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The One and Only: Single-Bidding in Public Procurement

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Abstract:

Approximately 23% of public procurement contracts in the European Union are awarded to the sole firm that submits a bid. The public procurement contracts market constitutes around one-seventh of GDP in developed countries, rendering any inefficiencies on this market a first-order problem. In this paper, I exploit a unique reform implemented in the Czech Republic that made it impossible to award contracts with only one bid and. Using a difference-in-differences strategy on the dataset of all public procurement contracts, I first show that the reform reduced prices by 10% relative to the estimated costs for single-bid public procurement contracts. Second, I provide evidence that procuring authorities started to provide significantly longer descriptions of procurement contracts and extended the timeframe for firms to prepare their bids. Last, I show that the prices of procurement contracts supplied by politically connected and anonymously owned firms were not reduced after the reform. The main contribution of this paper lies in estimating the savings attributable to the ban on single-bidding in public procurement.

Keywords: Single-bidding, Public procurement, Political connections, Corruption

JEL codes: D44, D72, H57

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1 Introduction

Public procurement accounts for 14% of the European Union member states' GDP (European Commission, 2017). Notably, a significant percentage of these contracts, around 23.2%, were allocated to the sole bidder (known as "single-bidding"; Fazekas and Kocsis, 2017). In some sectors—such as IT and telecommunication—single-bid procurement contracts account for 50–60% of all contracts (Fazekas, 2019). It is worth noting that this issue is not confined to Europe alone; even in the US, roughly 45% of the value of federal procurement contracts arise from tenders featuring just a single-bid (Kang and Miller, 2017). While there are instances when single-bidding is legitimate (such as in cases involving patented products or natural monopolies), the prevalence of such instances is unlikely to account for the disproportionately high share of single-bid contracts. Since a lack of competition is likely to induce higher prices (Decarolis, 2014), single-bidding may lead to potentially significant inefficiencies on the market. Consequently, any reform designed to address this phenomenon is worth studying from both an academic and policy perspective.

A number of other—perhaps less legitimate—explanations for low competition have been suggested, including incumbent advantages, a lack of publicizing, and corruption. In this paper, I consider these explanations and quantify the costs of single-bidding. To do so, I exploit a reform implemented in the Czech Republic that made it illegal to award a public procurement contract if only a single-bid was submitted. The reform came into effect on April 1st 2012 and impacted virtually all public procurement contracts tendered in open competitions in the Czech Republic. The reform created a need for contracting authorities to ensure at least a minimal level of competition in public procurement. I argue that this reform has had a number of different effects on procurement outcomes for different subgroups of contracts. For instance, I hypothesize a decline in prices for procurement contracts that were previously insufficiently publicized, but I anticipate no effect on prices (or cancellation) for procurement contracts delivered by potentially corrupt and/or collusive firms.

First, one plausible cause of single-bid contracts is an incumbent's cost advantage (Iossa

et al., 2019). The cost advantage can stem from contracts' specifications and/or how the qualification criteria are set. A ban on single-bid contracts would, in this case, lead to two effects that move contracts' prices in the same direction. Procuring authorities know that to be able to award a contract (and buy the goods or services that are contracted), they need to adjust the contract specifications so that firms other than the incumbent can bid. The incumbent is likely to notice this and bid more aggressively, as it knows that the competition is likely to increase. At the same time, firms that could potentially bid but did not do so prior to the reform (as they knew the incumbent had a significant cost advantage) are more likely to study the contract announcements (as they know that a single-bid contract cannot be awarded) and then bid. Based on these factors, I propose that within this subset of procurement contracts, the reform may lead to a decrease in the prices of delivered goods and services.

Second, a lack of publicity or insufficient information regarding procurement contracts can lead to low participation (Coviello and Mariniello, 2014). It is easy to imagine that if contracts are not properly publicized, potential suppliers may not participate. Similarly, if the description of a contract is not complete or is imprecise, firms may be less likely to bid due to the (cost) uncertainty attached to such contracts. My findings suggest that procuring authorities, accordingly, started to provide longer descriptions after the reform.

Third, public procurement markets are prone to corrupt activities and are significantly influenced by political connections (Goldman et al., 2013; Schoenherr, 2019; Decarolis et al., 2019; Titl and Geys, 2019; Baranek and Titl, 2024). A cross-country study by Fazekas and Kocsis (2017) provides suggestive evidence that a larger share of single-bid contracts is correlated with a higher level of perceived corruption. In cases where corruption (and/or collusion) explains low competition, procuring authorities and firms have little incentive to promote actual competition after the reform. Rather, firms prefer to collude in order to formally fulfill the requirements of multiple bidders while maintaining existing price levels. To explore this notion, I scrutinize a specific subset of contracts that are executed by politi-

cally connected and anonymously owned firms. My findings suggest that the relative prices of procurement contracts delivered by these firms did not decline (in contrast with other contracts).

Last, as mentioned above, there are procurement contracts delivered by natural monopolies or firms that hold patents. In such instances, a ban on single-bid contracts might be deemed unsuitable and ineffective, potentially resulting in the automatic cancellation of contracts involving patented products or those facilitated by natural monopolies. However, empirical data indicate that contract cancellations are generally not a prominent concern within the Czech public procurement market. Indeed, the share of cancelled contracts amounts to a mere 0.24% across all contracts and is only slightly higher among the contracts that are likely to fall within this category (0.33%—or 3 out of approximately 900 contracts are cancelled). I exclude these contracts for most of the main analysis.¹ A sub-section of the empirical results is devoted to assessing the effects of the ban on cancellation rates, and this investigation corroborates the conclusion that the ban did not lead to an increase in cancellations.

In a closely related study, Kang and Miller (2017) argue that achieving more competition through introducing an open procedure—in which any firm may submit a bid—as opposed to a discretionary procedure (often single-bid contracts) can be costly. In particular, they estimate that introducing the open procedure led to a higher level of competition, but, paradoxically, also led to higher final costs. This outcome is attributed to the constrained flexibility for post-negotiations (which in discretionary procedures might contribute to lower costs) and amplified expenses associated with attracting bidders. In the context of my setting, procuring authorities did not switch to a different procedure.² All contracts in my sample are procured using the open procedure. Hence, I can directly estimate the savings attributable to the ban on single-bidding without having to take into account the costs of changing the procedure. The main contribution of this paper, thus, lies in estimating

¹The exclusion of these contracts does not substantively change the results.

²The reform did not require them to change the procedure and my data shows that they did not change it voluntarily either.

the effects of the ban on single-bidding as a legal mean to enforce competition in the open procedure. On top of that, I study heterogeneity of the effect with regard to firms political connections (Titl and Geys, 2019; Baranek and Titl, 2024) and anonymous ownership (Palguta and Pertold, 2017).

The empirical analysis consists of two main steps. First, I present a set of stylized facts regarding the effects of the reform and discuss the underlying assumptions of my empirical approach. Second, I conduct a differences-in-differences analysis in which I compare relative prices before and after the reform for contracts that were impacted by the reform (treated group) and those that were not. My findings indicate that, for the majority of open public procurement contracts, the reform pushed prices down by roughly 10%. Furthermore, I provide evidence that procuring authorities started to provide significantly longer descriptions of procurement contracts and extended the timeframe for firms to prepare their bids. Last, I demonstrate that the effect on prices is not evident among procurement contracts supplied by politically connected and anonymously owned firms.

The remainder of the paper is structured as follows. In the second section, I describe the Czech public procurement market, with a particular focus on the reform of 2012. The third section describes the dataset and its construction. In the fourth section, I outline the empirical approach that I adopted. The fifth section presents the results and, in the sixth section, I offer a conclusion and present the policy implications of my analysis.

2 Institutional Setting

The Czech public procurement market is regulated by Act No. 137/2006 Coll. on Government Procurement. The act describes, among things, the set of possible evaluation criteria and the different types of allocation procedures that can be used. The execution of these regulations is the responsibility of specific procuring authorities.

This law has been amended multiple times. One major reform, designed to enforce

a minimum level of competition in public procurement, came into effect on April 1st 2012. Following this, any contract in open procedure with only one bid submitted must be cancelled (envelopes with submitted bids are not even opened) and the whole procurement procedure has to be relaunched. The reform was partially reversed on January 1st 2014. From this point onwards, the procuring authority had to relaunch the contract if there were fewer than two bidders (i.e., the contract was not cancelled completely). Hence, to ensure that my results are not driven by this subsequent or other reforms, I isolate a symmetric period of one year and nine months around the reform in which no other reforms took place. Later, after March 6th 2015, no rules regarding procurement competitions with less than two bidders were applied. I do not investigate these two later reforms per se, but it is necessary to be aware of how the legal framework evolved in order to be able to choose the right period of study. The reform was largely successful in enforcing more competition and followed by procuring authorities—the share of single-bid contracts was 18.7% before the reform and 0.6% after (the average for both periods was 11.5%).

Six basic allocation procedures are followed in the Czech Republic. The most prevalent (constituting more than 50% of procurement contracts worth approximately 61% of the value of the procurement market) is an open procedure in which any bidder can participate. Other procedures include the negotiated procedure (with or without publication), the so-called narrower procedure, competitive dialogue, and the simplified under-threshold procedure. In all except the open procedure, a procuring authority can easily limit the level of competition by asking only a limited number of companies to bid. In the context of this study, I concentrate solely on open auctions to ensure that the costs of running a tender cannot change (e.g., through switching procedures). A potential concern could arise if contracting authorities responded to the reform by switching from open auctions to a different allocation procedure (negotiated or approaching bidders). Such a shift could obscure a portion of the effects stemming from the mandated increase in competition. I therefore check whether there is some indication of this behavior. After the ban on single-bidding, only 3.5% of suppliers who

won contracts through the open procedure before the reform managed to secure contracts through the negotiated procedure after the reform. Furthermore, I do not find any significant shift from open to other procedures among the treated contracts. Thus, this concern appears to not be valid.

3 Dataset Construction

The dataset of public procurement contracts used in this paper contains all procurement contracts procured by any public institution above a threshold of 2,000,000 CZK (\$80,000) for goods and services and 6,000,000 CZK (\$240,000) for construction works. The dataset spans from 2006 to 2020; however, for the purposes of this paper, I focus on the period of one year and 9 months around the reform in 2012. The data contains information such as final prices, estimated costs of projects, the dates of announcement, the deadlines for bid submission, the dates of award, CPV (common procurement vocabulary) codes, and others.

To construct the dataset to analyze the effects of the reform, I proceed in two steps. First, I exclude contracts that are likely to be covered by patents or supplied by natural monopolies. This is done by excluding contracts in sectors (defined by 5-digit CPV codes) in which there were only single-bidding contracts over 5 years before the reform (roughly 2.9% of contracts). Second, these contracts must have been awarded in sectors³ in which contracts were supplied by firms that supplied contracts before and after the reform, procured by procurers that awarded contracts in the particular sector before and after the reform.

³Also here, I use 5-digit CPV codes to distinguish between sectors.

Table 1: Dataset of Public Procurement Contracts (2007–16).

(1)				
	mean	sd	p25	p75
Final Price [in 1,000 CZK]	16,900	109,000	952	11,500
Rel. Price	.816	.273	.641	.986
Preparation Time	35.5	26.8	20	50
Length Description	70.3	38.8	42	92
Complexity of Description	152.9	655.9	3.59	53.5
<i>N</i>	10,888			

Notes: Summary statistics on the basic variables of the sample used in the main specification. Final price is the amount of money paid for the procurement contract in thousands of CZK (\$1 = 24 CZK). The relative price is the final price divided by the engineering cost estimate. Preparation time is the number of days between the publication of the announcement and the deadline for bids submission, i.e. the amount of time in days that firms have to prepare their bids. Length Description is the length of short descriptions of public procurement projects expressed as the number of characters. Complexity of Description is the length of description divided by the engineering cost estimate multiplied by 1,000,000.

In Table 1, I present summary statistics on the main variables from the final dataset (restricted to the contracts used in the main specification). The average final price of a contract was \$700,000.⁴ The average relative price, which is defined as the share of the final price on the estimated cost, was 81.6%. The average preparation time is the time between the moment when the contract announcement is published online and the date of the deadline for bids submission. On average, it is 35.5 days.

⁴The exchange rate of 24 CZK for \$1—which was correct as of April 2020—was used.

4 Empirical Methodology

My empirical strategy is based on a difference-in-differences estimation. Hence, I need to define pre- and post-treatment periods as well as the treated and control groups of procurement contracts. Given that there are other reforms that came before or after this one, I define a window of one year and three-quarters around the reform, i.e., the period before the reform spans from October 1st 2010 to March 31st 2012 and the period after the reform from April 1st 2012 to September 31st 2013.⁵ However, extending the period is not feasible, as doing so would render my results susceptible to the influence of the January 2014 reform. Importantly, I have also tested the robustness of my findings by excluding the quarters directly surrounding the reform, a scenario that does not compromise the results' integrity⁶

4.1 Definition of Treated and Control Groups

The definition of the treatment group requires a more complex approach. I propose to define the treated contracts as procurement contracts in sector-procurer categories in which there was no competition in the 1.75-year-long period before the reform. As for the control group, I propose selecting contracts where the mean count of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform. This corresponds to approximately 2–4 bidders. Note that the main results remain if I define the control group as all other contracts.

The sector-procurer categories of contracts are defined based on the procurer identities, while the contract sectors are derived from the 5-digit CPV codes.⁷ In Table 2, I have summarized the changes in the levels of competition for the treated and control groups. By construction, the average number of bidders is equal to one for contracts in the treated group before the reform. Following the reform, this average number of bidders shifted substantially

⁵Note that since the reform came into effect in April, I can account for seasonal effects in public procurement by including month fixed effects.

⁶See Table 14 in Appendix.

⁷To test the robustness of the results, I also use the 3- and 4-digit CPV codes. The results are qualitatively similar and presented in Appendix.

in the treated group but remained largely unchanged in the control group. The effect of the reform on the shares of single-bid contracts among the treated and the control groups is presented graphically in Figure 1.

Table 2: Means of number of bidders in respective subgroups

	Before the Reform	After the Reform
Treated	1.0 (0)	3.09 (2.64)
Control	2.85 (0.79)	2.78 (0.79)

Notes: Summary statistics on the number of bidders for procurement contracts in the treated and the control groups before and after the reform. The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2–4). Standard deviations are in parentheses.

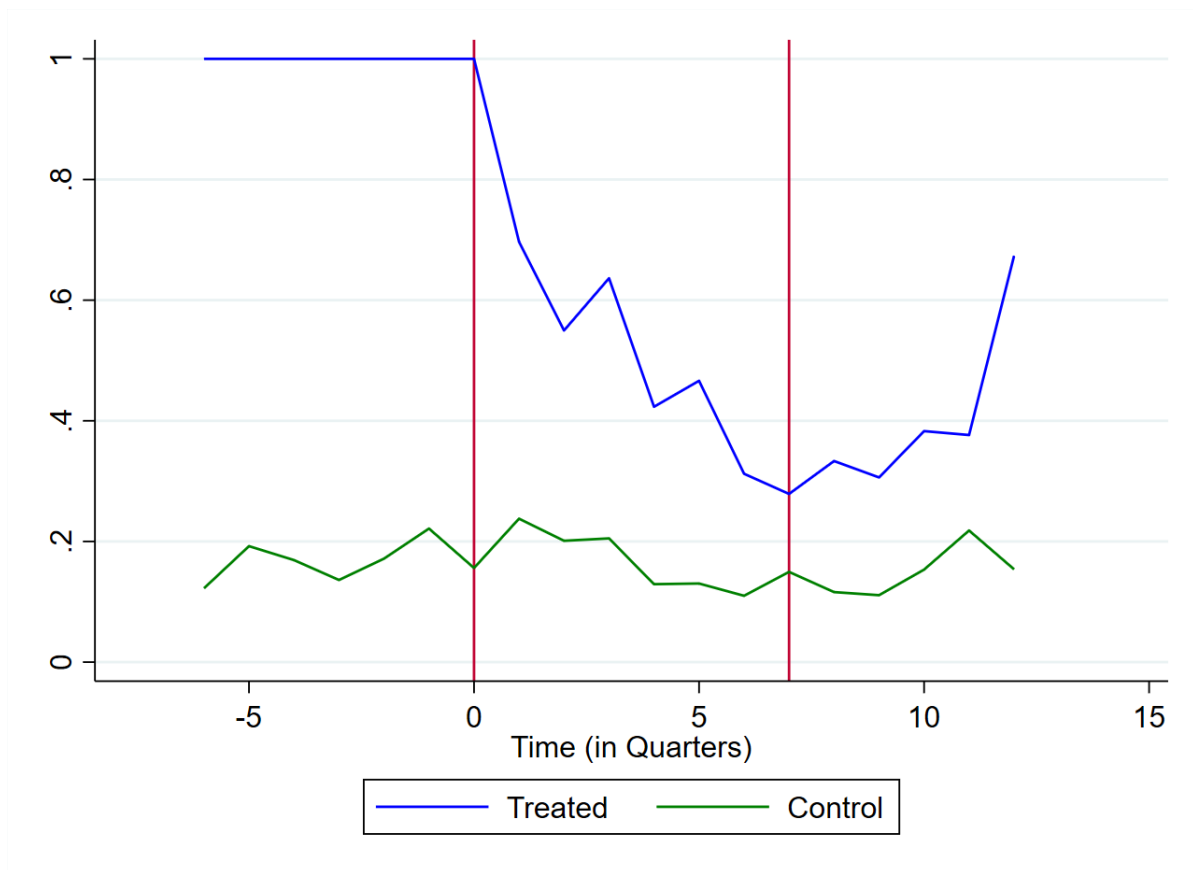


Figure 1: This figure plots the shares of single-bid public procurement contracts in the treated and control groups over time. The first red vertical line represents the time of the reform (April 1st 2012). The second red vertical line represents the reversal of the reform in 2014.

Before turning to the empirical results, it is important to illustrate the effect of the reform on the two groups of contracts over time. I visually depict this change in Figure 2, from which it can be seen, firstly, that the relative prices followed largely the same trends before the reform. Then, after the reform was implemented, the dots representing bins of treated and control contracts started to overlap. Similarly, the polynomial (quadratic fit) curves for both groups of contracts before and after the reform show that there was a gap between the relative prices of treated and control group contracts before the reform, which largely closed after the ban.

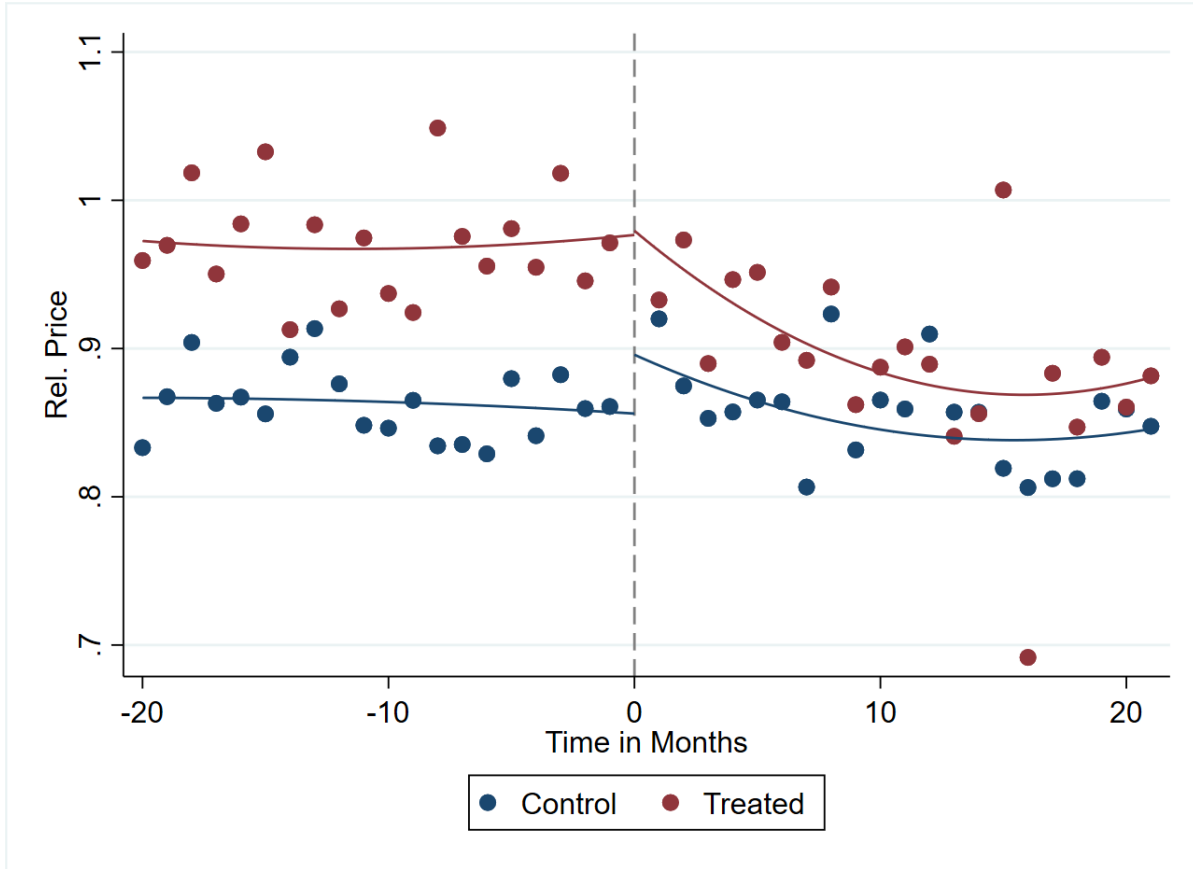


Figure 2: This binscatter shows the average relative prices of public procurement contracts in the treated and control groups before and after the reform. The treated public procurement contracts (in red) are the contracts in sector-procurer categories in which there was no competition (i.e. in each such contact only one bid was submitted) in the 1.75-year-long period before the reform. The control group (in blue) contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (2–4). The vertical dashed line is the reform time.

4.2 Estimation

To estimate the effect of the reform on relative prices, I run difference-in-differences estimations with a broad set of various fixed effects. First, I employ month fixed effects to account for seasonal effects. This is inspired by Liebman and Mahoney (2017), who show that at the end of the year, public authorities tend to spend the remaining budgeted resources. Second, I include sector fixed effects based on 6-digit CPV codes. Third, procurer fixed effects are added to account for differences among contracting authorities, especially in regard to how

they estimate costs (since it is done by a different team of officers in each procurer). Third, I include firm fixed effects to isolate the effect of the reform on how overpriced contracts firms deliver (i.e., I essentially measure the discount that the same firms "give" on average after the reform). Ultimately, I effectively compare the development of relative prices (the main dependent variable) for contracts in sector-procurer categories without competition before the reform with other contracts (the first difference) before and after the reform (the second difference).

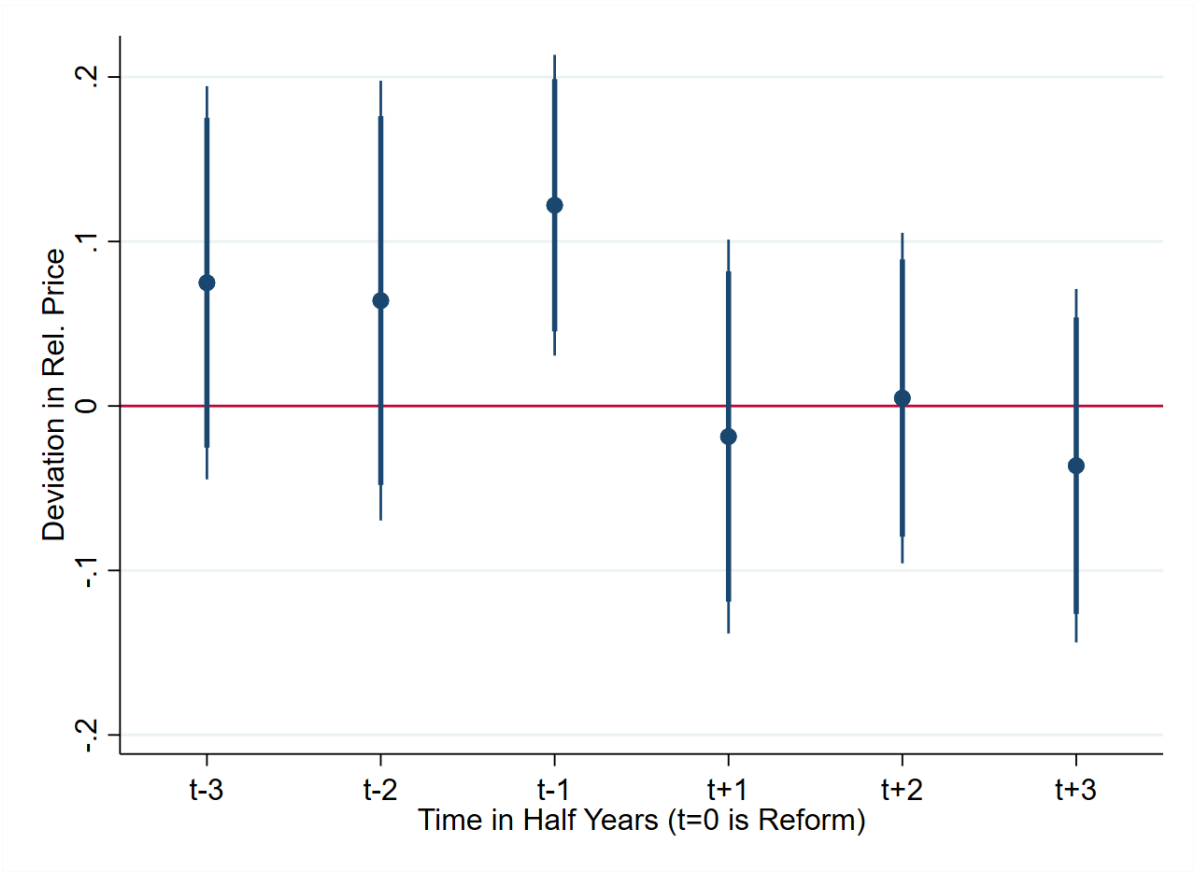


Figure 3: This figure shows the time trend in relative prices. The points represent point estimates obtained from regressing the interaction between treatment and time using time dummy variables (half-year intervals). The reference group comprises contracts awarded within the first three months after the reform. The confidence intervals are plotted as vertical lines, with the thinner lines denoting the 95% confidence interval and the thicker lines denoting the 90% confidence interval.

To identify the effect, the parallel trends assumption must be satisfied. This can be done

by visually assessing the trends. Although Figure 2 already suggests that this assumption was satisfied, I follow Autor (2003) and run a regression with a set of time dummies interacted with the treatment dummy variable in order to confirm it. To do this, I construct dummy variables for half years ($t-3$ up to $t-1$ and $t+1$ up to $t+3$) while the reference group is constructed using the set of procurement contracts awarded in the first three months after the reform ($t=0$). In Figure 3, the coefficients of the interactions are plotted alongside the 90% and 95% confidence intervals. Although the intervals overlap, we observe that the trends are parallel. The point estimate is approximately 0.1 before the reform and indistinguishable from 0 after the reform (this is relative to the reference group, i.e., the first three months).

The second important assumption we need to test is that the composition of types of contracts did not change for the treated and the control group over time. Upon examination, this assumption appears to hold true. Notably, the only substantial change in the share of contracts within broadly defined sectors (across 14 categories) concerns contracts related to industrial machinery (see Figure 15 in Appendix). Excluding the contracts in this sector from the estimations in the main specification changes neither the magnitude nor the significance of the main findings (see Table 16 in Appendix).

5 Empirical Results

The results of the baseline estimation are presented in Table 3 and suggest that enforcing competition leads to lower prices (see the interaction term). The magnitude remains virtually intact across specifications—suggesting that the price declined by approximately 10% after the reform across sectors. Moreover, the positive estimates of the coefficients in the first row indicate that the treated contracts were procured for significantly higher prices than the contracts in the control group.

Table 3: Difference-in-differences—complete sample

	(1)	(2)	(3)	(4)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Treated	0.0831***	0.0792***	0.0738***	0.0731***
	(0.0137)	(0.0120)	(0.0158)	(0.0212)
After	0.000449	-0.00533	0.000731	0.00920
	(0.0120)	(0.0077)	(0.0111)	(0.0120)
Treated \times After	-0.0935***	-0.105***	-0.104***	-0.108***
	(0.0184)	(0.0162)	(0.0208)	(0.0244)
Month FE	Yes	Yes	Yes	Yes
Sector FE	No	Yes	Yes	Yes
Procurer FE	No	No	Yes	Yes
Firm FE	No	No	No	Yes
N	8,019	8,019	8,019	8,019

Standard errors clustered at procurer level in parentheses

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

Notes: The dependent variable is the relative price (the share of the final price over the estimated cost). The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2–4).

Another important consideration is the potential bias that could arise if estimated costs were manipulated subsequent to the reform. Given that the dependent variable is formulated as the ratio of the final price to the estimated cost, any manipulation in the estimated costs could significantly impact the findings. I therefore check whether there are signs of

manipulation among the treated contracts by running the difference-in-difference estimation with the estimated costs serving as the dependent variable. The results, which are presented in Table 3, indicate that the estimated costs did not significantly change across the treated contracts after the reform, suggesting no treated-group specific manipulation.

Table 4: Difference-in-differences – complete sample

	(1)	(2)	(3)	(4)
	Est. Cost	Est. Cost	Est. Cost	Est. Cost
Treated	-0.203	0.110	-0.104	-0.296
	(0.1826)	(0.1621)	(0.1456)	(0.1809)
After	-0.388***	-0.146**	-0.160**	-0.253***
	(0.0996)	(0.0641)	(0.0789)	(0.0866)
Treated × After	0.0960	-0.00607	0.0898	0.194
	(0.2827)	(0.2487)	(0.2149)	(0.2439)
Month FE	Yes	Yes	Yes	Yes
Sector FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Procurer FE	No	No	No	Yes
N	4,780	4,780	4,780	4,780

Standard errors clustered at procurer level in parentheses

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

Notes: The dependent variable is the estimated cost of a procurement project. The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2–4).

A further concern revolves around the possibility of an increased number of contract cancellations within the treated contracts post-reform. To address this, I undertake an analysis akin to the main specification, employing a binary cancellation indicator as the dependent variable (see Table 13 in Appendix). Moreover, simple summary statistics show that cancellation is not a major issue—the share of cancelled contracts is 0.42% on all contracts in my dataset, with the rate being 0.28% before and 0.51% after the reform.

5.1 Aggregation

Although this result is an average over all contracts, a simple aggregation of total monetary costs caused by low competition (before the reform) can still be informative from the policy perspective. Given that the results suggest that the relative prices declined by approximately 10% and the share of single-bidding contracts on the total volume was around 22.7%, the total spending on the treated contracts declined by:

$$0.227 * 0.1 = 2.27\% \tag{1}$$

This is the equivalent of 0.32% of GDP. However, this number should be interpreted with caution given that the observed savings might potentially come at the expense of quality.

5.2 Quality

To understand whether a similar decline in prices also took place among contracts where quality differences are very small or non-existent, I turn to a market of raw materials and office equipment. In the newly constructed sub-sample, I include the following standardized products: *(i)* construction structures and materials and auxiliary products to construction (except electric apparatus) such as bricks, cement, and *(ii)* office and computing machinery, equipment and supplies (except furniture and software packages) such as paper, pencils, and photocopiers.

The estimates pertaining to the sub-sample of these standardized products are presented in Table 5. The results provide evidence of the effect of the reform on the relative prices. Notably, the effects seem to be particularly pronounced for products that are standardized and can be readily traded by any firm. The effect seems to disappear, though, when fixed effects are accounted for, suggesting it is not the same firms offering discounted bids, but rather new competitors entering the market that are driving prices downward. These findings indicate that, at least for this subgroup of procurement contracts, the reform induced savings for the public sector without incurring significant changes in quality.

Table 5: The effect of the reform on the relative prices—standardized products

	(1)	(2)	(3)	(4)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Treated	0.237***	0.184**	0.291***	0.122
	(0.0833)	(0.0799)	(0.1098)	(0.1945)
After	0.191***	0.0786**	-0.0450	-0.0204
	(0.0410)	(0.0380)	(0.0575)	(0.0829)
Treated \times After	-0.290***	-0.240***	-0.296***	-0.195
	(0.0958)	(0.0884)	(0.1015)	(0.1722)
Month FE	Yes	Yes	Yes	Yes
Sector FE	No	Yes	Yes	Yes
Procurer FE	No	No	Yes	Yes
Firm FE	No	No	No	Yes
N	455	455	455	455

Standard errors in parentheses

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

Notes: The dependent variable is the relative price (the share of the final price over the estimated cost). The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2–4).

5.3 Mechanisms

In this section, I consider two hypotheses that could explain the underlying mechanisms driving the increase in competition. First, I test whether procuring authorities extend the timeframe within which potential bidders can prepare their bids. Second, I test whether the

procuring authorities provide more information about contracts in sectors where competition was previously low. To conduct these tests, I employ two measures regarding the extent to which procuring authorities provide information about contracts: a complexity score and a simple measure of the length of the contract description.

First, I find evidence that procuring authorities allocate significantly more time for firms to prepare their bids for all types of contracts. This increase appears to be approximately 4.7 days (see Column (2) of Table 6), which represents an economically significant increase of around 12.7%. Second, the length of project descriptions was shorter before the reform for treated contracts by 6.5 characters (equivalent to 10% of the average length), but this was no longer the case after the reform. We also see a slight increase in the length of all contracts after the reform. Last, the complexity of the description ($\frac{\text{length of description}}{\text{value of project in mil.}}$) does not change (see Columns (3)). Overall, the last two findings suggest that procuring authorities realize that, in order to achieve higher competition, they need to provide more information for potential bidders (as these bidders may be less likely to bid if they are uncertain about the contracts due to insufficient descriptions). This appears to coincide with the goals of the reform. I also observe a negative and significant association between the length of description and relative prices in the sample, which suggests that additional information might, to a certain extent, help to attract bidders.

Table 6: Mechanisms

	(1)	(2)	(3)
	Prep. Time	Length Desc.	Complexity
Treated	0.644	-6.529*	-12.09
	(2.6170)	(3.3550)	(40.0937)
After	4.682***	2.335*	-12.51
	(1.2295)	(1.3977)	(11.4913)
Treated \times After	0.700	2.839	28.85
	(2.9423)	(3.6188)	(41.4936)
Sector FE	Yes	Yes	Yes
Procurer FE	Yes	Yes	Yes
N	9,686	13,348	13,348

Standard errors in parentheses

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

Notes: Prep. Time is the number of days that firms are given to prepare a bid. Length of Description is the number of words in the short description of procurement contracts. Complexity is a measure of how rich the description of a contract is (given as $\frac{Length\ of\ Description}{Estimated\ Costs} \times 10^6$). The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2-4).

5.4 Politically connected and anonymously owned firms

Finally, the last hypothesis to be examined is whether the effects of the reform on prices arise among contracts supplied by firms with preferential access to politicians (politically connected firms) or firms with anonymous ownership structures (such firms were found to be treated favorably in public procurement, see Palguta and Pertold, 2017). Politically connected firms are shown to win disproportionately high volumes of contracts (Schoenherr, 2019; Titl and Geys, 2019; Baranek and Titl, 2024), while the contracts delivered by politically connected firms also tend to be overpriced (Baranek and Titl, 2024). Anonymous ownership makes it impossible to identify conflicts of interest, which in turn makes it harder to hold firms (and their owners) accountable, meaning that these firms may be granted preferential treatment. Thus, for both groups of firms, politicians/public officers may have a reduced incentive to promote real competition. As a result, I hypothesize that if such firms win, no significant effect would be observable on the prices after the reform.

Political connections are here defined in two ways: personal political connections and firms' donations to political parties. The former aligns with the definition proposed by Baranek and Titl (2024) and the measure is constructed based on two administrative datasets: (i) company registry containing identities of firms' owners and members of boards for all Czech firms and (ii) political candidates running for all elections taking place in the Czech Republic. The dataset of personal political connections is obtained by matching the two datasets based on names, city of residence, age, academic titles, and occupation. The firms with such matches are labelled as being personally politically connected to a specific party. Based on the dataset of political parties in power, I then label each procurement contract as either being delivered by a firm connected to the party in power in a given procurer or not. Eventually, I identify approximately 3,500 (1.3%) connected firms among suppliers of public procurement contracts. The latter dataset of firms' political campaign donations come from www.politickefinance.cz.⁸ and covers *all* donations by firms to the major Czech political

⁸A Czech website run by Econlab, z.s.

parties from 2007 to 2018. The average donation is 144,681 CZK (\$7,234), while the median donation is 20,000 CZK (\$1,000).

Anonymously owned firms are defined as firms that do not have listed owners in the Czech company registry. This is the case when a joint-stock company issues bearer shares. An individual who bears the shares is entitled to property rights to the given company. In this situation, the owners were unknown to any supervisory body and ownership could have been transferred to anyone without any trace (Palguta and Pertold, 2017). Notably, a reform implemented at the beginning of 2014 introduced changes to these rules. However, the period after 2014 does not fall within the scope of this paper. The data regarding anonymously owned firms were provided by Datlab, s.r.o.⁹

For the purposes of the following analysis, I define three groups of contracts: *(i)* contracts supplied by a politically connected firms; *(ii)* contracts supplied by firms that donated to political parties; and *(iii)* contracts supplied by anonymously owned firms. These conditions have to be satisfied at the moment when a contract is awarded, i.e., if a firm is connected in 2011, but the contract is awarded in 2012, then it is considered a contract delivered by a politically connected firm.

In Figure 4, I illustrate the development of the relative prices of treated contracts delivered over time for the treated contracts by connected and non-connected firms. From this, two conspicuous patterns can be discerned. First, there is a clear spike in the relative prices of the treated procurement contracts delivered by politically connected companies. Second, I do not observe a decline in the relative prices for the connected suppliers in the period after the reform. The first observation suggests that the connected firms anticipated that it would be more difficult to extract rent after the reform and (successfully) attempted to extract the rent before the reform came into effect. The second observation could indicate that these potentially corrupt firms are able to influence procurement competitions to the extent that prices remain unaffected even after the reform. In Table 7, I examine the second proposition

⁹A Czech private company.

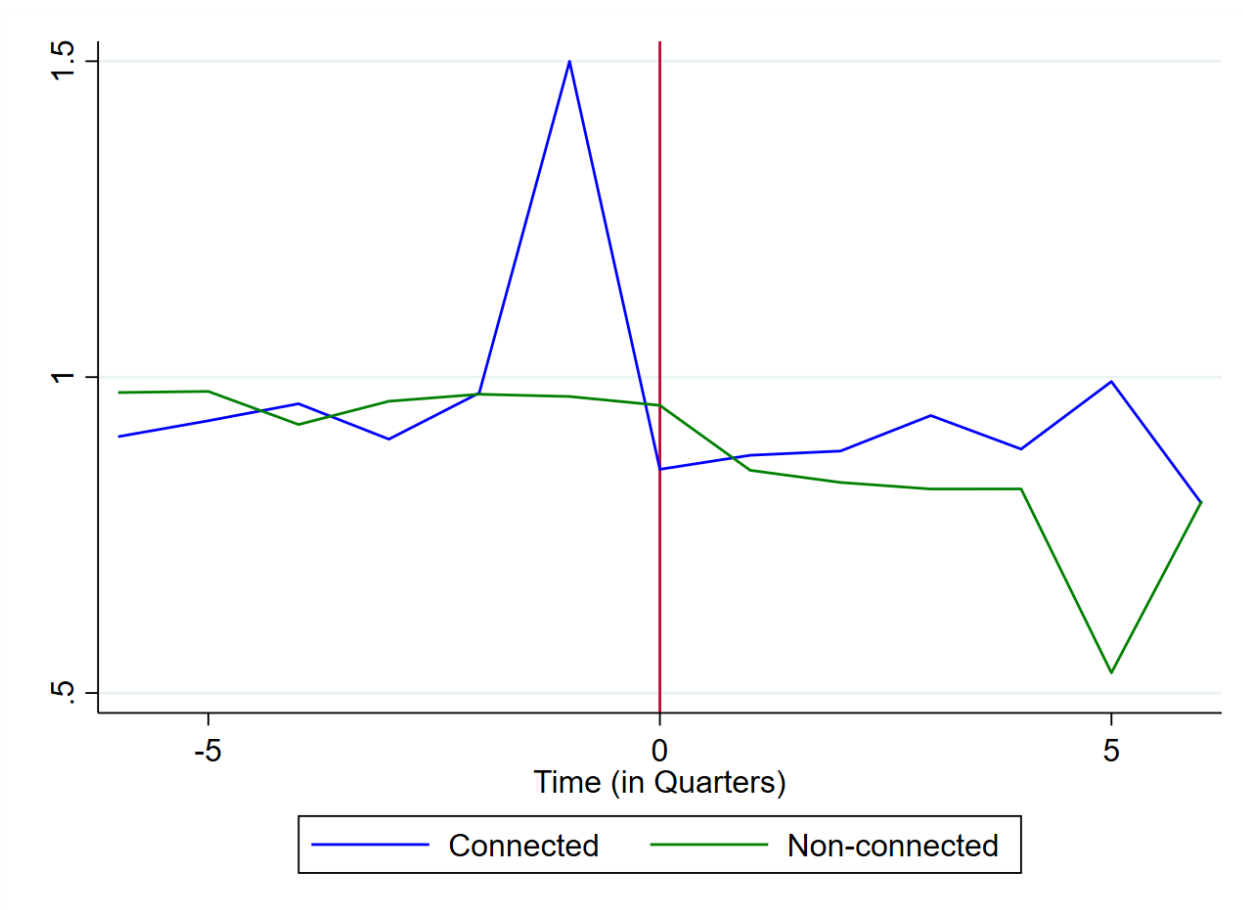


Figure 4: The blue and green lines represent the relative prices for treated contracts delivered by personally connected and non-connected companies, respectively. The red vertical lines represent the reforms in 2012, 2014, and 2015.

empirically. To do so, I run the same regression as in the main specification but restrict the sample to contracts delivered by corporate donors. I find that there is a general decline of around 3% in relative prices for politically connected companies (which is much smaller than the decline for other (non-connected) firms) and that the decline in relative prices is, in this case, not specific to the treated contracts.

Table 7: The effect of the reform on the relative prices for procurement contracts delivered by politically connected firms

	(1)	(2)	(3)	(4)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Treated	0.108*** (0.0353)	0.0336 (0.0373)	0.0424 (0.0568)	0.0328 (0.0688)
After	-0.0321*** (0.0109)	-0.0276** (0.0110)	-0.0393*** (0.0138)	-0.0316** (0.0156)
Treated \times After	-0.0149 (0.0474)	-0.0232 (0.0489)	-0.0292 (0.0652)	-0.0538 (0.0748)
Months around Reform	Yes	Yes	Yes	Yes
Procurer FE	No	Yes	Yes	Yes
Month FE	No	No	Yes	Yes
N	No	No	No	Yes
N	2,915	2,915	2,915	2,915

Standard errors clustered at procurer level in parentheses

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

Notes: The results of a regression with the same specification as the baseline on a subsample of procurement contracts delivered by politically connected firms. The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2–4).

In Tables 10 and 11 in Appendix A, I consider the equivalent hypotheses for firms donating to political parties and anonymously owned firms. I find that for these firms, too, the effect of the reform seems to be weaker than for the rest of the firm population. In the most

saturated specification, I do not find any significant effect on relative prices of contracts supplied by either firms donating to political parties or anonymously owned firms.

5.5 Robustness

5.5.1 Renegotiation

The findings of my analysis indicate a reduction in final contract prices relative to the estimated costs for the treated procurement contracts. One possible avenue through which these final prices could be lowered without impinging upon rent extraction is by means of renegotiations subsequent to the initial contract award. However, in the Czech Republic, the final cost of the contract needs to be equal to the winning bid. In cases where there is a serious reason for an additional cost increase due to unexpected circumstances, it 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. This mechanism restricts cost increases to a maximum of 20%. This could create a problem for my analysis as I do not observe direct cost increases for each contract in our data. Nonetheless, given that I observe all contracts between the specific supplier and procurer, I construct a variable that shows the probability that a given contract is indirectly renegotiated in the way described above. In the Appendix, I show that the average renegotiation actually declined after the reform, which suggests that my results from the main specification likely underestimate the savings and cannot be driven by an increase in renegotiation.

5.6 Timing changes

To further assess the robustness of my findings, I exclude the period spanning 3 months before and 3 months after the reform. This approach aims to mitigate potential influences from bureaucrats hastily awarding contracts just prior to the reform’s implementation or from the decline in the number of procurement contracts following the reform. The findings, which

are presented in Table 8, are substantively similar to the main findings but while indicating a slightly lower effect of a roughly 8% decline in relative prices.

Table 8: Difference-in-differences—6 months around the reform excluded

	(1)	(2)	(3)	(4)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Treated	0.0807*** (0.0143)	0.0646*** (0.0135)	0.0547*** (0.0154)	0.0824*** (0.0211)
After	-0.0888*** (0.0146)	-0.0542*** (0.0102)	-0.0387*** (0.0112)	-0.0318** (0.0140)
Treated × After	-0.0274 (0.0228)	-0.0670*** (0.0181)	-0.0590*** (0.0220)	-0.0773*** (0.0274)
Month FE	Yes	Yes	Yes	Yes
Sector FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Procurer FE	No	No	No	Yes
N	10,030	10,030	10,030	10,030

Standard errors clustered at procurer level in parentheses

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

Notes: The dependent variable is the relative price (the share of the final price over the estimated cost). The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2–4). The key divergence from the basic model lies in the exclusion of the three-month period both prior to and following the implementation of the reform.

5.7 Matching

Matching is an effective means by which to reduce imbalance in covariates between treated and control groups. Below, I use this method to check the robustness of my results. Specifically, I conduct an analysis that employs coarsened exact matching on sectors, coupled with a dummy variable indicating whether a contract falls below or above the threshold.¹⁰ The results of the regressions, which are presented in Table 9, appear to be slightly lower but are nevertheless statistically strongly significant.

¹⁰The Czech thresholds of 2,000,000 CZK for goods and services and 6,000,000 CZK for construction works.

Table 9: Difference-in-differences—matched procurement contracts

	(1)	(2)	(3)
	Rel. Price	Rel. Price	Rel. Price
Treated	0.0792***	0.0681***	0.0450**
	(0.0113)	(0.0157)	(0.0195)
After	-0.0175	-0.0332**	-0.00816
	(0.0132)	(0.0142)	(0.0144)
Treated \times After	-0.0964***	-0.0629***	-0.0588**
	(0.0185)	(0.0228)	(0.0265)
Month FE	Yes	Yes	Yes
Procurer FE	No	Yes	Yes
Firm FE	No	No	Yes
N	7,371	7,371	7,371

Standard errors clustered at procurer level in parentheses

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

Notes: The dependent variable is the relative price (the share of the final price over the estimated cost). The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2–4).

6 Conclusion

In this paper, I find evidence demonstrating that the implementation of a reform mandating a minimum requirement of two bids for every public procurement contract yields substantial monetary savings within the public procurement market. These effects persist even in the

domain of standardized goods, where differences in quality are inconsequential. Remarkably, these savings are estimated to be around 13% of the estimated costs for contracts that historically were awarded without competition. Given the volume of single-bid contracts on the Czech procurement market, this suggests that the monetary savings were roughly 2.95% of the market value. Politically connected firms and firms with anonymous owners appear to have (successfully) attempted to extract rent just before the reform came into effect. Specifically, I provide suggestive evidence that shortly before the reform the relative prices went up, most notably, for contracts delivered by politically connected companies. Moreover, for these firms, the effect of the reform on price does not arise.

This paper's findings have important policy implications. They indicate that a ban on single-bidding may change the behavior of public procuring authorities towards better practice. After the reform, which required higher competition, the procuring authorities started to extend the timeframe for firms to prepare their bids and they provided more detailed descriptions of contracts that had previously experienced inadequate competition prior to the reform.

Building upon the insights presented in this paper, future research endeavors could further delve into the topic by incorporating detailed information on the qualification criteria employed in the public procurement process. Such an approach would be invaluable in providing a more granular understanding of the specific aspects of procuring authorities' behavior that led to the observed reduction in prices.

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Appendix

A The effect of the reform on the relative prices for firms donating to political parties and anonymously owned firms

Table 10: The effect of the reform on the relative prices for procurement contracts delivered by firms donating to political parties

	(1)	(2)	(3)	(4)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Treated	0.128*** (0.0286)	0.0901*** (0.0296)	0.0839* (0.0444)	0.0812 (0.0564)
After	-0.0265*** (0.0095)	-0.0187* (0.0099)	-0.0168 (0.0127)	-0.00896 (0.0144)
Treated × After	-0.00943 (0.0406)	-0.0527 (0.0406)	-0.0525 (0.0503)	-0.0660 (0.0566)
Month FE	Yes	Yes	Yes	Yes
Sector FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Procurer FE	No	No	No	Yes
N	3895	3615	2995	2781

Standard errors clustered at procurer level in parentheses

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

Notes: The results of a regression with the same specification as the baseline on a sub-sample of procurement contracts delivered by firms donating to political parties. The dependent variable is the relative price (the share of the final price over the estimated cost). The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2-4).

Table 11: The effect of the reform on the relative prices for procurement contracts delivered by anonymously owned firms

	(1)	(2)	(3)	(4)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Treated	0.0476 (0.0797)	0.0736 (0.0957)	0.527 (0.3842)	0.435 (0.5312)
After	0.0454* (0.0248)	-0.0716*** (0.0271)	-0.0267 (0.0580)	-0.0224 (0.0559)
Treated × After	0.0935 (0.1343)	0.0295 (0.1368)	-0.0525 (0.3239)	-0.0488 (0.2563)
Month FE	Yes	Yes	Yes	Yes
Sector FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Procurer FE	No	No	No	Yes
N	8019	7654	6662	5924

Standard errors clustered at procurer level in parentheses

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

Notes: The results of a regression with the same specification as the baseline on a subsample of procurement contracts delivered by anonymously owned firms. The dependent variable is the relative price (the share of the final price over the estimated cost). The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2-4).

B Additional robustness

B.1 Average Renegotiation

Table 12: The effect of the reform on renegotiation

	(1)	(2)	(3)	(4)
	Avg. Reneg	Avg. Reneg	Avg. Reneg	Avg. Reneg
Treated	0.00128 (0.0013)	0.00281* (0.0016)	0.00321* (0.0018)	0.00350* (0.0020)
After	0.000988 (0.0007)	0.00234** (0.0009)	0.00211** (0.0010)	0.000953 (0.0010)
Treated \times After	-0.00278 (0.0021)	-0.00410* (0.0024)	-0.00445* (0.0025)	-0.00562** (0.0025)
Month FE	Yes	Yes	Yes	Yes
Sector FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Procurer FE	No	No	No	Yes
N	8019	7654	6662	5924

Standard errors clustered at procurer level in parentheses

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

Notes: The dependent is the average renegotiation defined as the share of the number of renegotiations on the total number of contracts for each procurer. The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2-4).

B.2 Cancellation

Table 13: Difference-in-differences—cancellation

	(1)	(2)	(3)	(4)
	Cancelled	Cancelled	Cancelled	Cancelled
Treated	-0.000227	-0.00201	-0.00224	0.00347
	(0.0031)	(0.0037)	(0.0044)	(0.0056)
After	0.00410**	0.00365*	0.00292	0.00335
	(0.0018)	(0.0021)	(0.0024)	(0.0029)
Treated × After	-0.00222	-0.00265	0.00101	-0.00441
	(0.0047)	(0.0052)	(0.0059)	(0.0068)
Month FE	Yes	Yes	Yes	Yes
Sector FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Procurer FE	No	No	No	Yes
N	7375	6955	5968	5283

Standard errors clustered at procurer level in parentheses

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

Notes: The dependent variable is a dummy variable equal to 1 if a procurement contract was cancelled and 0 otherwise. The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2–4).

B.3 Excluding time periods from the sample

Table 14: The effect of the reform on the relative prices—excluding three months just before and just after the reform

	(1)	(2)	(3)	(4)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Treated	0.0807*** (0.0143)	0.0646*** (0.0135)	0.0547*** (0.0154)	0.0824*** (0.0211)
After	-0.0888*** (0.0146)	-0.0542*** (0.0102)	-0.0387*** (0.0112)	-0.0318** (0.0140)
Treated × After	-0.0274 (0.0228)	-0.0670*** (0.0181)	-0.0590*** (0.0220)	-0.0773*** (0.0274)
Month FE	Yes	Yes	Yes	Yes
Sector FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Procurer FE	No	No	No	Yes
N	10030	9650	8544	7600

Standard errors clustered at procurer level in parentheses

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

Notes: The dependent variable is relative price (the share of the final price over the estimated price). The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2–4).

C Difference-in-differences assumptions

C.1 Composition of population of procurement contracts before and after treatment

Table 15: The effect of the reform on the likelihood of the winning company being a connected firm

Type	Before	After	Total
Transport	9.22	9.48	9.37
Energy	4.64	2.66	3.53
IT and telecommunication	10.32	14.12	12.46
Others	0.20	0.16	0.17
Office equipment	2.36	3.18	2.82
Forestry and agriculture	9.90	2.33	5.65
Medical equipment	12.20	13.30	12.82
Clothes, shoes, and other similar equipment	1.62	1.31	1.45
Legal and other advisory	7.65	5.69	6.55
Natural resources	4.21	2.22	3.09
Construction	17.18	17.39	17.30
Industrial machinery	9.90	20.81	16.02
Technical services	3.73	4.33	4.07
Healthcare, social care, and educational services	6.88	3.02	4.71
Total	100.00	100.00	100.00
	(3516)	(4503)	(8019)

Notes: This table summarizes the number of contracts in the basic 14 sectors in the periods before after the reform (treatment). The period before is defined as the period from July 1st 2010 to March 31st 2012 (the time of the reform) and the period after is defined as the period from April 1st 2012 to December 31st 2014.

By inspecting Table 15, one can see that the composition has not changed a great deal. A single bigger change regards the contracts of industrial machinery. Below, I run a regression identical to the main specification (Table 3). The sizes of the effects are slightly lower than in the main specification (9.4% versus 12.9%), but the direction remains the same.

Table 16: The effect of the reform on the relative prices—excluding industrial machinery

	(1)	(2)	(3)	(4)
	Rel. Price	Rel. Price	Rel. Price	Rel. Price
Treated	0.0634*** (0.0125)	0.0657*** (0.0139)	0.0449** (0.0183)	0.0608** (0.0266)
After	-0.0121* (0.0066)	-0.0131* (0.0074)	-0.0111 (0.0094)	-0.00161 (0.0117)
Treated × After	-0.0751*** (0.0210)	-0.0999*** (0.0221)	-0.0679** (0.0270)	-0.0943*** (0.0335)
Month FE	Yes	Yes	Yes	Yes
Sector FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Procurer FE	No	No	No	Yes
N	6734	6433	5501	4831

Standard errors clustered at procurer level in parentheses

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

Notes: The dependent variable is relative price (the share of the final price over the estimated price). The treated public procurement contracts are contracts in sector-procurer categories in which there was no competition (i.e. in each such contract only one bid was submitted) before the reform. The control group contains public procurement contracts where the mean number of bidders is statistically equivalent to the count of bidders observed in the treated group following the reform (i.e. 2–4).