-value is 0.385. Note that which contracts receive a subsidy from the EU is not random, so we cannot make a causal claim that the oversight leads to full mitigation of the effects of political connections. However, our results suggest that the oversight matters and is a policy instrument that needs to be inspected both in practice and in future research as a tool to fight corruption in public procurement. Note that this finding is in contrast with the findings of Calvo et al. (2019); Giuffrida and Rovigatti (2018), who study the effects of oversight in US federal procurement. They find that active procurement officers' oversight is associated with excessive red tape and, thus, overall negative outcomes.

Table 9: Interaction of connections and higher oversight

	(1)	(2)	(3)	(4)	(5)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Connection	0.0497***	0.0440***	0.0391**	0.0715***	0.0483*
	(0.0141)	(0.0131)	(0.0171)	(0.0236)	(0.0278)
Cofunded	-0.0155*	-0.0100	0.00469	0.00948	0.00555
	(0.0087)	(0.0073)	(0.0068)	(0.0099)	(0.0096)
Connection × Cofunded	-0.109***	-0.0718***	-0.0669***	-0.0908**	-0.0376
	(0.0173)	(0.0162)	(0.0173)	(0.0390)	(0.0287)
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm/procurer FE	No	No	No	No	Yes
N	31,393	31,393	31,393	31,393	31,393

Standard errors in parentheses

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

Notes: The outcome variable is the final price over the estimated cost. Additionally, we include an interaction of political connection and subsidy from the EU. We also control for the logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections throughout all specifications.

In the analysis in Table 9, we do not distinguish whether the mitigating effect comes from oversight by the Commission and/or the national and regional agencies. While we cannot examine this question directly, we can test whether oversight matters only if it is done by a superior level of government (for instance, a ministry) supervising a lower level (a region or a municipality). If the mitigating effect observed in Table 9 is concentrated and can be fully explained by either inter- or intra-government level oversight, it is unlikely that the oversight by the Commission matters for the effect of political connections.

We examine this proposition in Table 10. In Column (1), we focus on inter-level oversight in which a ministry (or more precisely its agency) oversees a procurement project procured by a city or a region, and in Column (2), we focus on intra-government level oversight in which a ministerial agency oversight projects procured by a (potentially different) ministry.[34] The sum of the coefficients for political connection and intra-level oversight is statistically significant with the point estimate equal to 0.1, t-statistics 2.08 and corresponding p-value 0.038. Our findings thus provide suggestive evidence that the oversight by the Commission does not play a major role. However, an inter-government level oversight withing the country matters. The results also imply that intra-government oversight does not seem to mitigate the effects of political connections. Note that unfortunately we do not have contract-level information about whether and how other (not co-funded from the EU) contracts have been overseen so we cannot test whether the same effect is present for these contracts.

The intuition behind this result may be that it is arguably easier for connected firms as well as for involved officers and politicians to make agreements about favoring particular companies within one government or one level of government compared to agreements among different levels of governments. This would then, in turn, suggest that oversight is more effective when it is done across government levels, because there are no or less agreements about favoring particular (connected) firms.

[34]Unfortunately, we cannot distinguish fully whether a project was supported from a regional or national operational programme. However, given that only 10% of contracts were supervised by regional agencies, we consider the oversight to be done on the ministerial level for all contracts in our analysis. This is, of course, a simplification that introduces measurement error. We argue that the bias introduced by the measurement error would drive our estimates down in the case of inter-government level oversight and up in the case of the same government level oversight.

Table 10: Interaction of connections and higher oversight

	(1)	(2)	(3)	(4)
	Rel. Price	Rel. Price	Followups	Followups
Connection	0.0713***	0.0616**	0.362	0.232
	(0.0250)	(0.0252)	(0.3494)	(0.3147)
Inter-level Oversight	-0.00163		-0.102	
	(0.0118)		(0.0814)	
Connection × Inter-level Oversight	-0.113**		-0.651	
	(0.0487)		(0.4053)	
Intra-level Oversight		-0.0240*		-0.0187
		(0.0125)		(0.0781)
Connection × Intra-level Oversight		0.000375		0.0172
		(0.0194)		(0.1177)
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm/procurer FE	Yes	Yes	Yes	Yes
N	23,244	23,244	23,244	23,244

Standard errors in parentheses

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

Notes: The outcome variable is the final price over the estimated cost in Columns (1) and (2) and the value of repairs over the engineering estimate of costs in Columns (3) and (4). Additionally, we include an interaction of political connection and a dummy for either inter-government level oversight (Columns (1) and (2)) or a dummy for intra-government level oversight (Columns (3) and (4)). The dummy for inter-government level oversight is equal to 1 if a project was subsidized by the EU and procured by either a region or a municipality, otherwise equal to 0. The dummy for intra-government level oversight is equal to 1 if a project was subsidized by the EU and procured by either a ministry, otherwise it is equal to 0. We also control for logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections throughout all specifications.

A second important outcome for which oversight could matter is quality. In Table 10, we also present the same regressions with the outcome variable changed to the inverse measure of quality (*Followups*). The findings suggest that oversight is not related to the quality of the delivered procurement projects.

5 Cost Analysis

Our final goal is to evaluate the cost of inefficiencies caused by the existence of favoritism. Specifically, we are interested in the savings coming from eliminating the connection between procurers and firms. We make several simplifying assumptions. If we see a connected firm winning, then there is exactly one connected firm competing and if we see a non-connected firm winning, then there are no connected firms competing. We, thus, omit scenarios where the technical specifications are tailored to one firm but another nevertheless wins the auction. It is likely that such a firm will need to bid more compared to the case where bidding conditions are not tailored. This will lead to an underestimation of the total cost as such cases inflate the prices of the control group.

Under these assumptions, we can use the fact that we already see the counterfactual scenario in our data. We observe the prices of procurement contracts that are not allocated to connected firms. Furthermore, assuming a homogeneous effect of connections (which seems reasonable based on our analysis of heterogeneity), we calculate the total cost as:

$$\Delta_{cost} = \Delta_{price} \cdot (value\ of\ connected\ contracts)$$

The point estimate corresponding to Δ_{price} can be obtained from Table 3. We take the most conservative estimate, namely 6%. The *value of connected contracts* is 1.6 billion USD as calculated at the end of Section 3.2. The calculation then leads to the total cost of favoritism equal to $0.06 \cdot 1.6\ billion$, that is 96 million USD in the studied period of 13 years. Given that the value of connected contracts is 6% of the total value of the market,

an overall increase in procurement expenditure is 0.36%. The whole public procurement market is worth about 14% of GDP (The European Commission, 2016) and the GDP of Czechia is about 282 billion USD (The World Bank, 2022). Thus, assuming the effect is homogeneous across the whole market for public procurement, this translates to *yearly* savings of 142 million USD.[35] To put this number in context: it is roughly half of the budget of the third most populous city in the country or the government's spending on social benefits for handicapped people.

Even though these are significant losses, they are an order of magnitude less than what is found in the literature. Schoenherr (2019) calculates a back-of-the-envelope welfare cost of political connections equal to 1% of GDP, and Brugués et al. (2020) suggest that 2 percent to 3.5 percent of all government expenditures overpricing is caused by political connections. Abstracting from clear institutional differences among countries, we need to stress that while the back-of-the-envelope calculation by Schoenherr (2019) does not distinguish between the volume of misallocation and the actual costs of misallocation, Brugués et al. (2020) does not take into account the quality of delivered projects.

Consider the following example to illustrate the importance of distinguishing between the volume of misallocation and the actual cost of misallocation. A contract worth 1 USD is awarded to a favored firm that would not have won it without a connection. Thus we define the total misallocation as equal to 1 USD. However, the social costs are only equal to the difference in costs between the actual scenario and the counterfactual, where the contract is delivered by the firm with the lowest costs (assuming no changes in the quality of the delivered work) – if there is a firm that could have delivered the contract at the same quality for 0.80 USD, then the cost is 0.20 USD. Even though favored firms receive about 6% of all procurement contracts, the actual costs of this kind of favoritism are much lower, as only the overpricing of the contracts and the quality of the project necessarily matter for the government. The cost is thus only a fraction of the volume of misallocation.

[35]The calculation is as follows: $0.14 \cdot 282000 \cdot 0.036 \approx 142\, million\, USD$.

Our approach isolates the costs from misallocation. Similarly, if the quality of procurement projects delivered by politically connected were significantly better, the increase in prices would be justified. In that case, favoritism could even improve welfare. However, we show that the quality is not better, and thus, we can rule out this possibility. Our findings do not show that the quality is significantly lower, so we do not take this into account in the back-of-the-envelope calculation.

Our approach, however, does not capture whether procurement contracts supplied by connected firms are necessary. One can imagine that a contract could be made up and then directed to the connected company. We do not have information to examine whether procurement contracts are needed. The cost evaluated in this paper could thus underestimate the actual welfare effect and it should be interpreted as the amount of resources transferred from taxpayers to connected firms. Furthermore, politically connected companies could cooperate more efficiently with the public sector, such that less supervision and cooperation from the side of the public sector are needed. We do not have information about the ex-post cost of procurement contracts, and thus we cannot directly evaluate this.

6 Conclusion

In this paper, we study the effect of political connections on the performance of public procurement. We find evidence that contracts supplied by politically connected firms are procured for a higher price. Moreover, we establish that there are no gains in quality associated with buying from connected firms. These socially undesirable effects of political connections are highlighted by the fact that even contracts for homogeneous goods (where there are no quality differences between suppliers and the procurers should simply buy what is cheapest) are overpriced when delivered by connected firms. Our conservative estimate suggests that tenders allocated to favored firms are overpriced by 6%, for a total loss of 96 million USD (approximately 0.36% of the value of the market). We contribute to the previous

literature by showing the effects on price while controlling for expected costs of procurement contracts and by calculating the cost induced by favoritism in public procurement for the public sector. These costs should be seen as a transfer from taxpayers to connected firms. Finally, we provide suggestive evidence on the mechanisms of favoritism.

Our calculations likely result in an underestimation of favoritism and corruption in this market. We can only analyze contracts that are awarded to firms that have an explicit connection to a procuring agency. It is impossible to collect a dataset capturing all personal connections, and measuring corruption is very hard. Thus, our estimates of the welfare cost should be interpreted as a lower bound of the total cost. Furthermore, if procuring agencies tailor contracts to a particular connected bidder, then our approach potentially underestimates the impact of political connections. The reason is that we omit the impact on contracts where a bidder was connected and did not win. But in such contracts, any tailoring is likely making it costlier for all non-connected firms to prepare a bid and to deliver, and thus, it increases the prices of such public procurement contracts.

Insofar the findings of this paper offer do not offer clear policy recommendations. Misusing discretion or restricting competition in favor of connected firms might have clear policy prescriptions. If the source of favoritism is contract manipulation, however, a policy recommendation is less obvious. There are policies that try to mitigate the effects of at least the most obvious issues related to political connections – conflicts of interest. For example, the Financial Regulation of the European Union that came into power in August 2018 requires that *"... persons, including national authorities at any level, involved in budget implementation under direct, indirect and shared management, including acts preparatory thereto, audit or control, shall not take any action which may bring their own interests into conflict with those of the Union"* (Article 61). The member states of the Union have to enforce these rules. Nevertheless, these rules do not apply directly when a former/future politician takes a position in a private company. We do not want to suggest that it would be optimal to prevent, for instance, former politicians from working in the private sector, but our results

suggest that it might be useful to analyze possible regulation of these activities. Such rules are common in the private sector in the form of competition clauses, and such a policy exists for European Commissioners, who have 18 months (paid) "cooling-down period" after they finish their term.

More promising, our findings indicate that stricter oversight of contract allocation is advisable. This seems to be especially the case when a higher level of government provides oversight over a lower level. The level of necessary oversight is likely country-specific, and the level of oversight and regulation is likely higher than in countries with better institutions and lower corruption levels.

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Appendix

A Election Terms Within the Studied Period 2004-2018

Table A1: Election Terms Within the Studied Period 2004-2018

Municipalities	Regions	Parliament	Total
2006-2010	2004-2008	2006-2010	3
2010-2014	2008-2012	2010-2013	3
2014-2018	2012-2016	2013-2017	3
	2016-	2017-	2
3	4	4	**11**

Note: This table presents elections terms at all levels of government that are studied in this paper. Note that parliamentary elections are decisive for the central government and ministries.

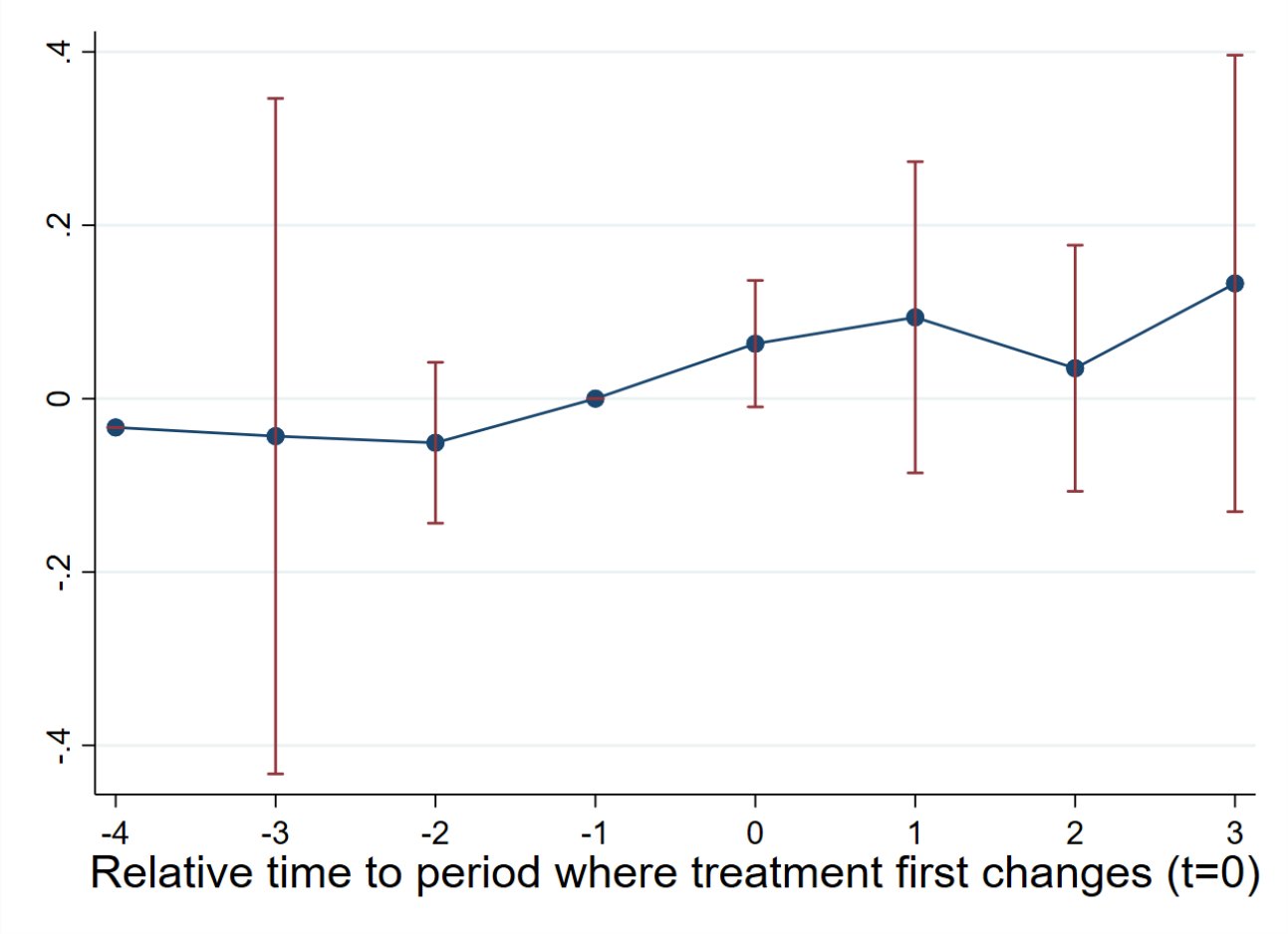
B Event Study

de Chaisemartin and D'Haultfoeuille (2022) developed a test to check the assumption of parallel trends between switchers and groups that have stable treatment. Figure A1 shows the outcome of the test. Individual coefficients are presented in Table OA2. The figure shows that the differential pre-trends are smaller, negative, and insignificant while the differential trends after a treatment switch are positive and the first one is significant at 10%.

It shows the expected pattern, that is the effect of the future political elections is close to 0 and mostly insignificant and the effect of political connections starts to be significant after the elections when the parties to which the suppliers are connected get voted in power. The figure reassures us that our results are not driven by particular pre-trends. Note that the

point estimate at the visible drop at $t = -1$ is very close 0 and highly insignificant so we do not interpret this as any type of pre-elections punishment for firms that will be connected after the elections.

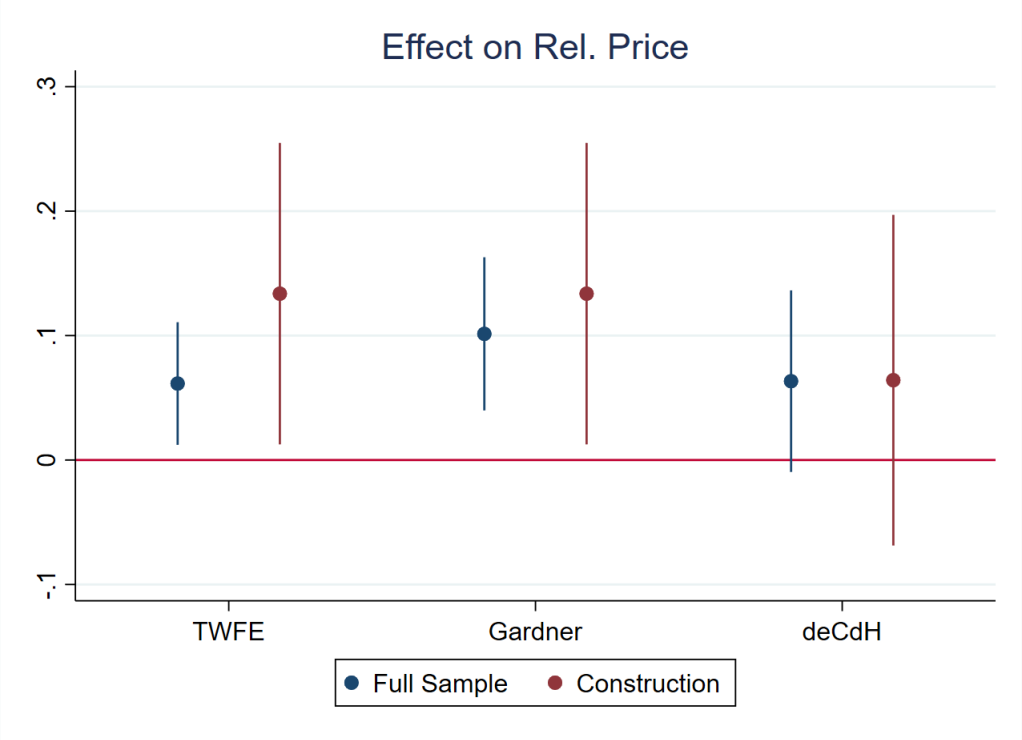
Figure A1: Effect of political connections on relative prices



Notes: The difference in the relative price is on the vertical axis and time in years from elections on the horizontal axis. To the left of zero, the line shows placebo estimates proposed by de Chaisemartin and D'Haultfoeuille (2022). At $x = -1$, the placebo is normalized to zero. To the right of zero, the blue line shows the estimates of the effect of political connections on relative prices of public procurement contracts using the estimator proposed by de Chaisemartin and D'Haultfoeuille (2022). The estimates are calculated using the Stata command `did_multiplegt`.

C The Effect of Connections on Prices – Full Sample and Construction

Figure A2: Effect of political connections on relative prices – robustness



Notes: The point estimates come from the standard two-way fixed effects estimation (TWFE in blue), the estimator proposed by (deCdH in red de Chaisemartin and D'Haultfœuille, 2020), and the two-stage estimator by (Gardner in green color Gardner, 2021). The line represents 95% confidence interval. All specifications include procurer-firm, year, and industry fixed effects.

D Probability of Final Price Above the Estimated Cost

Table A2: Probability of Final Price Above the Estimated Cost

	Non-connected	Connected	T-stats/Z-stats	P-value.
Above Estimated Cost (T-test)	.1268118	.183526	-4.422	0.0000***
Above Estimated Cost (Mann-Whitney)	57,310,000	12,499,121	-4.421	0.0000***
Observations	34,222			

Notes: The table shows parametric (t-test in the first row) and non-parametric tests (Mann-Whitney in second row) of the difference in the probability of the contract price being above the estimated costs for connected and non-connected suppliers. *** p<0.01, ** p<0.05, * p<0.1

E Renegotiation of Procurement Contracts

Previous research by Decarolis (2014); Decarolis and Palumbo (2015) show that renegotiation can be another way how costs of contracts are increased ex-post often in exchange for lower initial bid price in, for instance, lowest price auctions. This is also the core finding of Brogaard et al. (2020), who find that political favoritism in the US occur through renegotiation of already awarded public procurement contract. We do not observe this in the data. The reason behind that is likely that renegotiation of procurement contracts in the Czech Republic is much more strictly regulated than in other countries. Generally, the final cost of the contract needs to be equal to the winning bid. If there is a serious reason for additional cost increase due to unexpected circumstances and the additional works are technically or economically inseparable from the initial contract, the additional contract is handled in the following way. The procurer starts a new tender using the "negotiation without publication procedure" framework and awards this contract directly to the original supplier. [36] The

[36]In our data, we do not directly observe the renegotiation of each contract. However, we can link contracts with their renegotiations with a high success rate. We isolate all contracts between a specific firm-

cost increases due to this are capped at 10%. In Table A3, we examine whether the average renegotiation is higher for procurement contracts delivered by politically connected firms and find no statistically significant evidence that this is the case.

Table A3: Renegotiation of contracts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Reneg	Reneg	Reneg	Reneg	Reneg	Reneg	Reneg
Connection	0.00262	-0.00665	-0.00769	0.00187	-0.000370	-0.0061	-0.0013147
	(0.0067)	(0.0065)	(0.0095)	(0.0213)	(0.0093)	(0.0132)	(0.0545495)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No	No	No
Firm-year FE	No	No	No	Yes	No	No	No
Firm/procurer FE	No	No	No	No	Yes	Yes	Yes
N	24,702	24,702	24,702	24,702	24,702	24,490	2,817

Robust standard errors clustered at procurer level in parentheses

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

Notes: The specification is= similar to he one of Table 3. The outcome variable differs and here it is the measure of average renegotiation. We also control for the logarithm of the estimated price, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications. The last two columns employ alternative estimators: (Gardner, 2021) and (de Chaisemartin and D'Haultfœuille, 2020), respectively.

procurer pair and we know which of these are renegotiations. Based on this we then compute the expected renegotiation for each contract.

F Heterogeneity of the Effects of Political Connections

Throughout the analysis, we group together contracts procured by 3 different levels of government and we also do not distinguish between the effects of connections relatively close to the times of elections and in the middle of election terms. There are competing theories for the amount of corruption on different levels of governance. There is less public oversight over procurement contracts that are procured by smaller procuring authorities. This would lead to the hypothesis that the effect of political connections is smaller for ministries than for regions or even municipalities. But perceived corruption appears to be higher at the highest levels of government which would lead to the opposite hypothesis (François and Méon, 2018).

Regarding the timing, politicians might try to increase the probability of their re-election and because of that try to avoid any scandals and corrupt behavior.[37] This leads to the hypothesis that the effects of political connections would be lower shortly before elections. On the other hand, they might know that the probability of being elected is very low and they could then rather try to help themselves by giving away contracts to firms they are connected to.[38]

To determine whether these hypotheses are valid, we empirically test for the differences in the effects (see Table A4). In Column 1, we study heterogeneity across levels of governance, and in Column 2, we look at heterogeneity based on the timing in the electoral cycle.

In Column 1, we interact the level of governance dummy with the connection dummy. The baseline group is municipalities. The estimate of the effect is close to the original estimates but it is noisier in the smaller sample and, thus, the coefficient is not statistically

[37] This is inspired by extensive literature on political business cycles. The theoretical grounds of this broad stream of literature are laid out in Rogoff and Sibert (1988) in which the authors argue that there are similar electoral cycles in, for instance, taxes and government spending.

[38] This idea is similar to the argument of Besley and Case (1995) who study the behavior of the US governors between 1950 and 1986 and show that there are differences in the behavior of the governors that can run again and those in the last term (those facing binding term limit). This according to the authors results in a fiscal cycle. In our setting, there are no binding limits, however, there can still be reasons why politicians would either do not consider running (family reasons, etc.) or their chances of winning are very low (because of, for instance, a policy of the politician's party at the national level). And then, these politicians might arguably change their behavior as well.

significant. The effect on regional levels seems to be the largest and on the other hand, ministries have the lowest size of the effect. However, all three coefficients are insignificant on conventional levels and are not statistically different from each other. This is because we effectively split the sample into 3 sub-samples, and consequently, lack statistical power. We are rather inclined to interpret these results as not having sufficient evidence that the effects differ.

In Column (2) of Table A4, we examine two different issues: *(i)* whether there are differences between the effects of political connections in time relative to the time of elections and *(ii)* whether there is any pattern in time for non-connected contracts only. The answer to our first hypothesis is in the last 3 coefficients. We see that there are no differences in the estimated coefficient in time – namely it does make a difference whether a contract is awarded just after the elections (the reference group in the 1^{st} row) or 2, 3 or 4 years after the elections (7^{th}, 8^{th} and 9^{th} row, respectively).

Our identification relies on the variation in connections that changes with elections (a different party overtakes power). Thus, we are interested whether the time from elections (the number of the year before/after the elections) affect the non-connected firms. In such a case, our identification would not be valid. This is reflected in 4^{th} up to 6^{th} row of Column (2). We can see that there is no time pattern for non-connected firms either.

Table A4: Heterogeneity

	(1)	(2)
	Rel. Price	Rel. Price
Connection	0.0827***	0.100***
	(0.0317)	(0.0281)
Connection × Region	0.0452	
	(0.0506)	
Connection × Ministry	0.0163	
	(0.0408)	
2 Years after Elections		-0.00226
		(0.0051)
3 Years after Elections		-0.00285
		(0.0051)
4 Years after Elections		0.00709
		(0.0053)
Connection × 2 Years after Elections		0.00211
		(0.0304)
Connection × 3 Years after Elections		0.000963
		(0.0284)
Connection × 4 Years after Elections		-0.00607
		(0.0333)
Industry FE	Yes	Yes
Year FE	Yes	Yes
Firm FE	No	No
Procurer FE	No	No
Firm/procurer FE	Yes	Yes
N	23,244	23,244

Standard errors in parentheses

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

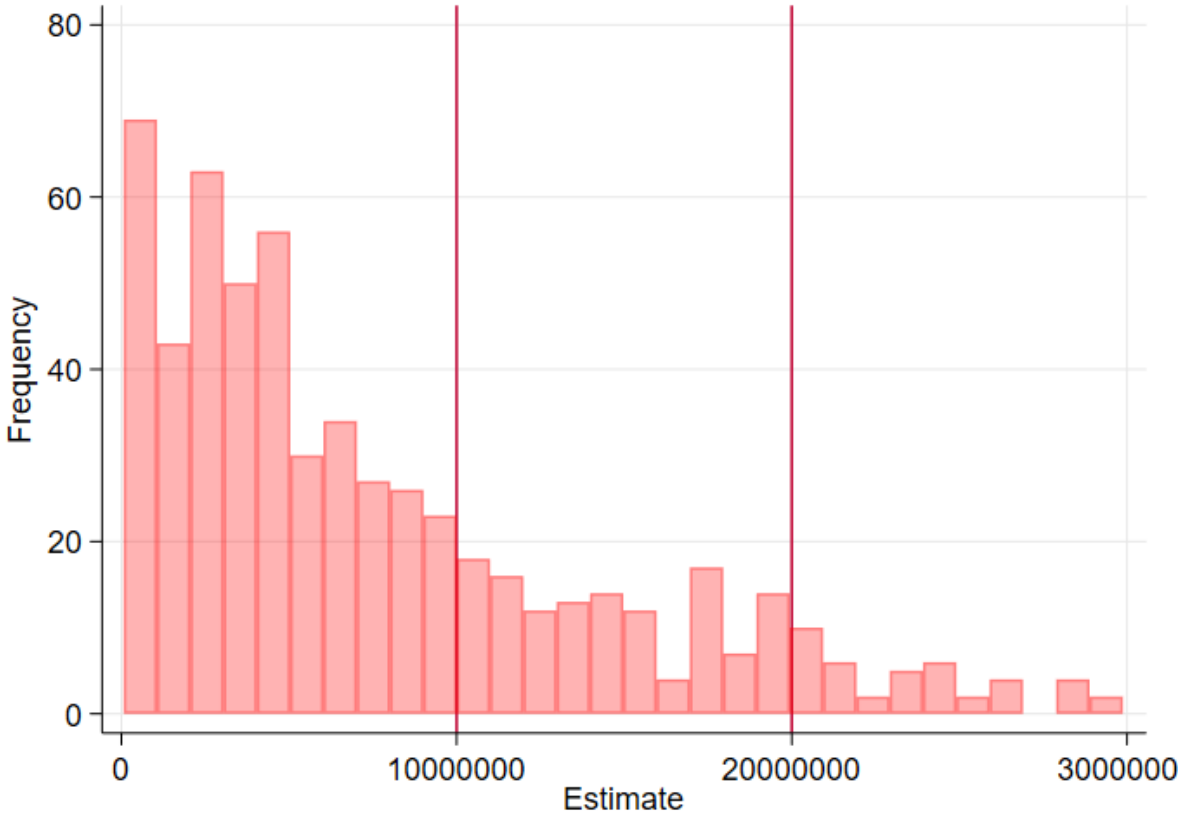
G Robustness

G.1 Manipulation of Estimated Costs

A systematic bias in the engineering estimates would be a potential threat to our empirical analysis. We already demonstrated that the estimates are a very good predictor of the realized price. However, we want to further discuss the potential incentives of procurers to manipulate them. Palguta and Pertold (2017) show that some projects might have downwards manipulated estimates so that procurers fit the projects below the threshold where they enjoy more discretion and they can award the project under less transparent conditions. First, we showed that politically connected firms are not more likely to receive contracts through discretionary methods and so the incentive to undercut estimates should not be prevalent in this scenario. To further support this claim we plot the estimates and wee see that there is no significant bunching (see Figure A3). There are only 17 contracts that are allocated to connected firms in our sample and their estimated prices are in a generous 500,000 CZK bandwidth below the discretionary threshold. These thresholds were 10M and 20M CZK.

A more likely scenario might arise where procurers overestimate the cost estimate to mask the potential overpricing of the final contract. We cannot completely rule out such a scenario and in this case, we would only get a lower bound for how overpriced contracts are.

Figure A3: Bunching around thresholds



Notes: In this figure, we study the possible bunching of contracts allocated to politically connected firms around the discretionary threshold. On the horizontal axis, there is the estimated price of a contract and, on the vertical one, frequency. The two vertical lines mark thresholds for changes in discretion.

G.2 The Effect of Placebo Connections

We assume that only connections to the party in power matter. However, one can imagine that in some cases being connected to the opposition could help. Thus, we redefine connections as connections to any other party than the party in power and call them *Placebo Connections*. Subsequently, we re-run the analysis with this new measure of connections. Such test can be seen as either *(i)* a test of the mechanism behind favoritism (that is whether a firm must be connected to the "right" party or it is sufficient to be connected to any po-

litical party) or *(ii)* a placebo test for our preferred measure of political connections. We present the results in Table A5. The point estimates are insignificant and very close to 0. The negative sign on most of them would suggest that companies connected to the opposite bid even lower than others (perhaps as they might fear that they have disadvantages). However, the size of the effect is negligible.

Table A5: Placebo test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Placebo Connnection	0.0033	0.0062	-0.0104	0.0022	-0.0444	-0.0510	-0.7001
	(0.0069)	(0.0072)	(0.0113)	(0.0137)	(0.0428)	(0.0444)	(0.6919)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No	No	No
Firm-year FE	No	No	No	Yes	No	No	No
Firm/procurer FE	No	No	No	No	Yes	Yes	Yes
N	34,007	34,007	34,007	34,007	34,007	29,005	2,922

Standard errors in parentheses

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

Notes: The specification is a replication of Table 3 with connection defined as a connection to any party that is not in power. The outcome variable is the final price over the estimated cost. We also control for industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections throughout all specifications.

G.3 Close Elections

A possible concern with our approach could be endogeneity of the measure of political connections – as the ambitious firms could be at the same time seeking sectors with high profit margin (so we would find the positive effect of connections on prices) and actively seeking political connections. This would lead to an upward bias of our estimate of the effect of political connections. We construct thus a measure for narrowly won elections. Subsequently, we test whether the effect of political connections differ for procuring authorities where the party won very convincingly. The effect of political connections for contracts procured by procuring authorities where the party in power won narrowly can be considered a valid estimate as contracts can be randomly assigned to treatment – it is difficult to imagine that firms get connected to the party in power that will be in power when it is very uncertain which party will win.

We consider elections narrowly won if the difference between the winner and the second strongest party is less than 5% of votes. Otherwise, we call them convincing elections. If there was no effect of political connection on contracts that were procured by procuring authorities where the party in power won narrowly, it would suggest that the effect we find in our main specification (Table 3) is biased. If the effect was significantly larger for contracts procured by procuring authorities where the party in power won convincingly, then the parties in power were able to better make use of their political power in favor of their favored firms if winning more convincingly. The results in Table A6 suggest that our approach is valid and the effect of political connection is significant, positive, and similar in magnitude to the estimates from our main specification.

Table A6: Narrow Elections

	(1)	(2)	(3)	(4)	(5)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Connection	0.0412***	0.0273*	0.0344**	0.0740**	0.0589**
	(0.0141)	(0.0145)	(0.0175)	(0.0327)	(0.0259)
Close Elections	-0.00304	0.00311	0.00177	0.00148	-0.00621
	(0.0030)	(0.0043)	(0.0047)	(0.0418)	(0.0063)
Connection × Close Elections	-0.0366*	0.000845	-0.00605	-0.0409	0.00397
	(0.0191)	(0.0194)	(0.0223)	(0.0368)	(0.0332)
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	34,007	34,007	34,007	34,007	34,007

Standard errors in parentheses

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

Notes: The specification is similar to the one of Table 3. Additionally, we include interaction with a dummy variable equal to 1 when a party won convincingly and 0 otherwise (narrow elections). We define narrow elections as the situation when the difference in votes between the winner and the second strongest party is 5% or less. The outcome variable is the final price over the estimated cost. We also control for industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections throughout all specifications.

G.4 Specification Tests

We extent the main analysis of the paper by adding an analysis that uses the relative price as the explained variable. We define the relative price as the final price divided by the estimate. The following table shows that in this specification our results are comparable to the ones in the main body of the paper. The coefficients are slightly attenuated, which is natural as the median relative price is below one and therefore the baseline is lower compared to our analysis using the logarithm of price as an explanatory variable and logarithm of estimate as a control.

Table A7: Effects of political connections on contract price - the number of bidders included as a control

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Price	Price	Price	Price	Price	Price	Price
Connection	0.0151	0.0196*	0.0252*	0.0336*	0.0585**	0.0656**	0.0644
	(0.0118)	(0.0105)	(0.0147)	(0.0203)	(0.0235)	(0.0307)	(0.0448)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No	No	No
Firm-year FE	No	No	No	Yes	No	No	No
Firm/procurer FE	No	No	No	No	Yes	Yes	Yes
N	34,007	34,007	34,007	34,007	34,007	24,173	3,191

Robust standard errors clustered at procurer level in parentheses

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

Notes: The outcome variable is the final price over the estimated cost. We also control for the number of bidders, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections throughout all specifications.

H Verification of the Matching Algorithm

The measure of quality of procurement projects used in Section 4.4 relies on the accuracy of the matching of initial projects and follow-up repairs. To verify that the accuracy of our matching algorithm is sufficient, we isolate all contracts of a mid-sized procuring authority – the city of Most in the Northwest of the country, and manually label all repairs found by the algorithm as a match or a mismatch. The accuracy rate base on this verification method is 93%. For details about the matching procedure and the verification, see Baranek (2020). Note that we use only the procurement projects that have a repair word from our library of repair/maintenance words in the description. This makes the task of manual verification feasible as it reduces the number of possible matches of follow-up projects but a factor of 50. Manual inspection of a small subset of other projects suggests that this step does not make our verification invalid. It does not lead a loss of follow-up projects.

I Theoretical Model

We describe a model where a subset of bidders – politically connected firms – receive preferential treatment in the process of public procurement allocation. Specifically, the buyer tailors the technical specification to favor the connected firms, which induces asymmetries among interested bidders.

This approach mimics the analysis of bidding subsidies in public procurement (Marion, 2007; Krasnokutskaya and Seim, 2011; Athey et al., 2013). In this class of models, the auctioneer uses bidding subsidies to achieve specific distributional goals, most often favoring smaller businesses. As a by-product, this might change the prices of the procurement contracts. Our setting differs in that there is no explicit bidding subsidy; rather the auctioneer artificially changes the underlying cost structure of the bidders. By employing technical requirements, he forces the bidders with costs c_i to bid as if they had costs $\tilde{c}_i$.

For each contract, there are two types of bidders – favored ones and non-favoured ones. In the first stage, all potential bidders (we will use the word bidder and supplier interchangeably) receive an entry cost shock FC_i. Based on this shock and the knowledge of the number and types of potential bidders, they decide whether to pay this FC_i and enter the procurement auction. This reflects the costs of preparing the technical proposal of the project, which is a necessary condition for submitting a bid. Bidders might also be heterogeneous on other dimensions so entry costs and bidding costs might be drawn from different distributions.

Upon paying the participation costs and entering, the bidders learn their costs of delivering the project c_i and the number and types of other entrants. The costs of firms are drawn from the distribution F_i.

$$c_i(z) \sim F_i(X_z)$$

where X_z is a vector of the characteristics of auction z. We assume an independent private value setting which is the standard in the procurement literature. Afterwards all bidders

compete in a lowest price auction.[39]

Political connections are thus going to affect this game by increasing costs for non-connected firms. If contract z is such that there is a connection between the procurer and some supplier, then all non-connected bidders are going to suffer a cost penalty δ when bidding. This means that the costs for completing such a project are now $(1+\delta)c_i$ instead of the original cost c_i for all non-connected i. We will denote by $\mathcal{F}$ all contracts such that there is a connection between the procurer of the contract and some supplier and we denote by $\mathcal{F}_j$ all suppliers that are connected to the procurer j.

Summing up the setup of the game, there are actions completed in the following order:

- t = 0: Connections between procurers and suppliers are exogenously established through an affiliation to a political party.
- t = 1: Tender for contract z starts. Suppliers learn costs of entering and decide whether to pay $FC_i(z)$ to enter the tender.
- t = 2: Suppliers submit bids according to either realized costs $c_i(z)$ or costs including the penalty if $z \in \mathcal{F}$ and $i \notin \mathcal{F}_j$.

The following propositions summarize the effect of favoritism on the outcome of the procurement auctions:

Theorem 1 *There is an uncertain effect of favoritism on both the final price and the intensity of competition (number of bidders).*

There are several effects that affect the final outcome in the opposite way.

Theorem 2 *Favoritism weakly increases the costs of the winning firm.*

This statement is trivially true as the costs are either unchanged or artificially increased.

[39] A similar model is used by Krasnokutskaya and Seim (2011) to describe bidding behavior in highway constructions.

Theorem 3 *Holding the set of participants constant, favoritism has an uncertain effect on the expected minimum bid.*

Inflating costs of a subset of bidders increases the bidders' costs but it will also change the bidders' equilibrium behavior. Intuitively we would expect a cost increase. However, this favoritism might also decrease prices if the auctioneer favours firms that are otherwise less competitive. Such examples are often discussed in the literature studying bidding subsidies. See Maskin and Riley (2000) or Marion (2007) for further discussion.

Theorem 4 *Favoured (nonfavoured) bidders are more (less) likely to enter which drives the prices down (up).*

Our final proposition shows the differential effect of favoritism on entry probabilities. Roberts and Sweeting (2013) discuss the issue of entry in much more detail.

The model does not give us exact qualitative predictions. However, it is useful for fixing ideas about which channels could be impacted by favoritism and we will use it to specify assumptions that are needed to calculate the costs of distortions caused by favoritism.

The empirical literature generally finds that preferential treatment of a subgroup of bidders.[40] leads to inefficiency and higher prices (Marion, 2007; Krasnokutskaya and Seim, 2011) Such results are consistent with our finding. We document that costs increase both through the channel of restricted entry and through implicit bidding penalties in the bidding stage.

[40] Albeit not due to corruption but due to an explicit bidding subsidy

A Online Appendix

A.1 The Effect on Prices – Alternative TWFE Specifications

In Table OA1, we present alternative, less saturated specification of the regression from Table 3. In Column (1), we essentially regress the ratio of final price over the estimated costs on the dummy for political connection. In the following columns, we add fixed effects. We start by controlling for heterogeneity of procurers by including procurer FE (Column 2), sorting of firms to different contracts by controlling for both firm and procurer FEs (Column 3), and including firm-year and procurer-year FEs (Column 4). The heterogeneity of procurers can come from, for instance, geographical locations of procuring authorities or some specific requirements, which would lead to systematically higher or lower prices (such as special security requirement of a ministry of defense or secret service). Adding firm FEs capture such heterogeneity on the side of firms, for example, if a group of suppliers was systematically cheaper and also connected, it could influence our estimate of the effect of political connections (even though this is not what we are interested in). Both procurer (Column 2) and firm (Column 3) fixed effects were time in-variant. This might not be realistic as, for example, the productivity of a particular firm might change. Therefore, we include firm-year and procurer-year FEs.

Table OA1: Effects of political connections on contract price

	(1)	(2)	(3)	(4)	(5)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Connection	0.0252	0.0289***	0.0270*	0.0445**	0.0602**
	(0.0154)	(0.0102)	(0.0145)	(0.0224)	(0.0253)
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	34,007	34,007	34,007	34,007	34,007

Standard errors in parentheses

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

Notes: The outcome variable is the share of the final price divided by the estimate. We also control for the industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications.

A.2 Event Study Coefficients

In Appendix, we present results of the parallel trends test developed byde Chaisemartin and D'Haultfoeuille (2022). The graphical presentation is in Figure A1. Individual coefficients are presented in Table OA2.

Table OA2: Effect of connections on the final price

	Estimate	Standard error	Observations
DID_M^0	.0634*	.0372	2,170
DID_M^1	.0938*	.0915	1,207
DID_M^2	.0351*	.0724	559
DID_M^3	.1329*	.1343	365
$DID_M^{pl,1}$	-.0509	.0474	1,067
$DID_M^{pl,2}$	-.0433	.1988	26
$DID_M^{pl,3}$	-.0332	.0000	10
Industry FE		Yes	
Year FE		Yes	
Firm-procurer FE		Yes	

Standard errors in parentheses

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

A.3 The Effect on Quality – Alternative TWFE Specifications

Table OA3: Effect of connections on quality

	(1) Followups	(2) Followups	(3) Followups	(4) Followups	(5) Followups
Connection	0.274	0.178	0.139	0.495	0.0847
	(0.3543)	(0.1571)	(0.1167)	(0.4517)	(0.2379)
Estimated price	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	16,866	16,866	16,866	16,866	16,866

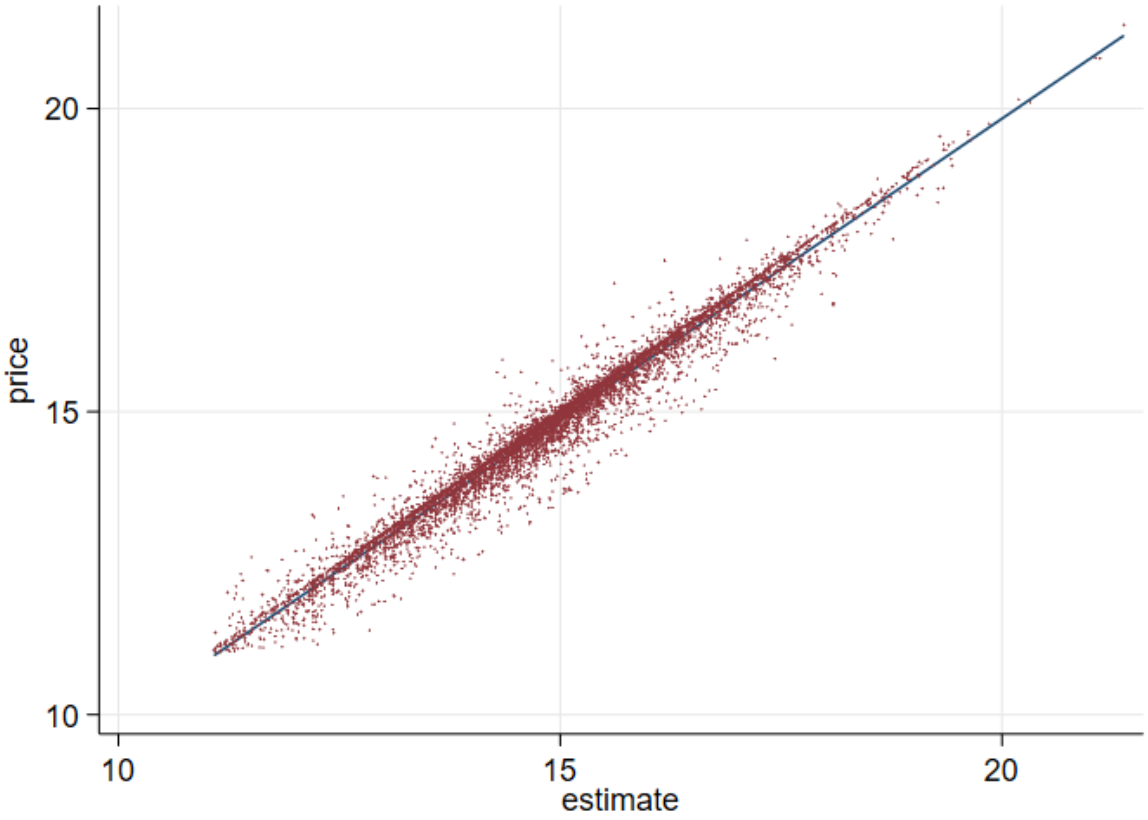
Standard errors in parentheses

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

Notes: The outcome variable is the value of repairs over the engineering estimate of costs. We also control for industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections throughout all specifications.

A.4 The Market of Homogeneous Goods

Figure OA1: Cost estimates and prices of goods



Notes: The log of the engineering estimate of costs is on the horizontal axis and the log of the final price on the vertical one. Each dot represents one public procurement contract. The red solid line is a regression line. Note that compared to Figure 1 the sample is limited to procurement contracts of homogeneous goods.

Table OA4: Effect of connections on the final price for contracts for goods

	(1)	(2)	(3)	(4)	(5)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Connection	0.0457	0.0555	0.0687	0.278**	0.274***
	(0.0333)	(0.0348)	(0.0490)	(0.1414)	(0.0857)
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	7,162	7,162	7,162	7,162	7,162

Standard errors in parentheses

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

Notes: The outcome variable is the ratio of the final price over the engineering estimate of costs. We control for industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications.

A.5 Mechanisms of Favoritism

A.5.1 Detailed TWFE Specifications

Table OA5: Open auctions

	(1)	(2)	(3)	(4)	(5)
	Connection	Connection	Connection	Connection	Connection
Open	0.00142	0.00133	0.000575	-0.00143	0.00171
	(0.0017)	(0.0017)	(0.0017)	(0.0021)	(0.0019)
Estimated price	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	34,007	34,007	34,007	34,007	34,007

Robust standard errors clustered at procurer level in parentheses

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

Notes: The outcome variable is a dummy equal to 1 if a procurement contract was allocated to a politically connected firm. The main independent variable is a dummy equal to 1 if an open auction was used. We also control for the logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications.

Table OA6: Scoring criteria

	(1)	(2)	(3)	(4)	(5)
	Connection	Connection	Connection	Connection	Connection
Scoring	-0.00410**	-0.00510**	-0.00120	0.00492*	-0.00475**
	(0.0019)	(0.0020)	(0.0020)	(0.0025)	(0.0022)
Estimated price	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	34,007	34,007	34,007	34,007	34,007

Robust standard errors clustered at procurer level in parentheses

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

Notes: The outcome variable is a dummy equal to 1 if a procurement contract was allocated to a politically connected firm. The main independent variable is a dummy equal to 1 if scoring criteria were used. We also control for the logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications.

Table OA7: Length of description

	(1)	(2)	(3)	(4)	(5)
	Connection	Connection	Connection	Connection	Connection
Length	0.0000810***	0.0000417*	0.0000516**	0.0000366	0.0000699***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Estimated price	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	34,007	34,007	34,007	34,007	34,007

Robust standard errors clustered at procurer level in parentheses

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

Notes: The outcome variable is a dummy equal to 1 if a procurement contract was allocated to a politically connected firm. The main independent variable is the length of the text string describing the product. We also control for the logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications.

Table OA8: Complexity of the projects' description

	(1)	(2)	(3)	(4)	(5)
	Connection	Connection	Connection	Connection	Connection
Complexity	0.0000345***	0.0000297***	0.0000196***	0.00000699	0.0000115**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Estimated price	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	34,007	34,007	34,007	34,007	34,007

Robust standard errors clustered at procurer level in parentheses

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

Notes: The outcome variable is a dummy equal to 1 if a procurement contract was allocated to a politically connected firm. The main independent variable is $\frac{length\ of\ description}{value\ of\ project\ in\ mil.}$. We also control for the logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications.

Table OA9: Effects on the number of bidders

	(1)	(2)	(3)	(4)	(5)
	Bidders	Bidders	Bidders	Bidders	Bidders
Connection	-0.457***	-0.359***	-0.253**	-0.320	-0.246
	(0.0960)	(0.0967)	(0.1219)	(0.2009)	(0.1773)
Estimated price	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	33,251	33,251	33,251	33,251	33,251

Robust standard errors clustered at procurer level in parentheses

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

Notes: The outcome variable is the number of participating bidders. We also control for the logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications.

Table OA10: Lengths of description, complexity, and the number of bidders

	(1)	(2)	(3)
	Nr. of Bidders	Nr. of Bidders	Nr. of Bidders
Length	-0.00736***		
	(0.0008)		
Log(Length)		-0.565***	
		(0.0578)	
Complexity			-0.00128***
			(0.0002)
Estimated price	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Procurer FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Procurer-year FE	No	No	No
Firm-year FE	No	No	No
Firm/procurer FE	Yes	Yes	Yes
N	22,582	22,541	22,582

Standard errors in parentheses

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

Notes: The outcome variable is the number of bidders. The main independent variables are the length of the text string describing the product (*Length*), the logarithm of the length of the text string describing the product (*Log*(*Length*)), and *Complexity* measured as $\frac{length\ of\ description}{value\ of\ project\ in\ mil.}$. We also control for the logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications.

A.5.2 Alternative TWFE Specifications

The analysis of the mechanisms behind the observed favoritism in the main text (Section 4.5) uses the empirical specification with the dependent variable and the independent variables being switched, relative to the rest of the paper. The reason is the variables such whether an auction is open, whether it is an open auction, or the length of the description are determined ex-ante. They are decided before it is decided whether a firm that won the contract is connected or not, therefore, our standard specification with political connections as an independent variable could suffer from reverse causality. Nevertheless for the sake of transparency, we include regressions with our standard specification below in Table OA11, OA12, OA13, and OA14. The results support our interpretation of the mechanisms provided in the main text.

Table OA11: Open auctions

	(1)	(2)	(3)	(4)	(5)
	Open	Open	Open	Open	Open
Connection	0.0151	0.0136	0.00722	-0.0215	0.0280
	(0.0179)	(0.0177)	(0.0219)	(0.0322)	(0.0307)
Estimated price	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	34,007	34,007	34,007	34,007	34,007

Standard errors in parentheses

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

Notes: The outcome variable is a dummy equal to 1 if a procurement contract was allocated through an open procedure. The main independent variable is a dummy equal to 1 if a politically connected firm supplied the contract. We also control for the logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications.

Table OA12: Scoring criteria

	(1)	(2)	(3)	(4)	(5)
	Scoring	Scoring	Scoring	Scoring	Scoring
Connection	-0.0337**	-0.0382**	-0.0113	0.0524*	-0.0589**
	(0.0157)	(0.0152)	(0.0189)	(0.0271)	(0.0267)
Estimated price	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	34,007	34,007	34,007	34,007	34,007

Standard errors in parentheses

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

Notes: The outcome variable is a dummy equal to 1 if scoring criteria were used. The main independent variable is a dummy equal to 1 if a politically connected firm supplied the contract. We also control for the logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications.

Table OA13: Length of description

	(1)	(2)	(3)	(4)	(5)
	Length	Length	Length	Length	Length
Connection	5.535***	2.694*	4.053**	3.966	6.728***
	(1.4308)	(1.4077)	(1.7315)	(2.7315)	(2.3508)
Estimated price	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	34,007	34,007	34,007	34,007	34,007

Standard errors in parentheses

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

Notes: The outcome variable is the length of the text string describing the product. The main independent variable is a dummy equal to 1 if a politically connected firm supplied the contract. We also control for the logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications.

Table OA14: Complexity of the projects' description

	(1)	(2)	(3)	(4)	(5)
	Complexity	Complexity	Complexity	Complexity	Complexity
Connection	24.99***	21.90***	18.28***	13.52	19.89**
	(4.6606)	(4.7588)	(5.9655)	(11.5509)	(9.9796)
Estimated price	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes
Procurer-year FE	No	No	No	Yes	No
Firm-year FE	No	No	No	Yes	No
Firm/procurer FE	No	No	No	No	Yes
N	34,007	34,007	34,007	34,007	34,007

Standard errors in parentheses

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

Notes: The outcome variable is is $\frac{length\ of\ description}{value\ of\ project\ in\ mil.}$. The main independent variable is a dummy equal to 1 if a politically connected firm supplied the contract. We also control for the logarithm of the engineering estimate of costs, industry fixed effects, month fixed effects, and a dummy capturing whether the party in power changed in the last elections are included throughout all specifications.